

The Financial Crisis of 2007: Our Waterloo or Take a Chance on IPE?

DAVID LEBLANG and SONAL PANDYA

Department of Politics, University of Virginia, Charlottesville, Virginia, USA

Questions about the 2007 financial crisis abound. There are debates about its origins: do they lie in the lax lending policies of banks, the negligence of credit rating agencies, or inadequate government oversight? There is scrutiny of government responses: should Lehman Brothers have been allowed to fail? Can fiscal stimulus restore economic growth? And there is disagreement about the consequences of the crisis: why have some economies recovered faster? Has the crisis recast partisan divisions? These are big and important questions; the search for answers will occupy scholars and policymakers alike for some time to come.

In some respects the field of International Political Economy (IPE) is well-equipped to engage these questions. By the eve of the crisis IPE had become a normal science. It organized itself into well-defined research areas examining the sources of voters' policy preferences; the influence of political institutions and processes on policy outcomes; and varieties of international economic cooperation. These core areas structure scholarly inquiry by anchoring the field to a common framework. For a given type of policy, we can readily identify the frontiers of knowledge in each core area and possibilities for advancing these frontiers. Not surprisingly innovation has been steady and incremental, just we would expect from a normal science. Many scholars, though working in this framework, have opened up IPE by introducing the insights and methods of other disciplines. To the many debates about the economic crisis IPE brings a deep understanding of the sources of economic policy and market responses to political events. IPE is most likely to contribute answers to questions that fit into this extant framework.

This pattern of innovation, however, makes it difficult to access many of the central features of this crisis. We, of course, are not alone in this regard; both scholars and policymakers are only just beginning to take stock of their theories in the wake of this very "unexpected" crisis. The breadth and complexity of the crisis complicated efforts to define precise puzzles that fit neatly into existing frameworks.

This crisis is potentially a transformative moment, a call to rethink old assumptions and be open to bold and innovative ideas. We answer this call by identifying two broad themes that underlie the many questions that this crisis raises: the relative speed of economic and political events; and the tight connections between different types of policies within and across countries.

These themes are, of course, longstanding features of international politics and economics. The current crisis invites us to revisit them to find new insights into the framing of substantive questions, data sources, and methods.

The first theme we label “politics in real time.” Consider a couple of motivating examples:

- On September 29, 2008 the U.S. House of Representatives voted to reject the creation of the Troubled Asset Relief Program. During the preceding five-hour floor debate, the Dow Jones Industrial Average fell 777 points and the stock market as a whole lost approximately \$1.2 trillion in value; losses due to the anticipated failure of the bill. Within days the House and Senate voted to establish a modified version of the program.
- On September 30, 2008 the Irish government pledged to insure all bank deposits regardless of the amount. Within hours the United Kingdom, faced with massive capital outflows, followed in suit. Within days, other European Union members and the United States also raised the ceiling for government insurance of deposits.

These examples illustrate the speed with which economic events and policy choices influence each other. For evidence of markets’ velocity one need only turn on CNBC (if you have cable) or access a Bloomberg terminal (if you have one handy) to track the real-time effects of political events on exchange rates, bond prices, or stock indices. We lack similarly rich data on political responses to economic events. Although major news organizations frequently conduct public opinion polls, these polls lack the stature of well-defined market indicators. Prediction markets which track speculators’ views of the success or failure of policies are novel but similarly lack stature.

What can real-time political information contribute to IPE research? Just as standard financial market models assume rational expectations on the part of market participants, models of the political process often implicitly assume that political actors behave similarly. In practice, there are a variety of political actors—voters, lobbyists, politicians, and bureaucrats—each inhabiting different institutional environments. Some of these environments are rich in information (e.g., legislatures) while others are not (e.g., budgetary agencies). Such variation precludes summary metrics analogous to market indices. Rather, models of political expectations need to be tailored to the type and pattern of information that different political institutions generate. In order to explain the divergence between legislative outcomes and voters’ *ex ante* expectations of those outcomes, we need different models than those that explain legislators’ predictions of budget announcements.

