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Presidential Address

The Profession and the Crisis

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So we're having an economic crisis. I say "having," not "had," because we have by no means recovered. Financial panic may have subsided, stocks may be up, but employment remains far below pre-crisis levels, and unemployment — especially long-term unemployment — remains disastrously high. And while you can make the case that the economy is slowly on the mend, slowly is the operative word. We have already been through two years of economic purgatory, and there's no end in sight.

There is a real sense in which times like these are what economists are for, just as wars are what career military officers are for. OK, maybe I can let micro-economists off the hook. But macroeconomics is, above all, about understanding and preventing or at least mitigating economic downturns. This crisis was the time for the economics profession to justify its existence, for us academic scribblers to show what all our models and analysis are good for.

We have not, to put it mildly, delivered.

What do I mean by that? As I see it, there are three main complaints one can make about economists and their role in the current crisis. First is the complaint that economists fell down on the job by not seeing the crisis coming. Second is the complaint that economists failed even to see the possibility of this kind of crisis — and that by pointing out the possibility, they could have helped head the crisis off. Third is the complaint that they have either failed to offer useful advice on what to do after the crisis struck, or that they have offered such a cacophony of voices as to provide no useful guidance for policy.

As I see it, the first complaint is mostly — though not entirely — unfair. The second is much more substantial: anyone with some knowledge of history should have realized that the age of financial crises was far from over. But the most damning failure of economists, I'd argue, was their acquired ignorance of what I've called depression economics — the principles that should govern policy after a financial crisis has left conventional open-market operations impotent.

So let me walk through these issues one at a time.

PREDICTING THE CRISIS

What should economists have known about the impending crisis, and when should they have known it?

Ask any one economist that question, and by and large the answer is that they should have known what he or she knew, and can be excused for not knowing more. Me too!

Clearly, it's not fair to demand that economists have known that Lehman would go bust on September 15, 2008; in fact, I think most people would agree that it's unrealistic to have expected economists to get either the year of the crisis or the firms that fell first right. But should they have seen a crisis building several years before it happened? Should they have had at least a rough idea of how bad it would be?

Well, from my point of view — which, because I'm like everyone else, is that what I saw and no more is what everyone should have seen — it still seems bizarre how many economists failed to see that we were experiencing a monstrous housing bubble. As Robert Shiller has documented — and, crucially, was documenting in real time circa 2004–5–6 [Shiller 2005] — the rise in real housing prices after 2002 or so took them into completely unprecedented territory. It was the clearest market mispricing I've seen in my professional life, even more obviously out of line than the dot-com bubble, which at least had the excuse that it involved novel technologies with unknown potential; houses have been with us for 7,000 years or so, and we should have a reasonable idea of what they're worth.

So why were so relatively few economists willing to call the bubble? I suspect that efficient market theory, in a loose sense — the belief that markets couldn't possibly be getting things that wrong — played a major role. And in that sense there was a structural flaw in the profession.

What about what would happen when the bubble burst? I personally failed to realize how big the "knock-on" effects would be; and according to the self-justifying principle, I'm tempted to say that nobody could reasonably have been expected to get that right. But actually, we should have seen that coming too — maybe not in full detail, but even a casual walk through historical crises should have indicated that a housing bust was likely to bring large financial and balance-sheet problems in its wake. I kick myself every once in a while for failing to think that part through. In particular, those of us who had worked on the Asian financial crisis of the 1990s had placed large weight on balance-sheet effects [Krugman 1999]. Why didn't I think of applying the same logic to the coming bust in US home prices?

Beyond that, surely experts in banking and finance should have been aware of rising leverage, of the growing reliance on unregulated shadow banking, and so on. It's quite remarkable how few warnings we had that the system might be dangerously fragile. By all means, let's give credit to people like Rajan [2005] who saw some of it; but the very fact that such people were given a hard time for their analysis is telling about the profession.

Still, as Yogi Berra said, it's tough to make predictions, especially about the future. There are so many things going on in the world, many of them off any modeler's radar, that the profession's failure to see this crisis coming is not, in my mind, anything close to its biggest sin.

UNDERSTANDING THE POSSIBILITY OF CRISIS

One can make excuses for the failure of the economics profession to foresee that the 2008 financial crisis would happen. It's much harder to make such excuses for much of the profession's failure to realize that such a thing *could* happen.

Banking crises are, after all, a theme running through much of modern economic history. Nobody should be able to call himself a macroeconomist unless he has a working knowledge of what went down in 1931, both in the United States and in Europe. And you don't have to go back to the 1930s, either, as long as you're willing to step outside the United States and core Europe. With the Scandinavian crises of the early 1990s, the Asian crises of the late 1990s, Argentina, and so on, there should have been ample reason to at least consider whether it might happen here.

