

The challenge on Phillips

Simon Chapple*

The substance of my argument

The main points of my original article (Chapple 1998) were as follows. There is a stable non-linear trade-off between the rate of inflation and the level of unemployment (or economic activity) in Bill Phillips's theoretical and empirical macroeconomic work between 1954 and 1962 - the period during which the Phillips curve entered into the professional consciousness. Phillips consistently suggested that this trade-off could be exploited for long run policy purposes and there is no reason that we should not take him at his word. Inflation expectations are only discussed once in Phillips's writings on the curve over the period and then in the context of their influence on aggregate expenditure, not on the temporal position of the Phillips curve, and only when zero inflation is the policy target. While Phillips sometimes expressed a normative *policy* preference to be at the point on the curve where prices were stable, this preference was not derived from any *analytical* conclusion that the trade-off was unstable. Thus there was no trade-off *misinterpretation* of Phillips's work when Samuelson and Solow bought Phillips to America in 1960: the profession got the interpretation of Phillips just about right.

Readers interested in detailed chapter and verse documentation of these conclusions with reference to Phillips's publications over the 1954-1962 period are referred to my original article (Chapple 1998).

Leeson's replies do not clarify these basic points. I stand by every one. It would be valuable to know which of my basic points Leeson disagrees with and why. My first challenge for Leeson is to address my points, one by one, with direct textual evidence from Phillips's published writings on the Phillips curve between 1954 and 1962.

My second challenge is for Leeson to find any quote from Phillips's work between 1954 and 1962 which indicates the potential interaction of anti-inflationary policies, adaptive inflation expectations and path dependence on the position of the Phillips curve anywhere as explicitly as the alleged creators of the trade-off myth of Phillips's work, Samuelson and Solow (1960):

[I]t might be that the low-pressure demand would so act upon wage and other expectations as to shift the [Phillips] curve downward in the longer run - so that over a decade, the economy might enjoy higher employment with price stability than our present day estimate might indicate.

But the opposite is also conceivable. A low-pressure economy might build up within itself over the years larger and larger amounts of structural unemployment...The result would be an upward shift in our menu of choice, with more and more unemployment being needed just to keep prices stable. (Samuelson and Solow 1960, p. 163).

Perhaps there is more mileage in developing a research programme on the professional misinterpretation of Samuelson and Solow!

On Leeson's reply

I will deal briefly with the substance of Leeson's papers in an effort to get to the nub of where he and I disagree.

Leeson (1998, p. 87) gets off to a sound start by acknowledging that Phillips *did* have a trade-off between inflation and unemployment (or activity) in his work between 1954 and 1962 (at least in Leeson's new language of the "compromise" zone). I think that Leeson thinks Phillips's "compromise" trade-off is non-trivial and policy-exploitable. If so, this acknowledgement is good. This is progress. We can find some grounds for agreement.

However Leeson's latest division of Phillips's curve into three zones - runaway inflation, the compromise zone, and the zone of catastrophic unemployment - is both new and problematic. Phillips's main message was the existence of a non-linear "compromise" between inflation and activity rather than Leeson's apocalyptic and discrete zonal triad. One should be wary about imposing on Phillips a triad and an apocalyptic language which is not of his making.

I am also pleased to see Leeson acknowledging the existence of a stable trade-off between production and inflation in Phillips's work on the closed economy. However it is inaccurate to argue as he does that Phillips's (1959) curve is a theoretical equation. It is clearly an estimated equation.

Other aspects of Leeson's reply are more problematic. He misses the fact that in steady state in Phillips's 1961 growth model the economy is only at full capacity by accident of parameter coincidence. A faster rate of monetary (demand) growth both raises inflation and raises the steady state rate of capacity. This is hardly the classical quantity theory of money; rather it is the classic Phillips curve trade-off!

