Review symposium. *Banking Across Boundaries: Placing Finance in Capitalism* †

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**Introduction**

Brett Christophers’s *Banking Across Boundaries* is an unusually wide-ranging and ambitious exploration of the role and ‘place’ of banking and finance in Western capitalist systems. Prompted by the Wall Street crash and its lingering aftermath, the book nevertheless delves deeply into the histories of banking, from merchant capitalism through the industrial age to the contemporary period of ‘financialized’ capitalism. This symposium features commentaries on the book from Fred Block at the University of California at Davis, Mark Blyth from Brown University, and Ewald Engelen at the University of Amsterdam, following which Brett Christophers responds.

Jamie Peck

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Placing and misplacing banking within the global economy

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Brett Christophers has written a wide-ranging, brilliant, and imaginative book about one of the most important topics in contemporary social science—the role of banks in the contemporary global economy. He deserves particular praise for the richness of his historical perspective and his intrepid scholarship in mastering the arcane details of how banking has been measured by different schemes of national income accounting over the last eighty years. Nevertheless, the final verdict on the book has to be critical because Christophers fails to deliver on the promise of his subtitle: the book does not satisfactorily ‘place finance in capitalism’.

The core of Christophers’s contribution is to remind us that through most of the 19th century, economists treated banking as basically an unproductive activity that was at best indirectly productive because it contributed to the efficiency of business firms. But in the 20th century, national income accountants were forced to revisit this issue as they sought to measure the economic contribution of different types of activity. Most early practitioners of this essential technocratic art basically opted to treat banking as similar to government in being unproductive by treating its output as equivalent to its payroll. The exception was Simon Kuznets in the United States who devised a method that calculated banking as producing an economic output that was greater than its labor inputs. When a variation on Kuznets’s methodology was embraced by the Bureau of Economic Analysis, the US became pretty much the only nation whose accounts treated banking as directly productive.

Christophers traces out how France and the UK began revising their accounts starting in the 1970s to adapt schemes in which banking was magically transformed into an economically productive activity. Not surprisingly, international standards as proposed by the United Nations System of National Accounts played a key role here with revisions in 1968 and 1993 that were widely influential. But his key argument is that in the United States, the UK, France, and Germany, in particular, national income statistics provided an important rationale for the increasingly aggressive moves to internationalize banking that began in the US in the late 1960s and accelerated in all four countries from the 1980s onward. To be sure, Christophers is careful to nuance his argument; he recognizes that other factors were important in dismantling barriers that had existed since the 1930s to a renewed internationalization of banking. He argues that the statistical sleight of hand that allowed banking to cross the boundary from unproductive to productive played a critical role in legitimating the movement of banks across international boundaries.

In making this important and persuasive argument, Christophers covers a lot of ground; he reviews the history of banking in the Middle Ages and recounts the history of modern economic thinking about banks. But along the way, he occasionally missteps and gets into arguments that are largely peripheral to his main storyline. For example, in talking about the creation of the post-World-War-II international financial order, he makes a strange argument about Harry Dexter White, the chief US architect of the Bretton Woods institutions. Two recent accounts of those events, Robert Skidelsky (2001) in the final volume of his Keynes biography and Benn Steil’s 2013 book, The Battle of Bretton Woods, argue without sufficient evidence that White was effectively an agent of the Soviet Union. Christophers, however, goes in the opposite direction and argues that White’s support for “productive” capital flows reflected his training as a neoclassical economist (page 113). The reality is that White’s views at this point were quite close to those of Keynes, but that his language was constrained by continuous pressure from US banking institutions that were already lobbying for an active international role in the postwar world.
While this issue is relatively minor, towards the end of the book, Christophers launches a misguided polemic against Greta Krippner and other recent writers who have talked about processes of financialization of the US and UK economies. Krippner is very clear in her book that the process of financialization that she describes is closely connected to US foreign economic policies that have forced other nations to finance the US chronic balance of payments deficits by lending the US hundreds of billions of dollars each year. Nevertheless, Christophers denounces her as a methodological nationalist and argues that what she and others have mistakenly called financialization is simply the more aggressive pursuit of foreign growth strategies by banks.

But this unnecessary attack backfires because it puts into sharp relief the questions that Christophers fails to answer. Christophers provides lots of indicators of the financial services that the US and UK export and the profits that their financial institutions earn abroad. But he never addresses the question of how these banks earn profits globally. It is ironic that, despite his preoccupation with the debate about whether banks are productive or unproductive, he ends up ‘black boxing’ banking institutions—never addressing the question of how they actually generate profits either at home or abroad.

The way I see it, globally relevant banking institutions have four potential profit streams. The first is standard intermediation: they raise money either with deposits or by borrowing in the money markets and then create credit on which they earn higher rates of interest. The second is that they can extract profits from production by exercising either direct or indirect control over industrial empires; this is what Lenin and Hilferding were describing as “finance capitalism” but which is now distinctly less common in developed nations. Third, they sell a range of services for which they earn fees or commissions such as raising money for private firms and governments in the capital markets. Finally, and most notoriously, they trade on their own account in foreign exchange markets, derivative markets, and commodities markets.

The difficulty with Christophers’s account is that he tells us almost nothing about the relative importance of these different profit streams in this recent period of bank internationalization. What is worse, he creates the misleading impression that much of the action is coming from international banks either accumulating a larger share of consumer deposits or deriving profits from production in the newly industrializing countries. He creates this impression by contrasting financialization with old-fashioned capitalist expansion—his view of what is going on with international banking. But such inroads by international banks have been negligible in some of the key countries such as India and China, so it is implausible that this is the key explanation for rising overseas bank profits. A more likely suspect is the spectacular growth of global currency reserves—now estimated at close to $11 trillion in value. With about a third of this held in dollar instruments, US-based financial institutions are able to earn substantial fees for moving this money in and out of different financial instruments while simultaneously exploiting the information in these international flows to enhance their own trading profits. To be sure, giant international banks also make money by helping foreign oligarchs manage their billions and by gaining a growing share of the business of servicing foreign corporations. But it is simply folly to separate the spectacular internationalization of US banks from the unique role that the United States plays in the global political economy; both in providing the major global reserve currency and in maintaining a huge structure balance of payment deficit for decades.

In the end, Christophers fails at ‘placing finance in capitalism’. His historical account of the period from the 1930s to the 1970s makes clear that the international economy worked well for close to forty years without globalized banking institutions. Was that period just a strange and temporary anomaly or does it suggest that concerted political pressure could again dismantle the global reach of Citibank and Deutsche Bank? Despite the limitations
of Christophers’s efforts, he deserves our gratitude for raising questions that urgently need answers.

