

A Reply to Steven Horwitz’s Commentary on “Great Expectations and the End of the Depression”

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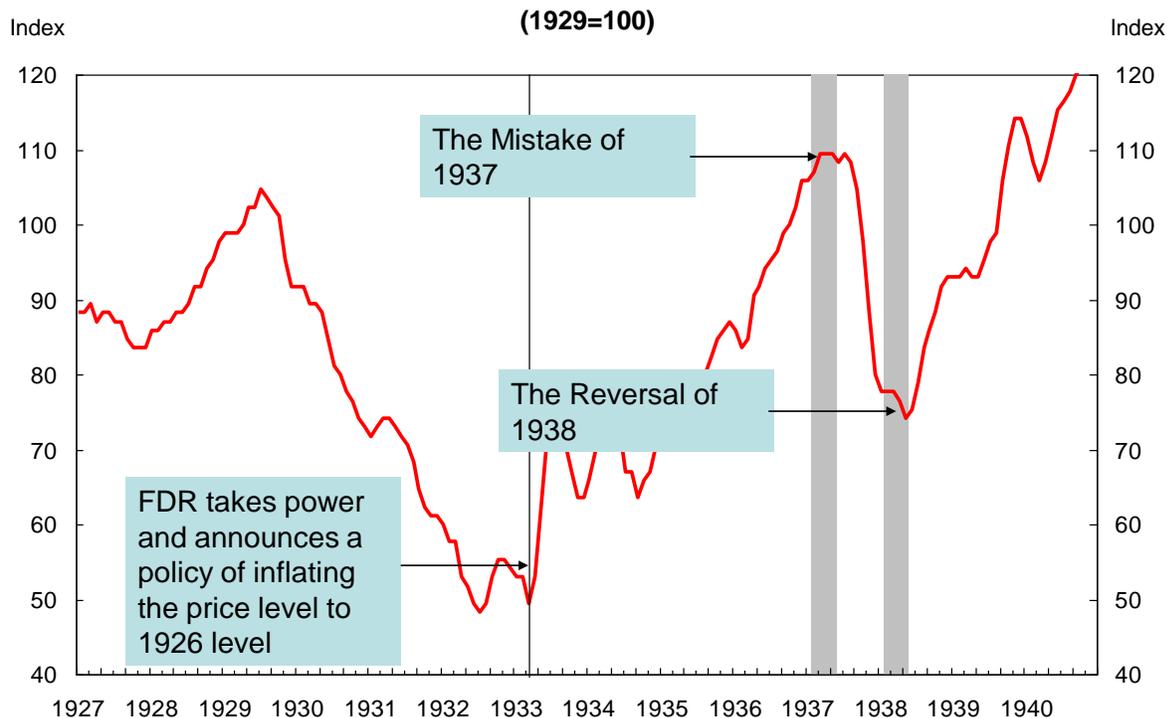
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This note responds to some issues raised by Steven Horwitz’s (*EJW*, September 2009) commentary on my article “Great Expectations and the End of the Depression” (*AER*, September 2008).

The views expressed in the paper are those of the author and are not necessarily reflective of views at the Federal Reserve Bank of New York or the Federal Reserve System. Any errors or omissions are the responsibility of the author. The author thanks Michael Bordo, Marco Del Negro, Andrea Ferrero, Valerie Laporte, Charles Steindel, and Jón Steinsson for comments.

Industrial Production



Source: Federal Reserve Board

Steven Horwitz's commentary on my article "Great Expectations and the End of the Depression" (*AER*, September 2008) appeared in the September 2009 issue of *Economic Journal Watch* (*Horwitz (2009)*). I thank him for drawing attention to the article. I also thank the editors for providing this forum for discussing the paper. This note responds to some of the issues raised.

