Riddles and Models: A Review Essay on Michel De Vroey's *A History of Macroeconomics from Keynes to Lucas and Beyond*†

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This essay reviews Michel De Vroey's important new book on the history of macro-economics, which extends to business cycles an earlier book by the same author on the history of involuntary unemployment. The review also offers a broader nontechnical survey of the issues and models that make up modern macroeconomics, including a reckoning of what we have learned since John Maynard Keynes and of the discoveries that still lie ahead. (JEL B22, B41, E12, E13, E32)

1. Introduction

Macroeconomics became a distinct field in the 1930s as a byproduct of inquiries by John Maynard Keynes and some contemporaries into the causes of, and cures for, mass unemployment and great contractions. In the eighty years since the *General Theory of Employment, Interest, and Money* was published, depressions and mass unemployment have lost their leading position as

economic maladies in advanced economies, except perhaps for brief periods in the stagflation of the 1970s and the Great Recession of 2008–09. Business cycle issues now share top billing in the field with other riddles in growth and development, asset prices and bubbles, invention and innovation, institutions and political economy, income and wealth inequality, banking and credit, globalization, and others.

Today's recessions feel more benign than they were in Keynes' time. Cycles have greatly moderated since 1945. Our diagnostic, descriptive, and policy tools have grown quite sophisticated, especially since the early 1970s when Robert Lucas's path-breaking article "Expectations and the Neutrality of Money" (Lucas 1972) introduced dynamic stochastic general equilibrium (DSGE) in macroeconomics. Michel De Vroey gives an excellent account of how the field evolved

from the methods and agenda of Keynes in the 1930s to those of Lucas in the 1970s, and all the way back to the New Keynesians (NKs) in this century. The account is detailed, respectful of the protagonists, and as fair as one can hope from someone with strong Keynesian priors. Those priors include ambivalence about "classical" assumptions like market clearing and rational expectations, and a strong attachment to the concept of involuntary unemployment.

Debating classical assumptions is central to this book, occupying considerable space in chapters 5, 6, 7, 19, and elsewhere. The author makes much of the methodological divide between Marshallians and Walrasians. For readers who are not well versed in the history of economic thought, Marshall-style economics stresses short-run quantity adjustments to external shocks, while taking prices and price expectations as given. Walrasian economics looks at the entire adjustment process for quantities, prices, and price expectations, in both the short run and the long run. Macroeconomics before Lucas had a distinct Marshallian flavor that evaporated with the rational expectations revolution in the 1970s.

Important as it is to recognize one's intellectual origins, DeVroey's emphasis on Keynesiana and Walrasiana keeps the book away from what the author himself defines, correctly in my view, as the quest of Keynes and his followers: identifying market failures on which the government should act (p. 176). Little space goes to listing those failures, or discovering their causes in policy events, financial frictions, externalities, panics, or other adverse shocks.

De Vroey's list of milestones marking the highway from Keynes to Lucas includes the invention of the Keynesian cross by John Hicks in 1937; the resurgence of monetarism in the 1950s; the natural rate of unemployment in the 1960s; mid-century disequilibrium macroeconomics from Don Patinkin to Edmond Malinvaud; the

introduction of rational expectations in macroeconomics by Lucas and Thomas Sargent in the 1970s; staggered wage contracts and other types of nominal rigidities in the 1970s and 1980s; the development and calibration of real business cycle (RBC) prototypes by Finn Kydland and Edward C. Prescott in the early 1980s; and more recent work of Jordi Galí, Julio Rotemberg, and Michael Woodford on the foundations of the NK paradigm in the late 1990s.

Additional markers on the road to modern macroeconomics would, and should, recognize what we have learned outside the narrow field of business cycles. Our newfound knowledge includes tools and ideas drawn from the one-sector and multi-sector growth theory of Robert Solow, Trevor Swan, Hirofumi Uzawa, David Cass, and Tjalling Koopmans in the 1950s and 1960s; from the endogenous growth theories of Paul Romer, Lucas, Philippe Aghion, and Peter Howitt in the 1980s and 1990s; from the consumption-based asset pricing models of Lucas, Rajnish Mehra, and Prescott in the 1970s and 1980s; from work on bubbles, panics, coordination failures, and multiple equilibria (ME) by Cass and Karl Shell, Douglas W. Diamond and Philip H. Dybvig, Russell W. Cooper, and Andrew John, Roger Guesnerie, and Jess Benhabib and Roger E. A. Farmer in the 1980s and 1990s; from work on political and institutional economics by William Nordhaus, Douglass North, Alberto Alesina, Guido Tabellini and Torsten Persson, Daron Acemoglu, and James Robinson over a longer period of time; on wealth and income inequality from Thomas Piketty and Emmanuel Saez over the last fifteen years; on monetary economics from Robert Townsend, Nobuhiro Kiyotaki, and Randall Wright since 1990; and surely some fundamental advances in general equilibrium, game theory, and information and uncertainty from authors who are known to everyone.

What have we learned from macroeconomics as we moved from Keynes to Solow to Lucas, and beyond to Prescott and the NKs? What important phenomena have we sought to explain? Have we fallen short anywhere? If so, what can we do to improve our models as predictors of reality, or to better the quality of our advice to policy makers? Sections 2 and 3 of this essay begin with a review of macroeconomic history through Michel DeVroey's eyes, stressing cyclical fluctuations in Marshallian and Walrasian economies with nearly homogeneous households and no financial frictions.

Much as we learn from that tour and from the author's long experience as a consummate expositor of economic ideas, De Vroey's reach is limited by his choice to soft-pedal growth theory, neglect financial markets as a major cause of coordination failures, and ignore the overlapping generation (OLG) model, which does not rate a single reference in the entire book.

Growth theory, in particular, is an unfortunate omission, for it furnishes the entire toolkit of modern macroeconomics, including the study of business cycles in which total factor productivity (TFP) now plays a leading role. Graduate macroeconomic teaching begins with growth models for two good reasons: it is very difficult to study the response of an economy to an external shock without an adequate understanding of growth dynamics; and it is equally hard to divorce trend from cycle without controversial assumptions about filtering time series data.

To shine a wider light on modern macroeconomics, section 4 adds statements from Keynes, Lucas, and others who suggested or explicitly articulated research agendas for the field. After a cautionary tale from ancient astronomy in section 5, sections 6 and 7 review the tools and narratives of macroeconomics; section 8 describes explained and

continuing puzzles. Section 9 sums up the intellectual legacy of Keynes and the NKs and compares it with that of Lucas and his epigones. Finally, sections 10 and 11 look to the future with a list of issues that seem both important and under-researched.

2.   *From Keynes To Lucas: A Guided Tour*

2.1 *Beginnings*

From its beginnings in the age of Keynes to this day, macroeconomics has dealt with important and controversial social issues that command the attention of thinkers all over the globe. At the heart of it all was the quest to understand and tame business cycles. Bank panics and mass unemployment have been with us long before Keynes, since the beginning of capitalism, adding grist to the mill of social theorists like Karl Marx, who thought of trade cycles as a virulent disease that could only be cured under socialism.

Was Marx correct in his assessment? Could capitalism, and the democratic institutions that went with it, survive the periodic, and occasionally deep, contractions that rocked economic life? This became the paramount question among social scientists in the 1930s, as the Great Depression in the United Kingdom and the United States lingered on much longer than anyone expected. Macroeconomics was, and to a large extent still is, the collective response of the economics profession to this question.

De Vroey's *A History of Macroeconomics from Keynes to Lucas and Beyond* gives an expertly guided tour of how that entire response to mass unemployment began and developed though narratives of ideas, formal models, empirical work, and policy proposals from the 1930s to now. The book packs enough detail for a one-quarter, or even full-semester, course on the history of economic thought. It is informed, satisfying, and

authoritative, much like the Michelin Red Guide to Paris.

2.2 *The Ebb and Flow of Ideas*

Keynes and Lucas are the leading figures in De Vroey's historical account, with Keynes commanding the first thirty years of macroeconomics, roughly 1940–70, Lucas commanding for the next thirty, and an uneasy balance between Keynesians and Lucasians marking the most recent twenty years. This battle of ideas is dotted by transformational events, or "breaches," as De Vroey chooses to call them. Breach one was Keynes himself, together with some early proponents of Keynesian macroeconomics like Hicks (1937), Modigliani (1944), and Klein (1950). Ideas by Friedman (1968) and Phelps (1968) on the natural rate of unemployment mark breach two, while breach three is a long-lasting attempt by Patinkin (1956), Clower (1965), Leijonhufvud (1968), Barro and Grossman (1971), Benassy (1975), Dreze (1975), and Malinvaud (1977) to lift the Manhallian macroeconomics of Keynes to the technical level of Walrasian general equilibrium through quantity rather than price adjustments. The novel concept was "disequilibrium," which clears markets by rationing the long side of each one when prices or wages are rigid or predetermined; the goal was to find how rationing in goods or factor markets depended on exogenous selections of prices and wages.

Breach four was DSGE, a radical departure from Keynes, led by Lucas (1972) who emphasized market clearing and rational expectations. Sargent (1976) and Barro (1977) were important early contributors in this endeavor to remake macroeconomics on classical or Walrasian microfoundations.

De Vroey identifies as breach five a number of related ideas in the 1970s and 1980s that focus on imperfections in factor markets arising from incompleteness or private information. The end results of these frictions were labor contracts, efficiency wages, and credit rationing. Important contributors include Fischer (1977), Taylor (1980), and Stiglitz and Weiss (1981).

Before frictions in factor markets had time to grow roots in the literature, they were swamped by the next wave of ideas, the RBC model of Kydland and Prescott (1982). Breach six was in effect the second coming of DSGE, now cast in the language of the Cass–Koopmans model of optimal economic growth. Augmented by persistent shocks to the aggregate production function, the Cass–Koopmans model underwent fluctuations in national income, and in its main components, which looked like postwar business cycles.

On the minus side, RBC models provide no guide for economic policy because all fluctuations are socially optimal, particularly so for monetary policy, which has no place in the optimum growth model. Central bankers were understandably concerned about this feature and highly receptive to the last breach on De Vroey's list, breach seven. This one occurs mainly in the late 1990s and marks a partial return to Keynesian ideas and models like the investment–saving (IS) schedule and the Phillips curve. Among the protagonists of this reversal are Calvo (1983), Taylor (1993), Galí (1999), Rotemberg and Woodford (1997), and Christiano, Eichenbaum, and Evans (2005).

2.3 *The Economics of Keynes*

De Vroey takes great pains to distill Keynes's research program without much help from the *General Theory*. His final list comes down to three items:

(i) Splitting unemployment into frictional or "normal" and involuntary or "abnormal," the latter being a symptom of labor rationing and a byproduct of insufficient aggregate spending.

(ii) Understanding the role of increased uncertainty (lack of reliable "news")

about the outcome of investment decisions.

(iii) Accomplishing all this in a mixed Marshall–Walras model of perfect competition without rigid prices or wages but possibly with incorrect expectations over the short run.

Keynes's failure to deliver on item (i) of the list is connected in De Vroey's eyes with item (iii): how can labor rationing occur without wage rigidity? Missing entirely from that list are "animal spirits," which both Keynes and Arthur Cecil Pigou thought relevant for investment. De Vroey dismisses this channel early on (p. 8) because, like Keynes and Pigou, he believes animal spirits to be fundamentally irrational and thus at odds with perfect information.

