Critical Dialogue

Banks on the Brink: Global Capital, Securities Markets, and the Political Roots of Financial Crises. By Mark Copelovitch and David A. Singer. Cambridge: Cambridge University Press, 2020. 232p. $39.99 cloth.
doi:10.1017/S1537592722002006

— Timothy J. Sinclair, *University of Warwick*
timothy.sinclair@warwick.ac.uk

Banks on the Brink is a systematic investigation into why some countries have proven more prone to bank failures and financial instability than others. Mark Copelovitch and David A. Singer focus on how international economic forces combine with the political decisions that shape the structure of financial markets. They examine capital inflows and the relative prominence of securities markets versus banks to explain how and why banks behave badly, taking on excessive risks. The authors find that it is the combination of capital inflows and well-developed securities markets that creates instability. They use both quantitative and qualitative evidence to substantiate their claims.

The central argument of this book is that capital inflows, perhaps attracted by rising interest rates, set the scene for financial disaster when associated with sophisticated, highly innovative securities markets. The capital inflow provides the resources, and the securities markets provide the venue in which those resources are deployed. The authors find that places with decentralized and less developed securities markets, like Canada, are less vulnerable to banking crises because the inflow of funds can be lent out via traditional loans to the domestic economy. Strong inflows and well-developed competitive securities markets together are a recipe over time for crisis. Banks engage in riskier behavior when they compete with sophisticated securities markets.

Although you might assume in societies with sophisticated securities markets that capital inflows would create a credit boom—a surge in the volume of bank lending—the authors' findings do not support this claim. What they find instead is that credit quality deteriorates as banks lend to riskier clients, weakening the strength of their lending book. Borrowers with better credit, we assume, are getting their money from the securities markets. This finding led me to wonder whether focusing the analysis in this book on capital inflows was perhaps not particularly useful. It might have made more sense to have focused directly on the issue of credit quality. Rather than metrics of inflows, it might have been more telling had Copelovitch and Singer assembled a metric of financial mania instead. If the real issue is deteriorating credit quality, for which there might be a variety of causes in addition to inflows of capital, then it makes sense to go straight to the more proximate indicators of looming financial crisis.

The authors ask why banking crises occur and answer that they happen when banks' customers lose confidence in them. Behind this is the challenge that all banks face: maturity transformation. Banks take deposits from their customers that can be withdrawn on demand. These are liabilities on a bank balance sheet. The assets—the loans made by banks—are made over a much longer time frame, with periodic repayment by installment. This makes banks vulnerable to a crisis of confidence, or a bank run, in which depositors collectively decide that their deposits are at risk and seek to withdraw them all at once. Traditional bank architecture makes the institutions look like classical temples to give banks at least the appearance of solid and venerable organizations, despite these inherent weaknesses. The authors describe how the collapse of Lehman Brothers led to the growth of uncertainty among counterparties that held Lehman's debt, which then made banks wary of providing support to each other, escalating the problem.

What is unclear to me is why Copelovitch and Singer do not take the next step in the analysis and examine the shift away from relatively expensive bank loans to cheaper bond issuance. The costs of maturity transformation and the alternative of securities financing mean that since the 1980s there has been a process of disintermediation of wholesale financing, starting in the United States and moving on to Europe and Asia. Securities financing is now starting to invade the development process. For banks to compete with lower-cost securities financing, they must either lower the costs of their loans or take on riskier borrowers that cannot access the financial markets. In the process these banks, at the wholesale level at least, cease to be banks in the sense we have known them because of this hunt to bolster their returns. Their traditional business has been so disrupted that they become much more amenable to high-risk strategies that the old-fashioned bankers of the 3–6–3 model (pay deposit interest of 3%, lend at 6%, and be on the golf course by 3 pm) would never have contemplated. But this transformative story about banks

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https://doi.org/10.1017/S1537592722002006 Published online by Cambridge University Press

worldwide is missing from the authors' account of the causes of financial crises. Also missing are the other financial disruptors, especially social media companies, that are encroaching on the banks' payments monopoly.

