1994

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Citation of this paper:
RESEARCH REPORT 9417

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September 1994

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THE EMERGENCE OF THE PHILLIPS CURVE AS A POLICY MENU*

by

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*Paper given at a conference in honour of Richard G. Lipsey held in Vancouver, B.C., September 9-10, 1994. I am indebted to Ronald Bodkin, Milton Friedman, Peter Howitt, Robert Leeson, Richard Lipsey, John Palmer, Neil Quigley, Grant Reuber and Nathan Sussman for comments, and to Toni Gravelle for research assistance. The usual disclaimers apply. The financial support of the SSHRCC is gratefully acknowledged.
INTRODUCTION

"It takes a heap of Harberger triangles to fill an Okun gap". Thus in about 1977 did James Tobin summarise then conventional (though already controversial) "Keynesian" views about inflation and unemployment as objectives of macroeconomic policy. His 1971 Presidential Address to the American Economic Association (Tobin 1972) had surveyed those same ideas at greater length, and they are still sufficiently well known to require only the briefest description here. First among them was a positive belief in a long-term trade-off between inflation and unemployment; and second, there were judgements, involving both positive and normative considerations, about how to measure the marginal social costs associated with the two phenomena. Those stemming from inflation could be estimated by calculating increases in the area of welfare triangles under a demand for money function as nominal interest rates rose, and those created by unemployment could be represented by the output losses associated with increases in that variable.

The notion of an inflation-unemployment trade-off has a long pre-history in economic thought, as readers of Thomas Humphrey (1993, part V) will be aware, but the fact that the trade-off in question is usually known as "the Phillips curve" provides evidence enough that it did not attract systematic attention until 1958, when Bill Phillips published his celebrated *Economica* paper documenting its apparent existence in close to a century of British data. That article was, in Tobin's opinion, "... probably the most influential macro-economic paper of the last quarter century" (Tobin, 1972, p.4, fn.2). At the same time, however, Tobin also pointed out that "Phillips himself is not a prophet of the doctrine associated with his curve ... [he] simply presented some striking empirical findings..." (Tobin 1972, p.4, fn 2). This fact was, of course, well understood in 1972, as it was that Richard Lipsey's important (1960) extension of Phillips work was directed at strengthening its theoretical foundations and drawing out its lessons for then ongoing debates about
the nature of inflation, rather than at working out its policy implications. (cf. Tobin 1972, p.9).

If Phillips and Lipsey were not responsible for the emergence of "The Phillips curve as a menu for policy choice", as Albert Rees called it in (1970), who was? And who attached that particular set of prices - Harberger triangles and the Okun gap - to the items on it? The usual response to such queries is that we should look to Samuelson and Solow (1960), and the work of the Kennedy Administration’s Council of Economic Advisors. Robert Leeson (1993) in particular has argued that the influence wielded by the policy trade-off interpretation of the issue in the United States must be understood in the context of the 1960 Kennedy campaign, and the role played by "Keynesian" full employment ideas, both in that campaign’s platform, and in the subsequent policies of the Kennedy administration. But, though there certainly was a significant American contribution to the development of the analysis in question, and though economists associated with the Kennedy administration were deeply involved in it, I shall argue below that the idea of the Phillips curve as a policy menu was present, albeit informally, in British discussions in the late 1950s, while there is also an all-important "Canadian connection" in the story which runs through a research study carried out for the Royal Commission on Banking and Finance, otherwise known as the Porter Commission, by Grant Reuber (1962), the pages of the Journal of Political Economy (Reuber 1964) then under Harry Johnson’s editorship, and into Johnson’s own influential writings on inflation and monetary policy (e.g. 1963, 1968, 1970,).

All in all, the evolution of the economic doctrine around which Tobin constructed his 1971 Presidential Address provides an excellent example of the almost accidental complexity which so often (perhaps always) marks the development of economic ideas. The doctrine in question evolved in fits and starts, out of a variety of components, and, as we shall see, the creators of those components sometimes had not the slightest idea or interest in how they were ultimately to be put
PHILLIPS, LIPSEY AND THE RADCLIFFE REPORT

Phillips' (1958) paper is too well known to need any detailed description here. Suffice it to recall that it was almost entirely empirical in character, and that its most striking finding was that post 1948 data on British wage inflation and unemployment lay closer to a curve fitted to similar data for 1867-1913 than had the original observations.¹ On the theoretical reasons for the existence of this relationship Phillips remarked that

"When the demand for a commodity or service is high relatively to the supply of it we expect the price to rise, the rate of price rise being greater the greater the excess demand. Conversely when the demand is low relatively to the supply we expect the price to fall, the rate of fall being greater the greater the deficiency of demand. It seems plausible that this principle should operate as one of the factors determining the rate of change of money wage rates, which are the price of labour services."(Phillips, (1958) p. 245)

And his theoretical arguments as to why the rate of change of unemployment and the rate of price inflation might also appear on the right hand side of the relationship, which need not concern us here, were equally sketchy. As to the implications of his results for policy issues, Phillips noted that

"... if aggregate demand were kept at a value which would sustain a stable level of product prices the associated level of unemployment would be a little under 2 1/2

¹Phillips' paper also contains extensive discussions of the 1913-1948 period, where the relationship was much less well determined. These discussions are not relevant to the topic of this paper, however.
percent ... . If ... demand ... would maintain stable wage rates ... unemployment would be about 5 1/2 percent.