Modern macroeconomics, however, gives us new ways to understand bubbles as rational behavior, following Tirole (1985), and to disengage rationing from price rigidity by appealing to private information, following Stiglitz and Weiss (1981) or Kehoe and Levine (1993). For example, labor rationing can be a byproduct of credit rationing. A reduction in collateral values can prevent productive firms from borrowing enough to rent capital or hire workers from unproductive firms. Saddled with too much capital and labor, contracting firms adjust by laying off some workers.

2.4 *The Neoclassical Synthesis and Beyond*

A desire to combine the static period-by-period disequilibrium of Keynesian theory with the long-period equilibrium of classical theory quickly gained ground among the epigones of Keynes. Perhaps the most innovative work came from Klein and Goldberger (1955). It was a structural economic model of the US economy consisting of fifteen equations and five identities, seeking to predict economic activity and simulate the impact of alternative policies.

Based on a dynamic version of the Hicks investment–saving liquidity preference–money supply (IS–LM) framework, the Klein–Goldberger (1955) model featured capital accumulation and technical progress through an investment demand schedule. It was estimated from US data using limited-information maximum likelihood, a novelty at the time. Despite some theoretical weaknesses related to wage adjustment, this was the first successful macroeconometric model, and the beginning of the economic forecasting industry in the United States. Improvements soon followed, chief among them being the MPS (or MIT–Penn–Social Science Research Council) Model coordinated by Franco Modigliani and Albert Ando. Praising Klein for this achievement, Lucas (1977, p. 219) describes it approvingly as "a fully articulated artificial economy which behaves through time as to imitate the time series behavior of actual economies."

Theoretical attempts to reconcile Keynesian economics with general equilibrium continued apace with the first edition of Patinkin's 1956 textbook, and unfolded over a period of twenty years, terminating with Malinvaud's explanatory essay in 1977. De Vroey's chapters 6 through 8 pay much attention to this literature, called "disequilibrium" in North America, and "quantity rationing" in Europe, where it achieved its greatest popularity. The common thread in this endeavor is to slow down price adjustment or stop it altogether. One popular device is to fix prices and assume markets to clear by rationing the long side—sellers if price is too high, buyers if the price is too low.

With a benefit of half a century's hindsight, it is hard for De Vroey, or anyone else, to see the point of this exercise, especially since the disequilibrium literature never provided a convincing account of the factors responsible for the slow price adjustment. What seems to have attracted many economists

to the futility of disequilibrium analysis is a widely shared belief that Walrasian equilibria are grossly unrealistic because they pay no attention to market imperfections or allow for the possibility of market failure. When the chips are down, De Vroey agrees with Axel Leijonhufvud's aphorism that Keynes and Walras were incompatible bedfellows.

2.5 *The Natural Rate of Unemployment*

Just after Phillips became enshrined as an icon of macroeconomic orthodoxy, the first cracks appeared in the Keynesian edifice. The work of Phillips suggested a large non-neutrality of money: by printing inflationary currency, the government lowers the rate of unemployment. This easy path to higher real incomes soon became too big a target for both theory-oriented and policy-minded economists. Two of them, Milton Friedman and Edmund Phelps, wrote influential papers arguing that if expectations of future inflation gradually adjusted to actual inflation, then there was no long-run trade-off between unemployment and inflation. Monetary policy was neutral in the long run, and probably in the short run as well, if expectations reacted quickly to observations.

Friedman's argument was not as sophisticated as Phelps's, which went deeper into the interplay of vacancies and unemployment, providing the impetus for subsequent extensions by Diamond, Dale Mortensen, and Christopher Pissarides that were to shape macroeconomic analyses of the labor market for many years to come. Friedman still won the popularity contest over Phelps by inventing the term "natural rate of unemployment," a term that survives to this day.

Was the natural rate of unemployment a useful construct? My own take is that it was not much better than Knut Wicksell's "natural interest rate." Natural rates are not operational concepts we include in our national income accounts; nobody knows what those rates are in the United States now or in any past year. Their main purpose seems to serve as a reminder that monetary instruments are not very useful in manipulating labor market outcomes, except perhaps over a short time horizon. If we want less unemployment, we should do something "real."

2.6 *Lucas and Dynamic General Equilibrium*

If Keynesian macroeconomics was an imposing milestone in a collective attempt to tame the Great Depression, what events drove Lucas and a few others to reject Keynes in the 1970s? De Vroey takes this question seriously and devotes the bulk of chapters 9 through 11 looking for answers. One good guess is that the mere passage of time weakened memories from the 1930s, and lessened the urgency to explain what had gone wrong. Another story is the spread of previously unavailable technical tools from economic theory and econometrics. A third one is, as De Vroey puts it on p. 203, a potentially utopian "ambition to straddle external and internal consistency," that is, to bring internal logic and respect for facts under the same methodological roof.

Since Lucas was one of the earliest practitioners of Walrasian macroeconomics in the Keynesian age, De Vroey views his work as another stage in the long struggle between macroeconomic opposites: Marshallians vs Walrasians, disequilibrium vs equilibrium, states of adjustment vs states of rest. My personal recollection as a naïve graduate student in the early 1970s is a bit different. What sticks in my mind are images of a group of young faculty dissatisfied with Keynesian orthodoxy at the beginning of a stagflationary decade when both unemployment and inflation were high. Lucas and his friends seemed, to my untrained eye, to be looking for something beyond IS–LM, Keynesian multipliers, and the Phillips curve. What

would empower monetary policy to change real outcomes? Was macroeconomics compatible with general equilibrium as it was understood by Hicks (1946 [1936]) in *Value and Capital* (1946) or by Phelps (1970) in the introductory essay of the collection that everyone called the "Phelps volume"?

In his seminal 1972 piece "Expectations and the Neutrality of Money," Lucas put together a full-blown DSGE model that displayed monetary non-neutrality under flexible prices and rational expectations. This was the very first formal general equilibrium model in the history of business cycle analysis, and one that sought to demonstrate that Walrasian macroeconomics was both possible and interesting. The theoretical framework was an OLG model with a two-period life cycle and two built-in frictions: limited information and a dynamic inefficiency. The latter friction generated a demand for a bubble-like asset like fiat money; the former one led to confusion between technology shocks and monetary ones. Real national income would change when producers could not separate monetary shocks from technological ones. Unanticipated monetary shocks could, in principle, move GDP even if anticipated ones might not.

Empirical research in the middle 1970s did not lend much support to this artificial distinction between innovations in money supply and anticipated changes, partly because variations in monetary aggregates quickly became public knowledge, and also because some anticipated changes, like the inflation tax, are known to be non-neutral. Empirical validation for neoclassical business cycle theory would have to wait another ten years until Kydland and Prescott (1982).

2.7 *Reactions and Counterreactions*

Disillusionment with Keynesian macroeconomics was not confined to a small coterie of young theorists who sided with Lucas. It also affected econometricians like Sims (1980), who thought that Keynesian macroeconomics was based on haphazard, careless, or poorly justified identifying restrictions. For a brief period, Sargent and Sims (1977) proposed to analyze business cycles as atheoretical VARs, that is, vector autoregressions, in which transient shocks drive real outcomes; or as SVARs, that is, structural VARs with some prior restrictions drawn from economic theory.

Older Keynesians reacted negatively to the DSGE approach as exposited in the 1972 Lucas article and in a companion piece (1976) on the pitfalls of mainstream economic policy evaluation. Their main line of argument was to rule out short-run market clearing as a suitable description of the goods and factor markets, without offering a credible explanation why supply and demand stayed apart.

Younger Keynesians were much more inventive than their elders in explaining how market frictions and increasing returns could lead to coordination failures and policy interventions. De Vroey dedicates chapters 13 and 14 of his book to an overview of various stories of market failure. Almost all of these stories are plausible but regrettably static; nearly half are firmly micro-founded—labor contracts, efficiency wages, menu costs, wage bargains, search externalities, ME, and staggered wages all receive some attention, and a few (staggered contracts, search externalities) are laid out in commendable detail.

Few of these models enjoy continued use in our time, except for Diamond's matching function, which forms the backbone of modern search theory, and staggered price setting, which retains considerable popularity in current NK modeling in the form of "Calvo pricing." Missing from De Vroey's list is the most enduring Keynesian idea from the 1980s, the credit-rationing model of Stiglitz and Weiss (1981) and the legacy it has spawned in the study of financial markets.

3.   *From Lucas to the New Keynesians*

3.1 *Real Business Cycles*

Was the theory of RBCs a watershed event for modern macroeconomics? De Vroey rightly thinks so and devotes chapters 15–17 of his book to introducing, analyzing, and evaluating what Kydland and Prescott (1982) contributed to our understanding of post–WWII business cycles. De Vroey's summary judgment is that in the 1980s, Kydland and Prescott did for Lucas what Hicks, Modigliani, and Klein had done roughly one generation earlier for Keynes: they quantified original thinking and made it operational for a large mass of colleagues.

The particulars of this endeavor are well-known; see section 6.3 for a longer summary. Kydland and Prescott (1982) gave up on the idea that cycles were started by monetary surprises, embracing the alternative of TFP shocks. To model those, Kydland–Prescott added persistent technology shocks to the stochastic version of the optimum growth model due to Brock and Mirman (1972). Data discipline came through "calibration," a new methodology of soft hypothesis testing that avoided the trouble of building and estimating structural economic models. Section 8.4 surveys the pros and cons of the calibration and estimation methodologies.

Calibration is simple and intuitive. It typically uses parameter values for tastes, technology, and endowments drawn from microeconomic data, panel observations, industry empirics, and the like. Then one adds highly persistent shocks to TFP and calculates the artificial model's equilibrium, generating imaginary time series on GDP, and its major components, and on labor supply and some other endogenous variables of interest. The last, and most important, task is to compare artificially generated time series with postwar US data. At the end of this procedure, Kydland and Prescott (1982) found

that their model matched pretty well the dispersion of US time series, except for one small flaw: hours worked fluctuated much more than wages in the data, about the same in the model.

That much empirical success was remarkable for a construct with no frictions of any kind: no public goods, no money or credit, no private information or adjustment costs. The model gained popularity at what seemed an exponential rate, attracting fans, practitioners, and imitators among young economists near the Great Lakes and elsewhere. The University of Minnesota, Carnegie Mellon University, the University of Rochester, and the University of Chicago rapidly established themselves as the vanguard of the RBC world.

Mainstream academics along the two coasts were less impressed with the Kydland–Prescott innovation, quickly suggesting that macroeconomics had split into two imaginary tribes—"freshwater" or Lucasian, and "salt water" or Keynesian. Solow (1988), the most respected "salt water" macroeconomist, seemed among the least impressed with RBC, arguing that growth models were built to study long-run phenomena, not business cycles, and that the RBC framework rules out many of the phenomena that macroeconomics had been invented to explain, like strategic interactions and market failures. Both of Solow's objections turned out to be prescient, as we shall see in sections 6 and 7 when we evaluate workhorse macroeconomic models.

The central role accorded to TFP within the RBC model also raised eyebrows among empiricists. Regarded by growth accountants (Abramovitz 1962) as a "measure of our ignorance," TFP suddenly became the main driver of postwar cycles, responsible for roughly two-thirds of output volatility in the United States. We will discuss in sections 6 and 7 why it is intellectually risky to give such a big role to a murky and poorly identified concept like the aggregate TFP.

