EJW
A JOURNAL OF THE AMERICAN INSTITUTE FOR ECONOMIC RESEARCH

Econ Journal Watch
Volume 8, Number 1
January 2011, pp 1-12

Unfortunately Unfamiliar with Robert Higgs and Others: A Rejoinder to Gauti Eggertsson on the 1930s

Steven Horwitz[1]

LINK TO ABSTRACT

I too appreciate Gauti Eggertsson's taking the time to respond to my earlier commentary on his *AER* article. One of the purposes in creating *Econ Journal Watch* was to have a space to have just this sort of dialogue, so that the interlocutors and the readership could learn from the interaction. I think Eggertsson's reply has moved the conversation forward, and I hope this rejoinder takes it another step.

In this rejoinder, I want to focus on five specific points. In my original paper (Horwitz 2009), I was concerned to show the ways in which I thought Eggertsson had missed the bigger story about the similarities between Hoover and Roosevelt, particularly the way in which his claims of Hoover's holding "small government" dogmas were, in fact, misleading, if not just wrong. I argued that there was not an abrupt regime change between Hoover and Roosevelt, at least in terms of substantive policy. I did concede that Hoover was more committed to 20-dollars-to-the-gold-ounce than Roosevelt was, but I think that the difference was more a matter of degree than a chasm. After all, Hoover's commitment was to a rather watered down version of the gold standard and Roosevelt did not complete the dollar's devaluation until January 1934.

1. Charles A. Dana Professor of Economics at St. Lawrence University, Canton, NY 13617, and Affiliated Senior Scholar, Mercatus Center, George Mason University, Arlington, VA 22201.

I agree with Eggertsson that the reflation of the money supply prevented the depression from worsening or lingering any longer than it did. I would maintain, as I did in the original article and in line with Friedman and Schwartz (1963) as well as later research by Bordo, Choudhri, and Schwartz (2002) and Timberlake (2005, 210-17), however, that this result could have been accomplished without ending the 20-dollars-to-the-gold-ounce as it then existed. So, in as much as Eggertsson's argument suggests that Hoover's commitment to the 20-dollars-to-the-gold-ounce got in the way of recovery, thereby requiring a regime change and a change in expectations, it overstates the case. Yes, reflation was desirable, given the policy mistakes made during the Hoover years, but that could have been accomplished within the "regime" of the existing gold standard.[2]

The discussion of reflation leads to my second point. I argued in my comment that Eggertsson seemed to be viewing the Great Depression as having ended by 1937, which I then noted was problematic, given that returning to pre-Depression levels of GDP and unemployment took several more years, perhaps as much as an additional decade if one accepts Higgs' (2006) view that the economy did not get all the way out until the postwar boom. Eggertsson argues in his reply that he agrees that the Great Depression was not over in 1937, but that this point works in his favor because the subsequent 1937-38 recession-in-a-depression resulted from FDR's abandonment of the reflationary policy that had generated the 1933-37 recovery.

But it is wrong for Eggertsson to treat 1937-38 as a separate episode. The abstract of Eggertsson's *AER* article says that recovery was driven by a shift in expectations and that this shift "was caused by President Franklin Delano Roosevelt's policy actions" (1476). One of those policy actions was the Banking Act of 1935, which authorized the Fed to change required reserve ratios. The Fed Board of Governors, chaired by Marriner Eccles, an FDR appointee, and fleshed out entirely by other FDR appointees acted closely with Treasury to make use of the new powers in upping the reserve requirements in 1936 and 1937. These actions were part and parcel of FDR's policy activism (see Higgs 2008). In studying the consequences of New Deal policy, we cannot selectively include some consequences and omit others. In as much as recovery was thwarted in 1937 by the monetary contraction, then recovery was thwarted by New Deal policy.

Explanations of the 1937-38 downturn have gone through several stages. The earliest argument was that it was caused by FDR's reversion to more contractionary fiscal policy just prior to the recession. In the eyes of traditional

2. My own policy preference would have been a more radical transformation of the monetary regime, but given the institutional arrangements of the time, the needed reflation could have been accomplished within them.

