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ECONOMISTS ON SAMUELSON AND SOLOW ON THE PHILLIPS CURVE

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Economists on Samuelson and Solow on the Phillips curve[*]

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Abstract

Samuelson and Solow published a widely read paper in the May issue of the American Economic Review of 1960. It discussed the causes of inflation, the Phillips curve, and related matters. Discussion of their paper frequently says that it presented the Phillips curve as a stable, exploitable relation, and hence played an important role in the development of inflationary policy. This is hardly so. Sometimes authors notice this, but they nevertheless say it was misread as advocating inflationary policy and hence played the same role in policy development. Close attention to what was said about it in the relevant period – the 1960s – reveals that it was not then seen as advocating inflationary policy at all. This raises a strange puzzle as to why it was that, rather suddenly, it came to be incorrectly said that Samuelson and Solow had been interpreted as being inflationist when they neither were that, nor had been interpreted in that way.

JEL: B22, B23

I. Introduction

It does not take much reading in the history of postwar macroeconomics to find out that Samuelson and Solow (1960) is a highly cited paper. But because it is so highly cited, it takes rather longer to develop a good picture of what people say about it. By repute, it is the paper that either pointed to, or anyway was understood as pointing to, the possibility of permanently reducing unemployment by moving round the Phillips curve with inflationary policy. It did not do that. This has been noticed by some, although they do not take the argument very far. But in any case, it is usually taken to mean Samuelson and Solow must have been 'misread' as demonstrating the benefits of inflation. Indeed, there are plenty of misreadings of it in the literature, and often silly ones, but not much of *that* one. The case is much more interesting.

I consider how the paper has been described in some textbook and historical accounts, and why they cannot be right; then what it did say, and why; then what Solow later said about it (not much, in fact); and then how it was read before about 1968; and how it was read after then. Then I can see what can be done to explain the whole muddle.

[*] I am grateful to Rune Frederiksen, and Monika Mrazova for translating passages of some of the papers cited herein, and to participants at the UK History of Economic Thought conference at Kingston University, 2010 for their comments.

II. Treatments of the impact of the paper

As an example of the way Samuelson and Solow's work has been described, one might consider Frisch (1977). He drew a diagram with a Phillips curve and a 'community indifference curve' supposedly showing society's preferences as between inflation and unemployment and claimed that Samuelson and Solow, 'propagated the Phillips curve as an instrument of economic policy' and that,

> 'According to their suggestions, a trade-off exists between the rates of inflation and unemployment, so that the government has the possibility of choosing alternative points on the Phillips curve with alternative rates of inflation and unemployment.'

Leeson (1997) offers another example of this kind of thing in a rather longer discussion. Although at some points in his paper Leeson notes numerous doubts about this expressed by Samuelson and Solow, in his summary of his own argument, no qualifications are mentioned when he says,

> 'Samuelson and Solow believed that they had uncovered evidence that suggested that tolerable and stable rates of inflation were associated with high employment'.

Similarly Sargent (1999):

> 'In 1960, Paul Samuelson and Robert Solow found a Phillips curve in the U.S. time series for inflation and unemployment. They taught that the Phillips curve was exploitable and urged raising inflation to reduce unemployment. Within a decade, Samuelson and Solow's recommendation was endorsed by many macroeconomists and implemented by policy makers'

Despite the impression all this sort of thing might create, there is simply no argument that Samuelson and Solow felt they had discovered a curve which was stable. Noting that Phillips (1958) had found his relation to persist over a long period of time, they said, p187,

> '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.'

And later, (p189) they say in connection with a close scrutiny of the data,

> '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'

That this was the 'most interesting' point is itself not something to be ignored, but in any case, they also made it very plain that they felt there was every chance of social change, policy, or economic developments shifting the curve in either direction in the future. Clearly, then, there is no case for the view that they say the curve is stable, whatever later commentators believe or say.

They did point to policy alternatives, presenting a diagram captioned as displaying a 'menu of policy choice' and by saying

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

Whilst they offer no adjudication, they do go on to suggest (p193) that the likely outcome of the 'tug-of-war' of politics will result in something in between the two.

After this they emphasized – again – that these 'guesses' related only to the 'next few years', and suggested that a low-demand policy might either improve the tradeoff by affecting expectations, or worsen it by generating greater structural unemployment. Then, considering the even longer run, they suggest that a low-demand policy might improve the efficiency of allocation and thereby speed growth, or, rather more graphically, that the result might be that it 'produced class warfare and social conflict and depress the level of research and technical progress' with the result that the rate of growth would fall. If, simply in virtue of being the last point, this has a slight emphasis then that is the nearest they come to advocating expansionary policy, and even that relates to achieving growth not to reducing unemployment *per se*.

But before they are convicted for being the source of Phillips curve inflationism, even on these slender grounds, there is another point. When they read off possibilities from their 'menu' there was no indication of which policy they preferred – the presentation of the two policies is perfectly balanced, even down to the repetition of 'in the years immediately ahead'. It is only after this, when they discussed the range of possible longer-term considerations – those that they thought might move the curve – that even the hint of favouring expansion arose.

So when they said in the discussion of the data that the 'most interesting' thing was that the relation had shifted upwards in the 1940s and 1950s, what they must have had in mind was that what goes up might also come down; and their expansionist case rests heavily on that possibility. It is not merely that they did not think the curve stable, but much more than this – and quite contrary to the later stories – it is the possibility of its moving, not its stability, that is the crucial finding if they are to be construed as proposing any particular policy.

II

None of this, however, even approaches the heart of the concerns of Samuelson and Solow's paper. The question they were addressing was that of the explanation of the inflation of the 1950s – particularly the period 1955-57 – and the implications it had for macroeconomics. Mild though that was later to seem, this 'creeping inflation' as it was called was, at the time, a source of much anxiety.

