The Genesis of Samuelson and Solow's Price-Inflation Phillips Curve: Rejoinder to Hall and Hart

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My paper (Hoover 2015) was only in part a comment on Hall and Hart's (2012) paper. Its other independent purpose was simply to clarify Samuelson and Solow's construction of their price-inflation Phillips curve. Hall and Hart make it clear in their 'Reply' that they are not interested in those aspects of my paper, but rather merely want to ask whether, using econometric techniques available in 1959-60, Samuelson and Solow (1960) could have produced the Phillips curve that they reported in their original paper. Hall and Hart show that a simple bivariate, quadratic regression in the inverse of the unemployment rate would not produce Samuelson and Solow's hyperbolic curve, but rather a hump-shaped curve. It is, however, an odd understanding of what constitutes the state of the art in econometrics in 1960 to think that the 'techniques available at the time' should be glossed as the specific regression that Hall and Hart estimate. In fact, there is every reason to believe that a sophisticated econometrician circa 1960 would have rejected Hall and Hart's regressions as spurious artifacts that, far from providing a 'challenge [to] the results as well as the policy implications of the Samuelson-Solow Phillips curve' (Hall and Hart 2012: 67), cannot be legitimately interpreted as bearing any usable economic interpretation.

It is unfortunate that Hall and Hart eschew interest in the actual construction of Samuelson and Solow's Phillips curve, first, because it embodies the target of their challenge; and, second, because, in coming to grips with how it is constructed, the underlying problems with their own estimates would have become clearer. Hall and Hart do not question the overwhelming evidence that Samuelson and Solow construct their price-Phillips curve by translation of an impressionistic wage-Phillips curve. And Robert Solow (2014) confirms my reconstruction:

> Your discussion of the passage from wage inflation to price inflation is exactly right. We simply took it that price inflation would approximate wage inflation minus productivity growth. This was a common practice in large econometric models of the time, unit labor costs being the key variable.

Hall and Hart also do not challenge the very strong evidence that the values chosen for wage-inflation and unemployment from which the price-Phillips curve is translated are justified exclusively on post-World War II data. Nor do they challenge the view – neither that it is Samuelson and Solow's view nor that it is, in fact, true – that the Great Depression and World War II were structurally different, so that they could not have had the same wage-Phillips curve. And here, again, the unchallenged details of the construction of Samuelson and Solow's price-Phillips curve matter; for, if the wage-Phillips curve is different in different regimes, given the relationship between the wage- and price-Phillips curves, then the price-Phillips

curve must be different in different regimes. And in that case, an estimate of a single equation using data from three distinct regimes can result only in a mongrel equation that is not interpretable economically.

This was understood, even in 1960. And that is what is ultimately important about whether the curve is meant to apply to the twenty-five years of actual price-inflation data, as Hall and Hart insist; or to apply to twenty-five years of 'normal' price-inflation data, as I surmise; or to apply only to the postwar price data – the caption referring to twenty-five years of data on Samuelson and Solow's Figure 2 simply being a mistake – which Solow himself suggests is a real possibility. My surmise has the virtue of making good use of Samuelson and Solow's actual discussion of the Great Depression. The problem with Hall and Hart's premise is that, given the way their price-Phillips curve has been constructed, Samuelson and Solow cannot coherently assert that it is stable over a period in which they see the wage-Phillips curve as shifting. Incoherence is both an unattractive and unnecessary hypothesis. Commenting on an earlier draft of my paper, Solow (2014) writes:

> I don't know where 'the last twenty-five years' came from. For sure we agreed that the Depression and war years could not be allowed to influence our notion of 'normal' relation, because they were not normal. Neither can I rule out careless phraseology.

Still, the point on which Hall and Hart are most insistent is that their econometric estimates have something to teach us that Samuelson and Solow could have learned. *They do not*. The problem is that it is only in very special circumstances, carefully controlled environments, that a simple bivariate regression corresponds in an interpretable way to the underlying data-generating processes. The function of specification tests is to reveal whether it is likely that these conditions are met. And it is the unlikelihood in this case that made Samuelson and Solow eschew regression analysis. Solow (2014) once more:

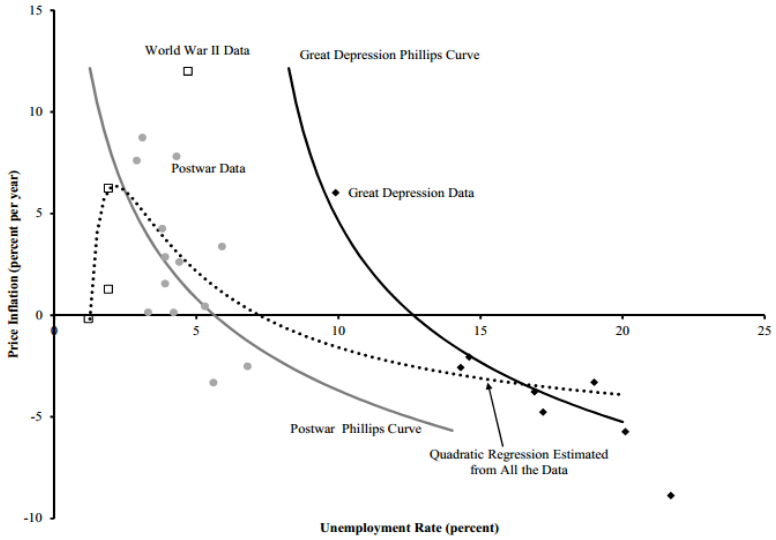
> It was obvious at a glance that US data were not consistent with any single bivariate relation of the kind that Phillips had found. If we were to do econometrics, then, we would have had to introduce other causal LHS [left-hand side] variables, maybe several of them. Collinearity and simultaneity would have appeared. Paul never had an appetite for that sort of thing. I might have, but there was no time (a common occurrence with Paul). So we were glad that it at least looked as if the immediate postwar years were more Phillips-like.

