

What shortcomings in macroeconomic theory and modeling have been revealed by the financial crisis and how should they be addressed in the future?

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This panel session is a wonderful opportunity to think about the lessons for researchers that have emerged from the recent economic crisis. I organize my comments around three points. First, the key modeling and policy issues associated with the recent crisis were crystallized in two thirty year old papers: Kareken and Wallace (1978) and Diamond and Dybvig (1983). So whatever else is true about the shortcomings of modern macroeconomics, the notion that it has little to say about the crisis is just wrong. Second, there are both good and bad reasons for why mainstream pre-2008 dynamic stochastic general equilibrium (DSGE) models did not place much emphasis on financial market frictions. Third, significant further progress on understanding key financial market puzzles will involve making progress on modeling heterogeneity in beliefs and persistent disagreement between agents.

Macroeconomists have long thought about financial crises. As Sargent (2010) argues, it is useful to center these thoughts on the models in Diamond and Dybvig (1983) and Kareken and Wallace (1978). The key policy question addressed in those papers is: 'What do government lender-of-last-resort and deposit insurance programs do to stop or promote financial market crises?'

Sargent (2010) calls these models polar because in Diamond and Dybvig's (DD) model, deposit insurance programs have purely beneficial effects, whereas in Kareken and Wallace's (KW) model, these programs have purely negative effects. These differences reflect the different causes of financial market instability in the two models. In the DD model, instability of financial markets arises from a temporal mismatch between financial intermediaries' assets and liabilities. In the KW model, instability arises from mispriced government deposit insurance and the implied distortions in banks' portfolios.

Consider first the DD analysis. Here banks are modeled as producing maturity and liquidity transformations that improve social efficiency. Banks' activities enable depositors to make long-term investments even though they hold liabilities that are short-term in duration. Through these activities banks facilitate risk sharing among people with uncertain future liquidity needs.

A potential problem in the arrangement is that for long-term investments to come to fruition, enough patient depositors must leave their funds in banks to avoid premature liquidation of banks' long-term investments.

Without deposit insurance, even patient depositors may want to withdraw their funds early, causing banks to prematurely liquidate long-term investments. In particular, bank runs can be triggered by patient depositors' private incentives to withdraw early if they think that other patient investors are choosing to withdraw their deposits early. Formally, there exist multiple Nash equilibria, some good and some bad in a welfare sense. The equilibria in which bank runs do not occur correspond to good social outcomes while the equilibria in which runs do occur correspond to bad social outcomes.

The good news is that, in the DD model, there is a 'simple' solution to bank runs: government-supplied deposit insurance. Even better news is that publically provided insurance is virtually costless to the government because in equilibrium, there won't be any bank runs.

There is nothing special about commercial banks, either in DD's analysis or in reality. DD's analysis applies to *any* institution that is in the business of liquidity and maturity transformation. Just substitute the phrase rollover risk for bank-run risk and go from there. In 2008, there were all sorts of institutions that were really banks in the DD sense but who did not have access to explicit deposit insurance. These entities, otherwise known as the shadow banking system, were the key players in the recent crisis.

Sargent (2010) points out that we can use the DD framework to provide a sympathetic interpretation of policymakers' response to the financial crisis. Policymakers looked out their windows in the early fall of 2008 and saw DD bank runs all over the place. From this perspective, the correct response was to stop the runs at the earliest stage possible by convincing creditors that their short-term loans and deposits were effectively insured. Accordingly, policymakers in the U.S. and Europe provided new short-term loans that markets weren't willing to supply and effectively guaranteed shadow banks' creditors' assets. In this way, policy saved fundamentally good institutions and minimized damage to the economy from the outbreak of bank runs. As in DD, the icing on the cake is that the 'rescue' could be accomplished at little or no eventual cost to the taxpayers.

The KW framework provides a competing and darker vision of recent events. Instability of banks in the KW model arises from mispriced government deposit insurance and the implied distortions in banks' portfolios. To make this point in as stark a manner as possible, KW suppose there are complete markets and that some people want to hold risk-free deposits. Absent deposit insurance, depositors who want risk-free deposits must hold them in banks that hold risk-free portfolios. Naturally such banks emerge in equilibrium. But suppose that there is deposit insurance that is either free or is priced too cheaply. Then it is privately optimal for banks to become as risky and as large as possible. With positive probability, banks will fail and taxpayers have to compensate banks' depositors.