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A curious case of caveats and causes: some thoughts on the causal story of Banking Across Boundaries

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Introduction
A dirty little secret of academic practice, at least in political science, is that you can assign books by certain presses without having read them, their argumentative structure being so rehearsed. Typically, for a graduate class, you would assign chapters 1, 2, and the conclusion. Chapter 1 will give you an overview of the book and its key arguments. Chapter 2 will give you the author’s place in the literature and the key theoretical claims they seek to make. The conclusion is then the introduction backwards, with more evidence to substantiate the claims made earlier, as taken from the empirical chapters that you have not read. Especially when gearing up for comprehensive exams, the secret is that one does not need to read the case material/data/tests that make up the bulk of a book. We take in on faith, rightly or not, that, to use a British phrase, ‘it does what it says on the tin’. In this regard Banking Across Boundaries (hereafter BAB) is an anomaly. Its core claims are innovative, important, twofold, and can be summarized quickly. First, the system of national accounts developed in the Bretton Woods era had a hard time dealing with banks. Their ‘intermediation services’ did not register as additive to growth in gross domestic product (GDP), banks being seen as a pass-through/matching-agency for the rest of the economy. Revisions to the UN’s template for ‘how to count what counts’ in the economy, the System of National Accounts (SNA), first in 1968, and then later and more substantially in 1993, finally got around this problem by identifying the taking-on of risk as the productive ‘thing’ that banks do, and so they could now be represented as ‘productive’ in the world.

Political actors championing banks’ interests, sometimes/maybe appealed to these national account numbers, but there is not much evidence that they did. Indeed, the period of international bank expansion preceded the change in the national accounts for the UK and France, while for the Americans it was old news since banks had always been seen as productive in their national accounts thanks to Simon Kuznets. Nonetheless, by shifting banks from the ‘unproductive’ to the ‘productive’ side of the national accounts ledger, the geographic expansion of banking that was already underway since the collapse of Bretton Woods if not before, was in some sense ‘enabled’ by this statistical treatment. Here then lies my first issue that I want to raise. BAB is careful to say that the change in national accounting standards did not cause the geographical expansion of banking. But if not, why am I reading so much about it? If the timing is off, what is there to explain?

The second argument is that the expansion of banking itself is geographic, and that matters because those who press the financialization thesis (Krippner, Epstein, and Jayadev, etc) are wrong—somewhat—maybe—a bit. In this regard, BAB tries hard to marshal evidence, and then not make any strong claims for it. For BAB the rise in the profitability of the financial sector that has occurred since the 1980s, relative to the rest of the economy at least, that these ‘financialization’ authors offer as evidence does not in fact signal a qualitative shift in capitalist accumulation dynamics. Per contra these authors, what is really going on is a geographical shift in banking activities (above all foreign branching out from the US and the UK) and (minor) the tax treatment of profit remittances. The shift to finance has been geographic, not structural. Geography trumps (late-capitalist) morphology.

My misgivings about causation and the caveating of claims notwithstanding, I have no fundamental problems with either of these claims per se, although I shall have cause to
question some aspects of the claims that surround them as we go forward. Indeed, I want to stress here at the outset that BAB is an important book that gives us important correctives to established narratives. But despite this I have one further problem with BAB: you have to work really hard to get to these two claims since the substance of the argument comes in the last two chapters. Much of the rest of the book, more or less, gets in the way. This is not to say that the rest of it is uninteresting—far from it. But if these are the core claims, then much of the rest of BAB, especially the first few chapters, is the proverbial ‘woods’ getting in the way of the ‘trees.’ Making this more difficult are the unnecessary caveats and qualifications to the book’s major claims alluded to above, especially regarding causation.

Woods and trees on the way to the findings
Regarding this first point, consider the areas that BAB tackles before getting to its core theses: a set up about FISIM (financial intermediation services indirectly measured) that does not come back until chapter 5; a review concerning the literature on performativity that does no work in the book itself aside from one mention of the word on page 190; an examination of the concept of value that has no bearing on the actual conclusions or the discussion of the national accounts per se; a prehistory of banking from the Middle Ages through the 18th century; plus Marx gives way to the marginalists. And this is only chapter 1. Again, it is all very interesting, but do we need it?

There are two problems when one tries to embed a relatively simple thesis in such historical complexity. The first is that, in trying to cover so much, the claims made on the way to the two major claims in BAB bog the reader down in ancillary and contestable side issues that have little bearing on the overall thesis. Emblematic of this problem is the discussion of performativity. Despite Barnesian performativity being extremely difficult to evidence directly, to the point that sociologist Kieran Healy gives it ‘the Scottish verdict’ of ‘not proven,’ even in the best of cases, BAB invokes it and seeks to contribute to this literature. Yet this theory-heavy discussion, along with much of the nods to Marxism and other kindred approaches, does not actually add to the substantive argument made at the end of the book. No one is performing anything, at least not in a Callon/Mackenzie sense, as far as I can see, anywhere in the narrative.

The second problem is that by opening up this broad historical territory, it invites contestation over its accuracy and, thus, the accuracy of the rest of the thesis. To take a few examples, arguably the worries that Aristotle and Aquinas had over the charging of interest had less to do with moral worth (or, in the latter’s case, his borrowing from Augustine that God alone owns time so interest is theft of the heavens) than for the effect of credit and speculation on societies that constantly live on the edge of indigence. Aristotle’s commentaries on trade are emblematic in this regard. Similarly, BAB makes the case that ideas did not matter in the ascendance of the capitalist interest, yet one could point to Hirschman’s The Passions and the Interests (1977) as making exactly that countercase quite convincingly. Similarly, citing Adam Smith is always tricky. Yes, he was very critical on banking in places, but in other places he applauded fractional reserve banking, and crucially, the multiplication of the credit base from the export of gold and silver that this makes possible. In sum, front-loading such contestable material distracts from where the book is going and risks losing the audience before it starts.

This problem is compounded by the ‘story-arc’ of BAB itself: “World’s apart”, “World’s aligned”, “Co-constituted worlds”. The titles are clever and inviting, but fail to capture the material that falls under these headings. My reading of the book (and I read it twice to make sure that I got it) sees the first part, “Worlds apart”, as somewhat redundant. Apart from the problem of excess historical detail and a too ‘longue’ a ‘durée’ for anyone’s good, it begins with a rather tortured disclaimer about causation. Rather than say X led to Y, and that X
is germane for the overall argument given factors B and C (forgive me for sounding so positivist, but in my dotage my ability to deal with fudge has declined rapidly) BAB wishes to talk of “technologies of enablement” (page 58) that allowed causes “to be seen as causes”.

Now in one respect I am the least likely scholar around who would critique an ideational argument, but the deployment of “enablement” and earlier “dialectic(s)” (page 11) leaves me wondering what is actually being claimed. Once things are being performed without actors and enabled in dialectic processes, agency evaporates and we are left with a world where processes coconstitute other processes that lead to outcomes. Life, in a sense, causes life, without any living beings, and I find myself unsure as to how to judge the claims being made. Is BAB right? How would I know? “World’s apart” travels from the late Middle Ages to the Huguenots to Industrial Britain, and yet as I journeyed so (twice), I wondered where was I going and what did all this have to do with FISIM?

