

Some Misunderstandings Concerning the Contributions Made by A.W.H. Phillips and R.G. Lipsey to the Inflation-Unemployment Literature*

Robert Leeson*

This article examines the work of A.W.H. Phillips and R.G. Lipsey, in the light of an ongoing series of articles by Nancy J. Wulwick. In contrast to Wulwick's argument, this article finds that (a) there is no convincing evidence to support the proposition that Phillips and Lipsey were unduly influenced by 'extra scientific' considerations; (b) Phillips had a balanced approach to cost-push and demand-pull inflation; (c) Phillips' rapid promotion was perfectly understandable, given his international pre-eminence in the field of macroeconometrics; (d) Phillips' data substitution for the years 1881-6 did not noticeably change the non-linearity of his curve; (e) Wulwick's empirical reconstruction of Phillips' work is derived from a wage series that is inconsistent with Phillips' data, an error that Wulwick has compounded by refusing to supply her data; (f) Lipsey (1978), not Wulwick (1987), was the first writer to notice the inconsistency in Lipsey's (1960) theoretical explanation of the Phillips relation.

For almost twenty years, Nancy J Wulwick (an LSE graduate) has been writing about LSE economists, with particular attention paid to R.G. Lipsey and A.W.H. Phillips. Wulwick's research appears to be organised around the familiar theme of the Phillips curve (or Paish doctrine) as an unfortunate "revision" of Keynesian theory (1981, p.339). Specifically, she addresses the conflict between the "LSE lesson" (p.321) and one of its opponents, the "Oxbridge" Nicholas Kaldor: "The LSE lesson began with theory, the theory of General Equilibrium..." (p.322). The authors of the LSE Phillips curve had to "guide its gazers to not see" certain inconvenient aspects of their theory: "the LSE series [had] specific purpose in the theory of General Equilibrium" (p.322). The LSE "discoverers projected those of their methods considered most persuasive" (p.326); "the experimenters concluded that the hyperbola fitted the recent past very well, and they aimed that this norm induce the future [sic]" (p.334).

But Kaldor, "the Cambridge lecturer (before World War II an LSE student then Lecturer) disposed of the 'Paishite variants'. The refutation ridiculed. On the LSE's board and page... the refuter copied a 'replica of the economy', or rather, the economy depicted by The School's Richard Lipsey, a writer of the orthodox economics textbook ... By means of LSE's lemma, suitably reformed, LSE's theorems - of the neoclassical Phillips Curve and of the Keynesian Paish Doctrine- have been disproved" (1981, pp.359, 363). The inventors of the Phillips curve presented "a peppered prescriptive macro economic lingo"; they were the "academic inventors of macro economic pidgin. ...Who needed a solid economic theory when a rather inconsistent hodgepodge (of Marshallian theory, Keynesian theory, and neoclassical theory) could be used to produce such politically satisfying results?" (1981, pp.550-552). In contrast, "Kaldor's insightful, ingenious, iconoclastic essays" pre-empted new growth theories by decades (1993, pp.338-9), leaving Robert Solow "tense" when confronted with these historical reconstructions (1993, p.321).

As a framework for research, this all seems to be an odd mixture of muddle and banality. The first edition of Lipsey's textbook was 1963, four years after Kaldor's article (1959). The Oxbridge-LSE dichotomy is also unreliable, since Kaldor had been at LSE for twenty years, between 1927-47 (Blaug, 1985, p.107). Kaldor also concluded - and I think that Wulwick has failed to refer to this - that "I think he [Phillips] has established the existence of this relationship" (1959, p.293). Wulwick has also failed to do justice to Lipsey and Steuer's (1961) attempt to compare Kaldor's explanation with their own.

More seriously, Wulwick has constructed an argument that amounts to unfair criticism of both Phillips and Lipsey. In section 1, I shall address Wulwick's interpretation of Phillips' work, focusing attention on Phillips' clearly articulated role for cost-push pressures (section 1.1); his unorthodox combination of data sources (section 1.2); the reasons for, and consequences of, Phillips' data substitution, for the years 1881-6 (section 1.3); Phillips' attitude towards stabilisation targets (section 1.4); plus some suggested reasons for his rapid promotion at the London School of Economics (section 1.5). In section 2, I shall examine Wulwick's assessment of Lipsey's contribution to the inflation-unemployment literature. I shall argue that Lipsey did not state that the Phillips curve was "immutable" (section 2.1); that Lipsey (1978), not Wulwick (1987), was the first writer to notice a theoretical inconsistency in Lipsey's (1960) model (section 2.2); and I shall also offer a discussion of Lipsey's econometrics (section 2.3). Some concluding remarks are provided in section 3. On the basis of the evidence that she has offered, Wulwick's arguments are both overstated and unpersuasive. I shall attempt to be constructive, and, wherever possible, to offer an alternative explanation or interpretation to that offered by Wulwick.

1. Wulwick's Interpretation of Phillips' Work

1.1 *Did Phillips enter "the policy debate over inflation on the demand pull side"?*

Phillips referred to his own empirical work as "tentative" (1958a, p.299) and "very crude" (1962a, p.11). Wulwick appears to connect this modesty with an ulterior motive. She makes five references to Phillips' "crudeness" in constructing his curve: "He deftly exploited a crude curve-fitting procedure ... to arrive at the desired hyperbolic curve" (1989, p.188; see also pp.173, 174, p.176). Wulwick (1989, pp.186-7) also describes Phillips as being "without compunction", "willing to do some 'dirty work'", and having "the flourish of a former engineer".¹ As is well known, Phillips divided the 1861-1913 data into six intervals, and derived a representative average for each interval. These six 'crosses' were then used to locate his famous curve. Wulwick questions why Phillips chose his particular intervals, and concluded that only a "deceptive answer" (1989, p.180) can be obtained by referring to Phillips' stated objectives. The real reason, Wulwick (1987, p.842) concluded, was that it "suited his polemical aim".²