Though they present only the most rudimentary specification tests, Hall and Hart's regressions show characteristic signs of misspecification. For the CPI equation, the *t*-statistics are at best borderline and the R^2 is low; for the WPI equation things are far worse. Both equations show signs of serial correlation. And when a Cochrane-Orcutt correction is made for serial correlation in the CPI equation, the coefficients become altogether insignificant (Hall and Hart 2012: 69, fn. 14).[1]

But put all that to one side. A regression estimated over distinct regimes does not represent underlying structure. Let us suppose that the world were just as Samuelson and Solow describe it, with the Phillips curve for the Great Depression shifted to the right relative to the postwar period. What would we expect to find

using Hall and Hart's methods? A simple simulation will answer the question. We use Hall and Hart's actual unemployment data. The price series is generated for the postwar years as the predictions of Samuelson and Solow's Phillips curve plus an error term (see equation (1) of my original paper). For the Great Depression years (1934-41), the exact same equation is used except it is shifted to the right (Samuelson and Solow 1960: 189). The World War II years are simulated by taking the actual CPI inflation values for those years: wage and price controls were in effect, and labour markets were disrupted by military service and industrial mobilisation, so that usual market relationships between prices and unemployment were broken. [2] Figure 1 shows the two data-generating price-Phillips curves, the scatter plot of the data, and a fitted regression using the same functional form as Hall and Hart estimate.[3] The important point is that this fitted regression looks nothing like the hyperbolic Phillips curves that generated the data. This is a simulation; it is not an attempt to provide actual estimates that can be compared quantitatively to Hall and Hart's equation; yet it is striking that our fitted equation shows that same type of humped-shaped graph that Hall and Hart found. Faced with data from three distinct regimes, the regression struggles to fit them all. But in doing so, it does not adequately represent any of them.

Figure 1. Fitting a Single Regression Equation Over Distinct Regimes Results in Misspecification: A Simulated Example



Hall and Hart (2012) ask us to take their regression results seriously. They interpret the upward-sloping segment as showing that 'in a low unemployment economy … lowering the rate of inflation actually reduces unemployment'. They treat the causal direction as running from inflation to unemployment. Regressions are directional; so if Hall and Hart are serious about the direction running from inflation to unemployment, their regression is written backwards. Their presumed causal direction is certainly backwards to the way in which Phillips or Samuelson

and Solow treat it: unemployment is a proxy for aggregate demand, and changes in demand change inflation rates. It is the same causal direction as Milton Friedman and the New Classicals, who see the Phillips curve as an aggregate supply relationship; but neither Friedman nor the New Classicals conclude that low inflation lowers unemployment. Of course, this is an old problem: correlation does not prove causation.[4] There are various ways to buttress a causal story: an appeal to theory, or to common sense, or to other kinds of empirical evidence, but Hall and Hart offer nothing along any of these lines. Their equation is misspecified and, if the test statistics had not already hinted at that fact, common sense and an appreciation for the economic history of the sample period should have made it clear. In the end, we can indeed learn something from Hall and Hart's equation – namely, what econometric misspecification looks like in practice.

The final point that Hall and Hart reiterate in their reply is their rejection of my claim that the Phillips curve was not especially influential in the 1960s. (Note that I did not also say in the 1970s, which they reference in their reply.) The policy influence is, I believe, a pervasive myth. Quoting, as they do, a number of commentators restating their conclusion is not persuasive evidence; after all, it is the nature of a myth that it is widely believed. But I will not reargue the point here, as James Forder's new book, *Macroeconomics and the Phillips Curve Myth* (2014), does it better than I possibly could.

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My thanks to Robert Solow for permission to quote from his 2014 letter to me.

Notes

1 The Cochrane-Orcutt correction is itself an appropriate way to deal with serially correlated residuals only in very special circumstances – see Hoover (1988) and the references therein.

2 For 1946-58, the data are generated as $\tilde{p}_t = -32.2346 + 46.545U_t^{-0.2155}\varepsilon_t$; for 1941-45 as $\tilde{p}_t = \tilde{p}_t^{actual}$; and for 1934-41 as $\tilde{p}_t = -32.2346 + 46.545(U_t - 7)^{-0.2155} + \varepsilon_t$; where $\varepsilon_t \sim$ N(0, 6.25) and the U_t = actual unemployment rates.

3 The estimated regression line is:

$$\underset{}{\tilde{p}_t} = \underset{(3.57)}{-6.52} + \underset{(4.46)}{55.25(1/U_t)} - \underset{(3.82)}{55.16(1/U_t)^2},$$

where the *t*-statistics are in parentheses; *standard error of regression* = 3.79 and $R^2 = 0.49$.

4 On the problem of causal inference in macroeconomics, see Hoover (2001).

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