The critical policy lesson is that if the government sets up deposit insurance and doesn't regulate bank portfolios, then it is setting the stage for a financial crisis. Similarly if the government deregulates financial institutions, it must first reform deposit insurance to prevent sewing the seeds of a crisis. US policymakers of the 1970's chose to not heed KW's advice. The resulting harvest yielded the Savings and Loans crisis of the 1980's.

The lesson for our time is clear. Any institution that knows it is too big or too complex to fail has an incentive to take on high degrees of risk. Claiming that you won't bail these institutions out isn't credible because it is not time consistent. After the fact it is just too costly to let the shadow banking system collapse and induce a huge recession. Straightforward KK logic tells you that have to regulate such institutions.

So here we have a darker view of the recent crisis. The U.S. deregulated financial markets in the 70's and 80's. Policymakers failed to effectively regulate financial firms and did not credibly commit to let shadow bank creditors suffer large losses. These firms took highly leveraged, risky positions, helping to ignite a huge run up in housing prices. Once real estate markets peaked, many important players in the shadow banking system became insolvent. Policymakers caved into the time consistency problem and effectively bailed their creditors out. So the class of 2008 policymakers had to pay for the sins of their predecessors who failed in their regulatory duties.

In sum we have two competing visions of the origin of the recent crisis and the appropriate policy response. KW's analysis instructs policymakers to be very cautious about lender-of-last-resort facilities and very sensitive to the risk-taking activities of banks. DD's analysis alerts policymakers to be sensitive to the possibility of runs on the financial system and the need for mechanisms to stop runs.

No doubt there is some truth to both visions. In real time policymakers had to place their bets on which of the visions was more important. It's clear that during the crisis they put their chips on DD. At the same time they promised to pursue financial regulation in the future. Many of us are waiting for a meaningful delivery on that promise.

Looking forward, macroeconomists face two key tasks. First, we need to assess the quantitative importance of the risks stressed in the DD and KW visions. Second, we need to help design institutional reforms for minimizing both sets of risks while allowing the financial system to perform its socially beneficial functions.

Now that the word quantitative has been mentioned, it seems fair to ask: ``where were the dynamic stochastic general equilibrium (DSGE) models during the 2008 crisis and its immediate aftermath?''

With some important exceptions, few DSGE models placed financial market frictions at the center of their analyses prior to the crisis. Important exceptions to this pattern are

Bernanke, Gertler and Gilchrist (1999) and Christiano, Motto and Rostagno (2009). While useful, these models have risk residing directly in real returns at the level of goods producing firms. But our recent problems originated in the financial system, not so much in the riskiness of production itself.

Since the crisis, there has been a burst of good work incorporating financial market frictions into DSGE models. But there are still very few DSGE models in which shocks originate from within the financial system, rather than the production sector. Moreover all of the DSGE models that I'm familiar with rely on exogenous shocks to someone's net worth. Such models can certainly be used to assess how policy should respond to shocks. But we need richer models that let us assess the efficacy of alternative regulations that make 'shocks' less likely and less intense.

We've certainly known for a long time that financial frictions could act as important propagation mechanisms to shocks. So why didn't DSGE modelers place more emphasis on those frictions? In practice we have to work with simplified models. So we have to make choices about which frictions to emphasize. One good reason not to have emphasized financial frictions in DSGE models is that until the recent episode, post-war recessions in the U.S. and Western Europe did not seem closely tied to disturbances in financial markets. True there was the Savings and Loans crisis. But it was a localized affair that did not metastasize into anything like the 'Great Recession'. Similarly, the stock market meltdown in the late 1980's only had minor effects on aggregate economic activity. So guided by the postwar data from the U.S. and Western Europe, modelers emphasized other non-financial market frictions in thinking about economic fluctuations.

There was also a bad reason for why DSGE modelers did not emphasize financial market frictions: we focused too narrowly on postwar U.S. and Western European data. Even if we leave pre-war history aside, the post-war era has been marked by numerous currency crises. Many of these crises, especially in the post-1980 period, amounted to twin banking/currency crises. Moreover, many of the policy debates, especially those surrounding the collapse of fixed exchange rate regimes in South East Asia, closely parallel the DD and KW debate.

The crisis countries in South East Asia were not running large deficits or suffering from any standard problems that would lead to an exchange rate crisis. So-called 'second-generation' models explain these currency crisis episodes as self-fulfilling expectation crises whose essential nature is a DD bank run (see Burnside, Eichenbaum and Rebelo (2008) for a review of alternative currency crisis models). In contrast, 'third generation' models emphasize the importance of implicit or explicit government guarantees to banks' foreign creditors under fixed exchange rate regimes. Burnside, Eichenbaum and Rebelo (2001a, 2004) argue that, in the presence of such guarantees, it is optimal for banks to expose themselves to exchange rate risk and declare bankruptcy if a devaluation occurs. But a devaluation transforms potential government liabilities into

actual liabilities. So government guarantees create the possibility of self-fulfilling currency crises.

