ISSN 1471-0498

DEPARTMENT OF ECONOMICS

DISCUSSION PAPER SERIES

NINE VIEWS OF THE PHILLIPS CURVE: EIGHT AUTHENTIC AND ONE INAUTHENTIC

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Number 724
September 2014

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Nine views of the Phillips curve: Eight authentic and one inauthentic.[1]

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September 2014

Abstract

There is a widely believed but entirely mythical story to the effect that the discovery of 'the Phillips curve' was, in the 1960s and perhaps later, an inspiration to inflationist policy. The point that this is a myth is argued in Forder, *Macroeconomics and the Phillips curve myth*, OUP 2014. One aspect of the explanation of how that myth came to be widely believed is considered in this paper. It is noted that the expression 'Phillips curve' was applied in a number of quite distinct and inconsistent ways, and as a result there was, by about 1980, an enormous confusion as to what that label meant. This confusion, as well as the multiplicity of possible meanings, it is suggested, made the acceptance of the myth much easier, and is therefore part, although only part, of the story of its acceptance.

JEL: B22, B29

Keywords: Phillips curve, expectations, Phillips curve myth

1. Introduction

Forder (2014) argues that the commonly-told, indeed 'textbook' story of 'the Phillips curve' is a myth. Elements of that myth include the claims that as a result of the work of Phillips (1958), the curve was at the centre of a great deal of attention and debate in the 1960s; that it was widely seen as offering a crucial insight to policymakers; and that it was the inspiration of inflationist policy then and in the 1970s. None of those things is true. Indeed, it is a stretch to say that any of them has an element of truth. They are part of a widely believed fiction and certainly the story taken as a whole has no historical merit.

In this paper I hope to illustrate one aspect of the circumstances which may have helped the creation of that myth. That is that there was a great terminological confusion over the expression 'the Phillips curve'. As I hope to show, that expression had numerous different meanings – *very* different meanings. They are different not just in such mundane matters as whether the curve is taken to be a cost-push or a demand-pull relation, or some other aspect of its theoretical rationale, but in much more fundamental ways. Some of the crucial differences are in the ways in which the idea of the curve is put to work in economic analysis and consequently what presumptions the expression 'the Phillips curve' invokes.

[1] Revised version of a paper presented at the History of Economic Thought Society, Montreal, June 2014. I am grateful for comments by Alessandro Barattieri, Norikazu Takami, Peter Docherty, and Rod O'Donnell.

When the diversity of usage of the expression 'Phillips curve' is made clear, it becomes apparent that any remark about 'the Phillips curve' – such as 'economists believed the Phillips curve showed a menu of policy choice' – is not, without more context, properly interpretable at all.

That confusion in itself is notable, but just as important is the point that it has apparently never been noticed. Even amongst those purporting to survey or assess 'the Phillips curve literature' there is no discussion of this point; nor even do those who have sought to write 'the history of the Phillips curve' advert to it. It is easy to see, I suggest, how an unnoticed multiplicity of usage could create a situation in which many different things might be believed about 'the' Phillips curve, and thereby help to explain how entirely erroneous beliefs could be formed.

2. One inauthentic view of 'the Phillips curve': The Phillips curve myth.

The myth is one story about the Phillips curve that everyone knows. That is the story that Phillips made a striking breakthrough in discovering a negative relationship of wage change and unemployment, that it was quickly interpreted, possibly with the inspiration of Samuelson and Solow (1960), as offering a 'menu of choice' to policymakers, and the policymakers promptly adopted inflationary policy to lower unemployment. Then Friedman (1968) and Phelps (1968) supposedly produced a second breakthrough with the proposition that a continuous inflation would come to be expected and hence incorporated in the wage bargain so that the 'menu' would disappear. After being resisted, their argument was eventually accepted.