According to Wulwick, "The demand-pull faction was dominated by monetarists but included right-wing Keynesians such as Frank Paish" (1987, p.838, n.5). She concludes that "... it seems clear that Phillips entered the policy debate over inflation on the demand-pull side" (1989, p.183). As evidence, Wulwick (1989, p.173) cites a conversation between Conrad Blyth and Phillips, shortly before his death: "It was a rush job", he admitted. He was about to go on sabbatical leave to the University of Melbourne, and with the debate continuing, "it was better for understanding to do it (the study) simply and not wait too long". After all, he added modestly, "A.J. Brown had almost got these results earlier" (Blyth, 1975, p.306)". But Phillips made no mention of the debate to which Wulwick refers. His precise words were: "It was a rush job. I had to go off on sabbatical leave to Melbourne; but in that case it was better for understanding to do it

simply and not wait too long. A.J. Brown had almost got these results earlier, but failed to allow for the time lags" (cited by Blyth, 1975, p.306).

Wulwick's argument appears to have the flavour of 'guilt by association'. In the 1950s, senior economists were involved in the dispute over the origins of inflation. Some were employed by the same institution as Phillips; another had been the external examiner for Phillips' Ph.D. Some were involved in the Radcliffe Committee on the Working of the Monetary System (May 1957 - August 1959) and the Cohen Council on Prices, Productivity and Incomes (1957-1962). These committees deliberated over the causes of inflation, taking evidence from those who had undertaken research into inflation, such as A.J. Brown, - "a cost-push Keynesian" (Wulwick, 1987, p. 839) - and several demand-pull advocates. Phillips discussed his work with Brown, and indeed, mentioned Brown in his conversation with Blyth. Phillips was immediately promoted to a chair at LSE, which Wulwick finds "curious" (1987, p.841). Therefore, we are led to believe, Phillips must have been a partisan in these disputes.

But this is not a compelling argument. Wulwick has not demonstrated that Phillips was unduly influenced by the Radcliffe or Cohen deliberations, or, indeed, that it is reasonable to associate his work with any simple policy formula. In his inaugural professorial lecture, Phillips stated that "The knowledge and understanding which have so far been gained are far from being adequate for a firm and detailed appraisal of economic policy" (1962, p.3). In so far as his thoughts were influenced by the Radcliffe Committee, he stated that "Much more research will have to be done, however, before the last word is said on these matters, and I would heartily endorse the Committee's statement that '... it is essential to have much greater and more systematic knowledge of the factors that make up the financial system and of their relative movements'" (1962, p.9).

To further support her argument, Wulwick (1989, p.184) states that Phillips "believed his empirical evidence showed that ... we can only reduce inflation for any given rate of increase of productivity, at the cost of higher unemployment" [Phillips, 1962, p.11; 1958, p.299]. This stabilisation policy proposal was premised on a unique and permanent relation between average $\Delta W/W$ and average U . That indeed was just what the exact curve-fitting procedure implied". However, the sentences following the words cited by Wulwick are revealing: "I think such a relation does hold now, and unless it can be changed we shall probably move towards a compromise solution with a rather higher average level of unemployment than in the past few years and a lower, though not zero, speed of inflation, perhaps with 2 per cent. unemployment with about 1 per cent. per year rises in prices. To consider whether the relation can be modified we must know why it is that wages continue to rise while there is significant unemployment. A number of possible causes are often mentioned, in particular lack of mobility of labour and industry, resulting in uneven geographical and occupational distribution of unemployment, competitive bidding by employers for the most suitable labour, and trade union pressures ... if it were widely and clearly understood among the members of trade unions that the full use of their power to force up money wages would only lead, at a given level of unemployment to a faster rate of inflation and that the Government would have no alternative but to check this higher rate of inflation, in part at least, by lowering demand and causing some increase in unemployment, it seems possible that the trade unions might see where their true interests lay" (Phillips, 1962, pp.11-3).

Phillips' work may have been interpreted by others as evidence in favour of a demand-pull explanation of inflation (Blinder, 1988, p.282; but see also Leeson, 1994b). However, one of the perennial themes of Phillips' work was a balance between cost-push and demand-pull. In his introduction to the 'theoretical Phillips curve', *The Relationship between Prices and Production*

(1954, p.307-8), Phillips specifically stated that the location of his curve was dependent upon cost-push factors such as trade union power: "there will be some level of production and employment which, given the bargaining power of the different groups in the economy, will just result in the average level of factor prices remaining constant, this level of production and employment being lower the stronger and more aggressively the organisation of the factors of production" (1954, p.307). The *Policy Implications* derived from his Australian analysis appear to be primarily concerned with minimising cost-push factors: "full employment and stable prices cannot be completely and simultaneously obtained without a change in the present attitudes and methods of wage determination" (1959a, p.4). In the period immediately prior to his crippling stroke, Phillips continued to work on these matters. I have located a handwritten article amongst his private papers, entitled 'Economic Policy and Development', in which he included "the relative strengths of trade unions and employer organisations" as the "natural candidates" for modification, in order to reduce the rate of price inflation (1968b, pp.3-4). There appears to be a very strong consistency in Phillips' work, with respect to the importance of these cost-push factors.

1.2 Phillips' data sources

Wulwick (1987, pp. 838-40) refers to Phillips' "orthodox data sources... Phillips' estimation technique was ad hoc and informal. It was only these special ad hoc procedures that allowed Phillips to see the determinate inflation-unemployment relation". She also states that Phillips estimated "this curve using the Phelps Brown-Hopkins money wage series along with the unemployment data for the period 1861-1957" (1989, p.187). The Phelps Brown and Hopkins wage series and the Beveridge unemployment series were available for the period 1861-1938, but "Phillips, like most theorists, was pretty innocent of any knowledge of the data" (correspondence from Lipsey, 19 February 1993). With his "trial and error" strategy (1958a, p.290) he used what can only be described as an unorthodox combination of data sets. For 1861-1880 he used Phelps Brown and Hopkins for wages, and Beveridge for unemployment; 1881-1886 Bowley (wages), and Beveridge (unemployment); 1887-1920 Phelps Brown and Hopkins (wages), and Beveridge (unemployment); 1921-1945 Ministry of Labour for both; 1946-1948 Ministry of Labour (wages) and International Labour Organisation (unemployment); 1948-1957 Ministry of labour for both.

