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Document Version
Accepted author manuscript

Published in:
Review of International Political Economy

DOI:
10.1080/09692290.2021.1997786

Publication date:
2023

License
Unspecified

Citation for published version (APA):
Helgadóttir, O. (2023). How to Make a Super-model: Professional Incentives and the Birth of Contemporary Macroeconomics . *Review of International Political Economy*, *30*(1), 252-280. https://doi.org/10.1080/09692290.2021.1997786

How to Make a Super Model:

Professional Incentives and the Birth of Contemporary Macroeconomics

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Acknowledgements:

My warmest thanks to Cornel Ban, Mark Blyth, Alex Kentikelenis, Alex Gourevitch, James Morone, Leonard Seabrooke, Eleni Tsingou, Jacob Hasselbalch, Cristoph Ellersgaard, Morten Ougaard, Stine Haakonsson, Lasse Folke Henriksen, Anton Grau Larsen, Duncan Wigan, Saila Stausholm, Rasmus Christensen, Stefano Fenoaltea, Jacqueline Best, Ben Rosamond, Ben Clift, Antoine Vauchez, Scott James, Vivien Schmidt and the entire Political Economy Group at Copenhagen Business School for reading and critiquing this article at various stages of development. All mistakes are my own.

Abstract:

Reinterpreting the rise of contemporary macroeconomics, this article argues that what mattered most in the shift away from postwar Keynesianism was a new form of modeling

called Real Business Cycles (RBC)—variations of which still dominate mainstream macroeconomics. But how could this form of modeling, championed by a handful of junior economists, affect a disciplinary coup in the face of strong opposition from Keynesian disciplinary powerbrokers? Content analysis, network mapping and deep reading of 197 articles by 331 authors suggest that RBC had a competitive edge as a tool of *individual professional advancement*, allowing it to rewire pre-existing networks of expertise in the face of strong opposition. Elaborating on the interdependence between individual professional appeal and the rise of new forms of formalized expertise, this article identifies three facets of RBC that made it a 'super-model', enabling its improbable takeover of macroeconomics: the ability to bond together a set of disparate ideas into a simple and workable whole ('glue'), deflect known criticism ('rubber'), and incorporate modified assumptions ('putty').

"I'm not sure whether you will take this as a confession or a boast, but we are basically story-tellers, creators of make-believe economic systems."

-Robert E. Lucas (2011, p. 1)

INTRODUCTION

Models rule great swathes of the social world (Fourcade and Healy 2017; Heimberger et al. 2020). But what dictates *which* models do so? This article offers a novel interpretation of the birth of contemporary macroeconomics, arguing that *individual professional incentives* were conducive to the rise of a new class of macroeconomic models that fundamentally rewired the discipline in the 1980s and 1990s. Economists' models, in other words, are of interest not just because they influence policy, make markets, and shape and constrain the questions that we can ask and the answers that we can offer (Henriksen 2013; Braun 2014; Fourcade 2009; MacKenzie 2006; MacKenzie et al. 2008) but also because they have variable appeal to individual practitioners that face quotidian professional constraints and demands in their working lives (Lamont 2009; Bourdieu 1988). In order to understand the rise of the currently dominant New Neoclassical approach to macroeconomics we must therefore open up the black box of New Neoclassical models and examine the ways in which they interact with professional incentives.

Understanding this matters because the rapid uptake of new models in the late 20th century quickly moved the goalposts of what was considered professionally acceptable scientific practice in the field. It marked the demise of neo-Keynesian postwar macroeconomics, which came to be seen as unscientific, and laid the groundwork for the 'technical neoliberalism' that dominates the discipline to this day (Stahl 2020). This ideational shift, in turn, drastically changed what was considered sound and informed policy, limiting policymakers' space for maneuver (Blyth 2002). These models, in other words, provided many of the 'rules of the game' that have dictated political life for the last several decades.

This is also a particularly timely examination as we are witnessing a number of macroeconomic shibboleths melting away in the crucible of policy exigency. Does that mean that we are in for another paradigm shift in academic macroeconomics? This article suggests that absent the rise of a new and user-friendly form of macro-modeling, we shouldn't hold our breath. In the meantime, the gap between academic macroeconomics and the demands of statecraft will yawn wider, creating an opening for atheoretical policy experimentation as the influence of academic macroeconomics diminishes.

The *ur*-model of contemporary mainstream macroeconomics, invented by Edward Prescott and Finn Kydland in the early 1980s, is called a Real Business Cycles (RBC) model. Through a variety of professional compromises and internal adjustments RBC evolved into Dynamic Stochastic General Equilibrium (DSGE) modeling, the workhorse device of the field to this day.[1] While this has received little more than passing attention in

[1] Or as Viner and Wills note, '[t]he [benchmark DSGE] model can be used to study the effects of technology shocks of the kind studied by growth theorists and real business-cycle (RBC) theorists' (2018, p. 15). Similarly, it was recently noted in a flagship journal discussing the prevailing macro models on the eve of the 2008 crisis: 'It is crucial to understand that these second-generation real business cycle models are built on

the voluminous political economy scholarship on the birth of neoliberalism (see Ban 2016 for an overview) and the economics profession (see Hirschman and Berman 2014 for an overview), the same is not true amongst economists: when Prescott and Kydland were awarded the Nobel Memorial Prize in Economic Sciences in 2004 for their groundbreaking work on RBC modeling,[2] the Nobel committee described RBC as no less than the first 'alternative to the Keynesian paradigm' in macroeconomic analysis.[3]

The two approaches to modeling the macroeconomy were fundamentally different: where neo-Keynesian macroeconomic models were large and complicated—acting like descriptions of the key components of the actually existing economy—RBC models operated like mathematical synecdoche, modeling the behavior of 'representative individuals' and then scaling up such insights and applying them to the economy as a whole.

The policy conclusions of RBC modeling were also a strong contrast to the Keynesian understanding of booms and busts: RBC concluded that economic downturns were *not* market failure that called for governmental intervention, but rather the collective outcome of optimal individual reactions to external shocks. Consequently, the model's policy implications were strictly anti-interventionist: by its logic, interference could only undermine rational individual responses, unnecessarily prolonging a crisis. In this way,

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the first-generation models, not on the Keynesian IS–LM model. That is, the New Keynesian models are simply real business cycle models with a few frictions added on. Thus, although it may be surprising to non-macroeconomists, a canonical real business cycle model, augmented with money and flexible prices so that monetary policy can be meaningfully discussed, has essentially the same implications for the fraction of business cycle fluctuations explained by various shocks and, perhaps more surprisingly, the same implications for policy as a canonical New Keynesian model (…) A typical New Keynesian model adds several frictions and shocks, but at its core, the key driving force for business cycles is a real business cycle model" (Kehoe et al. 2018; see also Nelson and Plosser 1982; Rebelo 2005).

[2] They were also recognized for their work on time inconsistency dilemmas.

[3] https://www.nobelprize.org/uploads/2018/06/advanced-economicsciences2004.pdf accessed on May 1, 2020.

RBC lent new scientific legitimacy to the *laissez faire* policy prescriptions of the pre-Keynesian world. Over time, various internal changes and additions to this model—or the turn to DSGE—carved out space to justify both monetary and fiscal counter-cyclical policy interventions (Vines and Wills 2018). Crucially, however, getting to this point, which was the *starting point* of the IS-LM models that were at the heart of postwar neo-Keynesianism, took decades of intellectual innovation and academic labor.

But what explains the rapid shift to a fundamentally different way of modeling the macro-economy in the 1980s? RBC's spectacular success was by no means preordained. To the contrary, when Prescott and Kydland began working on it an outside observer with an eye to common markers of influence would have been justified in concluding that the odds were stacked decisively against them: neo-Keynesian macroeconomics was still deeply entrenched in most high-ranking universities and embraced by a dense network of disciplinary powerbrokers and policymakers (Colander 1989, 2012). At the time, neo-Keynesian IS-LM models were also widely perceived to be good approximations of the macro-economy and essential tools for short- to medium-term forecasting in settings like the IMF and central banks (de Vroey 2016; Laidler 1999). With the notable exceptions of Hayek and Friedman, all Nobel Prizes for macroeconomic work had until this point gone either to neo-Keynesians such as Samuelson, Klein and Tobin or heterodox thinkers like Leontief, Myrdal and Kontorovich.

Although influential actors in the political sphere were already in pursuit of more market-oriented and anti-interventionist approaches to macroeconomic knowledge production (Blyth 2002; Ban 2016; Widmaier 2016; Mirowski and Plehwe 2009), Prescott and Kydland did not seem particularly likely contenders to provide it. At the time they

were relatively junior scholars at the respected but not top-tier Carnegie Mellon University (CMU). By contrast, previous attempts to displace neo-Keynesian macroeconomics, which had failed to meet their mark, had come from renowned economists affiliated with more prestigious institutions; Milton Friedman and Robert Lucas, for example, were based at University of Chicago.

Crucially, this paper makes the case that it wasn't primarily policy implications that made for RBC's appeal. Rather, for practicing economists, its professional promise lay both in its logical consistency and in its highly user-friendly, stripped-down methodology. The neo-Keynesian approach to modeling had been to build ever-larger systems of equations to more closely approximate the actually existing economy. This resulted in complex models that called for extensive teamwork and required serious computational power. Indeed, as Prescott noted in his 2004 Nobel address, in the 1970s the pinnacle of success in macroeconomic study was 'to have your equation incorporated' into large neo-Keynesian models housed in elite universities, research institutions or central banks—a state of affairs that he as an ambitious young scholar found deeply unsatisfying (Prescott 2006).