All the components of that story are false. It is entirely inauthentic. It is made up. Those components are properly, not pedantically, *false*. That case is made in Forder (2014), with parts of it also in Forder (2010a) and Forder (2010b). Highlights of what I hope is a comprehensive debunking of the Phillips curve myth include (1) the idea of a negative relationship between inflation and unemployment was not only not original to Phillips (as several people have noticed), but nowhere near a new or surprising finding in 1958. Phillips' work certainly did not do anything like 'fill a gap in the Keynesian system', and he taught the Keynesians nothing they wanted to hear; (2) Samuelson and Solow hardly propounded a tradeoff of any kind, but in any case, they influenced no one in an inflationist direction. (3) There was practically no one in the 1960s who thought a policy of inflation could lower unemployment for long, and no one took that lesson from Phillips; (4) The expectations argument was commonplace before 1968. Phelps even more or less said so when he presented his version of it; (5) if Friedman and Phelps had any effect on thinking in this area, it was to cause the expectations argument to be doubted, and to raise the possibility – adopted still only very occasionally – of running a policy of permanent inflation. This whole episode in the history of macroeconomics needs to be reconsidered. But that is not the task here – rather that task is to consider one aspect of the question of how such a string of historical falsehoods came to be adopted as a centrepiece of the post-war history of macroeconomics.

3. Some background: The theory of wages in the early post-war period

To say that the idea of a negative relation of wage change and unemployment was nothing new in 1958 is not to say that the level of unemployment was taken to be the crucial, or even an important determinant of wages. On the contrary, wage setting was understood very much as the outcome of particular institutional circumstances. Specialists in labour economics tended to reject the marginal product theory of wage determination. In this, Richard Lester was perhaps the most vociferous, but far from the only one. These scholars held that in so far as economic theory of the neoclassical sort said anything about wages, it could set limits to how high or low they might be, but within those limits, it had nothing to offer. Rather, insight was to be found by enquiring into management policy, bargaining strategy, the habitual or traditional relativities of various trades or groups of workers, and any other matter seeming to bear closely on the question of the fairness of wage agreements. Three such considerations often suggested were changes in the price level, the profitability of the employer, and changes in labour productivity. Beyond that, a particular wage would be agreed as the result, as it were, of a genuine, human bargain. The argument that this *was* the attitude of the 1940 and 1950s is more fully made, along with enough citations for most tastes in Forder (2013) and Forder (2014) chapter 1. In any case, this is what gives rise to the idea that theory said there was a (reverse) L-shaped supply curve (i.e. that there would be no inflation at less than full employment), or as Friedman (1971/1974) put it, that the price level was an institutional datum. It would be better to say that in considering the determination of the level of employment, wages and the price level were treated as exogenous. There was an L-shaped supply curve in the sense that there was no fundamental reason that the full employment in and of itself should generate a change in the price level.

4. Eight more ideas of the 'Phillips curve'

That is the background. In about 1958, it just so happens, three developments occurred.

(i) The curve of Phillips

One was that Phillips (1958) wrote his famous paper. As is sometimes noted, but with surprising little interest, Phillips used nearly a hundred years of data to derive a relationship between wage change and unemployment. That is a generous statement since the relationship was more fragile than is sometimes suggested, and certainly the data, and Phillips handling of it, left a lot to be desired. But the point that raised interest in 1958 (or might have raised interest, even if what it more often did was cause annoyance), was not that wages and unemployment had a negative relationship. It was that there was in Phillips' work a suggestion that the *same* relationship prevailed over such a long period. That was deep challenge to established theory since it said that all the institutional factors, historical developments, and such things as the creation of

the welfare state and the adoption of a commitment to full employment made no difference to the economics of wage bargaining. Far from wages being the outcome of psychology, historical perception, ideas of fairness, and human bargains, they were, in this startling suggestion, very much subject to something like an economic law of motion. It is not *true* of course, and Phillips did not really convince anyone (including himself) that it was. Further analysis too quickly delivered contrary results. It was in mocking this idea that Routh (1959) became the first to use the expression 'Phillips curve' in print. But the initial point of interest, the striking claim, the thing that made it *interesting*, was the suggestion of the discovery of an economic law, the overturning of accepted theory. That was the first Phillips curve. It was, as perhaps we should now call it, the curve of Phillips.

