Critical Dialogue

Ruling Ideas: How Global Neoliberalism Goes Local. By Cornel Ban. New York: Oxford University Press, 2016. 314p. $105.00 cloth, $33.95 paper.
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— Stephen C. Nelson, *Northwestern University*

In 1977, James C. Scott wrote a two-part essay on the "little traditions" followed by peasants who adopted formal religious identities but whose on-the-ground beliefs and practices often diverged from the strict orthodoxies of the "great traditions" promoted by central religious authorities ("Protest and Profanation," *Theory & Society*, 4(1), 1977: 1–38). Scott observed that doctrines, be they religious or otherwise, "are elaborated at the urban centers of civilization by intellectual 'high priests' and then propagated to the countryside. In the course of propagation they undergo a . . . transformation; some themes are lost, new ones are added, symbols acquire new meanings and are put to new purposes" ("Protest," p. 2).

Starting a review of *Ruling Ideas* with snatches from Scott's work may seem odd; after all, Cornel Ban's book is about neoliberal ideas and policy regimes, not about the spread of religious traditions. But Scott and Ban share a common interest: They want to better understand the conditions shaping how local "translators" interpret, refashion, and operationalize the "scripts" that missionaries from the powerful core want to spread. And in this deeply researched and richly detailed book, Ban breathes some new life into a long-standing research tradition centered on the question of how "foreign" models are transplanted into local settings.

Ban's analytical framework, laid out in the first chapter, seeks to explain why and how "edits" (p. 3) applied by local interlocutors to global neoliberal economic policy scripts yielded different types of policy regimes in two countries, Spain and Romania. To do so, Ban reconceptualizes an essentially contested term—"neoliberalism"—in order to make the case that local "edits" need not be treated as "anti-neoliberal." Rather, neoliberalism is conceptualized here as an internally heterogeneous, "temporally contingent," flexibly applied, and open system of economic thought, rather than the "seamless and time-invariant construct" that, Ban argues, characterizes how many other political economists have treated the concept (p. 11). What this alternative conceptualization does is expand the range of economic life that "neoliberal" thinking addresses. In this more capacious version, it is not just that neoliberal theorizing and policy advice seeks to make macroeconomic management and foreign-oriented economic policies more market conforming; neoliberalism is extended to touch on a range of other things, such as "supply-side" policies affecting firms' strategies and income-redistribution efforts. The boundary of neoliberalism is found where policy agendas breach any of three "fundamental goals" of neoliberalism: openness to international trade and financial flows; relying on perceptions of "financial market credibility" as a bellwether for loosening or tightening public finances; and basing growth strategies on the concept of "competitiveness" (p 10). Provided that these barriers are not overrun, "local translators" can splice in elements that do not fit well with the purer "source" neoliberal doctrine to create hybrid forms that remain in the neoliberal mode.

Rather than describe a wide range of hybrid forms, Ban lands on two different types: the "embedded" and "disembedded" varieties of neoliberal policy regimes. The edits to the neoliberal script under the embedded type—exemplified by the Spanish case—are meant to "maximize the policy space for downward redistribution of income and opportunities to compensate society against market dislocation," using things like taxation, health, and active labor market policies (p. 14). Under disembedded neoliberalism, by contrast, upward redistribution of income produced by market forces is accelerated, not counteracted, by the prevailing policy regime. Romania is the exemplar of the disembedded type.

To explain why these countries diverged in the type of neoliberal policy regime they developed, Ban points to four factors. First is the country's "institutional past," conceptualized as features (including a country's regime type and "geopolitical position," p. 20) that either facilitated or inhibited the circulation of neoclassical economic ideas within ecologies linked to policymaking. Second is the degree to which local translators depend on the symbolic and material support of the global hubs for neoliberal economic ideas. Third, the "cohesiveness" of the policymaking teams at the helm of the economy influences the character of the policy regime. Finally, Ban points to the vulnerability of countries to coercive pressure from outside actors (such as the International Monetary Fund)

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that hew to purer forms of neoliberalism as a factor that can lead to the "pruning" of "local hybrids" (p. 28).

To test these arguments, Ban compares the evolution of economic policy regimes in Spain and Romania, focusing most of the narrative on the post-1980s period (though due attention is given in the book to the much earlier developments in each country that shaped the policy regimes that came later). The granular reconstruction of how policymaking evolved in these two cases is impressive—although at times, the barrage of names and affiliations is so thick that it becomes difficult to keep track of who is who and what role they are playing in the story. Since I am not an expert on either of these cases, I had no strong reason to seriously doubt the author's interpretation of the key forces that drove each country's adaptation of "core" neoliberal economic beliefs into workable policy agendas.