In contrast to the complex machinery of IS-LM models, the micro-founded RBC approach was lean and portable, and therefore especially attractive to junior economists eager to put their mark on the profession.[4] The simplifications built into RBC meant that economists who did not have access to exclusive professional networks and costly computational facilities could nevertheless make important contributions to disciplinary

[4] The term 'profession' is used in the sense of Andrew Abbott to apply to a group with common work (2014). The article takes the study of economics to be common work because of the cohesive and hierarchical nature of the discipline (Fourcade et al. 2015).

debates. In terms of professional appeal to individual economists, then, RBC had an obvious advantage over neo-Keynesian macroeconomic modeling.

Indeed, although this drastically simplified model was initially harshly criticized by a number of powerbrokers within the discipline, its streamlined and user-friendly approach quickly took over the discipline. By introducing a fundamentally different philosophy of modeling, RBC transformed the daily 'tasks and problems' that academic macroeconomists faced and, as a consequence, set in motion a transformative shift in mainstream macroeconomic knowledge production. By the early 2000s, DSGE had become the go-to method not just of mainstream academic macroeconomists but also of the International Monetary Fund (IMF) and the world's most important central banks (Tovar 2009; Ban 2015; Helgadóttir 2021).

Building on this profession-centric analytical approach, the paper identifies three facets of the RBC approach that helped make it a 'super-model',[5] enabling its improbable defeat of reigning neo-Keynesian IS-LM models and subsequent takeover of the field. First, RBC models were built so that they could *bond* together simple ideas, some of which did not enjoy mainstream acceptance, into an indivisible and workable whole. Second, they were structured so as to *deflect* known disciplinary critiques and sidestep entrenched logical obstructions. Third, they were *flexible* enough to attract new users while also retaining their core thrust. As shorthand, these three characteristics will be referred to as 'glue' (bonding), 'rubber' (deflection) and 'putty' (bounded malleability).

While these traits were necessary - if not sufficient - to the success of RBC, they were not required of neo-Keynesian IS-LM models, which enjoyed the prerogative of being

[5] Here it should be noted that the term super-model is taken to refer to the combinatory logic of a set of formalized economic ideas, encompassing both the economic model *per se* and the exoskeleton of disciplinary assumptions and caveats that buttress it.

the first macro-models on the scene with no competitors to displace. In fact, with each paradigm shift, we should expect the bar for models' success to be inched higher, as competition is heightened, accepted critiques mount, and technological frontiers are pushed out. This is also why, as Latour evocatively put it, if you, 'look at two articles in the same field taken at a twenty-year interval…[it] is like comparing a musket with a machine-gun (1987, p. 48-49).' Understood through this lens, Prescott and Kydland's singular achievement was to invent a tightly bonded, well-defended and attractive device that could provide a New Classical explanation of downturns and resurrect the possibility of macroeconomics as a cohesive field ordered by a single universal logic, as had been the case before the Keynesian revolution of the 1930s.

This article contributes to the state of knowledge in ideational IPE in four key ways. First, and most fundamentally, it is a crucial and hitherto missing part of the analysis of the turn to neoliberalism near the end of the twentieth century (e.g., Blyth 2002, Ban 2016, Helleiner 1994, Best 2005). Second, and very related, it is an important addition to the now large and established literature on economic ideas (e.g., McNamara 1998; Blyth 2002; Schmidt; Widmaier 2004): if indeed economic ideas matter, then understanding the professional conditions and constraints under which academic economic ideas emerge is essential. Third, emphasizing the distinction between academic ideas and policy ideas requires us to be more circumspect when we discuss ideational change in general. For example, in academic settings where formal constraints apply and ideas cannot be joined together freely, 'bricolage' has little analytical value (e.g., Carstensen 2011) and underemphasized 'second order changes' in instruments may in fact be what sets in motion paradigm shifts (Hall 1993). Fourth, putting instruments or models at the center of our

analysis adds a new locus of analysis to recent work on ideational change that gives analytical primacy to professional status and resources (e.g., Seabrooke 2014; Seabrooke and Tsingou 2014; Fourcade et al. 2015; Ban 2016; Helgadóttir and Ban 2021).

The next section reviews key strands of the literature on drivers of economic knowledge production and fleshes out the concepts of glue, rubber and putty. This is followed by a section that presents data and methods. The empirical section of the article is then divided into two parts. The first part examines the roles of institutional status and funding in the rise of RBC. The second empirical section examines in detail the structural characteristics of RBC models that contributed to their remarkable professional success. A final section concludes.

DRIVERS OF MODEL ASCENDENCY

One explanation of ideational shifts in economics is that new approaches win out because they are superior science, the result of cumulative progress sweeping away the old and defunct to make room for the new and effective. Such logic underlies the claim that RBC/DSGE took the discipline by storm because it 'solved' the 'problem' of missing micro-foundations in macroeconomics (Sargent 1982; Backhouse and Boianovsky 2012; Duarte and Lima 2012; Hartley 2014). However, such *a posteriori* claims take the reigning state of affairs both as a given and as an endpoint, rather than as a contingent and potentially impermanent outcome to be examined and explained (Latour 1987).

While there is no doubt that some economists of note genuinely saw missing micro-foundations as a critical logical inconsistency, the fact is that for decades the mainstream of

the discipline did not consider micro-foundations necessary to the study of macroeconomics. Indeed, for forty years following the Second World War, the discipline of economics was unified by what is often referred to as the 'Neoclassical Synthesis'. However, the Neoclassical Synthesis was *not* a synthesis in the literal sense of the word. Rather, it was a tacit consensus to divide the field into micro- and macroeconomics, each operating according its own logic and methods. Unlike microeconomics, which centered on the study of individuals, neo-Keynesian macroeconomic modeling was a combination of Keynes' preference for the empirical study of aggregates (i.e., these models were not micro-founded) and the neoclassical tradition of general equilibrium modeling. In other words, Neo-Keynesians dismissed micro-foundations because they *did not believe that the economy was reducible to the individual level* (Laidler 1999; de Vroey 2016; Young 2013).

The 'lack' of micro-foundations was, in other words, not a bug but a deliberate and enabling feature of the Neoclassical Synthesis and the neo-Keynesian tradition that made up its macroeconomic side. It allowed a divided discipline to carry on working within the two largely autonomous silos of micro and macro. The fact that this later came to be widely understood as an inherent and irredeemable flaw of IS-LM models is testimony not to a sudden realization that the logical underpinnings of neo-Keynesian macro were imperfect, but rather to the breaching of these silos and the disciplinary victory of New Neoclassical economists and the 'representative agent' modeling they espoused (de Vroey 2016; Blyth 2002; Colander 2012). In short, the question of what permitted the rise of RBC cannot be answered by simple appeal to scientific progress.

Rather, in order to analyze the dynamics of model competition, this paper combines insights from the sociology of professions on the one hand and Science and Technology

Studies (STS) on the other. Each adds significant analytical value to the other, and jointly they shed new light on the trajectory of macroeconomic thought. The sociology of the economics profession has yielded important insights into the nature and repercussions of jurisdictional struggles and issue control (Farrell and Quiggin 2017; Fourcade 2009; Fourcade et al. 2015; Bockman and Eyal 2002; Seabrooke and Henriksen 2017; Ban, Seabrooke and Freitas 2016). However, this scholarship has yet to systematically open up the black boxes of economists' devices and examine the ways in which they are at once constitutive of professional competition and a consequence of it. By contrast, a number of scholars working in the tradition of STS have primarily focused on financial models (e.g. MacKenzie 2005; Callon and Muniesa 2005; Preda 2009) and have yet to account for shifts in macroeconomics by putting models squarely at the heart of the discipline's trajectory. This article is a first cut at filling in this gap at the nexus between the two literatures.

While the paper takes inspiration from the sociology of professions and STS, it calls some of the conclusions of these bodies of literature into question. Specifically, it takes issue with the notion that strong positions in institutional hierarchies and access to resources are necessarily decisive variables in the successful promotion of scientific ideas. Indeed, in subsequent empirical sections it shows that a small group of primarily junior macroeconomists at mid-range universities, with only routine access to resources, was able to successfully revolutionize the profession by introducing a device with exceptionally strong professional appeal. Here, however, a caveat is in order: the goal is not to argue that status and resources are irrelevant—quite obviously, they are not (e.g. Latour 1987; Eyal and Bockman 2002; Seabrooke 2014; Seabrooke and Tsingou 2014; Fourcade 2006, 2009; Fourcade et al. 2015; Ban 2016; Helgadóttir and Ban 2021)—but simply to show that there

are seminal instances where powerful tools with strong professional appeal have proven to be the most important drivers of change.

This, in turn, requires us to examine such tools seriously and systematically. This paper suggests that three key characteristics of RBC—glue, rubber and putty, for short—contributed to its professional appeal, allowing it to emerge as a super-model that ended the professional dominance of neo-Keynesian macroeconomics. Each of these will be elaborated on below and subsequently applied to the case of RBC in the empirical parts of the paper, but first it bears mentioning that this is not to be read as a definitive list of the attributes we should expect to see in dominant macro-models. Rather, it should be read as an example of the kind of sociologically analytical thinking that can help us better understand model ascendancy.