(ii) The inflation-unemployment dilemma

A second thing that happened at the same time was that serious doubts started to form as to whether the simultaneous achievement of full employment and price stability would be possible. The immediate post-war presumption was that it would. It is notable – although so casually misinterpreted – that the (American) Employment Act of 1946 said nothing about price stability. In the muddled historical thinking around the matter of the Phillips curve in the 1980s and later, this is sometimes taken as implying an acceptance that full employment would mean inflation was inevitable. That makes no sense if it is really going to be said that Phillips' work offered the first intimation of a negative relationship, but it is a mistaken interpretation anyway. Price stability was not mentioned because the proponents of the Employment Act had no notion of challenging its importance. The level of employment and changes in the wage rate were separate matters (or could be separated by elements of wage control and the like). That outlook is not exactly a consequence of the theory giving rise to the L-shaped supply curve, but it clearly does cohere with it. So it should. They were part of the same intellectual framework. So there was no need to state the obvious point that along with full employment, price stability was a goal of policy. (The opponents of full employment policy might raise a worry about price stability, but that would usually have been what made them opponents of it).

But in the United States in 1955 and 1956, the price level crept up despite levels of unemployment that many people thought made it impossible to think there was 'full employment'. The problem of 'creeping inflation' appeared. It posed a problem for policy, but also for theory since it was not clear what explained it. The idea of 'cost-push' inflation was one candidate and there were plenty of other variations. One that attracted particular attention was the idea of 'demand-shift' inflation, derived from Schultze (1959). It arose from the process of adjustment when demand shifted from one sector to another. It made average price rise the consequence of rising prices in expanding sectors, combined with downwardly rigid ones is shrinking sectors. It was therefore consistent with less than full employment, or just-full employment and in the latter case it was even possible to regard it as a symptom of a well-run economy since the maintenance of high employment facilitated the progress which resulted in the shifts of demand from one sector to the other. In that case, there was no real reason to

resist the creep of the price level that then occurred, but many found it very hard to accept the view that inflation should be allowed.

The question that was raised, then, was whether full employment and price stability were compatible. If they were, the experience of the mid-1950s would written off as anomalous. This gave rise to a number of enquiries into the existence of the 'full employment – price stability dilemma'. Bowen (1960a) was a notable contribution. There, and in Bowen (1960b) the author set out to determine precisely whether there was a conflict between the two goals, and if so, what caused it, and how severe a problem was. There is more that might be said about Reuber (1962) than that, but the same question of whether that problem existed underlay his enquiry. Although, perhaps, it became increasingly clear that there *was* a problem – that the dilemma *did* exist – the same kind of questions continued to be asked later in the 1960s with Bodkin (1966) and Levy (1966) being notable examples.

A characteristic of this sub-literature which from a later perspective needs to be emphasized in order to be made sufficiently clear is that the first point of the enquiry was about the possibility of achieving full employment and price stability simultaneously. It is worth noting that, in its historical context, that was not an easy question to answer. There was at the time no powerful theoretical reason to think they were not compatible; but there were grounds to worry – for example that aggressive trade unionism would emerge in response to full employment policy. Nor was there any thoroughly convincing data since the relevant period – that is the period of peace time full employment policy – was so short; and such events as reconversion and the Korean War impaired some of the data that was available. The second point of the enquiry was diagnostic – if price stability and full employment were incompatible, it was important to understand why. Even if he did not really bring much in the way of sharp conclusions, Bowen's theoretical enquiry was very much directed at considering the possible answers to that question.

Supposing, as these authors tended to suggest, that price stability and full employment were not compatible, the next issues were usually about how bad the problem was and what could be done to improve the position. Reuber stands out from the group as taking the possibility of accepting inflation seriously enough to attempt to calculate the optimal rate. Bowen was perhaps not willing to sacrifice much in order to achieve strict price stability, but even there the priority was to find measures to remove the dilemma, and that was very much the general stance in these enquiries.

