

## Book Review

Randall E. Parker's *The Economics of the Great Depression:  
A Twenty-First Century Look Back at the Economics of the Interwar Era*

This is Parker's second volume of conversations about the Great Depression. The first volume interviewed a number of prominent economists, such as Friedman, Schwartz, Samuelson, and Tobin, who had lived through the Depression. The new volume begins with an excellent overview chapter that does a nice job of summarizing what happened and laying out in general terms the research that is going to be discussed in the interviews. It then presents a series of conversations with some of the leading scholars on the Great Depression, starting and finishing with two of the grand old men of Depression scholarship, Peter Temin and Alan Meltzer, and wandering through many of the important contributors to this literature, including Lucas, Bernanke, Bordo, Calomiris, Cecchetti, Eichengreen, Hamilton, and Romer. It even includes some input from the more recent literature on the Great Depression in the form of my coauthor Lee Ohanian.

These economists typically did not live through the Great Depression, but many of them come armed with different toolkits than the earlier scholars. Moreover, even within this volume there are substantial differences in the toolkits that these scholars used to examine the Great Depression. The different toolkits account for the substantial differences in how these researchers approach the problem of the Great Depression, the extent to which theory is used in the analyses, the conclusions they reach, and the level of confidence that they have in their answers.

The focus of the interviews are on the causes of the Great Depression and the possibility that something like that could happen again. Issues like the importance of the monetary contraction, the role of financial factors and the gold standard are discussed in quite a bit of detail.

The Great Depression was a largely worldwide downturn that was extremely severe and protracted in several countries. The qualitative features of the Great Depression are relatively standard: output, employment, hours, consumption, investment, exports and imports all fell, and the bankruptcy rate rose for individuals, businesses and banks. Even the falls in prices and monetary aggregates were similar to what we saw during the downturns of the early 1920s and the late 19th century. So, what's novel about the Great Depression is its quantitative magnitude, not its qualitative characteristics. Given this, the fundamental issue for economists seeking to explain the Great Depression is to come up with a *quantitative theory* that can plausibly account for what happened.

The alternative to the quantitative approach is what I'm going to call the *narrative approach*. An example of this would be a storyline which described various interest rate and policy moves, and claimed that they could account for the movements in, say, output, without feeling the need to specify exactly what were the implicit impact and persistence coefficients being assumed and why these values were plausible.

What surprised me in reading over the interviews was the extent to which some version of a narrative approach is still being used. This is occurring despite the fact that the question of the causes of the Great Depression is a quantitative macro economic question and quantitative theory is the coin of the realm in macro for good reason.

Temin's interview develops an example of the consequences of this lack of quantitative precision, when he harkens back 30 years ago, to the numerous debates he engaged in with Friedman about the importance, or lack thereof, of monetary factors. The problem is that discussions of data – in the absence of theory that could guide both the qualitative and quantitative features of the data – doesn't settle anything.

What constitutes a quantitative theory? At its most basic level it is essentially an equation system that maps shocks and state variables into outcomes:

$$y_t = f(y_{t-1}, \varepsilon_t),$$

which when we linearize, gives us something like

$$y_t = Fy_{t-1} + G\varepsilon_t.$$

So a quantitative theory can be thought of as a specification of variables ( $y$ ), shocks ( $\varepsilon_t$ ), impact coefficients ( $G$ ), and persistence parameters ( $F$ ). The key step in any quantitative theory is identifying the shocks, and coming up with a reasonable way of specifying the parameters  $F$  and  $G$ .

One approach to quantification, is to use theory tangentially (if at all) and to estimate a statistical model without an explicit theoretical framework. Examples of the *quantitative atheoretic approach* would include descriptive regressions and vector autoregressions. However, this doesn't, in my view, constitute much of a genuine quantitative theory since the parameters don't have theoretical interpretations, and as a result you can't evaluate whether the size of the parameter is economically plausible. Moreover, the shocks will likely not have economic interpretations.

This doesn't mean that the quantitative atheoretic approach cannot be informative. For example, one can interpret Bernanke's banking panics regressions as saying that the banking

story may be worth exploring, but it's hard to see it as confirming evidence since there is no way to ascertain whether the reduced form coefficients in the regression are reasonable. Another influential example of this type of work is the cross-sectional relationship between exchange rate regimes and the timing of recovery (e.g. Chourdhri and Kochin 1980) which lead many of the scholars interviewed to think that the adherence to the Gold Standard played a major role in the Great Depression.

