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HARRY JOHNSON ON THE PHILLIPS CURVE

BY
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It is noted that Harry G. Johnson was widely admired for his broad knowledge of economics, and particularly for the excellence and synthesizing quality of much of his writing. His discussions of the Phillips curve and related matters are considered. It is found that they are brief, inaccurate, and inconsistent. It is clear that, despite his reputation, they should not be treated as authoritative. It is further suggested that rather than supposing that Johnson's knowledge and capabilities have been grossly exaggerated, it may be better to conclude that the Phillips curve was not nearly so important in the literature of the 1960s and 1970s as has been supposed.

Nor is it Homer nods, but we that dream
Alexander Pope, *Essay on Criticism*

I. INTRODUCTION

Although even just after his death, the details of the work of Harry Johnson (1923–1977) were perhaps not greatly remembered, during his lifetime his work was much admired. He ranged widely, and his most original contributions were in international economics, but he made noted contributions in monetary economics, and some of these also incorporated discussion of the Phillips curve. In what follows, I consider the most important of these comments and suggest that they were not as numerous, substantial, or well informed as conventional wisdom about the Phillips curve and admiration for Johnson suggest they should be. It would be possible to see this as suggesting a qualification of the extremely high regard in which his work has been held. In part, that may be what it is. The larger point, however, is that it should be seen as supporting

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the view that the story of the immediate impact of the work of Alban William Housego Phillips (1958) and its pervasive influence on research and policy over the following decade is simply a myth.

II. JOHNSON'S INFLUENCE AND REPUTATION

One might guess at Johnson's influence simply from knowledge of the volume of his output. Vicky Longawa (1984) compiled a bibliography of over fifty pages, noting that it could not be considered complete. In all, it contains well over 500 journal articles and similar works, to go with 27 authored books and published lecture series, 25 more edited books, 14 pamphlets, about 150 book reviews, journalism, and another 20-odd "miscellaneous" items. The first of them was published in 1948, and Johnson died in 1977, so it all comes to a publication rate a little faster than one item every two weeks of his career. Certainly, there are similarities between some of these, but as Jagdish Bhagwati (1977, p. 226) noted, it was a rare case when he added no new wrinkle when writing on a familiar theme.

Considered responses to his work certainly confirm such a guess. Donald Moggridge (2008), in a thorough biographical assessment, found Johnson to be a most important and impressive economist; Roger Middleton (1998) noted his importance; and Roger Backhouse and he (2000, p. xvii) described Johnson's "dominant role" in British economics in the 1960s. Tributes after his death also made admiration for him plain. James Tobin (1978) said, "For the economics profession throughout the world the third quarter of this century was an Age of Johnson … [He] bestrode our discipline like a Colossus" (p. 443) and was "the people's choice within the profession" for a Nobel Prize (p. 457).

Tobin, like others, particularly emphasized Johnson's tremendous breadth of knowledge of economics and described him as "a master of creative synthesis," saying, "He could organize the confusing variety of ideas, findings, and approaches in a field into a coherent whole. The structure was his original design. An outstanding example is his survey article 'Monetary Theory and Policy' [i.e., Johnson 1962a]" (1978, p. 446).

In a similar way, Thomas Courchene (1978, p. S22) said, "It was as if he carried the collected wisdom of macroeconomics in some special corner of his mind. Whenever a new idea came to light, Harry would inevitably write a piece reviewing it, frequently correct and extend it, and always integrate it into the larger corpus of macro thought in order to highlight both its significance and position in the development of knowledge." And then he said of the 1962 paper Tobin had singled out that, amongst Johnson's surveys, it was "far and away the most important because in addition to synthesizing the various threads of monetary analysis it provided a most valuable blueprint for research" (p. S23).

Peter Oppenheimer (1980) said that Johnson "had a fabulous ability to synthesize in a brief compass a whole body of analytical work drawn from various corners of the subject, tidying up controversies, making order out of confusion and at the same time pointing the way to subsequent stages of investigation," and noted his "driving zeal to familiarize himself with all research relevant to his own thinking" (p. xi).

A little later, David Laidler (1984) commented in a similar vein. He began his appreciation by noting that much of Johnson's reputation rested on his survey articles,

and shortly continued, "His ability to synthesize and expound the issues being tackled and the results being attained in all areas of the field ensured that their significance was grasped by the profession at large far more rapidly and effectively than otherwise would have been the case" (p. 592).

In due course, Max Corden (2004) gave similar emphasis to that synthesizing talent, saying Johnson's papers on monetary economics were "brilliantly synthetic, with remarkable historical perspective," and that "[h]is many surveys in trade theory, monetary economics, and, to a lesser extent, other fields were highly influential in guiding the development of academic economics for more than twenty years. No student reading list was complete without Johnson surveys. In a sense, apart from his original contributions, he was a historian of current and very recent economic thought."

Later still, in the *New Palgrave Dictionary of Economics*, Jacob Frenkel (2008), who was perhaps Johnson's most significant co-author (on the monetary approach to the balance of payments) again singled out the paper "Monetary Theory and Policy" for high praise, describing it as "widely acclaimed as a masterpiece in scholarship."

All this is far short of a full assessment of Johnson's work or a representation of what has been written about it. On the other hand, it clearly does establish that amongst his many achievements, his wide appreciation of the literature and the synthetic quality of his surveys were widely recognized, and forcefully advanced by authors whose views on that matter must carry weight; and amongst these, his 1962 paper "Monetary Theory and Policy" was foremost.

III. JOHNSON ON THE PHILLIPS CURVE

When we turn to Johnson's work on the Phillips curve, however, a different picture emerges. After a couple of earlier mentions of the possibility of a relationship between inflation and unemployment,[1] Johnson's (1962b) first mention of Phillips's paper came when he listed it, along with works by Richard Lipsey (1960), Paul Samuelson and Robert Solow (1960), and William Bowen (1960), as having found that wages are downwardly sticky in recessions.[2] Even then, therefore, he did not mention Phillips in connection with an "inflation-unemployment trade-off" or anything of that kind.

