A Review of James Forder, *Macroeconomics and the Phillips Curve Myth*, Oxford University Press, 2014

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Abstract

In this review, I argue that Forder makes a fine job in debunking the *story* told by Friedman in his Nobel prize lecture about the Phillips curve yet fails to assess the validity of Phelps's and Friedman's contributions to the Phillips curve *theory*.

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1 michel.devroey@uclouvain.be This review will appear in *History of Political Economy*.

Over the last few years, James Forder has made a name for himself among historians of economics by writing a series of papers reexamining the Phillips curve literature. The task he sets himself in the present book, entitled *Macroeconomics and the Phillips Curve Myth*, is to systematize and enrich his earlier contributions. The end result is a brilliant and beautifully written essay, and a stimulating read. Its author's cleverness shines through on every page. Its scholarship is impeccable. Forder seems to leave no stone unturned (I shall argue that there is actually one big stone which he has forgotten to upturn).The literature covered is impressive. To two hundred and twenty-eight pages of text correspond thirty pages of notes and forty-seven of references.

The book is a passionate prosecution plea. In Forder's words, "I would like to think that I have killed the old [Friedman's] Phillips curve story" (p. 217). The following three quotations, the first two about Phillips's 1958 paper, the third about the fate of this story, illustrate this:

> Objectively speaking, it [Phillips 1958 paper] was a negligible paper. It is not even the case that it inspired ideas which turned out to be wrong-headed. On the contrary, it inspired very little. … In so far as it had a unique idea – that the wage-change equation was invariant to institutional change – it was wrong, and not much better than meritless (pp. 207-8).

> [Phillips's paper] was quick and dirty, rudimentary, conspicuously sloppy, of loose and approximate reasoning, based on impaired and incompatible data, written in a wet weekend, and rushed prematurely into print (p. 17).

> The result is that so far as the history of post-war macroeconomic thought has been developed through stories of the Phillips curve, it would be best to throw that history away altogether and start again. There has been, one might say, an historical failure on a grand scale, and there is no point in setting about sorting through the rubble, because there is no reason to believe that there is anything of value there. The Phillips curve story is a *Just So* story – 'How the Phillips curve became vertical'. And it is nothing more (p. 205).

Forder makes two main contentions. The first is that the Phillips curve story is a myth; by which he means that it is false. The emblematic place where this intellectual swindle was first developed is Friedman's Nobel Prize lecture (Friedman 1977), the content of which is dissected in the book's introduction. Friedman's story is composed of a set of propositions related to the earlier Phillips curve literature, before Friedman and Phelps transformed it into the 'expectations-augmented Phillips curve'.

(a) The first proposition relates to Phillips's 1958 article. It is two-pronged: (aa) Phillips was the first to find an inverse relationship between wages and unemployment and, (ab) his paper exerted a huge influence.

(b) In the 1960s, macroeconomists, the second proposition runs, fell prey to a confusion between the nominal and the real wage.

(c) The third proposition is that these economists failed to appreciate the importance of expectations of inflation.

(d) Finally, the fourth proposition states that Samuelson and Solow's inflation/unemployment trade-off significantly influenced policy making.

Forder devotes a chapter to debunking each of these propositions; none of them, he concludes, stands up to scrutiny. If, taken in isolation, every element of a whole construct, here the Phillips curve story, is proven false, then the whole must be faulty as well. This is what Forder means when he claims that the Phillips curve story is a myth. Friedman's 1977 article proved influential enough to make the story conventional knowledge pervasively present in textbooks – a state of affairs that sparked Forder's ire and, as a result, his desire to restore the truth.

The book's introduction also makes it clear that Forder wants to go beyond the dismissal of the different tenets of the Phillips curve story. An additional aim of his book is to rectify the current view of the economics profession shaped by Friedman in his Nobel lecture. It depicts past economists as "struggling to articulate the simplest ideas, and disputing the obvious. … [a state] in which economics was bizarrely primitive or its practitioners were extraordinarily slow-witted" (pp. 1-2). Forder cannot concur with such an account. To him, the 1960s were a time "full of economic theory offering acute observations of the economic problems of the time" (p. 207), and his book exudes a feeling of nostalgia for the times before the neoclassical approach became mainstream. Hence, he brings forth what he calls the 'great divide in economics' (p. 198), bearing on the issue of wage bargaining, which to him is what discussions about the Philips curve always revolve around. This divide opposes explanations in which non-economic factors, such as fairness, are taken into consideration for wage determination and explanations resting on a 'strictly economic' approach – in other words, an opposition between the institutionalist and neoclassical approaches. For his part, Forder makes it clear that he favors the institutionalist line. His conviction is that this line needs to be rehabilitated, with the rehabilitation of the cost-push/demand-pull distinction as part of this process. From his perspective, the merits of 1960s economists are many: they were interested in the economy as it stood *hic et nunc* rather than in a search for immutable laws of economics. They resisted the view that "the generalization of economic relationships is always the highest goal of the analyst" (p. 65)], and they did not feel compelled to study wage formation in accordance with marginal productivity (p. 67).

