A response to David Laidler's review of Macroeconomics and the Phillips curve myth

A response to David Laidler's review of *Macroeconomics and the Phillips curve myth*, OUP 2014, by James Forder.*

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I. Introduction

I was of course pleased to see that David Laidler (2015), reviewing my *Macroeconomics and the Phillips curve myth*, recommended it as well worth reading, and accepted important points I made. One of those is that, as he put it, 'there is not a shred of evidence that macroeconomic policy anywhere attempted to exploit an inflation unemployment trade-off in the 1960s.' Another is that in the literature of the 1960s, expressions of the view that policy could be set on the presumption of a stable Phillips curve were very rare. From these points it follows not only that the big story economists tell themselves about the Phillips curve needs to be changed, but also that there are innumerable little bits of junk littering the academic literature and undergraduate textbooks, where pieces of a fictitious story are routinely repeated.

On the other hand, Laidler raised questions of what he called 'details' on which he found me at fault in various degrees. Concerning these points, I find most of what he said wholly unpersuasive, and none of it leads me to any significant change of view. In some cases, this is because Laidler has not understood the points I was making. Leaving those aside, the issues raised – interestingly, I think – are disagreements neither over bare facts, nor over simple 'matters of judgment'. Rather, they arise from considerations of the character of the enquiry underway and the sort of evidence that is required to establish conclusions about it. On each point where Laidler advances a view that is actually different from mine, he has made an argument or presented evidence that is in one way or another inappropriate to the conclusion he is trying to reach. He has the wrong *kinds* of arguments to make his case.

Much of what follows consists of an elucidation of this view and an elaboration on the points it raises. Some points concern questions about the assessment of evidence and bodies of evidence. Others come from the problem of identifying and keeping in mind the question of the intellectual content of a piece of work – that is, that aspect of a piece of work the author presumably intended, but in any case the generality of his contemporaries saw as, or perhaps should have seen as, advancing scholarship or research. On each point, though, the issue at stake is one of how we might construct effective arguments, or, alternatively, the bases on which we should reject ineffective ones.

These ideas, I hope, have an interest well beyond their leading to a response to Laidler's review. For one thing, they also form part of a response to de Vroey (2017), also reviewing my book, who at one point seems to adopt Laidler's views, whilst disavowing any intention to comment on them. And they certainly could be responses to many points about the book that have been made to me in other settings. More importantly, though, they are intended as ideas about the conduct of the history of economics, and whilst Laidler's review is the occasion of making these points, and whilst I do take the opportunity to respond to all his criticisms, it is a further objective to explore some issues concerning what should and what should not be counted as a worthwhile argument in the history of economic thought.

II. The burden of proof and the quantity of evidence

My book challenged what I regard as, and Laidler agreed is, a story very well established in economics. Plainly, that put me in the position of needing to make a powerful case. If I were merely to assert an alternative conclusion and illustrate it with some observations consistent with it, I could hardly expect to impress anyone, and neither should anyone feel I had somehow made it their job to collect further evidence to find out whether my conclusions happened to be correct. It would be perfectly within reason simply to disregard what I said.

So, one component of the conventional story – the 'myth', as I call it – has it that the econometric Phillips curve literature of the 1960s is full of inflationist conclusions. Seeking to argue that it was not, and accepting that the burden of proof was on me, I considered a large volume of literature. Specifically, for the period before any influence of Friedman (1968) became apparent, I sought to consider, in Chapter 3, every fully published, English-language econometric study of wage change (or inflation) where unemployment was treated either as one of its principal determinants or as a likely candidate to be one, even if the author's conclusion was that it was not. No one has yet suggested any I missed; and that was my idea of the 'econometric Phillips curve literature' of that period. I discovered that very few contributions to it reached clearly inflationist conclusions, and all the serious ones that did were Canadian.

Laidler, saying I was too polite to put it in such terms, inferred that by identifying them as 'Canadian', I meant to imply that they therefore lacked influence, and asserted that I underrated their 'significance for the development of macroeconomics'. To substantiate this, Laidler indicated the particular importance of a number of incompletely identified sources, which are Lipsey (1965), said by Laidler to be important partly on the basis that he was cited by Phelps (1967); Reuber (1962) and its fully published version, Reuber (1964), and the citations of his work by Harry Johnson; Bodkin (1962), fully published as Bodkin (1966); and Klein and Bodkin (1964).

Laidler was under some misapprehensions about what I argued. First, my observation about the Canadian studies (p. 75) related to the econometric literature, and Lipsey (1965) and Phelps (1967) were not econometric. Second, the point was not about Canadian economists, but studies using Canadian data (which Bodkin's was not, and Canada was not the focus of Klein and Bodkin's). Third, warmed as I am by having my manners noticed, my way of putting it had nothing to do with any supposition about the influence of Canadians. My point was that in Canada – uniquely, as far as I know – there had been public debate about whether reducing inflation would cause unemployment to rise even temporarily, with the Bank of Canada apparently advancing the view that it would not. That, I thought I had made clear (p. 80), might explain the different attitude of the authors studying 'the trade-off' in that country, and the interesting point was that absent that rather peculiar context, serious studies of 'the Phillips curve' contained practically no inflationism at all.