Of them all, the one that became best known in later times was the one that was the least substantial – Samuelson and Solow (1960). Probably without being aware of Routh's paper, and for no discernable reason, they chose to use the expressions 'Fundamental Phillips relation' and 'Phillips' curve', and even once – just once – 'Phillips curve' in describing the rather dubious relation they observed in American data. Contrary to a great many later assertions, they did not advocate inflation; they did not say the Phillips curve was a stable menu. Most interestingly, I think, no one from the 1960s seems to have both treated

them as advocating inflation and accepted that view (For chapter and verse: Forder (2014) chapter 2). That last point, arising from rather extensive study of the literature ought to kill off the suggestion that they were instrumental in propagating the tradeoff view of the Phillips curve. On the contrary: Theirs was a conference paper and shows plenty of signs of being a little rough and ready. It ranged widely considering the possible causes of inflation in America; noted that there appeared to be no long-enduring relationship of the kind Phillips had found in England. They nevertheless thought there was a problem, and considered solutions to it, and had a go – a rough and ready go – at quantifying it. And they ended, repeating the hope that something could be done to improve the 'American Phillips' curve'.

Here, then, is a second kind of Phillips curve. Nothing like Phillips' enquiry into the deep forces determining wages, it was a vehicle for investigating the achievability of the two central macroeconomic goals of the time. Whether they were simultaneously achievable, how much needed to be done to make them so, and what it was that needed to be done, were the questions this tool was used to address.

(iii) Econometric wage change equations

Around about 1958, as it happens, there was also an increased interest in specifically econometric analyses. I suppose that has something to do with increased computing power and perhaps a development of techniques – or spreading understanding of techniques – that could be employed with that power. No doubt those two things also had some tendency to draw students into doctoral work which took advantage of the new possibilities.

One of the areas ripe for econometric exploration was that of wage change. The earlier theoretical literature – the literature which emphasized employer profit, price change, and productivity as concrete aspects of the wage setting environment – had been descriptive and sometimes made intensive use of statistics. But such things as wage change equations were rarities, and where they can be found, they are tuned to macroeconomic modelling rather than the exploration of the issues raised by the labour market theorists. After 1960, that changed.

Lipsey (1960) is justly regarded as an exemplary piece of work, and in his case, the inspiration of Phillips is clear. He emphasizes unemployment and the change in unemployment as the determinants of wage change, just as Phillips had, and added systematic consideration of price change. The point of Phillips' focus on unemployment, though (and of Lipsey's too, if one reads between the historical lines), was to dismiss the earlier literature. But Lipsey was the exception. Others estimating wage change equations embraced the older literature. Perry (1964) was a notable one, giving attention to the importance of profit in wage change. Kuh (1967) was another sophisticated study, presented by its author specifically as 'an alternative to the Phillips curve', and emphasizing productivity in preference to unemployment (or profit) as a key determinant of wage change. Hines (1964) was another paper very much conceived by its author as a

challenge to the view that unemployment was the crucial determinant of wage change. Studying Britain, Hines thought he could show that shifts in trade union aggressiveness were the drivers of variations in wage settlements. There were plenty of others – almost all drawing on the older labour economics literature, or developing it. (Details: Forder (2014) chapter 3).

In this literature, the terminology of 'the Phillips curve' did creep into wider and wider usage. So by the time of Santomero and Seater (1978) at the latest, there was nothing too incongruous in their including all these papers within their domain as they surveyed 'the Phillips curve literature'. That did, though, conceal the range of these enquiries, and the point that a good number of the papers were conceived as challenges to the centrality of unemployment in wage change.

All this work was concerned with the estimation of equations explaining average wage change. Almost all included price change; after 1965 the vast majority included some measure of the tightness of the labour market; in the more sophisticated studies, something else was usually present as well – profit, productivity, union aggressiveness, or whatever. Practically none owed anything to Phillips in that practically none were concerned with any idea of finding unchanging truths of the economic world rather than understanding some aspect of other of the immediate circumstances of the research being done. Nevertheless, in common usage of economists, we have here, a third kind of Phillips curve – it is any estimated wage change equation, including any number of explanatory variables. That is how the term came to be used, and in that sense, to say that there was a large 'Phillips curve literature' is an authentic use of the expression, and perfectly true.