His first discussion of the Phillips curve came in a survey of theories of inflation (1963a). In this paper, he twice (pages 39 and 55) described an idea sometimes called "demand-shift inflation," according to which inflation could be said to "lubricate" the labor market. The argument was that because of downward rigidities of nominal wages, if there were differences in the balance of supply and demand in different sectors or regions, there might be inflation even when the overall level of demand was just sufficient for full employment. This gives rise to an element of inflation-unemployment

[1]Johnson (1956, p. 19; 1958, p. 151) considered the possibility of reducing inflation by means of lower demand. In the earlier work, he suggested the cost in terms of unemployment would be high; in the later one, he suggested that Britain had in any case suffered from excess demand.

[2]Johnson's paper was a reprint, with a small number of substantive changes, of a lecture (Johnson 1961). Only the later version contained specific citations. Sylvie Rivot (2016) reconsidered the question of downward wage rigidity.

trade-off in that maintaining higher unemployment can reduce the number of excess demand sectors or regions and hence stop the price drift.

The first time he mentioned it, Johnson merely stated the argument. The second time, showing no sign of recognizing the repetition, he introduced it by saying, "Before leaving the cost-push versus demand-pull debate, it is appropriate to comment briefly on the alternative theory of inflation" (1963a, p. 55), which he said had "enjoyed widespread but brief popularity" (p. 55) after being advanced by Charles Schultze (1959).[3] Here, he made two criticisms of the argument: it was flawed by its failure to investigate the monetary circumstances of inflation, and by imprecision concerning the definition of full employment. Whatever the merits of those points, the claim that the popularity of the argument had been brief is bizarre—it continued to be favorably discussed well after 1963.[4]

Johnson then moved to discuss the Phillips curve.[5] He did not seem to contemplate the possibility that the demand-shift argument might account for a Phillips-type relationship, but presented the curve as avoiding the cost-push/demand-pull debate (1963a, pp. 56–57):

> While the debate over cost-push versus demand-pull was raging in the United States, a new and very interesting approach to the problem of inflation and anti-inflationary policy was developed by A. W. Phillips. This approach by-passed the argument over the cause of inflation.…
>
> The central contribution of Phillips' approach is to substitute an empirical relationship between the rate of inflation and the percentage of unemployment for the vague literary and judgmental arguments about how much reduction in employment would be necessary to halt inflation that had previously dominated the debate about economic policy.

Johnson was clearly ill-informed, since Phillips's paper was about wage change, not inflation. Certainly the two are related, and certainly Phillips saw that. But that does not mean that they do not raise separate questions. Even the title of Phillips's paper made his intentions clear. It was "The Relation between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861–1957"—but there are plenty of other indications pointing in the same direction.[6] Johnson's complaint about "literary and judgmental arguments" was a characteristic of his attitude, but the idea that Phillips was bypassing the cost-push debate is very strange. The level of unemployment was taken to be demand-determined, and Phillips's curve related that to wage change. The determining factor is therefore demand. To the extent that Phillips's analysis was taken to point to an explanation of inflation, it was demand-pull inflation that was diagnosed.[7]

[3]The issue was later analyzed by Norikazu Takami (2015).

[4]Forder (2014, ch. 3, part 5) considered the impact of the argument in the 1960s.

[5]He had given it a brief mention in the introduction (as the "Philips" curve), where he said it had had "considerable influence on the practical analysis of the inflation problem" (p. 33).

[6]Forder (2014, ch. 1) considered Phillips's intentions more fully. Apart from anything else, if Phillips had been principally interested in the relationship of inflation and unemployment, he would have estimated that relationship. It is perhaps implied by Conrad Blyth (1975, p. 306) that Henry Phillips lacked the data to estimate either relationship until Phelps Brown told him about the wage data in Phelps Brown and Sheila Hopkins (1950). That only makes my point more strongly. If Phillips wished to estimate an inflation-unemployment relationship, he would have been told about the price data in Phelps Brown and Hopkins (1955).

[7]Forder (2014) makes a fuller case that Phillips was analyzing demand-pull inflation.

Johnson also saw problems with the curve. First, he said (1963a, p. 56) that it could well be argued that low rates of unemployment were associated with unpredictable wage increase, whereas at high unemployment, wage change varies little with the level of unemployment. Then, he observed it was only a statistical relationship with "little general and well-tested monetary and value theory behind it" (p. 57). That was much the same point as he had made about Schultze's work, although he did not link the two. Furthermore, he said that the data for Phillips's work came from times when there were a variety of conditions prevailing, "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" (p. 57, with very minor typographical corrections). His idea was presumably based on the adjustment of expectations, or something similar. The argument would be that in the past, the variety of circumstances made such adjustment impossible or ineffective. But if policy sought to achieve a stable point on the curve, that would change.[8] Still, he said the curve "appears to be the most reliable" (p. 57) of relationships between various goals.[9]

Later in the paper, he moved on to matters more directly affecting policy choice, and criticized the view that there was no conflict between price stability and full employment, suggesting that it depended on defining full employment so as to make it seem true, and that there was nothing "sacred or commandingly desirable" (p. 63) about the price stability level of unemployment, considering the costs it involved. He said, "The argument for inflation as a means of securing full employment is an obvious one, though such a policy would soon encounter balance of payments problems in a country on a fixed exchange rate, and it might be ineffective in a country where labour is relatively immobile" (p. 64).