Still, the substance of Laidler's claim was that my conclusion was vitiated by the particularly influential character of these works, and that deserves comment. Some of the data he introduced is irrelevant. Phelps (1967) is one of the papers usually held to have put a stop to Phillips curve inflationism, so, although there is more that might be said (and I said some of it in Forder (2010b)), I do not really suppose Laidler wishes to treat it as a source of the same thing, and certainly not in the relevant period, which is, but for one year, before it was published. In addition to not being econometric, Lipsey's paper was also not inflationist, as I argued in Forder (2014, p. 245 n. 9) and Forder (2010a, p. 340), and Phelps did not say it was. I cannot see, and Laidler does not suggest any reason to think Bodkin's work particularly significant – indeed, as he notes, it was not published until 1966. That left it only two years to have any inflationist influence before – according to the conventional story – the whole deck of cards was thrown in the air by Friedman (1968). There is slightly more to be said for the relevance and importance of Klein and Bodkin (1964), but that is slightly more than nothing, and still comes to little (*vide* Forder (2014, p. 71))). So, that leaves Reuber and the citations of his work by Johnson.

Of Reuber's work, Laidler said it was undertaken for the Porter Commission (on Canadian monetary policy) – Porter (1964) – at the suggestion of Harry Johnson; Johnson then invited Reuber to present the work in Chicago; it was published in the *Journal of Political Economy*, of which Johnson was then an editor; and Johnson himself cited it at least twice. All those things are true, but the questions are whether and how they might amount to a case that Reuber's work was notably significant for the development of macroeconomics.

It seems that Laidler took the view that specifically Johnson's interest in the work is the important point. I suppose it might seem that his interest is either constitutive of a work's being important or indicative of it, or perhaps that it causes it. The first of those is egregious – it is not that work that interested Johnson is *for that reason* important in macroeconomics. The second possibility might be argued along the lines that Johnson had such fine insight as to what was important that the historian can reasonably rely on his judgment – if he thought it important, we may assume that it was. In the specific case in question, that idea can be rejected because, as I have now argued in Forder (2017), on the matter of the Phillips curve, Johnson was chronically confused and often inconsistent. In that area, any general supposition of his fine judgment needs to be suspended. The idea that Johnson's interest *causes* a work to be important is something to be further considered in the next section, but since all it does is suggest that the work might have become important, it is not, properly speaking, *evidence of* importance at all.

Supposing, then, that Johnson's name casts no magic spell over the argument, all Laidler has really done is to create an air of activity around Reuber's work. But an official origin, a seminar presentation at a major university, publication in a leading journal, and a couple of citations do not make a paper influential. Many inconsequential papers have a pedigree as good as that. To show a paper had 'significance for the development of macroeconomics' requires much more. Here, the burden of proof is on Laidler, and the declaration of an opinion, dusted with data consistent with it, does not discharge that burden.

More evidence could certainly be sought. One possibility would be to investigate how survey writers other than Johnson treated Reuber. Then we would find that his work was unmentioned in Bronfenbrenner and Holzman's (1963) 'Survey of inflation theory'. That, of course, might be because it was unavailable to those authors, although their references include 15 other items from 1962. On the other hand, Reuber was mentioned in connection with the trade-off by Laidler and Parkin's (1975) 'Inflation – a survey'. That mention, though, was in a footnote where Reuber's paper was listed as one of 12 studies of Phillips-type relations in 'other countries' (i.e., not the UK or the US), and was given no special emphasis. The paper's inflationism was mentioned nowhere by Laidler and Parkin. That would seem to be hard to explain if, in fact, this aspect of the paper were of much significance. (Laidler and Parkin quite rightly give Reuber more credit on another issue, but that is nothing to do with this discussion.) So, that small sampling of further evidence hardly advances Laidler's case. One cannot rule out the possibility that there is more evidence that would support his view, and if someone cares to collect it, then it should be considered. But, as things stand, that has not been done. As things stand, therefore, Laidler's case is inadequate.

III. An aspect of the quality of evidence

There is, though, a further and more fundamental point: as well as being too little in quantity to raise a serious case for Laidler's claims, it is also rather poor-*quality* evidence. This is because none of it does more than suggest conjectures about Reuber's influence – none of it is an instance or demonstration of that influence. The idea that Johnson's interest *might* cause a paper to become influential is in that category. Similarly, it is clearly a good possibility that something written for the Porter Commission might become influential as a result; or that, if presented in Chicago, it could well be that it becomes widely noted, etc. But these are all conjectures, and the necessity of making them to draw the desired conclusion is a weakness of the argument.