I remain less than fully convinced, however, by Ban's answer to one question: How much were the policies that "disembedded" markets in Romania a product of structural forces (namely, the pressure to attract outside investment and the leverage of the international financial institutions over the country) that had dramatically narrowed the country's policy space? Ban argues that this interpretation is insufficient because the Romanian neoliberal regime went well *beyond* what outsiders demanded; the main illustration of this claim is the adoption of a flat tax on personal income and corporate profits in January 2005. But is it so surprising that a fringe idea might hold appeal for officials in one of the poorer countries in the region that wants to attract foreign investment and retain its remaining pool of skilled workers? The author argues that "it takes a radical neoliberal view of state-society relations and of what states can do to attract investment in order to take such a bold step" (p. 90). Coming around to that view seems likelier when peers (all of which are competing for capital and workers as barriers to mobility fall) take the same bold step (and it is worth noting that flat-tax schemes were implemented in Estonia and the other Baltics, Russia, Serbia, Ukraine, Slovakia, and Georgia *before* Romania passed its own tax reform).

The primary issues I raise in this review are not about interpreting the evidence in the book's case studies, however. The first issue I want to engage concerns the formulation of the theoretical framework that undergirds the analysis. Ban describes four factors that influence how global neoliberal scripts are "edited" by local translators. One variable-based way to grasp the argument is that if all four factors take certain values ("high" historical levels of openness, moderate dependence on neoliberal-oriented transnational networks, high levels of institutional cohesion, relatively high capacity to resist coercion from external forces), we end up with the "embedded" type; if the variables take the opposite values, "disembedded" neoliberal policy regimes are the more likely outcome. If there are only two ways in which these factors can be configured, then there is no need to theorize further; one kind of configuration (high openness, moderate dependence, high cohesion, low vulnerability) feeds into one type (embedded), and the other kind (low, high, low, high) generates the other type of policy regime (disembedded). But there is nothing in the theoretical framework that suggests that these are the *only* possible configurations. I craved more extensive theorizing of what other kinds of policy regimes might be produced by these alternative possible configurations—and how we could go about testing for these alternatives.

This brings me to another question: How should the arguments in this book be evaluated? Ban justifies the research design, which is a paired comparison of the Spanish and Romanian cases, on several grounds (pp. 28–30). He briefly notes that the "scope of the book" extends to the "semiperipheral and postauthoritarian European contexts." I could not uncover the rationale for these scope conditions; the arguments certainly seem to me to apply more widely. And even if the arguments are limited only to postauthoritarian, semiperipheral states (with the first condition being conceptually clearer to me than the second), I would have more confidence in the external validity of the typology if more cases than just Spain and Romania were examined—even if those additional cases were not investigated with the depth of knowledge that Ban brings to his primary cases.

The case selection rationale in *Ruling Ideas* does not match either the "most similar" (two cases matched very closely on the key observable explanatory factors but that vary on outcomes) or "most different" (comparing cases that vary widely on key independent variables but look similar on the outcome of interest) logics. Spain and Romania diverge in many ways—not least of which is their relative levels of economic development. (According to data from the IMF's World Economic Outlook, in 2016 GDP per capita in Spain was 186% higher than in Romania—a difference that shrank from a near 800% gap observed in 2000.) Looking at carefully chosen "out of sample" cases to see how well or poorly the argument travels is one way to establish the external validity of the arguments. Testing for external validity is important because we want to ensure that the theoretical typology and the explanation for observed variation in types applies to more than just two cases; in extending Ban's novel framework, we want to have a sense of the *representativeness* of the Spanish and Romanian cases, and I think that the book is lacking on this front.

Dorothee Bohle and Bela Greskovits's earlier work ("Neoliberalism, Embedded Liberalism and Neocorporatism," *West European Politics*, 30(3), 2007) seems to suggest a natural set of additional comparative cases: Their description of the Baltics bears a striking resemblance to the "disembedded" type, and they even apply the

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"embedded neoliberalism" descriptor to the "Visegrád" countries in central Europe. Going beyond the two cases would have also addressed concerns about what Andrew Bennett ("A Lakatosian Reading of Lakatos," in Colin Elman and Miriam Fendius Elman, eds., *Progress in International Relations Theory*, 2003) calls "use novelty" standards of theorizing, by which he means the ability of theories to explain new cases that are independent from the cases that may have influenced the construction of the theory.