Here, the emphasis on labor mobility suggests the importance of different sectors or regions, and hence the lubrication argument, but Johnson did not say what the "obvious" argument was—and neither did he connect the discussion to the Phillips curve (or Schultze). There is, for example, no suggestion that the Phillips curve might provide information as to what level of inflation would be required to achieve any particular level of employment. He did, though, remark again on the point that policy could be undermined by the adjustment of expectations (referring specifically to the adjustment of nominal interest rates in circumstances of inflation).

Right at the end of the paper, he referred to the difficulty in choosing the "policy combination yielding the optimum mixture of growth, price stability, and employment" (1963a, p. 65), and said (p. 66):

[8]It may be thought this reads too much into Johnson's discussion. However, discussing the inflation-growth relationship earlier in the paper, he had considered the adjustment of wages to expected inflation (pp. 44–45). As I have shown (Forder 2014), the argument about expectations later attributed to Milton Friedman and Edmund Phelps was very widely known in the 1960s. When that is realized, it is apparent that Johnson's slightly allusive way of commenting was sufficient to make the point. Laidler (1984, p. 606) considered the earlier passage but still concluded Johnson had not appreciated the argument. He, though, was presuming the argument was generally unknown in 1963.

[9]The point, it should be emphasized, was not that Phillips's work was the best of its kind, but that there was better evidence on an inflation-unemployment relationship than various others, such as one between inflation and growth, that were, as noted particularly by Schwarzer (2014), widely discussed in the 1960s.

> For intelligent policy-making, in principle it is necessary first to be able to quantify the Phillips curve relation among objectives, and second to be able to attach weights of values to the numbers along the axes. For the trade-off between unemployment and price stability this can be done, though so far the only attempt to do it is to be found in a study conducted by G. L. Reuber.

In another slip, or perhaps adaptation of terminology, "the Phillips curve relation among objectives" would seem to be a trade-off relation between any variables, but in any case this appreciation of the work of Grant Reuber (1962) was more or less the end of the paper.

Despite Johnson's reputation for knowledge of the literature and clear synthesis, this account of the Phillips curve is badly arranged, and misleading in some details. That probably reflects the fact that the ideas were confused in his mind.[10] Even so, it is notable that the Phillips curve was a long way from being at the heart, even of a "survey of theories of inflation." This was, though, one of Johnson's most substantial treatments of the Phillips curve.

Reuber's work featured elsewhere, although sometimes in a different guise. Johnson (1968a) noted Reuber's introduction of the "reaction function,"[11] but on the question of the Phillips curve, he said that whereas Reuber had "significantly advanced" (p. 142) trade-off analysis using the Phillips curve, Edmund Phelps (1967) had shown that if it were to be used to optimize policy, the incorporation of consideration of expectations meant that the problem was one of comparing the discounted future losses due to rising inflation and the decreasing future gains from lower unemployment. That is an important point about Phelps that is often not perceived because he is frequently seen simply as a critic of the Phillips curve. Here, Johnson rightly saw Phelps as *developing* the trade-off approach (and made no mention of Milton Friedman).

That same year, he took something of a different view when he said of the inflation-unemployment problem that it had been understood for some time but that attempts to address it had been limited to proposals for reforms to make price stability and full employment more compatible, "until the nature of the choice involved was formalized in the concept of the 'Phillips curve'" (1968b, p. 985). He elucidated this by saying that the "simplest" form of the curve hypothesized a relation between unemployment and the rate of increase of wages or prices. He said that early empirical work had suggested a "surprisingly stable econometric relationship," but that subsequent research questioned both its theoretical foundations and statistical reliability (but he specifically cited nothing). He went on then, nonetheless, to assume the reality of the curve in the following discussion.

Moggridge (2008, p. 336) saw the discussion as an example of Johnson's synthetic and expository powers, but it is worth noting that, even having assumed the existence

[10]Questions might be raised about the whole paper, and perhaps it is notable that in the original publication, although not the reprints, Johnson described it as a "highly personal" treatment reflecting the author's judgments rather than "attempting to give fair representation to the contributions of various writers" (p. 29). The comment has the appearance of making an excuse for being slapdash.

[11]Reuber's policymaker reaction function estimated policy actions in terms of economic conditions. It was arguably the most important aspect of Reuber's work, disappearing from view when the Phillips curve became so fascinating (although Laidler [1997] was perfectly clear about it). Johnson saw its importance, taking up the approach immediately himself with William Dewald (1963).

of the curve for the purposes of the argument, he did nothing with it except question whether it led anywhere. He suggested society might be treated as choosing a point on the curve on the basis of a social welfare function, but this way of looking at it was "unfortunately rather empty of economic content" (p. 986) in the absence of an actual account of social preferences (which he thought hard to devise or agree upon). He then proceeded to observe that, in any case, such analysis rested on the assumption that the position of the curve did not depend on the expected rate of inflation, mentioning, without specific citations, Friedman and Phelps as having argued this assumption was incorrect and the curve could not be used "as a basis for secular policy-making" (p. 988). That, though, as he had previously made clear, was not what Phelps had said, and the point about the shifting curve—here attributed to Friedman and Phelps—had been made even by Johnson himself five years earlier.

