



FORUM

Response to my critics

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Abstract

Central banking studies continues to consolidate around common foundations, but points of tension and disagreement persist. In this reply, I discuss three such points raised by contributors to this forum. These relate to the concept of infrastructural power, the significance of financial stability policy, and questions of historiography. I also offer some reflections on future directions for the study of central banks.

Keywords: central banking; financialization; global financial governance

Think of industrial relations research. The scholars in this interdisciplinary field (political economists, sociologists, and heterodox industrial economists) tacitly agree about the importance of their work for understanding capitalism; they agree on the structural forces and institutions to be studied and conceptualized; and they thereby differentiate a field of coherent, but also controversial and lively academic debate. Speaking to the rising importance of these organizations in financialized capitalism, we have achieved something similar at a smaller scale for central banks. Social scientists beyond mainstream economics – sociologists, political economists, and legal scholars – have developed a common sense of purpose in their work, a shared stock of technical expertise (e.g., about money creation), historical-empirical knowledge, and a common orientation toward controversial issues and conceptual distinctions that are relevant for central banking. Indeed, as Paul Tucker’s review shows, who formerly served as Deputy Governor at the Bank of England, our debates resonate with problems of practical importance, even though we do not necessarily cast them in the same terms as practitioners do.

So what is the *enjeu* of central banking research? A common thread that runs through the contributions to this forum is that the interlinked developments of finance and central banking since the 1970s have reached an impasse – a situation that demands explanation, critique, and a debate about possible reforms. Financialization has raised the political status and increased the systemic importance of central banks. Yet, while this process at times seemed to increase central banks’ own policy capacities, the real possibilities for steering economies into more sustainable and equitable directions have shrunk. More bailouts, more finance-led macroeconomic policy, and more governance tied up with volatile markets lead central banks and us all into a trap of over-financialization, growing fragility, and macroeconomic weakening. To describe this, Nathan Coombs uses the succinct formula of central banks being ‘dominant but captive’.

Big questions about private versus public power, the limits of macroeconomic steering, of distributional politics, and the role of democracy versus technocracy are at stake. Yet, as Saule Omarova writes, the central banking scholarship of which I speak does not frame big questions in abstract terms. '[T]he research journey [rather] starts with a concrete issue of law or policy [...] academic expertise is . . . rooted in the deep technical understanding of how the financial system operates, how it is governed, and where the existing modalities of operation and governance are misaligned or otherwise problematic'. Omarova uses this description of her work to draw a contrast with my 'wide canvas' book. But I would entirely endorse her description as a methodological blueprint. From Daniela Gabor's critical macrofinance to my own sociological work, we engage with the technical details of policy conduct and operational features of central banking because we believe – and can show – that the big issues are being negotiated and decided, at least to some extent, at such technical-operational levels.

Overall, then, I see this book forum as documenting the considerable progress that we have achieved in central banking studies and the formation of common foundations for such research. Still, there exist different interpretations of historical evidence, disagreements in the weight being given to different sides of conceptual distinctions, and heterogeneous ideas of how to take this research forward. I will address these issues in the following brief sections, starting with the big issue of (infrastructural) power, then turning to the charge that I have neglected financial (in-)stability, then to more particular disagreements with my historical narrative, and finally to implications and normative conclusions to be drawn from my book.

Who really has (infrastructural) power?

Both Nathan Coombs and Greta Krippner agree in their critique that I attribute too much agency and power to central bankers, while neglecting the might of private finance. Krippner relates such critique to my use of Michael Mann's theory: It's not central banks that can use finance as a source of infrastructural power; it's finance that possesses infrastructural power over central banks and the state at large. I believe that such critiques arise from misunderstandings and different uses of concepts rather than posing fundamental challenges to my argument. First, the usefulness of Mann's concept arises from the fact that infrastructural power is always conditional and opens the door for private influence on the state and policy. At the same time, it is hard to deny that during the inflation targeting (IT) period, central banks were able to expand their claims over macroeconomic policy by enlisting expansive financial markets into their governing projects. For instance, the introduction of new modes of coordination with private markets that I describe as one critical component of IT was more than just a decision to follow market expectations and abdicating responsibility to private actors. Indeed, central bankers only managed to leverage such interactions with markets for governing purposes because they introduced new devices to represent and influence the linkages between long-term interest rates and their own policy rates – the yield curve. Only with these devices did shifting yield curves become transparent objects for public and private actors to coordinate around. Moreover, in a period in which central banks could stimulate (or dampen) aggregate demand by inducing expansions or contractions in credit or asset markets, finance served as a vehicle for a central bank-led macroeconomic management, which comes out clearly in Cieslak's and Vissing-Jorgensen's (2018) discussion of the 'Fed put'. Last but not least, central bankers in the 1990s and the early 2000s converged around a vision of global finance as a market-integrated system secured by collateral because they found that such market system bolstered monetary policy capacities. This comes out clearly from the sources of the Committee on the Global Financial System that I as well as

other scholars cite. True, such design choices also served private interests, as central bankers were keenly aware. But I do not think it is useful to mobilize such evidence for arguing that, ‘in the final instance’, the entire development of monetary policy resulted from private sector demands.