Because of the strong curvature of the fitted relation in the region of low percentage unemployment, there will be a lower average rate of increase of wage rates if unemployment is held constant. . ." (Phillips 1958, p. 255-256)

and that is all. Phillips’ larger body of work was, of course, deeply concerned with policy issues, and his curve first saw print (in output-price-inflation, rather than unemployment-wage-inflation, space) as a dynamic adjustment equation linking output and prices in a model designed to investigate the stabilization properties of various policy feed-back rules. (cf. Phillips 1954). In (1958) Phillips was thus mainly concerned with quantifying one important equation of a rather complicated macro-dynamic system, and he obtained the above-mentioned estimates of what unemployment rates were compatible with price and money-wage stability as one outcome of that effort. During the 1950s, the emergence of inflation in the U.K. had, with increasing frequency, been attributed by some commentators to the maintenance of "over-full employment". Just how far the goal of "full employment" might have to be compromised to attain price stability was, therefore, a much discussed question.²

Even so, the fact that Phillips’ paper contains a mere three sentences about this matter, whose substance is quoted above, is surely evidence enough that contributing to currency policy debates was not his main aim. These three sentences were, nevertheless, positively adventurous in

²Phillip’s answer was, essentially the same as that of his colleague, Frank Paish, who was a leading proponent of the "over-full employment" viewpoint. His views on this matter are to be found set out in the essays collected in Paish (1962). Readers whose professional memories do not extend back to the 1950s will find it difficult to believe that the suggestion that an unemployment rate in the 2 - 2 1/2 per cent range was the best that the British economy could be expected to sustain was a matter of acute and often heated controversy, but it was.
comparison to what Lipsey offered his readers in 1960. Lipsey's paper was seminal to be sure, but not as a source of policy doctrine. Rather, it (a) redid Phillips' empirical work using least squares regression equations, and corrected some details of his conclusions; (b) formalised Phillips' casual theoretical arguments (quoted above) in the process providing us with what seems to have been the first search theoretic analysis of the generation of unemployment; (c) developed an argument, based upon aggregation phenomena, as to why the rate of change of unemployment should appear in a wage inflation equation; and (d) discussed the positive significance of the results generated in the paper for then ongoing debates about "cost-push vs. demand-pull" explanations of inflation.³

Lipsey's (1960) paper was in fact a model, and intended to be a model, of value-free positive economics. On policy, it contained but two remarks. First

"... great caution must be exercised in trying to infer from a statistically fitted relation between W and U what would happen to wage rates if unemployment were held constant at any level for a long time ... [W]e would expect the degree of inequality in its distribution between markets to change substantially. We would thus expect the macro-adjustment function to shift." (Lipsey 1960, p. 475)

Second, after cautioning the reader that "There are at least three very serious problems..." (Lipsey 1960, p. 476) affecting the likely robustness of Phillips' above-mentioned estimates of the unemployment rates likely to be associated with price and money wage stability, it offered the

³Lipsey's major concern was with this controversy. He emphasised that excess demand for labour could be caused either by demand side or supply side factors, but also noted that the fractional coefficient attaching to price inflation in his wage inflation equation would, if further work showed it to be robust, refute the idea that inflation was explicable as the outcome of a non-convergent wage-price spiral. His subsequent work with Max Steuer (Lipsey and Steuer 1961) on the role of profits in the inflationary process also focused on this controversy, while the fullest account in his (1963) Introduction to Positive Economics of his work on inflation-unemployment and inflation-profit interaction occurs in an Appendix entitled "Cost-Push versus Demand-Pull: a Case Study" (pp.431-441) to Chapter 33, "The Theory of the Price Level".
following footnote:

"When policy decisions must be made they have, of course, to be based on the best evidence available at the moment. A premature applications to policy can, however, easily discredit a hypothesis that is potentially very fruitful" (Lipsey, 1960, p.487, fn. 61)

This warning of Lipsey’s might now seem prophetic, given the fate of the Phillips curve in the 1960s and 1970s when it did perhaps exert some influence on policy, but his caution is better understood as a reaction to the intellectual climate in which he was working in 1959-60. The 1950s had seen a growing interest in macro-stabilisation policies, including monetary policies, not only in Britain, but in the U.S. and Canada as well. The emergence of inflation, and accompanying debates about the viability of "full-employment" policies in the U.K., to which I have already alluded, were part of this broader picture. The Radcliffe Committee was set up in 1957 to study the British monetary system and reported in 1959; the Commission on Money and Credit began its work in the United States in 1958; and, the conflict between Governor James Coyne of the Bank of Canada and the Diefenbaker government, which would culminate in the Governor’s resignation in 1961 and the setting up of the Porter Commission, was well under way by the turn of the decade. Lipsey may have been intent on doing positive economics in 1960, but he could not avoid being aware of the discussion of macroeconomic policy goals, and the potential for conflicts among them, that was going on around him.

The Radcliffe Report is particularly relevant at this point in the story. Not only was it the first of the three major reports that appeared during the period under study, but it was in large measure written by Phillips’ and Lipsey’s LSE colleague Professor Richard Sayers who, as editor of Economica, had been a careful reader of Phillips’ paper even before it saw print. Though
balance of payments considerations were among the objectives of monetary policy identified by the Radcliffe Report, the first three items on its "summary list of objectives" were the following.4

"(1) A high and stable level of employment.
(2) Reasonable stability of the internal purchasing power of money.
(3) Steady economic growth and improvement of the standard of living."

(Committee . . . p. 22, para 69)

All this was conventional enough, as was the fact that the Report went on immediately to "... acknowledge that there are serious possibilities of conflict among them." (Committee. . . p. 23, para 70). But the following passage from the Report makes a direct connection between this conventional policy wisdom and the Phillips curve.

"It is sometimes argued that the rate of rise of wage-rates is very closely connected with changes in the percentage of unemployment. But it has also been argued to us . . . that over a significant range of variations in the demand for labour there is a 'band of indeterminacy' within which the precise rate at which wage rates and prices rise . . . have little or no connection with the pressure of demand in that or the preceding year. All that can be asserted as agreed opinion is that, as the fullness of full employment rises, the risk of accentuation of a rise of prices increases." (Committee. . . p.21, para 64, italics added)

The Radcliffe Report gives no references to support the conclusion drawn here, but I have shown elsewhere (Laidler 1989) that it derives from the evidence given to the Committee by Sir Robert Hall on behalf of the Treasury, and by Professor A.J.Brown, whose (1955) book The Great Inflation had anticipated Phillips work in plotting scatter diagrams of wage inflation and unemployment but

4It is worth noting that Lipsey's (1963) textbook, pp. 517-519 identified exactly the same factors as the goals of macroeconomic policy.
had concluded that the relationship in question was a rather loose one.\textsuperscript{5} Also, and crucially in the current context, I established that, though Phillips did not give evidence to the Committee, his (1958) paper had been brought to its attention by Sayers, and was the source of the argument that "...the rate of rise of wage rates is very closely connected with changes in the percentage of unemployment." which the Committee considered and rejected.