A little later, Johnson again varied his account, saying, "In this context a major contribution to the theory of economic policy—in my judgement the only significant contribution to emerge from post-Keynesian theorizing—has been the 'Phillips curve'" (1970a, p. 110). There, he cited Phillips (1958) and described the idea as being that of a relationship between unemployment and other variables, on the one hand, and nominal wage increase, on the other. He said that an inflation relation could be deduced from it and—citing Reuber (1964)—that policymakers could be advised to choose a point on the curve. He noted that there were older doubts about it—without indicating where they had been stated—and said that they had been revived by Bernard Corry and Laidler (1967) and Albert G. Hines (1968), and then considered the issue of expectations. On that, he cited Phelps (1967) and Friedman (1968), describing the latter as the "most elegant" statement of the argument (1970a, p. 112). He then said that the most thorough empirical examination of the expectations issue was to be found in the papers of a conference from 1968: Stephen Rousseas (1968). He drew attention to the comment of Tobin (1968), which he said was "especially apposite" (p. 112 n60), and said that of two econometric papers, Solow's (1968) suggested, and Phillip Cagan's (1968) "does not effectively refute," the existence of a non-vertical Phillips curve. So, he said, "The outcome is a 'sophisticated' Phillips curve, based on a dynamic version of 'money illusion', which still offers a trade-off to the policy-makers, though its slope is steeper than that implied by the 'naive' Phillips curve" (1970a, p. 112).

That is a notable remark for three reasons. One is that Solow did not explain the finding in terms of "dynamic … money illusion," whatever that is. Another reason is that Johnson appears to have adopted the view that the Phillips curve was non-vertical, but to have offered no reasonable theory that might explain that outcome.

Either of those oddities might be understood by reference to Tobin's comment. In that comment, he described a version of the lubrication argument, similar to that of Schultze (1959), along with various points about wage stickiness, suggesting the importance of the idea. That would provide a rationale for a non-vertical curve. Tobin also said that the Phillips curve idea was "in a sense a reincarnation in dynamic guise of the original Keynesian idea of irrational 'money illusion' in the supply of labor" (1968, p. 51). Whether that is a good summary of matter is questionable, but it does seem likely that it was this remark that led to Johnson's way of putting it; and that it was Tobin's argument he had in mind as the theoretical explanation of Solow's results —although he did not say so. The fact that the argument was similar to the one that, when put by Schultze, Johnson dismissed is also notable, but his approval of Tobin's version is clear.

In terms of Johnson's understanding of the Phillips curve literature, the third point is more serious. It is not correct that the "sophisticated" curve was steeper than older ones, since those older ones nearly always had price change as a variable explaining wage change, and it tended to have a coefficient much like that on "expected inflation" in Solow's paper, so the slope of the curves was very similar.[12] On this point, Johnson seems to have been seriously misinformed (as perhaps was Tobin).

From there, Johnson continued by saying that recognition of the Phillips curve had prompted governments to use incomes policy to change the relationship. That is perhaps in contrast with what he said elsewhere (1968b, p. 985) to the effect that it was *before* the recognition of the Phillips curve that the inflation-unemployment issue had been tackled in terms of "institutional reforms designed to increase the perfection of competition in the goods and labor markets of the economy" (although he did not actually mention incomes policy). In any case, as Roger Backhouse and James Forder (2013) argued, the adoption of incomes policy, certainly in the UK, had nothing to do with any recognition of the Phillips curve.[13] Still, Johnson cited Lipsey and Michael Parkin (1970) as suggesting that previous studies had been misleading and incomes policy was actually harmful, and ended his discussion, seeming to approve their work, but without further conclusions.[14]

In a lecture course for graduate students at the London School of Economics, later published as a textbook, Johnson (1971a) said more about the Phillips curve. He again referred to Tobin (1968), this time copying his mathematical model to explain the link between the adjustment of expectations and the vertical Phillips curve (pp. 162–163). His wider discussion must have left students bewildered, as the remarks on the curve are not well organized or well integrated into his discussion. Johnson presented the curve first (pp. 150–151) as "the Phillips' curve" and as a Keynesian attempt to advance the theory of inflation by rationalizing a link from aggregate demand and supply to wage and price change. He observed in a sentence that Laidler and Corry had criticized the approach (without giving a source), then twice noted that there was an issue as to whether it was best to think of wage bargaining in real or money terms, and tried to use the curve to elucidate the cost-push/demand-pull distinction. He then discussed the Quantity Theory, returning to the Phillips curve (without the apostrophe) on pages 156–163.

There, he said that attitudes to the curve had gone through three phases. One held that policymakers should aim at the level of unemployment that stabilized prices. The second was that "policy makers were held to make a choice along a set of indifference curves" (pp. 156–157). And, third, citing Reuber (1964), Johnson said that "policy makers have been assumed to get behind their trade off and quantify the social costs of both inflation and unemployment" (p. 157). And the curve, he said, was widely applied to policy discussion in the United States (but cited nothing).

[12]The points that Phillips (1958) did not take systematic account of price change and that Samuelson and Solow (1960) can be read that way create a contrary illusion. The facts as stated arise from a close consideration of the relevant literature described by Forder (2014, ch. 3, part 2; and p. 175).

[13]It is apparent from Goodwin's book (1975) in relation to the United States; Flanagan, Soskice, and Ulman's (1983) concerning Europe; and Beggs's (2015) on Australia that the same thing is true elsewhere.

[14]The Lipsey–Parkin work was, in the light of arguments made by Kenneth Wallis (1971), often agreed to be flawed, but that was after Johnson's piece was published.

The appearance here is that Johnson was making it up. Certainly, zero inflation had been seen as the proper goal, but that was not an idea that arrived with Phillips's (1958) work. Quite what the distinction between the second and the third phases was intended to be is not clear, but Reuber (1964) was in fact the first to put indifference curves together with a Phillips curve in fully published form, although his work was hardly noted at all (except by Johnson),[15] and certainly did not lead to much in the way of further attempts actually to calculate the optimal point on a Phillips curve.