The second broad theme from the financial crisis concerns the fundamental role of simultaneity. To fix ideas consider the canonical

Mundell-Fleming conditions. These conditions state that policymakers can achieve only two of three policy desiderata: an independent monetary policy, a fixed exchange rate, and capital mobility. Current scholarship rarely takes these kinds of simultaneity seriously. Scores of papers have been written—some even published—that use a single equation to analyze the determinants of a specific type of policy. When these papers do recognize that each individual policy choice is a part of a broader array of international economic policies they do so, in general, by including one or both of the other policy choices as “independent” or “predetermined” variables. We think that these connections have been inadvertently overlooked as a consequence of the discipline’s collective desire to be parsimonious.

Just as the current crisis shows the need to model the continual interaction of politics and markets, it also demonstrates that there are tight connections between policy areas. Consider the Obama administration’s fiscal stimulus plan. It provides for massive government spending to stimulate demand and ultimately, employment. At the same time financial regulators raised the minimum capital requirements of depository institutions. Can stimulus work when one policy injects capital into the economy while another effectively constrains lending of that capital? It will be difficult to know if these two policies are analyzed in isolation.

Similarly, political and economic factors link countries together. A crisis that began in the U.S. spread rapidly across the Atlantic and infected financial institutions that were heavily exposed to derivative securities and U.S. subprime mortgages. Very rapidly even countries with “healthy” financial institutions felt the shockwaves as global commodity and financial markets seized up. Closer examination reveals considerable variance in countries’ exposure to the crisis: the International Monetary Fund forecasts that in 2009 India’s economy will grow by 5.1 percent and China’s by 6.7 percent. This is in contrast to the United States (–1.6 percent), the United Kingdom (–2.8 percent) and Germany (–2.5 percent). Why is it so? Is it because China and India have relatively protected financial market? Is it because their business cycles are “decoupled” from the advanced industrial economies? Or is there something else related to their domestic regulatory environments, the actors that choose to participate in their financial markets, or something completely unrelated? To understand this we need a model that allows us to separate out the causal connections between states and domestic and global markets. And testing these theories will demand careful attention to methods. In the extant literature we commonly pool data, impose a homogeneity constraint on the parameters rather than testing whether it is valid. We take dynamic data and smooth out the residuals and ignore that our “independent” variables are not necessarily independent. A better research design has theoretical priors about why data should be pooled, residuals should be heteroscedastic, and why right-hand side variables should not be treated as independent.

These themes resonate with many substantive questions but our larger question, of course, is how to use these observations about the crisis to advance IPE as a field. To advance theoretically, empirically and methodologically, we suggest a renewed focus on the micro level implications of macro political and economic outcomes. We currently have assumptions about the way that behavior translates into macro outcomes but reality often reveals that models are wrong. Recall that at the beginning of 2008 President Bush offered cash transfers of up to \$600 per taxpayer in an effort to stimulate the economy. The plan did not have the intended effect because recipients did not spend the transfers. Instead they deposited their checks into savings accounts or used the money to pay down debt. This is an example of a policy response that did not work because of insufficient attention of the necessary behavioral prerequisites.

A growing body of IPE research examines the consistency of individual policy preferences with the distributional implications of those policies. These studies examine attitudes toward economic inflows, including trade inflows and immigration. This research provides a novel way of testing foundational theories in IPE regarding the distributional effects of international economic flows. Stated preferences, however, cannot necessarily explain a myriad of actions that we typically assume follow from those preferences. We propose extending these theories to incorporate stylized facts about behavior.

Both sociology and behavioral economics demonstrate that individuals systematically deviate from the strict assumptions of classic rationality. These include bounded rationality and the many dynamics associated with imperfect information; loss aversion as embodied in prospect theory; mental accounting that describes how the framing of choices influences conclusions; and hyperbolic discounting and its influence on time horizons. These aspects of behavior can be combined with extant theories of preferences to open up new classes of observable implications to empirical tests. There is a gap between theories of stated preferences and both theories of political actions, like voting and campaign contributions; and politically-relevant outcomes that are mediated by both preferences and behavior. Currently we assume that actions follow from stated preferences. This assumption runs counter to these more complex accounts of human behavior. By incorporating these behavioral concepts we can bridge sophisticated theories of preferences with a range of actions that we assume follow.

