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FRIEDMAN'S NOBEL LECTURE RECONSIDERED

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Friedman's Nobel Lecture reconsidered¹

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Abstract

In his Nobel lecture, Friedman built on his earlier argument for a 'natural rate of unemployment' by painting a picture of an economics profession which, as a result of foolish mistakes, had accepted the Phillips curve as offering a lasting trade-off between inflation and unemployment and were thereby led to advocate a policy of inflation. It is argued here that in fact the orthodox economists of the time did not accept Phillips' analysis; almost no one made the mistakes in question; and very few advocated inflation on bases vulnerable to Friedman's theoretical critique. The Phillips curve was put to various uses, but advocating inflation was hardly amongst them. It is suggested that one lasting result of the uncritical acceptance of Friedman's history is to limit what appears to be within the reasonable range of views about macroeconomic policy.

1. Introduction

It has been accepted as a fact of history that, because of some fundamental errors of theory, much of the economics profession was once persuaded of the desirability of lowering unemployment by inflationary policy or, as it is sometimes put, 'exploiting the Phillips curve'. The acceptance of this unflattering view is attributable more than anything to the discussion of the development of post-War macroeconomics in Friedman (1977) – that author's Nobel Lecture. There, it will be recalled, he went well beyond what he had said in Friedman (1968), his renowned Presidential Address to the American Economic Association. Whereas the earlier presentation developed the idea of the natural rate of unemployment and offered an expectations-based critique of demand management, the Phillips curve, *per se*, played only a minor role. But in the Nobel Lecture, as in one slightly earlier presentation – Friedman (1975) – he specifically offered a critique of what he took to be the erstwhile consensus and made the Phillips curve and its misinterpretation the centrepieces of his account of how it was that futile attempts to maintain low unemployment resulted in the rise of inflation in the 1960s and 1970s.

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It might very well be argued that policy did not in fact cause this inflation,² and it could also be said that, contrary to the impression given by parts of Friedman's lecture, the Phillips curve has nothing to do with the 'Keynesian economics', properly understood.³ My objective, however, is to question the picture Friedman painted of the attitude of the economics profession generally to the Phillips curve and what it was taken to imply for policy. The background of his picture, considered in part 2, is of a profession that eagerly welcomed Phillips' research as a valuable addition to their apparatus. In fact, the profession at large was rather sceptical of Phillips' findings, and some of the most prominent individuals were distinctly hostile to them. The middleground is of a naïve theoretical understanding of the relationship depicted in the curve. But as part 3 shows, practically no one made the mistakes alleged by Friedman to be at the heart of the problem. And the foreground, of course, was that this enthusiasm and credulity led to the advocacy of inflation as the means of achieving full employment – until the expectations critique was accepted. Although, as I note in part 4, inflation-advocacy did occur, it was rather rare; usually restricted to those with little influence, just as often before Phillips as after him; and in other cases based on a far more effective theoretical argument than Friedman seems to contemplate. These things being so, I consider, finally, the lasting legacy of Friedman's description of intellectual history.

2. The reception of the Phillips curve

A 'stable relation'?

Friedman (1977) p454 said that 'professional analysis' had gone through two stages of which the first was,

'the acceptance of a hypothesis ... that there is a stable negative relation between the level of unemployment and the rate of change of wages – high levels of unemployment being accompanied by falling wages, low levels by rising wages'

and that it

'was widely interpreted as a causal relation that offered a stable trade-off to policymakers'

The emphasis on the 'stability' of the relation is crucial, but Friedman's treatment reveals a basic misunderstanding. The intent of Phillips (1958) had nothing to do with stability under a policy of inflation. His claim was that the relation had been stable for a long period of time. He estimated it for the period 1861-1913 and then noted (p293-295 and p297-298) that later data points up to 1957 were either very close to the curve or could be explained away. This he took to suggest that the same underlying relation survived in the later period. He had no data that would have revealed the effects of sustained inflation, and made no reference to the possibility.

² As it has been by De Long (1997) and Mayer (1998).

³ As was made clear by Sawyer (1985) and Harcourt (2000).

The significance of Phillips' finding is that it amounts to a claim that the relation is independent of any of the extensive institutional or political changes which took place over the period. The pertinent consequence was that the Phillips curve seemed to offer a simple but powerful account of inflation. Acceptance of the analysis amounted to acceptance of the view that price stability could be achieved – but could only be achieved – by an appropriately high level of unemployment. The importance of 'stability' was not that there was a 'trade off', but – quite the contrary – that there was only one way to stop inflation.⁴

A Keynesian theory of inflation?