Johnson then said that the "Quantity theory approach" assumed that people adjusted their demand for money according to expectations of inflation, whereas the Keynesian approach "and with it the Phillips curve" assumed "stable price expectations." That must have been intended to question the Keynesian position, although it is peculiar that the point was made by reference to the Quantity Theory, rather than expectations affecting wage bargaining—and all the more so in light of the fact that elsewhere in the same lectures (p. 96), he said that Friedman's account of the Quantity Theory was basically Keynesian. In any case, he moved to give a further brief description of the expectations argument, and said that Solow had argued that "[p]eople are rational and adjust to inflation, but they are subject to some money illusion" (p. 159). He did not say where Solow argued that, but seems to have had his 1968 paper in mind, since he again used an argument from Tobin's comment in the same volume, to remark on it. He then considered "Rees' contribution" (p. 160), presumably meaning Albert Rees's paper of 1970. If so, demand-shift inflation was a major part of the argument, but Johnson's discussion was too brief to bring that out. He merely had Rees supposing that real wages could be reduced by inflation. Then he said that the Phillips curve was useful in analyzing incomes policy, cited David Smith (1968), and mentioned Lipsey and Parkin as having made a breakthrough—but again without identifying the work (which must be their 1970 paper). The discussion ended rather abruptly on the ineffectiveness of incomes policy. In a "reading list" of more than twelve dense pages at the end of the book, the work of Phillips (1958) went unmentioned—as did that of Solow (1968), Tobin (1968), Smith (1968), Lipsey and Parkin (1970), and Rees (1970). Here, the impression is that the Phillips curve literature was not important enough for *any* reading to be suggested.

In an editor's introduction, Johnson (1971b) made a brief comment that the problem of inflation had been approached in terms of an empirical relationship "customarily described as the 'Phillips curve'"—here defined as a wage-change relationship. He said "the concept was first invented" by Phillips (p. ix). There is some appearance of these remarks' being hurried and really just intended to motivate the inclusion of papers on the Phillips curve in the volume. Nevertheless, the idea that Phillips was the first to think of the possibility of stopping inflation with higher unemployment is incorrect, and foolish.

[15]On the ISI Web of Science database, Reuber's paper is reported as being cited seven times up to and including 1968. Two of those are by Johnson, and two others are more concerned with reaction functions than Phillips curves. (Not all of Johnson's mentions feature in that database.) Laidler (1997) had the objective of finding the first presentation of the Phillips curve as a trade-off function. He correctly found it in Reuber, but the fact that it was possible to ask that question at that date shows the lack of impact of Reuber's work.

Elsewhere, at about the same time, Johnson compared the Keynesian revolution and the monetarist counter-revolution (1971c). He said that his specific concern was with the reasons for the "speed of propagation of the monetarist counter-revolution" (p. 3). The curve was mentioned only once, when he said that Keynesian orthodoxy (in contrast to John Maynard Keynes's views) sought to "explain essentially monetary phenomena in terms of a mixture of real theory and *ad-hoc*-ery, and specifically to explain inflation in terms of real effective demand and the Phillips curve" (p. 7). There was no hint there of the Phillips curve's making a case for inflation, although Johnson's meaning is opaque. It shortly became more so as he went on to identify the key determinant of the success of the monetarist counter-revolution as being that it "encountered a policy problem—inflation—for which the prevailing Keynesian orthodoxy has been able to prescribe only policies of proven or presumptive incompetence, in the form of incomes or guildelines policy, but for which the monetarist counter-revolution has both a theory and a policy solution" (p. 12). One thing the Phillips curve surely did do was provide a response to the problem of inflation: raise unemployment. If he thought the curve was part of the Keynesian orthodoxy, Johnson's position was incoherent.

In his de Vries lectures on "the monetarist controversy" (1972), there is a somewhat longer discussion of the Phillips curve, but there, Johnson deviated from his earlier judgment that the curve was the "only significant contribution to emerge from post-Keynesian theorizing." Instead, mentioning Phillips (1958) and Lipsey (1960), he said, "The evolution of Keynesian theory since the immediate postwar period has in fact produced only one significant contribution to monetary analysis—the Phillips curve, relating the rate of wage inflation to the level of unemployment." But this time it was "merely a contribution to empirical understanding," rather than theoretical understanding (p. 58).

He said that analysis based on the curve had become suspect as inflation rose, and elaborated this by saying that "ever since its invention," there had been good reason to wonder whether it illegitimately linked behavior at full employment with that of depression, and that it was based on simple theory, so that it "derives intellectually from a Hicks–Patinkin type of model with a constant nominal quantity of money, but simplifies that model for mathematical convenience into one in which price movements in a particular market are derived solely from the excess demand/excess supply position in that market" (1972, p. 59). That makes it all much too complicated —Phillips's idea was that wage change was determined by the supply and demand for labor.

Then, Johnson said there was a third point "of much less scientific but much more practical importance," which was that "the Phillips curve approach to inflation policy seems to have broken down completely in the face of the inflationary facts of the past two years or so" (1972, p. 59). This, he said, was "embarrassingly inexplicable" for *both* Keynesians and monetarists. It is an extraordinary remark, since he had so recently presented the Phillips curve as having led Keynesian orthodoxy astray and thereby providing an issue on which the monetarists had "both a theory and a policy solution."

At that point there was no mention of the possibility that changing expectations was part of the story—whether part of a monetarist story or otherwise—and Johnson moved the discussion immediately to "the rise of monetarism." The expectations issue and mention of Friedman's (1968) Presidential Address were there (1972, p. 65), and Johnson again cited the Rousseas volume before introducing what he called the "most recent empirical finding" (about the curve) for the United States: there was a relation "far less favourable" than the "original" (pp. 65–66). That was very much like what he

had previously said about Solow's (1968) findings. Here, though, he offered a different judgment about it, saying that finding was theoretically and empirically unsatisfactory (saying nothing specific about why), and that future work would probably look to information and adjustment costs—citing Rees (1970) again. Here, then, he suggested there was much more to that work than the idea that inflation could lower real wages.