Thus, as early as 1959, the potential of the Phillips' curve to define a menu for policy choice had been noted and discussed in an extremely important policy document. That is why Phillips' and Lipsey's care in avoiding policy conclusions should be seen as reflecting more than just the routine caution of academic investigators. It was also, particularly in the case of Lipsey, writing after the \textit{Radcliffe Report}'s publication, a response to a very real possibility that theoretical and empirical results might be applied before their validity had been properly established. In Britain in 1959, that is to say, it was lack of confidence in the empirical robustness of the Phillips curve that denied it a role in the discussion of macroeconomic policy, not any failure to appreciate its potential relevance; and Lipsey's 1960 study, emphasising as it did the curve's theoretical and empirical complexity, should have done nothing to increase that confidence.\textsuperscript{6}

\textsuperscript{5}Leeson (1994) attributes the apparently greater tightness in the relationship found by Phillips partly to his unorthodox estimation techniques, and partly to his "highly unorthodox choice of data sets" (Leeson 1994, p. 16, italics in original). Phillips' estimation methods are described by Wulwick (1989) and his data by Leeson (1994, p. 15-21). These details are not central to the argument of this paper, though they do bear upon the important issue of how strongly the evidence favoured the idea of a stable Phillips curve.

\textsuperscript{6}Even so, in (1963, pp. 521-523) Lipsey did discuss the possibility of paying a price in terms of inflation for attaining lower unemployment. What he conspicuously did not do, however, was suggest that his own, or anyone else's, empirical work on the Phillips curve could provide an estimate of that price. The discussion is cast in rather general terms, with emphasis being laid upon the tentativeness of existing knowledge about the terms of this trade-off, as well as those involving the growth rate. However, see fn. 15 below.
DEVELOPMENTS IN THE UNITED STATES

The discussion of macroeconomic policy trade-offs was already ongoing in Britain when Phillips’ article appeared. The same is true of the United States, where The Commission on Money and Credit, a privately financed group whose members could, according to their Chairman "... fairly be said to represent a consensus of American philosophy and economic judgement today" (Commission ... (1961), Preface, no page number) was created in 1958. The Commission began its work by identifying, with the aid of an advisory panel of distinguished economists as well as a professional research staff, a series of problems to be considered. It then commissioned research studies dealing with each of them. Of particular relevance here are the studies of Lawrence Klein and Ronald Bodkin, who "... were asked to prepare an analysis of the empirical aspects of the trade-offs among the three goals: high-level employment, price stability and economic growth" (Conard et.al. (1964), Foreword, no page number) and Tibor and Anne Scitovsky whose brief was "...to prepare an analysis of the welfare aspects of economic growth, high-level employment, and price stability" (Conard et.al. (1964), Foreword, no page number).

Thus, the Commission’s views on what constituted the proper goals of monetary policy were essentially the same as those of the Radcliffe Committee, and they were, at least at the outset, more willing to entertain the possibility that systematic trade-offs might exist among them; and the American group went beyond their British contemporaries in conceiving of the problems thus presented as being best addressed by studying not only the positive nature of those trade-offs, but also the welfare considerations that might enable the goals to be weighed against one another. Furthermore, this conception of the policy problem as involving the definition of a menu and the pricing of the items on it, seems to have been formulated without reference to, and perhaps before the publication of, Phillips’ (1958) paper, and certainly before the appearance of Samuelson and
Solow's (1960) "Analytical Aspects of Anti-inflation Policy", which is usually credited with having set American discussion of policy trade-offs going in earnest.  

The practice of thinking about systematic trade-offs among macro-policy goals was thus already current in the U.S. before Samuelson and Solow introduced the Phillips curve itself into American discourse and an audience already existed there for their discussion of it as representing such a trade-off. For, careful though they were to warn readers about the limitations of their analysis -

"Aside from the usual warning that these are simply our best guesses we must give another caution ... It would be wrong ... to think that our ... menu that relates observable price and unemployment behavior will maintain its same shape in the longer run. What we do in a policy way during the next few years might cause it to shift in a definite way" (Samuelson and Solow (1960, p. 193)

- they did nevertheless characterise the curve as "a menu", and were perhaps the first to do so. Moreover, they not only followed Phillips in providing estimates of the level of unemployment consistent with price stability - 5 1/2 per cent in the U.S. case - but they also put a price in terms of inflation - 4 1/2 per cent per annum - upon reducing unemployment to 3 per cent. (cf. Samuelson and Solow (1960), p. 192) This latter step was one that Phillips had not taken, and as

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7See Leeson (1993) for a forceful elaboration of this view. It should be noted, however, that Samuelson was a member of the Commission's advisory board, and may well have had a hand in setting its research agenda.

8It is worth noting that the two possible policy effects canvassed by Samuelson and Solow involved first the influence of policy on inflation expectations, and second the influence of persistent low demand pressures on what they termed "structural" unemployment. Thus they may fairly claim to have anticipated, if only briefly, future discussions of both the expectations augmented Phillips curve and labour market hysteresis.
we have seen Lipsey would soon refuse to use his estimates even to compute an unemployment level compatible with price stability.

Samuelson and Solow derived these "best guesses" from what they called a "modified Phillips curve for U.S." which plotted "average price rise (per annum)" against "the unemployment rate" (cf. Samuelson and Solow 1960, p. 192, fig. 2). Apart from remarking that the curve was "roughly estimated from the last twenty-five years of American data" (p. 192, fig. 2) they gave no information about how it had been estimated nor even about the precise data they had employed in their work.\(^9\) It is not too surprising, then, that acceptance in the United States of the Phillips curve as defining a well determined policy trade-off was by no means immediate. When, for example, the Commission on Money and Credit reported in 1961, their conclusions were much the same as those of the Radcliffe Committee had been.