This point leads to a second comment on Friedman. He interpreted Phillips' finding as being welcome to Keynesians whereas in fact it was most unwelcome. Friedman (1977) p469 said that the Phillips curve

'filled a gap in Keynes's theoretical structure. It seemed to be the "one equation" that Keynes himself had said "we are... short"'

The use of this particular quotation is remarkable. It comes from the appendix to chapter 19 of Keynes (1936) and is part of the discussion of the limitations of Pigou (1933). It is Pigou, not Keynes, who is being said to be short the equation. In any case, Friedman's implication that the Keynesians were lacking a theory of inflation is quite wrong.⁵ The theory from Keynes (1940) was a theory of excess-demand inflation. That gave rise – under considerable simplification – to the theory of the 'L-shaped' supply curve by which aggregate supply was perfectly elastic until full employment was reached and perfectly inelastic thereafter. This suggested that good, Keynesian, management, by targeting full employment precisely, could deliver both it and price stability.⁶

Such was the basic outlook, for example of the thoroughly Keynesian Clark et al. (1949) (see especially p43-46), writing for the OECD. They, like many others, recognized what they took to be a subsidiary point, that inflation might in fact start to rise before full employment was reached because of such things as supply-side bottlenecks and the development of pockets of monopoly power. Naturally, as low but persistent inflation continued throughout the 1950s, these 'subsidiary' considerations attracted more attention and the outlook developed into the theory of cost-push inflation. A crucial aspect of that – argued powerfully from a Keynesian perspective by, for example, Balogh (1958) – was that it held that cost developments were not for practical purposes controllable by the restriction of demand, and such restriction would therefore be ineffective in reducing inflation.

⁴ As noted most emphatically by Leeson (1997a), this was certainly the interpretation Phillips himself put on his work, although in the later literature this kind of position was perhaps more associated with Paish (1958) and the final chapter of Paish (1962/1966). Sumner (1984) is unusual amongst post-1977 authors in recognizing the true significance of 'stability' in Phillips' work.

⁵ Friedman is not, of course, the only person to suppose that the curve supplied a needed pricing equation. Tobin (1972a) does the same thing.

⁶ It is arguable whether this is much of a 'theory' of inflation rather than just a statement as to how to avoid it. But the point is that there was no gap in the understanding of the matter. It could also be said that Phillips provided a precise equation, but in dozens of studies following his work, no one ever confirmed the precise equation.

This had two consequences of importance for the current discussion. One is that those who took this view of inflation were unlikely to accept the Phillips curve, since it purported to show that the level of demand was the crucial determinant of inflation. The second was that, from the Keynesian perspective, the problems of inflation and unemployment were essentially separate. Employment was determined by demand, while the tendency of unions to raise wages when employment was high could be controlled by measures such as direct price control, moral suasion, incomes policy, and perhaps even increasing the degree of product market competition and openness to international trade. On orthodox theory of the time, subject to the effectiveness of these measures, there was no impediment to achieving full employment and price stability.⁷ Phillips' analysis threatened this elysian vision.

Adopted 'with alacrity'?

These points make it no surprise that it is hard to see much justification for the further claim of Friedman (1977) p469 that the hypothesis of

'a stable relation between the level of unemployment the rate of inflation was adopted by the economics profession with alacrity'

In fact the general tone of responses to Phillips' work was rather hostile.⁸ Some simply did not believe it – Robinson (1937) p30-31 was one of the first to warn of a general tendency for full employment to promote rapid wage increases, but she never accepted the stability of a particular relation, and in Robinson (1973) p8 she was happy to dismiss the 'late-lamented Philips curve'.

Others produced analysis to dispute Phillips' findings. Amongst them was Griffin (1962) p381 who found that the data showed that 'The 'Phillips curves' shift systematically as percentage unemployment is reduced'. Routh (1959) p299 said,

'his statistical material is not appropriate...; different wage and unemployment series will yield significantly different results; and that even if the imperfections of the material are disregarded, Professor Phillips reads more into his evidence than it can in fact sustain'

One large group of Phillips-curve critics believed that different variables were better predictors of wage change than were unemployment and changes in unemployment. For example, Hines (1964) found trade union membership to be more important than unemployment; Kuh (1967) also found unemployment not to be a powerful predictor of wage changes and suggested that productivity changes performed better.

Others, such as Bhatia (1961), tried to construct Phillips-type relations for the United States. He found (p296)

⁷ There is a temptation to define 'full employment' as the price-stability level of unemployment in Phillips' analysis, but this arises from accepting Friedman's account. In 1958, 'full employment' was defined by reference to the number of people unemployed, or its relation to the number of vacancies, and inflation was simply a different problem.

⁸ Leeson (1998) similarly noted a number of early, hostile response to Phillips. But he believed both that the curve was accepted 'with alacrity' and that it was treated as exploitable.

'The influence of the changing rates of unemployment is not clearly marked... Although a relationship appears to exist between unemployment and the rate of change of money earnings in the period 1900-42, this relationship has varied from one period to another... from 1932 to 1942 earnings rose continuously despite the prevalence of high levels of unemployment'

The reaction of Kaldor (1959a) perhaps shows most clearly the state of Keynesian thinking at the time. He gave a detailed and immediate response to Phillips arguing that even if unemployment were raised, this would not prevent wage push inflation. Wage increases were, he felt, determined more by profits than unemployment so demand policy would control inflation only if it prevented profit growth, and this, in turn, could certainly not be done without compromising the growth of output.⁹ Consequently, he said (p295),

'I have dwelt on Professor Phillips' views at some length, simply because a misguided diagnosis of the causes of wage inflations can play such a vital role in policy decisions which are of the utmost importance to the economy.'