The discussion, in 1972, of the most recent empirical finding in a context where nothing later than 1968 was cited is notable, but the same is true a couple of years later. Johnson (1974) presented the "original" curve as stating wage change as a function of unemployment, explained the expectations argument, attributing it to Friedman and Phelps, and again cited Solow (1968), Cagan (1968), and Tobin (1968) —and nothing later. Even by 1972, there was much more work of quality and importance he might have cited;[16] and even by 1974, Johnson seems to have been completely ignorant of it.

Next, note might be taken of a comment on Irving Fisher (1926). The facts are that Fisher estimated a relationship between unemployment and inflation, but the paper was little noted in the 1960s. It was brought to light almost simultaneously by Arthur Donner and James McCollum (1972) and the editors of the *Journal of Political Economy*—namely, Robert J. Gordon, Johnson, and George Stigler, who were so excited by the discovery that they reprinted the whole of Fisher's paper, and commented (1973, p. 496), "It is not generally known that the first statistical investigation of the relationship between inflation and the unemployment rate was performed not by A. W. Phillips in 1958 but by Irving Fisher in 1926." Here, it seems to be forced on the reader that Phillips was principally studying inflation; it is precisely the point being emphasized. And yet, as noted above, that is simply not correct.

Then there is Robert Nobay and Johnson (1977), in which, apart from advertising the reprint of Fisher's paper in 1973, the authors made no mention of the curve until they said that Friedman's Presidential Address to the American Economic Association (1968) "is known for its challenge to the Keynesian use of the Phillips Curve for the 'missing equation,'" and that the curve "gained widespread acceptance as the 'trade off' between prices and unemployment, through its use in this respect by Paul A. Samuelson and Robert Solow," but also that "careful reading of the original Phillips' [sic] article will confirm that he was aware of and did in fact 'test' for the distinction between real and nominal wages" (1977, p. 479).[17]

Again, Johnson was ill-informed. In that paper Friedman did not use the expression "missing equation" in this or any context, and neither did he describe the Phillips curve as specifically Keynesian. Friedman (1970) used that expression, but that was the first time. The claim about Samuelson and Solow became widely believed, but is not correct, as shown by Forder (2014, ch. 2, part 4). In any case, Johnson had never seen them in that role in his earlier work. If anyone, it was Reuber who should have been mentioned at that point, but he had apparently been forgotten. The attempt specifically to exonerate Phillips of error is novel in Johnson's work, but since no details of the "test" he conducted were given, it is hard to interpret.

[16]Widely noted examples would be: Solow (1969), Robert Lucas and Lionel Rapping (1969), George Perry (1970), Robert J. Gordon (1970), and Gordon (1971). Even if the de Vries lectures were written in 1971, most of those would have been available.

[17]They could better have said that a careful reading of Friedman (1968) would reveal that their interpretation of it was not correct, as is apparent from Rivot (2015), Forder (2016), and Forder (2018).

These, then, are Johnson's principal discussions of the Phillips curve. There are just passing references in other places, but none of them change the picture. Clearly, that picture is one of substantial ignorance and confusion on Johnson's part.

IV. JOHNSON'S LACK OF ATTENTION TO THE CURVE

A further matter is to consider particular occasions when Johnson said little or nothing about the curve, but where something, or more, might have been expected. There is, for example, nothing resembling a Johnson survey of the Phillips curve literature. Neither—remarkably, considering that it is normally thought to be the period of its greatest prominence in economic debate—does he seem to have made any significant mention of it between 1963 and 1968.

Equally, note could be taken of his treatment of Keynesian economics. Robert Dimand (2001) identified four works as Johnson's chronicling of the Keynesian revolution: Johnson (1962b, 1971c, 1972, and 1976). Considering what is so often said, including by Johnson, about the important role of the Phillips curve in Keynesian thinking, one would expect them to offer much discussion of it. Yet, of these, only the third—the de Vries lectures—has a discussion that could be regarded as substantial. Of the others, the first mentions Phillips's work only as one of four finding downward wage stickiness; the second mentions it only in connection with the alleged ad hoc character of Keynesian analysis; and the fourth, not at all. There are, then, two other of Johnson's papers post-dating 1958 that had a reasonably close connection to the history of the Keynesian revolution. One, "Monetary Theory and Monetary Policy" (1970b), contained no mention of the Phillips curve. The second was his paper with Nobay, considered above, although it was more about monetarism than Keynesianism. Counting that one—which said nothing clear and correct about the curve—there would then be something significant said in just two out of six papers where the subject matter was Keynesianism, or Keynesianism and monetarism.

Johnson's lack of attention to the Phillips curve can be seen in another way. In his de Vries lectures (1972), only Phillips (1958), Lipsey (1960), and the non-econometric Rees (1970) were actually cited from the Phillips curve literature (and the Rousseas volume was mentioned without any paper being identified). On the other hand, an endnote on the inflation and growth relationship (on pages 33–34) has seven references (two by Johnson himself). On the debate about the stability of the demand for money as compared with that of the multiplier, started by Friedman and David Meiselman (1963), there are two endnotes on pages 71–72, containing, in all, thirteen citations. Yet, as of 1971 or 1972, enormously much more work had gone into the estimation of wage-change equations than into either of those areas.

There is, however, a still more striking point of this kind. It comes from Johnson's survey "Monetary Theory and Policy" (1962a), so highly praised by Tobin and others. It contains no mention of the Phillips curve.[18] The paper's title might seem to suggest the curve was not relevant, but in "Recent Developments in Monetary Theory" (1963b),

[18]Frenkel (2008) seems to imply that Johnson (1962a) expressed skepticism about the Phillips curve. If that is what was intended, it is simply a mistake: Johnson did not mention it.