When they wrote, the postwar period, and the period in which practical experience of Keynesian policy in peace-time conditions could have been acquired, was still very short. What would later appear to be regular patterns of behaviour were then barely discernable as such, but were rather more like one-off incidents, which might or might not provide general lessons. And to make matters more difficult the effects of reconversion from a war economy and then the Korean War (1950-53) clouded the picture further. But by the end of the 1950s there was enough data to make it just about possible to begin to learn from experience. Samuelson and Solow's was an early attempt to bring together what one might call the accumulated lessons of the experience of the period and to determine the prospects of actually achieving full employment and price stability.

At that time, no one denied that excessive aggregate demand would cause inflation. Some would explain this relation entirely in terms of government borrowing, or of monetary growth, but probably more would have done so in ways derived from Keynes (1940). In that way of thinking, employment was determined by the level of aggregate demand, but if demand were excessive, inflation would result. An aspect of the basic and simplest versions of this picture was that for demand to be 'excessive', it must be greater than the level required for full employment. In that case, inflation and unemployment were problems arising in distinct circumstances. On the other hand, it was possible – and in fact quite normal – to doubt that inflation between 1955 and 1957 was due to excess demand. Unemployment in those years struck many as being too high to be consistent with 'full employment'.

So alternative explanations of inflation were needed, and several were suggested, most of which have tended to be described in not altogether appropriate terms as 'cost

push' theories of inflation. The central, elementary, case simply said that organized labour was able to use its bargaining power to raise wages faster than productivity and this resulted in price increases. But there was a variety of other more or less closely related accounts. One was a similar in nature to the idea of unions causing inflation, but had producers with market power raising prices. If, as was sometimes suggested, these producers were producers of inputs, then this would obviously be cost inflation, but they could equally well be producers of final consumption goods in which case the terminology is not quite appropriate but since the problem was not one of excess demand, this kind of phenomenon would tend to be grouped with 'cost push' inflation. A variation was considered by Galbraith (1957) suggested that there was a kind of collusion between steel producers and unions which combined to raise their wage and prices, and consequently many other prices as well.

Such approaches to inflation were later greatly disparaged on the basis that prices could not continue to rise unless the policymaker allowed for nominal demand to increase to accommodate the higher prices, and therefore that the real source of the inflation lay with the policy, and with excessive demand. But these points are based on a poor appreciation of the cost push idea. In the 1950s the question under discussion was not whether a low enough level of demand would bring price stability. Almost anyone would have accepted that it would and that in that sense the policymakers' willingness to validate wage increases, or whatever, with expansions of nominal demand could be blamed for inflation.[1] But that did not mean that the only way to achieve price stability was to restrict demand, nor that that would be a sensible way.

Lerner (1960b), using his idiosyncratic terminology of 'buyers" and 'sellers" inflation, captured the point perfectly, saying (p 121)

> The appropriate treatment for buyers' inflation is to cut down the excessive spending that causes it. This may be done by a restrictive monetary or fiscal policy. But if restrictive monetary or fiscal policy is used against sellers' inflation, spending is reduced when it is not excessive, so that we get a deficiency of demand, depression and unemployment. The inflation will continue, however, unless the induced depression is severe enough to destroy

> the power of sellers to raise their prices. This may call for more depression than the authorities are prepared to impose or the public willing to suffer. We then get insufficient depression to stop the inflation and we suffer from both evils at the same time.

And if demand restriction was not an appropriate remedy, and perhaps not an effective one, other ideas as to what to do suggested that wages (or prices) might be affected by non-demand means, including, paradigmatically, the urgings of policymakers, or the implementation of incomes policy, and the like. And indeed, the 'Guideposts' which sought to influence both wages and prices came to be a key feature of the Kennedy Administration's policy. They were greatly supported by cost push proponents including both Samuelson and Solow, and opposed by those who denied the possibility or the reality of cost push, including Friedman. So, if inflation was attributable to cost push in the mid 1950s it might be possible, by some appropriate adjustment, to avoid it without reducing demand, and hence without reducing employment. In that case, despite what had happened in the mid-1950s, it would be possible to achieve price stability and full employment simultaneously.

Another way of explaining inflation, which Samuelson and Solow clearly noted, was one explored in depth by Schultz (1959). The argument begins with the idea that if demand shifts from one sector to another, it might be that prices rise in the latter more readily than they fall in the former. If that was so, the average rate of price increase would depend on the extent of such shifts in demand as well as whatever market imperfections prevent downward adjustment where it would be appropriate. Even in this basic form it is clear that whilst high levels of employment will be conducive to this kind of inflation, the speed and degree of the shifts in demand is also crucial. Schultze suggested that there had been particularly large shifts in the mid-1950s and that this – rather that the level of employment itself – was the explanation of the inflation at that time. Inflation was then a more or less natural outcome of economic development and change.

With these arguments current, and bearing in mind that there is no sure way to give a numerical value, as distinct from a conceptual definition, to 'full employment', it is easy to see why, at the end of the 1950s, there could be a debate as to its compatibility

with price stability. This, then, was the broad issue to which Samuelson and Solow's paper was addressed: Were price stability and full employment – or, as it was sometimes put, were price stability, full employment and collective bargaining – compatible in the America of their times?

The bulk of Samuelson and Solow's paper consists of a discussion of how hard it would be ever to decide whether inflation was due to excess demand or to cost push factors, and they ended up concluding that the kind of aggregate data they had did not allow them to distinguish between the various possible causes. They did contemplate (p191) the possibility of a 'vast experiment' in the form of a reduction of aggregate demand – if small reductions in demand had a dramatic effect on inflation, the inflation must be demand pull, if not it would be something else.

It is once they have considered the question of the causes of inflation – and noted the danger of trying any 'vast experiment' – that Samuelson and Solow make the remarks quoted above: 'Our own view will by now have become evident…'.[2] What has become evident is that their view is that one cannot readily distinguish the various sorts of inflation but that nevertheless, one can see that there was inflation, and the relationship between it and the level of unemployment could, they thought, be roughly quantified, in a way that quite overtly applies only to the circumstances of the time and is intended as much to characterize the extent of the problem as to offer choices about it. The conclusion, then, is not an optimistic one about how little inflation is required to achieve full employment, but the pessimistic one that there is a serious difficulty in achieving full employment and price stability at the same time. And then their paper ends, unsurprisingly, with an expression of hope that something can be done to rectify the situation.