One of the fascinating issues considered in this book is the break in Germany between a world of persistent financial stability before 2008 and a new era of instability associated with Finanzplatz Deutschland. Copelovitch and Singer describe how Frankfurt became the focus of an initiative to compete with London and New York as financial centers. The effect, of course, as they describe it, was to transform Germany into an Anglo-American style financial system, in which securities and financial innovation have become much more important than they were traditionally. This has, Copelovitch and Singer show, changed the behavior of German banks, making them much more like those in the United States and United Kingdom and much less like those in Canada. It is a great story. But I think it could have been told better. This part of the book is precisely where fieldwork, especially elite interviews, might have drawn more sharply the motivations driving the change and the consequences for the new German financial industry.

The book's final chapter considers the merits of a range of policy responses to financial crises. They issue a caution that, although global imbalances are a cause for concern, there are many more cases of capital inflows that do not result in banking crises than those that do. They go on to examine increased capital requirements, the imposition of capital controls, breaking up banks that are too big to fail, and the merits of reintroducing the Glass-Steagall separation of commercial and investment banking ended by the Gramm-Leach-Bliley Act of 1999, offering a series of thoughtful observations about the issues with these possible measures.

This last chapter is the most cautious one of the book. This makes sense because as political scientists the authors are accustomed to drawing conclusions from facts and from reasonable probabilities drawn from those facts. But it is also disappointing. I think in this final chapter Copelovitch and Singer might have allowed themselves greater freedom to think beyond the bounds of the existing policy debate, which seems not to have taken us very far from financial regulation as it existed before the global financial crisis, despite the Dodd-Frank Act of 2010. Much of the modest change since then has been focused on tightening regulative rules and increasing bank capital, even though the widely praised bank capital provisions in Spain did not save that country from catastrophic financial crisis after 2007. But surely the global financial crisis showed that financial innovation in securities markets can quickly outpace regulative rules.

In this context, perhaps we need deeper policy-making that addresses the conditions that allow for innovation to be undertaken in a safe way. The obvious comparison is with the chemical and pharmaceutical industries or with the training and regulation of medical professionals. Why do we tolerate less safety when it comes to finance, given the devastation that we know can follow a crisis involving these markets? The failure to regulate seriously following a crisis has involved an unwillingness to recognize the responsibilities that should fall on the shoulders of those who participate in these markets. When you consider the extraordinary incomes that some in banking and finance receive, this does not seem too much to ask. Despite talk of deglobalization, few countries have the choice to embrace a less efficient securities market like that in Canada, and all banks face pressure from disintermediation and new financial ventures inside the social media industry eager to disrupt banking.

Banks on the Brink is a well-developed study that makes a substantial contribution to the political economy of money and finance. The quantitative work and the historical case studies are thoughtful, clear, and insightful. The book underlines in the most compelling way how banks, in specific circumstances, can engage in disastrous behaviors and why they are compelled to do so. Although the authors might have done more to unpack the social mechanisms that make this so, they chose to frame this study in a more structural way. Subsequent work might usefully focus on precisely those social mechanisms.

Response to Timothy J. Sinclair's Review of *Banks on the Brink: Global Capital, Securities Markets, and the Political Roots of Financial Crises*

doi:10.1017/S1537592722001992

— Mark Copelovitch

Timothy Sinclair raises several interesting points in his review. First, he criticizes us for not focusing on "more proximate indicators of looming financial crisis" (such as credit quality) than our key explanatory variables: capital inflows and financial market structure. To be sure, more "proximate" factors help explain some crises, as numerous books already highlight. Yet these variables dominate the existing literature and frequently miss the forest for the trees. We show that market structure and capital inflows interact to form a dangerous cocktail that has contributed to banking crises in industrialized countries since the 1970s. Furthermore, as we show in our historical case studies of Canada and Germany, today's financial market structure is the result of deeply contentious political battles over the very long run. "Proximate" factors also matter, but past work has overlooked the nonproximate deeper causes of financial instability.

Sinclair asks why, in focusing on the size of securities markets relative to traditional commercial banking, we "do not take the next step…and examine the shift away from relatively expensive bank loans to cheaper bond issuance." Our primary metric in the statistical analysis is the ratio of stock market size to private credit from the banking sector,

https://doi.org/10.1017/S1537592722002006 Published online by Cambridge University Press

but this is but one of many proxies capturing a more general trend: the rise in the relative size of securities markets. Bond issuance is part of this trend, and bond market size is highly correlated with stock market size. Indeed, using the size of bond markets in our models yields substantively identical results. Ultimately, the relative size of traditional banks/banking to nonbank financial activity/intermediaries is what matters, not the precise form of nonbank activity itself.

