

Phillips in Retrospect*

by

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Every economist has heard of Bill Phillips, most of them for the wrong reason. His 1958 empirical study (ch.25 in this volume), which seemed to establish the existence of an inverse trade-off between wage-inflation and unemployment in the previous 90 years or so of United Kingdom data, was, in its author's own opinion, "quick and dirty"(Ann S, Schwier, p 25), and "done in a weekend" (Bob Gregory to Leeson, p. 11)¹. Anyone who knows the rest of Phillips work will find Charles Holt's judgement that this famous paper was "without question . . .his least solid piece of work" (p.313) completely uncontroversial. Nor did the Phillips curve stand the test of time, though Fatemeh Shadman-Mehta does show here (ch. 34) that the inverse inflation unemployment trade-off such as Phillips found in the 1863-1913 data is still there when modern econometric techniques are deployed. Converted by others into a policy menu in the 1960s, the Phillips curve is now widely blamed (largely wrongly in my view, as I briefly argue in fn.7, below) for the policies that led to the great inflation of the 1970s and 1980s. Its creator's other written contributions have largely faded from the sight of all but specialised readers, and there were not many of these others in the first place: barely enough to fill a slim book, even with all surviving unpublished and unfinished essays included.

All this has long been a problem for those of us who still remember and unabashedly admire Phillips work in macroeconomics and econometrics, and have wished to see it accurately remembered and his reputation restored.² With this book, Robert Leeson has taken a major step towards such a rehabilitation. He has collected together all of Phillips' written work in economics and econometrics - amounting to under 250 pages - and supplemented it with a series of commentaries and reminiscences by no fewer than 30 others, including himself. Everyone interested in macroeconomics should read the resulting volume, which would, however, be a great deal easier to use for scholarly purposes had its rather thin index had a few more entries,

¹Unless otherwise explicitly indicated, author, chapter and page references in what follows refer to the volume under review.

²Let me declare an interest here: though I was never close to Phillips, I attended his lectures on stabilization policy as a final year undergraduate in 1958-59, had the privilege of being his very junior colleague in 1961-62, and worked with one of his most able intellectual grandchildren, Peter Jonson, in the the 1970s. This review is not, then, a disinterested commentary.

and been supplemented by a dated bibliography of Phillips' writings and a *curriculum vitae*.

Phillips and his Research Agenda

Bill Phillips was an extraordinary human being: the same unimpeachable personal integrity that supported his heroism as a prisoner of war of the Japanese lay just below the surface of his academic work too. James Meade, who found him "unaffected, undemonstrative, true and lovable" as a friend, and recognised him as a "commonsensical and versatile genius" (p. 19) seems to me to have been astonishingly unperceptive in referring to him as "a rolling stone intellectually"(p.18). On the contrary, Phillips' work in economics and econometrics was all-of-a-piece, and stemmed from his profound sense of the potential social importance of macro-stabilisation policy. If he seemed to lose interest in that topic in the mid-1960s, that might have been partly because he was disillusioned at the way in which his important contributions were being ignored, while the "quick and dirty" 1958 piece was attracting so much attention, but it was surely also because he could neither see any way of carrying his work further forward in a useful direction at that time, nor had any interest in playing intellectual games for their own sake.

Phillips was, as we shall see, working on problems that required the estimation of continuous-time dynamic models in an era when the state of the econometrician's art extended only (and only just) to systems of simultaneous equations in discrete time, and when most empirical work even in leading U.S. universities was still carried out with electric-mechanical calculators. It is small wonder that even a man of his quite extraordinary dedication should conclude by the mid-1960s that he had carried the task he had set for himself as far as was then feasible. His decision to move from London to Canberra in 1967 and begin serious work on the economics of China looks to me much more like the act of a profoundly honest man who wished to continue to earn his living by doing the most socially useful work of which he was capable, than an intellectual retreat on the part of someone with a short attention-span. Phillips had, after all, learned speak and read Chinese while a prisoner of war, and not many China experts then or now combine this skill with capacities as an economist on Phillips' level. He might even have returned to his original line of enquiry as computing technology rapidly improved in the 1970s, but we cannot be sure. Though Bill Phillips lived until 1975, he never recovered from the

crippling stroke which he suffered in 1969.