Johnson's idea of "monetary theory"—let alone policy—was broad enough to include a substantial discussion of the consumption function. More particularly, though, in the 1962 paper, Johnson specifically bemoaned the *lack* of quantitative assessments of the inflation-unemployment issue (p. 368). He said that there was a conflict of objectives in monetary policy, but that "there has been a tendency to evade the issue by denying the possibility of conflict or by insisting that conflicts be eliminated by some other means than sacrifice of the achievement of any of the objectives" (p. 367). Then he went on to say that when there had been a clear recognition of such conflicts, "the arguments about alternative compromises have been qualitative and nonrigorous; rigorous theoretical exploration and quantitative assessment of the costs and benefits of alternative compromises between conflicting policy objectives remain to be undertaken" (p. 368).

In light of this, it can hardly be argued that omission of mention of the Phillips curve is either the consequence of the focus of the paper or merely an oversight; Johnson specifically said there was nothing playing the role in which he and an enormous quantity of literature later put the Phillips curve.

Perhaps it might be suspected that the preparation of the paper was either too hurried or that it was complete so long before publication that Phillips's paper was not available to Johnson. As it happens, though, Moggridge (2008, pp. 242–245) reported that Johnson agreed to write the piece only after having established he would have the summer of 1961 available, and even so was late completing it. For someone who wrote twenty-seven books and more than 500 papers in about twenty-nine years, this one was certainly not quickly produced. That dating also makes it apparent that Phillips's paper was published before he started work, but more striking than that is this: Johnson's paper contains 130 references. Of these, 49 were published in 1959 or later, with a handful more 'forthcoming.' For all that I have said about the limitations of Johnson's accounts of the Phillips curve, that is ample testimony to his keeping up with the literature more generally. And it makes his omission of Phillips's 1958 paper all the more conspicuous.

It seems that this conclusion is inescapable: In the summer of 1961, Johnson either did not know about the Phillips curve, despite its supposed fame; had forgotten about it, and not rediscovered it in his research for his paper, despite finding so many other recent works worthy of comment; or else did not regard it as an assessment of the trade-off. Clearly, none of those is to be reconciled with the view that Phillips's work made a very rapid impact and was accepted as suggesting the existence of such a trade-off.

V. SOME PARTIAL DIAGNOSES

None of Johnson's accounts of the Phillips curve gives a satisfactory account of anything more than the most basic aspects of the issue, and they rarely do that. Even Laidler (2015, p. 3) said Johnson did not get everything right. But we need to go much further than that. The points made here, taken together, are not consistent in the view they present—they are not even nearly consistent. It is perhaps of interest to consider what might have been influencing what he wrote, even if that must involve some speculation.

To some extent, the temper of his remarks might be explained by aspects of his character and outlook. The fast writing, which was obviously a feature of much of his work, certainly resulted in the cutting of some corners; and some of his later work may have been affected by illness. His disparaging attitude to Keynesianism, and British Keynesianism in particular, is apparent in much of his work and well documented by Moggridge. The Phillips curve seems sometimes just to provide a vehicle for expressing that attitude, although in his more serious treatments of Keynesianism, it disappears. There are also perhaps signs of an inconsistency of attitude to Friedman—sometimes ignoring him, sometimes giving his work admiring emphasis.[19]

There is more to be said about his comments on Reuber's papers, since they arose from the Porter Commission (Porter 1964) on Canadian banking and finance, for which Johnson also worked. The first of them was a working paper for the commission; the second, the published version of part of it, appearing in the *Journal of Political Economy*, of which Johnson was editor at the time. The commission arose in part from a serious dispute, more fully described by Marcel Bélanger (1970), over monetary policy, which had seen the central bank appear to deny that counter-inflationary policy had any negative consequences for unemployment.[20] It was thereby led to insist on the absolute priority of inflation control. This attracted criticism from a large number of Canadian economists, as described by Scott Gordon (1961), who insisted that there was a policy "trade-off" in the sense that inflation control would inevitably be accompanied by an increase in unemployment.

Questions concerning inflation and "the trade-off"—in this particular sense—were therefore very much to the fore in Canadian policy discussion. Reuber's work for the commission was originally planned as an enquiry into the debate over cost-push and demand-pull inflation. In the end, though, as Laidler (1997) described, he gave much more attention to the estimation of the "terms of trade" between inflation and unemployment (or other pairs of objectives) and the costs of each. That change of approach seems to have been due to Johnson's influence, since, as Laidler noted, Reuber said he had been "instrumental" in giving his work its present "orientation." But we can see the effects of Reuber's views on Johnson as well. It was in the survey of "Monetary Theory and Policy"—written just before he went to the Porter Commission—where he had commented on the lack of such work. Then it was immediately after, and while commenting on Reuber's work, that he disparaged the "vague literary and judgmental arguments" that had preceded it (1963a).

Reuber's (1962) work, having the origin it did, was naturally concerned with the relationship of inflation, rather than wage change, to unemployment, but of course he surveyed the related work, including that of Phillips (1958) and Samuelson and Solow (1960), and he referred to "the Phillips curve" as an inflation relation, perhaps adopting that from the latter authors. And, naturally, Reuber's work did focus on the existence and terms of a "trade-off." These were the things that at least initially came to describe Johnson's idea of what the "Phillips curve" was all about. So, putting all this together, it seems very likely that Johnson learned of the curve, and learned to treat it as describing a trade-off between inflation and unemployment, from Reuber, in 1962.

[19]The general question of Johnson's attitude to Friedman is perhaps something to be explored further, with the work of Boyer (2011), as well as that of Moggridge, being relevant.
[20]For example: Bank of Canada (1958).

