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ABSTRACT

In 1982 Finn Kydland and Edward Prescott published “Time to Build and Aggregate Fluctuations”, an event generally recognized as marking the birth of real business cycle theory. Economic historians and prominent macroeconomists reacted with vigorous critiques of the first generation of RBC models in the 1980s. RBC proponents persisted, encouraging new methods and objectives for macroeconomic research and new standards for explanation and model validation. In the ensuing decades, DSGE methods and models, including New Keynesian variants, flourished among macro theorists, but economic historians, many empirical and policy oriented economists, and private sector analysts largely ignored them. This paper suggests that this differential uptake should be understood not only or primarily in terms of which models explain the data better, but also as the outcome of contests over the bounds of acceptable argument within professional communities.

The Birth of Real Business Cycle Theory

In 1982 Finn Kydland and Edward Prescott published “Time to Build and Aggregate Fluctuations”, an event generally recognized as marking the birth of real business cycle theory (the terms were first used by John Long and Charles Plosser in 1983). Echoing Robert Lucas’s policy ineffectiveness postulate and their earlier “Rules Rather than Discretion” paper (1977), Kydland and Prescott challenged the usefulness of systems of equations macroeconomic models, in particular the likely success of such models in predicting the effect of actual or considered changes in policy.

Kydland and Prescott argued that the behavior of macroeconomic aggregates had to be modelled consistently as the collective outcome of the decisions of countless identical individual optimizing agents. Most distinctively, they insisted that business cycles (now business cycle phenomena) should be accounted for in terms of the same exogenous supply side factors used in the Solow model to explain long run economic growth. Combining these elements, Kydland and Prescott prosecuted an assault on the IS-LM neo-Keynesian framework that had dominated research, teaching, and policy work in the postwar period. As in the case of Lucas and Sargent, they promised an approach that would yield superior results in the realm of forecasting and prediction. But their main objective was to effect changes in the way economists built and evaluated models that could “explain” events that had already occurred, and that is where their impact has been.

But that impact has been uneven. As of 2017, we can say this: RBC/DSGE models were used broadly in academic macroeconomics, somewhat less so in the research departments of central banks, hardly at all in economic history, and not at all in the private sector. What explains

this differential uptake? Much can be attributed to conventional objections to the realism or explanatory power of the new models. But it's not possible to understand the intellectual history simply in terms of which approaches explain or 'predict' the data best. The methods advanced to validate RBC models and distinguish among the success of their various offspring are inadequate – they are not up to the task. One needs to interpret what has happened in part as the outcome of conflicts within professional communities over the bounds of acceptable argument. In order to do that it helps to understand more of the background.

Background and Context

The economic history of the 1970s forced economists of all stripes to address two weak spots in Keynesian theory. The first was the possible role of supply shocks in contributing to business fluctuations. The second was the behavior of the Philips curve in an inflationary environment. The oil shocks of the 1970s called attention to both. Kydland and Prescott, building on ground prepared by Lucas and Sargent, exploited the disarray caused by the (temporary) reductions in potential output due to these shocks.

In the quarter century after 1948, most U.S. macroeconomists had paid relatively little attention to aggregate supply. Scholars and textbook writers certainly understood the importance of economic growth, but tended to assume that potential output grew at a relatively steady rate in the background. Crystalizing lessons learned during the Great Depression and World War II, macroeconomists focused on taming the business cycle through aggregate demand management. Through its control of countercyclical fiscal and monetary policy, government stood in the economy's wheelhouse. With an emphasis on short term analysis, potential output was taken as

temporarily fixed, and macroeconomics instruction placed little emphasis (except perhaps for a chapter at the end of the book) on what we would call today aggregate supply shocks.

The principal counterweight to neo-Keynesianism in the late 1960s and 1970s was monetarism; the two most well-known proponents of these approaches were Paul Samuelson and Milton Friedman, who aired their differences in academic venues as well as dueling Newsweek columns. The two frameworks were, however, more similar than their spokespersons were prepared to acknowledge at the time. Each located the principal source of business fluctuations in aggregate demand shifts – changes in the growth rate of nominal income that might be induced by changes in the growth rate of the nominal money supply or shifts in the IS curve (or the money demand function) that could alter velocity. Although they had different views on how empirically stable was velocity (formally, how close to vertical the LM curve was), Neo-Keynesians and monetarists formed part of the same macroeconomics language group, a fact that became much more apparent in the light of the RBC challenge. If neo-Keynesians spoke French and monetarists spoke Italian, RBC modelers spoke the equivalent of Hungarian or Finnish.

Groundbreakers for RBC Models: Lucas and Sargent

Real business cycle theorists intended to remove government from the economy's wheelhouse and replace it with a representative private household. That goal would be advanced, as had been adumbrated by Lucas and Sargent, by promulgating radically different models of how the economy operated. Classical economics, against which Keynesians rebelled, assumed that full employment – the economy at potential with all markets clearing—was the normal case, and that Keynesian “exceptions” should be viewed empirically as vanishingly

small. Lucas and Sargent (1979) described in searing terms what they saw as the challenge posed to Keynesian orthodoxy by the demise of the traditional Philips curve in the 1970s:

...we identify those aspects of this framework which were central to its failure in the seventies. In so doing, our intent is to establish that the difficulties are fatal: that modern macroeconomic models are of no value in guiding policy and that this condition will not be remedied by modifications along any line which is currently being pursued (1979, pp. 1-2).