In the second chapter of “World’s apart” the woods get still more dense, and the upshot of it all seems to be the claim that the first time finance went global (which is normal in the grand scale of things) it was not driven by ideas, or by banks. Instead, despite the caveat about causation, really big things such as war, empire, modernization, and ‘economics’ ‘explain’ (cause?) what is going on. But surely the sum of these very large factors is pretty much social life itself? Given such broad brush-strokes what is there left to explain? As BAB notes on page 84, “arguments for openness were not needed because the world was, for the most part, open” is telling in its redundancy. For if this is the case, why do we need a whole chapter showing that such arguments were absent? And yet, once again, were they really so absent?

Take the discussion of Ricardo and mobility on page 88, for example. He may well have never invoked the price–specie–flow model as is contended, but if one reads all of Ricardo’s Principles, one may find that the argument for comparative advantage is derived from the common rate of profits forcing capital to exit one market and move to another. It does not stand alone as modern economics has it. Such exit, rather obviously, presumes capital mobility. How else would capitalists open up new markets in other places where there was a comparative advantage if their capital was immobile? More generally, if the ideas of the period were irrelevant, why did Ricardo, Malthus, and all the rest spend so much time writing them down and publicizing them? Again, as in the first chapter, why bother digging all this up in the first place? Just as there is no need to start in the late Middle Ages to tell a story about the 1993 system of national accounts, there is no need to prove the effective absence of ideas in a much more remote period to do the same.

**Out of the woods**

Given all this, for me BAB really starts on page 106 where the discontinuity that makes the present moment so ordinary is made apparent: the retreat to the nation of finance in the Bretton Woods period from its early open range ascendancy. This chapter situates the argument much better than the foregoing two and does the real theoretical heavy lifting on the construction of what is productive and what is not, and how that boundary has shifted over time and place. Key here is the contribution of banks to gross value added and thus to GDP and the paradox of a profitable sector showing up in the national accounts as unproductive: the banking problem.

Ultimately we learn the banking problem was really a British and French banking problem because the US and the Germans simply did the national accounts in a different way. Yet BAB’s strength lies in its dissection of the accounting methods employed in different periods and places, showing that you could make banks either productive or not depending upon the use of income methods versus output methods, the invocation of entire dummy industries to use banking services as an intermediate input in the 1968 SNA, and a host of other issues. Yet, as BAB argues, “we simply cannot understand the UK and French approaches [that make banks by definition unproductive] unless we understand the … intellectual contexts.
in which they were decided.” So this chapter sets up the banking problem and the boundary problem that is the core of the book while the intellectual context is the subject of the next chapter, and that takes us back to the end of the Bretton Woods era.

Chapter 4 wants to go beyond the standard retelling of this period as being the emergence of the Triffin dilemma and the impossible trinity on a global level due to the growth of the Euromarkets, and in this respect BAB succeeds in some interesting ways. The fact that US banks were first out of the gate for expansion long before arguments for financial liberalization became dominant is indeed an important corrective to the existing literature. Indeed, this spread of US banking in the 1960s and early 1970s is especially well detailed. But again, to get there, BAB has to take us back quite far to take us a little forward. Kuznets, Yntema, the misquotation of Henry Morgenthau, and even Smoot Hawley are discussed and dissected. And once again I found myself wading through all this waiting for the narrative to start again.

History matters, we are repeatedly told, but not all of it matters equally. Puzzling for me in this regard is the absence of any sustained discussion of the capital flight from Europe prompted by the abortive early return of convertibility in Europe in 1947, the ultimate result of which was the Marshall Plan and the placing of international bankers on the naughty step of the US State Department for a decade. Also missing is the Cold War and how its imperatives dictated what counted as productive finance (investment in job-creating goods-making at home that can be taxed to pay for welfare states). The musings of national accountants, while important, happened in a much broader context. Occasionally, and sporadically, power and the Cold War makes an appearance, as when we are told that, “the US government was, as Helleiner has shown, bent on preserving … hegemony” (page 167). But this pops out somewhat disconnected from what came before. Likewise, when the topic shifts back to the discussion of the construction of national accounts, of SNA 1953 and the changes in SNA 1968, where we had already been before, I felt the argument being pushed in a causal direction—that the change in accounting standards ‘caused’ the subsequent growth of finance, but here, as late as the end of chapter 4, I cannot find a clear causal statement that this is in fact what BAB wants to show me. Indeed, I was told earlier that BAB does not want to make this claim. So where are we at this point?

**Into the clearing**

Luckily, chapters 5 and 6 clarify it all and bring the argument together at last. Chapter 5 is for me far and away the best part of the book, for the simple reason that the historical jumping around and overheorizing stop and the story really starts. Agents, real people with names and projects, appear. Firms with agendas and coalitions with interests that produce ideas to further those interests come to prominence. And without any sociological obscurantism whatsoever, except for the invocation of the word “performed” on page 190 for no good reason, the effect of ideas on practices through time is laid out beautifully. You may have had to wade through a lot to get here, but it is worth it when you do.

Chapter 5 is indeed a “battle over ideas” and BAB details this in a wonderful manner. American Express, Citigroup, and AIG bankrolled the lobbying and ideational assault of policy entrepreneurs such as Geza Feketekuty while the executives of these firms pressed the case for the liberalization of banking as a trade issue in international fora. It is here that we get closest to a clear causal narrative where the lobbying of these firms, which had been going on for a decade, enjoined with the changes in the SNA treatments of banks in the UK and France, banks that were also expanding internationally, to create a chorus for openness and liberalization.

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(1) It also makes me question my reading of Helleiner. I searched the footnote concerned but found no page number to substantiate it.
But even here there is a bit of an endogeneity issue at play. As BAB notes on page 204, it was not just banking that was becoming more central to the economies of the developed world: it was services in general. Today the US and UK are over 80% service driven. Given such a general shift towards services, many of which are exportable but not bank based (from consultancy to call centers), it is hardly a surprise that trade in services suddenly found a new resonance. But in such a light, does the change in SNA 1993 really matter that much even if it is endogenous to the same set of processes? It seems that French and British banks were expanding internationally regardless of such changes.

This brings me back again to the issue of causation. One does not need to be a positivist or embrace linear efficient causation as the sine qua non of social scientific explanation to see that, while there is a correlation between the changes in SNA 1993 that made banks in the UK and France productive, it not clear that this caused their internationalization. But putting up front that one does not wish to talk about cause and instead wants to invoke “technologies of enablement”, “assemblages”, and “performativity” is no defense. Indeed, in doing so the cause of the book is hindered not helped. At the end of chapter 5 I still find it hard to know the extent to which SNA 1993 propelled banking to anywhere other than where it was already heading, especially given that the UK and the French were fully incentivized given the political climate of the 1980s and 1990s to catch up with the Americans.