This restorative purpose pervades the whole book. For example, one of Forder's indictments of Phillips's paper is that it took the wrong track:

> Phelps's suggestion offered not only a way for economics to take a step towards a scientific status in the manner sought by Lipsey; but also threatened to reveal that wage determination owed nothing to its apparent human aspect. A great many deeply held views about wage bargaining would have to be abandoned. Indeed, many people would have had to change their vision of economics (p. 31).

In this review, I will not discuss the validity of Forder's criticisms of the four individual tenets of the Phillips curve story.[2] Rather, I prefer to insist on what I view as the

[2] On this matter, I refer the reader to David Laidler's review of Forder's book (Laidler 2016). His judgment on the four propositions is mixed. As for proposition (a), the landmark character of Phillips's paper, Laidler finds Forder's assessment right with an important caveat: Phillips's paper may well have been shaky and

main weakness of his book: debunking the Phillips curve *story* is hardly tantamount to debunking the Phillips curve *theory*. The relevant distinction here is between theoretical and meta-theoretical discourse. The latter consists of comments on a theory; it does not 'produce' theory. Meta-theoretical works, an example of which is the history of economics, are far from useless; it is just that their contributions are of a different nature. Friedman's Nobel Prize lecture is a meta-theoretical piece. I concur with Laidler when he writes that it "had more to do with exaggerating the originality and importance of his own 1968 paper than with setting out an accurate historical record." In short, it was poor history of economics. For sure, Friedman's inaccurate account was not innocent. Macroeconomics comprises an ideological dimension. It bears on policy conclusions and these necessarily tend to ascribe either market forces or government the leading role in managing the economy. Sacrificing accuracy for a good cause is a choice that somebody like Friedman – a peerless master in persuasion – could embrace. However, this does not make Friedman's theoretical contribution to the Phillips curve theory literature – his 1967 Presidential Address – void. It must be assessed on its own, for its quality as a theoretical contribution. Oddly enough, Forder fails to tackle this. This is the big stone left unturned.

The history of the Phillips curve theory, as a macroeconomics episode, is tumultuous, comprising several twists and turns. The first is the transition from the 'curve of Phillips' to the Keynesian Phillips curve driven by Lipsey. Being part of the IS-LM apparatus, the Keynesian Phillips curve shared one of its central features, at least as far as the theoretical IS-LM model is concerned, namely its static nature. Indeed, in this model the short- and the long-period frameworks are insulated from each other, with any possible gravitation from the former to the latter discarded. Such a lack of dynamic perspective amounted to assuming away the possibility of an unstable Phillips curve, except as a verbal caveat. Friedman and Phelps seized on this lacuna by zeroing in on the gravitation process. Once such a perspective was adopted, expectations could not but receive pride of place. Instead of being a factor mentioned yet remaining non-modeled, they had to become an ingredient of the model. Thus, the door for the vertical long-period result opened. Such a result cannot be minimized as Forder does. From a theoretical point of view, it marked a watershed. In my book, *A history of Macroeconomics from Keynes to Lucas and Beyond* (De Vroey 2016), I argue that Phelps's and Friedman's models, both based on some information imperfection, differed significantly. Phelps's contribution (Phelps 1968) was theoretically more solid than Friedman's. Though lauding it, macroeconomists did not know what to do with it. As for Friedman, his Presidential Address offered a verbal type of reasoning; yet a model, rather half-baked but

much criticized yet writing that it was negligible and had no impact is "simply not true". As for propositions (b) and (c), the confusion between monetary and real wage and the lack of regard for expectations, according to Laidler, Forder is right but he pushes his conclusions too far. Finally, as for proposition (d) (the impact of Samuelson and Solow's paper), Forder is right in claiming that it had no impact on policy making, yet he is less so when stating that there were almost no economists pushing towards using the trade-off.