Sinclair also suggests that our focus on capital inflows was not useful. I confess that I find this baffling. The exponential growth in cross-border capital flows is the single most important development in global finance in the last 50 years. The literature is clear that capital inflows are a major correlate of banking crises. Yet they only trigger crises in some cases and not in others. Explaining this puzzle and the effects of these massive changes in global finance is vital. Other variables surely matter, but our findings show clearly that deeper structural factors also do, and they evolve through complex, long-run political processes.

Sinclair suggests our German case study would have been better told with more fieldwork and interviews. Of course—yet there are trade-offs in everything. Our puzzle was "Why do large capital inflows cause financial crises in some cases and not in others?" Our answer involved testing the impact of capital inflows cross-nationally and exploring the politics of long-run historical transformation in financial markets in two countries over 150 years. A more detailed contemporary German case study would indeed be interesting. It was beyond the scope of our book.

Finally, Sinclair finds our policy implications disappointing. Because we provide a wealth of evidence demonstrating that large capital inflows and declining bank capital are the central determinants of banking crises, it is not surprising that our recommendation is to require banks to hold significantly more capital. We also clearly discuss why regulating bank activities, size, and other dimensions is likely to be far less effective in ensuring financial stability. Here, as with earlier parts of the book, we clearly disagree with Sinclair about which factors are the most important determinants of crises and why.

I thank Sinclair for his review. I wish it had engaged more directly with the theory and empirical evidence of the book we wrote, rather than the very different one he wished we had written.

To the Brink of Destruction: America's Rating Agencies and Financial Crisis. By Timothy J. Sinclair. Ithaca: Cornell University Press, 2021. 228p. $39.95 cloth.
doi:10.1017/S1537592722002158

— Mark Copelovitch, *University of Wisconsin-Madison*
copelovitch@wisc.edu

This is an excellent book that all scholars of financial crises and political economy should read. Timothy Sinclair has written a lucid, detailed, and thoroughly researched investigation of the role of U.S. ratings agencies in the global financial crisis of 2007 and beyond.

The core puzzle of the book is why the major ratings agencies remain, essentially unreformed, at the heart of Wall Street and the global financial system: "Understanding what the agencies really did, and why, and how they survived the ignominy of their involvement in the crisis is the purpose of this book" (p. 2). As Sinclair notes, the ratings agencies played a central role in the global financial crisis, yet they emerged without substantial new regulation, major changes to their business model, or losing their central role in global finance.

Why is this the case? Sinclair offers three explanations: (1) the agencies' uncertainty about the sustainability of their business model after the 2001 Enron crisis; (2) the rise of Fitch, transforming the comfortable Moody's–Standard & Poor's ratings duopoly into a more competitive three-way oligopoly; and (3) the need for some agency to offer ratings and the lack of other institutions that satisfy market participants as superior alternatives. Sinclair also offers a more tentative argument about the continued dominance of what he labels the "exogenous" view of financial crises—the belief that crises are a bug, not a feature, of markets— in popular media and political discourse, which I discuss further.

As a descriptive project, this book is truly outstanding. It is clear, concise, very well written, and rich with qualitative evidence from Sinclair's meticulous research and interviews. Chapter 4—which explains what structured finance, securitization, and repurchasing agreements ("repo") are, the ratings agencies' role in them before and during the global financial crisis, and how these came together to trigger the collapse of Lehman Brothers and the broader crisis in 2008—is perhaps the clearest explanation I have read anywhere on these issues. Likewise, Sinclair's in-depth chronology and analysis—including detailed recaps of congressional hearings on the ratings agencies during the crisis—is superlative. Chapter 6 also includes excellent discussions of the role of the ratings agencies in the Eurozone crisis and why calls for greater financial transparency are red herrings in the search for increased financial stability. These are sections of the book that will serve as outstanding readings for many years on both undergraduate and graduate syllabi in international political economy (IPE) courses.

That said, I found the book's theoretical argument less compelling. Sinclair spends a good deal of time knocking down what he frames as the stock explanation of the crisis. In this view, "the ratings agencies were the key players in the germination of the financial crisis," "providing inflated ratings for the bonds associated with subprime lending" (p. 6). The key here, Sinclair argues, is that this view treats financial stability as the norm, and financial crises as the exception, brought on by greed and bad behavior: "crises

https://doi.org/10.1017/S1537592722002006 Published online by Cambridge University Press

can only occur because people do bad or illegal things, or because there is some defect or 'failure' in institutions, perhaps caused by government. Crises are therefore exogenous to markets, reflecting problems external to them" (p. 8). In contrast, Sinclair proposes an alternative view, in which "crisis is a normal, if not daily, event in financial markets" (p. 8) and one that is "endogenous" to the structure of markets themselves.