The situation changes with 2008, as I argue myself in the final empirical chapter of the book. Financial actors in bloated, highly integrated financial systems can call on central banks for support. The latter respond without having any clear macroeconomic case or consistent governance project that they can tie to such financial demands. The rationale becomes one of security – pre-emptive interventionism to avoid bigger disasters, often vaguely understood (‘systemic risk’) (Özgöde, 2022). Whether it is correct to claim that this implies a growing ‘infrastructural power’ for private financial actors, I have my doubts. In most cases, we here speak about structural power in the traditional, Block-Lindblom-sense. Benjamin Braun (2018) has argued that private finance can have infrastructural power if actors control infrastructures on which central banks depend for policy implementation or transmission. But even for such cases, I would argue that infrastructural power is a misnomer. More properly, we should conceptualize this as structural power (i.e., control) over the sources of infrastructural power used by public actors. Infrastructural power is a capacity to govern complex social and economic processes rather than just a position that gives control. But these are nuances in the use of concepts that should not distract from broad agreement. Just as Coombs recounts at the beginning of his essay, latest during the COVID-19 pandemic, we saw that central banks use macroeconomic rationales only as vague and sometimes delusional frames to cater to private finance’s immediate demands. We have indeed moved from infrastructural power to infrastructural capture.

My neglect of financial stability policy

Coombs, Tucker, and Omarova all suggest that I underemphasize central banks’ financial stability and regulatory roles. Capturing the gist of this critique, Omarova writes that ‘a lot of that reinvention [of the central banking-finance nexus] happened through the multitude of central banks’ actions outside their monetary policy ambit, which facilitated and encouraged the gradual restructuring of the financial system’. The reviewers provide different, even somewhat conflicting perspectives on how such regulatory and supervisory activities evolved. For Tucker, I underappreciate the Bank of England’s and other central banks’ active attempts to reign in the post-1970s financial system. Omarova, on the hand, suggests that the Fed consecrated the extension of public subsidies from commercial to shadow banks. This happened not only through the central bank’s advocacy for various deregulations but also ‘through... seemingly technical interpretations of specific statutory provisions’. Coombs more bluntly claims that, in my book, ‘not even the iconic failures of Franklin National, Herstatt Bank, Continental Illinois, Long-Term Capital Management or Lehman Brothers receive a mention’.

It is true that my book primarily makes an argument about central bank power being constructed through monetary policy development, and that monetary policy is the main terrain on which central banks have forged new relationships with market-based finance. I would defend that claim, if only because I do think that even for regulatory central banks (like the Fed), monetary policy is the primary source of legitimacy. But supervision and regulation are important. I admit that my book does not give them sufficient attention. But I do make a contribution to the existing literature on these issues. Before the 1970s, monetary and financial stability policies (*avant la lettre*) were not much differentiated. What we then see with the establishment of a distinct, relatively enclosed jurisdiction of regulation is that practices with explicit or latent regulatory purposes exercised by central bankers get lost in translation. Neither monetary expertise nor the monetary power of

central banks are integrated into a strong regulatory practice, not even within central bank organizations that maintain regulatory responsibility. Contra Coombs' critique, I discuss how this produces paradoxes during and in the aftermaths of financial crises, when the differentiation of regulation with monetary policy breaks down because central banks in these situations act as lenders and market makers of last resort (though it is true that the respective crises that I do discuss are not included in the index). All of this, of course, is not to deny that more work needs to be done on regulation and supervision, and I hope that the gaps that my book leaves serve as another reason to engage in such work.

Specific critiques of my empirical account

Paul Tucker, with his extensive first-hand knowledge of key developments at the Bank of England, offers several critiques of my case-specific analysis that I will turn to next. In particular, he argues that there existed a much starker difference between the Bank of England as 'advisory' versus 'independent' central bank than I admit, and that I generally underemphasize the role of Treasury mandarins as well as British politicians, who have learnt their ways to mess even with an independent BoE.

I fully accept that my historical narrative is partial. I did not engage in the details of party politics around monetary policy and large concentrated on key moments at which I saw governmental choices to be critical (Barber's 'Dash for Growth', Thatcher's ill-fated monetarism; Healy's acceptance of published money supply targets; Howe's push for 'expectational monetarism', to name just a few). Party-political developments and strategic choices, for example, Gordon Brown's clever move to shield Labour from market attacks by granting independence, matter. Yet, I stick to my major claims with regard to the Bank of England's critical role in shaping the development of monetary policy as well as its own institutional position. Why did the Bank prioritize 'private influence' with the Treasury over an overt monetary policy stance in the late 1970s? Because it believed that this was a securer institutional foundation at a point in history at which it frankly had no clear idea of how to practice policy. I would see the same reason for why the Bank failed to offer a politically viable alternative to the ill-fated (half) steps toward 'monetary base targeting' in the early 1980s. But the major case for the Bank's agency comes from the developments in 1992–1993. Eddie George and Mervyn King fundamentally transformed the monetary policy game at that historical moment. Britain had dropped out of the European Exchange Rate mechanism, but the Bank remained under the authority of Chancellor of the Exchequer; just as Thatcher, John Major would not consider independence. However, change did happen because the Bank figured out how to practice expectational coordination with markets and use the short-term interest rate instrument to predictably influence the yield curve in a radically transformed UK financial sector (connected by repo rather than bill markets). Rather than politicians making big reforms, these innovations and their interpretation with the means of advanced Post-Keynesian economics fundamentally changed understandings within the organization and beyond about the central bank's role. This was a key reason why Gordon Brown's decision for independence only constituted a next, yet important step.