De Vroey regards RBC as a valid intellectual paradigm that provides a potentially useful description of reality, even though many of its underlying assumptions abstract a great deal from the working of modern market economies. He credits Kydland and Prescott (1982) for contributing to our understanding of "normal fluctuations," preferring to leave the analysis of rare events like the Great Depression to economic historians. Two faults he finds are that RBC conflates equilibria with optima and, in a tribute to his own Marshallian roots, he is skeptical once more of the postulate that markets are always in equilibrium. When it comes down to policy issues, DeVroey prefers to be guided by NK ideas. We turn to these next.

3.2 *New Keynesian Macroeconomics*

The appellation "New Keynesian" applies to a family of macroeconomic models in which neither welfare theorem holds. Equilibria are never optima, and optima cannot be obtained as monopolistically competitive equilibria, no matter how clever policy makers can be. These models share three building blocks described in some detail by De Vroey in chapter 18: Dixit–Stiglitz monopolistic competition at the industry level, leading to markups of commodity prices over unit costs; staggered Calvo pricing leading to price rigidity; and an inflation-stabilizing Taylor rule for monetary policy. First-order conditions for the aggregate household and monopolistic firms generate an IS curve and a Phillips curve, which combine with an interest-rate setting Taylor rule into a system of three equations. These equations look much the traditional IS–LM framework of the "neoclassical synthesis," now augmented with time lags and derived from something that looks like an optimizing DSGE framework.

A clever monetary policy, this setting fully neutralizes price rigidities because, under the optimal policy, no firm will want to change prices, even if it was authorized to do so, and output would be as large as if prices were fully flexible. To be sure, free entry into monopolistic industries would raise output even more, but entry is ruled out in these models. Monetary mistakes, i.e., random deviations from optimum monetary policy, do make a difference in output because price adjustment is limited by Calvo pricing restrictions.

Calibrated versions of early NK models do about as well as RBC structures in matching the dispersion of postwar US time series. Neither approach accords with the hump-shaped impulse responses displayed by structural VARs. But additional features like adjustment costs and irreversible investment are easier to shoehorn into NK models, which makes them better suited for econometric policy evaluation.

De Vroey gives the NK model a qualified endorsement as a legitimate member of the DSGE fraternity, tempered by reservations about exogenous price rigidities and prohibitions against entry in product markets. He applauds monetary non-neutrality in the model but regrets the Walrasian language and the abandonment of involuntary unemployment as a focal point in macroeconomic research.

My own verdict on the NK paradigm, explained more fully in sections 6 and 7, is less generous than De Vroey's. I find it hard to accept as a legitimate DSGE variant a three-equation model with no demand for cash or credit, little scope for investment or capital accumulation, and lots of unusual assumptions about pricing and industrial organization. Relative to the RBC paradigm, the NK one gives up much internal logic without getting significant empirical traction in return. As we shall see later in this essay, superior data matching in the NK world can be obtained by adding a lot of noise, that is, many extra shocks and other substantial departures from the original three-equation model. The end product delivers a decent

data match at the cost of intolerable complication.

3.3 *De Vroey's Final Verdict*

Chapters 20 and 21 sum up some valuable lessons De Vroey has learned from his long service in teaching the history of economic thought in Belgium, France, and North America. His are not snap judgments formulated by partisanship, made with a superficial knowledge of macroeconomics or in ignorance of what his predecessors and contemporaries had to say about a subject that he obviously cares for. He views Keynes as an explorer who took some steps from Marshallian toward Walrasian economics, chiefly by assuming competitive goods and factor markets, without achieving his main goal of explaining large-scale market failures. Robert Lucas receives enormous credit for pioneering the use of the Walrasian model family we now call DSGE, and for improving greatly the internal consistency of macroeconomics. Prescott also impresses De Vroey as leading the use of neoclassical growth theory in the study of business cycles, and taking DSGE to postwar data.

Still, De Vroey remains ambivalent about DSGE models even when he stretches that category to include the IS–Taylor rule—Phillips curve structures common to NK macroeconomics. He thinks they are well suited to the study of "small" or "normal" business cycles, but not sophisticated enough to analyze or evaluate policies intended to tame bigger fluctuations like those common in North America before 1940. Large departures from "normalcy," De Vroey reckons, should be studied by economic historians (p. 387).

Among the shrewdest judgments in this book is one that the author shares with Leijonhufvud: a good yardstick for assessing progress in macroeconomics is how well we understand the causes of, and cures for, coordination failures. He predicts, or perhaps hopes, that massive market failures will receive renewed emphasis in future research as a Keynesian goal to be achieved by giving the financial sector a leading role in macroeconomic models (p. 388).

As of this writing, financial frictions do seem well on the way to achieving a central place in macroeconomics. We will delve more deeply into this topic in section 7. Much research on financial frictions and the coordination failures they can cause is now formulated in the language of neoclassical growth theory, especially in the descriptive and optimum growth models of Solow–Swan and Cass–Koopmans, as well as in the related OLG model inherited from Samuelson and Diamond. Understanding business cycles, and assessing what DSGE models have to say about them is forbiddingly difficult without a deeper look at the workhorse models invented in growth theory.

4. *What Is Macroeconomics About?*

The *General Theory* does not define a laundry list of theory or policy issues that Keynes regarded as central to the understanding of business cycles. One can make an informed guess about what interested him from his persistent mistrust of "classical" theory, and also by looking at the table of contents in his book. De Vroey, and Leijonhufvud (1968) before him, sum up the Keynes research agenda, in the manner already laid out in section 2.3: involuntary unemployment, failure of the invisible hand, role of fiscal policy in taming market failures.

Coordination failures used to be of particular interest to many, who placed them at the heart of Keynesian macroeconomics. Solow, for example, wrote:

> I […] think the source of fluctuations lies more frequently in shifts to […] what used to be called intended saving and investment […] Even if an increase in saving today involves an intention to consume more in the future, no

one knows what [the savers] will want to buy or when they will want to buy it. Current investment decisions are made by other people with different beliefs, different motives, etc. (private correspondence, 03/21/2006).

With the benefit of eighty years worth of hindsight, it appears that interest in unemployment switched away from Keynes's notion of involuntary job loss to the simpler one of frictional or search unemployment stressed by Diamond, Mortensen, and Pissarides. The profession continues to keep price rigidity and monopolistic competition firmly in focus. Both concepts remain central to the development of NK narratives, as we saw briefly in section 3 and will see again in sections 6 and 7. In those sections we will also review how NKs ceased to worry about coordination failures, and related phenomena of grossly inefficient competitive outcomes that could be cured by credible policy commitments and other interventions. People who studied these failures called them by a variety of whimsical names: "bank panics," "sunspot equilibria," "animal spirits," "news," "sentiments," or "self-fulfilling prophecies." Here, we will call them ME.

Two generations after Keynes, Lucas expressed his research goals with much greater clarity. In an interview with *Econ Journal Watch* (Klein and Daza 2013, p. 234), he declares growth to be the central issue in macroeconomics. Thirty years before that, he and Sargent ambitiously defined the goal of business cycle research to be the building of a classical macroeconometric model analogous to the Klein–Goldberger model and its offshoot, the Penn–FRB–MIT model, which dominated policy debates in the 1970s and 1980s. As section 2.4 suggests, Lucas and Sargent's stated goal was to build

> [...] an econometrically testable equilibrium theory of the business cycle, one that can serve as the foundation for the quantitative analysis of macroeconomic policy (Lucas and Sargent 1981, p. 89).

To achieve this goal, the authors continue (ibid. p. 60),

> The key step in obtaining such models has been to relax the ancillary postulate used in much classical economic analysis that agents have perfect information.

Where Keynes looked for qualitative explanation, Lucas seeks precise quantitative guidance. As of this writing, we have no functioning, let alone widely acceptable or commercially viable, classical macroeconometric model at hand or just over the horizon. The reasons will be discussed in section 10; some of them do relate to the modeling of information as Lucas and Sargent guessed in 1981.

The development of DSGE in the 1970s and 1980s, and of consumption-based asset pricing almost simultaneously, raised utopian hopes that a single theoretical platform would be able to explain simultaneously a good-sized list of essential facts from economic growth, business cycles, and asset markets. Could we distill a good chunk of macroeconomics down to a few "laws of motion," that is, to a compact system of stochastic difference equations that would fit time series data and provide policy guidance? For a while, it appeared, to this writer at least, that a simple storyline was within reach that would be consistent with growth miracles and disasters, the moments of destrended time series in rich and poor lands, economic responses to important cyclical shocks, observed returns on risky and riskless assets, the home bias in international asset portfolios, and several other anomalies.

One specific wish list of facts to be explained is found in Azariadis and Kaas (2007) and is reproduced below. The list starts with growth anomalies. One is the overwhelming importance of exogenous variations in TFP that account for more than half of international differentials in the standard of living and in its rate of growth.

Another is that growth does not always look ergodic: living standards in some poor countries are not catching up with the world average. In particular, convergence in per capita income fails impressively in Latin America and sub-Saharan Africa where incomes lost ground, relative to the world average, in the second half of the twentieth century before gaining ground in this century. Why did these countries not sustain, up to fifteen years ago, bursts of rapid growth like other developing nations? A third growth anomaly is persistent international differences in the growth rates of aggregate consumption among rich nations with diversified and open financial markets. In the second half of the twentieth century, Japanese consumption per capita grew faster than the world average: about twice as fast as the United States and *nine times* as fast as Switzerland. Swiss and Japanese consumption patterns seem to reflect the path of domestic income, not world income as they would in the simplest DSGE model with perfect capital mobility and identically homothetic utility functions.

Another important business cycle puzzle is that emerging economies smooth their production and consumption less than rich countries. The growth rate of output and aggregate consumption in emerging economies like Argentina and Turkey deviates from trend twice as far as that of developed countries. On rare occasions, even rich countries go through deep or long-lasting recessions like the United States in the 1930s and Japan since the 1990s. Can factor productivity fall that much? If so, is it because technology collapses, because the market loses its ability to allocate resources to firms and consumers who value them the most, or for some other reason connected with policy and politics? Yet another puzzle concerns the dynamic response of GDP to productivity and interest-rate shocks. Those responses are not the monotone convergent paths predicted by

standard DSGE; they look more like irregular, hump-shaped waves. An additional riddle is the Great Moderation, i.e., the pronounced fall in macroeconomic volatility, and the almost equally pronounced rise in microeconomic volatility since the 1980s.

Financial markets bring to the table their own mysteries. The large equity premium, volatile equity prices, low returns on short-maturity public debt, and the identification of bubbles remain unfathomed questions that are unlikely to be resolved until we have better clues as to how markets discount streams of future income. Whose discount rates are reflected in the valuations we observe: the representative everyman's, that of a small group of wealthy investors, or nobody in particular? To make matters more challenging, the distribution of wealth relative to that of income is disproportionately skewed toward wealthier persons and the self-employed. The richest 5 percent of wealth holders own more than half of all financial wealth in the United States. Their median wealth-to-income ratio is more than twice as high as that of all other citizens, even though their median age is identical. Why do the rich have higher saving rates? Is it because they are furthest away from subsistence consumption, because they want to help their progeny, or for some other reason?

Despite much progress in understanding business cycles and economic growth, most of the riddles on our list remain riddles. And the search for a common theoretical platform has not advanced much. Multilingualism reigns supreme in macroeconomics. We still investigate unemployment in the search-theoretic language of Mortensen and Pissarides; make monetary policy recommendations in the dynamic IS–LM language of the NKs; and study growth with the tools of Solow, and business cycles in several distinct tongues.