"There is no universally applicable guide indicating the amount of wage and price increase, if any, which is likely to be associated with a specified increase in demand for labor or how much unemployment, if any, is likely to be associated with a particular wage increase."

(Commission . . . (1961), p. 38)

Their caution here, and the disappearance of economic growth from their discussion of conflicting policy goals, was in fact well justified in the light of the research studies they had commissioned. Neither Klein and Bodkin (1964), who paid particularly careful attention to trade-offs involving growth, nor the Scitovskys (1964) had found any systematic relationships among economic growth on the one hand, and inflation and unemployment rates on the other. As to inflation and

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\(^9\)Though "average price rise" presumably differed from "increase in hourly earnings" which appeared on the vertical axis of the only other figure in their paper--a scatter diagram--by some measure of productivity growth.
unemployment, Klein and Bodkin carried out formal econometric tests that yielded estimates of the level of unemployment compatible with price stability (based on post-war data) that were slightly larger than Samuelson and Solow's "best guess". (cf. Klein and Bodkin (1964) p. 394). The Scitovskys' assessment of the matter was essentially the same, based on "The experience of the United States in recent years" (Scitovsky and Scitovsky (1964) p. 446), though they presented neither formal analysis of their own, nor any reference to anyone else's, to substantiate this conclusion.

On the matter of an inflation-unemployment trade-off, Klein and Bodkin simply did not discuss its terms, while the Scitovskys remarked, again without referring to any formal analysis of United States data, that

"In this country, the statistical correlation between wage increases and unemployment rates, though present, is very much less pronounced, [than in the U.K] and especially unemployment rates of less than 5 percent seem to have little or no restraining effect on the upward trend in wages." (Scitovsky and Scitovsky 1964, p. 445)

Thus, though the Commission on Money and Credit had initially conceived of macroeconomic policy choices being made on the basis of a well defined menu, with prices attached to the items on it, the state of empirical knowledge did not, in 1961, enable them to turn that conception into an agenda for practical policy.10

It should also be noted that though the Scitovskys did identify lost output as the major cost

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10 In his doctoral dissertation, mainly written in 1960-61 but not submitted for publication until 1963, and finally published in 1966, Klein's collaborator and student, Ronald Bodkin, did estimate a linear Phillips curve in price-inflation-unemployment space, and use the results to compute a cost (1.76 per cent per annum) of attaining an unemployment rate of 3 per cent in the United States. cf. Bodkin (1966, p. 279).
of unemployment, when they considered the effects of inflation on money holding, they concluded, citing A.J. Brown (1955, p. 23) that

"The evidence shows that the public is willing to pay certainly up to 15 percent interest, perhaps more, for the convenience of holding its accustomed stock of purchasing power in the form of cash ..." (Scitovsky and Scitovsky (1964), p. 450)

They argued, in effect, that, over relevant ranges, the Harberger triangles of which Tobin was to make so much a decade later were non-existent, though they made no explicit reference to the then already existing literature on the welfare analysis of inflation. 11 Though they did discuss the influence of inflation on the efficiency of market mechanisms, and its redistributive effects too, the Scitovskys found little to worry about here either, concluding that

"the public...tends to think of inflation in terms of hyperinflation and all the dire consequences that this entails. This attitude seems the more unreasonable since we have never experienced anything approaching a hyperinflation ..." (Scitovsky and Scitovsky (1964), p. 470)

The January 1962 Economic Report of the President, the work of President Kennedy's newly appointed Council of Economic Advisors consisting of Walter Heller (Chairman), Kermit Gordon, and James Tobin, is justly famous for having set the (Keynesian) policy agenda for the rest of the decade. There are certain things in this Report...relevant to our story: for example, the unemployment rate corresponding to full employment was said to be 4 percent (cf. Economic Report...1962, p.46), well below the Samuelson-Solow "best guess" and the Klein-Bodkin estimates of the unemployment rate compatible with price stability of 5 1/2 per cent or a little more;

11Thus they cited neither the papers of Cagan (1956) nor Bailey (1956).
while carefully worked out estimates of the output losses associated with unemployment - the Okun gap - were also presented - "Each percentage point of progress toward 4 percent in the unemployment rate has meant a gain of roughly 3 percent in total output in postwar periods of expansion" (Economic Report. . . 1962, p. 50).\(^{12}\)

The Report, however, contains no explicit discussion of a systematic trade-off between inflation and unemployment when the latter variable was in the 4 - 5 1/2 per cent range. On the interaction of these variables, the views it expresses are essentially indistinguishable from those of the Radcliffe Committee and the Commission on Money and Credit. It espoused stabilization policy, particularly fiscal policy, as a means of reducing unemployment to 4 per cent:

"... the target for stabilization policy is to eliminate the unemployment which results from inadequate aggregate demand without creating a demand induced inflation. . . 4 percent is a reasonable and prudent full employment target for stabilization policy." (Economic Report. . . 1962, p. 46)

The Report did refer to "the partial conflict which exists between minimum unemployment and certain other national objectives. . . served by stability of the general price level" (Economic Report. . . 1962 p. 44) noting that "There is no precise unemployment rate at which expansion of aggregate demand suddenly ceases to affect unemployment and begins to affect solely the general price level" (Economic Report. . . 1962, p. 47); but it offered no further analysis, qualitative or quantitative, of the nature of "partial conflict" in question or of any trade-off it might present. Rather, the Council

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\(^{12}\)Arthur Okun was a member of the Council staff during 1961, and a consultant thereafter for several years. The 4 per cent unemployment target was, according to Ronald Bodkin, Okun's colleague at Yale from 1962 until 1965, an informal assessment of what might be possible, rather than the outcome of any systematic empirical study of the matter. Bodkin's own best guess at that time had been 3 per cent. See fn. 10 above.
simply attributed its existence to what we would now call "cost-push" forces, so that

"...the policies of business and labour, no less than government, will in large measure
determine whether the 4 percent target can be achieved and perhaps bettered...without
unacceptable inflationary pressure." Economic Report... 1962, p. 48)

The Report therefore discussed, at considerable length, the ways in which the policies of business
and labour could be influenced by wage-price guideposts.