The function of *glue* in this context is to bind together discrete ideas, creating a novel whole. Component ideas need not be recognized or mainstream in order to become part of an influential new assemblage. The whole, in other words, can be stronger than its constituent parts—as was very much the case for RBC. However, the 'fit' between component parts matters for the robustness of a new unit. If different elements are formally highly compatible, as was the case for RBC, even highly contentious ideas can be glued in tightly, making for an invulnerable whole. Assemblages that cohere less firmly—IS-LM, for example—can be pried apart with less effort, even when they have considerable practical appeal. When building formalized structures of academic knowledge, in other words, tightness of fit can matter more than fitness.

Modeling choices that serve to deflect, divert and redefine fall under the rubric of *rubber*. This concept has some overlap with Gieryn's notion of 'boundary work' in that it

highlights the malleability of the boundaries that demarcate legitimate knowledge production from non-legitimate knowledge production (1983, 1999; see Kranke 2020 for a recent application). It also has some kinship with concepts such as 'issue control', 'problem identification' and 'agenda setting' (e.g. Seabrooke and Henriksen 2017; Lefsrud and Meyer 2012). However, it diverges from these concepts in that it focuses specifically on the ways in which professionals navigate the minefield of *intra*-disciplinary critiques and use redefinition to overcome them and/or move the goalposts of pre-existing debates. Insider knowledge and skill are required both to successfully identify key obstructions and to defuse them or map out new solutions that permit their circumvention. The defensive way in which RBC was constructed to do just this can be discerned both in its technical and rhetorical dimensions (Carruthers and Espeland 1991). In the case of New Classical macroeconomics, bypassing longstanding objections took considerable formal experimentation and was successful only after a number of false starts—including Friedman's insufficiently formalized monetarism and the overly eclectic Lucas-Barro approach, both of which proved too vulnerable to known disciplinary critiques to seriously challenge neo-Keynesian macroeconomics.

Finally, the concept of *putty* refers to a device's circumscribed malleability, which serves to attract new users while also retaining basic structural integrity. It takes inspiration both from Kuhn and Swidler. Kuhn made the case that in order to be successful, new forms of knowledge production should be 'sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve (Kuhn 2012 p. 11)'. Indeed, Kuhn's whole notion of 'normal science' as a project of 'tidying up' loose ends presupposes enough openness and flexibility to engage the working mainstream of a scientific

profession. This, however, tells us little about the sources of continuity in knowledge production. Swidler's argument that certain ideas are anchored by social practices that force 'people to return to common structures' addresses this issue. Models and other devices can be considered examples of such social practices, producing what Swidler refers to as 'repeated interactional patterns' that result in continuity over time (2001 p. 85). RBC, for example, lent itself to a certain amount of adaptation, generating plenty of loose ends to work on, but it also had a stable and tightly bonded core that acted as a focal point and a set of rules for practitioners to return to time and again.

The combination of these qualities, a delicate balance between stability, plasticity and strategic redefinition, helped give a greater number of scientists a stake in the new device and as a result the professional network that subscribed to it grew quickly. By comparison, the large neo-Keynesian models were less coherent, vulnerable to criticism and less open to individual contribution. As a result, they proved poor at securing the cooperation of a new generation of scholars.

Taken together, then, the three concepts of glue, rubber and putty can help us understand why a change in the prevailing political atmosphere—say, a broad societal or political turn to neoliberalism—and/or the desire of a group of experts to change the dominant form of knowledge production, are not enough for such change to actually take place. Change in model-based professions cannot simply be willed into existence. Rather, in such disciplines, professionally attractive alternative models must come into play for it to be possible to affect permanent and meaningful change. In the case of RBC, a robustly assembled device that eliminated known obstructions and had strong professional appeal for new users was key to a revolution of the kind Prescott and Kydland set in motion.

A key point that emerges here is that in terms of methodology, New Keynesians working with DSGE models are much closer to their New Neoclassical counterparts than they are to postwar neo-Keynesians. After all, the knowledge production of both New Keynesians and New Neoclassical economists is carried out with what is at a fundamental level an RBC approach to modeling (Blanchard 2018). It is therefore probably not a coincidence that before the onset of the Great Financial Crisis both groups of economists—their attention broadly focused on the same problems, variables and methods—tended to agree with Lucas' assessment that the 'central problem of depression prevention ha[d] been solved, for all practical purposes' (Lucas 2003, p. 1). For example, noted New Keynesian Ben Bernanke attributed the pre-crisis 'Great Moderation' to theoretical advances in macroeconomics and successful management of the economy. This rapprochement between macroeconomists that were ostensibly from opposite ends of the ideological spectrum sprang from their shared brand of new micro-founded macroeconomics that was RBC.

Though the development of macroeconomics post-2008 is outside the scope of this paper, it bears mentioning that the approach presented here also goes some way towards explaining the surprising resilience of the reigning RBC/DSGE paradigm in the aftermath of the Great Financial Crisis (Thatcher and Schmidt 2013; Helgadóttir and Ban 2021). While there has been no shortage of scathing and substantive critiques of DSGE modeling and its failures as a tool of policy (for an overview, see 2018 special issue of Oxford Review of Economic Policy), professionally useful alternative models have been in short supply.

Here, the case of monetarism is also an interesting counterfactual. Decades before

Prescott and Kydland published their work on RBC, Milton Friedman and Anna Schwartz failed to pull off a comparable disciplinary coup. In their *A Monetary History of the United States* they offered an interpretation of the Great Depression that, unlike Keynesianism, did not attribute it to insufficient investment and consumption but rather to mistaken monetary policy (1963). However, while Friedman and Schwartz's claims generated a great deal of debate and some policy experimentation (albeit much less than is commonly assumed: Delong 2016; Krippner 2007), they were *not based on formal modeling or a workable device*. Instead, they relied on empirical analysis of a vast amount of data. Friedman attempted to formalize their insights only later and in response to strong pressure from his neo-Keynesian critics. Furthermore, as economic historian De Vroey concludes, when he did finally formalize monetarist thinking, 'he fell back on a construct akin to the classical sub-variant of the [Keynesian] IS-LM model. Thereby, he implicitly admitted that, as far as the analysis of the economy as a whole is concerned, monetarism is embedded in no specific framework different from the Keynesian one' (2015 p. 85-86). The monetarist counter-revolution, in other words, failed to produce a device that could restructure macroeconomic thinking and rewire the discipline. At the end of the day, as Gregory Mankiw has concluded, when compared to the RBC revolution that came after it, 'the monetarist-Keynesian debate of the 1960s look[s] like sibling rivalry' (Mankiw 1991, p. 22).

DATA AND METHODS

This article is based on analysis of both primary and secondary sources. While the secondary sources are primarily drawn from scholarship in economic history, the primary sources consist of all *Google scholar* articles published between 1982 and 1994 that contained the search phrase 'real business cycle*' (what RBC stands for), and which were either published in a top-20 ranking peer-reviewed economics journal[6] or that have received a hundred citations or more. This combination is meant to capture both articles that were seen as important contributions to the debate on RBC at the time of publication and ones that have emerged as important over time, through the process of citation. The time bracket is demarcated on the one end by the publication of Prescott and Kydland's seminal 'Time to Build and Aggregate Fluctuations', which first introduced RBC analysis, and on the other by the publication of an important edited volume, *Frontiers of Real Business Cycle Research*, that heralded RBC's entry into the mainstream of macroeconomics.

This search yielded a total of 197 articles by 331 authors, which were subjected to a combination of content analysis, network mapping and deep reading (Krippendorff 2009). In this case, the articles were hand-coded into categories that captured their orientation vis-à-vis RBC. The coding categories used were: 'foundational', 'normal science', 'fusion', 'critical', 'competing', 'literature review' and 'not applicable'. 'Foundational' was reserved for the handful of articles that first introduced RBC to the study of macroeconomics. 'Normal science' was used in the Kuhnian sense to capture research that aimed to tie up loose ends and resolve the new *internal* puzzles that emerged as RBC took hold. The category of 'fusion', by contrast, was used to capture efforts to reconcile RBC with policy

[6] Based on rankings from Laband and Piette (1994) for the 1980s and Kalaitzidakis, Theofanis and Stengos (2003) for the 1990s.

conclusions that differed from the initial, *laissez faire* iteration of the model put forth by Prescott and Kydland. This encapsulates the New Keynesian literature that contributed amendments to RBC that eventually resulted in what is now called DSGE modeling. Articles that had critique as their central goal were coded as 'critical' while articles that outlined alternative approaches while engaging with RBC were coded as 'competing', whether they contained critical components or not.

Next, the results of the coding were mapped onto institutional affiliation and funding. The goal here was twofold. First, to examine institutional patterns of support and contention and, by extension, to assess the role of institutional hierarchy, prestige and status in the rise of RBC. Second, to establish whether different contingents benefitted from the support of similar or different funding bodies and whether they were supported to variable degrees. The open source software *Gephi* was used to generate the visualizations of the resulting patterns.

Finally, examination of the professional politics baked into RBC modeling required a deeper understanding of both the content and structure of the RBC literature and the ways in which it differed from previously mainstream macroeconomics. The sections of the paper that examine this therefore stem from a combination of deep reading of the coded articles and contextualization from secondary sources.