That sort of evidence, it might be said, is better than nothing, but even that claim must be handled carefully. The first question is whether there is better evidence. So, for example, there might have been a 'Reuber school', or a 'Reuber curve' closely associated with his analysis. That sets a high standard, but there could be a discoverable tendency to adopt his form of analysis of the trade-off problem, or an adoption by someone, somewhere, of his particular policy proposal as to what the optimal rate of inflation is; or perhaps even wide notice that he made a good case for that number. As far as I know, none of these things can be said of Reuber's work on the trade-off. Then, there are always citation counts. They raise hazards of their own, but it is possible to guard against them – for example, by discovering what the citing authors tend to say about the cited work, whether it is correct, etc. If we were to find that a large number of authors cite a work, appear to understand it, and approve of it, then there is clearly a respectable basis for regarding it as significant in some way that could be more precisely described in any specific case. By chance, I scratched the surface of that kind of analysis of Reuber (1964) in Forder (2017 p. 515 n 15), and that gave me no reason to think it an influential paper.

Beyond those sorts of things, there might be evidence of a kind that is specific to a particular question. In the case of the importance of Reuber's work in the following period, it seems to me that Laidler (1997), of which Laidler (2004) is a fuller version, offers such evidence – and evidence in support of my view. There, Laidler noted that various authors to whom a trade-off interpretation of the Phillips curve is often attributed did not in fact advance it, and set out to discover where the idea did originate. That origin he found in Reuber (1962) and (1964). He thereby successfully gave Reuber's work status as a curiosum. In making that case, he had no reason to address the question that is now at issue: If Reuber's was *influential* work, why was it necessary to have a research project to *discover* it? But here we are – the fact is that the work had sufficiently little impact for it to be a possible subject of such an enquiry in the 1990s. That is good evidence that I was right to regard it as unimportant in economists' understanding of the inflation-unemployment problem in the 1960s and 1970s.

The problem of assessing evidence can also be considering the light of another point arising from the question of Reuber's influence. Whereas Reuber's work is said by Laidler to be of significance because it was done for the Porter Commission, in the event, the Commission (p. 419) specifically rejected inflationism (as Laidler said, elsewhere in the review). That clearly shows the absence of inflationist influence from Reuber to the Commission. And this is high-quality evidence because it does not require weighty conjectures for its point to be apparent: It clearly and directly shows the absence of Reuber's influence over the Commission.

The point arising from these examples is that when higher-quality and lower-quality evidence are both available on just the same point, but seem to suggest different conclusions, the higher-quality evidence must take precedence. One has only to consider the hypothetical case where the Porter Commission had adopted Reuber's view. That would certainly be a sign of his influence. But in that case, we simply would not care who sponsored the work. The point would very probably not be made, and if it were, it would not be because it was thought to show his influence – that would have been achieved by the fact that his views were adopted. But then, equally, when they reject his view, the fact that they sponsored his work does not show his influence. We know what we need to know from their response. Here, Reuber had no inflationist influence, and the fact that he wrote for the Commission adds nothing to Laidler's case. Similarly, and more broadly, if we could find more people than just Johnson who were influenced by Reuber – those people who adopted his idea of the trade-off, or approved his analysis or whatever – then the fact that he presented it at Chicago and that kind of thing would lose its interest.

There is, then, a further point of some significance, not only in my rejection of Laidler's views but, I suspect, fairly broadly in arguments about the history of economics (and elsewhere, no doubt). The fact that high-quality evidence takes precedence over low-quality evidence on the same point also means that where both are available, the presentation of the low-quality part alone can never carry conviction. Evidence of different quality might be accessible at different times, or to different researchers, of course, and we must work with what we have. If, for some reason, it were impossible to discover what view the Porter Commission took of Reuber's ideas, then the fact that they asked for the work would create some conjecture-based, and not terribly strong, presumption that it influenced them. That would be something. But where, as here, higher-quality evidence *is* available, it really should be a rule of conduct that reliance is never placed on low-quality evidence on the same point. Low-quality evidence may indeed be better than nothing – if nothing is all we would otherwise have. But when better-quality evidence on the same point is available, the low-quality evidence is worthless. Then, it is not better than nothing. And, consequently, if reliance is placed on such evidence, the argument should simply be dismissed – a burden of proof can never be discharged by low-quality evidence when better-quality evidence is available. In this case, my limited enquiry into what better-quality evidence says suggests there is nothing to be said for Laidler's conclusion, but that is superfluous to the point that a case made in Laidler's way can never be persuasive.