And ending on a segue
In this regard chapter 6, which I can usually rely on to be a conclusion, is not. It is a separate argument about the robustness of the financialization thesis. Personally, I think that BAB has challenged it quite seriously, so long as financialization is defined as abnormally high profits coming from home-domiciled banks relative to the rest of the same national economy. One could (and perhaps should) broaden the definition out a little to include such factors as the brain drain caused by finance on other sectors due to the multiples it gives in compensation over other sectors, the cost of a free option on the public of too-big-too-fail institutions on growth, and the collapse of US gross domestic capital formation despite the booming of investment banks, but that is perhaps for another day. As it stands, BAB shows how much that seems to be domestic financialization is in fact internationalization of the banking services of US and UK and French banks. Or does it?

Three issues remain outstanding for me in this regard. The first is the profit share accorded to ‘rest of the world’ transactions (figure 6.1, page 242 and figure 6.7, page 262). The boom period is 2006–08. Only in those periods do we see overseas profits beating domestically generated profits. But there is a problem with picking this period. This is also the period of the global banking glut where the US hosted over 161 foreign banks “who collectively raised $1 trillion of wholesale bank funding, of which $645 billion was channeled for use by their headquarters” (Shin, 2011). Now what did those UK and French and German banks do with all this cash? They bought US subprime mortgages. Fully 70% of the SIV’s set up to deal with mortgage CDOs were European (Blyth, 2013, page 85). Given this, much of the profits being made abroad by US banks are actually being made by selling domestic assets to foreign banks with local (US) offices who use local (US) money to buy what are to them foreign assets, which are then parked offshore. Yes, this shows up as an international transaction, but to paraphrase Star Trek, ‘it’s financialization Jim, but not as we know it.’ BAB may have dealt a blow to the financialization thesis as it stands, but the wider notion of economies being financialized, and that being a qualitative change, remains, in my view, intact.

The second is the claim that “repeated waves of globalization … have decisively put paid to the national economy as a material reality.” For a book that takes such a careful approach to causation this is a very strong claim. It is also wrong and misleading. To take but one example: the Eurozone periphery is in an austerity-induced depression precisely because
of the material reality of these economies being national economies deprived of their own printing presses. Unable to act as a national state and yet being required to bear the costs of one, they cannot inflate or devalue: all they can do is deflate internally, with very material consequences. Their bond markets are national markets with national yield spikes paid by national publics. Their labor markets are national labor markets with national unemployment rates. Such claims may make for a good rhetorical flourish at the end of the book, but they are deeply misleading as analytic insights. National accounts may ‘fetishize’ the national scale, but so long as taxation, democracy, and currency remain national responsibilities, we are stuck with them, and their very material consequences.

Third, and finally, I come round again to the issue of causation. At the end of BAB’s thorough examination and partial demolition of the financialization thesis we read the following. “Where, then does all of this leave the financialization argument? The answer remains unclear” (page 264). Really? Because throughout chapter 6 I saw clear causal claims and a plethora of data making a strong case against it. I may still resist calling it a knockout as per my concern noted above, but it has been given a pretty searching exam. Why then the hesitancy to make more of a deal out of it? Perhaps it is style, or perhaps it goes back to the hesitancy to embrace causation that we find throughout BAB? Chapter 6 may have been a segue rather than a conclusion, but it was similar to the rest of the text in one regard: this hesitancy to say X causes Y is endemic to the whole book and it has a very real and material cost—the impact and importance of what is being said suffers.

We are five years out from the biggest financial bust ever. Growth remains sclerotic. Policy verges from the benign (US) to the malign (eurozone periphery). And the one thing we desperately need is greater knowledge of the financial sector in terms of its productive value to society, its costs to society, how (and where) it makes its money, and all the rest. BAB gives us a great deal in this regard, but the reader has to make it into an argument since the book backs away from embracing cause. Is finance productive? I think BAB has an answer to that and the answer is no since the 1993 SNA ‘risk-fix’ is just as dodgy as all the other ones. But without making a causal claim, BAB is unable to say so.

The field of economics thinks in a language of correlation through econometrics and yet talks a language of cause which is consequential for the lives of millions via the logics of central bank independence, expansionary austerity, and all the rest it peddles as its wares. Political economy, especially its critical variant, cannot afford to sit on the sidelines of noncommitment and expect to be heard. BAB is structured around a language of cause, which is the only language that allows intervention, and yet it eschews that language and qualifies its findings to the point that we are at the end ‘unclear’ about what it all means. BAB instead wants to note how some things ‘enable’ other things to happen. I do not think that in the current moment ‘enabling’ is good enough, which is why I think BAB is a very good book, but it would have been a much more powerful book had it not hidden from a logic, and language, of cause.

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Geography can explain much, if not all…

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Christophers’s *Banking across Boundaries* is a rich and rewarding read. It covers the history of economic thought and European banking in part 1, the construction of national accounts in the US, UK, France, and Germany in part 2, and discusses the creation of a global market for banking services and whether or not this caused financialization in Anglo-American economies in part 3. And that wide-ranging intellectual exercise is held together by the exciting and highly topical issue of the (un)productiveness of banks. Do they belong to the productive sphere of capitalism as banksters claim; the sphere of circulation as Marxists claim; or, worse, are they mere parasites that syphon off wealth created elsewhere, by other factions of capital, as bank bashers claim?

Each of these discussions builds on distinct literatures, raises and debates new albeit-related issues, and hence calls for independent critical treatment. In part 1, for instance, Christophers claims that premodern banking operated in a world that was marked by openness and hence lacked the need for a legitimating narrative in favour of ‘disembedding’—as was the case in the world of closed national borders that came into being after World War 1. This image of premodernity as coming straight out of the neoliberal rule book is distinctly different from the one painted by Karl Polanyi (1952 [1944]), E P Thompson (1977), Eric Hobsbawm (1962; 1975), Christopher Hill (1963), and their ilk, raising pressing questions about the empirical justifications for Christophers’s view.

Similarly in part 2, where the history of national accounts is recounted. To explain why US officials put banks on the productive side of the ledger and why French and British officials did not—with Keynes making an important, little-known cameo appearance—Christophers makes much of a famous remark made by Henri Morgenthau, head of the American delegation at Bretton Woods, stating that Bretton Woods was about saving the world from financial speculators. In the international political economy literature this quote is usually read as representative of the United States’ postdepression aversion to high finance [see Helleiner (1996) for many] and as such as hard to reconcile with the simultaneous categorization of banks as productive in US national accounts.

