Reconsidering the L-shaped aggregate supply curve

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The observation that the 'L-shaped aggregate supply curve' once featured in the simplest Keynesian models is scarcely made, I think, except with a view to emphasizing what a primitive idea it was. In those models the price level would be unaffected by changes in demand while there was any unemployment, but at full employment, increases in demand would have no effect beyond inflation.

It was clear, of course, that in fact neither wages nor prices were necessarily stable below full employment. One does not need to look to stagflation to see how the problem was appreciated – there are numerous, often anxious, contemporary discussions of the inflationary problems of the 1950s. Chandler (1951) treated it at length; Brown (1955) studied the inflation of 1939 to 1951 and called it 'The Great Inflation'. It was for him (page v) 'one of the greatest, if not the greatest' inflation in history and was driven by what he called 'the inflationary processes which had been going on since 1939, and were still very much in progress' at the time of his study. He clearly did not think that over full employment was the extent of the explanation. When Burns (1951) said 'inflation does not wait for full employment' it would not have been one of his controversial claims.

Consequently it is no surprise to find that Lipsey (1966) – the second UK edition of his textbook – called the L-shaped approach (p702) 'a simple theory' which was 'commonly used in elementary macro-economics'. He also thought it should be rejected. As Friedman (1974) and Lipsey (1978) both saw the issue, these Keynesian models failed to relate prices and employment in a reasonable way and therefore a further equation was needed to 'close the model'. They parted company on the appropriate way to do that but in the form in which Friedman (1977) developed his position, they shared the view that Keynesian economics in fact came to treat the Phillips curve as providing this 'missing equation'.

Particularly in Friedman's hands, that story was continued with a narrative to the effect that the Phillips curve came to be seen as offering a 'menu' to the policymakers and thereby came to be the basis on which 'Keynesian' policy became inflationary. As elaborated by others, the finger is often pointed specifically at Samuelson and Solow (1960) as the originators of this interpretation, so that, for example, Lipsey (1978) thought they had given a lead in this, and even he, despite earlier views, had followed it in Lipsey (1965).

Sargent (1999) p2 says

> '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 policymakers.'

As Laidler (2003) p22 has quite rightly noted, citing De Long (1997) and Mayer (1998),

> 'The trouble with this story is that Samuelson and Solow (1960) did not urge raising inflation to reduce unemployment, and that policy-makers had begun to follow no such recommendation by 1970.'

Focussing more on doctrinal authenticity than policy choice, Harcourt (2000) p305 called it 'an intellectual and political disaster' that any such trade-off came to be identified with Keynesianism.

But if one recognizes, as I suggest in Forder (2007a) one should, the fictitious character of what might be called 'Phillips curve Keynesianism', that does nothing to breath plausibility into the L-shaped curve. The Keynesian establishment certainly did not believe they faced a stable tradeoff, but did they then believe – in the face of the evidence – that inflation could not be a problem below full employment?

The answer, I shall suggest, is this. The idea that there was a model containing an L-shaped supply curve in the 1940s and 1950s is a later reconstruction. There is almost no reference to any such thing from the time. Having said that, if it is properly understood, then it is an eminently reasonable reconstruction. The crucial point about it was that the problems of inflation and unemployment were treated as being separate problems.

Rather, the analysis of wage determination turned on the understanding of a wide range of factors affecting wage-setting practices and bargaining relations. In this analysis, 'theory' in the sense of an all-encompassing, comprehensive theory, was eschewed and emphasis was placed on the necessity of understanding actual cases and building a picture from them. The result was that the efficacy of analysis based on any ordinary appreciation of 'supply and demand' was denied, and attention was focussed on the sociology of unionism, political preference, the perception of moral imperatives, bargaining skill, and like matters, none of which, at the time, were treated as being ultimately susceptible to reduction to maximizing behaviour. A notable omission – albeit not quite a complete omission – from most lists of relevant factors, however, was the level of unemployment – either overall unemployment or industry-specific unemployment. There was, therefore, no equation to link wage change with unemployment because the understanding of the determination of wage change – at least within the contemplated range of outcomes – gave no role to unemployment. Hence the L-shaped curve.