In his writings of the early 1970s, not least his Presidential Address, Tobin would make
much of such guideposts as means of shifting the Phillips curve to the left, and would argue (Tobin
1972, p.17) that the 1962 Economic Report... had considered them for just this purpose. In fact,
the Report justified them in a subtly different, and by then quite old-fashioned, way: it accepted
price stability as an appropriate policy goal, and presented guideposts as a means of reducing the
unemployment rate compatible with it. The fact is that the 1962 Economic Report of the President
made no reference to any belief as precise as that "Phillips curve doctrine implies that lower
unemployment can be purchased at the cost of faster inflation" (Tobin 1972, p.4). Hence it could
not, and did not, contrary to Tobin’s later claim, "...set forth wage and price guideposts...in the
hope of 'talking down' the Phillips curve." (Tobin 1972, p. 17, italics added). It merely attempted
to "talk down" a point defining the unemployment rate compatible with price stability.

The 5th (1961) edition of Paul Samuelson’s Economics did, it is true, contain a discussion
of "the social dilemma" (Samuelson 1961, p. 383) presented by the Phillips curve, which was drawn
in a figure entitled "Cost Push Phenomena" (Samuelson 1961, p. 383). That discussion, moreover,
ended with the question,

"How can a mixed economy, without relying unduly on inefficient direct wage and
price controls, give itself a Phillips curve in which closer approaches to high
employment can be made without engineering a considerable price creep?"

(Samuelson, 1961, p. 384)

This is somewhat closer to the position attributed to the Kennedy Council of Economic Advisors by Tobin (1972), and Samuelson was, of course, closely associated with the Council; indeed his colleague, Robert Solow, was a member of its staff in 1961. Obviously, Samuelson had, by that year, come to regard the Phillips curve as a useful expository device for framing discussion of the inflationary problems that might be consequences of high employment policies. But that was as far as he went. He did not, for example, use the curve as the starting point for even an informal cost-benefit analysis of the consequences of accepting anything other than a zero inflation rate.

In the light of the evidence presented here, it is hard to avoid the conclusion that those American economists who are usually credited with having developed the idea of the Phillips curve as a policy menu had not in fact come to appreciate fully its potential in such a use in the early 1960s.

THE PORTER COMMISSION, REUBER AND JOHNSON

The Porter Commission, more formally known as the Royal Commission on Banking and Finance, began its work in 1961, in the wake of the resignation of Governor James Coyne of the Bank of Canada. Though the Coyne affair had raised important quasi-constitutional issues about the division of responsibilities for the conduct of monetary policy between Parliament and the Bank of Canada, which were of mainly local interest and need not concern us here, a major policy disagreement had lain at its heart: Governor Coyne had given high, some indeed argued sole, priority to the pursuit of price stability at a time when the Diefenbaker Government had been more intent on promoting high employment, and he had defended his policies by arguing, among other
things, that price stability was a prerequisite to sustained economic growth and that there was no conflict between its pursuit and the maintenance of high employment. Thus the same three policy goals, and questions about conflicts among them, that had concerned the Radcliffe Committee and the Commission on Money and Credit were also high on the Porter Commission’s agenda.

The Radcliffe Committee had confined itself to soliciting evidence from interested and informed parties, while the Commission on Money and Credit had devoted an extensive budget to commissioning studies from academic researchers. The Porter Commission took evidence in the British manner, but it also had research studies carried out by its own staff, mainly during the summer of 1962; and that staff was already familiar with the latest British and American research on inflation, unemployment and growth before it began its work.

In particular, Harry Johnson, who had left the University of Manchester for the University of Chicago in 1959, was one of its members. Johnson had given evidence to the Radcliffe Committee (though on the need for better monetary statistics, rather than on anything relevant to the topic of this paper) and he had been in constant informal contact with Lipsey and his LSE colleagues before his departure for the United States through the Board of the Review of Economic Studies and the Oxford-Cambridge-London joint seminar. An early version of Lipsey (1960) had been presented at Manchester where it had received helpful comments from another Canadian, Stephen Kaliski, who in due course set about both refining Lipsey’s analysis and applying it to Canadian data. A preliminary version of his paper (Kaliski 1964), which was ultimately published under Klein’s editorship, was already in circulation in Ottawa by the summer of 1962. Moreover, Johnson had a set of preliminary typescript versions of the Commission on Money and Credit
Studies as early as mid-1961, and these too were available to the Porter Commission staff. There was, that is to say, nothing parochial about the intellectual context within which that staff did its work.

Even so the advances made by the study that particularly concerns us here, Grant Reuber’s "The Objectives of Monetary Policy" written between March and November 1962 were, by any standards, remarkable. Reuber acknowledged Johnson as being "... instrumental in giving the study its ... orientation ... " (Reuber 1962, p.2) That orientation involved developing systematic theoretical and empirical analyses of

"(i) the 'trade-offs' among full employment, price stability and economic growth, given the present structure of the economy and the present complement of policy instruments;

(ii) the trade-offs among these policy objectives in the preference function of the policy-making authorities according to which conflicts among the objectives have in practice been resolved.

(iii) the relative economic benefits to be gained by advancing one objective at the expense of others, given the structural trade-offs among objectives." (Reuber 1962, p.3)

Like Klein and Bodkin (1964), to a preliminary version of which, (attributed to Klein alone, because Bodkin did not assume joint authorship until mid-1962) he made several references, Reuber found "... no evidence of a significant relationship between longer term economic growth...