Wulwick's papers were written in the aftermath of the *Journal of Money, Credit and Banking Project* (Dewald *et al*, 1986), and she specifically volunteered to provide what she calls "Phillips' data" upon request (1989, p.180, n.18). Indeed, she wrote of Cripps and Tarling that "To assess their results, it is necessary to know the sample and the method of pooling the data, yet the participants have not yet made their data available upon request" (1993, p.335). I can only presume that her commitment to replication is no longer operative, having rejected three times my requests for her data. Wulwick is aware that I have Phillips' handwritten calculations for his famous curve. I have also been able to obtain Wulwick's data through a circuitous process. Wherever her wage series came from, it was not from Phillips (Leeson, 1995h). Her empirical inferences must therefore be regarded as unreliable.

1.3 *Phillips' data substitution for the years 1881-86*

Phillips constructed a curve for the period 1861-1913, and then plotted data for the seven sub-periods, each corresponding to identifiable trade cycles. The data appeared to trace out regular anti-clockwise loops around his curve. He noted, however, that Figure 4 (1958a, p.287), for the period 1879-86, did not fit the pattern of the other periods (1958a, p.291). He therefore experimented with other data sets, and found that using Bowley's index of wage rates, the regular anti-clockwise loop re-emerged (Figure 4a, 1958a, p.287). Given the explorative and "tentative" (1958a, p.299) nature of his inquiry, this may, or may not, have been a wise procedure. Lipsey (1960, p.5) certainly criticised him in this respect: "We cannot eliminate one [series] merely because it does not conform to our hypothesis". But Phillips devoted an extraordinary proportion of his article to a discussion of the sources and peculiarities of the data (particularly in comparison to some current econometric practises). I can detect nothing underhand in Phillips' procedure. With the benefit of hindsight, it is possible to criticise Phillips for all sorts of reasons (Leeson, 1994g); but deviousness is not, in my judgement, one of them.

Yet Wulwick (1989, pp.176-84) writes as though she has uncovered a 'conspiracy', involving Phillips and others, to locate a non-linear relationship: "Yet Phillips blamed the inconsistency between his hypothesis and the data on measurement error". The subtitles under section II of Wulwick's article read: "*The assumption of non linearity*", "*Finding non linear data*", "*Choosing a non linear specification*", "*Constructing a non linear graph of averages*", "*A demand-pull relation*", and "*The deflationary policy conclusion*" [emphases in original]. Wulwick (1989, p.179) concluded: "To this end, Phillips' data substitution in fact created a noticeable increase in the non-linearity of the graph (Lipsey, 1960, p.5)".

Again, this is not a convincing argument. First, in my judgement, the change in the curve would be more accurately described as "barely noticeable". The data substitution, which Phillips undertook for the years 1881-6, changed the *coefficients* in Lipsey's equation (1960, p.5), but glancing at Lipsey's diagram (1960, p.4, Fig. 3), the curve associated with Phillips' six crosses (2), appears almost indistinguishable from the curve (8) derived from raw data, which did *not* use Bowley's controversial data. There appears to be no change in the curve, at least before six per cent unemployment; and the maximum vertical distance between the curves is less than one-quarter of one percentage point, and this occurred at eleven per cent unemployment (Lipsey, 1960, p.4, Fig. 3; see also Gilbert, 1988, p.82). Secondly, the only two aberrant observations in the data that Phillips was suspicious about, were 1881 and 1882. Thirdly, the data sources prior to 1884 are, as is well known, very scanty (Routh, 1959; Wulwick, 1989, pp.178-9). Fourthly, in James Meade's evocative phrase, "The Phillipian method was to take out a spanner and have a look". Phillips clearly stated that he was investigating the possibility that "some peculiarity may have occurred in the construction of Wood's index for these years" (1958a, p.291). Lipsey also noted that there was a "suspicious stability in wage rates over the period 1881-85 in spite of wide variations in employment" (1960, p.5, n.4). Besides, Phillips regarded his empirical work as illustrative and not definitive. His discussion of the data substitution was in the context of his loops, or more precisely the absence of the loop in Wood's index, for the trade cycle 1881-6. The implication of Wulwick's analysis is that Phillips was (dishonestly?) choosing data, not for his stated reason, but for reasons associated with some ulterior polemical agenda. More evidence is required before this accusation can be sustained.

1.4 *Did Phillips' model permit a trade-off?*

Wulwick (1989, pp.171, 185-6) states that "stabilisation policy required a target and Phillips assumed the target of a constant price-level rather than full employment ... Phillips' model

permitted a $\Delta W/W-U$ trade off with the proviso that U be maintained for several years while economic equilibrium was re-established". This is not quite correct. Phillips devoted his career to formulating policies which could eliminate the unemployment associated with "deficiency of aggregate demand" (1962, p.13). He was also concerned to encourage more flexible labour market arrangements to facilitate rising productivity and living standards. He also warned that the Government might be "prevented by inflation from maintaining a high aggregate level of demand for labour ..." (1962, p.13). There are plenty of reasons for distancing Phillips from the trade-off interpretation of his work, not the least of which is the way he explained his model to students, such as Adrian Pagan. Phillips sought to make the operating characteristics of the macroeconomy better - to improve its design capacity - and was very sceptical of specific stabilisation targets (conversation with Pagan, 5 November 1993; see also Leeson, 1994a; 1994c; 1995c).