So ingrained today in – I think – almost all economists, is the idea that there is in some sense a tradeoff between inflation and unemployment that it can be difficult to see how there might not be. One approach would that of Sargent and Wallace (1975) – rational expectations and price flexibility eliminate the tradeoff. From a different perspective, Arestis and Sawyer (2009) do challenge the utility of the Phillips curve as an analytical tool, whilst not altogether denying the existence of a tradeoff. And their presumption that the Phillips curve is at the heart of most macroeconomics is apparent. But more often, I think, even those who would not follow Friedman far down the road to the natural rate of unemployment have sought alternative ways of rationalizing the Phillips curve, rather than ways of denying its existence – Tobin (1972), Desai (1975), and Rowthorn (1977) being three notable cases. And although the facts have forced everyone to admit that if there is a Phillips curve, something makes it shift, it still seems that confidence in the utility of the underlying relation is undented. Indeed, Ball and Mankiw (2002) p117 apparently felt some quite strong language was appropriate in treating doubters on the point. 'Skeptics', they said,

> 'are sometimes tempted to use the shifting Phillips curve as evidence to deny the existence of a short-run tradeoff. This is pure sophistry. It would be like observing that the United States has more consumption and investment than does India to deny that society faces a tradeoff between consumption and investment. The situation is not hard to understand and, in fact, arises frequently in economics. At any point in time, society faces a tradeoff, but the tradeoff changes over time.'

But an inspection of the labour economics literature of the 1940s and 1950s shows that it is possible to treat wage determination and unemployment quite separately and the recognition of the value in those thoughts might suggest that there is a simple response to the Phillips curve. That is not to try to accommodate it within a wider framework, not to try to rationalize it in terms more realistic than Friedman's, but simply to reject it.

Two separate strands of thinking about wages can be found in the immediate postwar period. One of these consists of reactions to the difficulties that might be caused by trade unions seeking to raise wages in a context where the government had effectively guaranteed that policy would secure full employment. It was easy to see – and no very precisely worked out theory was usually thought necessary – that one likely result was continuous inflation. Some of those who considered this problem were amongst the most committed to the Keynesian revolution – Robinson (1937) is perhaps the quickest off the mark, but the centrality of the thought in early practical Keynesianism, at least in the United Kingdom, must be evident from the lengthy discussion of the matter in Beveridge (1945). Others were of an altogether different disposition. Unionism was abhorrent to Henry Simons, but he was no friend of full employment policy and feared a great inflation. Demand management was abhorrent to Viner (1950) who said certain Keynesians 'took a good look' at the problem 'and run away'. Others saw the problem but felt it could be fairly readily addressed. Fellner (1946) thought occasional and slight increases in unemployment would control it; some thought that unions could be induced to see their own interest in restraining wage demand; some thought that price control would be necessary; and some, like Slichter (1948) thought public opinion could be brought to bear to encourage employers to resist wage demands. No one, surely, missed the basic point that full-employment policy changed the terms of wage bargaining.

There was, however, another strand of thought about the relation of unions and wage setting which was principally the preserve of specialist labour economists, and which, amongst many other things, presumed and emphasized the causal importance of the process and character of bargaining in wage setting. The roots of this literature lay in the work of Alfred Marshall and the Webbs, and perhaps in Davidson (1898), and it was firmly based on detailed empirical work in studying the behaviour of actual unions and particular bargains. It is sometimes said to have been a branch of economics lacking an adequate appreciation of theory. That is hardly true, but many of the leaders of the field were thoroughly sceptical of conventional theory as they were of the possibility of developing anything better without detailed factual knowledge.

In particular they rejected the view that there was any piece of economic theory which could provide more than general guidance as to the level of wages. Numerous theories had been offered and it was fairly common practice for discussions of wages to recite them and the reasons for their rejection. For example, a key figure in the field in the immediate postwar period was Richard Lester. In his 1941 textbook he offered a 'parade of theories' – but for the most part they were paraded for the purpose of ridicule. He considered 'subsistence theories' – 'a group of nationalistic economists, who wrote in England and France between 1630 and 1775, propounded some false notions about wages …'; moving on he shortly observed, 'The fallacies of the wage-fund theory are so apparent that it is surprising the doctrine was widely and enthusiastically preached in England and America for half a century'; 'Marx's wage theory is subject to a number of objections…' and so it went on.