These concerns notwithstanding, thanks to Ban's innovative theorizing and extensive empirical research in two interesting cases, we now have new directions to pursue when it comes to the question of how global neoliberal ideas have transformed national policy regimes.

Response to Stephen C. Nelson's review of *Ruling Ideas: How Global Neoliberalism Goes Local*

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— Cornel Ban

I am grateful to Stephen C. Nelson for his kind remarks and a healthy debate, and I have nothing to add to his succinct presentation of the empirical contributions of the book. My current thinking on the global spread of forms of technocratic knowledge was stimulated by his elegant synthesis of the analytical apparatus that *Ruling Ideas* advocates. As an interdisciplinary political economist, I was particularly pleased to read a review that places one's book in the august context of James C. Scott's work on social translation. Most of all, however, I welcome Nelson's call on scholars to use this book's analytical framework to think of other, less linear configurations of neoliberalism than the embedded—disembedded spectrum captured by the book's neo-Polanyian scope.

Nelson is an experienced comparativist with superb knowledge of Latin America, and I was delighted to read that in his view the book's account of varieties of neoliberalism may travel to contexts wider than semiperipheral and postauthoritarian European contexts. The conservatism of my statement about scope was perhaps excessive. At least, by covering the post-2008 crisis in Spain and Romania the analysis certainly was no longer solely about postauthoritarian states but also about ordinary European democracies. I was only half convinced, however, by Nelson's suggestion that not using the "most similar" or "most different" case section logics is a problem for the book. The introduction makes it clear that these cases were selected by using a different strategy (critical or least/most likely cases), and I found it beyond the book's immediate concerns to restate the terms of the debate between the most similar/different camp and the least/less likely one. Nelson is right that the analysis could benefit from adding more cases, but given the granular and context-sensitive research entailed by the sociological analysis of the economics profession and the state, such an expansion would have been unwise in a book-length argument.

Regarding the empirics, I was stimulated by Nelson's observation that disembedded neoliberalism may have been triggered by the imperative to retain the country's remaining pool of skilled workers. This imperative was certainly key in the intriguing class politics of the Romanian state but was marginal in the political economy of adopting radical neoliberal reforms, such as flat taxes benefiting the very top of the income distribution, privatized pension systems offering diminished pension returns, or labor regulations that international organizations judged to be abusive. Indeed, students of skilled Romanian migration would be hard-pressed to find progressive taxation, public pensions, or progressive labor regulations as drivers of the migratory phenomenon, yet they would find more support for the more embedded forms of neoliberalism from the countries where they emigrated.

Nelson wonders if the radicalization of Romanian neoliberalism was not driven by Romania's poverty and its government's race to attract foreign direct investment (FDI). I agree with Nelson that radicalizing neoliberal ideas and policy regimes is a form of signaling to foreign investors. But I am wary about the logic behind it. Development studies scholarship is clear that disembedded neoliberalism is just one of the many signals that developing countries can use and that, indeed, some of the most successful developing countries attracted FDI with neodevelopmental states whose ideational foundations were at odds with neoliberalism. Moreover, the book's comparative historical method brings to the fore the fact that after 1990, the need to signal to foreign investors was a constant. What changed was the dominant policy regime, which went from neodevelopmentalism in the early 1990s to disembedded neoliberalism from the late 1990s onwards. To top it off, given the structural and macroeconomic ills of the Romanian economy during the early 1990s, FDI was, in fact, most needed when neoliberalism was utterly rejected by the ruling elites of the time. Indeed, therein lies the value added of taking varieties of neoliberal ideas seriously across time, not just across space, and having a clear sense of how different forms of professional, economic, and institutional capital shape the politics that explain that variation in the first place.

The Currency of Confidence: How Economic Beliefs Shape the IMF's Relationship with Its Borrowers. By Stephen C. Nelson. Ithaca, NY: Cornell University Press, 2017. 248p. $39.95 cloth.