1.5 Was there something "curious" about Phillips' rapid promotion?

Despite being provided with assessments from James Meade and Henry Phelps Brown, Wulwick (1987, p.841) concludes that "... it seems curious that the LSE quickly promoted Phillips to Professor of Economic Science and Statistics". This is strange comment. James Meade regarded Phillips as "a genius," and as "an admirable and outstanding person" (correspondence from Meade, 26 February, 1993, 11 May, 1993); "I cannot refrain from explaining how I owe my basic education in the design of stabilisation policies to Professor A.W.H. Phillips to whom, above all, I would express my gratitude. I shall never forget the hours which I spent with him at the London School of Economics discussing these matters" (1971, preface). Lionel Robbins, who, it will be remembered, was the "incubus" from which the LSE econometricians felt obliged to escape (de Marchi, 1988, p.148), also regarded Phillips as "a genius" (conversation with Robert Mundell, 2 November, 1993; conversation with Graeme Dorrance, 3 November, 1993). Indeed, Robbins encouraged Phillips' econometric work (O'Brien, 1988, p.36), and as early as 1950, regarded him as being "on the international map as an economist of profound grasp and originality" (cited by Barr, 1988, p.315). Milton Friedman has indicated the extent to which he was influenced by Phillips in his search, in the early 1950s, for an adaptive expectations explanation of inflation (correspondence from Friedman, 25 August, 1993). Henry Phelps Brown and B. Weber (1953, pp.263, 278-80) also indicated their substantial debt to "Mr A.W.H. Phillips" (who was, at that time, a Ph.D. candidate) for the form of the argument about inflationary expectations (see also Phelps Brown and Ozga, 1955, p.8, n.1). Paul Samuelson, who is reputed to have an "utter disdain for lesser mortals" (Blaug, 1985, p.215), recently wrote about his "revered friend Bill Phillips" (correspondence from Samuelson, 13 August, 1993). Harry Johnson (1977a, p.206), who could not be described as someone with a tendency to flatter, regarded Phillips' work as, possibly, one of the two "really seminal idea[s] contributed by British economics in the whole post-Keynesian period"; Phillips was "brilliantly original" (Johnson, 1991 [1976], p.116; see also 1970, p.110). Johnson (1977b, p.756) also incorporated a posthumous tribute to Phillips' pioneering work, in his Chairman's remarks at the Third World Congress of the Econometrics Society. William Baumol concluded that "... had he lived longer [Phillips] might well have won a Nobel Prize" (Baumol *et al.*, 1992, p.301).

Phillips made an extraordinary contribution to macroeconomics and econometrics, mostly as a hidden co-author of other people's work (correspondence from Lipsey, 15 October 1992). He was working (1968a), just before his stroke in 1969, on an approach to econometric policy evaluation, which was subsequently named after Robert Lucas (Blyth, 1989; Leeson, 1994a, p.612). Phillips' life and work remains "a source of inspiration and example to this and coming

generations ... what a tragic loss his early death entailed" (correspondence from Harcourt, 6 October, 1994).

2. **Wulwick's Interpretation of Lipsey's Work**

Richard Lipsey is in a unique position to reflect on the Phillips curve episode - from birth pangs to death rattle. He provided much of the original impetus for this research (1960; Lipsey and Steuer, 1961); he has also shown a remarkable capacity for self-castigation on this topic. In my own work, I have found Lipsey's essay (1978) in Phillips' Memorial Volume to be the most helpful guide to Phillips' work. This essay was, I think, the first to set Phillips' empirical work in its proper context - namely, the stabilisation problem which dominated his professional life. Almost all other works on Phillips have neglected this context, and, as a result, have failed to do justice to the subtlety of Phillip's contribution. I have shamelessly borrowed from Lipsey's essay in my review of Phillips' stabilisation exercise (1995c). So too has Wulwick. But having other fish to fry, she has neglected to acknowledge her debt to Lipsey.

2.1 *Did Lipsey argue that the Phillips curve was "immutable"?*

Wulwick (1987, p.841) states that "Phillips functions, it was thought, might be 'immutable' (Lipsey, 1960, p.19)". Yet the precise quotation from Lipsey (1960, p.19) is: "These conclusions have a number of interesting real-world implications: If one wishes to predict the rate of change of money wage rates, it is necessary to know not only the level of unemployment but also its *distribution between the various markets of the economy* [emphasis in original]. The macro-function *need not be accepted as immutable* [emphasis added] even if the individual functions are". If unemployment were to remain substantially unchanged for a long time, (as a result of deliberate policy, perhaps), the Phillips curve, Lipsey (1960, p.30) argued, "may not be a very good guide ... a very low degree of confidence might be attached at this stage to a particular estimate of the parameters" [emphasis in original]. Lipsey (1960, p.31, n.1) also warned about a "premature application to policy". In the following year Lipsey and Steuer (1961, p.150, n.2) concluded that the absolute explanatory power of the Phillips curve was "not very great". In 1962, Lipsey (1968 [1962], p.247, n.3) argued that the Phillips curve "might shift automatically after high demand had been maintained for some time". Wulwick's inference, therefore, seems unreasonable.⁴

2.2 *Was Lipsey (1978) or Wulwick (1987) the first to notice Lipsey's 'slip of the pen'?*

Wulwick (1987, pp.847-8) has also constructed an elaborate methodological argument based on an elementary 'slip of the pen' in the original outline of this theory: "the researchers erred ... what caused Lipsey and Archibald's oversight? Perhaps they were merely careless with the geometry, or it might have required too deliberate an effort to detect weaknesses in their favourite mode of explanation". Yet Wulwick, in 1987, has 'discovered' an inconsistency which seems remarkably similar to the 'slip' which Lipsey had admitted in an essay presented to Phillips on the occasion of his sixtieth birthday, in November, 1974 (1978). In this contribution to Phillips' posthumously published *Festschrift*, Lipsey acknowledged the role that Phillips curve scholars played in misinterpreting Phillips' work: "These warnings were however, quickly forgotten, and many economists including myself [Lipsey, 1964], were soon plotting Phillips curves in ΔP , U space along with policy makers indifference curves, and determining the optimal combination of ΔP which usually occurred at a positive level of ΔP " (1978, pp.56-7; see also 1981, p.557, n.16).