III

One interesting aspect of later life of this paper is that Solow made a number of further comments about it which are sometimes seen or presented as revealing inflationist intent in that paper. And if one is predisposed to that view of the 1960 paper, even the title of Solow (1978) – '*Down the Phillips curve with gun and camera*' might give that impression. And the fact that there Solow says (p3), 'Any time seems

to be the right time for reflections on the Phillips curve' suggests an all-pervasiveness of the curve in his thinking which is not otherwise evident. In fact the paper is mainly concerned with a discussion of the role of expectations and the question of how one might respond to Friedman's (1968) argument. Solow was certainly unconvinced by that argument but the 1960 paper is described as having offered a 'guess' which turned out to be reasonably accurate for a few years, and suggests that that is all that should be expected.

A comment which has been quoted more than once and attracted some attention comes from form Solow (1979). He said,

> 'I remember that Paul Samuelson asked me when we were looking at those diagrams for the first time, 'Does that look like a reversible relation to you?' What he meant was 'Do you really think the economy can move back and forth along a curve like that?' And I answered 'Yeah, I'm inclined to believe it' and Paul said 'Me too.' And thereby hangs a tale.'

When it is quoted just like that and in the context of an argument that Samuelson and Solow took an inflationist position, as it usually is, it might not be immediately apparent that the diagrams originally under discussion were those from Phillips (1958), not Samuelson and Solow's paper. It certainly would not be clear that Solow immediately drew the contrast between this and the American data, which he said did not reveal stability of the curve. He went on to say that for the post War years, he and Samuelson found something that looked like a Phillips curve, labelled it as a menu, and that their relation performed reasonably well for a period 'given that we only intended this as a schematic thing, not the result of formal statistical work', but that after that the relation changed.

As the paper then develops Solow describes his scepticism about literal-minded versions of the expectations hypothesis, and offers a number of ideas about the causes of inflation at less than full employment and the relation of inflation to unemployment in the post-War period. There is no further reference to Samuelson and Solow. The paper is from the alumni magazine of the Massachusetts Institute of Technology and is obviously written for an intelligent, non-expert audience, and is intended to be both

informative and entertaining. It also shows signs of being quickly written, and there is nothing surprising in the fact that it contains the occasional off-hand remark like that at the end of the quotation. Reading the whole piece, it is difficult to see what specifically Solow had in mind as being the 'tale', but it was certainly not to say that this piece was the origin of Phillips curve inflationism.

It is also notable that even the question of the importance of Samuelson and Solow's paper in Solow's mind is questionable. Solow (1962) was a discussion of the need for expansionary policy published shortly after the author left the staff of the Council of Economic Advisers; Solow (1964a) is a deeper discussion of the causes of unemployment at the time, but still very much concerned with the practicalities of policy; and Solow (1973) is written in a similar vein concerning later developments, with also with a retrospective treatment of the early 1960s. None of them mentions either the Phillips curve or Samuelson and Solow (1960). Solow (1964b) does discuss the Phillips curve and there he said that if demand restriction had its main effect on employment and output rather than inflation, 'policy faces a nasty dilemma' and that it was yet to be seen whether institutional change could resolve it – but even there he makes no mention of Samuelson and Solow. Solow (1968) mentions the paper, but there he merely said that he and Samuelson had discussed the cost push – demand pull debate.

There is no discussion of the paper in Klamer (1984), but in Solow's later conversation in Snowden and Vane (1999) p284-5, there is. There, Solow explains that he and Samuelson had been attracted to Phillips' work because of the long run of data it dealt with but had found that there was not the same stability in the American data. The authors were, says Solow, 'just curious' and in considering American economy in the 1950s, they felt it provided 'some help in understanding slow inflation'. That sounds about right for a conference paper. Solow also noted that the authors had been aware that there was an issue about expectations and specifically rejects the picture painted by Leeson (1997) of what Solow quite fairly calls his depiction of a 'dark drama of conflicting ideas and personalities'.

An interesting point emerges from Solow (1988/2005). There he again notes how cautious the paper was, but also reports that Assar Lindbeck had said to him that,

notwithstanding the qualifications, it was written in a tone which was too optimistic, and it was this that led to its being so influential in promoting inflationary policy. To this, Solow seems to assent, saying 'I had to admit the justice of that observation'. Perhaps he had this conversation in mind when he wrote Solow (2002) which is specifically a retrospective view of the 1960 paper. It again notes the numerous nuances of the discussion, but says that the expansionist inclinations of the authors are visible and that they were later proven too optimistic. All that may be true. But what of the view that the paper had the effect of promoting inflationary policy?

IV

Even in the first few years after the publication of the paper, it is cited a good number of times. In a large number of cases, it is referred to as demonstrating the existence of a 'dilemma'. In such cases the significance of the point is that Samuelson and Solow demonstrated that, contrary to what was hoped, full employment and price stability appeared not to be compatible. The existence of the 'tradeoff', even when that is the word used was seen as a disappointment, not an opportunity to manage policy for low unemployment at insignificant cost in inflation.

Thus, for example, Hansen (1960). He noted Samuelson and Solow's results, and although he considered them 'tentative', he said

> 'It may be doubted, however, that we can achieve both a satisfactory level of employment and price stability without major improvements in our anti-inflation weapons'

and drew from this the conclusion that

> 'We are suffering from the serious delusion that there is a harmony of interest between the various goals we seek.'