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— Cornel Ban, *City University of London*

Stephen C. Nelson has written a splendid book. Indeed, it is the most sophisticated account to date of how economic

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ideas and their policy implications shape the relationships between international economic organizations, such as the International Monetary Fund, and their borrowers. While this is not the first account of how a specific brand of economic ideas popularly associated with neoliberalism is woven into the organizational culture of the Fund, *The Currency of Confidence* is the first book that embeds those institutionalized ideas into concrete causal mechanisms about the treatment of borrower governments. In the book, these mechanisms are shown to shape variation on three discrete dimensions: access to credit, comprehensiveness of policy conditionality, and the rigor deployed in enforcing the loan conditions in question.

In my view, Nelson deploys very compelling evidence that material interest and power are *not* the only essential features of IMF–borrower relations, as much of the conventional wisdom has it. Nor are institutional pathologies the definitive constructivist concept regarding the lives of international organizations (IO)s. Instead, he shows that the closer the degree of fit between the economic ideas of IMF mission staff, executive directors or managing directors, and those of the officials of the borrower countries, the more likely that the country in question will get fewer conditions and less pressure to enforce those conditions. Furthermore, while many have conjectured in the past that the Fund changes the political outcomes of the borrowing countries, Nelson is the first scholar to demonstrate how this happens in concrete and very systematic empirical terms.

The book is exemplary in the fine assembly of quantitative and qualitative methods, and generations of graduate students should use it as the gold standard in mixed methods. Nelson marshals several original databases drawn from IMF programs from the 1980s and 1990s, refined and dynamic quantitative analyses of those data sets, *and* a granular comparative historical excursus of Argentina's experience with the IMF between 1976 until 2001. The book is written with verve (a rare asset in mixed-method books) and exhibits a high sensitivity to local detail. Last but not least, the work is replete with smoking guns. IMF chief economists stating openly that policymakers operate under Keynesian uncertainty (p. 23) are at the top of my list of constructivist favorites. In brief, *The Currency of Confidence* stands to be a new classic on the shelf of the literature on IO–borrower relations.

That said, the book has two problems whose persistence reflects the lack of systematic interdisciplinarity in political economy in general. The first problem concerns the core concept of "neoliberalism," while the second has to do with the standards of proof to be used in demonstrating that someone's professional experiences can be reliably coded as proxies of neoliberal views.

Specifically, while the author's core argument is sound, it has a serious problem of conceptualization that reflects some of the field's faulty understanding of neoliberalism as a body of economic ideas. For Nelson, the bundle of theoretical principles posited as constituting neoliberalism has four main pillars: Economies are best represented through formal modeling showing how markets settle in equilibrium; competitive markets are efficient on average; free trade is welfare improving; and economic agents have rational beliefs (pp. 8–9).

While these principles sound familiar to every political economist schooled in the international political economy canon, they would be puzzling to historians of economic ideas, a scholarly community that political economists could benefit from engaging with more. Rather than serving as an exclusive marker of neoliberalism, the use of complex formal models seeking equilibria was in fact the methodological brainchild of postwar neo-Keynesianism, a known synthesis of neoclassical and Keynesian ideas encased in a fascination with mathematizing economics. Indeed, midcentury neo-Keynesians were challenged from both the right (Austrian School, Chicago School heavyweights) and the left (post-Keynesians) for their econometric hubris. Could it be, then, that what the Fund recognized as familiar in Argentina and other borrowing country elites—assumed by the book to be intellectually neoliberal—was not shared ideas but shared methodologies and tools to interpret the world? Could it be that it was this shared formal modeling training, along with shared vocabularies and calculative devices—rather than economic ideas—that gave the IMF the confidence that the technocrats they worked with "did the right thing"? In other words, perhaps it was tools, not the ideas, that did the trick.

Similarly, the proposition that liberalized trade is welfare improving was, with some qualifications, the very cornerstone of postwar neo-Keynesianism. Likewise, the Bretton Woods system was set in place in part to prevent a return to competitive protectionism. Neo-Keynesians had been almost as hostile to structuralist trade theories as neoliberals were, and the latter only added a difference of degree, as they pleaded for trade freed from *all* restrictions. Furthermore, while rational expectations are undoubtedly the exclusive identity marker of neoliberalism, they should not be conflated with rational beliefs, an early neoclassical postulate that the neo-Keynesian models used as much as their neoclassical predecessors. This last point brings to the fore Nelson's problematic tendency to conflate neoclassical economics and neoliberalism. While neoclassical economics lent itself to extremely diverse hybridizations, with bundles of economic ideas that need not even be supportive of capitalism (remember market socialism), as a school of economic thought, neoliberalism is a much more specific, narrower, and harder to hybridize historical formation that is steeped in the New Classical (counter)revolution in economics during the 1970s.