Lipsey (1978, p.58) also stated that "in my own attempt to rationalise the Phillips curve I did not follow Phillips' lead". Lipsey acknowledged (1978, p.59) that upon re-examination, his theoretical explanation (1960, pp.12-23) produced a "relation that does not look like a Phillips curve ... The steep segment of the curve [1978, Fig. 4.3, e(ii), p.59] in the negative ΔP range is unsatisfactory". It had a slope of -1, unlike the curve fitted to the data which became horizontal for the post-1923 period, at +1 per cent. ΔW (Fig 9, 1960, p.24). Since, according to Wulwick's bibliography (1987, p.856, item 54), she is familiar with Lipsey's discussion, it might be considered unfair to Lipsey to 'expose', without acknowledgment, this inconsistency (or, at least, a closely related inconsistency), a dozen years or so after Lipsey had acknowledged this error.⁵⁶

2.3 *Lipsey's econometrics*

Wulwick (1987, p.841) also states that "Lipsey did not specify rejection rules, but on standard criteria his test failed to verify the Phillips relation in respect to either the long run or short run. Economists nevertheless did not drop the Phillips curve as positivist methodology would predict". Again, this is strange comment. Lipsey clearly explained why he had chosen *two* levels of unemployment variables (1960, pp.2-5). Contemporary economists were, like Lipsey, correctly concerned not with their *individual* statistical significance, but with their *joint* statistical significance (correspondence from Lipsey, 19 February 1993). Since, for the 1861-1913 period, $F(2, 47) = 67.2$ (critical value of F, at the one per cent. level = 3.23), it seems safe to reject the hypothesis of joint statistical insignificance (Sleaman, 1981, pp.4-5). Both the inter-war and the 1948-57 unemployment variables also appear to achieve joint statistical significance at conventional levels (Sleaman, 1981, pp.10-11; Thomas, 1984, pp.2-4).

3. Concluding Remarks

In this essay, I have attempted to evaluate fairly (i.e. on the basis of the available evidence) Nancy J. Wulwick's interpretation of the contribution made by Phillips and Lipsey to the inflation-unemployment literature. Wulwick has made a contribution, but her interpretation - particularly with respect to inferred motivation - appears to be both ungenerous and unpersuasive. On the basis of the evidence offered, she has not made a convincing case. In my judgement, this is because her analysis is based, in part, on a tendency to quote out of context (see sections 1.1 and 2.1, above), and, in part, on a tendency to make assertions, without offering a well-thought-out and balanced case (see section 1.5, above). Her discussion of Phillips' theory is apparently derived from Lipsey; but no acknowledgment is forthcoming (introduction to section 2). Her discussion of the inconsistency in Lipsey's theory may, or may not, be an original footnote to Lipsey's re-examination (section 2.2); but why has she not acknowledged her debt to Lipsey in this context?

Nancy J. Wulwick has also attempted to extract (incriminating?) information from various economists in order to bolster her argument. Chris Archibald, for example, found that his initial contact with her "seemed innocuous"; but he "read with increasing horror" her reconstruction of events: "I cannot express my indignation at her impugning Bill's integrity in this vile way". (correspondence from Archibald, 9 June 1994). Wulwick's work could only lead to controversy; but she has demonstrated that she is unwilling to engage in scholarly cooperation to resolve these differences of interpretation.

For Wulwick (1987, p.854), economists are "inevitably partisan", and Lipsey's work is evidence of "wasted time".⁷ She also writes, inaccurately, of Lipsey being one of "these Labour

Party members" (1988, p.23).⁴ But ungenerous and misleading re-interpretations of one's predecessors' work is not, by itself, terribly edifying, and invites a similar examination of one's own work.⁵ There are, no doubt, many instances to be found of inadvertent errors in this episode of macroeconomic history.⁶ But attributing motives is a hazardous operation, which is why historians of thought should be guided by Bertrand Russell's (1961, p.58) advice: "The right attitude is neither reverence nor contempt ...".

- * Department of Economics, Murdoch University, WA 6150. This paper requires a justification. During the course of researching for a Ph.D. on *A.W.H. Phillips and the Political Economy of the Inflation-Unemployment Trade-Off* I became aware that my own rather jaundiced view of Phillips and Lipsey (the authors of the common misfortune associated with the trade-off interpretation of the Phillips curve etc.), was completely at variance with the impression that I was gaining from correspondence and conversations with those who were active in the economics profession in the 1950s and 60s. To borrow an analogy from thermodynamics, I was aware of a state of thermal disequilibrium. A moment of epiphany came when I uncovered the details of Phillips' wartime incarceration (Leeson, 1994a). I subsequently located one of the sources of my (unsympathetic) misapprehensions about these scholars in the writings of Nancy J. Wulwick that are the subject of this paper. I have tried, as far as possible, to adjust this note so as to present her work as fairly as possible. I am grateful to Max Steuer for allowing me access to the LSE M^T Seminar records. A large number of scholars (including two referees from this journal) have also made valuable suggestions about this essay. None should be implicated in its contents.