To start with, let me summarize the paper Horwitz (2009) is commenting on. The *AER* article (Eggertsson, 2008a) proposed that the recovery from the Great Depression in 1933-37 was driven by a shift in expectations. Expectations about prices turned from deflationary to inflationary, while expectations about output turned from contractionary to expansionary. This shift was triggered by a policy regime change characterized by deliberate policy actions by Franklin Delano Roosevelt (FDR). The policy actions included abolishing the gold standard and explicitly aiming for reflating the price level to pre-Depression levels. Furthermore the policy actions included a vigorous fiscal expansion, driven by an increase in government spending and budget deficits. The main contribution of the article is to formalize this hypothesis, which is not entirely new (see e.g. Temin and Wigmore (1990)), within a dynamic stochastic general equilibrium model and explicitly modeling the policy choices of FDR in an infinitely repeated game where the government cannot commit to future policy. In this game the regime change is modeled as "exogenous" abandonment of certain policy "dogmas" or "rules" which constrained Hoover but not FDR (these rules were the gold standard dogma, the small government dogma

and the balanced budget dogma). The paper shows how an exogenous elimination of the dogmas can endogenously explain the policy actions (within the context of a Markov Perfect Equilibrium of the policy game) and quantitatively account for the recovery in prices and output. The paper offers both narrative and quantitative evidence to support this reading of history.

While there are many interesting issues raised in Horwitz (2009) I have chosen here to focus only on the main themes. Horwitz (2009) presents six major criticisms of the *AER* article which are summarized in his introduction. I address each in turn; quoting parts of his claims (while referring readers to his article for more detail) and then provide some brief responses.

1. “It is wrong to view the Great Depression as over in 1937. Just as recovery from an illness is a return to one’s normal state of health, economic recovery is a return to economic normalcy.”

I agree with Horwitz (2009) that the Great Depression was not over in 1937. This, however, does not contradict the paper. Instead, it supports it. Let me explain. The *AER* article is about the first part of the recovery, that is, the period 1933-37. In particular, the article focuses on the turning point in 1933 when the largest downturn in U.S. economic history (1929-33) turned into the largest non-wartime expansion in U.S. economic history (1933-37)—a point that coincided with FDR’s inauguration in March 1933. There was a second contractionary phase of the Great Depression in 1937-38. The “Mistake of 1937”—the action that triggered the second phase—was to abandon the reflationary regime that FDR embraced in 1933 (The ‘Mistake of 1937’ is the title of an earlier paper of mine Eggertsson and Pugsley (2006)). It was only when the administration recommitted to a reflationary regime in 1938 that the recovery resumed.¹ Thus, the sequence of events in the second phase of the Great Depression, shown in the figure above and hence the slowness of the full recovery, provides *additional support* for the *AER* article, rather than contradicting it.

2. “Eggertsson is accurate in his depiction of Hoover as committed to the policy of maintaining the public’s right to convert 20 dollars into an ounce of gold. But that view did not preclude the result that Eggertsson thinks needed to be obtained, namely significant monetary expansion.”

This appears to reflect a misunderstanding. Section V (pp. 1504-06) in the *AER* article explicitly incorporates the gold standard, modeling it as an *inequality constraint*: The money supply had to be *less than or equal to* gold reserves held by the U.S. government in certain proportions. There

¹This about-face on reflation is marked as the “reversal of 1938” in the figure. The two shaded areas signify the turning points and are derived from newspaper accounts see Eggertsson and Pugsley [2006] for details. I use industrial production, since that allows me a month-to-month reading. While I discuss these developments in detail in the article “The Mistake of 1937,” I give the gist of the argument in footnote 11, page 1480, of the *AER* article.

was nothing that precluded the government from holding more gold than corresponded to the outstanding monetary base. As the paper points out, this inequality was not binding in 1929-33. The U.S. government, in other words, had more gold than it needed to support the outstanding monetary base and was thus free to expand money supply. This is also pointed out in Horwitz (2009, see pp. 317-18), citing Friedman and Schwartz. I see no disagreement here. What was strictly binding in 1933 when FDR took power was not the gold standard—and this is a central point of the paper—but the *zero bound on short-term nominal interest rates*.