But there is no inconsistency here, so Christophers claims. Morgenthau has been serially misquoted and did not in fact phrase a blanket rejection of banking but merely attacked a few bad apples, allowing Christophers to paint a picture of American economics and economic policy making as being fully converted to the tenets of neoclassical economics, which, with its atomistic, utility-based, and market-invested worldview, simply had had to ascribe productive functions to banking since it does receive market prices for its services.

This is all well and good, but how does the claim of full and painless conversion to neoclassical economics compare with the historical record, which suggests a painful “struggle over the soul of economics” in the 1920s, 1930s, and 1940s (Yonay, 1998) and plenty of strongholds of institutional economics and Keynesian economics even now, as is indicated by the continuing battle between ‘saltwater’ and ‘freshwater economists’ (see Krugman, 2009)? This raises once again pertinent questions about the empirical backing for Christophers’s claim that it is ideas rather than interests that make the world go round.

But it is part 3 that I got worked up over most. For it is here—in particular chapter 6: “Anaemic geographies of productive finance”—that Christophers launches a frontal attack on a body of work to which most of my own academic writings have one way or the other contributed, namely the financialization literature. This is a wide-ranging, highly diverse
corpus of researches—tying together contributions from comparative political economy, critical accounting studies, heterodox economics, economic sociology, and geography—which have all flagged the growing empirical, theoretical and political importance of finance in, of, and for contemporary capitalism [see Engelen and Konings (2011) for an overview]. While there is no consensus on theory, method, data, metric, or definition in this literature, in a broad sense it is all about the very same rise of finance that Christophers himself addresses.

Despite being fully aware of these caveats, Christophers explicitly restricts himself to “a specific understanding of financialization” (page 232, my emphasis): namely, the strand that is concerned “with the structural dynamics of the capitalist economy” or, in Krippner’s words, with “a pattern of accumulation in which profits accrue primarily through financial channels rather than through trade and commodity production” (2005, page 174). He recognizes “the salience” of other strands in the financialization literature, but nevertheless launches a strongly worded academic diatribe against financialization as such, in the process losing sight of many of his earlier subtleties.

Here is how his assault goes. Since the metrics used by scholars like Epstein and Jayadev (2005) and especially Krippner (2005; 2012)—value added, profits, portfolio income, etc—are constructed in terms of the very same national accounts [ie, gross domestic product (GDP)] that served as the backdrop of the story of internationalization of banking that Christophers tells in part 3, they are subject to the same geographical skews and functionalist biases.

The most important one, according to Christophers, is the absence of a geographical breakdown of the sources of revenue and profit that are aggregated as ‘exports’ in national accounts. As a result, financialization scholars have drawn unwarranted, again according to Christophers, inferences from an empirical rise of aggregate revenues and profits of US and UK banks. Krippner, for instance, uses employment share and share of value added in the US to demonstrate that the contribution of FIRE (finance, real estate, and insurance) to US GDP has increased from a little less than 10% in 1950 to a little less than 25% in 2001, while over that same period its employment share remained small (between 5 and 8%) and stable (2005, page 178).

According to Christophers, metrics such as these are fundamentally flawed because financialization scholars have failed to confront the possibility that what they have framed as the outcome of ‘financialization’ per se is actually an effect of globalization. If one tries to correct for the ‘anaemic geographies’ of national accounts, it appears, according to Christophers, that

“since 1999 overseas markets have made a disproportionate contribution to US finance sector profit growth, which is to say that the rate of such growth … would have been lower without the contribution from overseas markets. … In view of the scale of this internationalization, it is at best misleading to read off from trends in spatially-circumscribed profitability metrics … a fundamental mutation in contemporary or ‘advanced’ capitalism” (pages 262–263, 264).

At the end of the chapter Christophers draws the harsh conclusion that the combined financialization literature might as well be dumped:

“One we reinsert the wider geographies of capitalist accumulation into the spatially-anaemic conceptual framework that the notion of financialization inhabits, the need for a theory of structural change [of capitalism] ebbs away; geography can explain much, if perhaps not all of the finance sector’s apparent ascendency. And if we doubt the financialization of capitalism, of course, we must also question the conclusions that have been drawn from it” (page 265, italics in original).

This is unfair for at least three reasons. First, scholars like Krippner are acutely aware that the metrics they use can carry both financialization and globalization explanations. In fact, Krippner dedicates a whole section of her 2005 paper to unravel precisely this issue
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(page 193–198), arguing that FIRE exports are relatively limited and hence cannot account for the sector’s rising share of value added of the overall US economy. While Christophers makes much of the increase in the export income of US banks since 1986 (page 260), he fails to put his figures in perspective: the figure given for 2010, $91 billion, is next to nothing compared with total US export of $2.1 trillion, which is itself a mere 14% of US GDP, and is negligible relative to total US banking assets of $14.4 trillion—approximately the same size as the US GDP.

Of course, this is not true for the UK, where the internationalization of banking did indeed result in a financial hydrocephalus in the form of the City of London and where as a consequence financial services have on many measures grown exponentially. According to figures from TheCityUK, a London-based lobby organization for the financial services sector, total financial services exports increased from £22.5 billion in 2000 to £47.2 billion in 2011, down £4 billion from a high of £51.2 billion in 2008 (TheCityUK, 2013), which is approximately 8% of total UK exports, which in turn is almost one third of UK GDP.

In other words, financial services exports make an incomparably larger economic splash in the UK than in the US. And since it is the UK that serves as Christophers’s “exemplary situation”, his “anaemic” argument explodes in his face: UK exceptionalism implies that his conclusions cannot be extrapolated to the US, since banks are so much smaller and financial markets so much more important for the allocation of capital in the US than in the UK, let alone to other political economies with other banking structures (Verdier, 2003). Failing to acknowledge this is nothing less than a capital crime for a geographer, especially one who reproaches others for falling into the “territorial trap” of privileging the level of the nation-state (Agnew, 1994).

Second, Krippner and others have cast their financialization nets much wider than Christophers is willing to recognize. Krippner emphatically stresses the importance of including wider corporate transformations in her definition of financialization: that is, the increasing share of financially based revenues in total corporate profits. Hence, a crucial part of the financialization of the US economy consists of firms like Chrysler, Boeing, General Electric, and Exxon generating increasing shares of revenues and profits from financial transactions. This development too, for Krippner, cannot be accounted for by the spatial dispersal of value chains that is captured as ‘globalization’ and which, according to global production network theory, leads to the concentration of the handling of internal capital flows in US-based or UK-based holdings and hence to a disproportionate share of financially based revenues in those jurisdictions.

Third, Christophers’s claim that it is geography not “the structuralist dynamics of the capitalist economy” (page 232) that accounts for the appearance of financialization fits awkwardly with the wealth of academic and nonacademic reports from recent years which have stressed the extraordinary and historically unprecedented growth of financial instruments, public and private indebtedness, banks’ balance sheets, pension savings, assets under management of hedge funds, private equity funds, sovereign wealth funds, etc.