They argued that although reduced form equations might serve as a useful basis for unconditional forecasting, the big empirical macro models could not successfully be used for conditional forecasting, because the structural parameters featured in these models would not reflect the possible impact of policy changes on them. Immediately prior to the above quoted passage, they wrote:

The task now facing contemporary students of the business cycle is to sort through the wreckage, determining which features of that remarkable intellectual event called the Keynesian Revolution can be salvaged and put to good use and which others must be discarded. Though it is far from clear what the outcome of this process will be, it is already evident that it will necessarily involve the reopening of basic issues in monetary economics which have been viewed since the thirties as "closed" and the reevaluation of every aspect of the institutional framework within which monetary and fiscal policy is formulated in the advanced countries....

Although suggesting soothingly that the framework of traditional Keynesian macroeconomics should be scrutinized to determine which parts should be discarded, they also telegraphed their expected conclusion: all of it. Because of the then dominance of traditional Keynesianism, the burden of getting a hearing was high:

So wide is (or was) the consensus that the task of macroeconomics is the discovery of the particular monetary and fiscal policies which can eliminate fluctuations by reacting to private sector instability that the assertion that this task either should not or cannot be performed is regarded as frivolous, regardless of whatever reasoning and evidence may support it (1979, p. 10).

Like Lucas and Sargent, Kydland and Prescott moved casually from a claim that new requirements and conventions would improve forecasting, to assertions about how scientific models in macroeconomics aimed at explaining how the world works had to be built. Economic historians – as well as most macroeconomists -- don't do forecasting.¹ They build models that explain or account for events that have already taken place. Many believe that there is or should be no distinction between the methods, models, and evaluative criteria appropriate for forecasting and those appropriate for explanation. Forecasting is prediction, and explanation simply retrospective prediction. Economic research aimed at forecasting, however, has additional goals: managing the economy, if one is a central banker; making money, if one is in the private sector. The best approaches to prediction will not necessarily offer the best foundation for explanatory science, and vice versa (Yarkoni and Westfall, 2017).² Purely statistical methods that forecast well may give little explanatory insight into the inner workings of an economy. Similarly, models that do well at explanation may not be useful for prediction. (Schmueli, 2010, pp. 298-299.) The connection between Lucas and Sargent's promise of superior forecasts and what they argued were better ways of modeling the world was therefore weaker and more problematic than is generally appreciated.

That said, the Lucas critique proved extraordinarily effective rhetorically, and these subtleties did not deter RBC pioneers. Four years after Kydland and Prescott's 1982 article, Larry Summers assessed their contribution to explanatory research in macroeconomics that probably mirrored the reactions of most economic historians. His judgment: "...real business

¹ Thus the common response among many macroeconomists ("not our job") when criticized for not having predicted the 2007-2009 financial crisis and recession.

² See Mukherjee (2017) for a recent example from dermatology that nicely illustrates this point.

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” (1986, p. 24).

Three decades have elapsed since then. Lucas won the 1995 Nobel prize in economics; Kydland and Prescott went on to receive the 2004 prize, and Sargent won it in 2011. Their prescriptions for what should, indeed, had to be under the hood, along with favored empirical and evaluative methods, achieved substantial penetration within mainstream macroeconomic research aimed at explanation. RBC models birthed a broader class of DSGE models. New Keynesian economists adding markup pricing among monopolistically competitive firms and Calvo assumptions about the frequency of price changes, and, having thereby introduced rigidities in the rate of price adjustment, demonstrated that many of the traditional Keynesian conclusions could be reached while adhering to at least some of the new modeling strictures. In the last decade, DSGE models began to be developed and used in central banks.

Their forecasting performance, however, remains unclear. Noah Smith (2016) describes it as “abysmal” (see also Edge and Gurkaynak, 2010). Within the private sector, there has been no take up at all of DSGE models.³

When it comes to explanation, economic historians think of themselves as modernists, to use the language of McCloskey (1986). Logic and evidence remain high among criteria used in judging the quality of an explanation. In their quest to improve their knowledge of how the

³ Smith writes, “As far as I’m aware, private-sector firms don’t hire anyone to make DSGE models, implement DSGE models, or even scan the DSGE literature. There are a lot of firms that make macro bets in the finance industry, investment banks, macro hedge funds, bond funds. To my knowledge, none of these firms spends one thin dime on DSGE. I’ve called and emailed everyone I could think of who knows what financial industry macroeconomists do, and they’re all unanimous they’ve never heard of anyone in finance using a DSGE model.” (Smith, 2014).

world works, modernists rely on observational data and the “experiments” provided by history. Theory, as a means of organizing received wisdom about empirical relationships, plays a role in these explorations. With few exceptions, however, economic historians ignored or found wanting the developing literature on RBC models. IS/LM Keynesianism, augmented by an expectations-augmented Philips curve and a more explicit treatment of aggregate supply proved adequate for most purposes (see, e.g. Field, 2011; Eichengreen, 2015).

Macroeconomic historians bring perspectives, empirical knowledge, and understanding of data sources of particular interest to the larger profession. In 2009, following the financial crisis and ensuing recession, objections to directions in theory reached a boiling point, as scholars reacted and responded to the extraordinary episode in economic history then unfolding. A number of prominent economists (Buiter, 2009, Gordon, 2009; Krugman, 2009; Solow, 2008, 2010; this is an incomplete list) publicly questioned the value of developments within academic macroeconomics since the 1980s, particularly the penetration of RBC approaches and methods and their descendants.