INSTITUTIONAL PRESTIGE AND FUNDING

Prescott and Kydland and their early allies, the most notable of which was Robert Lucas, were well aware of the revolutionary implications of their work and from the start they

went in guns blazing, presenting papers with provocative titles such as 'After Keynesian Macroeconomics' [7] and 'On the inapplicability of optimal control (read: Keynesian macroeconomic management) for policymaking' at conferences full of neo-Keynesian economists.[8] In his Nobel interview, Kydland recalled that during one early presentation of the latter paper, '[a]ll hell broke loose. Everyone was trying to locate the error'—albeit in vain.[9] Yet, as the network below (figure 1) shows, RBC's critics were not to be trifled with. Not only did they hail from some of the most prestigious institutions in the world, the names behind the nodes also read like a who's-who of the profession, including Lawrence Summers, Gregory Mankiw, Janet Yellen, Ben Bernanke, George Akerlof, Julio Rotemberg, Alan Blinder, Joseph Stiglitz, Glenn Hubbard, Mark Gertler, Robert Hall, Bennett McCallum, Allan Meltzer, Stanley Fischer, Brad DeLong, Bruce Greenwald and Otto Eckstein.

[7] Lucas and Sargent 1981.

[8] This was an early draft of Kydland and Prescott's seminal "Time to build."

[9] https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2004/kydland-bio.html., accessed May 1, 2020.

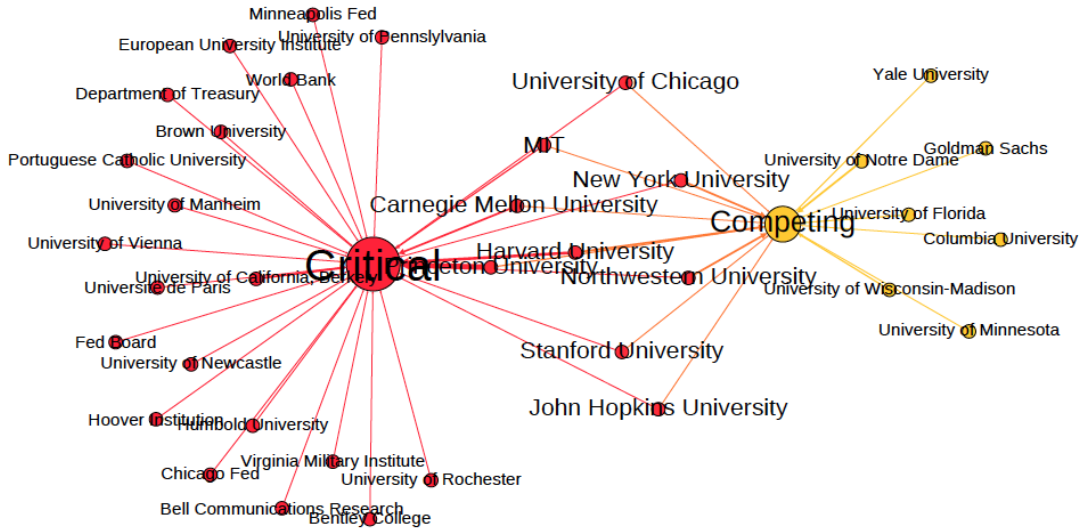

Fig. 1. —Network of institutional affiliation of critics and competitors of early RBC scholarship, published between 1982 and 1994. Source: Google Scholar, accessed on May 10, 2016.

Criticism seized on a range of issues, including unrealistic assumptions, the presumed role of variables that cannot be observed or measured, empirical disconfirmation, and the absence of money, credit and heterogeneity. Lawrence Summers concluded that:

> These [RBC] theories deny propositions thought self-evident by many academic macroeconomists…Let me follow Prescott in being blunt. My view is that real business cycle models of the type urged on us by Prescott have nothing to do with the business cycle phenomena observed in the United States or other capitalist economies. Nothing in Prescott's papers or those he references is convincing evidence to the contrary (1986, p. 23).

What is more, RBC's *laissez faire* policy implications appeared shocking even to politically conservative economists. Gregory Mankiw found that, 'to the extent that it trivializes the social cost of observed fluctuations, real business cycle theory is potentially dangerous. The danger is that those who advice policy-makers might attempt to use it to evaluate the effects of alternative macroeconomic policies or to conclude that macroeconomic policies are unnecessary' (1989, p. 79).

Nevertheless, in just a few years, the most vociferous criticism of RBC faded out, while a number of economists from universities all over the world began doing RBC's 'normal science'—Kuhn's term for the tying up of loose ends that inevitably appear as a new form of knowledge production takes hold—and trying to amend it in order to reconcile this new way of modeling with some of the policy conclusions of earlier neo-Keynesian work. The result was a surge in scholarship that was at a fundamental methodological level an RBC approach, but also incorporated frictions or modified assumptions that permitted policy conclusions of a more Keynesian bent than the first-generation of RBC models. This was the birth of DSGE modeling—which is another way of saying that it was the birth of the mainstream of contemporary macroeconomics. Or, as Olivier Blanchard, recently put it, '[t]he current [DSGE] core [is] roughly an RBC structure with one main distortion, nominal rigidities' (2018, p. 52). Going into greater detail, Vines and Wills describe a broadly agreed upon 'benchmark' DSGE model in a way that also makes its descent from RBC clear:

> The benchmark model is a microfounded representative-agent model. It is clearly a general equilibrium model since there is an analysis of demand and supply in the

> goods market and the labour market, and also in the money market and the equity market. It includes equations for consumption, investment, and price-setting that are derived from intertemporal optimization. Inter-temporal budget constraints are critical in determining asset prices. There is short-term stickiness in wages, and adjustment costs influence investment… The model can be used to study the effects of technology shocks of the kind studied by growth theorists and real business-cycle (RBC) theorists (14-15).

The network below (figure 2) maps the disciplinary reception of RBC between 1982 and 1994 onto institutional affiliation. The larger an underlined node, the more articles were written adhering to that view. Strikingly, it shows what a very small group of scholars (small green node marked 'foundational' underneath the larger blue node marked 'normal science') from a handful of mid-range universities (primarily CMU and University of Rochester) set off a debate that then exploded across the discipline. Further, that much of the early heated debates between supporters ('normal science') and critics ('critical' and 'competing') played out between scholars at prestigious institutions, while much of the literature that then sought to bridge RBC and Keynesianism ('fusion') came from midrange and peripheral institutions. In other words, RBC quickly secured the cooperation of much of the discipline, notably junior scholars and those affiliated with mid-range institutions, in spite of the opposition of gatekeepers and elite players. It does not seem to be the case that variables such as affiliation to prestigious institutions or elite support can help us understand RBC's meteoric rise; quite the contrary, in fact. Was RBC's success then perhaps due to access to plentiful resources or external bankrolling?

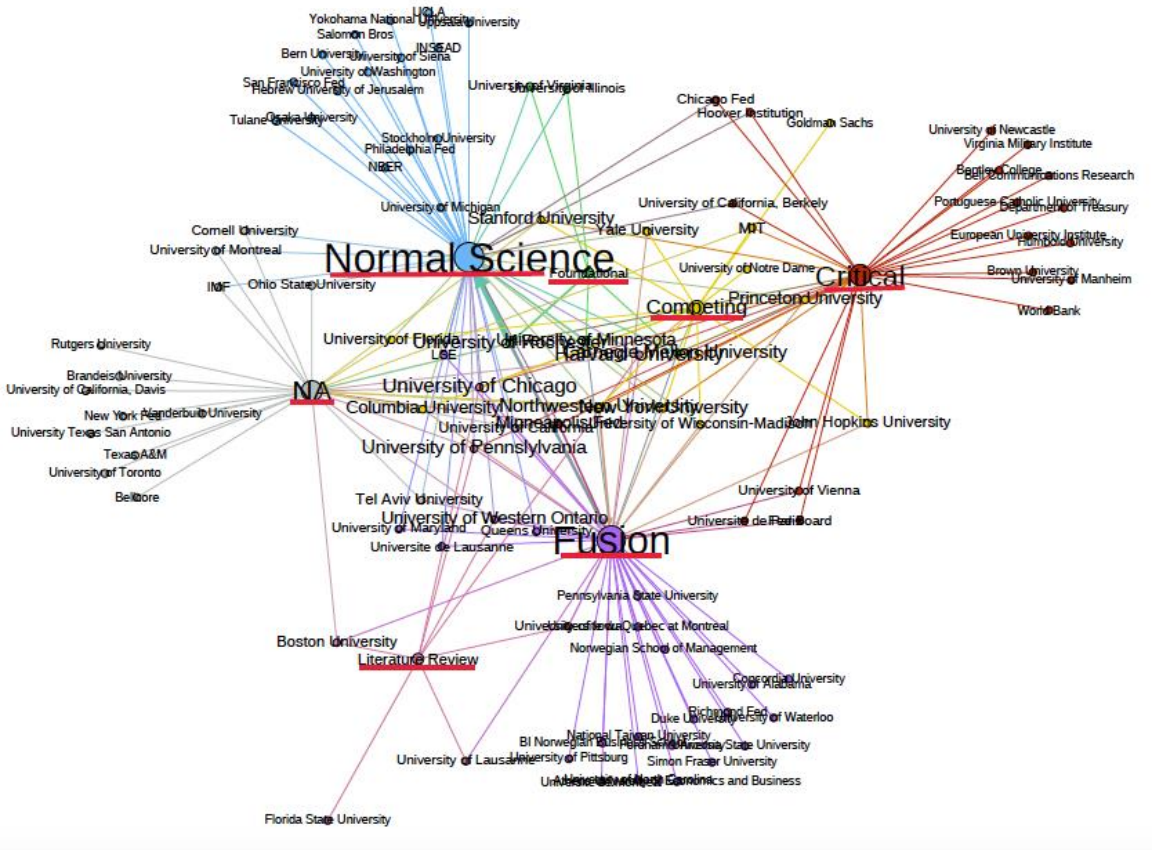

Fig. 2. — Network linking institutional affiliation and stance on RBC, based on articles published from 1982 to 1994. The divergent node size for these key nodes is indicative of the number of articles adhering to each view. Source: Google Scholar, accessed on May 10, 2016.