Even a quick glance at reporting by the Bank for International Settlements (BIS), the International Monetary Fund, the Financial Stability Board (FSB), or McKinsey Global Institute (MGI), which over the years have tried to paint a more aggregate, global picture of the ‘financial revolution’ of the last three decades, suffices to dispel any doubts about the extent of financialization of global capitalism. On every available indicator—asset values, activities, balance sheets, market capitalization, number of available financial products—global finance has risen to several multiples of global GDP in a relatively short span of time: approximately twenty to twenty-five years. Over that period the balance sheets of the banks of the G20 and the EU have ballooned to twice global GDP, shadow banking has risen to a
size equivalent to total global GDP, while the gross notional value of derivatives reached an unbelievable amount of twenty times global GDP before the crisis.

These staggering figures point, if anything, to what indeed can only be seen as the result of a truly ‘structural change’ in the modus operandi of contemporary capitalism. While I am indifferent to the kind of moniker that should precede capitalism to capture these features, financialization obviously has the best papers.

Of course this raises the question why Christophers has painted such an unsympathetic picture of what, on all accounts, is an influential strand of the financialization literature? But even more pressing is the question why Christophers did not perceive that his inability or unwillingness—I do not know what it is—to countenance these facts of global financialization would turn his own account—willy nilly—into a closet apology for a sector that nearly blew up the world.

While this is clearly not the spirit behind the book—the afterword (pages 278, 279) speaks in no uncertain terms about the need to move beyond our current “growth obsession” towards “a healthier global political economy” and the explicit acknowledgement that banks possess “destructive capacities”—the reader does come away from this book with a couple of uneasy messages. Part 1 tells the reader that financial openness is “natural”, part 2 that American financial imperialism was driven by idealism rather than naked geopolitical interests, and part 3 that critics of American financialization have been misled by their “aneamic geographical” imagination and that there is in essence nothing wrong with American capitalism. These are unconventional conclusions, to put it mildly, for an author who poses as a “radical (geographical) political econom[ist]” and spends fourteen pages discussing the work of the founding father of that tradition, Karl Marx.

Is it Christophers’s idealist ontology, which does not allow for a more cynical, revealing, bullshit-exposing reading of the accounts on which his stories draw and which Blyth (2012), for one, in his recent, blunt account of the crisis, flags as one of the most important functions of the social sciences? Given its a priori ontological reductionism—discourse is all that matters—idealism is unable to contrast discourse and outcome, talk and walk, sermon and practice, mouth and money: there simply is nothing outside of discourse to confront it with. As a result, idealist narratives have a tendency to end up being happy, saccharine-coated, comforting, slightly naive accounts of a world without unintended consequences, sleights of hand, politics by stealth, false consciousness, cynicism, and outright evilness. Such a sanitized world, I am afraid, is not the planet we inhabit.

Or is Christophers’s aggressiveness typical of the sort of intellectual anger geographers tend to betray towards those that are near to them but fail to take space seriously? The statement that “geography can explain much, if perhaps not all of the finance sector’s apparent ascendancy” seems to suggest as much.

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Is finance productive (and other important questions)? A response to Block, Blyth, and Engelen

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Commentaries by one’s peers invariably say a great deal about one’s book. Sometimes these commentaries can be relatively bland affairs. At other times they are the opposite: lively, animated, and engaged. The commentaries to which I have been invited to respond here fall into the latter category—they are robust and full-blooded responses all three (and, in one case, invested with a strongly pejorative tone). Typically in such a scenario the vigorous engagement with the text means one of two things. Either the text is fundamentally flawed and the commentators set about forcefully demonstrating this. Or it deals with important but complex and hence contestable questions, over which sensible analysts, all with comparable scholarly intent, can nonetheless come to different and sometimes contradictory conclusions. I would hope that my book—and the commentaries upon it—represents an instance of the latter, and this response is certainly written in this spirit.

Bounds and grounds of explanation and critique

One of the central such contestable questions raised by my book and the commentaries clearly concerns ideas and their significance. To wit: how material to the dynamics of historical—geographical change are ideas, and especially economic ideas? A core argument I tried to develop in Banking Across Boundaries is that they are very important indeed. I made this case with respect specifically to the idea of economic ‘productiveness’, and specifically in terms of its application to the activities and institutions of banking and finance.

I start this response by stating this because two of the three commentaries gathered together here address this issue head-on, albeit from very different sides. For Engelen, it is clear, I take ideas, or ‘discourse’, much too seriously, although it should be said right at the outset that his characterization of my approach in the book as one of “idealist ontology” (page 254) is, to say the least, curious and problematic (in the sense of being wholly inaccurate).(1) For Blyth, as one might expect of a scholar known for his interest in the politics of ideas, the opposite is true. My arguments about the importance of ideas are altogether too cautious for him. The “caveats and qualifications” with which I invest my major claims are deemed “unnecessary” (page 246). And this is especially the case in regard to questions of ideational “causation” (the notion that ideas, and their mobilization, might have been causative of certain material phenomena). “Get off the fence!”, Blyth seems to be imploring me; “make your claims and be confident in them!”

Unfortunately, this is just one of a series of serious misrepresentations that Engelen makes of the book in his commentary, for motives that I cannot and will not begin to guess at. I was minded simply to ignore these in this response but for two reasons decided I ultimately could not: first, because I cannot discount the risk that readers of Engelen’s commentary will take it as an accurate representation of the book’s arguments, which it is not; and, second, because Engelen’s evaluation of the book is not unconnected to his misrepresentations thereof. I will therefore indicate—at appropriate junctures—the most significant such misrepresentations, but I confine this corrective work strictly to the footnotes, leaving the main text to focus on the more interesting terrain of discussion of the book’s (and reviewers’) actual arguments. The first significant misrepresentation, then, is the accusation of “ontological reductionism—discourse is all that matters” (page 254). This has zero foundation. Much of the book, as I explain in the Introduction, is about the raw materialities of international banking and finance—whose history, as I emphasize, is “sculpted in part by economic discourses” (emphasis in original), “but the dynamics of which are clearly irreducible to those ideas” (emphasis added) (pages 11–12); indeed, chapter 2 argues explicitly and at length for the lack of ideational influence in the era covered there. Not only that, but the book is also in large part about how those historical—geographical materialities themselves shape, in turn, the ideas in question.
I am temperamentally sympathetic to Blyth’s criticism here; I do not deal well with perceived “fudge”, either. But the reasons for my caution are, I think, good ones. In particular, I would respond by saying the following: ideas are implicated in history in all sorts of different ways, and sometimes this implication is much more obvious—and much more obviously efficacious—than it is at others. To take a pointed example, there is Blyth’s own recent object of critique: the idea of austerity (Blyth, 2013). The power of this idea is, of course, plain for all to see. Not all (economic) ideas, however, are as transparently and palpably mobilized and put to work politically as the idea of austerity is. Does this mean, though, that ideas which circulate in different discursive milieu, and which shape politics and policy in much less overt and directly traceable ways, are necessarily less important? I would argue not.