(iv) The investigation of specific policy proposals

A further development from those econometric estimates is that the idea of a Phillips curve started to appear in discussions of the effectiveness of specific policy proposals. Precursors of the approach would perhaps be Klein and Ball (1959) who actually included a dummy variable in their (British) wage-change equation to investigate the possibility that, because of changed trade union attitudes, wages rose faster during the Conservative government after 1951 than in the Labour government of 1945-51. Perry (1966) is a clearer case, however. He noted that his equation overpredicted wage change after 1962 and suggested that the discrepancy might be explained by the Presidential 'Guideposts' (i.e. 1960s vintage American incomes policy) in that year. Perry (1967) worked out his case more precisely, using a dummy variable for the period of incomes policy, and attracting critical comment from Throop (1969) amongst others. Meanwhile, Jefferson, Sams, and Swann (1968) was one studying British data in the same kind of way. The literature took a different turn with Lipsey and Parkin (1970) who estimated different (British) Phillips curves for periods with incomes policy and those without – the slope was changed, they thought. That made the assessment of incomes policy more difficult than if they produced merely a shift, since the overall effect on inflation would also depend on the level of demand at the time the policy was introduced.

Particularly in Britain the same sort of approach was taken to the assessment of 'regional policy' when the idea of such things as differential tax rules for poorer regions started to feature prominently in policy (as it did after 1967). The central issue was whether lowering unemployment in the high-unemployment regions would make for a better national Phillips curve. Thirlwall (1970) was one such investigation.

These studies take an important step beyond any of those so far considered. Whilst they remain empirical studies, they treat the existence of the Phillips curve – in some form or other – as a maintained hypothesis. The ideas, for example, that incomes policies shift the Phillips curve, and that this can be discovered by including a dummy variable for the years when such policy operates are not propositions worthy of attention unless and until it is decided that the underlying relationship, to which the effect of incomes policy is appended, has been understood. Whether the Phillips curve should have been so treated in the late 1960s is not really the point of this paper but I would note that at that time there was much more debate about it than has later been recognized. Kuh and Hines, although later incorporated as part of the literature, in one usage of the expression 'Phillips curve', were clearly, as of the dates of their papers, disputing whether it existed. And even if it were accepted that it did, the question of exactly which variables should be included was a live one, to say nothing of what parameters they would have. (Indeed Simler and Tella (1968) argued Perry was wrong about the Guideposts precisely by offering a different specification of the underlying inflation unemployment relation). In any case, here is the fourth kind of Phillips curve: A relationship which is *assumed* to exist in order to investigate some other question.

(v) Theorizing the negative relationship between wage change and unemployment

In all that literature, the idea (even if sometimes disputed) that low unemployment would be associated with quickly-rising wages was treated as not much more than common sense. Samuelson (1947/1965) had thought of the speed of price adjustment depending on the degree of inequality of supply and demand; Phillips had said just the same thing about commodities in general before applying the thought to the labour market. Where further theory was advanced, it might be in the form of appealing to the common sense idea that high profits might induce large wage increases, or that union militancy might. The question of the microeconomic analysis of what wage-setting process would give rise to a Phillips curve, however, went more or less unexplored. The nearest approach was the 'demand-shift' argument of Schultze (1959), which was later applied to 'the Phillips curve' using that label by Rees (1970) and Tobin (1972). Later, Rowthorn (1977) suggested a theory which turned on the competing interests of different classes. It is important in the history of the Phillips curve because it combines formal theory, the idea of cost-push inflation, and the terminology of 'the Phillips curve'. But none of these really brought rational-actor economics to the question of why there was a relationship between wage change and unemployment or what microeconomic facts would determine its shape and location.

That theoretical turn came with Lucas and Rapping (1969) and Phelps (1969), and many of the contributors to Phelps (1970b) also addressed it. This work tends to be described as if it were propounding a long run vertical Phillips curve and hence as seeking to show that the 'Phillips curve' as commonly understood could only be a short run phenomenon. A reading of Phelps (1970a) makes it clear what a mistake that is. His view of it was that *of course* the rate of inflation made no difference to equilibrium unemployment. He does not seem to have thought anyone doubted that. The question of interest was why that was not true in the short run. The search theories emerged to address that question and gave the answer that temporary misperceptions of real variables would give rise to labour supply and demand decisions that would not be optimal from a full information perspective. They were to become by far the most noted responses to the issue, but others were possible. Although he was really focusing on other matters, Wachter (1974), for example, considered how the wage setting arrangements within firms could generate a similar outcome.