The network below (figure 3) maps out funding sources declared by the proponents and opponents of RBC. Two caveats are in order: first, the network does not reflect amounts of money, since these are not declared, but rather the number of grants that different camps received. However, since much of the funding for both sides came from the

same sources it seems reasonable to assume that the amounts in question were not vastly different. Second, it is possible that not all grants were reported and, if this is the case, funding bodies with known political affiliations might be the first to be left out. This network should therefore be interpreted with some caution.

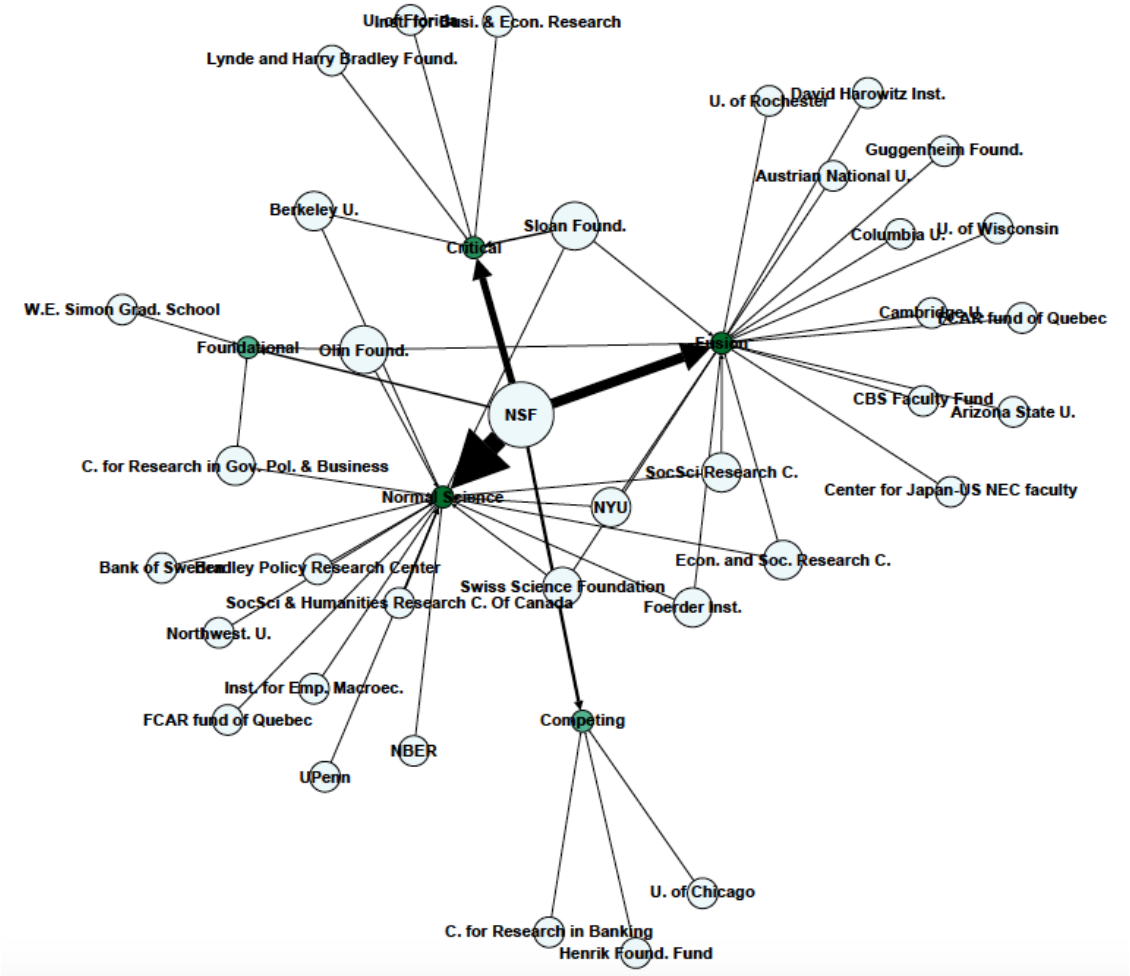

Fig. 3. —Network of funding sources for different stances on RBC in articles published between 1982 and 1994. Funding for all sides of the debate came primarily from mainstream funding bodies and intramural university sources. The darker the nodes for different camps, the more grants they received. The size of the

funding nodes is indicative of the number of grants that flowed from them and the thickness of the ties reflects the number of grants connecting any two nodes. Source: Google Scholar, accessed on May 10, 2016.

Overall, funding does not appear to tell a more decisive story than institutional affiliation. Fusion was the best-funded category, followed by normal science. Critical, competing and foundational research received fewer grants. However, the differences between the divergent camps were not marked. Further, the greatest number of grants for all sides came from known and mainstream bodies: the National Science Foundation, the Sloan Foundation and the Social Science Research Council, to name a few. These funders supported various types of research, often in rough proportion to research output, or the number of articles published. Other sources of funding were primarily internal university funds. The notable exception to this trend is the conservative Olin Foundation, which had a considerable presence and supported foundational research, normal science and the fusion of RBC with other scholarship. On the whole, however, the rapid rise of RBC does not seem to be attributable to funding nor does political bankrolling seem to have played a notable role in its ascendency. The following section turns to the examination of the device of RBC *per se* and the novel professional incentive structures it embodied.

BUILDING A SUPER MODEL

RBC as Glue

During the 1970s, New Classical ferment bubbled and boiled under the seemingly cohesive surface of the postwar Neoclassical Synthesis (Mirowski and Plehwe 2009). But even as

New Classical economists and other ideological and political opponents bristled under neo-Keynesians postwar dominance, a number of puzzles that were critical to their program remained unsolved. Key among them was the inability of New Classical theory to formally account for booms and busts, or 'the business cycle'. Robert Lucas, model and mentor to both Prescott and Kydland, had concluded that addressing this gap was crucial to toppling the neo-Keynesian edifice (Lucas 1977). But the solution to this 'problem' had to be persuasively formulated through the formal medium of modeling. In the mid-1970s, Robert Lucas tried his hand at the business cycle challenge himself, as did Robert Barro. However, both their models centered on actors' conflation of nominal and real prices (Lucas 1973; Barro 1976), and this *misperception* fit poorly with the assumptions of rational actors and perfect information that were at the core of the New Classical worldview. Their internally inconsistent—or insufficiently bonded—work failed to transform the discipline.

For decades, every few years, new and potentially revolutionary methodological and/or theoretical insights rose to the surface, only to pop bathetically, barely rocking the neo-Keynesian dominance. To name but a few, there was the Arrow-Debreu general equilibrium proof (1954), Muth's rational expectations thesis (1961), Lucas and Rapping's notion of the inter-temporal substitution of labor and leisure (1969), Brock and Mirman's use of stochastic shocks (1972), Lucas' micro-foundations critique (1976) and the Hodrick-Prescott-filter. Prescott and Kydland's ingenious move was to glue a number of such disconnected ideas very tightly together into a coherent formal whole that was stronger than its constituent parts, and then to firmly embed this new construct in the historical narrative of the discipline of macroeconomics.

Before Prescott and Kydland's intervention these anti-Keynesian ideas had by and

large not had a great impact on research in macroeconomics. But once they were bonded together into a tractable and robust formal device, they revolutionized the discipline. More than twenty years after they first presented the RBC model, Robert Lucas was still in awe of their achievement: 'How did they ever think to put all these pieces together in just this way?' he marveled in a 2005 retrospective of RBC (2004, p. 777). This section of the paper explores the ways in which Prescott and Kydland strengthened their hand by gluing together ideas, generating tightly linked logical constructions that had both *synchronic* and *diachronic* dimensions, discussed in turn below.

In this context, synchronic bonding refers to the way in which they tied together free-floating contemporary ideas into a tightly interlocked model. Figures 4 and 5 illustrate the way in which RBC modeling bonded ideas together more tightly than the neo-Keynesian approach. If we, for the sake of such an illustration, momentarily transform the component parts of economic models into Lego blocks, then neo-Keynesian macroeconomic modeling can be pictured as a number of blocks placed next to each other. Each one represents a key schedule or sector of the economy. Notably, the blocks are contiguous but not interlocked and, as a result, component parts can easily be moved, rearranged, added or removed without changing the fundamental nature of the whole.