The key to understanding Phillips' research agenda lies in two biographical facts: just before the war, he had qualified as an electrical engineer; and just after it he took a degree in sociology at the London School of Economics.

As part of the latter program, he had to study some basic economics. He encountered the then rather new, but already almost standard, IS-LM interpretation of Keynesian economics. That model's intellectual dominance arose from its rendering technically tractable a key sub-set of the problems with which economists had struggled in the 1920s and '30s. It had done so by reducing complicated dynamics to comparative statics, but the simplicity of IS-LM was deceptive, hiding all manner of difficulties, not least in the area of stock-flow interaction. For Phillips, there were obvious (because he was a genius) parallels between this model's configuration as well as the macro-stabilization issues its users wanted to address, and the continuous time dynamic systems and the control problems analysed by electrical engineers.

With some advice and encouragement from his fellow student and friend Walter Newlyn (see pp.31-38), who would in due course make a distinguished career in the Economics Department at the University of Leeds, he set about designing a machine in which water flowed through transparent pipes and/or gathered in reservoirs, to demonstrate the macro-economy's properties. He described this machine, and the economic interpretation of its workings, in his first publication (ch. 10). There had been analyses along such lines before, complete with diagrams - see for example Foster and Catchings (1923) - but Phillips actually built his machine, and it worked too, give or take an unfortunate tendency to spring leaks, that had nothing to do with its basic design (See Elizabeth Johnson p. 23). He thus constructed a working physical representation of what Alan Coddington (1976) would later call "hydraulic Keynesianism".

In those times, universities were still run by senior academics rather than professional administrators, and so it came about that, when Lionel Robbins' attention was drawn to Phillips' astonishing accomplishment in 1950, that recent recipient of a pass degree - the lowest non-failing grade that could be awarded - in sociology was appointed to the faculty of what was

arguably already, and certainly soon to become, Britain's premier economics department. By 1958 Phillips had been appointed to the Tooke Chair, earlier held by Hayek, though his inaugural lecture (ch. 22) was not given until 1961. This was to be his last paper to appear in a journal (*Economica* 1962), and his second last publication, but in the intervening decade he had helped to transform for ever the way in which economists think about questions of stabilization policy.

Phillips' machine, though intended primarily as a teaching device, was also a pioneering analogue computer (See Doron Swade, ch. 14), which is why a working model now graces London's Science Museum, and it could not have been built if he had not had a crystal-clear understanding of the dynamics of stock-flow interaction in continuous time. Anyone who doubts that this in itself was a rare and notable achievement in 1950 need only recall that the debate among Patinkin (1956), Archibald and Lipsey (1958) and Clower and Burstein (1960) about stock flow interactions in and out of the steady state, which is the one still remembered as having finally tidied up the basics of this contentious topic, did not even begin till Patinkin's brief visit to LSE in 1957. But for Phillips, understanding the economic dynamics was but a means to a much more important end, namely the analysis of stabilization policy.

The first sentences of the three substantive articles (chs.16, 17 and 21) in which he extended the basic insights that had gone into constructing the machine, and developed their implications, make his intentions plain: "Recommendations for stabilising aggregate production and employment have usually been derived from the analysis of multiplier models, using the methods of comparative statics. This type of analysis does not provide a very firm basis for policy recommendations. . ." (1954, p.134) "In an earlier article I used a number of dynamic process models to illustrate the operation of certain types of stabilisation policy." (1957 p.169) "The purpose of this article is to develop a simple aggregative model that may be used to study both the problem of reducing short-period fluctuations of an economy and the problem of attaining longer-term objectives relating to employment, the price level and growth" (1961 p. 195) These articles do not deal with questions of optimal control, as the title of Adrian Pagan's otherwise exemplary introduction to the first two of them misleadingly suggests, but with questions about how what we would now call policy reaction functions might be configured in order to ensure that their addition to the system would make it more, rather than less prone to

fluctuate. It was only later, in the 1960s, that Phillips became explicitly concerned with optimal control, as Pagan (p.131) does indeed make clear.