This general approach to the problem was commonplace, both at the end of the 1950s and later. Many economists, certainly including those who had most influence over policy, rejected the view that inflation could be controlled by raising unemployment, and with it, the Phillips curve.

A further indicator of how far from the truth it is to suppose that the Phillips curve quickly came to dominate thinking is the number of authors who say little or nothing about it, even in the context of policy discussion. In Caves and Krause (1968), only Smith (1968) discusses the Phillips curve at any length and he comes to no particular conclusions about it. Beckerman (1972), which aims to assess the Labour government of 1964-1970, is surely a place one would expect to find discussion of it if it had been important to policymakers. In fact the book contains only one mention of it, in a footnote, and saying the curve receives too much attention. Dow (1964) hardly refers to the curve, and Stein (1969) says nothing at all about it. But how could it be, on Friedman's account, that it played no role in 'The Fiscal Revolution in America'?

It would of course be wrong to suggest that Phillips attracted no supporters – Lipsey (1960) is the outstanding early example that proves otherwise. But the view that anything resembling Phillips' formulation was either quickly or widely accepted – particularly that it was quickly and widely accepted by those most concerned with policy – is seriously mistaken. It was not a convenient finding for the orthodox of the day, it did not fill any gap in their theory, and it was not accepted with alacrity, or even in many cases, ever.

3. Explaining and exploiting the inflation-unemployment relation.

The idea that the point of interest in Phillips' research was its stability in the face of institutional change was soon lost. Practically no one showed much interest in estimating a single relation for long periods of time and those who did failed to find

⁹ Lipsey and Steuer (1961) took issue with the point about profits, but the issue is how the curve was received, not who was right.

one. 'The Phillips curve' became the label for estimated relations between inflation and unemployment, along with, invariably, other variables. Estimates of that kind dated back to Fisher (1926) and Netherlands Central Statistical Office (1933).¹⁰ Researchers of the topic amongst Phillips' contemporaries included Klein and Goldberger (1955) and Dicks-Mireaux and Dow (1959), to name only the most notable of the econometricians concerned. But Samuelson and Solow (1960) named the relation after Phillips. From then on, and in that sense, there was indeed a great deal of attention focussed on 'the Phillips curve', but still not in the way that Friedman suggested.

Real and nominal variables

Friedman (1977) p469 summarized his story of the Phillips curve saying that Keynesian economics had had the effect that

'the age-old confusion between absolute prices and relative prices gained a new lease of life'

so that in considering the relation of wages and employment,

'economists would analyse the relation between unemployment and nominal rather than real wages ... and would implicitly regard changes in anticipated nominal wages as equal to changes in anticipated real wages'

Leaving aside for the present the question of anticipations, on the treatment of the distinction between real and nominal variables, Friedman is wide of the mark.

Admittedly, Phillips own treatment of this point was inadequate. He noted that cost of living increases might affect the wage bargain but modelled them as doing so only on occasions when that increase was greater than the increase in wages that would otherwise be indicated by the level of unemployment. The mistake was quickly pointed out by Knowles and Winsten (1959) p120 – more critics of Phillips – who said that his neglect of the 'recent tendencies' of prices gave him 'the handicap of appearing something of an anachronism'. Even he, however, shows no sign of failing to understand the distinction between real and nominal variables.

However, it is extremely difficult to find others amongst the large number of people estimating Phillips curves who make any such mistake. On the contrary, it is thoroughly standard practice to include change in the price index as a variable explaining wages. If one looks to Phillips' immediate successors, Lipsey (1960), Lipsey and Steuer (1961), Dicks-Mireaux and Dow (1959), and Klein and Ball (1959) and Dicks-Mireaux (1961) all consider them. Even amongst his predecessors, Tinbergen (1951) and Klein and Goldberger (1955) included them and Klein and Bodkin (1964) evidently thought the inclusion of price changes in the wage equation perfectly routine.¹¹ Furthermore both Klein and Goldberger, and Klein and Ball observe that although their estimations relate to nominal wages, their inclusion of

¹⁰ So says Tinbergen (1937) p16

¹¹ They say (p389) that in wage equations 'there is customarily an allowance' for price change.

prices as an explanatory variable removes any material concern arising from the theoretical proposition that bargains should be struck in real terms.

As time went on, and prices began to rise more quickly, the econometricians certainly did not cease including prices in their equations. Vanderkamp (1966) is an where the author thought the view that there was no money illusion was debatable but nevertheless thought that price change should certainly be included in the wage equation. And when Friedman (1968) shifted attention on to expectations, all worthwhile studies included some measure of expected price change. Clearly it was never, in the research literature, presumed that bargains were struck without regard to real values.

An inspection of textbooks reveals the same picture. Ackley (1961) p449 whilst simultaneously drawing attention to the theoretical point, observed as a matter of fact that it seemed 'almost automatically accepted' that

'Wage rates should rise at least as much as the cost of living, quite independently of labor supply and demand'.