IV The importance of the 'intellectual content' of works under discussion

Some pieces of work become highly cited because they are believed to be the origin of some particular idea. Amongst other things, that may sometimes provide a convenient labelling of the idea, but in the study of history, it can be an important matter whether that source is truly the practical origin of that idea. Sometimes, that sort of question leads to a precursor hunt where the point of interest is taken to be the identification of the earliest statement – however little-noted in its own time – of an idea, or something like that. A different issue, though, concerns whether the idea in question was an aspect of the intellectual content of a piece of work at all – that is, the question of whether it would have appeared to contemporaries (or the author) that the idea was original, or presented in a novel way or with innovative application. There may or may not be unknown precursors, but that is another matter. The intellectual content of a paper arises from what it is properly understood to contribute in the intellectual context in which it appears.

A case in point concerns Friedman (1968). That paper is the one often said, along with Phelps (1967), to be the origin of the idea that if there were persistent inflation, expectations would adjust so that wage bargaining would change, and the Phillips curve would then shift. As well as presenting complementary arguments in Forder (2010b), and later in Forder (2018), I argued in Forder (2014, pp. 81-89) that this attribution is incorrect. The expectations argument was very old news before 1968, and very familiar to economists of that time. A consequence is that that idea was not the (or part of the) intellectual content of Friedman's paper. That is not because there existing little-known precursors of his work, but because the idea in question was commonplace when he wrote.

Laidler accepted that the importance of expectations in the inflation process was much discussed, but said this was particularly because of 'Austrian' economics, which exaggerated the danger of the acceleration of inflation, and since, by 1967, there had been long experience of moderate but not explosive inflation, their argument was discredited. Consequently, he argued, there was scope for Friedman and Phelps to bring a different argument – an argument with a different intellectual content, as I am putting it – that described more narrowly the circumstances in which inflation would accelerate. For this reason, he felt I insufficiently credited them with originality.

This, I do not accept, and the point turns on the consideration of the intellectual content of the arguments. The 'Austrian' argument was that if prices were expected to rise, that would lead to a fall in the demand for money (or 'increase in velocity'), and hence inflation. Then, it might well be that the expected rate of inflation would increase, the demand for money would fall further, and the process could be explosive. This argument made it possible to think that very low rates of inflation would invariably or often lead to hyperinflation but, as Laidler said, that view was thoroughly discredited by 1967 or 1968.

If the 'Austrian' argument were exclusively the basis on which expectations of inflation were discussed in the period, Laidler would be right to say that Friedman and Phelps' ideas about expectations were new. But when I made the case that the 'Friedman–Phelps' expectations argument was widely known before their statements of it, I did it by citing numerous other statements of *their* idea. I knew about the 'Austrian' arguments, but by no means relied on them (and elsewhere (p. 121) I too noted they had been rejected). Perhaps I should have commented on the distinction, but in fact I simply ignored them. Overwhelmingly, the instances of the 'expectations argument' with which I made my case were versions of the argument that operated through the labour market and wage bargaining, or were broader than that, and none operated exclusively through the demand for money. The argument about wage bargaining was the argument I showed was well known, and therefore the idea of *that* process was not an original contribution of Friedman or Phelps.

So, Laidler is right to emphasize that the identification of the intellectual content of an argument is crucial. Consequently, one might say, the tendency some seem to have merely to look for key phrases, such as 'expectations of inflation', and presume that certain ideas must be associated with them, is intellectual nonsense. He is wrong, though, to suggest that I failed to realise this or failed to make my case, and wrong to say that the argument I was discussing was discredited during the 1960s. Consequently, his point makes no contact with my argument.

V. Exorcising the ghost of the conventional view

In all this, I suspect that a ghostly version of the conventional view of the matters has an influence it should not. The danger is that, for example, even after it has been accepted that the story of widespread inflationism is incorrect, it continues to seem to add plausibility to related assertions.

To illustrate the point, one might consider the question of how widespread Phillips curve inflationism would be explained if it were shown that it could be traced nowhere but to Reuber and a few others. In that case, there would be a question as to what explained his exceptional influence, and I suppose 'Johnson promoted Reuber's work' would, in the absence of anything better, seem to be a reasonable answer. But that is not the position. It is not a datum that someone, somewhere, was an influential Phillips curve inflationist. The question of whether Phillips curve inflationism ever had any notable influence at all is precisely what is at issue. So in fact, 'Johnson promoted Reuber's work' is just a piece of trivia.

Similarly, if we say to ourselves, 'we know, because it is constantly repeated, that Friedman and Phelps revolutionized thinking', and 'Forder shows that there was nothing new about the idea of expected inflation mattering', then of course it will be an easy ride to 'prove' that something else about Friedman and Phelps was very important. But that is not the position. It is *not* a datum that Friedman and Phelps revolutionized thinking.

In circumstances like these, error can creep in through failure to keep sufficiently in mind the point that when the conventional view is rejected, it is *dead*. In that case, it does not somehow convey plausibility to arguments about important exceptions or modified versions of itself. Such arguments, if they are to be made, have to be fully substantiated. There must be evidence sufficient to establish them without the benefit of a spurious plausibility provided by the ghost of disproven ideas.