Nor are crises a case of something that can happen in practice, but not in theory. Diamond-Dybvig [1983] isn't a perfect model of what we've just gone through, but it is a canonical model showing how bank runs can happen — and it's hardly obscure. Nobody should be looking at the stability of a financial system without thinking to himself, "Hmm. Is there a way this system could experience a Diamond-Dybvig-type crisis?"

It's true that Diamond-Dybvig tells us that deposit insurance ends the possibility of bad equilibria in which everyone tries to pull out of the banks, creating a self-fulfilling prophecy of financial collapse. And I'm afraid that the way many economists read the paper was as an essay in economic history, a description of what could go wrong in the bad old days. But this was a crude mistake. In fact, a proper reading of the D-D paper, far from making the profession comfortable about the stability of our system, should have raised major doubts.

For the right question to ask after reading Diamond-Dybvig is, what constitutes a "bank" from the point of view of this model? And the answer is that it doesn't have to be a big marble building with a row of tellers — that is, a depositor institution. As far as the model is concerned, a bank is any institution that borrows short-term and uses the funds to make longer-term, illiquid investments. And that, right there, should have led to the next question: what institutions do we have that fit this definition, but are not depository institutions, and are not covered by either deposit insurance or the regulations designed to limit the moral hazard that insurance creates?

If economists had followed that line of thought, they would have been led right to the risk posed by the rise of shadow banking. They would have seen that money-market funds and repos were functionally just like deposits, but without the safeguards. They would, in short, have realized that a 1931-type banking crisis was very much a real possibility in 21st-century America. But they didn't.

Now, I'm increasingly of the view that what we've been going through is more than a banking crisis, that it's a more general balance sheet crisis. And I like to think that my own recent work with Gauti Egertsson trying to model that kind of problem [Eggertsson and Krugman 2010] helps to clarify the nature of such a crisis. But like the possibility of a banking crisis, this was a possibility of which economists should have been well aware. I've already mentioned the importance of balance-sheet considerations in analyses of the Asian financial crisis of the 1990s. And there is a long if somewhat thin tradition of focusing on leverage and its macro risks, running from Fisher [1933] to Minsky [1986] to Koo [2008].

And if you want your models formal with plenty of math, there's Kiyotaki and Moore [1997], which is a model of how falling land prices can force a reduction in spending by debtors — very much like what actually happened after 2008. I suspect, however, that the Kiyotaki-Moore paper failed to have much impact on policy analysis because it was set entirely in a real-business-cycle-type framework; more on that sort of thing in a minute.

The overall point should be clear: economists had good enough intellectual frameworks to have seen the risk of something like the banking and balance sheet crisis that burst upon us in 2008. But they ignored that risk.

My best answer is that they were caught up in the spirit of the times, with its faith in the wisdom of markets and of the financial industry. Nobody could deny the possibility of runs on conventional banks, which have happened so often in history. Few could deny that debt deflation had happened in the past. But to argue, or even to think about, the possibility that the old evils could manifest themselves in new forms would have been to question the whole basis of decades of policy, not to mention the foundations of a very lucrative industry. You don't have to invoke raw corruption (although there may have been some of that) to see why this was a line of thought few were willing to pursue. And by not pursuing that line of thought, the profession fell down badly on the job.

Yet the profession's worst failure wasn't what it failed to see before the crisis. It was what happened after crisis struck.

DARK AGE MACROECONOMICS

Early in 2009, when the Obama stimulus was under discussion, I was stunned to read statements from a number of well-regarded economists asserting not merely that the plan was a bad idea in practice — a defensible idea — but that debt-financed government spending could not, in principle, raise overall spending. Here's John Cochrane:

"If the government borrows a dollar from you, that is a dollar that you do not spend, or that you do not lend to a company to spend on new investment. Every dollar of increased government spending must correspond to one less dollar of private spending. Jobs created by stimulus spending are offset by jobs lost from the decline in private spending. We can build roads instead of factories, but fiscal stimulus can't help us to build more of both. This is just accounting, and does not need a complex argument about 'crowding out.' "

I won't go into detail here about why that's wrong. Let's just say that statements like this reveal a complete ignorance of almost 80 years of macroeconomic analysis. Even the simplest multiplier model tells you that while it's true that $S = I$, that equals sign cannot be replace with an arrow running from left to right.