It is true that in most Phillips curve estimates up to the mid 1970s, the coefficients on price changes or expected inflation were less than one, meaning that the adjustment of wages to prices was, apparently, less than complete. That may have surprised or disappointed the monetarists but it is merely a fact about what the data said, not an elementary mistake by the analysts.¹² The fundamental point that in one way or another one must account for price changes was accepted by all the principal analysts after Phillips and almost all of them before him.

The expectations critique and the theory of the Phillips curve

A second dimension to Friedman's argument on this point is the implication that, as a result lack of attention to expectations, there was no viable theory of the Phillips curve. The argument – by convention variously attributed either to Friedman (1966), Phelps (1967), or Friedman (1968) – was that wage bargains would be struck in the light of expected inflation so that reductions in unemployment resulting from inflation would persist only for as long as it took expectations to adjust to reality. This led Friedman (1977) p458 to say,

'There is no stable trade-off between inflation and unemployment; there is a "natural rate of unemployment" (U_N) which is consistent with the real forces and with accurate perceptions; unemployment can be kept below that level only by an accelerating inflation'

The implication – made perspicuous by the word 'only' – is that once the expectations-critique is accepted, a long-run Phillips curve could not be rationalized.

¹² I leave aside, obviously, econometric issues arising from simultaneity of the price and wage equations, on which see Hendry and Wallis (1984)'s discussion of Sargan (1964). There may well be imperfections in the literature on that level, but the issue is whether there was a new lease of life for the age old confusion. Plainly there was not. Many other technical questions are considered by Santomero and Seater (1978), not least those relating to the proper way of modelling expectations.

A number of theoretical rationales for the Phillips curve are vulnerable to this kind of argument. Phillips himself followed Samuelson (1948) and Hansen (1951) in treating the speed of price adjustment as determined by the degree of excess demand. It seems reasonable to suppose that that relationship would change in a context of persistent inflation. Lipsey's (1960) approach was similarly vulnerable.

But these are not the only theoretical explanations of the Phillips relation. A different line of thinking was deployed, for example by Tobin (1972b). He suggested that at any point in time some labour markets will be in excess demand and others in excess supply. If nominal wages rise more readily in those in excess demand than they fall in those in excess supply, there will be some tendency to an increase in average wages even when the sum of excess supplies is slightly greater than the sum of excess demands. A high enough level of unemployment will force wages down in the excess-supply sectors, but the achievement of wage stability requires unemployment. There is then clearly a trade-off at least at low rates of average wage increase. In this kind of case, as it is sometimes put, inflation 'lubricates' the labour market.¹³

This argument had been made many times before 1972, and indeed Friedman (1958) p251-2 had considered a version of it. There, he suggested that persistent inflation would transform downward nominal rigidity into downward real rigidity. In other words, even in labour markets in excess supply, nominal wages would rise. Whether this is correct clearly turns on what it is that explains the original nominal rigidity, and possibilities abound. Some may explain specifically nominal, but not real, rigidity. And in that case the expectations critique does not show that there can be no long-run trade off. Versions of the lubrication argument continued to be presented after Friedman's Nobel Lecture – for example, by Baumol (1978), and it forms a component of the argument of Akerlof, Dickens, and Perry (1996).

Therefore, not only were all the principal estimators of Phillips-type relations clearly conscious of the importance of changes in the price level, but the theorists of the Phillips curve also had readily available an account which responded to the expectations critique, and suggested the possibility of a persistent trade-off.

4. The advocacy of inflation

Friedman (1977) followed up his assertion that the Phillips curve had been understood as offering a stable trade off by suggesting that this mistake underlay a policy of seeking to reduce unemployment by means of inflation. He said (p455),

'in the circumstances of the post-World War II period, when governments everywhere were seeking to promote 'full employment', [the rate of inflation consistent with] it tended in any country to rise over time'

¹³ It is perhaps worth noting that 'the natural rate of unemployment' could still have the sense given to it by Friedman (1968) – the level that would arise from the solution of the Walrasian equations – and one could assent to the both the view that unanticipated inflation will reduce unemployment and that anticipations will be modified in the face of a persistent inflation so that no permanent effects are available *from that source*. But there would still be a trade-off resulting from the asymmetric speeds of adjustment towards the Walrasian equilibrium.

It is by now apparent that Friedman's picture exaggerates the importance of the Phillips curve, and certainly of mistakes about it. That in itself suggests that his assertions about the policy consequences of Phillips' research should be treated cautiously. But further, I suggest that little of the interest that was shown in the Phillips curve related to the advocacy of inflation. Some authors do express themselves in a way that suggests they are treating the curve as stable under inflation, but when they do so, very often they are either assuming that policy will aim at price stability or have something else altogether in mind. In those cases where it is presumed to be stable by an author who is advocating inflation, the lubrication argument is almost always visible. What is very rare indeed, but ought to be common on Friedman's story, is actual advocacy of a policy of inflation on the basis of an account of the Phillips curve vulnerable to the expectations critique.