So it is clear that the message he took from Samuelson and Solow was that the problem existed and several others referred specifically to them as authority for the same point.[3] A closely related group are impressed by their arguments for the

possibility or the existence of cost push inflation. As Kindleberger (1967) p217 put it, they 'dispose of the *a priori* belief that cost-push inflation is impossible', and the same point was made elsewhere.[4]

A large and interesting group are those who cite Samuelson and Solow specifically as authority for the view that the Phillips curve or the American Phillips curve was *unstable*. Bronfenbrenner and Holzman (1963) was, for a long time, the authoritative survey of the theory of inflation – They say that Samuelson and Solow's is the best known study of the American 'Phillips curve' and that they found it had shifted in the post war period – and that is all they say about Samuelson and Solow. Shonfield (1967) said that Samuelson and Solow constructed a Phillips curve for the United States and showed that 'there is no long-term stability of the curve', and suggested that the explanation for this was that the expectation of high employment had made employers more willing to grant wage increases. And Kuh (1967) – who was certainly no enthusiast for Phillips' argument – went even further than this and, rightly or wrongly, attributed to them 'a candid exposé' in the form of 'a scatter diagram indicating no relation for the aggregative data'. Again there are plenty more like this.[5]

Others from this period could just about be understood as reading Samuelson and Solow as suggesting the existence of a stable tradeoff, but these authors reject that view, or else reject the desirability of inflationary policy on some other basis. In either case it can hardly be the case that these instances show that Samuelson and Solow were the source of the belief in the desirability of inflationary policy. Chandler (1960) and Lerner (1960a) – both conference comments on Samuelson and Solow – made the point that if one did try to lower unemployment with inflation, the required inflation rate would not be stable. Beyond that, neither showed any sign of believing that proposing inflationary policy was an objective of Samuelson and Solow's paper. Chandler noted the caution they had used in advancing their 'guesses' as to possible policy outcomes, and Lerner remarked that the most interesting thing in their paper was the discussion of the difficulty in empirical distinction of cost and demand inflation. He also said they 'questioned' the view that high output cannot be maintained if accompanied by inflation, but took the point no further than that.[6] Scott and McKean (1964), citing Samuelson and Solow, say of the Phillips curve describes 'the relationship between rates of inflation and rates of unemployment, and each point

on the curve represents a combination of these two rates that policy makers might choose'. They suggest that Samuelson and Solow 'apologise' for inflation on this basis, but their own piece is thoroughly anti-inflationary (on the basis that inflation harms growth).

A variety of miscellaneous comments on Samuelson and Solow can also be found. But many of them are not really discussing the inflation problem at all, and none give any sustenance to the view that those authors were believed, by the economists of the 1960s, to see a stable Phillips curve, or if they did, that this was accepted as making a case for inflation.[7]

Whilst this catalogue of those citing Samuelson and Solow up to about 1968 is certainly not complete, it does not appear that there is *any* author then who could be said to have learned the benefits of inflation from the idea that Samuelson and Solow had found a stable Phillips curve. Most see them as propounding the instability of the Phillips curve, or of describing the requirements of anti-inflationary policy, or simply pointing to a problem, or if they are seen as suggesting inflationary policy, the authors in question reject that proposal. It seems that the nearest to finding an inflationist message in the paper which he is prepared to accept is – strange to say – Phillips (1962). In a passage which clearly presumed that policy would not be set to achieve price stability, he referred to their estimates as if they might guide American policy. But he also said that he thought those estimates were too optimistic and also noted that they depended on the assumption of 'the continuation of the conditions of the post-war period', so even he was not really treating their curve as stable.

V

But a little later, the principal themes of the literature started to change. One thing, which might perhaps seem just to be an oddity, is that Samuelson and Solow's paper became frequently cited for the fact that they described a relation relating price change and unemployment rather than wage change and unemployment. Gray (1968) may have been the first to emphasize this, citing them as the source for a 'derivation' of a 'modified Phillips curve' of this kind. And Smyth (1971) picked them out as his sole example supporting the claim that 'The Phillips curve has been widely interpreted

as providing a relationship between the rate of inflation and unemployment'. This point receives no emphasis in the earlier period, but in the later one it became commonplace, and persistent.[8] Peston (1971) stands out from this group because he is critical of them over the same point, observing, very much in passing, that they were amongst the first to 'take the two dangerous steps' of drawing a smooth Phillips curve without a scatter of points, and to draw it as price-change relation.

At the same sort of time there was slightly more tendency to see Samuelson and Solow as propounding an exploitable relation, although generally speaking its existence is still rejected by those who do so. Hume (1970), citing Samuelson and Solow, says that with the Phillips curve it is 'supposedly possible to find the degree of deflation of demand … which… would be consistent with any particular rate of wage-caused inflation'. He is clearly sceptical about it himself, as the word 'supposedly' reveals, but in any case after a brief consideration of arguments he says, 'so the use of the Phillips Curve as a basis for anti-inflation policy prescriptions can be misleading.' There is no indication that he thinks it might be used as a basis for inflationary policy prescriptions. Lohani and Thompson (1971) say that Samuelson and Solow suggest the possibility of trading-off inflation and unemployment, and furthermore that the view that this is possible is commonplace (although they cite no others). They immediately doubt it, pointing to the expectations critique. Okun (1971) is an interesting case, since he was so much involved with policy analysis. He says that Samuelson and Solow thought 4% unemployment would be associated with 2 or 3% inflation and perhaps implies they thought this would be sustainable, but the point of the paper – *The mirage of steady inflation* – was to deny such a possibility.

Rothschild (1971) made the point that Samuelson and Solow treated a relationship between price change and unemployment, and said,

> 'after the publication of Phillips' article Samuelson and Solow (1960) carried his analysis fully into the sphere of inflation policy. They shifted the Phillips curve to a price change-unemployment diagram… and named the construct a 'menu of policy choice' for policy decisions. Since then the 'trade-off' question has been continuously discussed in general and quantitative terms.'

He makes no further mention of Samuelson and Solow, and says nothing of their caveats and warnings over the stability of the curve, and nor of any policy they might have suggested. But he does go on to a discussion of the consequences of the existence of a stable curve, and the implication that the idea comes from them is fairly clear. He – most unusually – seems prepared to contemplate inflationary policy. But although he has traced the idea to Samuelson and Solow, the arguments for it are very much his own.