As political economists, we could all benefit from reading more history of economic thought, and even a book as fine as Nelson's suffers from the consequences of not doing so.

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These problems are not just semantic. The loose use of the term "neoliberalism" generates serious empirical misdiagnoses and puzzling policy implications. For example, the qualitative analysis of Argentina carried out in this book is weakened by the implicit assumption that the world of economic ideas in the 1970s and 1980s was such that whoever was not a structuralist was a neoliberal (Chapters 4 and 5). This is not entirely accurate. Structuralist economics was also contested by neo-Keynesians, and if Keynes had been alive it is likely that he would not have liked what the Economic Commission of Latin America had to say.

Arguably, the structuralist–neoliberal dichotomy applied to the 1960s and 1970s is the reflection of a related weakness of the book: the assumption that work experience in the Bretton Woods institutions and/or training in a highly ranked U.S. economics department or business school at any time is a reliable proxy for neoliberalism (pp. 58–59). In making this assumption, Nelson draws on a time-honored, yet ahistorical, sociologically thin, and empirically flawed strand of literature in political economy.

First, many students of the IMF would object that the Fund operated on an orthodox brand of neo-Keynesianism until the mid-1970s, and so working there in, say, the late 1960s was hardly indicative of neoliberal proclivities proper. Second, this is even truer of the World Bank, which was (and still is to some extent) an extremely diverse set of professional ecologies, particularly until the early 1980s. Third and most importantly, however, getting a degree anywhere in elite U.S. institutions outside of Chicago or Carnegie Mellon during the 1960s did not make one a neoliberal. Until well into the late 1970s, the American economics professoriate was solidly neo-Keynesian, and the neoliberals who did not make it to the Chicago safe haven were trying to eke out a living in maverick business schools and less prestigious places. Moreover, MIT was a neo-Keynesian stronghold up until the cusp of the 1980s. In my study of Spanish economics, I found that at the height of the Keynesian era at the London School of Economics, Spanish graduate students did their Ph.D.s with Lionel Robbins, a diehard anti-Keynesian, and so it all really depended on who one's advisor was.

At a minimum, for Nelson's book this means that an unknown number of borrowing country officials identified as having a U.S. economics degree before the 1980s (to be on the safe side) may have been wrongfully coded. This does not mean that many of the economists that Nelson identified as neoliberals were not neoliberals by the 1980s or 1990s. Certainly, many of the invoked names were. Yet what it does mean is that they may have been neoliberal for other reasons than the core variable of the book (socialization experience during graduate school). Indeed, it would be utterly sensible to argue that resocialization in new institutional settings, career incentives, hypocrisy traps and other events can plausibly make one lose beliefs from decades ago acquired in graduate school.

Nelson admits that graduate school and working experience in Bretton Woods is a crude indicator (p. 61) and goes on using it in the quantitative model. Given the large number of coded individuals, extensive calibration would have been unrealistic, perhaps. Yet at a minimum, it would not be inapt to use the history of economic ideas scholarship, sociological accounts of the discipline, and interviews with major figures in economics (both are enormously useful in my experience) to code the intellectual profile of the relatively limited number of U.S. elite economics departments that matter for Nelson's story, and thus anchor the quantitative analysis in less ahistorical microfoundations. Coding two to three publications of the graduate school advisors around the time of graduation would not be excessively daunting for a representative sample either. Such alternative approaches are time consuming, but the same blunt indicator (elite U.S. training in economics = neoliberal beliefs) often persists in the qualitative case studies. Harvard, MIT, and Columbia are presumed to be incubators of neoliberalism during the late 1950s (p. 95), a contestable proposition. In the same case studies, the proposed neoliberal metric (graduate training in elite U.S. economics department) is inconsistently applied. Examples of those automatically considered to be neoliberal include, for example, an ambassador to Washington, DC, in the early 1960s, at the height of Keynesianism (p. 103), having a Harvard business degree from 1943, the apex of war economy Keynesianism (p. 105), serving as IMF executive director during the late '50s (p. 120).

As for the policy recommendation that follows (the IMF should hire more economists with non-U.S. graduate degrees in order to ward off neoliberal bias), we should be skeptical. Recent research on the training of European and Latin American economists suggests that these non-U.S. graduates outdo their American counterparts in terms of this bias. As Marion Fourcade concluded in her *Economists and Societies* (2010) opus, even French economics training has become "Americanized."