This is true, as far as it goes. The "bad actors" argument surely is the popular conventional wisdom. But I do not think it is the dominant explanation among IPE scholars. Indeed, IPE scholars and economists of many stripes have recognized for decades that market failures exist and are an inherent part of finance. The causal story here—a search for yield driving flows into structured finance in the United States, creating a bubble in subprime lending and collateralized debt obligations (CDOs) that eventually burst, bringing down Lehman Brothers and triggering a global financial freeze—is one now widely recognized as the key narrative of the global financial crisis.

Ultimately, Sinclair's critique of "market-centered approaches" boils down to "economists and financial market scholars don't pay enough attention to politics." I fully agree. But that tells us little about the political economy of the ratings agencies. Certainly, "populist views" on ratings agencies are simplistic in understanding the distributional consequences and politics of financial crises. But this is something of a straw man. IPE scholars of all bents surely agree that these approaches have major limitations and ignore politics at their peril. I wished more time had been spent instead engaging with the work of materialist political economy scholars on crises and policy responses to them, which highlights the importance of domestic interests and institutions and their material distributional consequences.

That said, I share Sinclair's critique of critical theorists' approach to ratings agencies: "not taking the agencies and what they do seriously enough. If they are just another institution of capitalism…why bother studying the agencies themselves" (p. 72). Indeed, just as we cannot understand the political economy of the global financial crisis by looking for bad individual actors alone, we also cannot really understand it simply as a structural feature of a failed capitalist system, as critical theorists argue. Focusing on key actors, institutions, and strategic interaction between them is crucial for explaining how and why crises occur and the policy responses to them.

I also agree with Sinclair's view, articulated in his "social foundations" approach, that "purely materialist explanations for the existence of the ratings agencies are deceptive" (p. 73). And I found his discussion of how and why social and ideational variables matter compelling. However, I think few serious IPE scholars would disagree with Sinclair's claims that both ideas, beliefs, and social facts (on the one hand) and material factors (on the other) "matter" in explaining the ratings agencies' role and importance in the events of the global financial crisis.

Instead, the main point of disagreement among IPE scholars, surely, is about the *relative* importance of social and material factors. Here, I found the book less convincing. For example, Sinclair returns repeatedly to the importance of there being three main ratings agencies, rather than two or many, and he identifies market competition and material incentives among Fitch, S&P, and Moody's as key factors shaping their behavior after 2001. Yet the empirical portions of the book do not tell us exactly *how* important this competition was, relative to the ideas, beliefs, and perceptions that Sinclair argues are central explanatory variables.

Elsewhere, Sinclair argues that there is a "two-pronged explanation" for why the ratings agencies have persisted in their role: (1) the lack of better institutions to solve the information problems of disintermediated capital markets and (2) the continuing dominance of the Big Three. I am again quite convinced he is right on both accounts. But there is no real test of the relative importance of these two factors, nor is there a test of the degree to which the Big Three's continued dominance is due primarily to market actors' *beliefs* that some judgments carry more weight than others or whether it is due mainly to *material* political economy factors about the competitive environment.

Thus, when Sinclair concludes that we must "reject the dominant market-centered understanding of ratings and embrace the social foundations understanding advocated in this book," I am not fully convinced (p. 164). I do not believe that his book has settled the debate about the relative importance of ideational versus material factors in driving the political economy of ratings agencies. Unquestionably, both "matter." But the extent to which each matters—both in the case of the ratings agencies and more broadly in the political economy of financial crises—requires further research.