Hence, in part, my reluctance to use the language of “causation” in relation to the idea of productiveness. The notion that banks are economically productive, and the active circulation of this belief in relevant political–economic fora, enabled—but did not cause—the internationalization of Western banking and finance in the latter part of the 20th century, particularly once such internationalization came to require multilaterally negotiated trade liberalization. Is this argument a “fudge”? Absolutely not. As I explain in the book, in making this particular claim my objective is not to refute the many existing accounts which emphasize causative nonideational factors. It is to add to them an enabling factor—the politics and power of ideas—that has conventionally been underplayed, and often totally neglected, in the literature on such internationalization.(2)

The second area in which Blyth admonishes me for my caution, meanwhile, concerns my critique in chapter 6 of a particular thesis concerning capitalism’s putative “financialization”.(3) This thesis, by now widely rehearsed, and associated in particular (though by no means solely) with the sociologist Greta Krippner (especially Krippner, 2005), is that contemporary capitalism, especially in its US-rooted and UK-rooted variants, has structurally financialized: it has undergone a deep-seated shift, whereby the balance of ‘the economy’ between financial and nonfinancial activities and income sources has tilted firmly and substantively in favour of the former. In chapter 6 I critically interrogate this thesis and argue that there are good grounds for questioning it.(4) Those grounds, however, are not sufficiently strong in my view

(2) As such, it is a misrepresentation to argue, as Engelen does, that according to my book “American financial imperialism was driven by idealism rather than naked geopolitical interests” (page 254) I go to great lengths in the book to explain that in making an argument about ideational “enablement” my hope, as noted above, was to “add merely an incremental and essentially complementary perspective” to the various existing explanations of the internationalization of Western finance and that it was therefore “not one of the book’s objectives explicitly to challenge these” (page 58). (Hence Blyth’s frustration with my perceived reticence.) Where ideas were mobilized to help prise open resistant international financial markets in the 20th century, the book insists, brute geopolitical forces and “overt ‘dollar diplomacy’ exercises” were absolutely not absent, but were instead “actively and explicitly layered” with the “objective economic ‘sense’ of open markets” (page 85).

(3) Engelen infers that, in questioning one particular reading of financialization, I question them all—“in a broad sense it is all about the very same rise of finance that Christophers himself addresses” (page 252); I offer a “diatribe against financialization as such” (page 252)—but again this is untrue. In the relevant section of the book (pages 232–233) I distinguish clearly and concisely between different theories of financialization, and I unambiguously restrict my critique to one such thesis and one alone. Circumscribing my critique accordingly, I say the following about the theories which lie beyond it: “I do not explicitly consider in this book these (and other) alternative understandings of financialization” (page 233).

(4) Engelen states that I fail to acknowledge critical differences between the UK and the US in developing my critique of ideas about financialization, and assume that conclusions related to the UK can simply “be extrapolated to the US”—thus committing a “capital crime” (page 253). This is untrue. “The picture for the US is not quite as clear”, I emphasize (page 259) at the outset of the section on the US (having analyzed the starker UK case), before spending several pages (259–264) detailing qualitative and quantitative differences between the two and critical specificities associated with the former.
to justify dismissing the thesis altogether (the data are just not good enough to allow certainty either way, is ultimately my conclusion), which leaves Blyth once again frustrated with my apparent fence sitting.

Given the popularity and influence of Krippner’s and others’ arguments about financialized capitalism, it is not necessarily surprising (though it was, to me at least, somewhat disappointing) that two of the commentaries on my book—Block’s and Engelen’s—should be largely preoccupied with my critique thereof, which takes up just 16 of the book’s 281 pages (pages 239–243 and 255–265). Financialization is a hot topic, for all sorts of analytical, conceptual and political reasons; and many scholars—including, as he acknowledges, Engelen himself—are heavily invested in it.

But despite that, and without wanting to appear evasive, I actually want to say relatively little about financialization here. The reasons are simple but also profound ones. Firstly, financialization—or the geographical–political–economic trends nominally captured with this label—is complicated, even if the label itself suggests something straightforward, clean, and transparent. In the space available for a response such as this, it is, in my view, literally impossible to deal systematically and adequately with the relevant issues, data, and interpretations without doing violence to them. I definitely do not think Block’s and Engelen’s objections to my argument are without merit; Block’s central observation regarding the different mechanisms of banks’ revenue and profit generation is especially pertinent, and my relative insensitivity to these in this particular part of the book is indeed ironic given that my discussion elsewhere of the treatment of banks in national accounts turns on exactly such distinctions. But nor do I think their counterarguments satisfactorily recognize or allow for the nuances and richness of my original critique—and I cannot hope to convey those here. It may sound trite but, to appreciate my critique, readers do need to read it in full.

Secondly, there is the issue of certainty, or finality, to which Blyth’s frustration pertains. Blyth wants finality and certainty (including from me). Block and Engelen think, on the question of financialization, that we already have it. But the whole point of my critique was to question whether we really do. (And what is at stake if we do not?) A brief renewed consideration of the matter here can scarcely deliver such finality; sometimes, even at the end of long books, there are open questions, and issues that cannot be tied off completely. My view remains that on the question of financialization—again, of financialization in one particular guise—there are meaningful grounds for scepticism and caution. Is this really so threatening? Krippner, significantly, appears to think not, arguing in her book on the modern political history of finance in the US that “enthusiasm for the concept of financialization has run far ahead of serious attempts to establish evidence for this phenomenon” (2011, page 23). This, in the end, was—and is—precisely my point.

Don’t look back in anger

Aside from his grievances with my questioning of one particular financialization thesis, Engelen’s principal other critique of arguments I made in the book concerns chapter 2, which, based largely on secondary literatures, deals with the geographical structures of international banking and finance in the pre-20th century era. (5) I make two main claims:

(5) That I document in chapter 4 a “full and painless [US] conversion to neoclassical economics” (page 251) in the early 20th century is—for the record—also erroneous. I do not. My specific argument, rather, is that the envisioning of banking as productive in the US national accounts of the 1930s and 1940s was entirely consistent with a generally positive perspective on banking in those two decades among leading (neoclassical) US economists, who, as JK Galbraith often later observed, blamed government more than bankers for the Great Crash and Great Depression. (Indeed I do not make any particular claims about the conversion to neoclassical economics—about its speed, completeness or the degree of pain involved—except to observe that it happened.) Notably, the accounts’ envisioning of banking as productive was also consistent with the views of the US Bretton Woods negotiator
first, that banking and finance prior to World War One were historically characterized by little of the national protectionism which engulfed the monetary and financial world for the following three decades; and second, that the generally open and integrated nature of international banking and finance in this period was not ideas driven—partly since such openness “lacked the need”, as Engelen notes of my thesis, “for a legitimating narrative” (page 251). (Quite how Engelen squares his recognition of my argument about the absence of discursive/ideational influence—or of the need thereof—with his blanket accusation of “idealist ontology” (page 254) is unclear.)