It is later still that the idea emerges that Samuelson and Solow had been instrumental in convincing economists generally of the exploitability of the tradeoff. At about the same time as Frisch (1977), Nobay and Johnson (1977), giving an historical account of the debate over monetarism, identify the Phillips curve as the 'missing equation' in the Keynesian system and clearly presuming it underlay inflationary policy say that it

> 'gained widespread acceptance as the 'trade off' between prices and unemployment through its use in this respect by Paul A Samuelson and Robert Solow.'

They say no more about the role of the Phillips curve, nor who specifically is supposed to have followed Samuelson and Solow in this view, nor the views of Samuelson and Solow themselves. But they do try to acquit Phillips of any misunderstanding of the relation between real and nominal wages, and thereby clearly suggest that Samuelson and Solow and those who (supposedly) followed them were wanting in that area. Feldstein (1981) notes that they did point to instability, but emphasizes their role in making the Phillips curve a 'basic tool of policy analysis in the 1960's' with the result that 'many economists and politicians concluded that it made good sense to accept a permanently higher inflation rate'.[9]

Bordo and Schwartz (1983) similarly claim that their work led to inflationary policy but in two other respects went further than most, saying

> Phillips (1958), Samuelson and Solow (1960), and Lipsey (1960) reported evidence of a stable inverse relationship for the U.K., the U.S., and other countries…

The pure fantasy of 'other countries' seems to be unique. But the emphasis on a finding of a stable relation is interesting because it is the exact opposite of so many earlier readings, and became something greatly emphasized about the work of Samuelson and Solow.[10]

An odd case that shows the difference time can make is that of Lipsey (1978) said that in his early work he had been cautious about the policy implication of the Phillips curve but that in Lipsey (1965) he had forgotten about his own earlier warnings – and adopted the Samuelson and Solow view of it. The 1965 paper does not, in fact, show much sign of being inflationist, but more interestingly, that paper does not even mention Samuelson and Solow. So, quite apart from the issue of whether the 1965 paper was inflationist, what is clear is that it is only in the later one that Lipsey feels that such a view is to be associated with Samuelson and Solow.

By the time of Humphrey (1985) this idea about Samuelson and Solow must have been fairly well established because he noted (p24) what he took to be the importance of

> 'the Samuelson-Solow interpretation of Phillips' curve as a menu of policy from which the authorities could select the best (or least undesirable) inflation-unemployment combination and then use their policy instruments to attain it.'

Although it may be an overly fine distinction, this perhaps puts the emphasis more on inflationism than merely on the idea that the curve is stable. But again there are also plenty of others of this general type.[11]

A further development may be detectable in the transference of this interpretations of Samuelson and Solow to a wider group of economists. Whereas the first to say that they believed in an exploitable Phillips curve were advocates of the vertical Phillips curve view, in due course, post-Keynesians joined in. So for example, King (2002) makes Samuelson and Solow the villains in associating Keynesianism with the Phillips curve. Best and Widmaier (2006) say that Samuelson and Solow thought the Phillips relationship 'enduring' and clearly think they played some role in diverting thinking from Post Keynesian lines.

While this view was developing, it should be noted, there were also those who continued to see Samuelson and Solow quite correctly. As a consequence, what they said tended to be unexceptionable. Laidler (1997) cites them for finding the curve unstable, and Laidler (2003) is specifically and pointedly critical of Sargent (1999), noting the ineffectiveness of his footnoted equivocations to the passage quoted above. There are plenty of other, unexciting examples.[12] But one group perhaps stands out. These, evidently having noticed that Samuelson and Solow did not propound a stable Phillips curve, were nevertheless persuaded that the economists of the 1960s had treated them as if they did. Thus Ormerod (1994) p120, for example, is entirely fair to Samuelson and Solow in noting their caution, but nevertheless implies, without much discussion, that others took an inflationist message from them. Haldane and Quah (1999) advert to Samuelson and Solow's caution but assert that many others took the idea of a stable tradeoff from them.

And as in the earlier period there are a collection of those making miscellaneous remarks, sometimes accurate, sometimes not, sometimes more like tomfoolery than anything else but in any case not on the question of the encouragement of inflationary policy (details on request). And there are also, at last, but in an unexpected way, those who do indeed seen in Samuelson and Solow the origins of the idea of a stable tradeoff, and who approve. But they are not the mainstream economists of the 1960s, as yet unenlightened by Friedman. They are of the twenty first century and are specifically rejecting the vertical Phillips curve, as they would have it, still rejected by the data. They look to Samuelson and Solow not quite as those who taught them this message, but as distant precursors of their own work. Thus – Lundborg and Sacklen (2006) and Graham and Snower (2008) are arguing for the possibility of a long run tradeoff seem to give Samuelson and Solow *credit* for believing the same.

But in the part of the literature which had come to see Samuelson and Solow as the source of damaging, inflationist policy proposals, as time went on it became possible to find more extreme and more emphatic claims about them. So, for example, Galbraith (1997) says that in 1968 mainstream economists were committed to 'Samuelson and Solow's version' of the Phillips curve, although he says nothing about what that was, and this claim is simply adopted by Snowdon and Vane (1999) who show no sign of recognising a possibility that they might be anything but inflationists,