As economies recovered, and memories faded, temperatures cooled, and there was some falling off within the general profession of interest in what might be learned from history. Some things within macroeconomics had changed, perhaps permanently, but much had not. Still, the Great Recession contributed, as had the history of the Great Depression, to frustrating the New Classicals’ objective of banishing explanation of economic fluctuations based on changes in aggregate demand. The issues raised in the 2009 critiques remain with us and continue to be acknowledged (Korinek, 2015; Christiano, 2017; Reis, 2017).

The Impact of RBC models in Economic History

An examination of a number of metrics confirms that the RBC impact within the field of economic history has been minimal. A JSTOR search for the words ‘real business cycle’ in The Journal of Economic History for the years 1980 through 2010 reveals one full text article reference, a contribution addressing the procyclicality of TFP between 1890 and 2004 (Field, 2010).⁴ References in four book reviews also turn up. A similar search within the Economic History Review returns hits in two book reviews and none within articles. A search in Science Direct within Explorations in Economic History identifies one article using RBC methods. On the other hand, if one does a similar search in the Journal of Political Economy or the Quarterly Journal of Economics the hits are almost too numerous to count. One reaches a similar conclusion examining recipients of biennial book prizes awarded by the Economic History Association or the EHA’s dissertation prizes. Some of these works consider topics for which RBC/DSGE explanatory methods would be difficult to apply, either because of the questions addressed or because of the paucity of data. But not all.

Certainly there has been research on the Great Depression and other downturns influenced by RBC research; work by Cole and Ohanian (1999, 2000, 2002, 2004, 2016), Braun and McGrattan (1993), and the collection of contributions assembled in Kehoe and Prescott (2007) stand out. None of this admittedly small body of research has been published in the flagship journals of economic history. It’s possible, of course, that there have been opportunities, even for those who might have considered self-identifying as economic historians, to publish in general interest journals. Or (this would not preclude the former possibility) it might be that

⁴ In September 2016, an article (Watanabe, 2016) appeared in the Journal of Economic History which did address RBC explanations of the Great Depression. It was overwhelmingly critical of them.

economic historians and journal editors remained skeptical of RBC models and methods, like Summers, questioning their explanatory value. Two decades after Summers' remarks, Christina Romer said of the work of Cole and Ohanian, "I think it is wonderful when anyone works on the Great Depression. But honestly, I think some of this research is a giant step backwards" (Parker, 2007, p. 135).

The attitudes among economic historians towards what is now considered 'modern' or 'consensus' macro formed largely in reactions to the original generation of RBC models. This essay discusses critical reactions to those models, without cataloging or assessing in a systematic fashion the ways in which academic macroeconomists working within the RBC/DSGE explanatory tradition claim to have addressed them (such efforts have likely contributed to détente in 'the macro wars'). Nor does it attempt an independent examination of the forecasting success (or lack thereof) of DSGE models developed in central banks. The intent here is to assess the impact of RBC/DSGE explanatory models and methods within economic history.

The Critical Reaction

Several persisting themes emerge in the negative reactions to the initial rounds of RBC research. Mankiw was typical: reliance on technology shocks along with high intertemporal substitution of leisure to explain economic fluctuations were "fundamental weaknesses" (1989, p. 79). In the late 1980s, Summers, Mankiw and many others dismissed RBC models as lacking empirical realism. Yet in 2017 a methodological approach birthed by RBC pioneers dominates academic macroeconomics and graduate level macroeconomics instruction, while economic historians, most policy makers, and private sector economists continue to resist the siren calls of 'modern macro'.

For economic historians, the traditional Keynesian insights (underemployment equilibrium, possible paradox of saving, etc.) that seemed originally so revolutionary continue to be used and found useful in research. Almost all of these were anathema to New Classicals, who insisted on an uncompromising adherence to optimization, reflected in the principle that macro models had to assume that all markets, especially the labor market, *cleared*. Out of bounds was an interpretation in which workers were forced off their ‘notional’ labor supply curves – unable to offer as much labor as they desired at wages prevailing for employed workers with similar skills and experience.⁵ If markets didn’t clear it meant that someone was leaving money on the table, a possibility, by assumption, ruled out of bounds.

Keynesianism, as amended and augmented by the late 1970s, was not ideologically opposed to optimization. It assumed such behavior by firms and households in modelling the response of planned investment and consumer durables spending to lower interest rates, as well as how households allocated their wealth between cash and less liquid assets. Keynesian economists did, however, maintain that some markets, particularly labor markets, might not immediately clear. Accepting parts of the Lucas critique – in particular the view that the Philips curve needed to be updated to reflect the effect of inflation expectations, economic historians rejected others, including the insistence on rational expectations, Ricardian equivalence, and market clearing as essential characteristics of “good” macro research.

Over time, ideas originally considered ridiculous became accepted, and those advocating them became influential. This did not mean it was impossible for critics to get a hearing. It did

⁵ The formulation originates with Patinkin (1956).

mean that for certain issues the hurdles became higher. Such dynamics are evident in any field of inquiry. Redefined bounds of argument change the extent, on a particular issue, to which individuals' minds are open to considering it. From the standpoint of those steeped in Depression economics, many of the features of new classical models continued to appear, if theoretically clever, empirically implausible.

