David Romer in What Have We Learned?

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The proliferation of books in which leading economists offer advice on how best to respond to the Great Recession provides snapshots of the current state of macroeconomics. Of particular interest to the GEM Project is the star-studded 2014 IMF collection, *What Have We Learned? Macroeconomic Policy after the Crisis* edited by Akerlof, Blanchard, Romer, and Stiglitz. It captures mainstream thinking well, revealing fundamental problems that prevent consensus modeling from supporting effective stabilization policymaking. This post looks at one of the best chapters, “Preventing the Next Catastrophe: Where Do We Stand?”, by David Romer.

Romer gets a number of big issues right. Most important is his emphasis that financial shocks are both “commonplace” and heterogeneous. He identifies six such U.S. crises in the past three decades. Three shocks produced minor domestic macro costs: the 1980s collapse in Latin American government debt that severely depleted capital in the U.S. banking system, the 1987 collapse in the stock market which was initially larger than the famed 1929 Black Friday debacle, and the late 1990s rolling devaluations and debt defaults beginning in the building-boom of the Asian Tigers, engulfing Russia and Eastern Europe, threatening Brazil and Latin America, and punctuated by the failure/bailout of the huge Long-Term Capital Management hedge fund. Moderate macro costs were generated in two financial shocks: the widespread 1980s S&L failures requiring bank bailouts, much greater than in 2008-09, by U.S. taxpayers (as well as the commercial banking system) and the turn of the century bursting of the dot.com bubble that directly wiped-out a trillion dollars in wealth and contributed to the 2001 recession, which was nonetheless surprisingly mild. By comparison, huge welfare losses were incurred in the recent Great Recession.

Romer’ brief history makes two critical points about financial crises. First, such shocks cannot be dismissed, as some economists persist in doing, as one-off instability phenomena that are somehow outside the mandate of macroeconomics. Second is an especially crucial policy-related question: Why have financial-shock outcomes differed so greatly over the past 30 years? In particular, what was it about 2008-09 that caused it to be so damaging? After all, Fed analysis of the mortgage market prior to the Great Recession assured Greenspan, Bernanke, and other stabilization authorities that the direct costs of a collapse in subprime paper would be relatively minor.

Romer does not elaborate on the crisis-heterogeneity issue. Abandoning that central question allows him to join most of his *What Have We Learned?* colleagues in focusing on regulatory policies designed to squeeze risk out of, and perhaps ultimately eliminate, large banks. Romer’s brief history of financial disturbances little supports the idea that large U.S. banks have played an intrinsic role in past financial shocks or that their elimination will prevent future ones. Lehman Brothers was, after all, relatively small; and very large banks that are inevitably interconnected with U.S. financial markets would remain ubiquitous elsewhere in the world.

The emphasis on regulation by mainstream economists, however, is not surprising. Consensus market-centric macro modeling that underlies the IMF volume recommendations cannot coherently accommodate a causal link from nominal demand disturbances to involuntary job loss, pushing Romer and his colleagues to downplay the dominating role aggregate-demand management, a fundamental error. The inability to suppress rational wage recontracting has forced theorists who eschew free parameters to orient their business-cycle research around voluntary job separation and unemployment produced in the ubiquitous search/match/bargain model class. It is difficult for even well-meaning macroeconomists to refute the analytic content of their careers, what they have written and what they teach. Out of fundamental limitations of mainstream macro modeling, ever greater banking regulation has become the ubiquitous, albeit the badly misleading, default instrument in the prevention of future Great Recessions.

The GEM Project provides a better model, one that is coherent, stabilization-relevant, and supportive of effective policies to thwart a repeat of the huge 2008-09 welfare loss. (See Annable and Schecter, “Modeling Extreme Instability” (GEM Project Working Paper No. 3, 2014).) Three elements of the generalized-exchange modeling of effective policy response to extreme instability are most relevant here. First, the Project demonstrates the necessity of two distinct classes of useful policymaking: financial-industry regulation and the management of nominal demand disturbances that propagate financial shocks, usefully separating the latter into spending disruptions that are either stationary or nonstationary.

Second, the Project demonstrates what economists should already know. The lion’s share of the welfare loss in the 2008-09 extreme instability resulted from the nominal-demand propagation of the housing-related shock. The direct costs don't come close to the propagation costs, supporting the Fed's analysis prior to the crisis. It follows that work to enhance the tools available to stabilization-authorities to effectively intervene in total spending is by far the most productive area in which to search for policies to prevent future Great Recessions. Designing ever more restrictive regulation is a deeply inadequate response. That conclusion is supported by the stubborn persistence of heterogeneous financial shocks. Romer mentions the inherently lightly regulated shadow banking system, which is well understood to be both the preferred home of outsized risk-taking activities and generally avoided in aggressive regulation, but avoids working through its important implications.

Third, the Project's generalized-exchange modeling identifies the crucial missing variable in the badly truncated mainstream analysis of extreme instability: investor/lender perception of the credibility of the trend real-side objective of stabilization authorities. (For elaboration, see Chapters 6 and 10 of the website's e-Book.) That identification provides an additional critical area of research needed to support the design and execution of extreme macro-instability prevention. It is extraordinary, especially from theorists who built careers on the criticality of managing confidence in central banks' nominal objectives, that real-side objectives receive scant attention in the IMF volume. All in all, the answer to the titular question in *What Have We Learned?* is, so far at least, disappointingly little.

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