I suspect the same problem, although showing in much starker terms, lies behind another of Laidler's criticisms of my argument. I considered (pp. 132–3) the survey of monetary theory and policy by Johnson (1962), noted that it made no mention of Phillips (1958), and said that it appeared from the evidence that Johnson had not heard of it, and that this in turn suggests that Phillips' paper had not made the large and instant impact often claimed for it. Laidler declared my suggestion 'implausible' and posited the alternative that Johnson was aware that

Bronfenbrenner and Holzman (1963) was being written at the same time, and omitted mention of the Phillips curve because he knew they would discuss it.

Limiting attention to the evidence Laidler considered, it seems that his argument leans heavily on a presumption of the impact of Phillips' paper. But that was precisely the matter in question, and my remark about Johnson (1962) was one piece of evidence amongst many I presented. It was 'one clue' to Phillips' lack of impact, as I actually put it (p. 132). That is not apparent from Laidler's review because he gave no indication that I offered other evidence for the same conclusion but, clearly, if we ask the question whether it is plausible that Johnson had not heard of a paper that is *known* to be famous, then Laidler's view might well be accepted. On the other hand, if that ghost is exorcised, and we start with carte blanche, it does not seem that Johnson's co-ordinating his survey with another, but giving his readers no indication of the omission from his paper, much less the reason for it, is more plausible than that he did not mention Phillips' paper because he did not think it important. Laidler's conclusion as to which explanation of Johnson's omission is more 'plausible' therefore rests on his presumption about the importance of the Phillips curve. He is not assessing my evidence, but explaining it in the light of the conclusions he has already reached.

That illustrates one aspect of the importance of recognizing the danger of assuming something that is in fact in contest, but on the specific case, it should also be noted that Laidler has presented a crucially half-baked version of my argument. What I said (p. 133) was that Johnson specifically bemoaned the *lack* of quantitative estimates of the inflation-unemployment trade-off, though when, in later work, he discussed Phillips (1958), that was what he said it offered. I suppose it is not to be argued that Johnson's hypothesized knowledge that something would be discussed elsewhere might plausibly lead him to deny its existence. So Laidler's idea must be rejected. (I understand as an incidental consequence of correspondence that Laidler now accepts the importance of this line of argument; meanwhile I have been led to regret the expression 'had not heard of' Phillips (1958) – the point is that Johnson did not think it important enough to prevent him denying the existence of quantitative work on the trade-off. The argument is more fully made in Forder (2017)).

Once again, though, the larger point concerns the handling of evidence – even ignoring Johnson's overt denial, Laidler prejudged the matter.

VI. The historical unimportance of Phillips (1958)

These ideas – on the relationship of established views and the burden of proof; the issue of the quality of evidence; the identification of the intellectual content of works under discussion; and the need to assess the evidence free of presumptions determined by the views under challenge – all feature in my response to Laidler's last substantial criticism of my book. The criticism is that I underrated the importance and influence of Phillips' own paper. I said it was a negligible paper, and except that Phillips' name happened to become associated with the idea of a relationship between wage change or inflation and unemployment, made more or less no impact at all on economics. Laidler described this as 'simply not true'. To substantiate this rather strong conclusion, he said,

It is not just that Lipsey and Samuelson and Solow followed it up in 1960, which facts Forder of course discusses. Rather, the point is that these papers were in turn widely read. Phillips' paper was still sufficiently well-known in 1963 [sic. should be 1962] that Bodkin (not published until 1966) could refer to it as "Professor Phillips' already classic piece", and in

1966 it was reprinted, (along with Samuelson and Solow) in M. G. Mueller's extremely successful collection of Readings in Macroeconomics meant for advanced undergraduates. And in his 1972 AEA Presidential address, James Tobin, while recognising that the idea of an inflation-unemployment trade-off had a long and complicated history before and after 1958, nevertheless referred to Phillips [sic] paper (not his curve) as "probably the most influential macro-economic paper of the last quarter century"; and so on. Evidently, negligibility and lack of impact come in many forms.

No. Negligibility is homogenous; it is fame that comes in many forms.

In the case in question, Laidler has agreed that inflationist policy based on Phillips' work is undetectable, so the importance of the paper, if it has any, must lie in its impact on scholarship, research, or perhaps teaching. In relation to Lipsey (1960) and Samuelson and Solow (1960), his point clearly turns on their genealogical influence, and of course he is quite right to emphasize the importance of that channel. But in scholarship, research, and teaching, we are seeking the influence of the intellectual content of Phillips' paper, so we must have a clear idea of what that was.