For example, a number of authors simply use estimates of the curve to advocate a policy of price stability. Typically, and understandably, they do not discuss its stability under a policy of inflation. One of these is Watanabe (1966). He estimated a Phillips curve for Japan and said, (p32)

'The purpose of the present study is to provide a feasible answer to this problem of rising consumer prices from an economist's (not from a politician's) viewpoint'.

He had no reason to consider the stability of the curve under a policy of inflation. Similarly, Eckstein and Wilson (1962) p392 revealed what they had in mind in the tone of their conclusion, saying, p406,

'These findings imply that there is no one critical level of unemployment which is consistent with "noninflationary" wage increases. If profits are high, a much higher level of unemployment is necessary than if profits are low"

Others made it clearer that they found the estimates of the required unemployment level alarmingly high, but again that is not the same thing as proposing a policy of inflation. For example, Klein and Bodkin (1964) p391 remark that the indicated rate of unemployment is 'more than we want to tolerate now', but they end up (p428) contemplating compulsory savings as a means of restraining price increases – and that, surely, does not reveal much tolerance of inflation.¹⁴

One particular case that has attracted attention is that of Samuelson and Solow (1960). Leeson (1997b) amongst others suggests that they had a special role in promoting the use of the Phillips curve to advocate inflationary policy. His case turns on the facts that without any formal econometrics they presented a roughly drawn 'Phillips curve' for America, captioned the graph as 'showing the menu' and offered their 'best guesses' at the inflationary consequences of various levels of unemployment.

The issue of whether, in fact, the effect of Samuelson and Solow's paper was to promote the acceptance of inflation is not under consideration here but it would be rather easy to overstate the extent to which the actual content of the paper argues for that outlook. The title of the paper is 'Analytical aspects of anti-inflation policy' and

¹⁴ Compulsory savings were previously advocated as an anti-inflation device by Slichter (1948b)

most of it concerns the problem of distinguishing cost-push and demand-pull inflation. When the authors do address the issue of the Phillips curve offering a 'trade-off', they raise a number of doubts about it and in particular say (p187),

'a past characterized by rising prices, high employment, and mild, short recession is likely to breed an inflationary bias'

This leads them to suspect that high rates of unemployment would be only temporarily necessary to control inflation. Later (p193) they say that if a low-demand policy were followed,

'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'.

This clearly amounts to a position very much like that later taken by Friedman, and argues in favour of sound-money policy.

It is true that in Samuelson and Solow these are speculations, not conclusions. They also contemplate (p190) the possibility that attempts to control inflation by demand management might create adverse shifts in the Phillips curve, either because it would tend to lead to geographical concentrations of unemployment or (p193) because it would produce an increase in structural unemployment. They also suggest that low demand might lead to 'class warfare and social conflict' and hence to slow technological progress. Whilst these things argue for expansionary policy, they also clearly reveal substantial doubt as to whether the curve is stable.

Even where they suggest (p193) that the 'non-perfectionist's goal' of 3% unemployment could be achieved by inflation of 'as much as 4 to 5 percent', this is immediately qualified not only by the words 'in the years immediately ahead', but also by a warning that policy decisions might affect the curve in the longer run and that 'it would be wrong' to imagine otherwise.

Overall, a balanced assessment of the actual content of the paper would say first and foremost that the Phillips curve plays rather a limited role in it. 'Guesses' are presented as to the inflation costs of various employment targets, but it is specifically and clearly noted that these relate to the short-run. The firmest position expressed about the Phillips curve is that there are a variety of things – on both the supply and demand sides – which might shift it. Indeed, Laidler (1971), amongst others, actually cited Samuelson and Solow as authority for the claim that there is no stable Phillips curve for the United States.

Laidler (1997) has also noted that it is no easy matter to find authentic, Phillips-curve based advocacy of inflation, even though that by the end of the 1950s discussion of various 'trade offs' of policy objectives was fairly widespread. He suggested that Reuber (1962) was the first to estimate the costs of inflation and unemployment and deduce the optimal point on the trade off. That study was a little-circulated paper for the (Canadian) Royal Commission on Banking and Finance (Porter (1964)), but it led to Reuber (1964) in which the author presents a diagram with a Phillips curve tangent

to a community indifference curve at a positive inflation rate, and says that the Phillips curve offers 'consistent and attainable combinations' of policy objectives. He ultimately concludes that for Canada, if international constraints were disregarded (and it is not clear that he thought they should be), the optimal combination would be 2.25% unemployment and 3.75% inflation. This is clearly something close to being a genuine piece of inflation-advocacy of the kind Friedman supposes was common, but it can hardly be said to have been influential. In fact Laidler notes (footnote 22) that the Royal Commission 'seems to have been quite uninfluenced by Reuber's study'. Indeed it was. It concluded (p419) – pre-empting Friedman on the point – that 'if significant price increase come to be anticipated', inflation would have no effect on output.

One group who clearly felt American unemployment had been too high in the 1950s were Kennedy's Council of Economic Advisors. One might have expected them, perhaps following the lead of Samuelson and Solow, to deploy the Phillips curve in the early 1960s. In fact they did no such thing.¹⁵ Certainly, they argued for expansionary policy, but they did so on the basis that it could be achieved without inflation. Accordingly, Kennedy's first *Economic Report of the President* said (p42)

'the target for stabilization policy is to eliminate the unemployment which results from inadequate aggregate demand without creating a demand-induced inflation'.