Consider an area where traditional Keynesians and new Classicals agreed – an increase in government spending might increase real output. In Keynesian models, fiscal stimulus from spending increases output and employment by drawing involuntarily idle labor and underutilized physical capital back into production. In new classical models, while tax cuts can't affect the real economy because of Ricardian equivalence, increases in real government spending can increase output. But the argument as to why this might occur was quite different. The action was entirely on the supply side, involving a representative household's decisions over an infinite horizon with respect to both voluntary labor supply and consumption/saving. Faced with an announced increase in government spending, households would consider themselves poorer by the amount of the current or future tax increases that would be needed to pay off the anticipated increase in government debt, and the (negative) effect on permanent income would cause them to supply more labor now and in the future. This argument, which traditional Keynesians found incredible,⁶ is widely accepted today in modern macro.

New Keynesian Models

⁶ Such thinking might be imaginable for sophisticated dynastically motivated individuals at the very top of the wealth and income distributions, but as a model of the decision making of an *empirically representative* household?

New Keynesian models generated some of the traditional Keynesian conclusions while at the same time appearing to respect some of the methodological precepts insisted upon by the New Classicals. Proponents could argue that these models were better than the original generation of RBC models because their conclusions did a better job of accounting for features of the real world. While New Keynesian DSGE models are widely described today as the ‘workhorse’ macro approach, RBC proponents weren’t persuaded (see Chari, Kehoe, and McGrattan, 2009), and traditional Keynesians didn’t see why they needed these models to explain why an underemployment equilibrium was possible.

Research originally described as ‘New Keynesian’ aimed to explain why price or nominal wage rigidity might be attributable to optimizing behavior (Mankiw, 1985). This work was neither dynamic, stochastic, or general equilibrium, but it did differ from the then current practice of simply assuming rigidity and considering consequences. At the time, this endogenizing of rigidity was considered an advance. Somewhat ironically, what came to be called New Keynesian DSGE reverted to a position that it was OK simply to consider the possible consequences of rigidity. Calvo pricing is as ‘ad hoc’ in its own way and as ‘inconsistent with optimization’ as the rigidities assumed in traditional Keynesian models. This is not necessarily a criticism.

Bounded Space Argument and Economic Correctness

In his 2016 book, Winning Arguments, Stanley Fish expanded on the idea that in any professional community, there are bounds to acceptable argument. The idea is best illustrated by considering the law. In legal discourse, some arguments are acceptable and some are not. Some types of evidence are admissible, others not. These rules are clearly specified, emphasized in

legal education, understood by all who participate in the legal community, and vigorously enforced in courts of law by judges. When an individual is on trial for robbery, for example, the prosecutor cannot introduce evidence that the individual has a long history of prior convictions, or evidence based on hearsay (Fish, 2016, pp. 130-31).

Those lacking legal training, and not part of that professional community, find much of this confusing, and even contrary to common sense. In bounded space argument, the arguments one is allowed to make and the things one is allowed to say are known in advance, specified in formal rules or in the tacit knowledge acquired by competent practitioners within a professional community. Fish's claims about the rules of legal procedure are indisputable. But he goes further, arguing that what we observe in courtrooms is an instance of a broader phenomenon. This lays the foundation for a more controversial claim: that what applies in the case of law applies equally in academics, even though academic communities lack the formal rule enforcers (judges) one finds in the law (journal referees, he suggests, might be seen as playing a similar role). He argues, for example, that within academics, disciplinary communities determine what questions may be asked and what type of arguments may be used, and which ones may not.

Whether we like it or not, Fish has a point. Academic communities bound the space of entertainable arguments and acceptable models. And this is not something that can simply be acknowledged and then deplored. It is an inherent feature of all forms of discourse and inquiry, including those that take place within academic and scientific communities. Anton Korinek (2015) reaches similar conclusions:

At its most basic level, the DSGE approach can be described as a research methodology for the field of macroeconomics. A research methodology defines the general strategy that is to be applied to research questions in a field, defines how research is to be

conducted, and identifies a set of methods and restrictions on what is permissible in the field.

A methodology consists not only of a set of formal methods, such as e.g. the powerful set of DSGE methods taught in graduate school, but also of a less explicit set of requirements and restrictions that are imposed on the researcher and that sometimes (act) more like unspoken social conventions (2015, p. 2).

Lucas and Sargent also acknowledged this reality as they mounted their assault on traditional Keynesianism in the face of the unmooring of the old style Philips curve in the 1970s:

This failure has not led to widespread conversions of Keynesian economists to other faiths, nor should it have been expected to. In economics as in other sciences, a theoretical framework is always broader and more flexible than any particular set of equations, and there is always the hope that if a particular specific model fails one can find a more successful model based on roughly the same ideas (1979, p. 5).

The reference to ‘other faiths’ suggesting that the struggle they anticipate would have features akin to religious war.