All this is discussed in quite extensive detail in the paper, but toward the end, in Section V. That section also stresses that even if the gold standard was not binding in 1933, abandoning it was important for expectations. Why? Because even if the inequality constraint was not binding in 1933, people may have believed that it *could* be binding in the future. Thus, the paper argues, eliminating the gold standard was important to shift *expectations* from being deflationary to being inflationary, as the gold standard put an upper bound on the possible monetary expansion *in the future*.

3. “As for the two other alleged ‘policy dogmas,’ namely, balanced budgets and small government, they not only were not ‘almost universally accepted’ but they greatly misrepresent what Hoover believed and what his administration did. “

The AER article is about the regime change that happened as FDR took office from Herbert Hoover and the sharp distinction between their policies. It is the change in policy that is of principle importance for the argument. Horwitz (2009), however, spends a surprising amount of energy to convince us that some other politicians at the time, such as e.g. President Harding and others wanted even smaller government than Herbert Hoover and hence that Hoover was a “big government” supporter relative to Harding. The characterization of Hoover as subscriber of “small government” is therefore misleading, the argument goes, since there were people that believed even more fervently in the principle of small government. This seems to me to be a little bit beside the point.

The argument in the paper only requires that FDR wanted *more* government spending than Hoover and that FDR was willing to maintain *higher* deficits than Hoover, i.e., that there was a regime change in 1933. What someone else wanted, e.g., President Harding or somebody else, and how Hoover’s view contrasted with those views, has limited bearing on the argument. It seems to me that the narrative record, the data and the model speak clearly to this: There was a clear regime change when FDR took power, and there was a sharp contrast between FDR and Hoover’s views on the size of the government, deficits and the gold standard.

Consider the data cited in the paper: Government consumption and investment was *90 percent higher* in FDR’s first full calendar year in office than in Hoover’s last year in office. This seems suggestive of a regime change. Deficit spending, by some measures, was as much as 66 percent higher in FDR’s first year, standing at 9 percent of GDP. This, also, is suggestive of a regime change. Further, the Annual Reports of the Treasury show a change in the basic budget strategy in 1933 (and this, to my best knowledge, has not been emphasized much before in the literature and I find this narrative evidence rather compelling). While Hoover’s Secretary of the Treasury planned to balance the budget—and failed—FDR’s Treasury Secretary planned to do deficit spending—and succeeded spectacularly. (See the discussion in Section I of the AER article.)

Let us briefly turn to the model and how it relates to this data. The *AER* article uses a relatively standard general equilibrium model (similar to what is now in use in most policy institutions and in academic papers on monetary policy) that shows that the regime change proposed can quantitatively account for a large part of the recovery in 1933-37. In summary, I think both the model based exercise and the data are quite supportive of my own reading of the narrative account.

4. “By using the phrase ‘policy dogmas’ to describe Hoover’s views while giving Roosevelt’s no specific label, Eggertsson implies that no dogmas guided Roosevelt’s policies.”

The *AER* article is not a novel reading of the historical record except to a very limited extent. It strikes me as conventional wisdom that Hoover and FDR differed in their views on balanced budgets, big government, and the gold standard (see, for example, Temin and Wigmore [1990] and references therein, as well as references in the *AER* article). What is new in the article is formalizing those particular policy views as explicit constraints, or “dogmas”, in a relatively standard dynamic stochastic general equilibrium model and formalizing this in a dynamic policy game and matching the results to the data.

Debating whether the belief in small government or balanced budgets should be called a “dogma” or something else seems to me to be somewhat redundant. I am perfectly happy to call the “dogmas” in the paper something like “rules” or just refer to them as they are described in the model: One equation says that the government will try to keep spending fixed at some particular (low) level (relative to FDR), another equation says that the government aims at balancing the budget (see equations 16 and 17 in the paper), and finally an inequality constraint for the gold standard (see inequality 53 in the paper). Whatever we call them, I think all these mathematical relationships have reasonable grounds in history.

Were there other equations that perhaps restricted the government’s choices during FDR’s administration (for example, a belief in the importance of redistributing wealth, or hostility towards capitalists)? Yes, perhaps, and this would be very interesting to explore. Horwitz (2009), however, does not suggest particular policy constraints that might be introduced in the model to approximate the narrative record and/or some salient feature of the data more closely. That seems to me to be the most interesting avenue for exploration going forward and I hope that the literature will develop in that direction. It seems to me that the most sensible measure of success in this regard is: Can these additional constraints give a better quantitative account of some particular feature of the data and/or the narrative record?