It set a target of 4% unemployment as an interim goal and added,

'If we move firmly to reduce the impact of structural unemployment, we will be able to move the unemployment target steadily from 4 percent to successively lower rates'

The point, obviously, was that removal of the 'impact of structural unemployment' was necessary to the further reduction of unemployment without inflation. That report is also the one which introduced the 'Guideposts' designed to encourage non-inflationary wage settlements.

The Guideposts themselves led to much controversy. One contribution which is particularly interesting in the current context is that of Samuelson (1967). He said (p54)

'Any criticism of the Guideposts which does not explicitly take into account the Phillips curve concept I have to treat as having missed the fundamental point of all economic policy discussions'.

¹⁵ Tobin (1972c) claims (p16-17) that the Council of Economic Advisors (on which he served), chose a 4% unemployment target in 1960 'with one eye on the Phillips curve' and aware that 4% unemployment had been achieved with 4% inflation in the mid 1950s. But there is no hint of that from the Report and there is no reason based on what was believed at the time that it should have been felt impossible to lower unemployment to 4% without inflation. He took a very different line in Tobin (1985/2005) p111 saying that 4% was an estimate of the 'inflation-safe unemployment rate' and 'That goal was achieved by the end of 1965, with negligible increase in the rate of inflation'. In Tobin (1995) note 3 he specifically denied that they were motivated by the idea of 'taking a ride' up the Phillips curve.

There is obviously some exaggeration here, but it is instructive to see what Samuelson was saying the fundamentals were. He says (p52-3) that if wages and prices were perfectly flexible, there would be no inflation below full employment. In that case 'the authorities would engineer fiscal and monetary expansion just up to the point of full employment'. This is clearly a statement of the 'L-shape' theory. But, he says, in the actual economy prices creep upwards before full employment is reached. The purpose of the Guideposts is to stop this happening.

The interesting point is that he goes on to characterize a 'good' Phillips curve as one which, at high levels of employment, is steep but indicates inflation arising only at low levels of unemployment, whereas a 'bad' one is more shallow but crosses the unemployment axis at a higher level of unemployment. (The full employment level is the same in both case). The 'good' one is, obviously, more nearly L-shaped. But the fact that this is the steeper curve surely shows that a policy of moving up it and accepting inflation was not under consideration.

In other cases, the vocabulary of the Phillips curve is deployed in a manner seeming to condone the view that it is exploitable, and perhaps that it should be exploited, but in fact the discussion concerns some other issue entirely. For example, Peacock (1972) used the same kind of diagram as Reuber but his purpose is nothing to do with advocating inflation, but to illustrate a point about the coordination of policy tools. Fromm and Taubman (1968) used it and said that one would want to find the optimal point on a properly specified Phillips curve, but this was a vehicle for their discussion of the econometric difficulties in discovering that specification, not for policy advocacy.

Lipsey (1965) also drew it, but in his case the point of the exercise was not to consider optimal policy but, as he put it (p210),

'to re-examine the controversy between the two competing explanations for higher rates of unemployment in the United States: the structuralist and the deficient-aggregate-demand theories'

And the paper offers a clarification of the conceptual distinction between them with a view to preparing the ground for an empirical determination of the issue as to which is the predominant problem. There is no issue about advocating any policy. Curiously, Lipsey (1978) is critical of his own treatment saying that he had forgotten the warning against presuming the curve stable; and again in Lipsey (1981) criticizes himself for presuming the relation stable. But these comments seem harsh. For the purpose to which he was putting the curve, its stability was immaterial. In Lipsey (1965) p211 he had said,

'problems arise, however, when the objective of reducing unemployment conflicts with other objectives such as maintaining a stable level of prices and a satisfactory balance of payments. The problems that concern us can be illustrated by [the diagram]'

Obviously the Phillips curve diagram does not depict the balance of payments, so it can only be illustrative, and problems 'that concern us' is a reference to the objective of clarifying the distinction between the two types of unemployment.

Clearly, the posing of this question presumes that some worthwhile sense can be given to the idea of demand-deficient unemployment, which, some would say, it cannot. But contemplating that possibility is not inflation-advocacy.¹⁶ Lipsey notes that eliminating such unemployment may conflict with other objectives and ultimately defines structural unemployment as the amount that remains when demand is optimized and frictional unemployment subtracted. Whilst the use of price stability to illustrate the conflict is suggestive of a stable Phillips curve trade-off, nothing whatever actually turns on that, and the point is explicitly only illustrative. No policy advocacy occurs.

Admittedly, when Brechling (1968) lifted the diagram straight from Lipsey (1965), he did it for the purpose of considering a possible trade-off between inflation and unemployment, and ended up concluding that such a trade-off might exist. He clearly had it in mind that an inflationary policy would be appropriate. Even so, it was a tentative conclusion, in the light of various data limitations that concerned him, and he did not advocate any particular policy. Indeed, he ended his paper on a non-committal note by saying, quite rightly, that more than inflation and unemployment would have to be considered in the optimization problem.