Technology Shocks as the Cause of Business Cycles

Real business cycle theory rejected the precepts of traditional Keynesianism even more categorically than had Lucas and Sargent, insisting that even in the short run fluctuations in nominal income would be neutral with respect to real variables. Kydland and Prescott suggested that short run fluctuations in output and employment were entirely due to the same types of aggregate supply shocks that drive the long run growth of potential. Here is how this idea was implemented in the initial models. An HP filter was applied to total factor productivity data, mechanically separating trend from cycle. The deviations from trend were identified as technology shocks and used as impulses generating fluctuations in output and other macro

variables.⁷ The shocks altered the marginal products of labor and capital and thus the returns to working and saving/investing. In response, people varied their labor supply and consumption/saving behavior, propagating the impulses, and generating business cycle phenomena in output, hours, consumption, and saving. Because these changes impacted the flow of capital accumulation, the effects of the impulse might persist even when the initial impulse was transitory. This channel, was, nevertheless, relatively weak; most persistence was the consequence of persistence in the shocks themselves.

It is widely accepted that TFP advance plays an important role in the longer run growth of potential output. In the RBC universe, there is no distinction between actual and potential output, so it was natural that technology shocks should be seen as significant in accounting for both the short and long run behavior of aggregates. Economic historians knew that technology shocks are predominantly positive and persistent, and if negative generally transitory. Aside from disasters like the destruction in antiquity of the library in Alexandria, or more generally, the collapse of the Roman empire, scientific and engineering knowledge doesn't typically regress. If the deviations from trend TFP truly measured technology shocks, it would be surprising if, against a background of positive trend TFP growth, negative deviations were sufficiently large to cause the level of total factor productivity to fall.

The growth rate of output is equal definitionally to the sum of the growth rate of output per hour and the growth rate of hours. If TFP continues to rise during a recession, albeit at a slower rate, then output per hour also probably continues to rise, albeit at a slower rate, so that a decline

⁷ In Kydland and Prescott the impulse series was computer generated, constrained to have the same standard deviation and autoregressive structure as the Solow residuals. Subsequent authors experimented with the use of the actual residuals themselves.

in output can only occur as the result of a sharp (voluntary) drop in hours. Economic historians found it hard to believe, given evidence on labor supply elasticities, that negative productivity shocks could account for big declines in output, such as occurred between the years 1929 and 1933, or to a lesser degree, between 1907 and 1908, 1981 and 1982, or 2007 and 2009.

To address this challenge, RBC proponents assumed high rates of intertemporal substitution of leisure, and broadened the universe of supply shocks to include legal, tax, or regulatory changes. Such changes are almost universally considered by those appealing to them to be negative in their effects on productivity. But economic historians also suspected that for a country like the United States which, since the Civil War, has experienced only moderate political disruption and minimal wartime destruction, regulatory changes stemming from legislation or judicial decision were unlikely to provide a plausible explanation for large downturns such as occurred between 1929 and 1933.

The numerous obstacles confronted by posited negative shocks in actually lowering output and employment help explain why Cole and Ohanian's accounts of the Great Depression, which emphasize the supposed high wage policy in manufacturing encouraged by President Hoover at a conference in November of 1929, and subsequently the National Industrial Recovery Act and other New Deal policies, has been viewed skeptically by most macroeconomic historians.⁸ The explanation of the "slow" growth of output between 1933 and 1937 as due to cartelization and

⁸ Cole and Ohanian's work did not actually contain a fully specified DSGE model, but was closely aligned with the Kydland and Prescott agenda. Their work underlies the analysis of the Great Depression in Chari, Kehoe, and McGrattan, 2009.

lax antitrust enforcement has likewise been greeted with some skepticism (output growth was in fact rapid under Roosevelt over those years, although it did not close the output gap).

RBC advocates cite procyclical TFP as evidence in favor of their maintained hypothesis. But such procyclicality can more plausibly be explained as an endogenous consequence of aggregate demand fluctuations, induced perhaps by labor hoarding but more likely by the inability of the private sector as a whole to get rid of physical capital (and avoid its holding costs) during recessions (see Field, 2010).⁹ While firms may choose to hoard labor, the economy in the aggregate is forced to ‘hoard’ physical capital.

Braun and McGrattan (1993) offer a distinctive analysis of the US and British macroeconomies during World Wars I and II using an RBC approach in which the impulses are not technology shocks but rather innovations in government investment, consumption, and conscription. But the analytics are premised on the RBC/DSGE explanation of why output and employment rise with an increase in government spending. In the case of World War II their argument eschews appeal to the simpler Keynesian story of multiplier effects in the face of fiscal

⁹ User costs, which capital holders must bear in bad times as well as good, are governed principally by the real rate of interest and the rate of depreciation. Depreciation flows on the entire capital stock are, contrary to widespread belief, largely invariant to utilization rates. Standard analyses treat machinery and equipment as representative capital goods (they are not) and assert that since recessions are times when capital lies relatively idle, they must also be times when depreciation flows are low (see King and Rebelo, 1998, p. 980). Mechanical equipment and perhaps some other subsets of equipment subject when operated to friction or heat deterioration may experience less wear and tear when idle. Depreciation due to changes in fashion or technological obsolescence continues unabated. Machines for which lower utilization means lower depreciation comprise only a portion of the equipment stock. And the vast bulk of the net stock consists of structures. Whether a building is full or empty, whether its rate of throughput is high or low, its roof will continue to wear out, and exterior paint will continue to oxidize at about the same rate (Field, 1985, 2010). In some cases, particularly where structures or factories are unoccupied, low utilization may actually increase depreciation. Aside from vandalism, the interiors of shuttered facilities may suffer from inadequate control of temperature, dust, or humidity. Idle machinery may suffer from lack of lubrication. The use of electricity input to proxy for variations in capital service flow is thus questionable. In criticizing work which addresses “measurement error” in TFP residuals by making adjustments to capital input based on variable utilization, Prescott (2016, p. 19) actually endorses much of this analysis of depreciation, although not the larger point about endogeneity.

expansion, accommodating monetary policy, and prewar slack in the economy. Although the authors do a nice job summarizing the macroeconomic facts during wartime, they don't offer much reason for an economic historian to believe that increased employment had anything to do with upward pressure on real interest rates leading to intertemporal substitution of labor supply toward the present (as the authors note, real interest rates didn't actually rise, and stock market performance was relatively anemic).