None of those theories had much currency by 1940, but Lester reserved his greatest ire for one that did, namely what he called 'the marginal productivity theory' of wages. That theory had certainly been presented as a solution to the important problem, for example by Clark (1899), who is usually regarded as the originator of the theory in fully developed form. He showed no doubt as to its value when he began the preface to his book saying

> 'It is the purpose of this work to show that the distribution of the income of society is controlled by a natural law, and that this law, if it worked without friction would give to every agent of production the amount of wealth which that agent creates. However wages may be adjusted by bargains freely made between individual men, the rate of pay that result from such transactions tend, it is here claimed, to equal that part of the product of industry which is traceable to the labor itself'

The theory had been controversial ever since, but Lester's attack on it, which presaged his better known controversy with Fritz Machlup and George Stigler could well be regarded as being based in good part on a refusal to understand the idea of economic theory. He gave a basic textbook account of the theory as requiring, inter alia homogeneity of labour, perfect mobility of factors and the factors have 'infinite continuity' so that one factor can be 'readily and completely substituted for any other'. There was no overt indication that he was giving a simplified version of the argument, and when he rather grudgingly admitted that the proponents of the theory knew the assumptions were not literally true, he alleged that this had led them to 'modify' the theory. But still Lester denied it any value, even in its modified form.

Lester's presentation was unreasonable, and it is hardly a wonder that Stigler (1947) threw some of it back at him even though the arguments that started the famous 'marginalist controversy' – contained in Lester (1946) – were far more level-headed. There, he presented the results of a survey of business attitudes which, he said, showed that the marginalist presumptions did not in fact animate business decisions and in particular that wage change did not give rise to much in the way of the predicted changes in employment. Machlup's response is principally remembered for his emphatic denial that it matters what businessmen say they are doing or what they say about the reasons for the decisions. They were put firmly in the position of white mice, to be observed, not interviewed.

One common characteristic of accounts of the debate is the emphasis given to the point that Machlup, unrelenting as his attack on Lester was, also incorporated into the debate the issue of whether firm's output pricing decisions were adequately captured by marginalist analysis – a proposition which had been denied by Hall and Hitch (1939), was again by Andrews (1949). As the debate developed, and as it has been recalled, for example by Mongin (1997) or Lee and Irving-Lessmann (1992), these arguments about industrial pricing emerged as the crucial questions. For the most part, the marginalists have been regarded as having much the better of the debate although the strengths of the alternative position are emphasized particularly by Lee (1998). But it should be noted that there are important respects in which any argument over the marginal product theory of wages ought to be distinguished from an argument about output prices.

One, at the most sentimental end of the spectrum, is the long-standing view that labour is special because it is inseparable from its human provider and can hardly avoid being conditioned by its provider's attitudes, whereas, as Eichner (1985) p79 put it, a barrel of oil cares nothing whether it is used to 'heat a house of God or a house of prostitution'. But more than this, there can have been few societies altogether bereft of recognition of the special moral status that labour-as-a-person gives to labour-as-a-factor of production. When this combines with the view that labour's bargaining position in the modern economy is usually a weak one, it can only give rise to an appreciation of the moral limitations of any ruthless, unfettered capitalism. That, of course, is an appreciation that might be widely shared, not just by workers themselves and outside the narrowest confines of economic theory might well be seen to affect the minds of employers as well.

But at the other end of the spectrum, there is a question as to the analytical possibility of the marginal productivity theory being correct. Those who advocated the marginalist view tended also to emphasize its 'gravitational pull' rather than instantaneous accuracy. But the law of gravitation is universal and if marginal product sets the payment of labour, one would expect it to apply to the other factors too. This however raises the question of whether such marginal payments will add up exactly to exhaust the value of the product.

The origins of the recognition of this problem are often traced to Wicksteed (1894) but it was notably, if enigmatically, discussed by Robinson (1934). The factor shares will add-up in the appropriate way only under conditions of constant returns and profit maximization – in other words, roughly, only under perfect competition. It is evident that this problem cannot be addressed along the lines of Machlup since the question is not whether one kind of behaviour really amounts to conformity to a certain theory or not. Rather it is a question of whether it is even arithmetically possible for the postulates to the theory to be correct. It is curious that this point is so rarely noted by the antimarginalists amongst the labour economists. Perhaps their fact-driven analysis would have made the idea of treating all factors in a parallel way an abstraction too far; and they were perhaps disinclined to give house-room to mathematical considerations even when they supported their position. But even so it is probably not too much to suppose that in a general way it gave strength to their position.