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13Direct evidence on what papers were available to the Commission’s staff is available in the form of frequent footnote citations in the papers of Reuber (1962) and Johnson and Winder (1962). In the summer of 1961 Johnson employed a graduate student (this author) to prepare abstracts of the Commission of Money and Credit’s research studies.

14Evidently the original intention had been for Reuber to carry out a study of cost-push versus demand-pull theories of inflation.
the degree of price stability or the level of unemployment" (Reuber 1962. p. 10), and he therefore concentrated his discussion of policy trade-offs on inflation and unemployment. Here we encounter analysis whose clarity and thoroughness stand in stark contrast to the casualness, even vagueness, that characterises so much of the immediately preceding and contemporary British and American discussion.

To begin with, Reuber’s theoretical discussion of "The Logic of Economic Policy" (pp.27-34) is focused on a diagram (Figure 1, p. 28) in which a Phillips curve drawn in price-inflation-unemployment space is presented as an "opportunities function" which portrays "consistent and attainable combinations of price change and unemployment" (Reuber 1962, pp. 28-29). And tangent to this Phillips curve is drawn a "preference function" which "represents combinations of the two objectives . . . that are equally acceptable to society." (Reuber 1962, p. 30) The point of tangency between the two curves is then characterised as, "[g]iven the structure of the economy and the policy instruments available, the best combination [of inflation and unemployment] that can be attained" (Reuber 1962, p. 30).\(^\text{15}\) In turn, those empirical aspects of his paper relevant to the current discussion are geared to giving quantitative content to just this qualitative analysis: that is, to estimating a Canadian Phillips curve, to attempting to assess the authorities’ utility function vis-a-vis inflation and unemployment, and to estimating in dollar terms the marginal costs of unemployment.

\(^{15}\)In (1965) Lipsey, who had previously been extremely careful to avoid treating the Phillips curve as a stable policy menu, drew a diagram essentially identical to Reuber’s. However, he made no use of it in the body of his paper, which was mainly devoted to analysing the determinants of structural unemployment in the U.S. Nor did he attempt to quantify the costs of inflation and unemployment. This paper was written in Berkeley during the 1963-64 academic year, after Reuber’s work had been completed, but before the appearance of Reuber (1964) in the Journal of Political Economy. Lipsey does not cite Reuber, had not seen his paper at that time, though he may have been aware of its outlines from conversations with other Canadian economists, for example, Kaliski. I am indebted to Richard Lipsey for discussions of this matter.
and inflation.

A detailed account of all this work is impossible in the space available here.\textsuperscript{16} Suffice it to note first that, with due acknowledgement being given to the work of the U.S. Council of Economic Advisors (as it appeared in evidence presented to the Joint Economic Committee of Congress in February-April 1961, however, rather than in the 1962 \textit{Economic Report of the President}) Reuber measured the marginal costs of unemployment in terms of variations in the associated output gap:

"... the average gain in private output would be some 4 to 5 per cent of private G.D.P. for each 1 per cent [sic] reduction in the percentage of the labour force unemployed up to the point of 'full employment'" (Reuber (1962 p. 17)

and second that he measured the costs of inflation in the light of the following argument:

"A correctly anticipated price inflation avoids the distributive effects of an incorrectly anticipated price inflation since interest rates and all other prices adjust to the expected rate of price change. No such adjustment is likely however, on money holdings. Consequently, a correctly anticipated inflation imposes a tax on holding money. As a result, the community substitutes other assets for money. Since money is virtually costless to produce and these other assets are not, this substitution imposes a loss in efficiency on the economy which can be regarded as the economic cost of price inflation." (Reuber 1962, p.

\textsuperscript{16}In particular I shall not discuss Reuber's efforts to infer the utility function of the Bank of Canada from an empirical estimate of a policy reaction function, other than to note that it suffered from an identification problem of a type later analysed by Douglas Fisher (1970). On the basis of the observation that the Bank of Canada responded more vigorously to a one percentage point change in the inflation rate than to a one percentage point change in unemployment, Reuber concluded that it attached a relatively high weight to controlling inflation. Though there is ample less formal evidence that these had indeed been Governor Coyne's priorities, it is nevertheless the case that Reuber's results were also consistent with the Bank being less concerned with inflation but believing that its policy tools had a relatively small influence on that variable.
Reuber attributed this line of reasoning to a then unpublished mimeo by Armen Alchian and Reuben Kessel, which seems to have been a draft of Kessel and Alchian "The Effects of Inflation" which appeared in the December 1962 issue of the *JPE*.\(^{17}\) He followed it through with empirical estimates based on a demand for money function for Canada estimated by Johnson and John Winder (1962) as part of their research for the Porter Commission and utilizing a formula provided by Johnson. (See Reuber 1962, pp.263-264) With the policy problem thus quantified, Reuber proceeded to find the optimal inflation-unemployment combination for the Canadian economy - 2 1/4 percent unemployment and 3 3/4 percent inflation - and, noting that maintaining a fixed exchange rate on an inflation free United States would mean that "a level of unemployment of 5 per cent might have to be endured" in order to hold the Canadian price level constant, he calculated the net costs of doing so at "about 3.4 per cent of G.N.P." (cf. Reuber 1962 pp.19-20).

In 1962 and 1964, Reuber did not discuss wage-price guideposts as a means of shifting the policy menu, though this matter was dealt with briefly in a later study in which he participated, mainly with regard to U.S. experience, though with much less thoroughness than the Council of Economic Advisors had already brought to bear on the matter. (see Bodkin, Bond, Reuber and Robinson 1966, pp.226 et seq., p.286)).\(^{18}\) It is therefore with regard to this aspect of the development of the Phillips curve as a tool for economic policy analysis that American claims to priority are strongest; for, as I have now shown, Reuber's Porter Commission study clearly

\(^{17}\)Johnson, as editor of the *JPE* would, of course, have had access to this paper prior to its publication.

\(^{18}\)All four authors were members of the Economics Department at the University of Western Ontario, of which Reuber was then chairman.
developed, for the first time, a fully articulated theoretical and empirical treatment of the Phillips curve as a policy menu, with items priced in terms of Okun gaps and Harberger triangles. There is, quite simply, no comparable work in the American literature of that time.