Leeson suggests that this trade-off may be mitigated by changes in δ or in β . However both δ and β are exogenous parameters. Moreover, Leeson to the contrary, δ is not the rate of change of factor prices!

In discussing Phillips's (1961) article Leeson on several occasions mentions exchange rate issues as if Phillips discussed them in this context. In fact Phillips's model is of a closed economy. There is no discussion by Phillips of an external sector or exchange rates. By mentioning open economy themes Leeson is unnecessarily mudding the intellectual waters.

Leeson is correct to argue that Phillips used estimated empirical parameters to linearise his model to allow a solution. However, there is no suggestion by Phillips at any point in the 1961 article that the non-linear trade-off relationship between inflation and capacity utilisation breaks down as Leeson at times implies: "There is reason to believe that the relationship between employment and wage changes is not linear and that at lower levels of unemployment a higher value of β [the response of inflation to capacity utilisation, assumed by Phillips to be slightly below one] would be appropriate [to solve the model] while for variations between say, 4 per cent. and 5 per cent. unemployment a suitable value of β might be as low as 0.1" (Phillips 1961, p. 365).

Leeson (1998, p. 98) suggests that he never argued that Phillips was the author of the expectations augmented Phillips curve. Again this acknowledgement is good. It clarifies the following comment, which could suggest to unwary readers - along with his contentions that the Friedman-Phelps critique of the curve be called the Friedman-Phelps-Phillips critique - that Phillips was the author of the expectations augmented curve:

Phillips's work has quite unjustly been denigrated as having neglected...inflationary expectations...Ironically the most frequently discussed

equilibrium in this 1954 'expectations augmented Phillips curve' was at stable product prices..." (Leeson 1997, p. 166).

Leeson argues that his reply:

demonstrates that Chapple has unwittingly surrendered his position about the stable trade-off in Phillips' work: when inflation generates the expectation of a continuation of inflation this, in Phillips' model, increases inflation still further, thus destroying the notion of a "stable" trade-off." (1998, p. 87)

This appears to be the nub of our disagreement. If I have unwittingly surrendered my position on the stable Phillips curve, let me wittingly claim it back! I have shown that Phillips's work involved a stable non-linear exploitable trade-off between inflation and unemployment (or capacity utilisation in a growth context or the level of production in a short period context). Phillips wrote down mathematical equations and drew theoretical and empirical curves describing stable inflation-activity trade-offs, as well as describing economically significant trade-offs in words. I have cited chapter and verse of Phillips's macroeconomics between 1954 and 1962 to support my point.

In the *one* place where Phillips deals with adaptive expectations he considers its impact on expenditure, not the temporal position of the inflation-unemployment trade-off, and in conditions where zero - not positive - inflation is targeted! Instability in the position of the Phillips curve at non-zero inflation rates - whether instability arises through inflationary expectations or exchange rate changes in an open economy - is not Phillips's central message. And I have provided abundant textual evidence for my analysis.

Conclusion

There was no gross misinterpretation of Phillips's curve by the economics profession in the early sixties or later. There is plentiful textual evidence in Phillips's work over a sustained period for the profession's non-linear stable trade-off interpretation of the curve to be considered broadly accurate. And Phillips had ample opportunity to disagree in print with the profession if he thought they had got him wrong. Significantly Phillips did not do so. It is reasonable to presume he did not respond because he did not feel terribly misinterpreted.

Leeson defends his interpretation in the face of substantial textual evidence by creating an acronymic and apocalyptic zonal trinity which owes a greater intellectual debt to the Book of Revelations than to Phillips. Once one takes Phillips at his word and accepts that he put forward the idea of a stable non-linear trade-off between inflation and activity, all else simply falls into place. There is no need to add new complications to understand Phillips's point or the history of the Phillips curve.

Overall Leeson's argument for professional misinterpretation of Phillips's curve seems - to me - to still lack any textual persuasiveness.

* Labour Market Policy Group, Department of Labour, Wellington, New Zealand.

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