Keynesians, this apparent link validated their belief that expansionary fiscal policy was key to recovery.[3] Later research cast doubt on this explanation, both because the size of the fiscal contraction was considered too small to have shrunk GNP as much as it actually fell and because monetary policy changes matter more. The decision to raise reserve requirements, from concerns about the inflationary potential of the large excess reserves of banks in the mid-30s, led to a shrinkage in the money supply as banks responded by holding even higher levels of total reserves to replenish their desired amount of excess reserves. There's no doubt that the banks' reaction to the Fed's moves played a role, but this reaction is still this far from the whole story. Indeed, recent research by Paul van den Noord (2010) argues that the combination of changes in fiscal and monetary policy was not enough to explain the 1937/38 downturn. Van den Noord points to other factors as having been "predominant."

One of the other factors involves the labor legislation of the mid-1930s, especially the Wagner Act. These new laws gave labor unions more privileges, encouraged stepped-up organizing and strikes, and resulted in new union contracts that drove wages up significantly, especially in the industrial sector. As Benjamin Anderson (1979[1949], 437-38) said quite clearly, FDR's landslide victory in 1936 solidified powers for the labor unions, especially the CIO unions, emboldening aggressive unionism and exacerbating the regime uncertainty of employers. Again, slicing things up as before and after 1937 is simply bad history. The onset of the subsequent recession seems consistent with the effects of wage increases and the attenuation of the freedom of contract, present and expected. The disproportionate amount of unemployment in the industrial sector is consistent with the labor legislation's disproportionate impact there.

I should note further that the belief that higher wages were the key to recovery and prosperity was perhaps the one idea that Hoover and FDR most clearly shared, along with many other members of the intellectual class, including many economists. One policy that was pursued consistently by both administrations, from Hoover's jaw-boning of industrial leaders and Smoot-Hawley's protection of domestic firms from wage competition to the wage-setting provisions of FDR's National Recovery Administration codes and the aforementioned Wagner Act, was this belief in the purchasing power theory of prosperity. The refusal to let nominal wages fall during the great deflation and the attempt to push them up during the New Deal were, in my view, major factors in generating the enormous human costs of the unprecedented unemployment during the Great Depression, as well as the depression's long duration. The labor

3. Even today, modern Keynesians such as Paul Krugman are making this argument and using it as a rationale for a second, larger, round of fiscal stimulus.

legislation view of the 1937-38 recession suggests, again, that there is much more continuity between Hoover and Roosevelt than some sort of major regime change. Again, Eggertsson cannot treat the 1937-38 recession as apart from Roosevelt's policies, when the Wagner Act and related legislation that caused it were signed by Roosevelt and therefore part and parcel of his 'regime.'

The other major factor that van den Noord points to is Higgs' work on regime uncertainty, which we are getting to. All told, the 1937-38 downturn, rather than helping Eggertsson's interpretation, raises issues about how he has sliced up the history.

My third point follows on the heels of this last observation. Eggertsson continues to maintain that there was a "sharp contrast between FDR and Hoover's views on the size of the government, deficits, and the gold standard" and that this view is the "conventional wisdom," which leads him to be left a little mystified by my insistence on the differences. Eggertsson also argues that all he needs to make his case is that FDR signaled "more" government spending and "higher" deficits than Hoover, and that this signal amounts to a regime change.

It is not clear that simply *wanting* more government spending and higher deficits really constitutes regime change. Eggertsson offers some data on the size of the greater spending and larger deficit. The Roosevelt administration was more activist in these senses than the Hoover administration was, but I think Eggertsson exaggerates the magnitude of the change.

Perhaps one way to see this point is to distinguish issues of scale and scope.[4] What unites Hoover and FDR is the fundamental belief that activist government is necessary both to prevent and to recover from economic crises. As I document in my original response, the historical record of Hoover's views is quite clear: he was a believer in active government intervention in the economy from his first days in government with the Food Administration in World War I through his time as Secretary of Commerce and as president. His own words are sufficiently clear, as were those of President Calvin Coolidge, who tried to keep Hoover at a distance for just this reason, and of Hoover's biographers, such as Joan Hoff Wilson. We certainly know Roosevelt's views about the role of government. Both presidents substantially agreed on the desirable *scope* of government power: for them, it was far more encompassing than their predecessors' preferred scope.