Notes

- ¹ This raises at least two points. First, it should be remembered that all the empirical work done in the Economics Department at LSE in the 1950s was performed on mechanical calculators. In the early 1960s this research was performed by research assistants inverting matrices by the Doolittle technique on electronic desk calculators. Secondly, subsequent research, in the era of easily accessed computing power, found merits in Phillips' procedure (see, for example, Gilbert, 1976).
- ² Wulwick (1987, p.842) asked "... why was a rectangular hyperbola chosen? Other simple functions would have gone through Phillips' crosses. First, Phillips' hyperbola was an "eyecatching" hyperbola which suited his polemical aim (Santomero and Seater, 1978)". Santomero and Seater (1978, p.500), wrote that the 'Phillips relation' was a simultaneous discovery, but it was "A.W. Phillips' paper which caught the profession's eye .. only Phillips drew the eyecatching, now famous curve that bears his name". The reference to the "polemical aim" is Wulwick's.
- ³ Richard Lipsey has written "what does it add to knowledge to incorrectly infer the motives of one of the finest persons I have ever known? Not only was Bill [Phillips] an almost saint-like person, he had a better technical expertise than almost all of his contemporaries ... the Phillips curve was borne as part of a scientific research program, not as part of a program to promote the author or some political idea" (correspondence, January 24, 1994); "I have absolutely no idea where Bill Phillips' political sympathies (if any) lay, nor how he voted - which tells you something about what we thought was important" (correspondence, December 15, 1994).
- ⁴ Nancy Wulwick has tried to persuade me that she intended to represent Lipsey in the context of his regional Phillips curves. But, to my mind at least, this is not the interpretation conveyed by her article. The first reference to Lipsey occurs on page 841: "Richard Lipsey, a colleague of Phillips at LSE, carried out a standard statistical test. The result was a long run curve drawn close to Phillips' curve ..." (1987, p.841). On the same page, under the subtitle "*Grounds for Acceptance*" Wulwick stated that "There were firm epistemological reasons for accepting the long run curve ... Phillips functions, it was thought, might be 'immutable' (Lipsey, 1960, p.19)".
- ⁵ In donning the mantle of originality, Wulwick has neglected to mention that there already exists a body of literature on Lipsey's theoretical work (see, for example, Kuska, 1966; Kuh, 1966; Vanderkamp, 1968; Holmes and Smyth, 1977; Smyth, 1977, p.427, n4; Smyth, 1979; Corry and Laidler, 1967; Corry and Laidler, 1968; Lipsey, 1974). Only Lipsey, 1974, and Holmes and Smith, 1970, appear in Wulwick's

bibliography; but neither are discussed in their proper context. For a discussion of Lipsey's theory, see Leeson, 1995d.

The internal inconsistency of Lipsey's model is remarkable for at least two reasons. First, it was a highly influential paper which most students of macroeconomics would have been exposed to in the 1960s. When Richard Lipsey was a visiting Professor at Berkeley in 1963-4, he found, at the 28 American Universities that he visited, including all the most famous ones, it was (with his General Theory of the Second Best) the most talked about paper. The AEA reprinted it in their volume on Business Cycles (correspondence from Lipsey, 19 February, 1993). Secondly, although his article was not refereed, Lipsey subjected its contents to an extremely rigorous scrutiny. Lipsey delivered his paper at a dozen British Universities before publication. Lipsey was also the chairman of the London School of Economics Staff Seminar on Methodology, Measurement and Testing in Economics (M²T). The aim of this seminar series was to turn economics into an empirical 'science' by reformulating economic theory so as to generate testable predictions which could then be confronted with the data (de Marchi, 1988). Between December 1957, and October 1958, the M²T economists met and discussed methodological problems, such as "What is a Theory?" Lipsey's paper was discussed in the second seminar devoted to a specific topic in economics. Three seminars were devoted to Lipsey's paper (26 November, 10 December, 1958) and four seminars on "Testing Theories of Inflation" (28 October, 4, 11, 18 November, 1959). In spite of all this critical attention, the internal inconsistency, discussed above, went unnoticed until Lipsey came to re-evaluate his work for Phillips' Memorial Volume.

Wulwick (1987, p. 854) stated "Of course upon retrospect, only two formulations, Phillips' first and Friedman's curve, advanced economic inquiry. Nevertheless, practitioners of other disciplines have wasted time...still other scientists have tried to save degenerating orthodox programs by illegitimate means".

Richard Lipsey has written: "I was never active in politics. I never supported a political party since none reflected my left wing views on social policy and right wing views on the efficiency of the market. My passionate concern was with the scientific base of economics. I started out life to become a scientist and got seduced by the idea of scientific laws of human behaviour... I did have an axe to grind, but it was to discredit the older *a priori* methodology of the classical economists as expounded by Robbins in the *Nature and Significance of Economic Science*" (correspondence, January 24, 1994).

There are a number of additional errors in Wulwick's work. See, for example, the misrepresentation of Phillips' data set (1989, p.178), and the reference to Phillips being able to "shift his curve up to fit the 1927-37 scatter" (1987, p.843).

It seems possible that Lipsey has trivially misquoted *himself* four times when transferring material from his 1960 article to his textbook. His data set was 1923-39 and 1948-57, not from "1923 to date (omitting the period of the second World War)" (1963, p.436). The reported coefficient was 0.69 (1960, p.26, equation 13) not 0.67 (1963, p.436). The R^2 for the equation containing only the level of unemployment was 0.64 (1960, p.5) not 0.80 (1963, p.438). The R^2 for the post-1923 equation containing only the level of unemployment was 0.28 (1960, p.25) not 0.60 (1963, p.438). But what light does this shed on this episode of macroeconomic history?

References

- Barr, N., 1988, "The Phillips Machine". *London School of Economics Quarterly*, 2:4, Winter, pp. 305-47.
- Baumol, W., Blinder, A.S., Gunther, S.W. and Hicks, J.R.L., 1992, *Economics: Principle and Policy*. (Sydney: Harcourt Bruce Jovanovich). Second Australian Edition.
- Blaug, M., 1985, *Great Economists Since Keynes*. (Great Britain: Wheatsheaf).
- Bergstrom, A.R., Cati, A.J.L., Peston, M.H. and Silverstone, B.D.J. (eds.), 1978, *Stability and Inflation: A Volume of Essays to Honour the Memory of A.W.H. Phillips*. (New York: John Wiley and Sons).
- Blinder, A., 1988, "The Fall and Rise of Keynesian Economics". *The Economic Record*, December, Vol. 64, No. 187, pp. 278-94.
- Blyth, C.A., 1975, "A.W.H. Phillips, M.B.E., 1914-75". *The Economic Record*, Vol. 51, No. 135, pp. 303-7.
- _____, 1989, "A.W.H. Phillips". In Eatwell, J. Milgate, M., and Newman, P. (eds.). *The New Palgrave Dictionary of Economics*. (London: Macmillan), Vol. 3, pp. 857-8.