Right now it seems hard to guess if our profession will adopt a common paradigm

from the ones already in use, or will invent an altogether new one. As we vacillate between neoclassical DSGE, NK dynamic IS–LM, and other contenders, it would be wise to remember that the natural sciences have fallen victim to false paradigms in the past. Ether in physics and phlogiston in chemistry are two stories that come to mind.[1] Perhaps the most egregious and longest-lasting fake paradigm was astronomy's geocentric theory. Section 5 elaborates on this theme.

5. *False Paradigms: A Cautionary Tale*

How should we judge the usefulness of a scientific paradigm like RBCs or NK IS–LM if the data are not overwhelmingly supportive or dismissive of either? One temptation is to follow the lead of trusted colleagues and friends, attempting to guess what *"average opinion"* will be in the near future. A good account of guessing about guesses is the famous "beauty contest," analyzed by Keynes in chapter 12 of his *General Theory*. This is an early example of multiple equilibrium in a game of coordination where the winner is not necessarily the person individual judges regard as most attractive, but instead the one most of them guess will be favored by the majority of their colleagues or by their "animal spirits."

This innocuous example of a coordination failure has an interesting, and less benign, counterpart in ancient astronomy, whose leading lights from Claudius Ptolemy in the second century CE to Tycho Brahe in the sixteenth almost uniformly believed that the earth was the fixed center of the solar system and that the sun revolved around that center. The earliest complete description of geocentric astronomy appears in Ptolemy's *Almagest* which became the astronomer's bible for over one thousand years until it was upended by Copernicus and Galileo, and put to rest by Isaac Newton.

It is hard to know if, and to what extent, geocentric astronomy interfered with progress in navigation, exploration, and international trade. Would Europeans have settled the Americas sooner if astronomy had become heliocentric before Copernicus or, for that matter, before Ptolemy himself? It turns out that heliocentric stories, advanced by Aristarchus of Samos and putting the Sun at the center of things, precede Ptolemy by almost four centuries. Those narratives became well known among ancient mathematicians like Archimedes, partly because Aristarchus took pains to compute sizes and distances for the moon, earth, and sun. His numbers, off by one order of magnitude or more, still came much closer to modern measurements than Ptolemy's own calculations.

Aristarchus lost the popularity contest to Ptolemy by a wide margin because heliocentric theory predicted the *parallax phenomenon*: distances among stars would change as the planets rotated about the sun. Ancient astronomers found no evidence for parallactic motion and modern astronomers have measured it to be miniscule. Ptolemy also managed to sway scientific opinion with common-sense counterfactuals like the huge winds that would ensue if the earth revolved around its axis.

In the end, geocentric astronomy prevailed over the correct paradigm because neither theory was decisively rejected by the observed motion of familiar objects like the sun and moon. Twenty-first-century macroeconomics finds itself in the delicate position of dealing with not two but three workhorse models (representative household, NK, and OLG), none of which is decisively favored by the evidence we have. None of our models can imitate the behavior of time-series

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[1] Ether was a thin elastic substance once believed to permeate all space. Ethereal vibrations were credited with generating light and electromagnetic radiation. Phlogiston was a hypothetical fire-like substance ostensibly released from combustible bodies as they burn, and from humans through breathing. Plants were believed to absorb most of the phlogiston released into the atmosphere.

data as well as an atheoretical system of VARs enhanced by persistent shocks. To understand why, section 6 reviews the strengths and weaknesses of those three workhorses.

6.   *Workhorse Models*

6.1 *Dynamic Stochastic General Equilibrium*

As of this writing, the three most popular models in macroeconomics seem to be the neoclassical or representative-agent household (RA) model, the OLG model, and the NK model. A collective name for all three is *dynamic stochastic general equilibrium*. Despite a common appellation, these three structures differ a great deal from each other in economic mechanisms, welfare properties, and their dynamic response to external shocks.

In more technical language, stationary equilibria in those models have dominant eigenvalues that are small, real, and positive for the simplest versions of every DSGE model written in discrete time (small with positive real parts in continuous time), a fact that puts them at odds with empirical VARs. Our simplest theories claim that small, one-time external shocks die out quickly, without causing any fluctuations, even though our data show a hump-shaped response from GDP that builds up slowly, maxes out, and then withers away. This problem is most serious for the RA model and least serious for OLG.

6.2 *The Welfare Theorems*

Another useful way to understand the differences among DSGE structures is to connect them with the familiar welfare theorems of static general equilibrium theory. The first welfare theorem asserts that "markets work well" every time or, in technical terms, that every competitive valuation equilibrium is Pareto optimal in a private ownership economy without externalities or public goods. The second theorem states that every desirable Pareto-optimal outcome can be reached through a market mechanism in a competitive economy. More precisely, the claim is that every optimal allocation of resources can be achieved as a competitive equilibrium after a lump-sum redistribution of initial endowments. Jointly taken, these two results amount to a belief in the power of a market economy to align perfectly private incentives with social welfare.

Macroeconomics has been ambivalent about the welfare theorems, especially the first one, which seems to undermine any rationale for government intervention. Leaving aside issues of income redistribution, what good would monetary or fiscal policy do if the invisible hand, like an experienced maestro, guides the private sector to perfect outcomes every time? Economists who accept both welfare theorems as approximate working hypotheses gravitate toward the representative or stand-in household RA model of Lucas (1978), and Kydland and Prescott (1982). Those who reject the first theorem and accept the second one typically prefer the OLG model of Allais (1947), Samuelson (1958), and Diamond (1965). Finally, economists who like neither welfare theorem feel some affinity for the NK paradigm of Rotemberg and Woodford (1997) and Gali (1999).

6.3 *The Representative or Stand-In Household*

Rooted in the neoclassical growth models of Cass (1965), Koopmans (1963), and Brock and Mirman (1972), this paradigm analyzes consumption, investment, work, and portfolio choice over time in the simplest possible setting. Households are very similar in their tastes, time horizons, and opportunities, and their individual actions are easily coordinated by price signals. As a result, economic outcomes are identical to the decisions of a

hypothetical, fully informed, and benevolent central planner who seeks to advance common welfare to the maximum extent allowed by technology and resource constraints. All changes in national income in this ideal society, both in the short run and long run, come from movements in labor services, capital services, and technology.

Specifics of the RA model are well known, and they abstract a whole lot of details, and a whole lot of heterogeneity from the economic life of real-world families. An abstract economy is made up of a finite number of similar or identical households with an infinite lifespan, all of them born at time "zero." All information is commonly shared at zero cost. Firms produce under constant returns to scale and earn zero profits. Each household consumes one or two physical goods ("consumption" and occasionally "leisure"); all economic units trade factors of production, consumption goods, and financial claims in anonymous, costless, and competitive markets; and everyone faces a common technology and a single lifetime budget constraint.

Like the standard neoclassical model of optimum one-sector growth, RA descriptions of the business cycle abstract from the influence of changes in the distribution of wealth, a major topic for Stiglitz (1969) and other contributors to descriptive growth theory. Their dynamic properties are dictated by strongly diminishing returns to capital services that quickly suppress temporary shocks to technology, capital, and labor. One-time disturbances die out quickly as the economy converges speedily and monotonically to a well-defined equilibrium like a steady state, a balanced growth path, or an asymptotic distribution in incomes. Fluctuations cannot occur unless bad shocks to "fundamentals" (tastes, technology, endowments) alternate with good ones.

A major contribution of the RA model has been to adapt the long-run apparatus of growth theory to the study of short-term

phenomena like asset prices in Lucas (1978), business cycles in Kydland and Prescott (1982), and quantitative asset prices in Mehra and Prescott (1985). A byproduct of this effort was the invention of DSGE, an entirely new language for macroeconomics that gives precise descriptions of how economic fundamentals determine equilibrium outcomes over time. DSGE reverses forty years of Keynesian methodology by reconnecting short-run events to long-run growth in a manner that is intuitively natural and mathematically elegant. Keynes and his successors thought of the short and long runs as conceptually distinct. Kydland and Prescott were bold enough to claim that the long run is a simple extension of the short run. The language they pioneered is now accepted by the majority of academic macroeconomists, including the younger epigones of Keynes. Standard DSGE models have the feature of just a few, easy-to-understand structural parameters, which facilitates comparisons of predicted equilibria with the data. This feature works both in favor and against DGSE, making the entire class of models easier to validate statistically, and also easier to reject.

By the 1990s, calibrated versions of the Kydland–Prescott model with persistent exogenous shocks in TFP became the dominant business cycle paradigm in academia, but not outside. As we already saw in section 3.1, model predictions about the means and standard deviations of many economic time series agreed with the data. Despite this success, central banks and the business world continue to rely on traditional IS–LM and its NK extensions to this day. One reason we have already broached may be that the RA model has empirically implausible dynamic responses to external shocks. Theory-free VARs show hump-shaped responses to monetary and other shocks, suggesting that modern economies have an internal mechanism that converts one-time external impulses to mini-cycles. RA models do not. Another

reason that we have also touched on is that RA models of the real-business-cycle variety do not leave much scope for money or monetary policy when financial markets are perfect and credit is not. Credit constraints are central to the work of Bewley (1980), Kiyotaki and Moore (1997), and many others.

A third concern is that RA structures, as their name hints, are poorly equipped to deal with heterogeneity. For instance, persistent differences in patience among households lead to peculiar equilibria in which the most patient person lends to everyone else and eventually comes to own the entire economy and to consume all that is consumed. One way out of this predicament is to impose severe restrictions on borrowing as in Becker (1980).

Milder forms of heterogeneity, like idiosyncratic shocks to technology and endowments, are easier to deal with when a variety of slightly different agents can be replaced by a "representative" one. A household is "representative" of a heterogeneous economy if the equilibrium values of prices and economic aggregates are preserved intact when the mass of heterogeneous households is replaced by a hypothetical stand-in household that owns all the resources of the economy, uses the best available technology, and shares everyone's preferences.

Under perfect financial markets, all households face the same price vector, which leads to a common growth rate in consumption between the current state of nature and any future state. We do not know precisely what would happen if financial trades faced realistic impediments or trading restrictions that would constrain borrowing and restrict arbitrage. Interesting attempts to model monetary and financial frictions (Bewley 1980, Townsend 1980, Kiyotaki and Wright 1989, Aiyagari 1994, Kehoe and Levine 1993, and Kiyotaki and Moore 1997) ended with mixed results, leading many to believe that financial impediments are quantitatively unimportant for business cycles (Adrian, Colla, and Shin 2012).

Correct as this belief can be when households are fairly similar and the income shocks they receive are common or well aligned, it is way off the mark when shocks to income, technology, or unemployment are idiosyncratic or industry specific. A quick way to grasp what is at stake is to imagine what would happen in a RA setting if *every household* suffered from strictly idiosyncratic shocks and *no household* could trade financial assets. What would a completely autarkic equilibrium look like and how would it differ from the benchmark situation of complete financial markets?

For the sake of concreteness, let us imagine how much output and consumption we could find in a Kydland–Prescott-style RBC economy with a number of otherwise identical households producing in complete isolation from each other if we started each one with a different amount of capital. The only departure of this economy from the benchmark one is the mobility of physical capital. Perfect markets, in this case, are equivalent to living in a global economy with complete capital mobility and zero labor mobility; autarky is the absence of all international factor movements.

