“The Samuelson and Solow Phillips Curve: Reply to Hoover”

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November 2014

Working Paper # - 2014-06
The Samuelson and Solow Phillips Curve: Reply to Hoover*

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* We are grateful to Kathleen Archibald, Mike Bryan, and Winn Fields for helpful comments on an earlier version.
The Samuelson and Solow Phillips Curve: Reply to Hoover

We appreciate the time, energy, and space Hoover (2014) has devoted to our paper. While we find his discussion interesting, much of it has little bearing on what we sought to accomplish. Hall & Hart (2012) was not about the ‘nuance’ or the ‘context’ that motivated Samuelson and Solow (1960), or about debating just how “critical” the Samuelson & Solow Phillips curve was to policymakers during the 1960s (it was surely important).

Our primary objective in Hall & Hart (2012) was far less ambitious or subjective. It was, quite simply, to answer one question: using straightforward econometric estimation techniques available at the time, could Samuelson and Solow have produced in 1959-60 the Phillips curve they claimed fit the data over the “…last twenty-five years”? Our answer was a resounding no. We were not interested in replacing Great Depression and World War II data with fictional ‘normal years’ data before estimating the Phillips curve. We were not interested in finding “point C” and estimating a postwar Phillips curve with a three-data-point postwar sample using a restricted estimation technique. And, as originally written, our paper most definitely was not about whether the estimation period for the Phillips curve should be twenty-five years, as Samuelson and Solow claim, or whether it should be the postwar period as Hoover (and other critics) claim. Our sole motivation empirically was to let the data—ALL twenty-five years of the data—speak.

Nonetheless, Hoover’s Comment does contain two major criticisms of our paper and several minor ones that deserve a response. We take up his two major criticisms in Section I, the minor ones in Section II.
I. Quadratic v. Hyperbola and Twenty-Five Years v. the Postwar Period

Hoover’s major criticisms of our paper are: (1) we estimated the Phillips curve as a quadratic and, (2) we estimated the Phillips curve over twenty-five years instead of the Postwar era.

Hoover criticizes us for adopting Lipsey’s (1960) approach and estimating the Phillips curve as a quadratic equation instead of a hyperbola. He argues that (1) Samuelson and Solow had a hyperbola in mind when thinking about the shape of the Phillips curve, and that (2) our appeal to Lipsey as justification for using a quadratic function is misguided because Samuelson and Solow did not have access to Lipsey’s paper in 1959 “when they first drafted their paper” (Hoover, 2014, p.15). Somewhat more critically, Hoover suggests that our real motive in using a quadratic equation was because it results in a hump-shaped Phillips curve over the postwar period. Constraining the estimated equation to follow a hyperbola, by contrast, ‘hides the hump’, the likely result being a postwar Phillips curve that is more compatible with the one in Figure 2 of Samuelson and Solow (1960).

We do not find any of these arguments compelling. It is one thing to assert that Samuelson and Solow had a hyperbola in mind when ‘eyeballing’ the US data\(^1\) or that they did not have access to Lipsey (1960) when writing the first draft of their paper, quite another to argue that these are sufficient reasons to reject using a quadratic function.

There is no reason to believe that Samuelson and Solow would have rejected the quadratic equation as being inappropriate for analyzing the US inflation-unemployment

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\(^1\) This claim is not entirely clear. Samuelson and Solow say (1960, p.190, emphasis added): “The English data shows a quite clearly nonlinear (hyperbolic) relation between wages changes and unemployment. ...Our American figures do not contradict this, although they do not tell as plain a story as the English. To the extent this nonlinearity exists...a given average level of employment over the cycle will be compatible with a slower rate of wage increase...the less wide the cyclical swings from top to bottom.”.
data over the twenty-five years of their study. Indeed, Solow indicates as much in his 1960 letter to Lipsey when he says (Hoover, 2014, p. 12, emphasis added) “…he contemplated a detailed empirical study along the lines of Lipsey’s paper”. Furthermore, as Hoover himself documents\(^2\), Samuelson and Solow did have access to Lipsey’s paper when revising their paper for publication in May 1960, so were aware that his preferred function was a quadratic.

The not-so-subtle accusation that we used a quadratic because it gave a hump in the postwar Phillips curve is absurd. It was not until 2010, long after we had written our paper, that this ‘Samuelson & Solow meant a Postwar-era Phillips curve’ meme came to our attention when raised by a journal referee. Then, and only then, did we estimate a Phillips curve for the postwar period. To suggest that we used a quadratic because it gave a hump in the postwar Phillips curve is to say that our decision to use a quadratic when we first wrote the paper in 2007 was based on the expectation of some anonymous referee insisting in 2010 that we estimate a postwar Phillips curve. We never anticipated estimating a postwar Phillips curve because we believed then, as we believe today, that the sample period Samuelson and Solow intended for their Phillips curve is twenty-five years, not the postwar period. Our decision in 2007 to use a quadratic, as stated in Hall & Hart (2012), was based entirely on Lipsey (1960).