But there were other arguments that were certainly deployed. Nor were the only arguments against the marginalist theory of wages. Others brought to the question a much more theoretical turn of mind than Lester. Handsaker and Douglas (1937) and (1938) presented evidence that factor shares did not conform to the Cobb-Douglas production function with payments equal to marginal products. It seemed to them better to suppose increasing returns to scale. In that case the sum of marginal products would be greater than the value of output. Reder (1947) recognized that the marginal productivity theory of factor payments rested on the assumption of profit maximization and suggested a number of reasons that firms not in perfect competition might not maximize profit. He does not advert to the adding up problem and the propositions he makes have no great originality but the general trend of the paper clearly puts him on the side of the anti-marginalists. Reynolds (1948) made an argument flow from the non-marginalist view of firms' pricing decisions. And Bronfenbrenner (1950) offered a collection of theoretical arguments tending to call into question the view that wages would normally equal marginal product.

One highly pertinent fact was that numerous studies found that equivalent labour performing the same tasks in a given locality would tend to be paid different amounts. This had been an observation made by the American War Labor Board when, with a view to arranging for uncontroversial wages set itself to discover 'the going rate' for various types of labour. Postwar studies found the same thing and Kerr (1950) p280 thought there was 'abundant evidence' on this point, even though he was writing before the publication of Reynolds (1951) which the later literature tended to regard as the locus classicus for this view.

What is more, many of the authors concerned sought to test further hypotheses that would clarify the debate. In particular, they noted that other characteristics of the employment contract tended to be more favourable to the worker in the same firms as were paying higher wages. It was therefore not, as theory suggested, that there was an equalization of 'net benefits' of various employment. They also addressed the question of whether such differences in wages tended to narrow with time and frequently found that they did not. If the defence of the marginalist propositions was to rest not on their literal truth but on their 'gravitational pull', a convergence of such wage rates should certainly have been observed. Reynolds and Taft (1956) noted that unionization tended to move wage structures towards that of the marginalist paradigm, clearly posing a challenge to conventional theory. There is more in other words – much more – to the claim that the labour economists based their conclusions on observation of actual behaviour than merely the point that businessmen do not articulate the reasons for their decisions in marginalist vocabulary.

Finally, the point which is, strictly, the relevant one here, is that labour economists of the 1950s clearly did not accept the marginal product theory of wages. That should already be clear in the case of Lester, but the same point was made – more moderately – again and again by others.

Dunlop (1957/1966) p10 was much more sympathetic but still rejected it saying, in the course of an account of the historical development of theory,

> 'The marginal productivity analysis of distribution remains at the heart of distribution theory, but the theory of wage rate determination erected at the same time has involved a retreat to a position largely outside economic theory… Like the wages fund doctrine before it, marginal productivity, including historically related supply notions, was not displaced by an alternative or competing theory. Its popularity has declined because of dissatisfaction with it as a tool of analysis.'

Reynolds (1949) was presumably making fun of Lester when he said (p vii)

> 'the customary practice is to build up marginal productivity analysis to straw-man proportions, so that it can later be demolished amid general rejoicing. This seems to me a waste of time and printing. On the principle that to ignore is to condemn I have left marginal analysis out of the discussion except at points where it served a specific purpose.'

But there were few of them, and on the question at issue, he was with Lester.

It is also notable that one cannot see a tendency for an acceptance of it to grow. Indeed, Pierson (1957) p30, whilst acknowledging the relevance of the theory in the discussion of long-term trends, saw if anything a trend away from its use in practical labour economics, saying

> in the area of partial-equilibrium analysis, theorists are putting less emphasis on the rather simple, almost mechanical view of wage determination based on the hypothesis of pure competition and developed in its most elaborate form in the theory of marginal productivity. Increasing attention is being given to formulations based on the noncompetitive hypothesis, such as bilateral monopoly, and on the interplay between competitive and noncompetitive influences.

Even later still, Hicks (1968) p321, commenting on his early work – Hicks (1932) – described what he called 'that terrible first chapter, entitled "Marginal Productivity and the Demand for Labour"' and clearly rejected the idea.

Thus most labour economists were more moderate than Lester in being able to see the appeal of marginalist thinking. Dickinson (1942) p80 put it well although he was no great defender of the theory, when he said that if managers are in any sense trying to get their money's worth, it must have some value. But the criticisms the labour economists had of it, and their grounds for rejecting it, were also broader and better developed than Lester indicated and none of the leading figures in labour economics seem to have treated it as offering an adequate account.