Finally, in chapter 7, Sinclair argues against what he labels the "exogenous" view of financial crises, which he claims sees crises as the result of individual villains ("the bad guy in the black hat") and institutions "outside of finance" (pp. 166–67). In this exogenous view, he notes, "Financial crisis is a deviation from the normal state of the market" (p. 171). Sinclair contrasts this with "the endogenous account," in which "financial crises begin with finance itself" (p. 172). Here, I agree with Sinclair that finance itself *is* the problem. But I disagree with his conclusion about what structural factors of finance are most important in causing financial instability. For Sinclair, the problem is "the important role of extreme forms of financial innovation," as epitomized in both the Enron episode and the structured financial instruments of the 2007 financial crisis (p. 176). But this overlooks the most important structural feature of "finance" that has been the problem in the global economy for decades:

https://doi.org/10.1017/S1537592722002006 Published online by Cambridge University Press

capital mobility. Since the collapse of Bretton Woods, there has been unprecedented financial liberalization and a more than 30-fold increase in the magnitude of cross-border capital flows. The rise of global finance has brought enormous benefits in terms of growth, development, and rising living standards in many parts of the world. And yet, just as the pre-World War I and interwar eras were characterized by both capital mobility and frequent financial crises, so, too, the return of capital mobility has been characterized by waves of banking, currency, and sovereign debt crises.

Few of these crises, however, have been driven mainly by financial innovation. Instead, from Latin America in the 1980s to East Asia in the 1990s to Europe in the 2010s, most of the post-Bretton Woods financial crises have been quite straightforwardly banking, sovereign debt, and bond financing crises. The common threads across these crises have been the magnitude and reversal of large cross-border private capital flows, not complex financial innovation.

In sum, although I agree with Sinclair that the "exogenous" approach to financial crises remains dominant in popular discourse, I disagree with his conclusion that the relevant "endogenous" factor is financial innovation. Rather, I see the resurgence in global capital flows since Bretton Woods as a far more important explanatory variable. Nonetheless, it is quite clear in the 2007 global crisis, and perhaps in others as well, that financial innovation and complexity *do* matter. Future research should seek to disentangle the relative importance of these two factors, as well as the relative importance of the social and material factors that Sinclair identifies as the key components of his social foundations approach. Scholars engaging in that work will find Sinclair's book essential reading, and his outstanding account of the ratings agencies' role in the 2007 crisis will remain important and relevant for years to come. This is an excellent book. I recommend it highly.

Response to Mark S. Copelovitch's Review of *To the Brink of Destruction: America's Rating Agencies and Financial Crisis*

doi:10.1017/S153759272200216X

— Timothy J. Singer

One of the things that motivated me to write this book was the continuing widespread misunderstanding of how the issues that beset the financial markets in New York, London, and elsewhere in 2007 blossomed into the global financial crisis soon after. So, it is especially pleasing to have generated an explanation of these events that Professor Copelovitch finds compelling. I have two major responses to some other points he makes.

Professor Copelovitch argues that few crises have been driven by financial innovation as I suggest. He then provides some examples to support his view that capital mobility is the real source of financial instability. In fact, each case he mentioned was linked to financial innovation: Latin America in the 1980s (inflation-indexed sovereign borrowing), East Asia in the 1990s (borrowing in US dollars and lending in local currency), and Europe in the 2010s (government bailouts of banks that had traded in securities associated with structured finance). Indeed, Copelovitch and Singer show in their book that it is the sophistication of securities markets that turns capital flows into crises. Without that sophistication there is no crisis. They note, "There are far more cases of large capital inflows, asset bubbles and macroeconomic imbalances that do not result in banking crises than cases that do" (p. 185).

The other issue is Copelovitch's contrasting the views of those he calls materialist political economy scholars, who place emphasis on domestic interests, institutions, and the "material distributional consequences of them," with my approach. My problem is that I do not see separate ideational and material worlds, at least not when it comes to human understanding and action. As Mark Blyth wrote in this journal 20 years ago, "Structures do not come with an instruction sheet." The material does not—cannot—assert itself without social mediation by us. This shapes how people understand what materialist scholars call incentives. Because of this, I do not see a need to *add* the material to my inquiry. It is already there. In this book, I provided, for example, evidence of how agents interpreted the competitive circumstances and market share dynamics in the agencies, especially in chapter 5. This is a "materialist" analysis to my mind, and the best sort too, because it does not involve me as a scholar imposing my understanding of what is important. The actors are fully capable of doing that themselves. That is why I do not think a test that distinguishes actors' "*beliefs*" about rating agencies from "*material* political economy factors" about inter-rating agency competition is likely to be insightful. There is no material sphere that is somehow apart from our beliefs about that sphere, because all structures must be understood by people. This intervening processing is required to make the material compelling to us, and this dynamic can certainly give rise to variable outcomes.

I want to thank Professor Copelovitch for his many insightful comments and praise for my book, and to note again how thoughtful and well-constructed I found *Banks on the Brink.*

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