and immediately move to try to acquit Phillips of the same error. Jossa and Musella (1998) seek specifically to consider the evolution of debate on the Phillips curve, but clearly make Samuelson and Solow the origin of Phillips curve tradeoffism. (They also say that Samuelson and Solow were the first to suggest that the Phillips curve could throw light on the relation between inflation and unemployment, although Phillips (1958) had done that). Tabb (1999), citing Leeson (1997) says that the menu interpretation was 'influentially endorsed' by Samuelson and Solow also makes it also the 'centerpiece of Samuelson's best selling introductory text.' In fact, in Samuelson (1961), Samuelson (1964), and Samuelson (1967) – the three editions of the decade – the discussion of the Phillips curve in contained in an appendix and takes less than a page - and none of them, incidentally, even mentions the Samuelson and Solow paper itself. Kirshner (2001) p43 said 'In the early 1960s, most Keynesian economists argued that governments could reduce unemployment indefinitely if they were willing to tolerate a higher rate of inflation', citing Samuelson and Solow as the only reference in support of this claim. Nelson (2004) p135 says, without further justification, 'In his academic work, Paul Samuelson was jointly responsible for the proposition that there was a permanent trade-off between unemployment and inflation in the US (Samuelson and Solow, 1960).' Barnett (2004/2007), quoting the New School website with approval says the paper is justly famous for presenting the Phillips curve 'to the world', which apart from anything else, seems rather to belittle *Economica*. As of August 2010 the New School for Social Research (2010) web site also says that Samuelson and Solow integrated the Phillips curve into the Keynesian 'edifice', that it gave an 'nice policy conclusion' so that 'In short, Samuelson and Solow (1960) argued that there was a definite inflation-unemployment trade-off which could be manipulated by policy-makers'. Even Fisher (2008) – writing a New Palgrave entry on the Phillips curve – said that Samuelson and Solow 'were bold enough to posit a stable and exploitable structural relationship between unemployment and inflation'.

VI

There is obviously a bit of a myth about Samuelson and Solow. They did not say the Phillips curve was stable, and they certainly did not deduce the desirability of inflationary policy from any supposed stability of it. Some people say they did either or both of these things, but there you are. Other people have said that Samuelson and Solow convinced other people of these things. Some of those who say that have kept quiet about what Samuelson and Solow actually said, or have recognised that they did not say these things themselves. But they are wrong too. Samuelson and Solow did not convince anyone of such things. Their real role in the story is that long after it had ended, some people incorrectly said they had convinced other people of something they did not say themselves.

It would be interesting to know how that happened. It is easy enough to see that there are facilitating circumstances in the shape of a general disregard for what people actually said when it happens to be convenient to point at them to fill a gap in a story. And the story that it is convenient to have them fit into is clear enough. It is the story of that great triumph of the scientific method in economics: The story of the advance from error in the form of the exploitable Phillips curve to the truth of the natural rate. A lot of people have noticed that the idea of the exploitable Phillips curve is nothing much to do with what Phillips was talking about so for the story to make sense, that interpretation has to be found somewhere else. And Samuelson and Solow's paper looks a bit more like a discussion of an exploitable Phillips curve that others. They do use the word 'menu' – and how much has been made out of that! And they do actually discuss policy.

Beyond that, I suspect that quite a lot turns on Friedman's two best known discussions of the Phillips curve. In Forder (2010a) I argued that the idea that *anyone* thought of 'exploiting the Phillips curve' in the 1960s is pretty well a mistake. There are a couple of cases, but the great bulk of the discussion of the Phillips curve was about other things altogether. But Friedman (1968) presented it as if one might think of exploiting it. He did that for the purpose of saying it would not work, but curiously, the discussion of the possibility that it might work largely *follows* his presentation. He still did not say that in fact governments had been trying to exploit it. That came only

with Friedman (1975) and Friedman (1977). It is interesting, perhaps that one can see changes in the discussion of Samuelson and Solow more or less following each of these.

Friedman himself, in 1968, used the expression 'Phillips curve' to describe the relation of price change to unemployment. With the one odd exception of Scott and McKean (1964), I don't think there is any econometric study before then that did that. And of non-econometric discussions, there may be no more than Samuelson and Solow and Friedman (1966). But after 1968 it became very common to use the expression interchangeably as a price change or wage change equation. Surely Friedman's usage is the explanation; and surely the change in usage is what brought the interest in the fact that Samuelson and Solow were the first to adopt it. That is probably not very interesting in itself, but it does suggest that attitudes to Samuelson and Solow were changed by Friedman's presentation and it is then perhaps not too much to think that the slightly greater tendency to see them as having initiated a discussion about tradeoffs also flows from hunting around for someone who might have done so.
On this basis, the fact that it is in 1977 that we see the first clear claims that Samuelson and Solow adopted a tradeoff view is I suppose attributable to the second wave of Friedman's argument. By 1977 I think it is fair to say that many people were persuaded that the 1960s had be a period when other people were persuaded of the exploitability of the Phillips curve. That, I suspect, caused some to read Phillips, and of course they could not find this argument. But enough conviction that 'everyone' believed it would force one to suppose that someone else had presented it that way. Later still, once it had become reasonably common to attribute this interpretation to Samuelson and Solow, others noticed that they did not say it either. But those people had some reason to believe that others had interpreted Samuelson and Solow as advocating inflationary policy, so the truth becomes more deeply buried.

No doubt one is inclined to say that since the things believed about Samuelson and Solow are so plainly false, it is a difficult matter to see why people would start believing them right then in 1977, or at any other time. But there is perhaps a comparison with my argument in Forder (2010b). There I presented a good number of statements of the so-called 'Friedman-Phelps' argument on expectations which pre-dated those authors' contributions – and there were many others I could have

presented. Many prominent authors wrote the argument in many prominent places, all through the post-War period (and before). Phelps stated that it was not new, and that is clear enough in Friedman too, if one reads between the lines. My conclusion was that it is simply impossible to believe that macroeconomists of the time were unaware of it, or surprised by it when Friedman made the argument. Yet the books all say it was new in about 1967 – and not just new, but a great insight too.

In that paper, I suggested that amnesia about the everyday nature of the argument served a useful purpose in helping to make it possible to believe the myth of the Phillips curve. Were it accepted – as it should be, of course – that everyone knew about the 'expectations critique' all through the 1960s, it would be impossible to believe that that was the period of a naïve 'exploiting' of the Phillips curve. To believe the myth of the Phillips curve, one must also believe that the expectations argument originates late in the 1960s. Well, to believe in the myth of the Phillips curve it helps to be able to point to some people who adopted the naïve view too. Either the view that Samuelson and Solow themselves adopted it, or the view that others, mistakenly or otherwise, were persuaded by them, is perhaps helpful. So perhaps the same sort of amnesia about the expectations critique can help to explain the role myth gives to Samuelson and Solow.