Hard as it is to calculate precisely the income consequences of going from a world with no arbitrage in capital yields to one with perfect arbitrage, the impact on world income will be enormous if there are big differences in initial capital positions. Economies that start without much capital would grow fast over time, but not as fast as if they could have attracted foreign direct investment (FDI). FDI would immediately equate capital and output per worker across the globe, allowing the world economy to approach its balanced growth path, or long-run equilibrium, sooner. Zero FDI may drag that convergence process out for a long time. Limited factor mobility would also prevent the sharing of idiosyncratic productivity shocks that would filter though to consumption, and to asset

prices as well. Bewley (1980) shows how monetary policy may able to improve this situation. However, monetary policy alone cannot help us replace a heterogeneous household sector with a homogeneous "representative" unit when financial frictions limit arbitrage opportunities.

6.4 *Overlapping Generations*

The OLG or life-cycle model came into prominence in 1965 with the publication of Diamond's classic paper investigating how national debt may interfere with the accumulation of physical capital. Diamond's results seemed broadly in line with those of Solow, Cass, and Koopmans but the similarities proved to be more apparent than real, as Shell (1971) found out a few years later. OLG economies can behave in a manner profoundly different from the RA model. They leave room for a large variety of heterogeneous households, competitive equilibria with poor welfare properties, perpetual budget deficits, perpetual rollovers of public debt, bubbles and crashes, financial panics, and other types of coordination failures. On the minus side, empirically plausible life cycles for economic activity, like 55 periods in years or 220 periods in quarters, lead to cumbersome mathematics and opaque theory, forcing users into the familiar straightjacket of the tractable, but often misleading, two-period life cycle.

The limitations of two-period life-cycle models are considerable. One casualty when working with those is that all wealth is owned by one cohort and the intergenerational distribution of wealth does not matter, as it would for any finite life cycle of three or more periods in which middle-aged people would buy the capital stock from the old and lend resources to the young. When the distribution of wealth does matter, it tends to counteract the influence of diminishing returns, leading to complex eigenvalues and damped oscillations. Ignoring the distribution of

wealth across generations limits the OLG model to the study of low-frequency issues and puts into question its suitability as an analytical tool for short- and medium-run topics, like business cycles and asset prices that lie at the core of macroeconomics. James Tobin made that point forcefully in 1980.

Like Allais and Samuelson before him, Diamond (1965) analyzed an economy with an infinite future in which a new cohort is born each period and expires two periods later. Members of each cohort, who may differ in tastes or incomes, trade with their contemporaries within and between generations. Economic outcomes are harder to coordinate in an OLG economy than they are in a RA one because the invisible hand has to deal with the plans of an infinity of households. Markets turn out to work well when households are impatient, and distant future events have little influence on today's choices. Coordination across time can fail in patient societies because the invisible hand has to distribute across time an infinitely valued stream of resources.

Impatient life-cycle economies behave in the long-run somewhat like representative household ones, with unique equilibria that converge rapidly—but not monotonically—to a well-defined and socially optimal steady state.[2] Pronounced deviations of OLG outcomes from the RA benchmark, and from Pareto optimality, appear in patient societies where the invisible hand works poorly if it is not aided by an adequate supply of bubbly assets or by government intervention through intergenerational transfers like social security or public debt. Valuations for bubbly assets (public debt, and similar securities that contribute nothing to aggregate consumption) rely strongly on beliefs about their future value. The invisible hand will coordinate markets well if those beliefs are

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[2] Cf. Azariadis, Bullard, and Ohanian (2004).

favorable, poorly if price expectations are pessimistic.

Financial and real estate bubbles are a key feature of life-cycle economies, one that sets them apart from the RA story. Bubbles are famous for imploding since the Dutch tulip mania in the seventeenth century; every one of them contains the seeds of its own destruction. But not every bubble pops; many are part and parcel of our credit markets, embedded in the term "trust," and the Latin verb *credere*, that is, "to believe," which spawned the noun "credit." Trust is what persuades households to purchase bitcoins; lenders to extend unsecured credit to borrowers; and aging couples to continue holding Japanese public debt at a negligible yield even though Japan's public-debt-to-GDP ratio stands now above 250 percent.

Trust itself is a benign bubble that successfully coordinates good outcomes while financial or bank[3] panics are coordination failures that destroy welfare. We do not really know why or when investors act in harmony to support good equilibria because no convincing arguments are available to explain the inflation and deflation of bubbles. In the postwar era, it appears that domestic fiscal policy has built up much trust among the Japanese citizenry. Avoiding panics and related coordination failures has been an important goal for monetary policy for at least two hundred years, since Thornton (1802) suggested that the Bank of England could avert bank panics by standing ready to supply adequate credit to reputable businessmen at a moderately high rate. Thornton guessed that last-resort lending would unfreeze the private credit market as commercial banks rushed to protect their customer base from the Bank of England and from each other.

OLG models have not been nearly as successful in analyzing everyday issues in business cycles and asset prices as they have been with the pathologies of bubbles, panics, and coordination failures, or in the analysis of social security. Two possible explanations are that the OLG model is under-researched; and that the little effort that has gone into the foundations of this model did not manage either to give a credible account of how a particular outcome emerges when many are possible or to strike a useful balance between life-cycle realism and mathematical tractability. My own count of serious theory papers on life-cycle economies tops out at fewer than one dozen papers since 1980, with only one of them well-read and widely cited. Tractable papers by Diamond (1965) and Blanchard (1985) give up life-cycle realism when they assume that there are two transactions per life cycle or that income does not depend on age. Life-cycle realism in Kehoe et al. (1991) and Azariadis, Bullard, and Ohanian (2004) ends up with intractable dynamical systems of impossibly high order, roughly twice the product of the number of transactions per life cycle times the number of state variables per time period. For example, a quarterly model with four state variables (physical capital, human capital, public debt, and money) would amount to a system of order just below 1,760.

Why has the coherent and realistic OLG model attracted so little research effort when compared with the more popular paradigms of RBCs and NK macroeconomics? Whatever the deeper reasons are, it seems that theory-inclined macroeconomists flocked to the RBC model in the 1980s and 1990s. Central banks and economists who prize empirics and policy migrated to NK ideas in the last fifteen years, partly because OLG and RBC never led to a credible macroeconometric model of business cycles and asset prices. Advocates of RBC and OLG

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[3] Friedman and Schwartz (1963) document the interplay of US bank panics and recessions before the Great Depression.

showed little sympathy for econometrics and little affinity for policy makers. NK ideas have done better on that score, albeit with a deeply flawed model.

6.5 *New Keynesian Macroeconomics*

The NK tag applies to a class of models vaguely in the spirit of Keynes but connected most closely with subsequent ideas by Dixit and Stiglitz (1977), Calvo (1983), Taylor (1993), Rotemberg and Woodford (1997), and Gali (1999, 2005) outlined in section 3.2. The invisible hand remains invisible and irrelevant in this research agenda. The starting premise is an extreme Marshasllian belief that, without suitable guidance from the government, particularly from monetary policy, rigid price signals cannot do a good job of orchestrating private decisions. Output is typically too low.

As we saw in section 3.2, this paradigm rests on three building blocks: a dynamic IS curve, an expectations-augmented Phillips curve, and a modified Taylor rule. Block one is a linearized description of a consumption Euler equation, that is, the first-order condition of a representative household smoothing consumption over time. Block two comes from the production decisions of monopolistically competitive firms that are allowed to charge higher prices if a particular coin toss is favorable, but not otherwise. The odds of a favorable event are independent of the overall inflation rate, which means that the average degree of price rigidity is the same in a low-inflation economy as it is in a high-inflation one. Neo-Keynesian Phillips curves seem to have debatable microeconomic foundations, much like traditional Phillips curves.

The last building block is a Taylor rule, which we can think of as the optimal decision by a central bank seeking to minimize a quadratic loss function that punishes inflation deviations from an ideal target, and output derivations from its "potential" value. Potential output is what would be produced if prices were fully flexible. The initial cohort of NK models was not particularly strong on breadth of scope, internal consistency, or mathematical elegance. This paradigm has little to say about growth, asset prices, fiscal policy, or the role of credit shocks in cyclical fluctuations. It also has difficulty meeting the usual requirements of internal consistency so easily satisfied by RBC, OLG, and other close relatives of general equilibrium theory. The comparative advantage of the NK narrative seems to be the willingness of its intellectual leaders to involve themselves in empirics and policy guidance. Modified NK models are good at tracing out the impact of interest rate shocks on the real economy as described in the work of Christiano, Eichenbaum, and Evans (2005), and its extension by Smets and Wouters (2007).

Christiano, Eichenbaum, and Evans (2005) take a step toward the goal set by Lucas and Sargent (1981), which was the construction of an econometrically testable and policy-relevant equilibrium theory of the business cycle. This time, business cycles have a decidedly NK flavor with all the usual building blocks, plus a number of additional assumptions that slow down and complicate the adjustment of prices and quantities to external shocks. The goal is to extract empirically plausible responses to interest-rate impulses, instead of the usual rapid monotone convergence one obtains from tightly micro-founded DSGE models. Extra assumptions in NK models include features that macroeconomic models typically ignore for the sake of simplicity or tractability: habit formation in consumption, fixed costs of producing, variable costs of adjusting the capital stock, and a large number of external shocks to monetary and fiscal policy, markups, technologies, and impatience.

Equipped with this much ordnance, the NK model does a creditable job of fitting

postwar US data, about as good as a Bayesian VAR. Impulse responses to monetary shocks are commendably hump shaped, and price and wage rigidities (especially the latter) are important explanatory factors of output movements, as are shocks to impatience and aggregate spending. Missing from this list are monetary and credit aggregates. There is no trace of the mechanism that economists from Friedman and Schwartz (1963) to Bernanke and Gertler (1995) have thought of as "the credit channel." Do business cycles have anything to do with credit and financial shocks? Are they set off by accidental movements in the federal funds rate, accidental movements in the production possibility frontier, or something else? To explore potential impulses that may set off cycles, we flash back in time to review ideas from a long line of economists starting with Keynes and Pigou and reaching to Acemoglu and Woodford.

7. *Business Cycle Paradigms*

From Pigou (1927) to Prescott (1998), most classical economists take cyclical movements to be market responses to changes in fundamentals that cannot be manipulated by policy tools unless policy itself is the "fundamental." On the other side, Keynesians from Hicks to Woodford are skeptical about the capacity of a market economy to recuperate quickly from a recession and hopeful that countercyclical policy can strengthen or accelerate the recovery. Monetarists and "creditists" from Friedman to Bernanke stress the primacy of monetary and credit factors in setting off and curing recessions. Expectationists from Keynes to Farmer consider recessions to be pure coordination failures that good policy can avoid or escape from, if it manipulates beliefs in the right direction. Finally, institutionalists from North to Acemoglu regard property rights and political agendas to be

the primary drivers of economic growth and, by extension, of business cycles as well. Many economists straddle camps, including three of our most famous colleagues: Keynes who turns out to be a Keynesian/ expectationist, Pigou who clearly showed sympathy for both classical and expectationist ideas, and Lucas who started out as a monetarist and slowly moved toward RBCs.

7.1 *The Neoclassical Story*

Of these five business cycle viewpoints, the neoclassical one tells the simplest and most easily testable story. Neoclassical thinkers view cycles as reactions to fundamental impulses, that is, to actual or predicted shocks shifting a vector of parameters that we would regard as strictly exogenous in a general equilibrium model. Taxes and regulations, government consumption, household patience, technology, and some factor endowments are examples of exogenous parameters that can and do change over time, often in unpredictable or random fashion. Output contractions in this story are socially desirable responses to shocks that raise taxes, hike the cost of invention or innovation, or undermine the productivity of capital and labor for any reason. Productivity shocks have become central to RBC theory because detrended GDP per capita is strongly correlated with the Solow residual.