Hoover (2014, p. 15) also argues “…the hyperbolic curve is [not] automatically to be preferred to a quadratic” yet he seems to believe it preferable because “…the acceptability of the hyperbolic cannot be tested within the framework of the quadratic curve.” Of course, we can apply the same (illogical) thinking to the quadratic--it is not automatically to be preferred to a hyperbola, but we believe it preferable because any

\(^2\) “…Solow first read Lipsey’s paper in the February 1960 number of Economica” (Hoover, 2014, p. 15).
“hump in the curve” cannot be tested within the framework of a hyperbola. These sorts of circular arguments are not very useful in advancing the narrative.  

Which function--the hyperbola or quadratic--is preferable? The answer is that function which best fits the data--the one that gives the most accurate ‘picture’ of the true relation between inflation and unemployment. Using a quadratic, we found a ‘hump’ in the inflation-unemployment relation for both the twenty-five year period and the postwar era. Using a hyperbola, and only three postwar data points, Hoover finds a postwar-era Phillips curve that is markedly similar to the one in Figure 2 of Samuelson and Solow (1960). He does not estimate the curve over the twenty-five year sample period. But what if Samuelson and Solow did mean twenty-five years? Will a hyperbola generate a line resembling Samuelson and Solow’s Phillips curve?

Using least squares, we estimated the Phillips curve as a hyperbola over the twenty-five year period 1934-1958. The estimated equation is (t-stats in parenthesis):

\[ P_t = 2.428 + 3.41 (1/U)_t \]
\[ (1.96) \quad (0.80) \]

\[ R^2 = .027, \quad \text{SEE} = 3.81, \quad F = .648, \quad DW = 1.21, \]

where \( P \) is the rate of inflation, measured as the Consumer Price Index, and \( U \) is the unemployment rate.  

Figure 1 depicts the Phillips curve that results from this estimation. The line is a hyperbola, but it is also very flat and it never crosses the horizontal axis. In short, it bears little resemblance to the Samuelson and Solow Phillips curve shown in Figure 2 of their paper (or, for that matter, the Phillips curve shown in Figure 2 of Hoover (2014)).

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3 The quadratic is more flexible than the hyperbola. It will approximate a hyperbola if the true relation in the data is hyperbolic, but constraining the estimate to follow a hyperbola will not approximate a hump-shaped estimate if the true relation is hump shaped. It is one thing to select a function that allows a hyperbola, as we did, quite another to restrict the functional form to a hyperbola.

4 The data on inflation and unemployment are the same as those used in Hall & Hart (2012).
Hoover’s second criticism of our paper is that the Samuelson and Solow Phillips curve “…curve is estimated for the postwar period” (Hoover, 2014, p. 9), not twenty-five years as they state in the caption to Figure 2 of their paper. Obviously, this is the critical issue in the controversy.

When we first started this project, we wrote Solow to inquire about (1) the data sources of their paper and (2) the sample periods used to generate Figures 1 and 2. In his reply, Solow (2007) said the sample period for Figure 2 was roughly the twenty-five year period 1935 to 1959. The sample period for the Phillip curve shown in Figure 2 of Samuelson and Solow’s paper was not the postwar period. It was twenty-five years (as stated in the caption to Figure 2 of their paper). Therefore, it is acceptable, using

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5 Hoover tries to reconcile this with the statement by Samuelson and Solow (1960, p. 192) that their Phillips curve was “roughly estimated from last twenty-five years of data.” The gist of his argument is that they believed the relation between inflation and unemployment over the twenty-five year period would have been similar to the postwar period were it not for “special factors” like those created by the Great Depression. In other words, treating the Great Depression “…as if it would have been similar to the Postwar period but for … special factors” (Hoover, p. 9) is sufficient to translate “roughly estimated from last twenty-five years” as ‘estimated from the Postwar period’. We do not find this explanation compelling, preferring instead to take Samuelson and Solow at their word.
Hoover’s words, to “dig in one’s heels” and say, “twenty-five years is twenty-five years.” And the outcome of our ‘let the data—ALL twenty-five years of data—speak’ was a markedly different Phillips curve from the one shown in Figure 2 of Samuelson and Solow (1960).

II. The Historical Importance of Samuelson and Solow (1960)

Hoover argues that we vastly overstate the importance that Samuelson and Solow (1960) had on the profession and on macroeconomic policy. We were wrong in claiming the Phillips curve was “…critical for policymakers” (Hoover, 2014, p. 15, 17); wrong to claim “The idea of the Phillips curve as dominating economic policy in the 1960s”; and wrong in suggesting the Phillips curve was “directly and deeply influential with respect to economic policy in the 1960s and 1970s”.