As in KW, fundamentals, in the form of government guarantees, determine whether a twin banking/currency crisis will occur. But, as in DD, the precise timing of the crisis is a multiple equilibrium phenomenon. So third generation currency crisis models build on key features of the two polar models of financial instability.

Now all models have to take *some* institutional frameworks as given. While flaws in our institutions can have intense effects, these effects occur at irregular, infrequent intervals. We can be blindsided if we focus on relatively short time series from broadly similar countries. The time series are not sufficiently informative to easily detect low-frequency fault lines in our institutions. Economic history and data from countries less similar to our own can be highly informative in helping to detect those fault lines.

I leave it to the economic historians to chastise macroeconomists for ignoring their work. I will chastise mainstream macroeconomists for not paying more attention to the post war experiences of emerging markets. The currency crises of the late 90's should have been the canary in the coal mine of financial deregulation. It wasn't and we paid a heavy price.

Does this failure imply that there is an alternative to DSGE models? Of course not. Central banks work on a decision cycle of a few months. Quantitative models are crucial for organizing discussions of the state of the economy and quantifying the likely effects of alternative policy actions. For these purposes, there are simply no coherent alternatives to estimated DSGE models.

Exactly which features should we embed in future DSGE models? Future clever theorists will come up with clever theories highlighting all sorts of possibilities. Some of these possibilities may even contain a grain of truth. But we need some way of assessing their quantitative importance for aggregate economic activity. The *only* way to do this is to incorporate the proposed frictions into DSGE models.

The trick will be to estimate and evaluate evolving DSGE models using cross-country evidence and not rely solely on time series methods using data over short periods for similar countries. How to do this in a systematic way poses an interesting challenge. I suspect that Bayesian approaches will be particularly useful in this regard.

From a broader perspective, macroeconomists face an even larger challenge. Researchers and policymakers obviously have long lasting, persistent disagreements about fundamental issues. Thirty years later and we're *still* debating the relative merits of KW and DD. This disagreement is not just a matter of academic interest. When the policymakers of the ECB convene to discuss the ongoing sovereign debt crises,

differences of opinion will revolve around different answers to the following types of questions:

Are Ireland, Greece, Portugal and Spain's debt problems primarily a contagion phenomena or are we just seeing fundamentals work their way out?

How important is the moral hazard problem of 'bailing out' a fellow member state?

What's the analog of 'regulation' in a monetary union of sovereign states?

Clearly differences of opinion matter and are persistent. While it is easy to generate disagreement on fundamental matters in a conference of economists or policymakers, it is remarkably hard to generate persistent disagreements between the agents in our models. I don't think we'll have convincing theories of asset prices and financial markets until we have convincing models of disagreement.

Nobody disagrees about the odds of drawing four aces from a deck of cards. But there is substantial scope for belief heterogeneity when historical evidence is at best a weak guide. For many questions this situation is the one we face. The data just aren't there to provide compelling evidence about the effects of low-frequency changes in fundamentals like regulations, the incentive effects of a monetary union on member country deficits, or the effects of changes in productivity growth rates on asset prices. Under these circumstances, heterogeneity in beliefs can evolve over prolonged periods of time with important consequences for market outcomes. See for example Burnside, Eichenbaum and Rebelo (2011) who analyze booms and busts in housing prices in models where agents have persistent disagreements about housing fundamentals.

Confronting the existence of persistence disagreements can push you outside the formal rational expectations framework but in ways where the richness or weakness of historical data remain informative. This research program is in its infancy but highly promising. See Hansen (2007) and Sargent (2008) for a review of the literature.

Let me conclude with the following observation for policymakers. Many of you were truly shocked to discover that staff models didn't account for some key features of the crisis. And you were certainly frustrated that the science of macro hasn't quite caught up with the art of macro. Before venting your frustration on the staff, remember the sage advice of the noted American decision theorist Don Rumsfeld: the art of policy making involves constantly being on alert for the unknown unknowns that we don't know we don't know.

In a crisis, the staff can help. State of the art models can help. Even 75-year-old books providing general theories of employment, interest and money can help. But a wise policymaker in the throes of a crisis will never focus on only one feather in his quill. And he will never be surprised that he will be surprised.

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