VI. CONCLUSION

Johnson's knowledge of the Phillips curve literature was extraordinarily limited. A single reference to the work of Corry and Laidler or Hines might suggest the tip of an iceberg of learning, but when those are recycled from paper to paper with so little else mentioned, any such impression is shattered. In any case, those two papers were both written to call Phillips-type relations into question, and so were not examples of the literature following Phillips at all. Johnson mentioned Rees (1970) a couple of times, but treated his work inconsistently, and there are his various mentions of work by Solow, Cagan, and Tobin, but they all come from the same rather obscure volume, and the oddity of the attention Johnson gave to it is emphasized by the fact that Solow reworked his analysis in what became a much better known piece (1969), which Johnson never mentioned. Apart from just a couple of appearances each for Lipsey (1960) and Samuelson and Solow (1960)—two papers that later authors on the Phillips curve have thought central to the development of the literature—there is then hardly anything other than his discussions of Reuber's work. Of about ninety econometric papers in the broad "Phillips curve literature" before Friedman's Presidential Address (1968) considered by Forder (2014, ch. 3), Johnson never mentioned more than a handful; of the work after it, he considered only the Rousseas (1968) volume; and, of the hundreds of papers that appeared in response to rising inflation in the early 1970s, sampled by Forder (2014, ch. 4), he seems to have known nothing. Furthermore, few as Johnson's sources were, a good portion of them might have come to his attention because of special circumstances, rather than from any genuine investigation of the literature. This is evidently true of Reuber. Laidler had been Johnson's research assistant, and the Rousseas volume may well have been given to him by Tobin—the mutuality of their respect for each other is apparent. As to Hines, Johnson (1971b) explained that his comments had excited interest at a conference he organized, and for that reason a paper from him was added to the published proceedings. Evidently, Hines had not been on the original program, and so it seems Hines's appearance at the conference was what brought his work to Johnson's attention. There really is no sign that Johnson ever thought it worthwhile to find out about the Phillips curve literature.

Furthermore, what he did say was often not correct and frequently inconsistent. Whether it be over Phillips's objectives; whether he was studying wages or inflation, the significance of the work of Phelps, or the influence of Reuber; or whether the curve was a theoretical contribution or merely an empirical one, Johnson was at sixes and sevens throughout.

One conclusion, then, is that Johnson's remarks on the Phillips curve certainly cannot be treated as authoritative. Within the bounds of reasonable scholarship, nothing about it can be established by an appeal to his authority, since, plainly, he never knew much about it.

Whether or to what extent Johnson's introduction of confusion over these things may explain the more general misappreciation of the literature described by Forder (2014) must be a more speculative matter. Clearly, the fact that his surveys were so highly regarded might suggest they were also influential. Generally, any such influence would be hard to trace, but there is one point where perhaps it can be more clearly seen. That concerns the use of the label "Phillips curve" for an inflation (rather than wage-change) relationship. That became commonplace usage in the 1970s, no doubt in part because of Friedman's (1968) adoption of it, combined with the slower-working

effect of undergraduate textbooks of the early and mid-1960s.[21] But in the first ten years after Phillips's paper, only Johnson frequently used the expression that way in the scholarly literature,[22] and the only econometric studies using that terminology were those of Reuber (1962 and 1964), and Robert Scott and John McKean (1964). Reuber's connection to Johnson is apparent, and, as it happens, Scott and McKean also thanked him for comments on their work. A particular role for Johnson in promoting this terminology is clearly a possibility—and perhaps he even influenced Friedman in this way, since they were both at Chicago at the time.

Another question is that of how much his poor understanding of the Phillips curve literature should lead to a general reassessment of Johnson's knowledge, understanding, and capabilities. Surely it must suggest something of that sort, but even the most extravagant claim based on the evidence considered here could not say much: the range of issues that I have raised is simply too narrow and gives no reason to doubt the quality of his surveys in international economics, or even the more purely monetary aspects of the works considered here. So, it might seem that all we learn is that even Homer nods—the greatest sometimes make mistakes.

If that is to be the explanation, then Johnson nodded long and often over the Phillips curve, but there is another response. The esteem in which Johnson was held is beyond doubt. If we consider the single most notable point in the preceding discussion—Johnson's denial in his survey "Monetary Theory and Policy" (1962a) that there were any quantitative estimates of the trade-off—a further point comes sharply into focus. That paper was the one that kept being singled out as the best of his synthetic writing. Rather than asking how it came about that Johnson omitted discussion of the Phillips curve, we should perhaps therefore ask how it is that, despite this omission, the paper was so highly regarded. The praise of that paper was no doubt based on recollections of it, rather than a close rereading at the time the tributes were written. But that makes the point of all the greater historical interest. When they initially read that paper in 1962 or later—four or more years after the publication of Phillips's paper—none of those authors thought that Johnson's denial of the existence of an estimate of the inflation-unemployment trade-off was a failing of the paper. They did not even allow that point to dent the exceptional praise they gave him and it.

So, perhaps we should note Alexander Pope's view. When Homer seems to nod, it is we who dream: we dream that he has erred because we have ourselves not understood. Homer only *seems* to nod. Johnson gave the Phillips curve so little attention because, as things were seen in his time, it deserved no more. Had Phillips's work been anything like the 'thunderbolt' alleged by Lipsey (2016, p. 418), that could hardly have been so. Phillips (1958) made little impact, and, in any case, was not concerned with estimating any "trade-off." Such a view, of course, is entirely consistent with the argument of Forder (2014) that the whole conventional story, so often told, of the history of the Phillips curve is no more than a myth that arose in the mid-1970s.

[21]The fifth edition of Samuelson's textbook (1961) used the expression "Phillips curve" that way. He confused the matter further by giving it a cost-push explanation. Other textbooks—particularly American ones—seem to have followed his. This and other matters relating to early textbook representations of the Phillips curve are considered by Forder (2015).

[22]Samuelson and Solow (1960) and Phelps (1967) both used the term in that way, although they both noted, somewhat apologetically, that they were adapting the terminology.

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