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[1] For example Humphrey (1998), writing what he called the history of 'the cost push fallacy' takes the view that the idea was just a piece of foolishness. Strangely he does not discuss the versions of the 1950 and 1960s. Earlier critics of the cost push view, such as Selden (1959) or Paish (1962/1966) chapter 17 rejected cost push explanations on the basis that it was a mistake to say that demand had not been excessive, but they show no sign of regarding the arguments as ridiculous in the way Humphrey suggests. In later versions of the cost push theory, sometimes called 'sociological' explanations of inflation, it was doubted whether demand restraint could stop inflation, or suggested that a serious attempt to do so would destabilize democratic institutions. Goldthorpe (1978) for example, took this view.

[2] Lesson's representations of their argument warrant further comment. In Leeson (1997) p143 he quotes them as follows ('…' in the original Leeson):

> "Our own view will by now have become evident. When we translate the Phillips' diagram showing the American pattern of wage increases against degrees of unemployment into a related diagram showing the different levels of unemployment that would be 'needed' for each degree of price level change

> . . . this shows the menu of choice between different degrees of unemployment and price stability as roughly estimated from the last twenty-five years of American data"

The impression that 'our own view' is principally about the existence of a tradeoff is clear. The words omitted by '…' are those quoted above. Leeson's continuation 'this shows…' is in fact not part of the text of Samuelson and Solow at all but the caption to their chart (and it should be 'This shows…').

[3] Vanek (1962) and Morag (1962) similarly cite them as authority for the impossibility of achieving price stability and full employment. Modigliani (1963) gave Samuelson and Solow as his example of authors who discussed the 'dilemma'. The orientation of Scitovsky and Scitovsky (1964) p446 is apparent: They said that recent American experience showed 'that with unemployment rates below 5 percent the general price level has continued to rise, and that even with unemployment rates of 5½ to 6½ percent, the rise, while slowed, has not been entirely halted.' They noted in this connection that this indicated that Samuelson and Solow's estimate of the level of unemployment required to stabilize prices had been 'on the low side'. Okun (1965) said 'they present us with a menu of policy choices that will not permit us to have our cake and eat it too…According to their analysis, the simultaneous pursuit of both full employment and price stability creates a very real dilemma.' Eagly (1967) regards it as the outcome of 'Phillips curve analysis' that price stability and full employment are incompatible – citing Samuelson and Solow amongst others. And van Meerhaeghe (1967) refers to them as an example of those who have claimed that price stability and full employment are incompatible objectives.

[4] Ackley (1961) cites Samuelson and Solow, in discussing the existence of cost push inflation although they, along with Phillips (1958) and others, are listed as 'see also' reference and it is not clear what point he takes from them. Hagger (1963) refers to them in connection with the problem of distinguishing cost and demand inflation. Similarly: Klacek (1970), Smith (1970).

[5] Pechman (1960) said that he thought Samuelson and Solow were 'quite right in emphasizing that one of the major causes of inflation may be inflation itself'. They do say that, although one might doubt whether they really gave it much emphasis, but he is surely not drawing the conclusion that their curve is stable. Woods and Ostry (1962), as well as noting that they took the view that the work done on determining whether inflation was cost push or demand pull did not truly distinguish the hypotheses also cited them as authority for the instability of the curve. Burtt (1963) is textbook of labour economics, which notes that Samuelson and Solow said that if unemployment were pushed down to 3%, the consequence would be inflation, and suggested that the issue involved was a matter of value-judgment. However, he went on immediately to assert that the Phillips curve was not stable and particularly to point to the benefits of wage-price 'constraints' created by the government. Kaliski (1964) cited them as authority for the claim that the US curve was unstable, and Sheahan (1967) for the view that more explanatory variables were needed to account for wage change. D'Amours (1972) lists them as one amongst others for authority that there are many reasons to think the curve unstable. Streit (1972) referred to them only in connection with the question of how apparent shifts in the curve were to be explained

and to say that the distance of the curve from the origin was often treated as a measure of market imperfection, again clearly recognising that they had found it to shift (although he could perhaps be understood to feel they nevertheless thought it exploitable).

[6] Laidler (2003) says that Lerner 'gently chides' Samuelson and Solow for *not* recommending high inflation as a means to low unemployment. If that is what he was doing then it clearly supports the view that he did not think they were inflationist. The remarks of Lerner that Laidler has in mind are these: 'One can apply the economic principle of equalizing marginal cost and marginal benefit, indulging in creeping inflation as long as the value of the additional output is greater than the damage from the additional inflation involved. As adjustment is made to the inflation it has to run faster and faster to keep output in the same place. When the damage done by marginal inflation becomes greater than the benefits from the marginal output, we have reached the point where the equalization of marginal social cost with marginal social benefit calls for currency reform.' On the other hand, I suspect he was joking since he continues by saying 'The cycle would then repeat. As long as rising prices and low employment are the only alternatives available, the orgies of inflation and the mornings-after of currency reform and devaluation are just what is prescribed by the sober application of the rational principles of maximization of benefits' and the whole comment, although thoughtful, is written in a light-hearted manner.

[7] Reynolds (1960) doubted there was much to be learned from their graph. Perry (1964), and Perry (1966), described their work as 'preliminary'. Blackburn (1966) – who could easily be read as himself treating the curve as stable – uses Samuelson and Solow as the backdrop to emphasizing the damage done by inflation, and cites them for their estimate of the level of unemployment required to achieve price stability – a policy goal he clearly took seriously. A case that might initially seem to point in a different direction is that of Ross (1966) who said that Samuelson and Solow 'paired' three percent unemployment with a four or five percent price rise. But this was in a piece introducing a conference discussion and in the context of presenting conflicting views of the Phillips relation. It is certainly not an endorsement of Samuelson and Solow and beyond the point that the problem of estimating a Phillips relation is a difficult one, no conclusion is drawn. And Jacoby (1967) seems to advocate 1.5% inflation and 4% unemployment and to suggest that this is achievable, saying Samuelson and Solow suggested it was almost possible. But he regards 1.5% inflation as price stability because of measurement bias in the price index. Eckstein (1968) says their work shows the inadequacy of annual data; John Adams (1968) says they made estimates 'analogous' to Phillips', but goes no further and does not appear to have regarded Phillips as an inflationist; and Lindauer (1968) says that they are concerned with identifying appropriate anti-inflation policies.