In the first instance, it should be obvious to the most casual reader that Hall & Hart (2012) was an empirical exercise, not a study of the historical significance of Samuelson and Solow (1960) or the Samuelson and Solow Phillips curve. Hall and Hart (2012) was neither about, nor were we interested in, litigating the historical significance of the Samuelson and Solow Phillips curve to macro policy in the 1960s and 1970s. Like

Readers surely must wonder why we failed to mention Solow’s response in 2010 when an anonymous referee first raised the “25-year vs. postwar era” sample-period issue. When we wrote Solow, we did not anticipate this controversy; our primary interest at the time was their data sources. After receiving Solow’s response in January 2007, we tucked his letter away in a file and, quite simply, forgot about it. When the sample-period controversy first came up in 2010, neither author remembered or recalled that Solow had addressed this matter in his 2007 letter (hence our response in 2010 and in Hall & Hart (2012, p. 70) that: “It was implausible that a paper of this status could have contained such an error for so long without someone having noticed”). It was only recently that one of the authors, while cleaning out his files, ‘stumbled’ onto Solow’s 2007 letter and reread it. Although Solow states in his letter that the sample period was 1935 to 1959, we estimated the Phillips curve over the period 1934-1958. This is because we did not consider that Samuelson and Solow would have had the opportunity to revise their paper in 1960 after 1959 data was available.
most macroeconomists, we believe Samuelson and Solow (1960) was an important paper, one that influenced policymakers and policy in the 1960s and 1970s.\footnote{The major players in the field at the time, both protagonists and antagonist, seemed to think so too. Consider, for example, Robert Gordon and Allan Meltzer, two of the more prominent players. In his history of the Phillips curve, Gordon (2009, p. 3) says: “Phillips does not mention policy implications at all, and this provides the setting in which Samuelson and Solow (1960) christen the relationship as the ‘Phillips Curve’ and explore its policy implications. So widely read and discussed was the Samuelson-Solow article that the term ‘PC’ entered the language of macroeconomics almost immediately and soon became the lynchpin of the large-scale macroeconometric models…” And Meltzer (2009, p. 856), in his definitive history of the Federal Reserve, says: “There is little reason to doubt, and abundant evidence to support, the conclusion that in the late 1960s the Council of Economic Advisers under Gardner Ackley and the Board’s staff under Daniel Brill relied heavily on a simple Keynesian model with a non-vertical, long-run Phillips curve.”}

Hoover’s other criticisms are exaggerated--nowhere in Hall & Hart (2012) do we claim the Samuelson and Solow Phillips curve was “critical” for policymakers or that it was a “dominating” force, “directly and deeply influential” in macroeconomic policy in the 1960s and 70s. The Samuelson and Solow Phillips curve quickly became another weapon in the arsenal of Keynesian policymakers already predisposed towards inflationist policies. It was an important weapon too, but was it the dominant weapon? We do not know, and made no such claim in Hall & Hart (2012).

In his abstract, Hoover (2014, emphasis added) says that we claim “…the “inflationist” course of US macroeconomic policy in the 1960s and 70s would have been different” had Samuelson and Solow used regression analysis to estimate their Phillips curve. Again, this mischaracterizes and exaggerates our argument. What we say in Hall & Hart (2012, p. 68, emphasis added) is that “…economic events might have turned out differently” and “…it might have been for better or worse” had Samuelson and Solow reported an empirically-estimated Phillips curve. And they might have!

Finally, Hoover criticizes us for ignoring Samuelson and Solow’s discussion of possible shifts in the Phillips curve that could improve or worsen the tradeoff. He is
correct—we paid little attention to this matter.\textsuperscript{8} Why? The answer is simple—our interest was with the shape of the Samuelson and Solow Phillips curve, not the stability of the curve over time.

\textbf{III. Conclusion}

We find little merit in Hoover’s criticisms of our paper. Hall and Hart (2012) was first and foremost an econometric exercise whose primary objective was to estimate the Samuelson and Solow Phillips curve over twenty-five years using real data, not imaginary data of the ‘would have been but for special events’ variety. Hoover provides no compelling argument to support his claim that Samuelson and Solow would have rejected the Lipsey-style approach and, as Solow affirmed in 2007, the sample period for their Phillips curve was twenty-five years, not the postwar period. As for putting Samuelson and Solow (1960) in proper context, our empirical results suggest that Leeson (1997) should be part of any contextual analysis of Samuelson and Solow’s Phillips curve.

\textsuperscript{8}So did most of the profession. Dixit (2014, p. 21) sums up the apparent reaction of economists at the time to Samuelson and Solow’s warnings about the (in)stability of the Phillips curve: “In the face of these cautions, it would take a brave reader to read an unqualified message of a stable unemployment-inflation tradeoff in this article, but many readers seem to have done so. Perhaps their simple Figure 2 was too beguiling. One picture is worth a thousand words, and in this case their picture seems to have drowned out five hundred words of caution.”
References