But what became clear in the policy debate after the 2008 crisis was that many economists — including many macroeconomists — *don't know* the simplest multiplier analysis. They literally know nothing about models in which aggregate demand can be determined by more than the quantity of money. I'm not saying that they have looked into such models and rejected them; they are unaware that it's even possible to tell a logically consistent Keynesian story. We've entered a Dark Age of macroeconomics, in which much of the profession has lost its former knowledge, just as barbarian Europe had lost the knowledge of the Greeks and Romans.

As long as monetary policy could bear the burden of macroeconomic stabilization, this didn't seem to matter too much: even as equilibrium business cycle theory became increasingly dominant in graduate study, central banks, like medieval monasteries, kept the old learning alive. But once we were hit with such a severe banking and balance sheet crisis that monetary policy hit the zero lower bound, it was crucial that the economics profession be able to weigh in knowledgeably and coherently on other possible actions. And it turned out that it couldn't.

You often hear people saying that the crisis has revealed the need for new economic thinking, for new ideas about macroeconomics. Yet the first priority seems to be to resuscitate old ideas. Brad DeLong describes an interview of Larry Summers by Martin Wolf as follows: "Asked to name where to turn to understand what was going on in 2008, Summers cited three dead men, a book written 33 years ago, and another written the century before last." And in my view, Summers basically got it right.

How did all this knowledge get lost? Well, being the age I am, I was able to watch the transformation of macroeconomics in real time, and I'd say that what happened was a runaway social process.

First, success in academic economics came from publishing "hard" papers — meaning papers that used rigorous and preferably difficult mathematics. This in itself biased publication toward equilibrium business cycle models, as opposed to the *ad hoc* modeling typical of what I consider useful macroeconomics. Graduate education, in turn, became increasingly focused on the kind of work that could get published and lead to tenure. Successive cohorts of students were trained only in the newly rigorous version of macro, which had lost touch with the field's previous intellectual achievements.

And as these cohorts became professors in their turn, they closed off both publication and promotion to anyone who questioned the dominant academic approach. Robert Lucas wrote more than 30 years ago — approvingly! — about how participants in seminars would "whisper and giggle" when someone presented a Keynesian analysis. No wonder that any non-equilibrium ideas dropped out of the curriculum and the conversation.

All of this would have been OK if the triumph of anti-Keynesianism was justified by superior empirical success. But it wasn't. As I read the history of the equilibrium approach, it's a story of failing upward. Lucas-type models clearly failed to account for the duration of slumps; rather than reconsider flexible prices and rational expectations, Lucas's followers moved on to real business cycles (RBC). RBC models failed to generate any strikingly successful predictions, and in fact lost whatever plausibility they had once productivity started becoming pro-cyclical rather than counter-cyclical. But by that time the people doing these models didn't know that there was any alternative.

And the result was that faced with a severe economic crisis, the profession spoke with a cacophony of voices. Or maybe a better way to put it is that the policy debate of 2009–2010 was virtually indistinguishable from the policy debate of 1931–1932. Long-refuted doctrines that should have been consigned to the dustbin of history were stated as if they were fresh new ideas — and they were fresh and new to many economists, because our profession had lost so much of its heritage.

In short, in responding to the crisis, the profession presented a sorry spectacle of unnecessary ignorance that didn't even recognize itself as ignorance, of bitter debate over issues that were resolved many decades earlier. And all of this, of course, made the profession mostly useless at a time when it could and should have been of great service. Put it this way: we would have responded better to this crisis if macroeconomics had been frozen at the level of knowledge it had in 1948, when Paul Samuelson published the first edition of his famous textbook. And the result has been to leave actual policy discussion without any discipline from the people who should be shaping that discussion: politicians and officials have been free to follow their prejudices and intuitions, never mind the lessons of history and analysis. Economists have failed to fulfill their social function.

NOW WHAT?

I'm sorry if I have painted a bleak picture of the role of economists in the crisis. Unfortunately, that's the way it looks to me. So what can be done to improve that picture?

Some economists are pushing forward with new macroeconomic models that incorporate the lessons of the crisis. Me too! And by all means, let's do that. But as I've said, our big problem was not lack of models.

There are also many calls for new economic thinking; there's even an institute dedicated to that project. Again, fine — but the biggest problem we had as a profession wasn't failure to keep up with a changing world, it was failure to remember what our fathers learned.

What we really need is a change in the destructive social dynamics that brought us to this point. And I wish I knew how to do that. But my problem is obvious: I'm an economist, and it seems that we need some kind of sociologist to solve our profession's problems.

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