Be that as it may, the broad results of this work of Phillips are commonplaces now, but to those exposed to them when they were first developed, who were used to analysing monetary and fiscal policy problems with IS-LM comparative statics, they were astounding. Phillips' summary of his conclusions for a general audience, presented in the first part of his 1961 inaugural lecture (ch.22) may be paraphrased as follows: The implementation of stabilisation policy is subject to time delays and lags, and so is its influence on the economy; if that policy is to help, rather than make matters worse, the time form of its responses must be carefully calibrated to the dynamics of the economic system that is to be stabilised, because even quite small errors in such calibration can make all the difference between success and damaging failure; the policy tool least likely to do harm is one that can be implemented quickly and will have rapid effects on aggregate demand; and the most dangerous is one that has large effects that appear only after a long delay. So certain kinds of fiscal policy, for example "a general sales tax . . . adjusted by small amounts at frequent intervals . . . would do the job" (p. 217); but much more quantitative knowledge than was available in 1961 was required to make the world safe for activist monetary policy.³

Now Phillips was not the only one uttering such warnings about monetary policy at that time. The similarities between his and Friedman's (eg. 1960) views are obvious. This is not entirely a co-incidence, for Phillips and Friedman met and had extensive discussions in 1952, during the latter's visit to the U.K., when, among other things, he had suggested to Friedman the adaptive expectations formula that would in due course appear in Philip Cagan's Ph.D thesis, later published as his famous (1956) article on hyperinflation. (See Cagan, p. 22). But there is an all important difference between their approaches, nevertheless. Phillips did not share Friedman's

³And because the world operates in continuous time, the quantitative information had to be obtained by methods that would yield the same numerical values for the parameters of the economic system regardless of the time intervals separating the observations used in the estimation process. Note that ordinary discrete time methods will not do this trick, as anyone who has contemplated the results of aggregating an equation with a lagged dependent variable fitted to, say, quarterly data, up to annual observations soon discovers. See Y. Mundlak (1961) for a contemporary discussion of this specific question.

belief in the inherent stability of the market economy, so where the latter sought to limit the harm that monetary policy could do by advocating a legislated constant money growth rule, Phillips canvassed the altogether more difficult alternative of seeking quantitative information sufficiently precise to enable it to do good by implementing well configured policy reaction functions.⁴ Hence his interest in the problems of estimating the parameters of continuous time dynamic systems, not to mention his concern, in his final unfinished paper (ch. 52) , with the influence of subjecting such systems to policy controls on the subsequent identifiability of those parameters.

The Phillips Curve

Phillips' work on the infamous curve that still bears his name was nevertheless related to his overall agenda of macroeconomic research. Feedbacks from output to price level variations and hence to the stock of real balances were integral to the model presented in his 1954 paper on "Stabilisation in a Closed Economy" (ch.16) which derived from his Ph.D thesis, and their modelling there represented a considerable advance over the crude treatment they had received in his hydraulic machine. Indeed, figure 16-11 of this paper (p. 151) is an analytic version of the Phillips curve drawn in price inflation output space. This relationship is also given a lengthy theoretical discussion in the text, as William Baumol (pp. 285-286) points out. It is thus a myth that the Phillips curve was a purely empirical relationship, devoid of theoretical foundations.

The curve was presented in (1954) as an adjustment equation describing the out-of-equilibrium behaviour of the price level, not as a structural equation of the steady state system, and that is also how, with suitable substitutions of variables, its money-wage unemployment version would also be presented in (1958), albeit quite tersely. The idea of the curve came from Phillips' reading of Bent Hansen's (1951) *Theory of Inflation*, but, though this is not how

⁴The reader's attention is drawn to the considerable confusion created in modern discussions of these issues by the use of the words "rule" or "rule-guided" to characterise both approaches to policy, despite the fact that they are very different from one another. This confusion also underlies the classification by some historians of monetary economics of anyone who ever said that the rate of money growth should be held steady as a "pioneer monetarist". The point about Friedman's rule was that it was to be legally binding.

Phillips ever put it, at least to my knowledge, it amounted to a formalization of the quite conventional price-output dynamics that underlay, for example, Patinkin's (1948) discussion of the real-balance effect. This perhaps explains the extreme hostility, noted by Lipsey (p. 237), displayed to the Phillips curve by such Cambridge Keynesians as Richard Kahn.