Putting "wedges" or multiplicative error terms into a theoretical model as in the work of Chari, Kehoe and McGrattan (2002), and using them to match the data is another approach that imposes a bit more theory than the atheoretical approach. However, to the extent that the wedges or error terms also don't have economic interpretations then only a limited amount can be learned. This is because these wedges are indistinguishable from model misspecification. Wedge-type observations certainly motivated Lee and I to pursue our analysis of the role of National Recovery Administration in the U.S.'s weak recover after the trough in 1933. But, they essentially motivated us to write down a different but related model.

But, at the end of the day, the goal of the quantitative approach is to guide us in developing, testing and refining our theories. For that reason, within modern macro the two main approaches to coming up with the parameters of a quantitative model are much more closely tied to the theory: calibration or estimation of a dynamic stochastic equilibrium model. Both these approaches involve specifying a theoretical model of the story you are interested in, and then using that model to quantify the phenomena and to test the theory. Both approaches to quantitative theory lead to a system of equations in which shocks and state variables map into endogenous variables, and the parameters and the shocks in the equations are interpretable from the theory. This *quantitative theoretic approach* has been productive throughout macro because it brings both theoretical insight and data to bear on the issue and provides sharp tests of hypotheses, which clearly focuses the level of discussion and analysis.

Interestingly, it seems to me that the commentators who fully accept the quantitative theoretic challenge have much less confidence in their ability to explain the Great Depression, especially the downturn portion between 1929 and 1932 or 1933. Starting from the narrative end of the research spectrum, Christina Romer is willing to state that

“In the Great Depression, the huge deflation was not the forcing variable; it was the consequence of an aggregate demand contraction that caused output to be about 40 percent below trend.”

While Ben Bernanke, who has done both quantitative atheoretic and theoretic work, says of the mysteries of the causes, depth and length of the Great Depression:

“I don’t think of any of them as a complete mystery. I think we have ideas about all of them. I think that we still may be missing some complete explanations in terms of quantitative magnitudes.”

And Robert Lucas, one of the intellectual architects of the quantitative theoretic approach, asks

“How did it happen that bank failures and monetary declines translated into huge movements in employment and production? We just don’t have a decent theoretical model.”

The lack of confidence among the quantitative commentators isn’t surprising. As anyone who puts numbers in models knows, it is a tall order to write down a model economy with plausible parameters, and account for the depression, and perhaps other observations as well. I am surprised that Romer can be so confident of her identification of a latent variable like a demand shock.

Among those who accepted the quantitative challenge to varying degrees, it seemed like there were three major divides. The first major divide was the difference of opinion as to whether the Great Depression was caused by special factors, or especially large shocks. Special factors might be thought of as including the gold standard, or Fed policy mistakes. Another example of a special factor was the NRA which operated during the U.S. recovery. Under the especially large shocks category, the fall in prices and money, of course, received a lot of attention. In addition, some version of a demand shock came up, as did financial factors like the stockmarket and banking panics.

The second major divide is the extent to which the commentators gave a large role for money or prices in the downturn. Lucas and Rapping (1972) were the first to my knowledge to put numbers in a model and try to quantitatively account for the Great Depression along this dimension. They developed a quantitative version of a signal extraction misperceptions model and found out that while their model could account for some features of the U.S. downturn, it predicted that the cessation of deflation should have led to a much stronger recovery than is in the data. Their experience of finding out sharply what their model couldn’t account for was extremely informative. It focused research on government policies that impeded recovery such as the NRA.

Bordo, Erceg and Evans (2000) argue that a one-sector growth model with nominally fixed wages can generate a large depression such as the U.S. experienced. Their work informs us that this may be a potentially useful channel, while at the same time making clear the challenges that a quantitative version of the sticky wage model faces. Bordo et al assumed

that all wages were sticky, while in the data agricultural wages fell sharply in both real and nominal terms. This appears to be a challenge to this theory. In addition, their model also implies counterfactual rise in labor productivity.