While initially small, neo-Keynesian macroeconomic models grew larger and more complicated as more and more sectors, or Lego blocks, were included in the model. This happened as the builders of the neo-Keynesian models sought to imitate observed realities ever more closely over time. This unlimited possibility of expansion is what the heading 'Sector n' is meant to indicate in the illustration below. The end result, in some cases, was the combination of hundreds of equations. Given the state of technology at the time,

running such models required both institutional access to computational resources and cooperation between many economists (Backhouse and Cherrier 2018).[10]

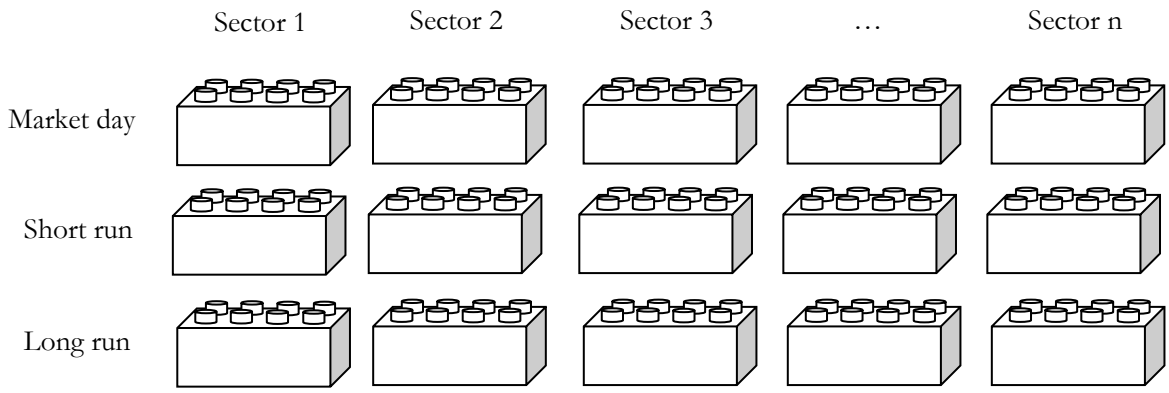

Fig. 4. —Loosely bonded but contiguous Lego-blocks, each representing a schedule or sector of the economy in neo-Keynesian macroeconomic models. [11]

At its base, this was 'a pragmatic approach—that is, a more data- than theory-constrained approach—the modelers were mainly aiming at accounting for the observed facts as best as possible' (De Vroey 2015, p. 189). To neo-Keynesians, this signified greater sophistication, precision and predictive power. For New Classicals, on the other hand, it was casuistic reasoning that produced 'jerry-built structures' (Lucas 1977, p. 8).

By contrast, Prescott and Kydland's stripped down RBC model did not seek to emulate observed economic realities or their component parts. Instead, it was comprised of

[10] The aspiration to account for observed empirical realities was also somewhat hampered by the computational capacity of the time, requiring model-builders to add cross-equation restrictions. By the time DSGE models started expanding, computational power had also increased by leaps and bounds.
[11] This is a Lego-fied and slightly altered version of de Vroey's illustration of Marshallian modeling (2009). The second illustration is my own.

a select few tightly interlocking and mutually reinforcing propositions that could not be pried apart without demolishing the whole. Taking a glance at the structure inside the black box of RBC reveals that where neo-Keynesian macroeconomic models offered detailed descriptions of the actually existing economy, the RBC model operated like mathematical synecdoche. Here we can think of it as a tower of stacked Lego blocks in the shape of an inverted pyramid, with all component parts securely glued together, each part holding up the next and increasing the analytical traction (figure 5).

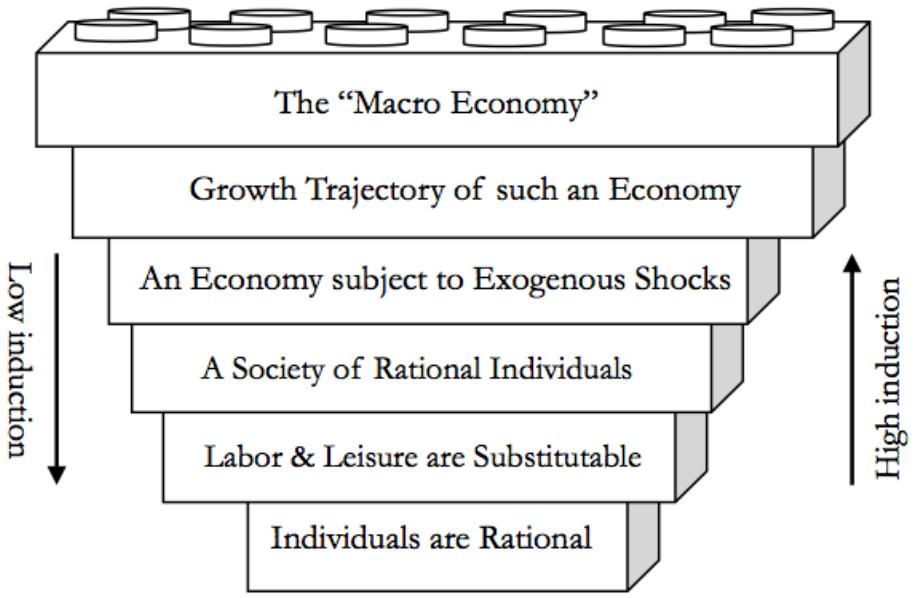

Fig. 5. —Interlocked and tightly bonded Lego-blocks, each representing a step in the logical sequence from axiomatic assumptions about individuals to conclusions about the economy as a whole in RBC modeling.

At the base of the stack of logical claims that make up this black box was a single rational individual. That individual then made decisions about whether to work or enjoy leisure based on prevailing wages, working more when wages were higher and less when they were lower. This is what 'inter-temporal substitution of labor and leisure' refers to. This individual was then 'multiplied' to form an entire 'society' composed of identical individuals who, faced with the same opportunities and constraints, would make the same choices. This is the 'representative agent' method.

This society of representative agents was then subjected to some hypothetical shift in external conditions that set in motion larger changes. This was referred to as 'exogenous shocks' or 'technology shocks.' The shifts could represent anything ranging from disruptive technical innovation to disastrous weather or rapid price changes. After the shock, real wage levels were altered, and individuals' labor-leisure calculation thus changed. The new trade-off between labor and leisure and the collection of individual choices that flowed from it then shifted the growth trajectory of the economy as a whole.

In this way, with each new layer affording greater analytical traction, overarching claims about 'the macro-economy' could be made based on the tightly bonded and indivisible edifice. The upshot of this interlocked structure was that these core ideas were all for one and one for all—no step of the argument could be removed. This is why even when prominent macroeconomists criticized certain inbuilt assumptions, they barely made a dent in the new program.

Prescott and Kydland's use of the 'Hodrick-Prescott-filter', more commonly referred to as the HP-filter, is a good example of this interlocked dynamic: in order to study business cycles, Prescott and Kydland first had to be able to differentiate between

the cyclical and the secular (or growth) components of the trends they were interested in. They did this by applying a filtering method that Prescott had pioneered a few years earlier with Robert Hodrick. This method, however, was far from mainstream. Indeed, Prescott and Hodrick had not succeeded in publishing their work on this method in a peer-reviewed journal. In fact, one particularly vehement letter of rejection had come from Milton Friedman (Young 2013).

It was only many years later, in 1997, when this filtering method had been mainstreamed as a component part of RBC analysis that the *Journal of Money, Credit and Banking*, with the authors' special permission, published the working paper in which the filtering method was introduced (Hodrick and Prescott 1997). RBC, then, hinged on the HP-filter and, conversely, the HP-filter became an accepted tool because of its role in RBC analysis. As part of an interlocked and coherent whole, ideas that had little traction on their own could both legitimize and be legitimized.

In contrast to synchronic bonding, *diachronic bonding* refers to the way in which, once the RBC agenda took off, Prescott and Kydland began yoking their ideas to established scholarship in macroeconomics, embedding RBC in the historical arc of the discipline. Two key moves in this direction were, first, to embed RBC in Robert Solow's 1950s growth model (sometimes also referred to as the Solow-Swan model) and, second, to narrate RBC as a natural and crucial next step in the progression of macroeconomic thought (Prescott 1986).

Prescott made the connection between RBC and Solow's growth model in his 1986 article 'Theory ahead of business cycle measurement,' which was part of a special issue on RBC published by the Federal Reserve of Minnesota (Prescott 1986). This

coincided with the height of the Keynesian counterattack against RBC; the special issue contained Lawrence Summers' assessment of the new approach (1986)—noted for how scathing it was even in a discipline that does not pull its punches. This was, in other words, the right time for Prescott to mine the literature for strong 'allies', to use Latour's terms.

Solow was an ideal candidate. He was a towering figure in the discipline and his growth model had spawned a vibrant literature of its own. In his 1986 article, Prescott posited that RBC provided the 'missing' micro-foundations for Solow's growth model as well as the cyclical complement to his analysis of secular trends. Prescott and Kydland also began referring to the exogenous technology shocks that animated their model as 'Solow residuals'[12]—much to the dismay of most growth economists (King and Rebelo 2000).[13]

From then on, the argument that RBC was an extension of Solow's growth model became a standard claim in Prescott and Kydland's work and, gradually, part of the prevailing disciplinary narrative of RBC (King and Rebelo 2000; Williamson 2005). The fact that Solow—a noted neo-Keynesian and a vocal advocate of the eclectic non-micro-founded methods that were at the heart of the Neoclassical Synthesis—disagreed with this interpretation did not prevent it from taking root.

Indeed, in 1987, the year after Prescott first claimed this lineage, Solow was

[12] Since the role of technology in growth cannot be measured directly Solow had estimated the impact of technology on growth as a residual, or what was left when other known factors were accounted for. He was, however, acutely aware of the shortcomings of this approach. Abramovitz (1956) famoulsy dubbed this residual as 'the measure of our ignorance'.