None of the Porter Commission staff studies was published in any orthodox format. They appeared only as bound typescripts and their circulation was rather limited. Reuber, however, almost immediately produced a shortened version of his study which was presented at the Chicago Money Workshop in early 1963 and published in the *Journal of Political Economy* in April 1964. All this obviously had a good deal to do with Harry Johnson, who was running the Chicago workshop that year while Milton Friedman was on leave, and also editing the *JPE*. It is, therefore, no coincidence that Reuber’s framework was soon placed before a wide readership by Johnson in his (1963) "Survey of Theories of Inflation"; this paper contained extensive discussion of the Phillips curve as a policy menu, and pointed out that

"For intelligent policy-making, in principle it is necessary first to be able to quantify the Phillips curve relations among objectives, and second to be able to attach weights or values to numbers along the axes." (Johnson, 1963 p.141)

It then singled out Reuber’s work as "so far the only attempt" (p.142) to carry out this task. And Johnson (e.g. 1968, 1970) continued to draw attention to his work in his subsequent writings on inflation and monetary policy.

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19 It should be noted, however, that there is a further Canadian connection in this literature, for in addition to Bodkin et. al. (1965), the pioneering empirical studies of the effectiveness of guideposts (incomes policies) were Brechling (1966) a paper first read to the Canadian Political Science Association), and Smith.(1968). Lipsey too comes back into the story in this context with Lipsey and Parkin (1970).
ACCELERATIONISM, EXPECTATIONS, AND THE STABILITY OF THE TRADE-OFF

The Phillips curve had a rather short life-span as a tool of policy analysis. The by now well-known story has it that: the accelerationist hypothesis of Friedman (1968) and Phelps (1967) emerged as a theoretical proposition in the late 1960s; the theory underlying the Phillips trade-off was now seen to be misconceived; its proponents had confused money wages with real wages; the curve was not, after all, a structural relationship; its parameters were dependent upon the conduct of policy and were bound to shift the moment attempts to exploit it were put in motion; the upward trend of inflation in the 1970s only served to confirm this theoretical insight; etc., etc.. Like the conventional story of the earlier emergence of the curve as a policy menu, so too is this tale of its demise somewhat oversimplified.

To begin with those who developed the Phillips curve were in fact aware of the money-wage-real-wage distinction, the role of expectations in the inflationary process, and the likely dependence of the Phillips curve’s shape and location upon the conduct of policy, albeit in different degrees depending upon the individual; though they certainly failed to attach to these matters the significance that, with benefit of hindsight, they deserved. We have already seen some evidence of this: recall Lipsey’s warnings about how policy designed to stabilise wage inflation might change the distribution of demand pressures across individual markets and cause the aggregate Phillips curve to shift; and note that one reason given by Samuelson and Solow for postulating a similar outcome was the possibility that "...low-pressure demand would so act upon wage and other expectations as to shift the curve downward in the longer run..." (Samuelson and Solow 1960, p.193)

Such comments and qualifications are in fact rather frequent in the literature we have been discussing. Indeed, Johnson’s (1963) survey paper on inflation, in which he first set out his own
account of the Phillips curve as a policy menu, and drew attention to Reuber's work, warned that "...there are some serious doubts about the applicability of the Phillips curve to the formulation of economic policy. On the one hand, the curve represents only a statistical description of the mechanics of adjustment in the labour market, resting on a simple model of economic dynamics with little general and well-tested monetary and value theory behind it. On the other hand, it describes the behaviour of the labour market in a combination of periods of economic fluctuation and varying rates of inflation, conditions which presumably influenced the behaviour of the labour market itself, so that it may reasonably be doubted whether the curve would continue to hold its shape if an attempt were made by economic policy to pin the economy down to a point on it." (Johnson 1963, pp. 132-133)

Kaliski (1964) is another writer who expressed similar concerns. Having raised the possibility that "... price changes might causally effect wages through their effect on the expectations of parties to the wage bargain" (p.5), he elaborated the point in a footnote:

"The reader might have wondered why money rather than real wages were inserted as a variable into a labour market model. One answer is that it is unwise to commit oneself to a complete absence of money illusion in such a market" (Kaliski 1964, fn. 9 p.5)

This remark of Kaliski's contains the vital clue as to why, in the 1960s, so many economists played down theoretical worries about the likely stability of the Phillips curve. In keeping with then widely accepted methodological priorities, they were unwilling to commit themselves to any particular theoretical position without corroboration from empirical evidence and the weight of the evidence seemed to show that theoretical worries were misplaced. Nowhere is a reliance on econometric results more apparent in the literature I have been dealing with here than in Reuber's treatment of what we would now call the "Lucas critique". Far from proceeding in ignorance of its
potential relevance to his analysis, Reuber took it seriously, but concluded that *as an empirical matter* ". . . the [Phillips] curves shown in the foregoing chart do not shift significantly in response to changes in monetary policy and in the mix of monetary and fiscal policy" (Reuber 1962 p.11); though his empirical work did identify foreign inflation as a variable that, in Canada, caused the terms of the inflation-unemployment trade-off to change.

Reuber also paid careful attention to a then current version of accelerationism. He cited Haberler (1961) as his source for this doctrine, which argued that creeping inflation would always tend to accelerate because of the effects of expectations. Reuber dismissed this hypothesis as " . . . very doubtful both theoretically and empirically" (p. 257) Theoretical arguments suggested that acceleration of even mild inflation was merely one possibility rather than logically necessary, and available evidence, notably that recently developed by Cagan (1956), showed that it was an empirically irrelevant possibility. Reuber was, of course, right to come to this conclusion, even in the light of the later Friedman-Phelps analysis: their version of accelerationism says that inflation will accelerate *only if attempts are made to hold unemployment below its natural rate*, and confirms that any rate of inflation will be stable at the natural rate. This all-important clarification of the accelerationist idea was not present in Haberler’s exposition of it. The basic point to be stressed

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20 This argument was a legacy of the Austrian analysis of the 1930s, and was frequently deployed by Lionel Robbins in Britain. In his textbook (1963), without referring to any adherent of the theory, Lipsey characterized it as "a very commonly-believed piece of folklore ..." (Lipsey, 1963, p. 430).