This brings me to Nathan Coombs' critique that the 'the book's minimal engagement with economic theory' is one of its key weaknesses. I do not think that this critique is fair. My extensive empirical analysis of monetarist experimentation looks at how monetarist ideas traveled into central banks, and how experts within these organizations (e.g., Charles Goodhart at the Bank of England; Kurt Schildknecht at the Swiss National Bank) translated these ideas in accordance with political, structural economic, and financial conditions of central banking. I also discuss, for the British and Swiss cases, how different relationships between inside experts and outside audiences (in the UK, not just academics, but also

analysts and think tankers) led to different perceptions about the legitimacy of monetarist policy. That Friedman's monetarism or the base targeting approach advocated by Brunner/Meltzer remained marginal to the actual policy practice is clear from the historical evidence and does not reflect my neglect of economics.

For IT, my argument again is not that New Keynesian economics is irrelevant but matters in ways that are often misunderstood. As I discuss on pp. 109–11, the existing literature sometimes suggests that IT is an applied version of such economics, with DSGE models guiding interest rate choices. By analyzing Paul Volcker's early experiments with interest rate signaling in the 1980s and IT's introduction in the UK, I show by contrast that key steps toward the development of IT were taken before there existed a New Keynesian consensus (on the practical importance of Lucas' neoclassical economics, only have a quick look at the graph p. 123) (Goodfriend, 2007). Calculations of 'the stars' (NAIRU; equilibrium interest rates), which then became more embedded in central banking during the late 1990s and 2000s, helped to refine expectational coordination with markets and rationalize such coordination toward external audiences. The story repeats itself: 'Portfolio rebalance' theory, the expert framework for QE, came circa 4–5 years *after* the initiation of massive asset purchase programs (on this, see Bernanke, 2020). My view, then, is that you will never figure out how economics truly matters if you, *prima facie*, start from the premise that central banks are technocratic enactors of scientific reason.

Moving forward

I agree with Omarova: In times of profound transformations, academics cannot escape the responsibility of laying out the field of possibilities as it presents itself to them. 'Captive' central banking should not persist, and we need to think seriously about how to change the status quo. In the best case, we can show when discussed 'solutions' to current predicaments are unrealistic or even just wishful thinking, and which serious options exist that have not even been considered seriously. For Omarova, most potentials for change reside in the introduction of truly public Central Bank Digital Currencies (CBDCs). Such CBDCs would, in her view, end central banks' (and the state's more general) dependency on commercial banks for managing the monetary system. This is an important debate for the coming years. One open question that I have is whether scholars see CBDCs as a path toward centralized credit allocation (i.e., credit can only be created by the issuer of CBDCs), or whether decentralized allocation will remain with private banks, who could for instance finance such allocation with time deposits that pay higher interest rates (with potential threats from shadow banks). I am highly skeptical of centralized credit allocation, as I am indeed skeptical of central banks acting as institutions to allocate credit directly on political grounds. I think that it is useful to differentiate the refinancing function, which properly resides with central banks (as banks' banks) from due diligence of projects and credit decisions. More direct public involvement in credit policies should happen via different bodies, e.g., public investment or development banks.

With regard to more democratic control over central banking, I think that the historical evidence reveals the problems of governments messing with central bank activities. For instance, governments are not good at setting interest rates. So I would maintain operational independence. What I would consider as a possible if not necessary route of reform is to reconceive coordination. The idea that monetary policy is entirely separate from the other economic policy domains (fiscal, financial, and industrial) is ridiculous. This is what the QE era and the energy and cost of living crisis have taught us. Therefore, to revive comprehensive coordination and to strengthen democratic accountability, it would be useful to establish executive committees consisting of central bankers, finance ministers, and other actors involved in economic policy to set out frameworks for the

governments' macroeconomic objectives. Such plans would define the rates of growth and inflation, developments in productivity, socioeconomic distributions, decarbonization, and financial stability (e.g., in the housing market) to be achieved for periods of 2 to 5 years. Central banks would remain independent in implementation but accountable for delivering policy in the framework of the coordinated governmental plan. If central banks would consistently undermine such plans, government and parliament could reconsider independence.

But next to normative questions, analytical ones remain. What has happened to macroprudential policy? How can we better incorporate current ideas from international political economy (e.g., on monetary hierarchies, global liquidity cycles, etc.) into comparative perspectives on central banking? What role do central banks play in twenty-first century geoeconomics and war? I count myself lucky to be part of a thriving research field that engages with these questions and will aim to do my bit for advancing it in the coming years.

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