All great books have flaws, and if Imre Lakatos was on the money, the point of progressive scientific work is to advance the state of the art through a conversation about the flaws. A superb book, *The Currency of Confidence* is no exception. Indeed, it is an excellent baseline from which to start honing and fine-tuning the conceptual and methodological tools with which we toil.

Response to Cornel Ban's review of *The Currency of Confidence: How Economic Beliefs Shape the IMF's Relationship with Its Borrowers*

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— Stephen C. Nelson

I thank Cornel Ban for his generous and thoughtful review of *The Currency of Confidence*. Time is the most

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valuable currency that scholars possess, and I am grateful for his willingness to spend some of his time on my book.

Both of the primary criticisms offered by Ban relate to the concept of "neoliberalism." My conceptualization of "neoliberal" economic beliefs rests on four pillars, which, as he notes, should "sound familiar to every political economist schooled in the IPE canon." But in Ban's view, this conceptualization is problematic because it is at once too restrictive to be able to accommodate the offshoots that we might want to call "neoliberal" but which would dispute one or more of the elements in my conceptualization (e.g., economists from the Austrian school who disliked the formalization of orthodox economic theories)—*and* it is insufficiently restrictive to be able to exclude "neo-Keynesians" who are not neoliberals but might share an affinity with one or more of the pillars. The solution to these conceptual deficiencies is to take a deeper dive into the history of economic ideas.

Far be it for me to disagree with the notion that we would all benefit from reading in the history of economic thought! But we need to be very clear about the purposes that these efforts to further refine our conceptualization of neoliberal will serve. If the purpose is making an ever-more-refined taxonomy of the different branches growing out of the tree trunk of neoliberalism so that we can more accurately sort economic officials into different categories, then this is crucial. If, however, we have a different purpose in mind—for example, trying to develop a measure of ideational orientation for lots of policy teams in lots of countries over lots of years—then working to come up with the "right" definition of neoliberal might not be the best step.

With a concept like "neoliberal," there is bound to be some fuzziness; as Nancy Cartwright and Rosa Runhardt ("Measurement," in Nancy Cartwright and Eleanora Montuschi, eds., *Philosophy of Social Science*, 2014) point out, socially constructed concepts do not have definite boundaries. But if we are interested in measurement, we need procedures that can capture the "family resemblance" between the units we want to categorize for the purposes of testing an argument. I develop criteria and a procedure to assign policymakers to categories (neoliberal or not) so that I can explain variation in IMF program design and enforcement.

Ban praises the "very compelling" multimethod evidence in the book but then suggests that different procedures might have changed the results. (It is hard to tell whether he thinks these other criteria would sharpen the findings or overturn them.) Changing the criteria to restrict the boundaries around the neoliberal measurement creates more problems than it solves, though. Take the periodization issue. Ban suggests that it is only in the late-1970s that "neo-Keynesian" beliefs were pushed out of the "incubators" for neoliberal beliefs. But from the 1950s onward, the design of the Fund's lending programs was based on the "financial programming" model, which was built on classical Humean—not Keynesian—foundations. (In fact, Robert Mundell took to calling it the "Hume-Polak" model after the IMF economist who originated the Fund's monetary model.)

Likewise, Ban suggests that neo-Keynesian beliefs (the content of which is not defined in the review or in *Ruling Ideas*) dominated elite American economics departments "well into the late 1970s" (except, that is, for some departments like Chicago and Carnegie Mellon). These kinds of caveats make systematic measurement difficult. When can the neoliberal takeover of mainstream economics be identified? How can you account for the unevenness of the takeover? And why does Ban hone in on the neo-Keynesian consensus here but suggest in *Ruling Ideas* that the "neoclassical-Keynesian synthesis" was in fact the "dominant postwar school of thought in the Euro-Atlantic area," and that this synthesis rested on "neoclassical bedrock" (p. 21)?

Ban ends his review on the question of progress, and I will end my reply on the same note. Is progress when we have reached complete agreement in answer to the question "Are policymakers X, Y, and Z neoliberals?" I doubt it. There is no transcendentally right or wrong characterization of a social construct like "neoliberal." In the pragmatist vein, I think progress is likelier to be marked by the number of important research puzzles that we have come closer to solving using measures of the concept that are well suited for different purposes.

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