It seems to have been Henry Phelps-Brown who suggested to Phillips that available data on money wages and unemployment might throw some light on the empirical content of his adjustment mechanism, and recently published scatter diagrams in A. J. Brown's (1956) *Great Inflation*, with which Phillips was familiar (Brown, p. xiii), must have also pointed in this direction. There is nothing surprising, then, in Phillips, whose innate curiosity was legendary, having devoted a metaphorical weekend to looking into the possibility. But where Brown had simply drawn the scatters, Phillips fitted a curve, and superimposed it upon them: an apparently stable empirical relationship, capable of formulation as a simple inverse function, was thus called into being, and in an intellectual milieu at the LSE where, under the influence of Karl Popper, empirical testing was *de rigueur* among the younger faculty.⁵ Lipsey (1961) followed up Phillips' study, and the subsequent history of the curve in mainstream economics needs no further elaboration here.⁶

It is, however, worth noting that Phillips followed up his own work in his own way. A subsequent paper dealt with Australian data, where, tantalisingly, real rather than money wages were the focus, and some preliminary work was clearly done on U.S. data too, for he referred to it in his inaugural lecture in 1961 (p.222). Phillips himself never presented the curve as a policy menu, but he was clearly aware that it could be interpreted that way, and might be treated as such by governments. That is why, when considering the implications of his work for the international monetary system towards the end of his inaugural lecture, he suggested that a “. . . limited degree of exchange rate flexibility would allow each country time to find by trial and error that

⁵This activity was centered on the famous seminar on Methodology, Measurement and Testing. Its deliberations over Phillips paper are discussed here by Holt (pp. 310 -311). De Marchi (ed.) 1988 is the standard source on Popper's influence on empirical work in economics,

⁶Complementary accounts of these subsequent developments are to be found in Laidler (1997) and Leeson (1997)

compromise between its internal objectives which was consistent with its exchange rate policy” (1962, p. 223)

But as that same lecture also made clear, what interested Phillips about his curve was not what it revealed about an inflation-unemployment trade-off that might be exploitable for policy purposes, but what it revealed about the unemployment rate that would rule when the economy was in an inflation-free steady state. And as a number of contributors to this volume note, in discussing the factors determining this unemployment rate, and what might be done to reduce it should it turn out to be uncomfortably high, as he thought it probably was in the U.S., he came very close to anticipating many a much later discussion of the determinants of the “natural” unemployment rate and how to reduce it.

Phillips’ Influence

Though Phillips is nowadays mainly remembered as the author of the 1958 paper that he himself thought rather peripheral to his main line of enquiry, the ideas which he found really important had considerable and visible influence for a while. In the 1950s and 1960s, the computational burdens of implementing his agenda must have seemed insuperable, but, by the early 1970s, the technology available to academics had advanced sufficiently to permit his erstwhile colleague Rex Bergstrom, along with their student Clifford Wymer, to estimate what was clearly a development of Phillips’ 1961 model of growth and cycles using United Kingdom data. This paper, the outcome of research largely carried out at LSE, was not published till (1976), but it was complete at least as early as 1974. It pioneered the application of continuous time estimation techniques to complete macroeconomic models, in which all the cross equation constraints implicit in the stock flow interactions that lay at their heart were observed and exploited; and the dynamics of various policy feedback rules could also be investigated by simulating the resulting system.

The empirical modelling of stock flow interactions that these techniques permitted turned out to be well suited to the then novel “monetary approach” to balance of payments analysis which was the focus of a research programme led by Harry Johnson at LSE and Alexander

Swoboda in Geneva. A substantial literature soon developed out of the adoption of the Bergstrom-Wymer techniques by economists associated with this programme. It dealt with the analysis of monetary policy issues particularly, though not exclusively, as they arose in open economies, and among its highlights were papers by Jonson (1976), Jonson, Moses and Wymer (1976), not to mention Gandolfo's substantial (1981) monograph. For a while it seemed to some - this reviewer included - that this work would have a serious and permanent impact on mainstream macroeconomics.⁷ This was not to be the case, and with hindsight I conjecture that this was for the following reasons.