In these sorts of arguments then, at the end of the 1960s or during the 1970s we see 'the Phillips curve' in yet another role. This time it is – particularly in the hands of Phelps and his collaborators – the object of theoretical investigation. Again, it might well be said that the existence of the basic relationship is a maintained hypothesis. It is certainly not being tested. But the role of the curve is to be the thing explained, not the thing estimated, nor the thing assumed while some other point is considered.

(vi) The description of stagflation

Just as a matter of terminology, the 1970s saw a new departure in the use of the expression 'Phillips curve'. During the 1960s, the expression – although applied to a wide and growing range of ideas – was nearly always a label for a relationship between wage change and unemployment (or something standing in the place of unemployment). One who used it differently – as a label for the relationship between inflation and unemployment – was Friedman (1968). His influence may well have facilitated its wider use in that sense. But more importantly, from perhaps 1969 in the United States, or a little earlier in the United Kingdom, it started to become apparent that the relationship between inflation and unemployment had deteriorated. Outcomes were significantly worse in the later period than they had been in the earlier one.

As conventional history is told, that was a great victory for Friedman and Phelps because they had supposedly 'forecast the breakdown of the Phillips curve'. Well, conventional history is wrong (see Forder (2014)). Neither of them forecast any such thing. But in what happened after 1970, simply as a convenient piece of terminology, so say 'the Phillips curve has shifted' (or 'broken down', 'disappeared', 'deteriorated', etc) was a useful way of summarizing the problem of the times: Inflation and unemployment were both worse than before. An aspect of this usage was that it became commonplace to treat 'the Phillips curve' as the relation between *just* inflation and unemployment. Quite in contrast to the econometric literature of the 1960s, the non-econometric discussions of the 1970s dropped all the additional variables. The thing that had *plainly* shifted,

after all, was the inflation-unemployment relation. That was what was meant when it was said that 'the Phillips curve' had shifted. It in making 'the Phillips curve' – as just a conversational marker – the simple inflation-unemployment relationship, it acquired another usage.

(vii) Explaining the deterioration of inflation-unemployment outcomes

Naturally enough, a great deal of effort went into explaining why that shift occurred. This was a new econometric literature. The variables that had been so important in the 1960s tended not to appear, but others did, so the econometric equations were not simple inflation-unemployment relations. The Wachter paper already mentioned was part of that. Others, like Perry (1970) in the United States, or Taylor (1972) in the United Kingdom, investigated how the changing composition of the labour force might have such an effect. Nordhaus (1972) tested a whole set of suggestions. And there was also testing of the idea that it was all a matter of changing inflation expectations – from Solow (1968) to Gordon (1977) – that was under consideration. In those papers, much attention was given to the problem of measuring expectations, and the extra explanatory variables flowed from that enquiry. In all of these, though, the point was not that there was any surprise that in conditions of high inflation, the labour market adapted to that fact. The literature was motivated by the enquiry into exactly how that adaptation should be understood. The puzzle was not *that* the 'Phillips curve' shifted, but it was to understand exactly why. So here another 'Phillips curve'. It has no particular econometric formulation; it has no particular theoretical rationale. It was merely the thing, the shifting of which was to be econometrically explained.

(viii) Further theoretical departures of the 1970s

Rather in the way that econometric estimates of 'the Phillips curve' became a vehicle for exploring the potential of incomes policy and regional policy in the 1960s, in the 1970s, it was posited as a starting point for further enquiry. Three lines of thinking in particular stand out – all of them principally theoretical.