Another branch of the literature focuses specifically on the econometrics of the Phillips relation and in particular the size of the coefficient on expectations. It should be remembered however that the discovery – frequently made until the mid 1970s – that it was less than one is a separate question from whether inflation is desirable.

Solow (1968) was one of those whose econometrics seemed to contradict Friedman. His and others' research suggested the coefficient was between 0.3 and 0.4 and he concluded that for empirically relevant cases, there is a trade-off. Having made a collection of further numerical assumptions, he said that (p16),

'there is in the not-so-short-run, a trade-off locus between inflation and real output; and that its position is such that high employment and price stability may be incompatible... What to do about it is a difficult and important question of policy. It is doubtful that there is any single, simple, dramatic solution.'

And he suggests no policy.

Solow (1969) presents similar econometrics and takes a similar line. This piece is presented specifically as a test of Friedman's hypothesis from his *Presidential Address*, which Solow describes (p2) as 'one of those things it is hard not to believe'. Later (p14) he says that his estimates

'offer no support whatever to the expectations hypothesis in its strict form. On the contrary, they imply the existence of a permanent trade-off surface...'

¹⁶ As noted above – footnote 7 – the temptation to presume that reducing unemployment and raising inflation amount to the same thing arises from the acceptance of Friedman's view.

He then presents some evidence that high rates of inflation might be fully incorporated into expectations, suggesting that a steady 8.5% inflation would have this characteristic. And he subsequently concludes (p17)

'Whatever may be true of Latin-American-size inflations or even smaller perfectly steady inflations, under the conditions that really matter – irregular price increases with an order of magnitude of a few percent a year – there is a trade-off between the speed of price increase and the real state of the economy. It is less favourable in the long run than it is at first. It may not be 'permanent' but it is good enough for me.'

This is as near as he gets to a discussion of policy, and, hence as near as he gets to the advocacy of inflation. In particular, its being 'good enough' for him did not lead to a policy proposal. Although the existence of the trade off is clearly stated, it seems to be very much a conclusion of a scientific enquiry. The hypothesis has been tested and the results are reported.

In the same vein, a number of studies published by the Brookings Institution in the years immediately following Friedman's Presidential Address denied the expectations hypothesis. Robert J Gordon (1970) was one, and could be read as concluding that policy of inflation would be desirable, although again he stops short of saying so. He is firmly convinced that there is a long run trade-off, but offers no particular policy advice. Gordon (1971a) and Gordon (1971b) are a much more inflationist pieces, but they are rather late to count as substantiating Friedman's argument, and in any case much of the force of them relates to the issue of whether policy should seek to reduce inflation rather than whether it should have permitted it in the first place. Others who addressed the question were out and out opponents of inflationary policy – Okun (1971) might think that the trade off exists, but is explicit that the policy of exploiting it as unimplementable.

There are also those who advocate inflation in one way or another, but not on the basis of the kind of Phillips curve Friedman had in mind. There are, first of all, those who clearly adopt the 'lubrication' argument associated with Tobin (1972b) and are led by this to advocate moderate inflation. Indeed, Gordon is probably in this category and this must be true of Tobin's more inflationary contributions, including where he does not state the theory fully, such as Tobin and Ross (1971) and Tobin (1972c). Indeed, this argument was so frequently presented that it is easy to imagine many of those who are silent as to their theory had it in mind.

Another instance of a variety of inflation-advocacy comes from those who have a form of unemployment hysteresis in mind. Inflation is advocated not because some point along the Phillips curve is preferable to the point at price stability, but because the policy in question will move the Phillips curve in a desirable direction. As has already been noted, Samuelson and Solow (1960) speculated on that possibility, and it was mentioned by Brechling (1968), and by Rees (1970). It was advocated more forcefully by Modigliani and Tarantelli (1973). They suggested that in a 'developing country' expansionary policy would enlarge the pool of workers in the active labour force and that once engaged, these new workers would contribute to keeping wages

down.¹⁷ The long run tendency would be towards stable prices at a higher level of employment than initially. Their discussion does presume that, except for their hysteresis effect, the Phillips curve is stable. But it is not the point of their argument to consider that, and their whole discussion is organized around the question of what it takes to achieve price stability.

There were, of course, true advocates of inflation basing their arguments on the Phillips curve, but for the most part they are more minor figures. Reuber was one – as already noted, not an influential one. Bowen (1960) was another, but his policy proposals were not repeated in Bowen and Berry (1963). Bodkin (1966) p279-280 estimated the rate of inflation that would result from a 'full employment' policy, and was therefore clearly contemplating such a policy. Robert A Gordon (1967) p182 certainly espoused a moderate inflation as the price for high employment. But this hardly amounts to consensus.¹⁸

If one casts the net a little wider, one could include Sultan (1957) – a textbook treatment which advocates inflation. Also Slichter (1948a), p42-45, who certainly favoured inflationary policy and continued to do so in popular writings in the 1950s such as Slichter (1952), Slichter (1954), and, Slichter (1957). Then there are the theoretically more sophisticated contributions of Vickrey (1955) and Kaldor (1959b), whose argument was based on the theory of Kaldor (1957). None of these, as will be apparent from their dates, found their motivation in Phillips (1958), and the last two authors were basing their argument on altogether different ideas.