5. “Yes, the Roosevelt Administration rejected the three positions attributed to Hoover (though in 1932 Roosevelt did promise to balance the budget), and, yes, its bets were decidedly more statist. But it is misleading to view its policies as anything close to a consistent strategy for recovery. At least in the first term, the Roosevelt administration was much more about long-run reform than about a coherent set of recovery policies, and there is no reason to believe that Roosevelt or his administration had a concerted plan about which specific policies to undertake to achieve long-run reform. Eggertsson (1476)

suggests that the regime change was intentionally designed to shift expectations. But much of what Roosevelt did he made up as he went along. The particular policies adopted or proposed by Roosevelt often changed from year to year, season to season, and even month to month.”

Horwitz (2009) is certainly right that there was a great deal of confusion during the initial days of FDR’s administration and—indeed—the whole recovery period. But as I argue in the paper, FDR’s seemingly confusing and contradictory policies all pointed in one direction: Everything was designed to curb deflation and inflate the economy. The hypothesis that this relentless pursuit of inflation triggered the recovery is central to my *AER* paper and to “The Mistake of 1937” (Eggertsson and Pugsley 2006), as well as to the paper “Was the New Deal Contractionary?” (Eggertsson 2008b)), which addresses the National Industrial Recovery Act.

Perhaps it is useful to think of this body of work as formalizing and quantifying the consequence of a general policy strategy elegantly summarized by the historian David Kennedy in *Freedom from Fear: The American People in Depression and War, 1929-1945*: “And yet, amid the chaos of the Hundred Days, and indeed through the tense stand-off of the interregnum that preceded it, one threat flashed and dove like a scarlet skein shot through brocade: inflation.”

6.”Eggertsson gives no discussion of Robert Higgs’ (2006a) work on the debilitating effects of uncertainty regarding rules, which Higgs dubs *regime uncertainty*. Eggertsson flagrantly overlooks the ways in which Roosevelt’s policies generated great uncertainty and great apprehension. The broad ideological dogmas of the Roosevelt Administration stepped up the assault on profits, property, and liberty, and involved virulently anti-business propaganda. As Higgs argues, the combination left private investors fearing for their property and the value of their long-term investments. By heightening apprehensiveness, Roosevelt’s bold experimentation snuffed out private investment in long-term projects. An electronic search determines that “private investment” never appears in Eggertsson’s article.”

It is true that I do not cite Higgs’ work on uncertainty regarding rules, which I am unfortunately unfamiliar with, or any work suggesting that regime uncertainty suppressed investment and output suddenly when FDR took power. The problem with a hypothesis that FDR “created uncertainty”—relative to Hoover—when he took office and that this curbed output and investment seems to me to be the data (see e.g. the figure above). It seems to me quite difficult to generate this story in reasonably calibrated quantitative model. The period 1929-33 marks the deepest depression in U.S. economic history while the period 1933-37 marks the sharpest recovery in U.S. economic history outside of wartime. The turning point was March 1933, exactly the month FDR took office, as seen in the figure above. Was this just a striking coincidence? And given this striking coincidence should we instead be surprised that the recovery was not even faster? My own prior is that the turning point was not a coincidence and my sense is that we should focus on models that can explain it. (In the paper I actually discuss the hypothesis that the recovery was a coincidence and show that this hypothesis has counterfactual implications for the movements in the short-term nominal interest rate).

Regarding investment: Investment, as it happens, also started improving, right around March 1933. This is shown in figure 1, panel A, p. 1478, of the *AER* article's introduction, using investment data from Temin and Wigmore (1990). Admittedly, the baseline model abstracts from investment. The appendix, however, extends it to include investment. I am uncertain as to why an "electronic search" through the paper did not turn up the discussion about investment and the associated appendix. Perhaps one should caution future readers not to rely too heavily on electronic searchers.

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