The myth has it that his paper offered the first taste of the idea of a negative relationship between inflation and unemployment. That is not correct – the idea of that negative relationship is another one, which was, as a matter of simple fact, widely known well before its conventional dating. I was at pains in Forder (2014, ch. 1 part 1) to emphasize this, just so as to focus attention on the issue of what was Phillips' original contribution. There are a few other crumbs of novelty here and there in his paper, but I argued that its big idea was that the *same* relationship between wage change and unemployment had held over nearly 100 years, and that was interesting because it meant, contrary to much theory of the time, that the determination of wage change was not much dependent on the institutional environment. The effect of unemployment on wage change was, apparently, unchanged by all the social and political development from 1861 to 1957. Laidler does not indicate that he feels I was mistaken about this, so the question is how that idea – perhaps modified or developed by those authors – was passed on by Lipsey and Samuelson and Solow.

It is a disappointment to me to find Laidler implying, as he does, that I failed to recognize the importance of the influence of these authors on others. In fact, I considered that point rather extensively. In my study of the econometric Phillips curve literature before 1968, I noted that it had various sources, of which Phillips' paper was only one. From analysis of that body of work, and its sources, I concluded that up to about 1965,

none but Lipsey really flows from Phillips, and none of the rest flows from Lipsey. (p. 60)

True enough, after 1965, more of the work was Lipsey-like, as I also observed. But many of those authors saw quite clearly that it is *Lipsey* they were following, not Phillips-at-one-remove. This flows naturally from the point, as I discussed in Forder (2014, ch 1 part 1), that Phillips' work was readily and quickly recognized, including especially by Lipsey, as being poor. Lipsey made numerous criticisms, simply threw away Phillips' statistical analysis to start again, and then rejected the principal conclusion about the 100-year relationship. That is what Lipsey's readers would have found when they followed his lead. To the extent that it had any relevance to what they were doing, they were therefore following Lipsey in *rejecting* the intellectual content of Phillips' paper.

No doubt it will be argued, as is perhaps hinted by Lipsey (1978), that notwithstanding that it was not original to Phillips, it happened that Lipsey learned of the idea of the negative relationship from him, so that if Lipsey was influential, the influence of Phillips is there, after all. But that is a very peculiar view. Consider the point that I learned of the multiplier from Lipsey (1979). That is a biographical fact about me, not a fact about the intellectual content of Lipsey's textbook. In my case, I had no idea of the multiplier before I read Lipsey, and it is possible, though it seems unlikely, that Lipsey similarly had never thought of a negative relationship between wage change and unemployment until he read Phillips. If that is the case, then so be it – it is a biographical fact about him, not a fact about the intellectual content of Phillips' paper. It does not, for example, create any suggestion that those reading Lipsey were similarly in the dark about it. To them, as I showed it must have been to the vast majority of economists, it would have been a well-known idea, given some econometric form by Lipsey and others, not something learned from Phillips-via-Lipsey. The intellectual importance of a paper does not arise from the fact that one reader happened not to know of an *ordinary* idea before reading it, and others were then impressed by the work of that reader.

In the case of Samuelson and Solow, one point, as I observed in Forder (2014, p. 35), would be that they did not adopt Phillips' analysis, so the possibility of their conveying the intellectual content of his work does not arise. But Laidler's implication that I failed to consider the point that they were widely read again creates rather a dim impression of my argument. I made specific comment on what was said about their paper in something like 150 other works (all in Forder (2014, ch. 2), including footnotes). Again, for the period before 1968, I considered every citation of Samuelson and Solow I could find (in this case, I have found a couple more since, but they do not change the story: http://wp.me/P7rUVD-2O). In my conclusion about what was said about them – about their influence – I said,

The economists Samuelson and Solow are supposed to have influenced saw in their paper the possibility or actuality of cost-push inflation, the instability, or non-existence of the American Phillips curve, or learned from them a list of reasons to expect it to move; or if they thought Samuelson and Solow treated the curve as exploitable, they simply rejected their argument. (p. 49)

There, my main concern was not with tracing Phillips' grandfatherly influence, but rather tracing any possible influence of Samuelson and Solow in promoting inflationism. Nevertheless, it is clear that not only was the intellectual content of Phillips' paper not transmitted to their readers, but neither was what the conventional story regards as that intellectual content!

My consideration of these papers, then, turned on a consideration of the intellectual content of Phillips' work and of those discussing him. Here, in contrast to the discussion of the expectations argument, Laidler has failed to appreciate the importance of identifying the intellectual content of the works under discussion. This is what leads him to suppose he has evidence of something when he does not. And, clearly, his supposition that he has a sweeping rebuttal of my argument based merely on an observation about what was 'widely read' cannot succeed.