The historical evidence is consistent with this perspective, including the admission by brain truster Rex Tugwell that much of the New Deal was simply extensions of programs that Hoover had begun: "When it was all over, I once made a list of New Deal ventures begun during Hoover's years as secretary of commerce and then as president…. The New Deal owed much to what he had

4. The following discussion owes much to Higgs (1987).

begun" (as quoted in Shlaes 2007, p. 149). Even though Roosevelt did take a step that Hoover did not by greatly attenuating the dollar's link to gold, this action was not a campaign promise of FDR's and, as noted earlier, it was not completed until Roosevelt had been in office almost a year. It seems much more accurate, as I argued in the earlier paper, to view Roosevelt as, to use Shlaes's (2007) apt term, "the great experimenter" than to see him as having promised an administration that was different in kind rather than in degree with respect to the role of government. It is also worth recalling that FDR, during the campaign, attacked what he saw as Hoover's fiscal irresponsibility and called for balancing the budget. If the idea is that a "regime change" would shift expectations, then the similarities between the two presidents and the fact that at least some of FDR's campaign rhetoric was, in fact, marginally less expansionary than Hoover's reality, suggest that there was not really a regime change; nor could it be said that FDR's promises would have shifted expectations ahead of his becoming president.

The underlying problem is that Eggertsson defends his interpretation of the differences between the two presidents as being the "conventional wisdom." And perhaps it is the conventional wisdom. But part of what motivated me to write my comment was that I knew that the historical record showed that the conventional wisdom was mistaken. Whatever the merits of the particular model that Eggertsson constructed, I had hoped to show that it was built on the unquestioned premise associated with that piece of conventional wisdom. Whatever one thinks of model building as a scientific strategy in economics, when one is going to apply a model to historical experiences such as the Great Depression, it is especially important to make sure that the application rests on historically accurate assumptions.

The claim that Hoover was no representative of small government may not be the conventional wisdom in economics, but it is hardly the product of cranks on the intellectual fringes. My original comment cited a number of sources in backing up that claim, including Hoover's own speeches, a prominent biography, and a Pulitzer Prize-winning book on the New Deal. Claiming that he was just following the conventional wisdom does not absolve Eggertsson of, in view of the evidence, getting things wrong. And those errors of history should at least make us skeptical of the explanatory power of the model that rests on them.

Fourth, I want to raise some issues about Eggertsson's response to my discussion of Higgs' work on investment and regime uncertainty. An electronic search of Eggertsson's *AER* article for the word "investment" shows that the word occurs 12 times. I reproduce serially the first eight of those occurrences:

1. "As if mobilizing the nation for war, the government went on an aggressive spending campaign, nearly doubling government consumption and investment in one year." (Eggertsson 2008, 1477)

2. "Panels A–C show a one-year window for commodity prices, the stock market, and a monthly investment index …" (1477).
3. "Similarly, investment nearly doubled in 1933 with the turnaround in March that year." (1477)
4. The complete heading of Panel A in Figure 1 reads: "Investment" (1478)
5. "Investment, commodity prices, and the stock market rebounded once FDR took office." (1478)
6. "The federal government's consumption and investment, for example, was 90 percent higher in 1934 (Roosevelt's first full calendar year in office) than in 1932 (Hoover's last)." (1481)
7. "Table 1 also reports total government expenditures. This measure includes several transfer programs and the gold purchases of the Treasury that are not included in the consumption and investment statistic, but which had an important impact on the government budget." (1481)
8. "Federal government consumption and gross investment" [a line entry in Table 1] (1482)

Occurrences 9 and 10 (of the 12) have to do with the presentation of the model ("there is no investment in the model", 1485; see also 1504, n. 74). Finally, in Appendix C on the data, we find: "The monthly investment series is an index of new plant equipment orders from the 1937 Moody's Industrial Manual (a14)" (1513),[5] and: "The federal government consumption and gross investment component of GDP is from the current NIPA tables" (1514).

In my view, it is paramount to distinguish government and private investment, and explore what is happening to private investment. As one can see from this comprehensive listing, Eggertsson talks of government investment and does not take pains to distinguish government and private investment. To make that point, I reported, accurately, that the expression "private investment" never appears in the *AER* article.

Higgs (2006, 7) argues that the key variable is net private investment. Although gross investment may have improved, when one takes depreciation into account, the picture is much gloomier. To make the importance of this point clear, I reproduce here some information from the Higgs' paper that I cited in my original comment (emphasis in Higgs' original):

5. This appears to be the source for Eggertsson's investment figure in Panel A of Figure 1, p. 1478. Eggertsson does not elaborate on this description of the data. This brief description—"an index of new plant equipment orders"—raises several questions: (1) Orders from whom? Does it include orders placed by government? (2) Orders for what exactly? What does "new plant equipment orders" mean? (3) Were all the orders filled? (4) If this source is meant to gauge national private investment, how good a gauge is it?