[13] Notably Solow also testified 'against' DSGE models in a U.S. Senate hearing after the onset of the Great Financial Crisis (http://www2.econ.iastate.edu/classes/econ502/tesfatsion/Solow.StateOfMacro.CongressionalTestimony.July2010.pdf, accessed May 1, 2020).

granted the Nobel Prize in economics. He spent a considerable portion of his Nobel lecture disavowing RBC, even as he admitted that its premise was very difficult to refute—a fact that he found 'disconcerting' (Solow 1987).[14] But even Solow's use of this most public of platforms did not suffice to sever the new association between his work and that of Prescott and Kydland.[15] As Latour would have predicted, as a function of repeated citation, Prescott's statement came to be treated as disciplinary fact.

Alongside this claim, Kydland and Prescott began publishing articles that traced the lineage of their method back even further than Solow. One article, for example, discussed a 'Conference on Cycles' that took place in 1922 at which scientists from several disciplines, including economics, agreed on a definition of cycles that the duo saw as compatible with their definition of business cycles. They also posited a connection between RBC and the pre-Keynesian work of Ragnar Frisch—a trailblazer of econometric scholarship (1990, p. 5). Yet these links, when examined, are quite tenuous. For example, the 1922 definition of cycles that Kydland and Prescott harken back to is broad to a fault: 'the word [cycle] denotes a recurrence of different phases of plus and minus departures, which are often susceptible of exact measurement' (1990, p. 5). The point here is not to suggest that the links that Kydland and Prescott posited were not 'authentic', but rather that they seem to have been animated more by a desire to cement a historical place for RBC within the discipline of macroeconomics than by an aspiration to explore and develop actual substantive correspondences.

RBC as Rubber

[14] https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/solow-lecture.html, accessed May 1 2020.
[15] Solow remains preoccupied with the use to which his model has been put, e.g. Solow 2001, 2008.

Another key stride in RBC's march towards professional dominance consisted in shifting the terms of the debate and sidestepping known disciplinary criticism. Keynesian macroeconomics was born from the privations of the Great Depression. From the outset, its primary goal was to uncover ways in which the state could prevent the reoccurrence of such a disastrous market failure. It was therefore an innately pragmatic, policy-oriented and activist discipline, erected around the central problems of unemployment and depression. Crucially, these problems were taken for granted or simply assumed to be actual facets of economic life.

The methodological redefinition at the heart of RBC effaced both this political agenda and these underlying assumptions by fiat. Taking rational individuals rather than market failure as a starting point meant that both involuntary unemployment and recessions that required government intervention were, formally speaking, excised from the picture. The socio-economic outcomes that Keynesians sought to understand and remedy simply ceased to be part of the scientific universe as redefined by the New Classicals.

What, then, was the impetus of New Classical/RBC macroeconomic study? Lucas' vision, adopted by Prescott and Kydland, was to generate a universal and dynamic theory that could account for the behavior of a number of variables throughout the course of both upswings and downturns by appeal to the same causal variables and as part of the same underlying laws of motion. In effect, the New Classical scholarship focused not on economic problems, as the neo-Keynesians had, but on *the patterns of co-movement of several macro-economic trends.* The level of activity, measured as the number of hours

worked, was at the core of this endeavor. The aim was for New Classical models to be able to predict, more or less accurately, the dynamic and mutable relationship of the level of activity to several other variables—including output, consumption, investment, prices, profits and interest rates—over the full course of the business cycle. The better the models could predict co-movements the more successful they were deemed.

This was a radical redefinition of what macroeconomics *should* explain and the sorts of expert statements that macroeconomists' models should produce. The enormous chasm that this created between neo-Keynesians and New Classicals can help us understand both why neo-Keynesian economists' intuitive reaction to RBC analysis was to try to 'locate the error' and why there was no 'error' to be located. Keynesian thinking was simply sidestepped. This was not just a fundamentally, disorientingly different way of looking at the world, but a way of looking that redefined economists' role in it. It was as if a team of oncologists were having a heated debate over how to remove an ailing patient's tumor when suddenly a group of biochemists showed up, interested only to stand back and observe a host of chemically induced processes in the patient's body run their course. They were at cross-purposes by definition and there was no common terrain even to discuss the tasks at hand.

This kind of redefinition and sidestepping of known theoretical issues can also be applied more narrowly. A prime example is Prescott and Kydland's use of the representative agent method, which was a contraversial but ultimately necessary move. Taking the stage to give his 2004 Nobel lecture, Finn Kydland said: 'I'm delighted to stand before so many people. I'm also very happy when I get to work with models with

many people.'[16] He would use a similar opening line on other occasions, and not surprisingly—it was a great lede, at once evocative and, for those in the know, a decisive disciplinary dog-whistle. The subtext was: at last, we have successfully micro-founded macroeconomic models so that they center not on abstract Keynesian aggregates but individuals.[17] The 'people' in Kydland's models were imaginary individuals—rational, fully informed and utility maximizing actors—the scaled-up actions of which were used to draw conclusions about the economy as a whole.

Crucially, however, as explained in the section on RBC-as-glue, these imaginary individuals were all *identical*. Prescott and Kydland's model economy was a virtual community of clones, all of which shared the same preferences and same information and dealt with the same constraints in the same way. It might therefore have been more accurate to say that Kydland's model contained not 'people' but a 'person'. This approach is what is referred to as the representative agent method.

Intuitively, it seems a peculiar methodological choice, and one that could leave New Classical modeling vulnerable to obvious critiques. For example, assuming that an economy is made up of homogeneous actors eliminates questions of inequality by definition. After all, if everyone is the same, distributional effects disappear. The same goes for coordination problems and questions of exchange; a representative agent framework can tell us very little about such topics. So why use it?

The answer is that it allowed Prescott and Kydland to sidestep a well-known and thorny theoretical problem. The representative agent framework offered a 'solution' to the *fallacy of composition of demand*, or the fact that individual demand does not add up

[16] http://econ.ucsb.edu/~kydland/econ101/Nobel_Lecture_withChartsApril20.pdf
[17] For more on microfoundations see e.g. Hoover 2010; Hoover 2014; King 2012.

to aggregate demand in a straightforward manner (Keen 2011). When Keynes pointed to fallacies of composition decades earlier, he did so largely based on intuition (1936). After the Second World War, however, Keynes' intuition was shown to be formally correct. The formal proof of the fallacy of composition of demand was formulated independently by three economists, Hugo Sonnenschein (1972), Rolf Mantel (1974) and Gérard Debreu (1976).[18] They demonstrated that even assuming full rationality, *one cannot add up individual demand curves to get a single market demand curve, while also maintaining a unique equilibrium.* Rather, adding up individual demand curves results in a badly behaved aggregate demand curve, which is to say one that can actually take any shape at all.[19] It can thus fail to cross an aggregate supply curve or it can cross it multiple times. This poses an obvious problem in a discipline that generates solutions by calculating the points of equilibrium where supply and demand intersect. This proof, formally known as the Sonnenschein-Mantel-Debreu (SMD) Theorem, was informally dubbed the 'Anything-Goes Theorem' for the devastating effect it was presumed to have on efforts to provide macroeconomic analysis with micro-foundations.

Indeed, at first glance it seems to buttress the postwar bifurcation of the discipline into micro- and macroeconomics and justify the use of different methods in the two branches. However, the SMD Theorem also showed that there was a way to sidestep this logical problem. It required making two very restrictive assumptions: first, all goods had to be homothetic, which means that they are consumed in the same proportion regardless of changes in income. Second, all consumers must have the same taste (Keen 2011). In effect, this is tantamount to saying that there can only be one good and only one

[18] Keen (2011) also credits Gorman (1953) with this insight.

[19] Other than overlapping itself, that is.

consumer, which is the essence of the representative agent approach that Prescott and Kydland relied on. This method of circumventing the problem has, predictably, been heavily criticized (Nelson 1984; Kirman 1989; Keen 2011; Hoover 2010; Solow 2010; King 2012). Yet, as an instrinsic, necessary and tightly bonded part of RBC modeling it has also proven robust to such criticism, cementing Prescott and Kydland's way of circumventing the logical problem at hand.

To sum up, then, Lucas offered a new vision of how modeling should work, what it should achieve and a logical rejection of the Keynesian approach - all in one neat philosophical package. Under the protective mantle of this vision, Prescott and Kydland then set about solving the technical issues that faced micro-founded macroeconomics, relying on representative agents by necessity. Taken together, this redefinition of the practice of macroeconomics represented a compelling totality. The next section shows that once all this was achieved, the one remaining obstacle to the rise of RBC was to spread the gospel and secure the cooperation of other economists.

RBC as Putty

As the section on RBC-as-glue showed, RBC had a simple and tightly interlocked structural core. However, as this section will argue, it also lent itself to a certain amount of adaptation. Such a putty-like quality stands to give a greater number of scientists a stake in maintaining and spreading new forms of knowledge production and can therefore be crucial to their success. By comparison, the large Keynesian models were more complex and less open to meaningful co-constitution. Individuals could make only small

contributions to them and as a result they proved poor at securing the cooperation of a new generation of scholars.

RBC's founders were aware of the potential for development and diffusion that their model's circumscribed malleability presented. Indeed, they encouraged such elaborations and, in some cases, even took a leading role in them. There were two notable routes through which RBC's malleability served to enroll new practitioners. One was the result of a typical property of new forms of knowledge production, as described by Kuhn: the fact that they generate new internal puzzles and leave a number of newly loosened ends untied. This results in an upsurge of 'maintenance work' that Kuhn referred to as *normal science*. This dynamic was immediately apparent in RBC, as it generated a host of new internal questions, providing individual economists with a number of new tasks to try their hand on.