Laidler's other ideas about the importance of Phillips (1958) are much less substantial, but raise points of principle. He cites Bodkin (1962, p. 12) calling Phillips' work 'already classic'. As it happens, I was there before him in quoting those words – Forder (2014, p. 250 n. 2). But in any case, for Laidler's purposes, that is very poor-quality evidence. The source is a doctoral thesis, and we do not look to students, even ones who go on to some eminence, for judgments about the 'classic' status of particular works. Furthermore, having given an account of what Phillips did, Bodkin began his next paragraph with the words 'Phillips' work has not gone unnoticed' (p. 14), and he went on to identify some people who had cited it. If the reader needs to be reassured that the work has not gone unnoticed, then the author's commitment to the view that it is 'already classic' is plainly fragile. So even if we were minded to take a graduate student's word for it, this would not be the occasion. Most likely, Bodkin's expression was just a slightly lazy way of introducing a paper that he wanted to discuss. Nothing of weight in the history of economics follows.

Tobin was at least an eminent economist at the time he wrote but, again, his words are only those of authority, and since that cannot be more than poor-quality evidence, we should be able to do better. In particular, if Laidler could locate Phillips' influence, then he would lose interest in citing Tobin to 'prove' that it must be there somewhere.

Still, there are other points pertaining to this case. Supposing we were going to prefer authority to the discovery of the facts, then the issue of identifying appropriate authorities must arise. One point calling Tobin's status into question, considered in my book (p. 156), is that Tobin (1966) discussed the relation of inflation and unemployment in terms very much like and entirely consistent with his discussions of the 1970s, but made no mention of Phillips. Apparently, although it was deep within the quarter-century when Phillips was supposedly so influential, Tobin did not *then* think his views were derived from Phillips (1958) at all. But if those views were not derived from Phillips in 1966, the same views were not derived from him in 1972, either. The point I thought I had made clearly enough is that Tobin's later discussions introduced 'the Phillips curve' in a role it had not previously had. He was not alone in that, and I said of others that their 'discussion of the Phillips curve seems to be something that was added to the authors' understanding after the events they were describing' (p. 156). The consequence is clear: Tobin is no authority on the history of the Phillips curve. Laidler's reliance on *that* authority therefore lends nothing at all to his case.

This line of argument can be taken one step further. When, in Chapter 7 of the book, I considered how the Phillips curve myth came to be so widely accepted, I identified certain seemingly important statements of parts of it and tried to show how errors accumulated. I identified Tobin (1972) as one of the earlier papers playing a role (p. 183). So, it is part of *my* story that there were people in the 1970s who said incorrect things about the Phillips curve, and Tobin was one of them. Laidler is therefore in the position of quoting those whose mistakes I say were the origin of the myth as providing evidence that it is true. That is clearly not to engage with my argument, and a purer case of allowing the ghost of the conventional view to prevail over analytical thinking would be hard to invent.

Of specific points Laidler made, that leaves the appearance of a reprint of Phillips (1958) in Mueller (1966). That, indeed, is something I did not discuss. The quality of this evidence is questionable, since it is not clear how impressed we should be by the contents of books of readings, particularly as it is a possibility that Phillips' paper might find its way in because of name recognition, rather than because it had any actual impact. Still, as to *quantity,* it can hardly be said that just one such piece of data is much. Presumably on the basis that it went to four editions, Laidler declared Mueller's work to have been highly successful, which I suppose gives it some standing. But then, Smith and Teigen (1965) went to four editions too. That is another book of readings, aiming to help 'the student to understand the mechanism and linkages through which both monetary and fiscal policy produce their effects on income, employment, the price level, the rate of growth and so on' (p. vii). Phillips (1958) does not appear there. So why would Mueller have evidential value, and Smith and Teigen not? That still is only two sources. I am not inclined to think this an important kind of evidence, but I did search the Oxford libraries for books with 'readings' and 'macroeconomics' in the title, finding nine more which were not exclusively from any particular journal, and were from somewhere near the appropriate period. One turned out to be a book of newspaper articles. Of the remaining eight, one included Phillips' paper; seven did not. Then there was R A Gordon and Klein's (1965) – '*Readings in business cycles*' – regarded by some as authoritative. But again, Phillips' paper was not amongst their 38 selections. If the contents of this kind of book is to be the basis of a challenge to my view, it is not off to a very strong a start, but I shall leave further evidence-acquisition to others. The important point, of course, is that if the evidence of books of readings is to be treated as important, we need to investigate the books of readings, not simply cite one that happens to point in the desired direction. Where there is plenty of evidence that could be deployed, one convenient piece of it cannot be persuasive.

There might be a wider issue of how the Phillips curve was treated in teaching. I made only brief mention of that in the book. Since then, in Forder (2015) I examined a range of textbooks, and what I found seemed very much to confirm my picture.

Laidler ended his catalogue by saying 'and so on', no doubt to indicate his confidence that more evidence could be presented. But more examples like these will achieve nothing, and I cannot see why anyone would suppose Laidler has kept what he regards as the best evidence to himself, serving us something inferior. Confident allusions to undisclosed evidence can hardly be persuasive in circumstances where the evidence that is presented is as ineffective as Laidler's.