To begin with the specific models spawned by this literature turned out to be extremely fragile in the face of new data, and relationships that had seemed stable when fitted down to the early 1970s, collapsed as new observations were generated and added to the sample.⁸ Demand for money functions everywhere began to shift as a result of inflation-induced institutional change, and this was bound to undermine the stability of any empirical macro model in which a key factor driving expenditure flows was a discrepancy between stocks of money supplied and demanded. Closely related, the still unexplained productivity growth slowdown of the early 1970s shifted the steady state growth paths relative to which these models' endogenous variables fluctuated, and at the time it occurred, this slowdown was not even perceived for what it was, let alone successfully modelled. In any event, the gross empirical failure of an initially promising class of models was sufficient to ensure that any agency looking for policy guidance from them, as a number of central banks were doing in the 1970s, quickly lost interest in them.

⁷There were many close contacts between the Inflation Workshop at the University of Manchester which Michael Parkin and I were running at this time, and the LSE groups. And both of us, not to mention Wymer, were subsequently visitors to the Reserve Bank of Australia, into whose research agenda Peter Jonson introduced modelling of the Bergstrom-Wymer type upon his return there from London in 1975.

⁸Monetary instability, which had been latent in United States policies towards financing the Vietnam War and the War on Poverty, had been contained for a while by the workings of the Bretton Woods system, but it came into full view when that system broke down in the early 1970s under the stresses that those policies had created. That is how, in my view, the inflation of the '70s and '80s began. It had nothing to do with attempts to exploit the Phillips curve.

Perhaps more important to the demise of this extension of Phillips' original research agenda, however, was the failure of his work, apart from that wretched (1958) curve, at any time to penetrate American thinking about macroeconomics. In the early 1960s, when his ideas about stabilization policy were new, the major item on the policy agenda in the United States was the Kennedy-Johnson tax cut, which was supposed to be an exercise in demand management. A body of analysis that demonstrated the patent absurdity of using for such purposes measures that required 18 months or more to deploy was hardly going to be popular among their supporters, while their soon-to-be-called "monetarist" opponents had little interest in following up a body of work whose ultimate aim was nevertheless to render stabilization policy feasible and effective. Furthermore, as Lipsey (pp.236-238) documents, many and various, and sometimes quite extraordinary, misinterpretations of the Phillips curve are ubiquitous in the American literature of the 1960s, and it is hard to believe that the often distinguished economists who perpetrated them could have been familiar with the wider body of Phillips' work, or if they were, had taken the trouble to digest it.

And then, along came New-classical economics, an approach to macroeconomic analysis that was quite antithetical to any work in the tradition to which Phillips' had sought to contribute. New-classical economics was, above all, about establishing the primacy in macroeconomics of maximising economic theory over empirical evidence, and with re-configuring on a new basis the monetarist case against stabilization policy. It was not about improving the empirical modelling that underlay such policy. The critical factor that its exponents stressed in making the case for their approach, and hence in attracting attention and followers, was the allegedly spectacular failure of an apparently well-established empirical relationship, namely, and ironically, the Phillips curve, which was demonstrably inconsistent with maximising premises, when it had, again allegedly, been made the basis for policy. And the potential audience for this case had already been rendered receptive by the fact that from the 1950s onwards maximising theory had demonstrated a considerable capacity for improving the component equations of standard macro-econometric models.⁹

⁹On all this, See Lucas and Sargent's (1978) polemic "The End of Keynesian Economics" To say that this article's presentation of the history of macroeconomics prior to the arrival of New-classical ideas is inaccurate would be to put it mildly, but what is important to note here is

What matters here is not whether the success of New-classical economics in the 1970s was deserved, but that it occurred, and more particularly that one of its key components, the “Lucas critique”, quickly became close to conventional wisdom. It was essential to Phillips’ approach to policy that the parameters of the system to be controlled remained independent of the rules being followed by those controlling it, but the Lucas critique had it that, in general, the properties of a macroeconomic system would be highly unlikely to remain invariant to the rules guiding stabilization policy. Hence, its widespread acceptance transformed Phillips’ hitherto progressive research agenda into a theoretical dead-end.