> In 1929, when gross private investment was \$16.2 billion, net investment was \$8.3 billion. Net investment fell precipitously to \$2.3 billion in 1930 and then became negative during each of the following five years. In the period of 1931 to 1935, net investment totaled *minus* \$18.3 billion. After reviving to positive levels in 1936 and 1937, net investment again fell into the negative range in 1938 (\$0.8 billion) before resuming its recovery. For the eleven-year period of 1930 to 1940, net private investment totaled *minus* \$3.1 billion. Only in 1941 did net private investment (\$9.7 billion) exceed the 1929 amount.

The fact that net private investment stayed negative for the span of 1931 to 1935 shows that the switch from Hoover to Roosevelt did not exactly revive the private sector. That the total of net private investment from 1930 to 1940 was negative and that it did not return to 1929 annual figures until 1941 suggest that it really did take at least the entire decade for the key sector of the private economy to get back to pre-depression levels. A decade with a negative total of net private investment is hard to describe as one in which a meaningful recovery of the private sector took place.

But the story becomes even clearer when we take another step and distinguish types of private investment as Higgs does:

> We can divide gross private domestic investment into three components that correspond to differing lengths of the newly created capital's expected economic life: gross private new construction (the longest lived); gross private producers durables (intermediate); and additions to business inventories (the shortest lived). During the last five years of the 1920s, on average, these components constituted the following proportions of private investment: 0.62, 0.32, and 0.06, respectively. During the business recovery that was in progress during the first three years of the Second New Deal (1935–37), however, the proportions were 0.38, 0.44, and 0.18, respectively, showing a marked shift away from the longest-term investments. The proportions remained much the same during the second business recovery of the Second New Deal (1939–41), when they were 0.45, 0.40, and 0.15, respectively. Clearly, the real investments made during the first and second Roosevelt administrations remained far more concentrated in short-term assets than the investments made during the latter half of the 1920s. (Higgs 2006, 22)

As Higgs points out, contemporary observers saw that appreciable long-term private investment was not occurring. For example, Phillips, McManus, and

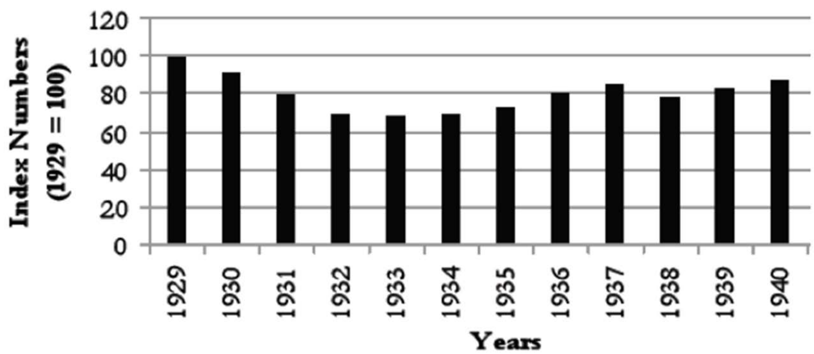
Nelson wrote in their 1937 book: "conditions in the investment market are still such that extensive long-term investment is not being made" (242; see also 218 n. 2, 219; Anderson 1949, 375, 377, 427-28). Economists of quite different ideological stripes—from Benjamin Anderson, Milton Friedman, and Anna Schwartz to Alvin Hansen and Kenneth Roose—agreed that long-term investment had not revived (Higgs 2006, 22).

Eggertsson has misunderstood Higgs' argument. It is not that "regime uncertainty suppressed investment and output suddenly when FDR took power" (Eggertsson 2010, 203). Rather Higgs' point is that regime uncertainty grew over the course of Roosevelt's presidency as he both lurched back and forth from one policy to another (amply documented in the historical record, including the memoirs of FDR's advisors, all of whom indicate that they were groping for possible solutions to the depression) and ratcheted up his attacks on business and economic freedom in general. The combination of these two trends led private investors to be highly reluctant to commit their resources in long-term projects, as the evidence from net private investment indicates. The attacks on the "economic royalists" were a unique feature of the Roosevelt years, especially from 1935 to 1939. Despite the general continuity between Hoover and FDR, these two elements reflect part of the difference in degree that characterized their penchants for government intervention. Roosevelt's interventions, especially the NIRA and AAA, were more comprehensive than Hoover's, and Hoover never engaged in the direct attacks on the business sector that Roosevelt did starting in the mid-1930s. When one also considers the relationship between the Supreme Court and the New Deal policies, whereby the court first declared the NIRA and AAA unconstitutional, but then in 1937 began to put its stamp of approval on similar programs, one can understand why businessmen were confused and apprehensive as to the rules of the game and would refrain from investing, particularly for the long run. Van den Noord (2010) suggests that Higgs' theory "would explain the sharp declines in investment that were the hallmark of the 1937/38 recession."