One concerned European Monetary Union. The Werner Report of 1970 set a timetable for the achievement of EMU and marked one of the highpoints of enthusiasm for that project (although their version came to nothing). A curiosity of their plan was that by 'monetary union' they meant the irrevocable fixing of parities with no band of fluctuation, not the substitution of a single currency for the national currencies. That appeared to leave policy in the hands of national policymakers and since observed inflation rates had been very different, raised the question of how the parities were to be maintained. There is obviously a mass of political economy that might be explored there, but a shortcut to an important issue was to deploy the idea of the Phillips curve. So it was that, for example, Grubel (1970) described one of the issues raised by EMU as that of national economic independence, and in his presentation, that was characterized by the possibility of choosing points on a Phillips curve. Balassa (1973) and others made the same sort of point. The Phillips curve was useful in giving the

analysts of monetary union an easy way to characterize one of the issues, even if, in the process they simplified it all too much.

A second case concerned the idea of the 'political business cycle'. That is most associated with Nordhaus (1975) who hypothesized a Phillips curve which became steeper but not vertical during inflation, vote maximizing governments, and voters who were averse to both inflation and unemployment. The exposition is mainly theoretical and is certainly the source of many conventional ideas on that theme. Nordhaus also thought he had some explanation of cycles in economic activity arising from the timing of expansions being determined by the timing of elections and the length of various lags, but the empirical work was hardly the lasting contribution.

Thirdly, perhaps, one might consider Kydland and Prescott (1977) and their progeny in the form of Barro and Gordon (1983) and the subsequent theorists of 'policy credibility'. Kydland and Prescott adopted the idea of the Phillips curve to set the stage for one of their examples of 'time inconsistency'. Another example concerned warnings that those who build in flood plains will not be rescued when the rain comes. Kydland and Prescott's point was that after the fact, the policymaker may not have the incentives to implement the earlier threat (or promise). Deploying a 'short-run Phillips curve', they suggested that if wages are set on the basis of prices remaining stable, the policymaker has an incentive to create surprise inflation to lower unemployment. As Barro and Gordon handled it, that made it a problem for the policymaker to make an effective commitment to price stability. Consequently – as it would later be put – there could not be a rational expectations equilibrium at zero inflation (Further explication: Forder (2001), Forder (1998)).

In none of these cases is the existence of the Phillips curve, much less any particular specification of it, argued for. In a sense it might be said it is not even asserted. It is merely assumed for the purpose of investigating what would follow if there were such a thing. Very probably, the fact that this was done indicates that – by that time – the existence of the Phillips curve was widely accepted, but it remains the case that the point of the enquiry, in every case, lies elsewhere than in making any point about the existence of the Phillips curve.

So here is a yet another idea of 'the Phillips curve'. It is a relation which is assumed as the starting point for what is mainly further theoretical inquiry, perhaps with incidental testing. All those enquiries raise genuine issues for economics; they all contribute to the corpus of understanding. But they are not like any other Phillips curves – here the curve is a way to get some other theoretical enquiry off the ground.

5. Explaining the Phillips curve myth

So there are nine ways in which 'the Phillips curve' has featured in economists' discussions. In eight of those cases the appropriateness of the use of the label could be questioned in the sense that they were nothing to do with Phillips (1958). One might then argue that there are eight inauthentic ways of using the

label, and it is authentically applied only to the curve of Phillips. The idea that the expression 'Phillips curve' is misapplied has certainly been expressed, although never with much attention to the range of misapplication. But really that makes the point of interest to be one of terminology, rather than one of historical understanding.

The alternative way of looking at it, is that there eight separate discussions up to about 1977 in which the label 'Phillips curve' was in fact applied. They are all actual, attested usages of the term in the economics literature. Then, after about 1975, there is one usage of the expression – the 'tradeoff interpretation' of the Phillips curve myth – which is *said* to have been a usage of the 1960s, but which was not. Those are not just points of terminology, but of history, and of historical importance.

One reason it is of importance is that because of the multiplicity of usage, it is clear that such questions as 'was the Phillips curve a depiction of cost-push or demand-pull inflation?' cannot properly be answered. Was it empirical or theoretical? Was it a tool for policymakers? Was it stable? All those things can be asked of particular treatments. Some people feel a temptation to ask them particularly of the curve in Phillips (1958). That is all very well, but if they go on to apply the conclusions to issues relating to widespread views of the 1960s or 1970s, then they make a serious mistake. None of these questions has proper sense. Nor does 'was there agreement on the existence of the Phillips curve?'