Instead of all-encompassing theory, what the labour economists of the 1940s and 1950s offered was a vision of wage bargaining where economic theory might set limits to the possible bargain, and would certainly suggest considerations which would feature in the final outcome, but it could not determine it precisely. In the case of non-unionized labour the tendency would have been to suppose that management would set wages within the limits indicated by economic theory, but not necessarily with a view strictly to profit maximization. In the case of unionized labour – which received most of the attention – the process would involve bargaining within the range of possibilities.

The matter was not to be left there, of course. The point was to reach towards an understanding of what it was that determined the outcome of such bargains. Again, the analysis they offered was gave little credence to such formalizations of bargaining as were available but strove for the realistic.

One way that questions of bargaining power might be considered further would be through the development of a formal theory of bargaining. Edgeworth's(1881) had analysed the 'bilateral monopoly' problem, and concluded that within a certain range, the outcome was indeterminate, but others had sought to develop the argument towards determinateness – Zeuthen (1930) and Hicks (1932) introduced different rates of discount for the parties. Pen (1952) and Shackle (1957/1966) moved towards determinacy by considering uncertainty as to the preferences of the other party.

But from the point of view of the labour economists of the 1950s, or even the 1960s when Foldes (1964) and Bishop (1964) developed further the line taken by Zeuthen and Hicks, none of this had much appeal. None of the formal bargaining models pointed to any considerations that had not been considered in the informal discussion, and until very much later, omitted some that had. In particular, in practice little attention was given to risk aversion in bargaining, although it was obviously important. And although labour economists often drew attention to the point that conduct in one negotiation might affect future bargains, the game theoretic treatment of this problem through the idea of 'reputation' was far in the future.

In any case, most of the specialists in the field, if not the theoriaphobics it was sometimes thought, were at least temperamentally ill-disposed towards this kind of abstraction. They would certainly not have found much utility in what later came to be called 'the theory of bargaining' and is exemplified, for example by Binmore and Dasgupta (1987). It is a notable detail in assessing the temper of the times that when Nash (1950) initiated the literature considering the matter in its purest, abstract essentials he took the trouble to make an explicit assumption that the two players in his game had 'equal bargaining skill'. That shows a perception of the limitations of the analysis that would not be shared by all those who followed him, but would be just the kind of thing to convince the experts of the fact-driven world of labour economics that the approach offered nothing to their problems. It is no surprise, then, that the first treatment of wage negotiations as a 'Nash bargaining problem' seems, according to its author, to be de Menil (1971).

One might well add, of course, that, even in its most developed forms, the theory of bargaining runs the risk of constraining analysis to whatever happens to fall within the current bounds of tractability, and that might be only a small part of the relevant considerations. This would surely have been the attitude of the labour economists of the 1940s and 1950s, had they been offered even the game theory from some decades later. And furthermore, the 'determinateness' of solutions was mathematical only – it did not reveal the numerical value of any wage settlement and consequently hardly added to the analysis of the problems receiving attention.

What was needed, then, was a different approach to the analysis of wage bargaining. For Dunlop (1944) p5 it was 'A fundamental tenet' that

> 'modes of behavior that are broader than economic theory contribute materially to the understanding of wage determination… the complex ends of rational activity as well as symbolic conduct cannot be overlooked'

And Lester (1952) p485, said that what was needed to understand wage determination was an exploration of the middle ground between

> 'the narrowly based traditional theory and the all-inclusive confusion of the German historical school or the American institutionalists'

and that

> 'Any theoretical formulation that allows for multiple motivation and includes numerous economic, psychological, political, social, and institutional factors must necessarily be eclectic and unprecise, devoid of simple solutions and subject to zones of indeterminacy'.

His view was that

> 'With the surrounding circumstances subject to wide variation and the possibility that the relevant factors may operate in diverse ways or not function within a certain range, one cannot assign a relative weight to each factor and may not even be able to draw conclusions that are generally valid. Perhaps the most that one can hope for, at least at this stage, is to construct an analytical framework by which to classify and arrange the material …'

So for example, Reynolds (1949) sought merely to list the kinds of factors likely to bear on the wage bargain. He considered a large number but a notable characteristic of his treatment – like that of many others – was how little prominence it gave to unemployment. At the time, it was common to think of 'unemployment in an industry' and one might have expected either that, or overall unemployment to be thought important in wage bargaining. But there is little sign of it being given any prominence.