- Brown, A.J., 1955, *The Great Inflation*. (London: Oxford U.P.).
- Cate, T., Colander, D. and Harcourt, G. (eds.), 1995, *Encyclopedia of Keynesian Economics*. (England: Edward Elgar).
- Corry, B. and Laidler, D., 1967, "The Phillips Relation - A Theoretical Explanation". *Economica*, May, pp. 189-97.
- _____, 1968, "The Phillips Relation: A Theoretical Explanation - A Reply". *Economica*, Vol. XXXV, p. 184.
- Croome, D.R. and Johnston, H.G., 1970, (eds). *Money in Britain 1959-69*. (Oxford: OUP).
- De Marchi, N., 1988, (ed.). *The Popperian Legacy in Economics*. (Cambridge: C.U.P.).
- Dewald W.G., Thursby, J.G., Anderson R.G., 1986, "Replication in Empirical Economics: The Journal of Money, Credit and Banking Project." *American Economic Review*, September, pp. 587-603.
- Fisher, M., 1978, In Bergstrom et al. (eds.).
- Gilbert, C., 1976, "The Original Phillips Curve Estimates". *Economica*, 43, pp. 51-7.
- _____, 1988, *The Development of Econometrics in Britain since 1945*. Unpublished Ph.D. Thesis. Oxford University.
- Goodwin, R., 1992, "Foreseeing Chaos". *Royal Economic Society Newsletter*, No. 77, April, p. 13.
- Holmes, J.M. and Smyth, D.J., 1970, "The Relation between Unemployment and Excess Demand for Labour: An Examination of the Theory of the Phillips Curve". *Economica*, August, pp. 311-5.
- Intriligator, M., 1977, (ed.). *Frontiers of Quantitative Economics*. (Amsterdam: North Holland), Vol B.
- Johnson, C., 1991, (ed.). *Monetarism and the Keynesians*. (London: Pinter Publishers).
- Johnson H.G., 1970, "Recent Developments in Monetary Theory." In Croome and Johnston (eds).
- _____, 1977a, "The Shadow of Keynes". *Minerva*, Vol. XV, No. 2, Summer, pp. 201-13.
- _____, 1977b, "Chairman's Notes on the Symposium on Macroeconomic Models and Policy". In Intriligator (ed.).
- _____, 1991, "What is Right with Monetarism?" In C. Johnson ed.
- Kaldor N., 1959, "Economic Growth and the Problem of Inflation". *Economica*, November, pp.287-298.
- Keynes, J.M., 1936, *The General Theory of Employment Income and Money*. (London: MacMillan).
- Kuh, E., 1967, "A Productivity Theory of Wage Levels - An Alternative to the Phillips Curve". *Review of Economic Studies*, Vol. 34, pp. 333-60.
- Kuska, E.A., 1966, "The Simple Analytics of the Phillips Curve". *Economica*, Nov, 34, pp. 462-7.
- Leeson, R. (1993a) "Policy-Induced Recessions: What have we Learnt?" *Economic Papers*, June, Vol 12, No 4, pp.85-96.
- _____, 1993b, "The Hard Facts of Fiction". *Australian Financial Review*, 27 October, p.19.
- _____, 1994a, "A.W.H. Phillips, M.B.E. (Military Division)". *Economic Journal*, May, Vol. 104, No. 424, pp. 605-18.
- _____, 1994b, "The Rise and Fall and Rise and Fall of Keynesian Economics?". *Economic Record*, September, Vol. 70, No. 210, pp. 262-266.
- _____, 1994c, "A.W.H. Phillips, Inflationary Expectations and the Operating Characteristics of the Macroeconomy". *Economic Journal*, November, Vol. 104, No. 427, pp.1420-1422.
- _____, 1994d, "Economists' Response to the Horror of War". *Australian Financial Review*, 25 January, 1994, p. 16.
- _____, 1994e, "The Collected Works of A.W.H. Phillips". *Royal Economic Society Newsletter*, July, No. 86, p. 17.
- _____, 1994f, "The Phillips Curve Paradox and the Smallest Probability in the History of Economic Research". *History of Economics Review*, Summer, No. 22.
- _____, 1994g, *A.W.H. Phillips and the Political Economy of the Inflation Unemployment Trade-Off*. Ph.D. thesis.
- _____, 1994h, "Who's Whispering in Clinton's Ear?". *Australian Financial Review*, May 18, p.19.
- _____, 1995a, "The Phillips Curve". In Cate, Colander and Harcourt (eds.).
- _____, 1995b, "A.W.H. Phillips". In Cate, Colander and Harcourt (eds.).
- _____, 1995c, "The Trade Off Interpretation of Phillips' Dynamic Stabilisation Exercise". Murdoch University Working Papers in Economics.