[8] Those making this point, without saying much else about the paper include Zarnowitz (1985), Hillier (1986), Richter and Diener (1987), Drobny (1988), Chang (1997), Gruen, Pagan, and Thompson (1999), Ferri (2000), Fitchenbaum (2003), Shepherd and Driver (2003), and Arestis and Sawyer (2006). Pearce and Hoover (1995) note they 'extended' the curve to price inflation and used it to analyse policy, but their main interest is in tracing developments in Samuelson's textbook. Chiarella and Flaschel (2000) went a step further as well as a step too far saying that Samuelson

and Solow *initiated* the view of prices that made them 'simple static markup theory' of wages or costs.

[9] Similarly, Desai (1981), citing Samuelson and Solow says 'The argument was made that by controlling aggregate demand and therefore the level of unemployment, a government (on behalf of society) could choose the rate of inflation it felt was desirable'. Later in the book he is nearer to being right when he says they deployed the curve as 'a tool for fashioning an anti-inflation policy'. Elsewhere he says slightly different things: Desai (1984) gives Samuelson and Solow a special place in deploying the Phillips curve as 'a policy tool in the short run' but notes some of the limitations they saw in their analysis.

[10] Henneberry and Witte (1976) say that the Phillips curve was treated as being stable and Samuelson and Solow is the only example they give, although what they actually say about those authors is that they are an example of an 'attempt' to interpret the curve as a structural relation. Walter Adams (1982), and in exactly the same terms, Adams and Brock (1984), clearly imply Samuelson and Solow thought the tradeoff would be available through the 1960s at least, which is perhaps only an exaggeration. Johnson (1983) said that Keynesian policy was based on belief in a stable tradeoff which was believed to be exploitable, citing Samuelson and Solow (and Lipsey (1965)). Coles and Chen (1990) said that Samuelson and Solow interpreted a particular kind of Phillips curve as offering a policy choice. Spulber (1989) p36, unusually, says that they both treated the curve as stable and suggested reasons it might shift. His reading does not appear close. Schettkat (1992) p3 who has them presenting a 'menu for politicians', although this book is full of citation mistakes of various sorts. Richter (1994) says that Samuelson and Solow, amongst others, thought there was a stable tradeoff because they neglected the Classical point that equilibrium in the labour market depends on the real wage and immediately propounded the 'lesson': 'do not neglect previous theoretical knowledge!' At least one can applaud the sentiment. Turner (1997), discussing the Lucas critique said 'Phillips (1958), Lipsey (1960) and Samuelson and Solow (1960) purportedly found stable relationships' and obviously thought this was all that needed to be said to reveal their error. Mackie (1998) p163 n21 says very clearly and quite wrongly 'The authors complied data from the twenty-five-year period following the Depression to demonstrate the stable relationship similar to that hypothesized by Phillips.

[11] Gittings (1979) notes that Samuelson and Solow thought the curve might move, but the implication of his piece is that they share with Phillips responsibility for the curve being regarded as stable and exploitable. Haberler (1985) discussed their paper, including their notes of caution, but said, 'I think enough has been said to justify the conclusion that the paper by Samuelson and Solow illustrates my point that Keynesian economics is characterized by unconcern about the dangers of inflation and neglect of inflationary expectations'. Rosenbaum and Ugrinsky (1994), discussing policymaking say Samuelson and Solow named the curve and found the tradeoff to be 'within acceptable boundaries' and said that inflation associated with 4% unemployment 'seemed manageable' And they say that when the Kennedy administration chose 4% as an unemployment target this 'came straight from the results of the Samuelson/Solow research on the Phillips curve'.

[12] Kitching (1971) picked out the points that they suggested that low demand might lead to permanently high unemployment and considered the use of incomes policy. Tussing (1975) and Spooner (1978) present them simply as having observed the existence of a policy dilemma, Laidler and Parkin (1975) p753 list them as one amongst 12 others in their text and another 12 in a footnote as 'seminal' contributions to the study of wage inflation. R A Gordon (1975) similarly discusses their work amongst others on the American Phillips curve, expressing a good deal of skepticism and describing their work as 'impressionistic'. Eckstein and Girola (1978) similarly list them as previous analysts of wage determination. Olson (1975) found the main interest in Samuelson and Solow's analysis of the controversy over cost push and demand pull inflation. Brunner and Meltzer (1976) is something of an oddity. They clearly believe that the existence of a tradeoff had been accepted, but indicate that they do not know how that happened, and they note that Samuelson and Solow thought policy might move the curve. Paldam (1978) says they say the curve moves. Backhouse (1985) says Samuelson and Solow examined the implications of the Phillips curve for anti-inflationary policy. Bayer (1990) is not a publication in a mainstream economics journal, and he makes several historical mistakes, but on Samuelson and Solow he is just right, saying that they made some guesses about the possibilities in the immediate future, and noting their prescience in remarking on the difference between the short and long run, and on the importance of structural reform. Meltzer (1998) says they thought the curve unstable, as do Perry and Tobin (2000). Waterman (2002) acknowledges that they recognized the problem about the adjustment of expectations. Ball and Mankiw (2002): 'Even Samuelson and Solow's (1960) classic discussion of the Phillips curve suggested that the short-run menu of inflation-unemployment combinations would likely shift over time', Mankiw (2006) makes the same point but also says that the later literature 'forgot' the caveats.

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