Reynolds, then, noted that certain factors were always brought into negotiation by one side or the other. These included the cost of living, the employer's ability to pay, and productivity. He also noted that these were in the character of debating points rather than anything that could be taken literally as the determinants of wages since circumstances would determine which side used which points.

He preferred to think of employers as having a minimum and a maximum wage they might pay. The first was determined by the requirement of attracting sufficient labour, and the second by considerations of profitability but not by the strict goal of profit maximization. In an ununionized labour market the wage would then be set by managerial discretion between these limits. In the case of the unionized market – which received far more attention – there was also the issue of what determined the union bargaining position.

He suggested that amongst the important consideration would be the profitability of the employers; and the point that wages were only one issue amongst possibly dozens that were being negotiated, so that agreements about the other points would affect the wage negotiations. He also noted that union leaders would be conscious of the wages being won by other unions and to expect themselves to be judged by their comparative performance.

This of course still left the question of what determined the outcome of the bargain. Reynolds had practically nothing to say about that. Part of the answer to that was that it was to be discovered by detailed study of actual bargaining processes. This made for a large research agenda, which was already underway when Myers (1947) summarized what was needed, saying (p368) that

> 'In this task the economist may well seek the assistance of his colleagues in psychology, sociology, social anthropology, and political science, because the kinds of questions that need to be answered are not solely, or even largely, economic ones.'

And he suggested a long list of questions about the nature of the labour market, the bargaining institutions and the circumstances and motivations of those involved. Many such studies were conducted and whatever the merits of that approach, one practical effect was that they served to reinforce the impression of the complexity and unpredictability of the process.

Another consideration of a slightly different kind was that of the fairness of the bargain. This is again something that would later be subject to attempts to treat it in a much more formal way. One version of that project is exemplified by Akerlof (1982), and another forms the basis of a substantial amount of evolutionary game theory, drawing particular inspiration from Frank (1988). The effectiveness of these approaches is not at issue because they are worlds apart from the treatment of wages in the 1940 and 1950s.

Rather, the point is that perceptions of fairness were causally effective in determining wages, and the perceptions of management and employers were as pertinent in the analysis as the perceptions of workers. Thus, for example, Dunlop (1948) pointed to the importance of established patterns of wages as preventing full or easy adjustment to differential changes in productivity. He suggested the concepts of 'wage contours' and 'wage clusters' as ways of theorizing about these things. Hicks (1955) made the same kind of point, insisting specifically that 'supply and demand' had never been sufficient to determine wages, that ideas of equity played an important role, and that it was 'impossible to make sense of what happens' if it was not accepted that the ideas in question were broadly shared by workers and employers. He, as did others, thought that issues of equity in the wage bargain focussed most naturally on the cost of living, differentials between groups of workers, and the profitability of the employer. Fogarty (1961) even thought that actual bargaining brought wages within range of the level implied by the medieval concept of the 'just wage', and Black (1968) thought the 'social and ethical elements in the wage bargain' were then 'well recognised'.

A notable characteristic of the discussion to this point is the slight role played by any kind of unemployment. Where it did feature in the discussion was in the context of the question of how a wage settlement would affect employment of the members of the union in question. Dunlop (1944) who suggested that it might be reasonable to think of unions as maximizing the total wage bill and that would obviously bring employment into the picture. However Ross (1948) argued, directly in response to this to the effect that unions were in no position to estimate the employer's demand for labour curve so that considerations of the effect of a wage bargain on employment cannot be a major factor in their bargaining position. This issue had been one that many other authors latched onto as a key theoretical dispute but it was a remarkably tame one, with both of the principal protagonists clearly seeing the merit in the other's position.

Shultz and Myers (1950) were critical of Ross and maintained that there were plenty of cases where the employment effects of possible wage bargains operated through the sorts of political channels Ross had in mind so as to influence a wage bargain. But even they summed up against generalizations, saying, (p380)

> 'Generalizations which are meant to apply to all unions, therefore, are difficult to support. This is the essential weakness in the position that the employment effect is of no importance in union wage decisions, or that it is found only in the exceptional cse. The range of environmental situations in which unions operate at any one time or over a period of time is great enough tot permit or impose a variety of pressures and policies, including a consideration of the effect of wage levels of employment in the particular firm or industry

Petshek (1950) was critical of Ross and Reder (1952) thought there was little to be learned from considering the debate in the terms in which the two protagonists put it but clearly thought there was some relevance in employment considerations.