- _____. 1995d, "Does the Expectations Trap Render the Natural-Rate Model Invalid in the Disinflationary Zone?". Murdoch University Working Papers in Economics.
- _____. 1995e, "The Political Economy of the Inflation-Unemployment Trade-Off". (forthcoming).
- _____. 1995f, "The Eclipse of the Goal of Zero Inflation". Murdoch University Working Papers in Economics.
- _____. 1995g, "'The Ghosts I Called I Can't Get Rid of Now': The Keynes-Tinbergen-Friedman-Phillips Critique of Keynesian Macroeconometrics." Murdoch University Working Papers in Economics.
- _____. 1995h, "Phillips' Original Data." Murdoch University Working Papers in Economics.
- _____. 1996, (ed.). *The Collected Works of A.W.H. Phillips*. (Cambridge: Cambridge University Press).
- Lipsey, R.G., 1960, "The Relation Between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1862-1957: A Further Analysis". *Economica*, February, Vol. 27, pp. 1-31.
- _____. 1962, "Can There Be a Valid Theory of Wages?" Reprinted in McCormick, B. and Smith, E.O. (eds.). (1968). *The Labour Market*. (London: Penguin).
- _____. 1963, *An Introduction to Positive Economics*. (London: Weidenfeld and Nicolson). First Edition.
- _____. 1974, "The Micro Theory of the Phillips Curve Reconsidered: A Reply to Holmes and Smyth". *Economica*, February, pp. 62-70.
- _____. 1978, "The Place of the Phillips Curve in Macroeconomic Models". In Bergstrom, A.R. et al. (eds.).
- _____. 1981, "The Understanding and Control of Inflation: Is There a Crisis in Macroeconomics?" *Canadian Journal of Economics*, XIV.4, pp.545-576.
- Lipsey, R.G. and Steuer, M.D., 1961, "The Relation Between Profits and Wage Rates". *Economica*, May, Vol. 28, pp. 137-55.
- Meade, J., 1971, *The Controlled Economy*. (London: George Allen and Unwin).
- Newlyn, W., 1992, "A Back of the Garage Job". *Royal Economic Society Newsletter*, No. 77, April, p. 12.
- O'Brien, D.P., 1988, *Lionel Robbins*. (London: MacMillan).
- Phelps Brown, E.H. and Weber, B., 1953, "Accumulation, Productivity and Distribution in the British Economy, 1870-1938". *Economic Journal*, June, pp. 263-88.
- Phelps Brown, E.H. and Ozga, S.A., 1955, "Economic Growth and the Price Level". *Economic Journal*, March, pp. 1-18.
- Phillips, A.W., 1950, "Mechanical Models in Economic Dynamics". *Economica*, 17, pp. 283-305.
- _____. 1953, *Dynamic Models in Economics*. Ph.D. Thesis. University of London.
- _____. 1954, "Stabilisation Policy in a Closed Economy". *Economic Journal*, June, pp. 290-323.
- _____. 1956, "Some Notes on the Estimation of Time-Forms of Reactions in Interdependent Dynamic Systems". *Economica*, 23 May, pp. 99-113.
- _____. 1957, "Stabilisation Policy and the Time Form of Lagged Response". *Economic Journal*, Vol. 67, pp. 265-77.
- _____. 1958a, "The Relation Between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861-1957". *Economica*, November, 25, pp. 283-99.
- _____. 1958b, "La Cybernetique et le Controle des Systemes Economiques". *Etudes Sur la Cybernetique et l'Economie*, N, 21, pp. 41-50.
- _____. 1959a, "Wage Changes and Unemployment in Australia 1947-58". Economics Monograph No. 219, Economic Society of Australia and New Zealand, New South Wales Branch.
- _____. 1959b, "The Estimation of Parameters in Systems of Stochastic Differential Equations". *Biometrika*, 46, pp. 67-76.
- _____. 1961, "Employment, Money and Prices in a Growing Economy". *Economica*, November, Vol. 28, No. 112, pp. 360-70.
- _____. 1962a, "Employment, Inflation and Growth". *Economica*, February, Vol. 29, pp. 1-16.
- _____. 1962b, "Estimation in Continuous Time Series Models with Autocorrelated Disturbances". Mimeo, in Phillips' papers, available through the London School of Economics Library.
- _____. 1967, "Analysis of the Operation of a Buffer Stock for Cocoa". Mimeo, in Phillips' papers, available through the London School of Economics Library.
- _____. 1968a, "Models for the Control of Economic Fluctuations". In *Scientific Growth Systems, Mathematical Model Building in Economics and Industry*. (London: Griffin).

- _____, 1968b, "Economic Policy and Development". Mimeo, in Phillips' papers, available through the London School of Economics Library.
- _____, 1978, "Estimation of Systems of Difference Equations with Moving Average Disturbances". 1966 Walras-Bowley Lecture, San Francisco. In Bergstrom *et al.* pp. 181-99.
- Phillips, A.W. and Quenouille, M.H., 1960, "Estimation, Regulation and Prediction in Interdependent Dynamic Systems". *Bulletin de l'Institut International Statistique*, pp. 335-43.
- Routh, G., 1959, "The Relation between Unemployment and the Rate of Change of Money Wage Rates: A Comment". *Economica*, pp. 299-315.
- Russell, B., 1961, *A History of Western Philosophy*. (London: George Allen and Unwin).
- Santomero, A.M. and Seater, J.J., 1978, "The Inflation Unemployment Trade-Off: A Critique of the Literature". *Journal of Economic Literature*, Vol. XVI, June, pp. 499-544.
- Sleaman, A., 1981, "The Relation Between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1851-1979". Paper presented at the 11th International Conference, Atlantic Economic Society, London School of Economics, August 20.
- Stern, N., 1992, "A Coupling and A Search". *Royal Economic Society Newsletter*, No. 77, April, p. 12.
- Schweir, A.S. and Schweir, J.F. (1982). "The Council of Economic Advisers and the Phillips Curve: Part I, 1959-69". *The Journal of Economics*, Vol. VIII, pp. 1-6.
- Thomas, J.J., 1984, "Wages and Prices in the United Kingdom, 1862-1913: A Reexamination of the Phillips Curve". Paper presented to the ESRC Quantitative Economic History Study Group, at New College, Oxford, 21-22, September, 1984.
- Vanderkamp, J., 1968, "The Phillips Relation: A Theoretical Explanation - A Comment". *Economica*, May, 35, pp. 179-83.
- Wulwick, N.J., 1981, "British National and Regional Economic Policy, October 1964-June 1967." Ph.D. thesis in Urban Studies, University of Kent at Canterbury, 675 pages.
- _____, 1987, "The Phillips Curve: Which? Whose? To Do What? How?". *Southern Economic Journal*, Vol. 53, pp. 834-57.
- _____, 1988, "Phillips' Approximate Regression". Seminar paper delivered on 14 April.
- _____, 1989, "Phillips' Approximate Regression." *Oxford Economic Papers*, 41, pp.170-88.
- _____, 1993, "What Remains of the Growth Controversy?" *Review of Political Economy*, 5.3, pp.321-343.