Later generations of RBC/DSGE models explored the possibility that since tastes are one of the two key primitives in the theory (the other being 'technology'), an economy could be subject to preference shocks which affected labor supply and/or saving behavior, and thus output and employment in different periods. In that case the preference changes would be considered impulse mechanisms. Economic historians might be more willing to entertain an influence on labor supply during the war as resulting from a preference shock resulting from patriotic appeals, but this is not a common feature of RBC narratives.¹⁰

Intertemporal Substitution

The intertemporal substitution of leisure is critical in propagating impulses in RBC models. Consumers do not just consume based on current income receipts as in a simple Keynesian model, and they are not just sophisticated Modigliani life-cycle optimizers. The utility function used by Kydland and Prescott is infinite horizon, suggesting that decision units be thought of as

¹⁰ "Although DSGE researchers have long ventured beyond productivity shocks and introduced all other kinds of shocks, productivity shocks are still the most common source of uncertainty in DSGE models, and the first type of shocks we typically tell our students to incorporate in their macroeconomic models. This prevalence stands in marked contrast to the much less robust empirical evidence on the relevance of productivity shocks" (Korinek, 2015, p. 4). The emergence of preference shocks as a category of possible impulse mechanisms in RBC models is perhaps ironic given Gary Becker's repeated insistence that appeals to changes in tastes should be a last refuge for a microeconomist.

persisting family units with strong dynastic and bequest motives. These households optimize not only over consumption/saving profiles, but also over how much labor to supply in different periods. RBC modelers ruled out of bounds the possibility that workers in 1932 or 1933 (or 1982, or 2009) might not be able to sell as much labor as they desired given wages prevailing for employed workers with similar skills and experience.

Assuming diminishing marginal utility, maximizing the discounted present value of utility over an infinite horizon implies consumption smoothing (as did Modigliani life cycle saving). Optimizing agents might therefore increase saving (and thus capital accumulation) in the face of a positive productivity shock in the current period. If supply shocks of whatever form made it more attractive to work now than later, then agents might shift labor supply to the current period.

The interactions between these two margins were mind-bogglingly complex. A snowstorm will cause chain installers along Interstate 80 leading to Lake Tahoe to drop whatever else they are doing, and head out to where the driving rain is turning to slush and snow. Clearly these individuals are engaged in some kind of intertemporal substitution, giving up leisure in the moment in exchange for the promise of more later. The smaller the region, sector, or time period examined, the more compelling are these kinds of narratives. But are they plausible metaphors for what happens over extended periods at the level of a national economy? The RBC explanation of the drop in employment during the Depression is that hundreds of thousands of people decided voluntarily to withdraw their labor because a bad technology draw worsened the relative attractiveness of working in 1930, 1931, 1932, 1933, as compared, say, with 1942 or 1943, with the better opportunities in the latter years rationally anticipated.

Labor supply (work/leisure) decisions are affected by both income and substitution effects, which may offset each other. This is also true for the saving/consumption decision. Considering only the current period, increased voluntary labor supply could therefore be the direct effect of a positive *or* a negative supply shock on wages, and the same is true for decreased supply. The microeconomic evidence on labor supply suggests that within the range within which real wages have tended to fluctuate, income and substitution effects roughly balance in the aggregate, leading to an approximately vertical aggregate labor supply schedule. The relative strength of income and substitution effects varies by age, gender, and other demographic groups. But overall, it's a wash (Kimball and Shapiro, 2008, p. 1).

It is sometimes suggested that RBC proponents simply ignore income effects. In fact, policy preferences often imply the view that income effects dominate among the poor and substitution effects among the wealthy, leading to a “growth agenda” emphasizing higher taxes net of transfers for the poor and lower taxes for the wealthy. For the most part, however, RBC models do not incorporate heterogeneous agents. MaCurdy (1981) provides a detailed theoretical and empirical treatment of labor supply response within the context of a life cycle model similar to that used in RBC models. His estimate of the elasticity of labor supply for prime age males working continuously is quite low: .15. RBC modelers needed to insist on a much higher intertemporal substitution of leisure (labor supply elasticity) in response to a productivity shock in order to motivate the coincidence of large voluntary changes in employment over the business cycle in the face of relatively small changes over in real wages. They dismissed MaCurdy's estimates on the grounds that hours of work were not continuously variable; the appropriate margin for aggregate analysis was whether one was in the labor force or not.