All this is slim pickings, though, by the standards of the debate over the Phillips curve. The argument is about whether employment is relevant, beside all the other factors, in more than exceptional cases. Dunlop and those who sided with him were thoroughly of the mainstream in rejecting the view that economic forces determined a unique wage, and even where employment was accorded a role, it was the employment effects of particular wage bargains – that is to say effects resulting from the employer's loss of business – that were thought relevant. There was no consideration of the general level of employment.

No doubt, if seriously pressed on the point, many of the labour economists would have been prepared to admit that the general level of employment might have some relevance to wage bargaining. But they would have insisted also on the unpredictability of that relation. When the theory of wage bargaining is freed from the shackles of the marginal product theory, wages are seen to be – within some range – indeterminate. That allows all manner of considerations to enter the picture. And questions of national policy and politics, would certainly have been amongst them. There is, on this sort of view, no foolishness in the government asking for wage restraint, and certainly not in pointing to some particular circumstance which demands it. Nor is there any foolishness in the government urging on the trade unions the point that full employment policy will only be sustainable with their co-operation.

Consequently, I suggest that the idea that unemployment might vary over a very wide range with no consistent or predictable effect on wage change forms a sound basis for attributing to these economists the idea of an L-shaped aggregate supply curve.

This general outlook also offers enlightenment on other issues. For one, the hostility with which Phillips (1958) was greeted has sometimes been presented as a puzzle – for example by Lipsey (1978). But Phillips began by asserting the plausibility of supply and demand for labour determining the wage – specifically putting the point as being that labour should be treated like anything else. In so far as his analysis seemed to show that the same relation between unemployment and wage inflation had prevailed for 100 years it was, of course quite obvious that it meant the labour economists must be wrong. It could hardly be that the bargaining relations were unchanged over that period.

It also, I think, suggests a reason that the Phillips curve literature was so slow to include expected inflation in its regressions. The idea that a steady inflation would come to be expected and hence had no effect on real bargains was, as I think I have shown fairly clearly in Forder (2007b), widely appreciated long before Friedman and Phelps made the point. But in the world of labour economics, bargains were very much struck in the here and now. Certainly, recent inflation would be a factor that – as Reynolds said – one side or the other would be sure to introduce into the negotiations. But it would be another thing to suppose that either side would have felt comfortable introducing econometric estimates of future inflation. So there was no reason to think of expectations, as such, as influencing the bargain.

As the Phillips curve literature developed much of it can be seen as trying to incorporate some of the considerations raised in the earlier literature. One important one was firm profitability. Perry (1966) gave great prominence to this, but many others also considered it. A few – notably Hines (1964) – tried to measure union militancy with a view to providing an equation showing that unemployment was not a determinant of wage change. But for the most part, those who took the view that it had no role, like for example, Turner (1958) tended to be averse to econometrics as well. In due course, more and more variables were added to the 'Phillips curve', often with weaker and weaker rationales. The resultant equations were not really 'Phillips curves' at all because they had departed so far from Phillips' point that unemployment was the crucial determinant of wage change. But more than this, the sense of the earlier discussion was lost – at least lost to those inclined to view econometrics as the way empirical matters were settled. In due course, Archibald (1969) labelled such things 'intruder' variables because they were inconsistent with the view that supply and demand determined wages – whatever effect the intruders had, it should be via their effect on supply and demand, not independent.

Finally, it must be noted that in the half century over which the Phillips curve has been estimated, and despite the undoubted progress in econometric understanding, and computing power, and the considerable expansion of the data set, it cannot be said that the Phillips curve estimation industry has been conspicuously successful.

Galbraith (1997) noted that estimates of the NAIRU seemed to change according to what happened to actual unemployment. Quite so – even to the extent of identifying the level of unemployment which will stabilize inflation, much less the whole shape of the curve, the project was yet to bear fruit. Twelve years later it is hard to see what has changed – except that some countries experience another decade of falling unemployment without inflationary pressure.

Is it really sophistry to suggest that macroeconomics might be better off without the Phillips curve?

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