But a more important thing about all this, I think, is that it made for a great deal of confusion. There was no clear meaning attaching to the expression 'Phillips curve' and consequently, nothing much could be said to be definitely true or false of it. Sometimes, it did feature as a 'menu' – but if so it might be to illustrate a supposed problem about monetary union; or as a means of testing the effects of incomes policy; or to measure the extent of damage done by labour market frictions. In none of those cases is it truly a menu from which anyone is advised to choose. But when – as in Phillips' or Lipsey's work – it was part of the science of understanding wage bargaining, it was not a menu at all. Nor was it in the discussions of the 1970s when the issue was why outcomes had become so much worse. True enough, later authors might put all these things in terms of policy choice – but that is later authors, and it is a symptom of a failure to understand the many and varied roles of the curve in the earlier thinking.

A further point is that anyone – very nearly anyone – could find some kind of 'Phillips curve' of which they approved, or could use in analysis, or could fit into a model. While the only kind of curve was a Phillips-Lipsey demonstration of a claim about wage bargaining, it had more enemies than friends. But when it became a depiction of the difficulty of achieving price stability and full employment, many others would express themselves in terms of 'the Phillips curve'. The same is much more true in the 1970s, when 'the Phillips curve broke down' became something that its friends and enemies could agree on. Because of this characteristic, there was a false-consensus on the usefulness of the idea – even if only as a way of talking about the difficulty of finding stable econometric relations. By the mid 1970s, economists of any theoretical outlook could easily

find a basis on which they could say that they accepted the existence of 'the Phillips curve' (cost-push, demand-pull, short-run, depicting the difficulty of achieving full employment and price stability). But this was an outstanding example of concealing substantive disagreement with ambiguous terminology.

And further, it all gives the impression of the centrality of the curve in the thinking of the time. An enormous range of discussions in macroeconomics would feature some sort of Phillips curve. Again, it is only because of the ambiguity of the expression, but that, along with the absence of recognition of the confusion that existed conveys a powerful impression.

And then, in the mid-1970s, stories – made up stories – started to be told about something called 'the' Phillips curve which had supposedly been the basis of inflationary policy (for a precise dating: Forder (2014) chapter 6). It is at that time too that it starts to be said that Samuelson and Solow led economists in that direction. None of the authentic views I have identified is truly one of a policy menu from which policymakers are invited to select an inflationary point. Arguments about the effectiveness of incomes policy are not. Studies of wage bargaining are not that. Theoretical enquiries into the matter of how information limitations might generate enough labour market imperfection to generate the curve certainly are not. Nor are expressions of concern at the outcomes of the 1970s. Even the idea that a problem of monetary union is that it might diminish national independence – in any case, perhaps the most artificial of the uses – is not advocacy of inflation. In a way, the nearest is the idea that the curve is a depiction of the problem of achieving price stability and full employment. Many economists were prepared to accept very low rates of inflation for reasons along the lines of the Schultze 'demand-shift' argument. Indeed, but that does not mean they were making the mistakes of the Phillips curve myth (or if it does, nothing has been learned, since that is still one of the bases of the policy of targeting positive inflation).

On the other hand, I think it probably is true that many of the forms of 'the Phillips curve' were close enough to suggesting an exploitable tradeoff, so that it could later seem that that was what was under discussion. One example: In relation to incomes policy the curve was really used to test their effect. But it is easy to imagine that the idea was to 'improve the menu' but so that a better choice could be made – with the point of *making* a choice then being central. It is much closer to the truth to say that the point was to find out whether incomes policy could lower inflation. It is a profoundly different attitude of mind, although the expression of the ideas might sound rather similar.

So when those made up stories started to be told, I suppose the absence of anything that was clearly and properly *the* Phillips curve, the fact that something going by that name certainly appeared to be central to macroeconomic discussion, and the fact that some of the things Phillips curves were used to discuss were not all that far from involving a choice to accept inflation made it much easier to accept them. There is more to it than that; both in terms of the genesis of the Phillips curve myth, and in terms of its acceptance – on which,

Forder (2014) chapter 7 – but the inconsistent use of the terminology can perhaps be seen as part of it.

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