In 1968, in his final and unfinished paper (ch. 52), Phillips himself came very close to developing the Lucas critique, as Robin Court (ch. 51) argues. Specifically, he showed that once a system was subject to control through a feedback rule, “observations during the period of control cannot be used to obtain improved estimates of parameters, which is a serious drawback” (p. 486). But this is not quite what Lucas later argued. Though Phillips’ result was extremely damaging to his own agenda, its basis was purely *econometric*. It was not derived from the fundamentally *economic* premise, namely that maximising agents will gather information about any policy rule and then act upon it, which underlay Lucas’s conclusions. This absence of any attention to maximising behaviour was not just a characteristic of Phillips’ last paper, but of his whole *oeuvre*, and in this his *oeuvre* reflected the style of macroeconomics of the 1950s.

There is nowadays a distinctly perjorative overtone to the phrase “hydraulic Keynesianism” which Phillips’ work inspired. Had it been in use in the 1950s or ‘60s, it would not have carried any such connotation, however. This is an accurate indicator of the extent to which New-classical ideas have changed the way all of us think about how macroeconomic theory should be done.

This observation should not be read as implying that I regard the triumph of New-classical macroeconomics as an unequivocal blessing. Those sound micro-foundations don’t

that it was nevertheless found persuasive by a large audience. The question of the article’s accuracy must be taken up at another time. But see fn. 7 above on the role of the Phillips Curve in bringing about the inflation of the 1970s.

seem to impose many restrictions on aggregate behaviour once one takes aggregation over heterogeneous utility and production functions seriously, and I can't think of any reason why one should not do so, particularly in the light of the empirical difficulties that the New-classical agenda itself has encountered over the years. While we wait for further developments in macro-theory, moreover, we still need quantitative policy-relevant information on the workings of the economy, and much of what we have these days seems to be based on empirically established regularities of a type that Phillips would have felt at home with.

Phillips was developing the ideas and using the vocabulary of error-correction mechanisms in the early 1950s, and as Peter Phillips (ch. 36) and David Hendry and Graham Mizon (ch. 38) tell us, it was his teaching, along with that of Dennis Sargan that inspired the development of many of the techniques that are now routinely used to estimate the functions that we use to describe the above-mentioned regularities. Furthermore, the advent of inflation targeting in the 1990s has helped stimulate a now flourishing literature of the role of reaction functions that incorporate feedback rules in the conduct of monetary policy not to mention their stabilising properties. Phillips would easily have recognised the relationship of such work to his, and from time to time, some contributors to the modern literature explicitly acknowledge its existence, though perhaps not often enough.¹⁰ In short, if those of his papers that Bill Phillips himself considered important have always been cited much less frequently than the 1958 *Economica* article, that does not mean that the ideas they developed have been less influential in the long run. The overarching scientific apparatus that he envisaged as the end product of his work would have been an estimated model with control mechanisms fully integrated into its structure, which would have been continuously re-estimated as new data accumulated. Such a model has proved impossible to build, but a number of the components that Phillips thought essential to its construction have turned out to be extremely useful, not to say durable, in their own right.

¹⁰For example Levin, Wieland and Williams, economists at the Board of Governors of the Federal Reserve System, note that their (1999) paper on “Robustness of Simple Policy Rules under Model Uncertainty” is in “the long distinguished tradition dating to Phillips (1954)”, but this is the only reference to Phillips’ work in the entire volume, entitled *Monetary Policy Rules*, in which their paper appears.

Concluding Comment

Historians of economic thought know that citation counts are a very imperfect measure of an economist's influence. Imagine, for example, what Ricardo's might look like were he cited every time someone mentioned comparative advantage, or Walras' were he cited every time anyone mentioned general equilibrium, or indeed on a smaller scale, what Bill Phillips' own count might look like had he written up the adaptive expectations idea and published it in a journal, instead of simply passing it on to Friedman and Cagan. Even so, Phillips' personality mixed creativity and unselfish modesty in such an unusually high ratio that his contributions have long needed a little help to find their proper and important place in the history of our sometimes unpleasantly competitive subject. It is as fortunate as it is just that his work has found a capable and dedicated advocate in Robert Leeson. We are all in his debt for this volume.

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