original, was hidden in it. Moreover, it addressed an issue that had been central in macroeconomics since Modigliani's 1944 seminal article, money non-neutrality. [3] The problem facing those economists like Friedman who were suspicious of the Keynesian approach was that it was difficult not to admit the existence of short-term real effects of monetary expansion. To them, the way ahead was to build a model in which such occurrences would be present yet interpreted differently. Friedman's paper achieved the feat of turning upside down the Keynesian view that the very existence of money non-neutrality was the proof that, before activation, the economy experienced a state of underemployment or disequilibrium. Friedman contended that the inverse was true: monetary expansion created an unsustainable disequilibrium. The game-changer was Friedman's paper rather than Phelps, even if Friedman's argumentation was sloppy. It was saved thanks to a new twist, Lucas's 1972 "Expectations and the Neutrality of Money paper" (Lucas 1972). While it aimed at reformulating Friedman's claim in a more rigorous way, the road taken by Lucas for achieving this aim involved a radical breach from the methodological vision underpinning Keynesian macroeconomics (and which Friedman accepted). While Lucas's paper was instrumental in strengthening Friedman's claim, its main effect was to impose new methodological standards for macroeconomics, thereby blazing the trail for a scientific revolution. As is well known, Lucas was not alone in setting a new scene. In relation to the topic of the Phillips curve, Sargent and Wallace's work strived at re-enforcing the policy-ineffectiveness claim by building models assuming rational expectations and price and wage flexibility in which even the short-period Phillips curve is vertical (Sargent and Wallace 1976). A further turn in this history was Fischer's staggering contracts model, in which he claimed that with sluggish wages, money non-neutrality was effective again (Fischer 1977). Finally, the last step to date in this history is the 'new Phillips curve', a central element in baseline second-generation new Keynesian models (Clarida, Gali, and Gertler 1999).

My historical sketch suggests that the history of the Phillips curve theory is defined by a series of landmark papers, each marking a progress with respect to the previous ones. This picture seems poles apart from Forder's analysis. Actually, it only is in as far as one accepts that the inadequacy of Friedman's story extends to his theory, a step that Forder takes implicitly. For my part, I prefer an alternative view, the parallel existence of two types of essays. Those of the first kind study the meta-theoretical literature on the Phillips curve literature. Forder's book, a critique of the biased history of economics inaugurated by Friedman, belongs to this first type. Works of the second kind are concerned with the Phillips curve theory. This is the case of my own work, which defends the view that over time the Philips curve theory has undergone a series of theoretical improvements.

[3] Curiously enough, Forder does not address it.

We are then left to wonder what makes a landmark paper. Why, more precisely, can Phillips's 1968 paper or Friedman's Presidential Address be considered thusly? Originality is certainly an important criterion. Contrary, to Forder, I contend that, for all their imperfections, both papers were original, especially Phelps's. But this is not sufficient. To qualify, papers must come at the right time. They must find an echo and generate a following. The off-the-cuff vision that a landmark paper is a definitive contribution is false. On the contrary, a landmark paper is one which has been superseded, having started a cumulative theoretical development process – a characterization that can be made only with hindsight.[4] With this criterion in mind, all the papers mentioned in my historical sketch can be regarded as landmarks.

This view that the Phillips curve literature has testified to theoretical progress runs counter to Forder's claim that it was a regress with respect to the institutional approach that prevailed in labor economics in the 1960s. Of course, our understanding of progress must be relative. If the development of economics is compared to a decision tree, as proposed by Leijonhufvud (1994), progress is limited to developments occurring in a specific branch of the tree. The progress alluded to above has taken place within mainstream macroeconomics, which belongs to the broadly defined neoclassical approach. Defenders of another basic approach – say, the institutional approach – will scarcely be impressed by such progress since in their opinion the basic methodological choices upon which the neoclassical approach rests are wanting. Fair enough. However, any criticism of the Phillips theory on the mere grounds that it belongs to the neoclassical approach will cut no ice either.

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[4] As said by Sargent in an interview with Klamer: "When we do research, the idea is that you don't produce a finished produce. You produce an input. You write the paper with the hope that it will be superseded. It's not a success unless it's superseded. Research is a living process involving other people" (Klamer 1984: 74).

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