Aided by theories of preferences grounded in behavior, we can examine how consistent stated preferences are with observable economic decisions. We can draw on a wealth of market research data to examine whether trade policy preferences are consistent with consumption patterns; do individuals with a stated preference for trade protection typically consume more domestically-produced goods? Similarly we can study portfolio allocation—do people hold policy preferences that maximize returns to their assets? To

be sure, this kind of work faces theoretical and empirical challenges. On the theoretical side there are serious questions of endogeneity. Empirical challenges include acquisition of information that links stated preferences to observable behaviors like consumption or portfolio allocation. We think tackling these challenges is worth the effort. If successful, we can open up new dimensions of research that present new testable implications of our theories.

How, specifically, does this innovation capitalize on the lessons of the financial crisis? A micro-level focus generates new classes of observable implications. Tests of these implications introduce into IPE real-time data about political beliefs. We can take models of government formation and dissolution, models of policymaker's preferences, and models of strategic legislative policy to better inform our empirical work. One could, for example, envision public opinion surveys that ask questions that probe not only the public's level of economic and political knowledge but also their preferences regarding various policy responses to the crisis. This is not so much a stretch from what is currently done by the mass media—polls constantly ask questions about desired policy responses—rather it is a claim that these sorts of questions need to be included in regular panels that also include a battery of political, economic, and demographic information. It is interesting, for example, to look at how preferences for a fiscal stimulus—and the type of stimulus—vary not just by levels of income and education, but also by partisanship, sector of employment, and financial portfolio. And having this information available in a panel would help us develop more dynamic models of the political economy. Further, with advances in experimental designs one could well envision such surveys being deployed cross-nationally to take advantage of differences in institutional environments.

A focus on the micro level also prompts us to reexamine the role of political incentives facing elected officials. This has been done at least in part. We know that politicians behave differently depending on where they are on the electoral clock and we also believe that these electoral incentives are conditioned not only by electoral institutions but also by partisan ideology. This, however, does not help us in understanding why both George Bush (a conservative) and Gordon Brown (a liberal) enacted similar bailout legislation. What it does tell us, however, is that partisan labels may be overshadowed by political expediency—a condition that we need better theories to understand.

Recognizing that incentives transcend these broad labels expands the applicability of our theoretical models. One view of the present crisis is that it began as a banking crisis where bankers, relatively freed of government regulation, engaged in very risky lending practices in pursuit of high returns. And there was little downside because the government stood by ready and willing to bail out the banking sector in the event of possible failure. While this description is written as an overly simplistic description of the 2007 crisis that began in the United States and spread to Western Europe, a similar description

could be written about the 1997–1989 financial crisis that began in South East Asia. Are these two crises really different in origin? Only time will tell but to truly get at the answer one must begin by looking in the right place.

This example also illustrates our broader concern with a focus on the micro-level. Here we do not mean that we should just examine individual level behavior; we mean this more generally, including the importance of specificity in our measures. A scholar may never think that the 1997 and 2007 crises are similar because the countries of South East Asia are characterized by a set of institutions that are less “transparent” and more “corrupt” than those of the United States or Western Europe. But is that really the case? It may be in the aggregate but if we are trying to measure the degree of government protection afforded the financial sector then we need to look elsewhere. And this point can be made more generally: if we believe that theory should be derived from theory then we need to be more careful about measuring what we need to measure rather than measuring what we can measure.

The 2007 economic crisis is an opportunity for IPE as a field to reflect on its collective goals and priorities. IPE scholars have to be patient observers of the world and ask substantively relevant questions, no matter how difficult they may be to answer. This orientation raises the bar of imagination and creativity as we collect data on new phenomena and ask original questions. We are, however, fully aware that political scientists, like the actors we study, respond to incentives. Inspired by the crisis we map some new ways of approaching substantive puzzles that hold the promise of opening up new dimensions to our fields. We think they have a lot of potential but only rigorous research can confirm our hunch.

In moving forward we as a discipline have to be aware of professional tradeoffs we face. The central tradeoff is between clean and elegant formulations whose marginal value is less, and messier interventions on really important topics. We recognize that it is simply easier to do research on well-established questions, to extend knowledge by tweaking existing work. To be sure, there is a lot to be said for incremental changes. For one thing they are probably more sustainable in the long run. We must remain vigilant that complacency and rigidity never stand in the way of true progress.