Another quantitative exercise that is discussed in some of the interviews is by Christiano, Motto and Rostagno (2003). They make quantitative the idea of Friedman and Schwartz that liquidity and shifts in the money multiplier can account for the U.S. downturn. Their theory accounts for the downturn of within a fairly complex model that includes a very large liquidity preference shock. Their very large liquidity preference shock induces a big increase in the demand for cash, lowers reserves, prices, and the money supply. This in turn leads to a reduction in loans, and entrepreneurial net worth. The combined impact of all this, and nominal wage rigidity, is to drive down economic activity as in the data. The model accounts for the continued weakness after 1933 by an implicit tax wedge between leisure and consumption. Their work helped sharpened the focus on examining data that can shed light on the size of these liquidity demand shocks. Lee in his published discussion of their paper noted that standard money demand regressions do not exhibit large shocks, which may pose a challenge for this theory.

Lee and I have also examined the evidence on output and prices during the period between 1929 and 1932 in a wide range of countries. We found that while output and prices fell on average, the cross-sectional connection between deflation and output is surprisingly weak. The reason for this is that the overall pattern of deflation was reasonably similar, however the output pattern is more idiosyncratic. We developed our own quantitative version of Lucas's misperception model which included an RBC-style productivity shock. When we viewed these facts through the lens of our model, we concluded that they are not consistent with the view that deflation was the predominate factor in determining output. However, ours was distinctly a minority viewpoint.

The final major divide among those who accepted the quantitative challenge concerns the role of "productivity shocks" in accounting for the movements in output. In the limited amount of data that we have on productivity and output during the Great Depression, there is a very tight connection between the two. Moreover, a standard RBC which takes productivity shocks as exogenous can account for much of what we see in the data. Many of the commentators, e.g. Temin, Hamilton, or Romer, seem to feel that the key shock was a demand shock, and find demand shocks much more plausible than productivity shocks, especially large negative productivity shocks. This leads some of them to react negatively to the RBC finding. However, I think this is in part a misinterpretation of the result.

I would interpret the productivity shock results not as saying that productivity shocks caused the Great Depression, but rather as saying that there was some unknown shock which

worked much like a productivity shock, and if we can identify and quantify this shock, then we can account quantitatively for much of what we saw happen. In this regard, its useful to note that aggregate externality models, which allow for expectational or demand shocks, are observationally equivalent to an RBC model with productivity shocks. Hence, its perfectly plausible that the true shock was a demand shock, and the RBC results are showing that if productivity responded sufficiently to demand, then the overall response of a quantitative model could look like much of what we see in the data.

On the other hand stopping at the level of measured productivity shocks without a theory for what lies behind them is pretty unsatisfactory. Lucas's comment that

“I'd hate to rewrite the Friedman and Schwartz book where the role Friedman and Schwartz assigned to monetary collapses is assigned instead to productivity shocks”

is hard to disagree with. A productivity shock is an innovation to a residual. While factors like scale economies, regulation, idle capital and financial distress could potentially have caused the observed reduction in the efficiency with which capital and labor were employed in production during the Great Depression, having an explicit theory that spell out these connections, and which we could validated by comparing the predictions of this theory with the data, would be a major advance.

Parker is an extremely knowledgeable interviewer and this enabled him to engage the interviewees in discussing their work. He's also a fairly opinionated interviewer and at points that detracts a bit. I thought the volume suffered slightly from the lack of a clear organizing intellectual framework or methodology, but that's probably inevitable for this sort of book. Also, some parts of the book required the reader to have a detailed knowledge of the interviewees work to follow what was being said. Despite this, its a very interesting volume in that it pretty clearly lays out the different approaches and conclusions of many of the key scholars of this period. I came away from the book struck by the fragmentary state of the science with respect to the Great Depression and the challenges that we still face in terms of developing a truly satisfactory quantitative theory of what happened.

## References

Bordo, M., C. Erceg and C. Evans (2000) “Money, Sticky Wages, and the Great Depression,” *American Economic Review*, December.

Chari, V., P. Kehoe and E. McGrattan (2002) “Accounting for the Great Depression,” *American Economic Review*, March.

Choudhri, E., L. Kochin (1980), “The Exchange Rate and the International Transmission of Business Cycle Disturbances,” *Journal of Money Credit and Banking*, November.

Christiano, L., R. Motto and M. Rostagno (2003), “The Great Depression and the Friedman-Schwartz Hypothesis,” *Journal of Money, Credit, and Banking*, Part 2.

Cole, H., L. Ohanian, and R. Leung (2007) “The International Great Depression: Deflation, Productivity and the Stockmarket,” research memo.

Lucas, R. and L. Rapping (1972), “Unemployment in the Great Depression: Is There a Full Explanation,” *Journal of Political Economy*, January/February.