The initial RBC models employed log linear utility functions, which have zero *static* wage elasticity: at a point in time the income and substitution effects due to a change in wages are exactly offsetting. This might seem to preclude the possibility that positive supply shocks could induce higher contemporaneous labor supply now and/or at any time in the future. The channel had instead to run not directly from changes in the expected time profile of real wages but indirectly through the effect of the shocks on wealth (permanent income) interacting with consumption smoothing. An increase in current period labor supply was thought of as a response to the temporarily higher interest rates consistent with the transition to a new (and higher) capital-labor ratio which led to the allocation of a higher proportion of output to saving and thus capital accumulation, as in Braun and McGrattan (1993) or Plosser (1989).¹¹ Thus one might still observe an association between higher wages resulting from the supply shock and increased labor supply (and output). Prior to the 1980s claims of such complex calculations were widely considered ridiculous (Mankiw, 1988, p. 444), but for much of the profession they became standard operating assumptions.

Microfoundations

Within the confines of the new bounds of acceptable argument, microfoundations became a *sine qua non* of good macro modelling. Questions about what this principle actually means, or how it is actually implemented in new classical models will now frequently be dismissed as in the category of settled, previously considered issues (see Romer, 2012, p. 232, or Blanchard, 2016, p. 3). One might think that this requirement reflects an insistence that modelling

¹¹ “Thus in the near term real interest rates rise, which induces intertemporal substitution of current for future work effort” (Plosser, 1989, p. 60).

assumptions be empirically realistic. A modeler committed to greater realism and possibly better prediction might want, for example, to exploit the fact that higher income individuals have lower marginal propensities to consume than do lower income individuals. The use of a representative agent rules this out, avoiding the aggregation problems associated with adding up the decisions of tens of millions of heterogeneous optimizing agents (Kirman 1992). The insistence on a representative agent is a matter of convenience more than anything that can be defended as realistic on empirical grounds. Milton Friedman advised us back in 1953 to stop worrying about the realism of assumptions. In that sense he was a supporter of the mantra that “all models are false”, echoed by Prescott.

In practice, ‘microfoundations’ for macro has meant that all agents optimize by maximizing a utility function subject to constraint. Since a particular theoretical specification may lack an empirical basis, judgements about whether it is acceptable can end up influenced by aesthetic or other considerations. The sometimes arbitrary designation of some but not other modeling practices as acceptable is a feature common to the consolidation of an intellectual revolution.

Calibration and Criteria for Empirical ‘Validation’

RBC modelers constructed an artificial economy, fed it impulse shocks similar to those experienced in the real world, and compared features of the model’s output with features of actual macroeconomic time series. Building blocks typically included a representative agent who inherited capital from the past, a Cobb-Douglas production function, and, in the original generation of models, a log linear utility function defined over consumption and leisure. Budget constraints insured that total output equaled total income equaled total expenditure, mimicking

the national income accounting identities. Modelers selected what they believe to be plausible parameter values including labor's share, the discount rate, and the rate of depreciation on capital. Technology shocks were extracted as the deviations from a TFP trend calculated using an HP filter. The model was "solved" numerically by inputting technology shocks similar to those extracted and having the model generate time series for output, hours, consumption, investment, the capital stock, etc. Standard deviations and co-movements of the generated series were then compared with those of data from the actual economy. A high degree of correspondence was treated as evidence for the success of the model.

Calibration involves tweaking parameters in pursuit of an even better correspondence. This iterative process involves the comparison of model output with the second moments and co-movements of actual macro series. The facts that the standard deviation and autocorrelation of technology shocks are extracted from actual TFP data, that the model parameters are sometimes estimated econometrically, and that the artificial data are, as described, benchmarked against actual data leads proponents to describe their inquiry as both scientific and empirical. But, as Milton Friedman observed in a 1997 letter in the Journal of Economic Perspectives, "There is a world of difference between mimicking and explaining, between 'can or may' and 'does'" (1997, p. 210).

RBC proponents were unclear about whether they saw themselves engaged in estimation, hypothesis testing, or something else entirely. Even if we could agree that the exercise of comparing second moments and co-movements has bearing on the validity of the model, proponents typically eschewed formal empirical tests of the degrees of correspondence. They simply eyeballed the results, pronounced themselves satisfied, and moved on. It is true that the

choice of parameters was often justified by appeal to empirical data. But there was no mechanism in these efforts that would allow us systematically to judge whether this is a good or bad model of the world. Trying to formalize the correspondence or goodness of fit tests is an interesting exercise (Watson, 1993) but doesn't get to the heart of the challenge posed in validating these models, where the tests applied have little scientific foundation and are largely a matter of convention (Korinek, 2015, p. 7).

In 2009 Emi Nakamura published a critique of RBC/DSGE models and methods. She showed that

in a model with the same basic structure as the bare bones RBC model, monetary, cost-push or preference shocks are equally successful at explaining the behavior of macroeconomic variables. Thus, the empirical success of the RBC model with respect to standard RBC evaluation techniques arises from the basic form of the dynamic stochastic general equilibrium model, not from the specific role of the productivity shock (p. 739).

Nakamura's exercise demonstrated that RBC evaluative methods have difficulty discriminating among different types of aggregate demand or aggregate supply shocks as possible causes of business cycles. If, within the confines of these models, widely different types of impulses could generate similar degrees of correspondence between second moments and co-movements, the "standard RBC evaluation techniques" can't provide information relevant to which of them may be important in causing business cycles. These conclusions reinforced arguments made earlier by Hansen and Heckman (1996, p. 1).