One excellent illustration of this dynamic followed the publication of a 1985 article by Rajnish Mehra and Prescott. This article, called 'The Equity Premium: A Puzzle,' showed that an RBC model does a poor job of predicting observed patterns in asset prices. Specifically, seen through the filter of Mehra and Prescott's model, it is difficult to explain the large gap in returns on equities on the one hand and government bonds on the other. This difference, which they estimated at 6%, is the articles' eponymous equity premium puzzle (so well-known by now that it is often abbreviated as EPP). Mehra and Prescott concluded that probably some adjustments had to be made to their model in order for it to fit observed asset prices more closely (1985).

This observation has since spawned a cottage industry of its own: in April of 2020, Mehra and Prescott's article had 7519 citations on Google Scholar. Proffered

explanations of the EPP pointed to factors ranging from tax policies on interest and dividends to loss aversion and moral hazard (McGrattan and Prescott 2001; Benartzi and Thaler 1995; Kahn 1990). But for the purposes of understanding RBC-as-putty, what really matters is the fact that this way of modeling both generated a new 'anomaly' that would not have existed in its absence and acted as the terrain on which possible explanations must be debated. The resulting debate, in turn, helped further legitimize and mainstream RBC.

Another example of the normal science of RBC concerns the role of technology shocks in generating business cycles. Prescott and Kydland's early work concluded that real and exogenous shocks explained around 70% of business cycle fluctuations.[20] Yet, such shocks could not be directly observed and Prescott and Kydland's work did not shed light on what, concretely, constituted a shock. Though Prescott and Kydland went on to bond their notion of shocks to the Solow residual, as discussed in a previous section, this did little to further elucidate the real-world nature of the kind of shocks that were at the core of their model and dispel the criticism that external shocks were an underdetermined element of RBC. As a result, a number of economists have set about trying to explain what actually constitutes technological shocks, pointing to factors as diverse as oil prices and seasonal fluctuations as possible drivers. RBC, in other words, raised a number of new questions that were internal to the logic of the model.

A second way in which RBC's flexibility has attracted new users is through what might be called 'fusion'. This process encompasses two dynamics. First, the tempering of RBC's strict assumptions in order to reconcile the method with observed realities of

[20] Kydland and Prescott 1982; Prescott 1986.

market failure. For example, instead of full rationality one can assume bounded rationality; or, more recently, instead of homogenous agents, one can model the interactions of a limited number of heterogeneous agents.

Over time, as the extent to which RBC was amenable to such co-constitution became more apparent, many skeptics came to accept its fundamentals, while also adjusting it in various ways. Doing this has permitted many RBC supporters to reach policy conclusions that differed from the *laissez faire* of early RBC analysis. This group came to be known as 'New Keynesians' and its amended RBC models as DSGE models. As previously noted, DSGE models form the bedrock of contemporary macroeconomics.

Second, fusion involves expanding the RBC approach to new areas of inquiry. This has generated a rich RBC/DSGE scholarship that opened new fronts of inquiry and new jobs on topics as varied as public finance, labor economics, international economics and monetary economics. RBC is thus being used to study some of the very things that the first iteration of the model simply assumed away. However, given the structural constraints of RBC, the conceptualization of these issues that the models can accommodate are often quite narrow. Monetary effects, for example, are primarily represented as shocks, although there are many other ways in which central banks and monetary policy can be impactful. Similarly, the literature that extends to finance revolves exclusively around the micro-level aspects of finance (asset prices) while disregarding and even negating potential macro-implications (financial crises).[21] Thus, even as RBC was extended to address new questions, many systemic and macro-level

[21] McGrattan and Prescott 2001

aspects continued to fall outside the scope of analysis, as they did in the first RBC models that contained no central banks and no financial sector at all.

CONCLUSIONS

Formal models can do extraordinary things: they have helped uncover fundamental laws of physics, directing our attention to patterns undetectable to the naked senses. They can be used to compute fluid dynamics and the stress points of solids, showing how best to build a jet engine and whether or not a bridge will bear the weight we want it to. They make predictions about how weather patterns or pandemics will unfold, the movement of rockets and the impact of simulated nuclear explosions.

But model building is also, as Robert Lucas has stated, a form of storytelling. As such it requires that narrative choices be made (Morgan 2001). Models embody assumptions about what constitutes legitimate knowledge production; about which variables matter and which do not; about how discrete phenomena interact and about what forces animate such interactions. As a rich literature in sociology has argued, such choices can be very consequential when it comes to modeling the social world, as we form and perform reality through the very act of modeling it (e.g., Mackenzie 2008; MacKenzie et al. 2008; Espeland and Sauder 2007; Fourcade and Healy 2017).

Prescott and Kydland's parsimonious way of thinking about the economy necessarily left out many of the elements that had featured prominently in the more holistic Keynesian approach. An incomplete list of absences includes involuntary unemployment, money, a monetary authority (or central bank), a financial sector,

heterogeneity and inequality. The very act of making choices of this kind, highlighting some aspects of the social world and effacing others, is inherently political. This belies claims that RBC analysis (or any other form of economic modeling) 'is by nature ideologically neutral' (Danthine and Donaldson 1993, p. 3), as its proponents have consistently argued.[22] One notable illustration of the political implications of model building, and one which has commanded considerable scholarly attention, is the lack of financial regulation in the run up to the Great Financial Crisis of 2008. This lack of regulation was directly informed by the narrative choices baked into the dominant mainstream economic models of the pre-crisis years (Baker 2013; Blyth 2013, Tooze 2018).

By contrast, less scholarly attention has been paid to the interplay between models and professional practices. Nevertheless, the fact is that not only did the rise of RBC drastically move the goalposts of economic thinking and policymaking, it also fundamentally transformed the daily 'tasks and problems' of mainstream academic macroeconomists. Thus, over the course of the last few decades, economists of the New Keynesian bent have had to exert a great deal of effort to carve out space within the constraints of the RBC/DSGE framework for the kinds of policy conclusions, observations and assumptions that were simply taken for granted during the heyday of Keynesian macroeconomics. The rise of RBC, then, resulted in a radical reallocation of resources within the effort economy of the discipline, focusing attention on intellectual puzzles that had not existed before RBC and which could only be authoritatively

[22] Lucas, for example, has said that, "[t]he classical issue of the proper role of government in a democratic society, of 'laws versus men' or 'rules versus authority,' are not going to be settled by technical advances in economics. It follows that no one's position on such basic questions needs to be threatened by such new technologies as may come to be at our disposal (Lucas 1979, cited in de Vroey 2015, p. 200)."

addressed through its lens. The other side of that coin is clearly that it diverted attention away from the kinds of questions and policy concerns that had occupied postwar Keynesians.[23]

The same holds in reverse: not only do models and devices shape the daily work and allocate the attention of professionals in formalized fields; model ascendancy is also inextricably bound up with professional concerns and incentives. Thus, another key claim of this article is that the rapid and remarkable rise of RBC can to a significant extent be attributed to the fact that the model served the professional interests of academic economists better than Keynesian modeling had. A logical extension of this claim is that unless we witness the emergence of new a new kind user-friendly macroeconomic model, we should not expect drastic shifts in macroeconomic knowledge production to be around the corner, even as the status quo of the state of knowledge sometimes seems to be on a collision course with economic reality.

The key goal of academics is to produce knowledge and publish their research. By and large, this is the gateway to other forms professional success such as tenure, higher wages, funding opportunities and access to scholarly networks and platforms. This article explores three characteristics of RBC that made it particularly suited to further this goal. First, it glued together a number of ideas into a simple and workable whole that

[23] One relatively recent example of the requirement to argue based on RBC/DSGE played out in the immediate aftermath of the Great Financial Crisis (GFC). In 2009, when Berkeley professor Christina Romer, then Obama's economic advisor, wrote a study using an "old" Keynesian IS-LM model that concluded that the Obama fiscal stimulus would have a high multiplier effect she was duly rebuked by her peers for deviating from appropriate modeling (Romer and Bernstein 2009). Soon afterwards, a number of prominent mainstream macroeconomists jointly rejected the study's conclusions based on the argument that it used outdated methods that were not accepted by the profession. In lieu of Romer's IS-LM model they offered a DSGE analysis that questioned the purported expansionary effects of the Obama stimulus. This work was more favorably received and is regularly cited in peer-reviewed *American Economics Review* articles taught in graduate schools (Cogan et al. 2009) For a more systematic look into the extensive pushback against the return to old Keynesian methods after 2008 see Ban 2016).

individuals could use; second, it featured an inbuilt defense against known disciplinary critiques; third, its circumscribed malleability both opened up a host of new questions and introduced a stable framework through which to tackle them. All of this made it uniquely attractive to academics who, as the truism goes, must publish or perish.

For other modeling professions, the incentive structure may be different. Their goal may be to maximize profits, minimize legal liability, contain pandemics or reach a wide audience. But regardless of what the specific goal is, social scientists should pay attention to it and remain vigilant to the interaction between professional incentives on the one hand and dominant forms of formalized knowledge production within any given profession on the other. Prevailing forms of knowledge production—which define the common work and daily tasks of professionals—are also likely to be those that further the individual interests of the common practitioner of a profession. When knowledge production also happens to be both politically consequential and largely opaque to lay actors and generalists, understanding that incentive structure is crucial.

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