Money is rarely a protagonist in classical models, which have an inherent bias toward monetary neutrality. One way to defeat this neutrality is to bring in Keynesian assumptions about sticky prices or wages, as in Christiano, Eichenbaum, and Evans (1999). Another important issue is the assumed exogeneity of TFP. It is hard to believe that large recessions like 1929–40 and 2008–09 are unfortunate retrogressions in our capacity to combine capital and labor or that they signal a temporary pause in our readiness to keep moving capital and labor to our most productive firms.

If recessions were responses to exogenous technology reversals, we should be able to connect them with temporary drops in patent applications and royalty income, or in the productivity of skilled relative to unskilled labor. If, on the other hand, recessions signal misallocated input—for example through increased dispersion of returns on invested capital—then input reallocation among firms should be procyclical. That is exactly what Eisfeldt and Rampini (2006) discovered in US data. Buera and Shin (2013), and many others, found that borrowing constraints and excessive regulation are responsible for as much as 40 percent of the TFP difference between the United States and the world's least-developed economies. These findings imply that short-term corrections to TFP are not completely decided by nature, but instead are partly man-made movements of an economy inside a fixed production possibilities frontier.

We know, for instance, that favorable credit conditions can bring economies closer to that frontier and to its potential output; declines in lending, for whatever reason, can reverse economic progress. Levine (2005) finds that, over the long run, improvements in financial intermediation matter for economic growth even after we correct for reverse causality, that is, for the removal of financial frictions that would automatically recede as an economy develops.

7.2 *The Keynesian Story*

Keynesian ideas were the foundation of macroeconomics during the Great Depression, a rare event with which our field has not fully came to grips. Economists are not in agreement about either the catalyst that started that event in the United States or about the policies that allowed it to linger until 1940. Monetarists like Friedman and Schwartz (1963) blame the Federal Reserve System for standing idle during a precipitous decline in money supply, bank deposits, and loans. Expectationists

from Keynes himself (1936) to Farmer (2016) think of the depression as an oversize financial panic set off by a massive deterioration in business and household sentiment. Pigou (1927), Romer (1990), and others explain the contraction as "news," that is, as a punishing rise in uncertainty that led to a massive cut in the demand for durable goods.

What we do know about the end of the depression is that it took place when everyone could reliably forecast a large and lasting increase in war-related federal purchases. That event gave the Keynesian story instant legitimacy among academics and policy makers, verifying its key prediction that recessions and depressions could be cured by deficit spending. NK models are partly in line with this prediction: monetary accommodation and positive fiscal shocks typically pull the economy out of a mild recession. The NK story, however, has little to say about deep contractions in output or to suggest how policy should deal with rare events.

The Japan experience after 1990 also casts some doubt on the traditional Keynesian policy prescription. Japanese GDP per capita is now where it was twenty years ago even though, in the interim, the general government has been priming the pump furiously with primary budget deficits averaging 4 percent of GDP on the way to a lofty public debt of two and one-half times GDP. One reason that Keynesian policies fell flat in Japan may be wrongheaded industrial policies that subsidized the continued operation of insolvent banks holding substantial portfolios of nonperforming loans. Cole and Ohanian (1999) present some evidence that ill-conceived attempts by the federal government to prevent the downward adjustment of US wages and prices needlessly prolonged the Great Depression.

What role do banks and financial markets play in recessions? An interesting narrative comes from monetarism and from a related strand of thought I will call "creditism."

7.3 *Monetarists and Creditists*

Modern stories that emphasize the role of financial factors in business cycles start with Friedman and Schwartz's (1963) *A Monetary History of the United States*. This magisterial book documents a close connection among three variables: money supply as measured by M_1, the deposit-to-currency ratio, and industrial production from 1867 to the Great Depression. The typical pattern is one of mischief by a small group of commercial banks leading to insolvency or suspension of deposit convertibility into gold. Next comes a bank panic that destroys deposits, tightens up lending, shrinks M_1 and private credit, and spills over to industrial production. An extreme case is the 1930s, when money supply dropped by one-third within three years (1930–33), and the deposit-to-currency ratio fell by nearly 60 percent. Roughly 7,000 banks failed and aggregate spending collapsed. Bank assets and outstanding loans also fell by 60 percent between 1930 and 1935.

More than one story agrees with these frightening facts. Monetarists find in them an elevated role for M_1 and much blame to lay on the Fed for its sluggish response to the rapid decline in deposits. After all, Thornton (1802) and other proto-monetarists clearly understood the roles central bankers could play as lenders of last resort during bank panics. Even in the course of normal circumstances, Bernanke and Gertler (1995) argue that monetary expansions may have a salutary effect on incomes through the *credit channel*. This term refers to the beneficial impact of lower nominal yields on the collateralizable net worth of borrowers, which not only lowers the cost of capital but also loosens borrowing constraints by reducing the opportunity cost to a bank of collecting information about borrowers.

Another story of the deposit collapse in the early 1930s is a pure bank panic of the type analyzed by Diamond and Dybvig (1983), but on the grand general equilibrium scale of a massive coordination failure. This direction brings us back to expectations and how they impact broader economic activity, a topic central to chapter 12 in the *General Theory*.

7.4 *Expectations and Multiple Equilibria*

The notion that "autonomous" shifts in market beliefs may be responsible for large economic movements and excessive economic volatility has a long history in economics, often as a verbal argument backed by casual observation. Pigou (1927) conjectured that half the amplitude of observed cyclical fluctuations comes from changes in market psychology and money supply. Both he and Keynes regarded "animal spirits" to be capricious sentiments abetted by irrational investors whose actions could cause real harm unless neutralized by appropriate monetary and fiscal policy. Shiller (1981) measures this harm as excess volatility in stock prices. These appear to fluctuate five times more in the United States than one would expect from actual changes in dividends and interest rates.

Pigou (1927), Keynes (1936), and Shiller (1989) were unanimous in taking excess volatility to be a symptom of individual and group irrationality. Modern work on ME shows that it does not have to be. Excess volatility turns out to be perfectly consistent with rational choice and rational expectations, and may persist without some form of collective or public intervention. The idea is quite simple: if all investors think stock prices will go up tomorrow, they place buy orders with their brokers, and prices duly go up, confirming prior beliefs. This syllogism motivated a cadre of academics working with competitive equilibrium models at the University of Pennsylvania in the early 1980s. In their words:

> The central message of this paper is that even perfectly well-behaved economies will typically admit rational expectations equilibria in which the expectations themselves spark

fluctuations in the level of business activity (Azariadis 1981).

The lesson for macroeconomics is that, even if one assumes the most favorable informational and institutional conditions imaginable, there may be a role for the government to stabilize fluctuations arising from seemingly noneconomic disturbances (Cass and Shell 1983).

[…] if economic activity can fluctuate from day to day in a way that is independent of economic fundamentals, then there may be an important role for the policy maker in designing regimes that can reduce fluctuations and increase economic welfare (Farmer and Guo 1994).

Similar results came from strategic or bargaining models of coordination failure at about the same time from Diamond (1982) and Cooper and John (1988). An early application of these ideas is the seminal piece by Diamond and Dybig (1983) who proposed the first explicit story of bank runs as ME. This paper is also a reminder of Thornton's suggestion that the government should take actions—in this case, deposit insurance or suspension of convertibility—that coordinate beliefs on the best available equilibrium. A few people also analyzed debt crises as ME, suggesting policies that would prevent or cure those problems.[4]

Despite early successes, Thornton's insight that equilibrium selection is an important task for monetary policy did not take hold among policy makers or academics. One reason surely is the reluctance of the academic community to take indeterminacy seriously without a mechanism that explains how markets select one particular equilibrium when many are possible. Solow (1997) catches the essence of that reluctance better than most commentators:

I feel acutely uncomfortable with this [expectations/beliefs] fudge factor that is capable of

having drastic effects but is so conjectural that it can be used to explain just about everything.

Another reason may be that the multiple equilibrium story did not crystalize into an overarching, commonly accepted, and widely used paradigm. The literature employed a variety of tools: static general equilibrium with incomplete markets, two-period OLG, RA with increasing returns, coordination games, search and matching with increasing returns, and others. A flood of models came out, focusing on oversimplified environments with clever examples of inefficient equilibria and little regard for empirical relevance or policy guidance. Farmer is a notable exception to this attitude. In joint work with Benhabib and Farmer (1994) and Guo (Farmer and Guo 1994), Farmer calibrated various types of RA models with external increasing returns to scale, treating beliefs as an exogenous shock. His models matched well certain moments of postwar US time series.

Multiple equilibrium models with sophisticated financial frictions proved of mixed use in explaining volatile exchange rates, accounting for abnormally large equity premia or providing a broadly accepted theoretical rationale for reduced-form Markov switching models of business cycles. This failure is particularly hard to understand because simpler types of financial frictions, like the agency cost story by Bernanke and Gertler (1989) and the collateral constraint story by Kiyotaki and Moore (1997), have been central in attempts to explain how external monetary and credit shocks are transmitted and amplified in modern economies.

After a long delay, very recent attempts to calibrate multiple equilibrium models with financial frictions[5] do show some promise, giving expectational shocks a substantial role in causing business cycles. In these studies, belief shocks about future credit rationing

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[4] Azariadis and Smith (1998), Asea and Blomberg (1998), Alvarez and Jermann (2000), and Gu et al. (2013).

[5] See, for example, Azariadis, Kaas, and Wen (2016) and Dai, Weder, and Zhang (2017).

are responsible for nearly half of the cyclical variation in GDP, with collateral shocks and technology splitting the other half. All of these shocks influence capital reallocation and, eventually, TFP. Missing from the list of output shifters are fiscal shocks. What can we tell about their contribution to business cycles and trend growth?

7.5 *Political Business Cycles*

Both institutional economists and economic historians have amassed a large volume of data connecting institutional improvements with economic performance. From North and Thomas (1973), who argued that property rights remove market inefficiencies, to Acemoglu et al. (2003), who found a causal link between weak institutions and economic volatility, institutionalists have advanced the plausible, but as yet inconclusive, claim that improvements in the written and unwritten rules of the "economic game" are one of the, and perhaps *the* most important, determinants of fluctuations and growth.

The growth focus of most institutional stories has, up to this point, masked any potential influence that politics may have on the origin and transmission of business cycles.[6] Every macroeconomic model that we know—classical, Keynesian, or otherwise—recognizes exogenous fiscal policy shocks as cycle-causing impulses. What if fiscal policy is not exogenous? Suppose policy is decided by some type of voting process, say by a median voter, as it is in Meltzer and Richard (1981) and subsequent models of income redistribution.[7] Then, almost by definition, fiscal shocks reflect political shocks, that is, unexpected changes in the identity of the median voter or in the set of choices that she is confronted with.

The simplest way to visualize political shocks is within the Meltzer and Richard model in which the median voter sets a uniform tax rate on all incomes, with the revenue rebated to all citizens as a lump-sum subsidy. Redistribution in this setting occurs if median income is below average income. Fiscal shocks in this economy can arise from unpredictable changes in the distribution of pretax income, which would shift the identity of the median voter. Policy shocks could also arise from random variation in the set of policy alternatives available to voters, that is, in *political agendas*, and finally from a little bit of noise, fickleness, or unpredictability in the decisions of the median voter.

Do fiscal policy changes cause business cycles? Forty-plus years after Nordhaus (1975) asked this question, we still appear to be in the dark. Political business cycles are one of several long-standing riddles in macroeconomics. We review these in the following section.