There are also a few who accept the expectations critique (or some equivalent argument) and nevertheless contemplate inflationary policy. Brechling (1969) speculated on this. There is no reason, he says (p161),

'why slowly accelerating inflation and high output should not be preferable to constant (or zero) inflation and low output. Moreover, a stabilization program might conceivably involve the maintenance of low levels of unemployment interrupted by sharp increases in unemployment to destroy inflationary expectations.'

Obviously, a more developed expectations critique might put paid to that, but of course he is not advocating the policy. Lerner (1960) is in the category of inflation-advocate, if one takes seriously his idea for alternating accelerating inflations with currency reform. Perhaps most notably, there is Phelps (1967) himself. His treatment revolved around the point that if there is a positive rate of time discount and adaptive expectations, there would be some optimal rate at which inflation would increase. It is a kind of inflation-advocacy, but deeply theoretical, and it is something of an curiosity that he is one of the people persistently quoted as having destroyed the rationale of the Phillips curve.

¹⁷ Brechling (1969) applied the same argument more generally, suggesting that some of those employed in a 'temporary' boom will turn out to have higher productivity than anticipated and the effective pool of labour will thereby be increased in size.

¹⁸ Although they would still be inflationists, any of these authors may have had a 'lubrication' argument in mind and their inflationism would not be vulnerable to Friedman's critique.

As to the Phillips curve itself, however, its role in practical policymaking would seem to be the opposite of what Friedman claimed. The first appearance of anything like it in *Economic Reports of the President* comes in 1969 where it appears in the context of a discussion of how to reduce inflation and the clear implication is that higher unemployment will do the job.¹⁹ Such a policy is not espoused, but rather the point is made that it would be preferable for the Guideposts to work. Similarly, Stewart (1977) says, with some sarcasm, of the Conservative government elected in the United Kingdom in 1970, that they promised to control inflation, and that, (p157)

Underlying the Government's confidence in the policy this time lay one of the most dazzling keys in the history of economics: the Phillips Curve

In other words, they were going to raise unemployment. In both countries, it would seem, the prominence of the Phillips curve (or a like relation) in the discussion of inflation-policy emerges when it is deployed to describe the requirements of price stability.

Some consequences of the acceptance of Friedman's account

That the economics profession ever made such foolish mistakes as Friedman claimed is hardly a recommendation of it; but nor is the fact that it quiescently accepted the view that they were so often made. For both these reasons, and simply as a matter of proper scholarship, the received account of the orthodox approach of the 1960s to the problems of inflation and unemployment should be corrected. As far as the history of economics is concerned, Friedman's Nobel Lecture is no more than mythologizing.

It should be noted, however, that although it is only a myth, Friedman's depiction of a naïve and indeed rather stupid professional consensus of the 1960s may not be entirely harmless. Two consequences in particular should be considered. The first is that Friedman subjected the that orthodoxy not merely to criticism, but to derision. He suggested that the greatest economists of the time had, to what can only be the deepest professional discredit, committed the most foolish of intellectual blunders. Once that is accepted, it hardly needs to be added that, shameful as it was ever to have made those inflationary mistakes, it would be more shameful still ever to make them again. Thus this romance of a Dark Age of Economics does much, I suggest, to shore-up support for approaches to policymaking based on central bank independence, inflation targeting, and fiscal conservatism.

Secondly, whilst he is remembered for dismissing the possibility of a long-run trade-off, Friedman's greater achievement was to put the short-run trade-off at the centre of almost everyone's macroeconomic analysis. The historically attested theoretical alternative to the vertical Phillips curve is not a trade-off in the long-run, but a denial of it in the short-run. On this view the problem of unemployment was, over a very wide range, simply a different problem from the problem of inflation. It is as much the triumph of Friedman's history as of his theory that it has become almost impossible to articulate the view that macroeconomic policy should be concerned

¹⁹ The discussion in question is at p95-98 of the Report of the Council of Economic Advisers. The expression 'Phillips curve' is not used, but the backdrop to the discussion is a chart of inflation and unemployment rates in recent years, which has displays the data with a semblance of a Phillips relation. The text clearly notes that the relation is 'neither mechanical nor precise'.

with unemployment without seeming merely to plan a day-trip up the Phillips curve. But this is in fact not the only view one might take.

It may be that recent experience in the United States and the United Kingdom will revive interest in a macroeconomics without the Phillips curve. In those countries, policy has evidently not – whatever the accepted theory says – been governed by the view that falling unemployment must be inflationary, and nor have efforts to make durable estimates of the NAIRU been altogether successful. The recognition that the economics of the Keynesian era was not just a matter of considering the ramification of elementary errors, and that that the Dark Age is in any case fictitious would, perhaps, facilitate the development of thinking in this area.

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