RBC proponents counsel scholars from other traditions to write down an optimizing model reflecting their views, generate model output, and see if it can match the second moments and co-movements of actual data. Why is this good advice, if this "test" can be passed by models

with such widely varying impulse mechanisms? Prescott, of course, had written that one shouldn't judge a model by how well it fit the data, but rather by how much confidence one has in the theory underlying it (1991, p. 171). The title of Nakamura's article, "Deconstructing the Success of RBC Models" befits its subject matter: RBC proponents might be considered our first postmodern economists.

Apparently near the limits of currently acceptable argument, Nakamura's paper met with a fate worse than criticism. As of the end of 2016, Google Scholar reported that it had been cited exactly *twice*. No one likes the implication that a research agenda one has pursued for years is not in fact advancing our understanding of how the economy works. Nakamura is too polite to state such implications directly. No such reticence inhibited Lucas and Sargent in 1979 as they challenged traditional Keynesianism.

1978 Era Macro

Robert J. Gordon's essay, "Is Modern Macro or 1978 era Macro More Relevant to the Understanding of the Current Crisis?" was also written in 2009, the year of peak reaction to 'modern macro.' Gordon answered his rhetorical question by maintaining that the synthesis reflected in the 1978 publication of his intermediate macro textbook and that by Dornbusch and Fischer continued to provide a more relevant and useful framework for actually understanding economic history as it was then unfolding than did modern macro. Gordon argued that 'consensus' New Keynesian DSGE models enmeshed themselves in a "web of contradictions" by incorporating price rigidities which were incompatible with market clearing. Price rigidities drive a wedge between 'notional' and 'effective' labor demand curves, and once one allows that workers might be off their labor supply schedules, it no longer matters what one assumes about

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their labor supply elasticities. Gordon developed a variety of other criticisms, in the process challenging Lucas's argument that because the Philips curve needed updating, the entire corpus of Keynesianism needed to be discarded. His manuscript has not been published.

These are not purely intellectual, theoretical, or rhetorical issues. The survival of a preference in some circles for 1978-era macro has meant that we now have a better understanding of the Great Depression, or the Great Recession, than would have been the case had economic historians as a group gotten with the program. The consequences of the RBC initiative extend beyond the diversion of resources. Aside from consuming intellectual energy, the revolution has done practical damage to our ability to understand and manage the macroeconomy, most specifically by corrupting our understanding of output gaps and how we calculate them. The HP filter is now used widely by RBC and non-RBC economists alike to generate trend as a proxy for the evolution of potential. This has propagated a 'consensus macro' conception of the business cycle in which output gaps are symmetrically distributed above and below potential. An older consensus saw deviations of actual from potential as *asymmetrically* distributed below potential, a view shared by traditional Keynesians as well as monetarists like Friedman, who endorsed it forcefully and supported it empirically in developing his plucking model (1963, 1969).¹² Insistence on the symmetrical view leads to this problem: many years in which the economy is identified as above potential will be associated with disinflation or declining prices, a clear violation of the definition of potential.

Conclusion

¹² See also deLong and Summers, 1988.

RBC proponents persuaded themselves, and a number of others, that they had developed a new scientific methodology. But it was never clear whether it was about hypothesis testing, or estimation, or something else entirely. Much faculty and student attention is now devoted to explaining the outline of the RBC research program and its progeny as well as critiques of it. In the process, the balance of macroeconomics research and in particular graduate level instruction has shifted subtly from studies of the characteristics of the economy to studies of models per se.

RBC models and their descendants tells us little about whether the principal source of business cycle fluctuations are aggregate demand or aggregate supply shocks. They contributed little to the diagnosis and treatment of the financial crisis of 2007-2008. They are not used by private sector analysts or most policy makers. Relatively few people today believe that negative supply shocks were responsible for either the Great Depression or the Great Recession. Relatively few believe that labor supply is subject to a high intertemporal elasticity of substitution. If Kydland and Prescott wanted most of the profession to accept these propositions they have largely failed.

But method was always front and center in the RBC initiative. As Prescott said in his Nobel lecture, “What I am going to describe for you is a revolution in macroeconomics, a transformation in methodology that has reshaped how we conduct our science (2006, p. 205). Nakamura’s work shows that the evaluative methods proposed – casual measures of the degree of correspondence between standard deviations/co-movements in model output and in actual macroeconomic aggregates, cannot adequately validate these models. RBC proponents have both claimed that it does and denied that it should. Whether the development of RBC models shunted the car of macroeconomic science onto the wrong track for generations is a judgment to

be rendered by future historians of economic thought. What is certain today is that it has imposed a heavy opportunity cost and, at a minimum, corrupted our understanding of potential output and the estimation of output gaps.

In 1988, Greg Mankiw allowed that empirical and policy research had benefited little from then recent developments in macroeconomic theory, but defended this disjuncture with an analogy drawn from the history of planetary motion. It would take time, he said, for new theoretical inquiry to yield practical results (1988, pp 437-48). It is now three decades later. Macro theory has developed along narrow paths, constrained by more or less stringent insistence on rational expectations and different interpretations of the implications of the Lucas critique, unmoored by evaluative methods capable of validating the different embodied stories, all ultimately justified by the promise of superior conditional forecasts that have yet to be delivered. Most economic historians reveal by their behavior that they remain unconvinced that RBC models and their descendants offer tools useful for their craft.

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