Engelen’s criticism here is forthright: this image of non-ideas-based openness is, he says, flawed. It contrasts with the picture of premodernity painted by the likes of Karl Polanyi, Eric Hobsbawm, E P Thompson, and Christopher Hill. But who among these four was an authority on—and wrote extensively about—international banking and finance? To the best of my knowledge, only Polanyi (finance), which is exactly the reason why I explicitly discuss—and dispute—his postulation of the classical gold standard as an ideationally driven achievement (page 86). Interestingly, Blyth offers a comparable critique to Engelen here, pointing to Hirschman’s *The Passions and the Interests* as counter evidence. But Hirschman’s book was not about configurations of international banking and finance, either; and my claims about the lack of need for or evidence of ideational buttressing hardly extend to pre-20th-century capitalist political–economic structures more broadly (which would be absurd).

So, to Engelen’s “pressing questions about the empirical justifications for Christophers’s view” (page 251), I can do no more than answer: the empirical justifications lie in the specialist literature that I consulted and cited—in the rich and rigorous work on the nature and determinants of financial openness in the era covered by chapter 2 by authors (the likes of Geoffrey Jones, Stefano Battilossi, Carlo Cipolla, Raymond De Roover, Larry Neal, and Charles Kindleberger) typically underappreciated outside of specialist financial history.

This matter of the geographical structures of international monetary and financial systems prior to the 20th century brings us to another important issue raised by Blyth’s commentary. If my account is too cautious and caveated, it also, Blyth suggests, extends too far back in time. The real meat of the book, in his view, is the stories it tells about financial ideas, financial materialities, and the reciprocal relationships between them from the 1930s onwards (chapters 3–6). Why, in part I, go further back in time, and especially why go back to debates about money and usury and to the geographies of money and finance in the Middle Ages and before?

There were, and are, three reasons for doing so, all of which, I think, bear highlighting. One—probably least compelling from Blyth’s point of view, I should imagine—is that I believe the arguments I make about the long period in question are, to one degree or another, original and important, and hence of interest per se, not least for historians (of finance and of economic ideas). Second, the book endeavours to contribute to a sizeable literature concerned specifically with the similarities and differences between patterns and drivers of ‘financial globalization’ in two different eras: that ending with World War One and that beginning shortly after the end of World War Two. Contributing fully to this literature means, naturally, addressing both eras. Third, and perhaps most importantly, I argue in the book that we cannot hope properly to understand modern notions of economic productiveness without going back in time to explore (as chapter 1 does) how, where, and why the ideational antecedents to those modern ideas emerged and evolved.

(5) continued.

Harry White; Block claims that my identification of White as a neoclassical makes no sense in the light of claims by Benn Steil and others that White was a Soviet agent (implying that the two are necessarily incommensurate), and yet Steil (2013, page 21) himself observes that White’s economics were considered “plain-vanilla”—which is to say, orthodox neoclassical.
Part of the reason for Blyth’s frustrations with the book, I think, is that he would have liked to see it venture an answer to a nominally simple question. Indeed, he poses this question in his commentary: “Is finance productive?” (page 250). I can understand Blyth hoping for an answer to this question in a book about banking, finance, and productiveness, but as I clarify in the introduction, that is not the question that the book sets out to answer. It asks (as well as several others) a very different question: how and why, when all reasonable evidence—at least evidence pertaining to recent years—points to the contrary, does the notion powerfully persist that finance can or might be productive? What discursive fields and calculative practices have historically been implicated in the materialization and durability of such a notion?

Blyth and (I think) Block, at least, seem to recognize the importance of this question and hence of my book. Engelen, sadly, does not appear to. Instead he excoriates the book effectively for what it is not: for not being another of the myriad existing accounts which expose the “bullshit”, “cynicism, and outright evilness” associated with contemporary banking and finance (page 254). But this angle of critique begs important questions. For one thing, does my book actually contest any of those characteristics of the financial sector? (Answer: no, of course it does not.) And does Engelen really believe that another book or article documenting those characteristics—least of all by me, with no public “voice” to speak of—will make any incremental difference politically or conceptually when the characteristics in question have already been repeatedly and fulsomely demonstrated in multiple media for anyone in the world who might care to pay attention to them?

Sadder still, however, is Engelen’s apparent anger at the very fact that I even sought to ask and answer a different question, and the bizarre (and actually quite insulting) conclusions he draws from my attempt to do so. My book ends up being, he says (page 254), “a closet apology for a sector that nearly blew up the world.” One of my messages to the reader, he writes, is that “there is in essence nothing wrong with American capitalism.” And, given that I am an apologist for the financial sector in particular and American (financialized) capitalism in general, it follows that Engelen is entitled to belittle me as “an author who poses as a ‘radical (geographical) political econom[ist]’.”

I am still not quite sure what to make of these “claims”. Indeed I am bewildered by where this critique, and its ‘edge’, are coming from, being frankly clueless as to how and where Engelen might have found evidence in the book for arriving at the conclusions he does. For, and here is the nub, Engelen does not tell us why—on what basis—he deems my account an apologetic one. It simply is. Perhaps it was the picture on the cover? Perhaps the alliteration in the title (in which case Blyth’s in trouble, too)? This is to be flippant, of course, but the flippancy has a point: Engelen does not say. We are none the wiser. But if, as I suspect, it is simply because my book takes ideas (too) seriously, and considers their mobilization to be intimately bound up with questions of power, then Engelen’s conclusions would imply that we really have not learned anything in the last forty years, including from the work of his fellow commentators (eg, Block, 1990; Blyth, 2002; 2003; 2013).

(6) This is something of a misrepresentation, too, for what it is worth. The argument I make in the book is that radical geographical political economy—a mainstay of the journal Antipode, and of the Antipode Book Series (http://eu.wiley.com/WileyCDA/Section/id-324286.html) in which my book appeared—has conventionally paid relatively little attention to the power of ideas, focusing instead on historical–geographical materialist approaches, and that it would benefit from taking ideas more seriously and integrating a sensitivity to such ideas with—not, needless to say, substituting this sensitivity for—its traditional materialist impulses. “Radical (geographical) political economy,” I therefore posited (in the sentence reversioned by Engelen), must “be constantly, critically attuned to the dominant economic ideas and theories that format our social totality” (page 280). This was me “posing”.


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