The turning point that Eggertsson sees in the broadly defined investment data conceals a much gloomier story in private investment, one that is consistent with Higgs' argument and supported by the historical record of the Roosevelt administration. Higgs also presents separate data on both the self-reports of businessmen at the time and the interest-rate spread on short- and long-term corporate bonds to support the claim that FDR's policies were discouraging private investment. It is true that the 1933-37 period was one of significant GNP growth, but the Higgs story suggests that this recovery was largely led by the government and government-related components of GNP, with private-sector investment making scant gains on net. There are a number of ways to create the appearance of growth by raising the government portion of GDP even as the private sector stagnates. If it is the latter that collapsed during the depression, however, we should be looking for growth there as real evidence of recovery.

Besides private investment, another useful measure of private-sector activity is hours worked. This measure separates out government employment and avoids ambiguities in the changing definition of "employment." Robert Higgs (2009) has parsed these data. Below I reproduce his figure of private nonfarm hours worked, 1929-1940. (Higgs also tabulates farm hours, but this measure varies little from about 23 billion over the cycle and therefore is omitted here.)

Figure 1: Private Nonfarm Hours Worked, 1929-1940 (billions)



Source: Higgs 2009, 6, from data reported in Kendrick (1961, 312-13).

While evidence on private investment, and especially long-term investment, is the closest thing we have to a decisive signal of genuine recovery, the data on hours worked in private economic activity also patently debunk the mythology of a sharp New Deal economic recovery. In the New Deal's eighth year, private employment remained substantially below its level in 1929.

Finally, I want to say a word about what one might call Eggertsson's methodological authoritarianism. His reply seems to assume that unless economic concepts or historical evidence can be put into a formal model, or expressed as a change in the constraints on Eggertsson's own DSGE model, it probably will not add to our stock of knowledge about the events in question. He makes this point specifically about regime uncertainty, saying that it would be "quite difficult to generate this story in a reasonably calibrated quantitative model" (203). I don't disagree with that statement, but difficulty fitting regime uncertainty into such a model is not, by itself, reason to dismiss its importance.

In his reply, which followed mine by a full year, Eggertsson writes: "It is true that I do not cite Higgs' work on uncertainty regarding rules, which I am unfortunately unfamiliar with…" (203). Unfortunate, indeed. What prolongs this misfortune, Eggertsson does not say. His admission that he was and remains unaware of Higgs' work on regime uncertainty and the Great Depression more generally is a perfect example of the problems that arise when model-building becomes fetishized as it has been in modern economics. It is not as if Higgs' work were obscure, as it has appeared in the *Journal of Economic History*, *The Independent Review*, and other journals readily available to Eggertsson. Higgs has gathered his series of path-breaking studies in a book published by Oxford University Press (Higgs 2006). Higgs, of course, is not working within the DSGE model-building tradition, but getting outside of that tradition to see what economic historians have to say—economic historians who have dug into not just the standard data, but a whole range of other sources of the era—is precisely what is missing from Eggertsson's original paper. I had also hoped that his reply to my comment would have included more substantive responses to Higgs' work, because I think a serious engagement with it could have moved the conversation forward in some interesting ways. Fortunately, Professor Eggertsson is invited to reply to the present rejoinder, so we have another opportunity to overcome his unfamiliarity with Higgs' research.