8. *Riddles and Answers*

8.1 *What Have We Learned Since Keynes?*

In the eighty years since the *General Theory*, macroeconomics has dealt with many empirical puzzles and quite a few thorny policy issues. What have we learned since then and what riddles remain? How close are we to fulfilling the goal, set by Lucas and Sargent in 1981, of a neoclassical or modern alternative to Klein and Goldberger's econometric model of the United States, which dates back to 1955 (Klein and Goldberger 1955)? Are we in sight of a macroeconometric model that would provide us with reliable prediction and serve as a foundation for the quantitative analysis of macroeconomic policy? This section attempts to sum up what has happened since 1981.

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[6]One exception is Nordhaus (1975) and a modest amount of research in political science that has not resonated in macroeconomics.

[7]See, for example, Alesina and Rodrik (1994) and Persson and Tabellini (1994), who are concerned with the impact of inequality on growth.

As we look back, there is much cause to celebrate rapid progress in the depth and quality of our toolkit. Statistical and mathematical tools at our disposal have greatly improved since the 1981 Lucas–Sargent manifesto, and so has the quality and quantity of our data from time series, panel, and cross-sectional sources. We know vastly more about households, firms, industries, governments, and our own past than we ever did before. From Kydland and Prescott (1982), Hall and Jones (1999), and many others, we have learned that the Solow residual is more often than not the most conspicuous proxy force behind output movements, seemingly the engine of national income. We do not really know what fuels this engine, but whatever does also moves GDP. The connection between GDP and the Solow residual could reflect causality in either direction or even the influence of deeper underlying forces. Plausible sources of productivity growth at the moment appear to be credit and politics in the short run, technology and institutions in the long run.

From Mortensen and Pissarides (1994) and Andolfatto (1996), we learned how to use search-and-matching tools to analyze unemployment as an equilibrium between jobs created and jobs destroyed when our economy is hit by idiosyncratic and aggregate shocks. The resulting outcomes look much like modern unemployment cycles but do not come close to the type of persistent mass unemployment observed in depression-era America or contemporary Greece. Mass unemployment seems a "rare event" arising from "rare" causes. It is not yet clear what those might be, other than a colossal failure of trust in economic institutions and political leadership. The puzzle is measuring "trust" and understanding why it collapsed from 1929 to 1933.

Townsend (1979), Stiglitz and Weiss (1981), and many others provided empirical researchers, from Jermann and Quadrini (2012) to Solis (2017), with ideas that help quantify the key role credit plays in US business cycles, college access, and human capital accumulation. We are still not close to agreement about the relative contributions of financial and nonfinancial shocks to the generation and propagation of business cycles, but we are making progress.

Optimism is harder to justify in the area of asset prices and stock market volatility where very basic facts like the excessive equity premium and the anemic risk-free rate still remain shrouded in mystery more than thirty years after they puzzled Mehra and Prescott (1985). Attempts to explain them involve narrowly specialized structural assumptions that would limit the applicability of any model in other areas of macroeconomics. Examples of such assumptions are habit formation, discount rate shocks, and Epstein–Zin preferences. Lettau and Uhlig (2000) point out that habits induce very big income effects, as would excessive amounts of risk aversion, which goes against what we know about household labor supply and saving decisions. If discount rate shocks are large, then why is US GDP so closely correlated with the Solow residual? Finally, preferences that lie outside the expected utility framework seem a bit too cumbersome for macroeconomic work.

What options are left for those who are not happy with the three stories reviewed in the previous paragraph? One option is the exogenous "rare event" argument due to Rietz (1988) and Barro (2006), which delivers a reasonable risk premium at a 5 percent disaster probability when the coefficient of relative risk aversion roughly equals five. Similar results are consistent with a no-borrowing constraint and a slightly higher risk-aversion coefficient in the OLG model of Constantinides, Donaldson, and Mehra (2002), which features a three-period life cycle. Catastrophic income drops and large financial frictions are stories that hold promise and deserve more work from younger researchers.

A third possibility is belief shocks to "animal spirits" that would raise the volatility of both individual incomes and asset returns, perhaps through fluctuations in borrowing constraints. This direction remains unexplored, except for a paper by Farmer (2016). Asset returns are still on the list of enduring macroeconomic puzzles together with growth disasters, and the causes and welfare costs of business cycles. We review those next.

8.2 *The Welfare Costs of Business Cycles*

If we are forced to rank macroeconomic puzzles by order of importance, what would we put on top? Many people's list, including my own, would start with cycles and growth: What fuels business cycles? What factors can we validly regard as significant engines of growth? Persuasive answers to these questions would be quantitative, listing shocks and engines in order of their contributions to variations of output around trend, but also to growth spurts, growth reversals, and other low-frequency issues.

At first glance, it looks like cycles should rank way below trends as a macroeconomic priority. In a carefully argued article, Lucas (2003) calculates that a representative household with unitary relative risk aversion would gain only 0.05 percent in certainty-equivalent consumption if all consumption risk were to vanish from US data. That figure goes up by one order of magnitude for certain groups of people hit by persistent idiosyncratic earnings shocks, as in Storesletten, Telmer, and Yaron (2001) and Huggett, Ventura, and Yaron (2011). Those shocks can be particularly damaging to finitely lived agents who start their earning careers in recessions. The thought experiment behind these computations deals with an economy hit by exogenous common or idiosyncratic shocks to technology and endowments.

Imagine next that a group of risk-neutral foreign investors agree to bear all these shocks at an actuarially fair price. Lucas

calculates the consumption-equivalent benefit of this transaction to the average domestic household. An unstated assumption behind this computation is that the removal of business cycle risk does not change the trend growth rate of the economy.

Suppose, however, that cycles and growth are not neatly separable, as they are not in the capital misallocation narrative of section 7.1. In that story, business cycles are movements *within* society's production possibilities frontier induced by financial frictions, not shifts in the frontier itself. For the sake of concreteness, assume that financial frictions are fully responsible for the slowdown of the Japanese economy, detailed in section 7.2, from a trend growth rate of roughly 2 percent to one that alternates (say, every year) between 2 percent and zero. Following Lucas, we can ask what welfare improvements would ensue if financial frictions magically vanished. As a consequence, the average growth rate shoots up from 1 percent to 2 percent.

Raising the average growth rate in this manner is equivalent to an enormous positive supply shock. The flat-consumption equivalent of this maneuver nearly doubles: it goes up by a factor equal to the utility discount rate β if households are risk neutral, even higher if risk aversion is important. Raising financial literacy, retraining labor, and speeding up the flow of capital to expanding industries are all examples of lessened financial frictions that could improve TFP growth.

As of this writing, we do not have a generally agreed-upon calculation of the welfare cost of business fluctuations. How do we identify the shocks that have caused past movements in economic activity? How do those shocks percolate through the US economy? NK econometrics has provided some preliminary answers to the first question. Progress on the second one remains slow because none of our workhorses possess complex eigenvalues that would endow our

models with the ability to convert a one-time external impulse into fluctuating motion.

8.3 *Empirics: Calibration or Estimation?*

Are DSGE models statistically relevant? Do they help us understand postwar (or, for that matter, prewar) business cycles? Are they helpful in evaluating alternatives for monetary and fiscal policy? And if the answers to these questions are not affirmative, what can we add to the DSGE paradigm to make it more data friendly?

Section 3.1 took a stab at these questions by looking at the pros and cons of the calibration methodology. The short answer was that calibrated RBC and NK models match well the first and second moments of major aggregate series, but cannot replicate dynamic responses to shocks unless we are willing to complicate their theoretical structure to the point of intractability. In what follows, we go a bit deeper into the issue of empirical validation: by what standard do we decide if a particular model "fits" the data?

This is partly an abstract, and somewhat philosophical, issue of what bar a theory must clear before it is deemed empirically plausible; it is also a statistical issue of identification, estimation, and inference. One person who thought that matching moments was too low a bar for any business cycle story is Christopher Sims, an econometrician who had expressed skepticism about the empirical relevance of economic theory in Sargent and Sims (1977). Twenty years later, Sims (1996) questioned the tendency of the RBC researchers to ignore econometric testing.

Sims shows some sympathy for simple, intellectually transparent theories like DSGE because they give clear predictions of what data should look like, but guesses that DSGE models are perhaps too simple to confront time series data. That he regards as a fundamental flaw:

> … theories need to confront data, not be protected from it (Sims 1990, p. 111).

Watson (1993) was among the first to put Kydland and Prescott's (1982) definition of empirical realism to a formal test. Kydland and Prescott defined empirical realism to mean that the artificial time series generated by the equilibrium of a calibrated model should resemble those of an actual economy." Watson proposed goodness-of-fit measures as a practical way to quantify "resemblance" or "closeness" between DSGE models and the stochastic process that generated recorded time series. The actual measure was the amount of noise one had to inject into a model to match the autocovariances of consumption, investment, and GDP. Not much error had to be added at low frequencies of more than eight years. At business cycle frequencies of six to thirty-two quarters, noise had to contribute half of the total data variance.

Watson (1993) paid little attention to dynamic properties of DSGE models that we already know to be at variance with reality; Cogley and Nason (1995), for example, found little resemblance between calibrated RBC dynamics and estimated cyclical dynamics. A more nuanced assessment comes from Hansen and Heckman (1996) and Browning, Hansen, and Heckman (1999), who seem to recognize the deep aggregation issues involved in choosing parameter values for highly simplified, cookie-cutter DSGE models when parameters come from microeconomic empirical studies of heterogeneous households and firms. Hansen and Heckman, in particular, view calibration and estimation as complements, with calibration being a first stage for more convincing econometric estimates of structural parameters that should precede quantitative policy evaluation.

Browning, Hansen, and Heckman (1999) give an interesting account of the nearly irreconcilable differences between micro parameters and macro ones. In principle, macro parameters can be calculated from general-equilibrium time-series averages

or from partial-equilibrium microeconomic studies. Microeconomic studies typically look at economies that differ quite a bit from DSGE models. They are primarily static and often use functional forms that are not homogeneous and, therefore, not consistent with balanced growth paths or other macroeconomic equilibrium states.

The gap between macro and micro parameter values widens as we add more heterogeneity. RBC theory needs a high wage elasticity for labor supply, which micro data only finds at the extensive margin, not the intensive one that representative households are, by definition, confined to. Micro studies also reject the constant relative risk aversion assumption that's popular in the DSGE universe because low-wealth households show more aversion to risk than wealthy ones. Women are more inclined than men to shift work hours into the future; their elasticity of intertemporal substitution is five times as high as that of men. Estimated time discount rates also vary widely by wealth, with the richest households being far more patient.

To this list we can add Hendrik Houthakker's famous example of individual producers, each using a Leontieff fixed-proportions technology, whose aggregate behavior is well described by a Cobb–Douglas production function. Reducing an economy with households that differ in patience, risk aversion, and income shocks to a representative household seems like a formidable, and perhaps impossible task, especially in the presence of financial frictions that limit arbitrage through the variety of securities traded, or the amount of non-collateral borrowing available. One may hope, like Browning, Hansen, and Heckman (1999), that macroeconomic calibration and microeconomic estimation will learn from each other as applied microeconomists become more comfortable with dynamics and macroeconomists begin to appreciate heterogeneity. These developments seem to be essential prerequisites for the building of empirically relevant and commercially successful classical econometric models of the type that Lucas and Sargent envisioned forty years ago.