21 It is, however, interesting to note that in its final report, the Porter Commission, which seems to have been quite uninfluenced by Reuber’s study, argued as follows: " . . . it is sometimes argued that rising prices are necessary to stimulate business enterprise and induce labour to move into more productive occupations. . . . if significant price increases come to be anticipated-and they soon would be if they continued year after year—inflation would have no stimulating effect" (Royal Commission . . . 1964, p.419) This is within a hairsbreadth of the natural-rate version of the accelerationist argument.
here, however, and which these examples illustrate, is that, thirty years ago, the requirement that an hypothesis be empirically tested before being taken seriously was just as much a given among the majority of younger macroeconomists as it is now that an hypothesis have "sound microfoundations". The modern reader might be inclined to think that the standards applied in the 1960s when judging the strength of empirical results were often naive and sometimes downright lax, and in hindsight there is much to be said for this judgement. But "positive economics" was in vogue in the 1960s, and as the decade progressed the growing availability of computers only increased its attractions.\textsuperscript{22} This was neither the first, nor the last, time in the history of macroeconomics that enthusiasm for a new doctrine was accompanied by excessive confidence in the importance and robustness of the results it was producing.

I would argue, moreover, that ultimately it was empirical evidence and not theoretical arguments which undermined, among mainstream economists, the idea of the Phillips curve as a policy menu. Phelps' (1967) development of the expectations-augmented Phillips curve was devoted to recasting its policy menu interpretation in a dynamic mould, not to attacking it. His analysis, and Friedman's too, were quickly accepted as significantly clarifying the Phillips curve's theoretical basis, and as providing a means of formulating sharper questions about the circumstances under which the relationship was likely to remain robust, but in the late 1960s not many followed Friedman in arguing that it discredited the previous decade's work on these matters. For several years afterwards, those who did not \textit{a priori} "commit [themselves] to a complete absence of money

\textsuperscript{22}The influences at work here were complicated, but it would be wrong to attribute too much importance to Friedman's (1953) essay on "The Methodology of Positive Economics". Among the particular economists we are concerned with here, it was the influence of Karl Popper and the way in which the arguments of his \textit{Logic of Scientific Discovery} could be used to oppose the deductivism of Lionel Robbins, that was critical. On this matter, see Neil de Marchi (1988).
illusion" - to use Kaliski's (1964) phrase - took the position that the practical import of the Friedman-Phelps analysis was that the Phillips curve was steeper in the long-run than in the short run. It was only in the 1970s that the combined weight of more refined statistical arguments based upon Muth and Lucas's formalisation of the rational expectations hypothesis, and - much more important in my view - clear empirical evidence of simultaneously rising inflation and unemployment generated in the post-Bretton Woods flexible exchange rate world, finally discredited the Phillips curve as a policy menu.

SUMMARY AND CONCLUSIONS

In this paper I have laid particular stress on the contributions of Grant Reuber and Harry Johnson to the development and propagation of the idea of the Phillips curve as a menu for macroeconomic policy, with explicit and quantifiable prices attached to the items on it. There can be no doubt that we must turn to Reuber's (1962) work to find the first fully and precisely articulated account of this particular economic doctrine. But the moral of the story which I have told is not that this policy menu was, after all, a Canadian invention. Rather, the lesson to be learned from the foregoing pages is how complicated the processes whereby this idea was developed in fact were.

The possibility of conflicts among policy goals, in particular full employment, price stability, and economic growth was, as I have shown, widely entertained in the late 1950s. The Radcliffe

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23 This was the position taken by Solow (1969), Tobin (1972), Johnson (1970, 1972), and indeed, I must confess, Laidler (1971) among others. The basic point here was that, with data generated in the 1950s and 1960s, a lagged inflation rate added to the right hand side of a Phillips curve invariably took a coefficient smaller than unity. Only with the advent of the rational expectations idea did it become possible to reconcile this evidence with the absence of a long-run structural trade-off between inflation and unemployment.
Committee, the Commission on Money and Credit, and the Porter Commission all organised some of their work around this notion. At about the same time, Phillips proposed his curve, not in this context, but as a means of closing an analytic gap in the structure of the macroeconomic model he was developing in order to study the stability properties of macroeconomic systems subject to systematic policy interventions. Phillips, and even more so Lipsey, then treated the curve as the basis of an exercise in positive economics, and Lipsey intended his work to contribute to the debate about cost-push versus demand-pull explanations of inflation. Samuelson and Solow too were concerned with this debate, but like the Radcliffe Committee, they also entertained the hypothesis that the Phillips curve represented a stable trade-off, well defined enough to be useful for policy analysis; unlike the Radcliffe Committee, however, and unlike the Commission on Money and Credit for that matter, having entertained the hypothesis, they did not reject it. The Kennedy Administration’s Council of Economic Advisors quantified the output costs of running the U.S. economy at less than full-employment while conjecturing that, if aggregate demand was boosted to a "full-employment" level, cost-push forces might begin to cause inflation to set in before it was reached, a policy problem to be tackled with wage-price guidepost. At the same time, and quite independently, Cagan, Bailey and Alchian and Kessel had developed a welfare analysis of inflation based on the idea of inflation as a tax on cash balances.

All of these disparate elements came together in Reuber’s analysis to create a precise statement of the policy doctrine which, with a considerable boost from the writings of Harry Johnson, was to form the centrepiece of so called "Keynesian" economics, particularly that associated with James Tobin, by the end of the 1960s. Moreover, and ironically, the very literature from which these ideas developed also contained more than a trace of the "accelerationist" ideas which, once validated by empirical evidence, would in due course undermine the doctrine in
question. It would be hard to find a better example of the unpredictable way in which economic ideas develop.
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