The harm such methodological authoritarianism can cause is that our ignorance of the history can indeed doom us to repeat it. As Higgs (2010) argued in the summer of 2010, the weakness of the recovery from the recent recession, particularly the persistence of the high unemployment rate, might well reflect significant regime uncertainty. Private investment is once again quite low, and the Obama administration has gained the enactment of two large regulatory programs (health care and financial reform) whose ultimate effects on the private sector are quite murky. The administration has also upped its anti-business rhetoric as the recovery has faltered. Higgs uses some of the same kinds of evidence he provided to demonstrate regime uncertainty in the 1930s, particularly differences in the yields on short-term and long-term corporate bonds, to offer evidence of such uncertainty in the current recession. The present situation is strikingly familiar, and scary, to those who use economic theory to interpret seriously the narrative historical record of the Great Depression. If we dismiss phenomena such as regime uncertainty because they do not fit certain a priori methodological strictures, we do so at our own peril and that of millions who continue to suffer because economists have not learned the lessons of history.

References

Anderson, Benjamin M. 1979 [1949]. *Economics and the Public Welfare*. Indianapolis: Liberty Fund. **Link**

Bordo, Michael, Ehsan Choudhri, and Anna J. Schwartz. 2002. Was Expansionary Monetary Policy Feasible during the Great Contraction? An Examination of the Gold Standard Constraint. *Explorations in Economic History* 39: 1-28.

Eggertsson, Gauti. 2008. Great Expectations and the End of the Depression. *American Economic Review* 98, September: 1476-1516.

Eggertsson, Gauti. 2010. A Reply to Steven Horwitz's Commentary on "Great Expectations and the End of the Depression". *Econ Journal Watch* 7(3): 197-204. **Link**

Friedman, Milton and Anna Schwartz. [1963] 2008. *The Great Contraction: 1929-1933*, Princeton: Princeton University Press.

Higgs, Robert. 1987. *Crisis and Leviathan*, Oxford: Oxford University Press.

Higgs, Robert. 2006. Regime Uncertainty: Why the Great Depression Lasted So Long and Why Prosperity Resumed after the War, in *Depression, War, and Cold War: Studies in Political Economy*. Oxford: Oxford University Press.

Higgs, Robert. 2008. Banking Act of 1935 + Fed's Exercise of This Authority = New Deal Policy. *The Beacon*, December 5. **Link**

Higgs, Robert. 2009. A Revealing Window on the U.S. Economy in Depression and War: Hours Worked, 1929-1950. *Libertarian Papers* 1. Article no. 4. **Link**

Higgs, Robert. 2010. Regime Uncertainty: Are Interest-Rate Movements Consistent with the Hypothesis? *The Beacon*, August 24. **Link**

Horwitz, Steven. 2009. Great Apprehensions, Prolonged Depression: Gauti Eggertsson on the 1930s. *Econ Journal Watch* 6(3): 313-336. **Link**

Kendrick, John W. 1961. *Productivity Trends in the United States*. Princeton: Princeton University Press.

Phillips, C. A., T. F. McManus, and R. W. Nelson. 1937. *Banking and the Business Cycle: A Study of the Great Depression in the United States*. New York: Macmillan. **Link**

Shlaes, Amity. 2007. *The Forgotten Man: A New History of the Great Depression*, New York: Harper Collins.

Timberlake, Richard H. 2005. Gold Standards and the Real Bills Doctrine in U.S. Monetary Policy. *Econ Journal Watch* 2(2): 196-233. **Link**

Van den Noord, Paul. 2010. Exit Strategy: Is 1937/38 Relevant? ECFIN Economic Brief. European Commission's Directorate-General for Economic and Financial Affairs, Issue 7, February. **Link**

About the Author

Steven Horwitz is Charles A. Dana Professor of Economics at St. Lawrence University in Canton, NY, and an Affiliated Senior Scholar at the Mercatus Center in Arlington, VA. He is the author of two books, *Microfoundations and Macroeconomics: An Austrian Perspective* (Routledge, 2000) and *Monetary Evolution, Free Banking, and Economic Order* (Westview, 1992), and he has written extensively on Austrian economics, Hayekian political economy, monetary theory and history, and the economics and social theory of gender and the family. His work has been published in professional journals such as *History of Political Economy, Southern Economic Journal*, and *The Cambridge Journal of Economics*. He has a PhD in Economics from George Mason University and an AB in Economics and Philosophy from The University of Michigan. He is currently working on a book on classical liberalism and the family. His email is sghorwitz@stlawu.edu.

Go to Gauti Eggertsson's "A Reply to Steven Horwitz's Commentary on 'Great Expectations and the End of the Depression'"

Go to Steve Horwitz's article "Great Apprehensions, Prolonged Depression: Gauti Eggertsson on the 1930s"

Go to Archive of Comments section