I suppose in response to all that it will be said that whatever else may be true, it was Phillips' work that was 'remembered', and so, as it were, he was lucky to be important; but, as things came out, he was still the important one. In this, again, I suspect that the ghost of the conventional view exercises an influence – it seems unthinkable that a paper so frequently cited, albeit by authors who then say nothing about it that is true but not trite, could in fact have had no worthwhile influence. The point, though, flowing from the consideration of its intellectual content, is that Phillips' work was *not* remembered. His name was used; but his idea made no impact.

So, when we give attention to the matters of the intellectual content of the works under discussion, and the quality and quantity of evidence, and do so without prejudging the matter on the basis of the conventional view, it becomes apparent that Laidler's points do nothing to advance his case. Again, then, there is nothing that would lead me to change my view, or anyone else to reach the extraordinary conclusion that what I said is 'simply not true'. Simple or not, it very much is true: as concerns its actual intellectual influence, as distinct from promoting name recognition (or 'citation recognition', perhaps) – Phillips' paper is negligible and very possibly the most overrated in economics. It is *fame* that comes in many forms – and a name being used without any true connection to the work is one of them.

VII. Two loose ends

There are two other points Laidler made on which I believe I did what he says I should have. One of his points was to the effect that I failed properly to weigh the point that pre-1968 econometric studies made the coefficient on price change in the Phillips curve an empirical matter, rather than assuming it to be equal to one. In the myth, of course, price change is said to have been ignored, so it is an important point that it was there at all. But I discussed precisely the matter of its being treated as empirical and the reasons the authors had for doing so (pp. 66–67) and considered the matter further in Chapter 5.

Second, he said R J Gordon was important in virtue of being ‘the author of a series of decreasingly sceptical empirical studies [of the expectations argument]’, and seems to imply I have not recognized this. But I said,

the work of [R J] Gordon demands special attention first of all because, in his earliest papers … he offered some of the firmest rejections of the vertical Phillips curve and drew mildly inflationist conclusions. But then, in later papers, he gradually changed his view and … accepted Friedman’s position. (p. 102)

And I gave him that special attention over the next pages.

VIII. Conclusion

So, nothing in Laidler’s review leads me to change my view and, in some cases, his remarks suggest lines of enquiry that point to a strengthening of it. On the question of the effectiveness of the case made in my book, that is all there is to it.

The substantive points leading to that conclusion are specific to the case, but the principles illustrated are not. One theme is that the assessment of evidence is much more important, and perhaps more difficult, than is sometimes realised. In considering the importance of any particular work, half a line from a graduate student is not good evidence; the fact that Harry Johnson was impressed by something is certainly worth knowing, but that evidence cannot carry the day against an actual assessment of the role of the work in the development of macroeconomics.

Poor-quality evidence is routinely dished out and accepted when its role is only to illustrate a point that is not actually in doubt. If we do not doubt that Phillips (1958) was an influential paper, then the fact that he was cited by Lipsey and Samuelson and Solow suggests an explanation. But that is only an illustration of something taken to be true. When the matter of the importance of Phillips’ paper is thrown into doubt – when it becomes a matter in contest – the recital of these sorts of facts must give way to an enquiry into the evidence of the extent to which the intellectual content of a work was passed on.

Similarly, the assessment of various sorts of evidence has a special pertinence when there is a firmly held view that is under challenge. It is inevitable that there will be some sort of reason that view is held – there will be a story that is told. When it is challenged by an array of arguments and pieces of data making up an alternative account, we cannot proceed on the basis that each component of the new story is to be assessed separately by its ‘plausibility’ in the light of the old story. That old story is under challenge. It cannot be the source of information – or feelings – about whether individual elements of alternative stories are to be preferred to it.

Equally, once it has been shown that crucial components of a story are incorrect – as when, for example, it is shown that the idea of a negative relationship between wage change and unemployment, or the idea that wage bargainers’ expectations would adjust to ongoing inflation, were commonplace long before their purported discovery – then those aspects of the old story must be discarded. That part of the story is dead *and must be buried*. It does not retain a phantasmal force over other components of the story.

A third theme concerns aspects of the importance of the identification of the intellectual content of various pieces of work – the identification of that aspect of the work that makes it important in intellectual history. Indeed, that may pose obstacles. It will very probably depend on the appreciation of what was known or presumed at the time it was produced; it may require comparison with the author's other works; or a consideration of debates of its time. Historical analysis is not completed when we have located some keywords, or counted the citations of a work. All that should be obvious, but apparently it is less obvious that a received view about what the intellectual content of a work is cannot stand against analysis of these things. If all we are interested in is whether someone's name is used, then that is indeed easier to find out, but not the same thing at all. The study of the history of economic thought requires us to study *the thought*.

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