8.4 *Growth Puzzles*

Persistent poverty remains a fact of life for about one billion of our fellow human beings. What does growth theory have to say about the poverty of nations? This question has occupied economists and economic historians since Adam Smith. It continues to absorb social scientists to our day, from Rosenstein-Rodan (1943) and Kuznets (1966) to Easterlin (1981) and Lucas (1988). Empirical work on growth regressions by Barro (1991), Levine and Renelt (1992), and, more recently, by Acemoglu, Johnson, and Robinson (2001) and Gorodnichenko and Roland (2017) shines some light on the reasons for delayed growth, but the overall picture is still murky because we cannot easily separate the contribution of the standard economic growth "engines" (investment rate, human capital stock, initial conditions) from the institutional and cultural environment within which they operate.

If we find, as Levine and Renelt (1992) did, that the investment-to-GDP ratio is a statistically significant and an econometrically robust growth engine, do we attribute this influence to the patience of savers, to the strong protection of property rights, or to a business-friendly society? Levine and Renelt also found initial income and human capital to be significant contributors to growth between 1960 and 1989 in a large number of countries. Convergence in per capita income is significant as well, if we correct for differences in initial human capital or introduce location dummies. Institutions do not correlate robustly with GDP growth rates but do so with income levels.

On the flip side of these findings, nonlinear regressions by Durlauf and Johnson (1995)

find no support for convergence in samples that include countries from Africa and South Asia. More disturbingly, Acemoglu, Johnson, and Robinson (2001) argue that differences in institutions, proxied by the mortality rates of early European settlers, explain three quarters of international differences in per capita income levels in a panel of sixty-four former European colonies across the globe.

One institution of importance is rights to property[8], which influences a society's economic performance in many ways. For example, weak enforcement of property rights attracts people into unproductive, purely redistributive activities that we know as rent seeking, bribery, and corruption or property crime. The pie to be shared among society's members is smaller than it could potentially be. Also, the lack of secure property rights affects the expected returns to all sorts of investment (Mauro 1995). In order to counteract the appropriation activities of others, productive agents have an incentive to shift resources that could be used productively into defensive security measures, like hiring private and government policemen, employing lawyers, prosecutors, and judges, etc. In a nutshell, appropriation activities divert resources away from directly productive use.

Inconclusive as many of these studies seem to be, they are all consistent with the view that growth is the complex outcome of powerful economic, political, and social interactions. To disentangle the forces at play, it would be useful to have at hand a structural growth model that accomplishes two goals: first, it treats culture as a slowly adapting "deep parameter" that captures the weight of past decisions on current behavior; and second, it models institutions as an object of a one-dimensional political choice. Politics would be settled by majority vote as in Alesina and Tabellini (1990), by a small

number of strategic players as in Skaperdas (1992), or by an elite as in recent work by Acemoglu, Johnson, and Robinson (2005). A prototype structure of this type is suggested in Grossman and Kim (1996), but the institutional and political sides need much elaboration. A good structural model would have to strike a happy balance between the tractability of the Solow residual and the sophisticated politics of institutional models favored by Acemoglu and Robinson.

These arguments have taken us a long way from Keynes and Lucas. It would be nice to reflect a bit on how much we owe to our "classics," and what we should make of the ideas we inherited from them.

9. *The Triple Legacy*

As told by Michel De Vroey, the field of macroeconomics owes an immense debt to three people: Keynes, Lucas, and Prescott. The first two set the agenda; Prescott invented the language. Keynes challenged the notion that an economy in deep trouble will recover by its own self-correcting forces; Lucas inverted the conventional logic that leads from price expectations to market-clearing prices; and Prescott reconnected the short-run and long-run dialects that used to divide macroeconomics.

The Keynes agenda owes a great deal to a distinguished group of followers from Hicks to Klein. In the early phase of his career, Lucas interacted regularly with John Muth and Sargent, who helped him explore the implications of rational expectations; and with Prescott, with whom he collaborated in early attempts to build models of dynamic general equilibrium.

Refining DSGE models, calibrating them to US data, and using them to track postwar business cycles are all brainchildren of Prescott and his doctoral students (especially Thomas Cooley, Kydland, Mehra, Richard Rogerson, and Gary Hansen) who invented

[8]This paragraph is paraphrased from Auerbach and Azariadis (2015).

a new language for macroeconomics that has spread around the world. The new language enables business cycle economists to communicate easily with growth specialists and financial economists; Keynesian and classical policy prescriptions to be evaluated with common criteria; and a large number of phenomena, from the dynamics of business firms to political economy, to fit a common theoretical platform that distills them down to a simple system of difference equations or "laws of motion."

Keynes put large business cycles at the heart of macroeconomics, especially the phenomenon of mass unemployment and the policies that would eliminate it and keep it from coming back. Deficit spending, in his opinion, would cure recessions, partly by replacing private spending and partly by helping coordinate it. Fiscal stimulus was the antidote to coordination failures set in motion by pessimistic business expectations about other businesses pulling back from spending and, in the process, laying off workers and depressing incomes. Keynes thought of recessions as periodic coordination failures: Pessimistic businesspeople do not hire because they predict weak demand for their products. Aggregate demand collapses as a result, and the pessimists are proven right.

Private spending suffered no massive failures after World War II, and the emphasis among macroeconomic policy makers inevitably shifted away from deficit spending and aggregate demand management to supply-side questions of optimal taxation and long-term growth. Macroeconomics slowly but decisively adopted the formalism and vocabulary of growth theory. What was *perfect foresight* in growth become *rational expectations* in macroeconomics. Fisherian consumers become infinitely lived households, and the ideas of Solow, Uzawa, Diamond, Cass, and Koopmans were refashioned to deal with traditional issues in business cycles, plus a host of new ones in asset prices, education and human capital, economic development, firm dynamics, and many others.

A sea change came over macroeconomics. Lucas put together a new agenda for the entire field, one that many academic economists have followed with remarkable fidelity for a quarter of a century. Why that happened is not entirely clear, at least not to this writer. Was Lucas a skilled diagnostician of the weaknesses in Keynesian macroeconomics? An accurate forecaster of what the profession would find interesting? Or did he just choose what he thought were interesting problems and persuade everyone else? Whatever the answer is, it may have something to do with Lucas' early exposure to Phelps' quest for the micro foundations of Keynesian economics as summed up in Phelps (1970). What we do observe with hindsight is a remarkable agreement between the topics Lucas chose to work on and those his younger colleagues picked for themselves.

Prescott is the person more responsible than anyone else for perfecting and spreading the growth-theoretic language of dynamic general equilibrium to the academic community. The language itself is precise and versatile. It applies in principle to economies much more general than the RA model that has been the mainstay of academic business cycle theory. It easily adapts to financial and informational frictions like the collateral constraints examined in Kiyotaki and Moore (1997). The slightly older Kehoe and Levine (1993) model of limited enforcement, a structure very close to the Kydland–Prescott benchmark but with heterogeneous households, is a treasure trove of coordination failures, financial panics, and interesting policy issues. Researchers have not really given this story the attention it deserves.

A widespread reluctance to enrich the RBC model with financial and other frictions may be one reason why dynamic general

equilibrium has not met with success outside academia. Seventy years after the death of Keynes, central bankers have little use for the new language of macroeconomics, including NK macro, which is popular in bank research departments but not in actual policy making. DSGE is not the foundation of any commercially successful econometric model that the business world turns to for short-run economic prediction. No DSGE model has the real-world impact enjoyed by the Wharton model and other descendants of the Klein–Goldberger model in the 1970s.

What remains popular are simple elaborations of the standard IS–LM model that still dominates undergraduate macroeconomics textbooks. This is a troubling state of affairs for those who regard economic theory as the servant of economic policy. What is keeping us from adding some policy relevance to the logically tight RBC and OLG models? Why can we not build coherent theory foundations of price adjustment and capital accumulation for the logically challenged NK model? And when can we expect an operational macroeconometric model of the United States based on dynamic general equilibrium? Section 10 attempts to outline answers to these questions.

10.   *Final Thoughts*

From Keynes, Lucas, and Prescott we inherited a culture, a language, and a long list of riddles. How far we go toward solving those riddles depends in part on how good we are in enriching our language to the point that it communicates with everyday life. Peering far into the past, we can trace the beginnings of the Italian Renaissance to Dante and the world dominance of the English language to Chaucer.

Whatever value future generations of economists find in DSGE will probably depend on the success of DSGE models as servants of economic policy. A prerequisite for that success is to find persuasive common-sense answers to long-standing problems in business cycles, economic growth and the management of economic policy, including events like bursting bubbles, liquidity crises, and other coordination failures. The to-do list is extensive, formidable, and many items on it are long overdue. It stretches from the internal dynamics of neoclassical growth theory to macroeconometric models; from the sources of business cycles to the engines of growth; from understanding unusual events like financial panics and currency meltdowns to prescriptions of how to avoid those in the first place, or to manage them once they occur. In view of the progress we made along the journey from Keynes to Lucas, none of these challenges seem beyond the talents, tools, and data of the profession.

10.1   *Internal Dynamics*

Our most basic problem, and consequently most immediate task, seems to be inventing a business cycle model with the *pendulum property*. Economies fluctuate around their steady state once shocked away from it, and converge slowly back to their rest position until they are hit by another shock. To achieve this goal, we need to swap the simplest workhorse models laid out in section 6 for subtler versions with complex eigenvalues. We saw in section 8.4 that complex eigenvalues will not only generate realistic-looking impulse responses to external shocks but will likely improve the chances of building successful structural econometric models on DSGE foundations.

Imagining a tractable neoclassical model of fluctuations with complex eigenvalues is not an easy job but I do not see a way around it. Older models of the trade cycle (Ezekiel 1938, Samuelson 1939, and Goodwin 1951) featured ad hoc "cobwebs" and multiplier-accelerator mechanisms with built-in cyclical forces that could convert one-time external shocks to

damped cyclical motion or to limit cycles.[9] Achieving that goal, without the paraphernalia of adaptive expectations or ad hoc investment plans, would allow us to track business cycles without a constant barrage of persistent aggregate impulses. Are we really convinced that the US economy moves about trend because the Federal Reserve errs persistently in setting the federal funds rate or the Treasury spends, with equal persistence, beyond its stated intentions?

Internally propagated cycles are a fundamental feature of OLG growth models with a life cycle equal to or greater than three periods. Realistic dynamics in this framework come at the expense of mathematical complexity that modern computation still finds hard to manage. Other alternatives include financial frictions that convert one-time shocks to persistent changes in credit limits in the manner specified by Bernanke and Gertler (1989), Kehoe and Levine (1993), Kiyotaki and Moore (1997), Azariadis and Smith (1998), and many others. The financial frictions story holds particular promise because output is strongly correlated to the volume of credit, especially so of unsecured credit (Jermann and Quadrini 2012, Azariadis, Kaas, and Wen 2016); and financial frictions seem to contribute much to output volatility (Cooley, Marimon, and Quadrini 2004). On the minus side, no existing model of financial frictions fits easily into a standard DSGE framework, with the possible exception of Kehoe and Levine.

Another, perhaps equally important, reason to introduce financial frictions into standard DSGE models is the potential power of monetary policy to combat the undesirable consequences of those frictions. Monetary expansions may be effective antidotes to private credit contractions if the credit channel works well. Articulating explicit mechanisms

for that channel seems a sensible way to introduce money and monetary policy in DSGE models.

10.2 *What Drives Business Cycles and Growth?*

Even a complete description of the credit channel would give us limited insight into the forces that drive business cycles. A more rounded story would help us weigh the relative contributions of shocks to secured and unsecured credit; to tariff and nontariff terms of foreign trade; to changes in invention, innovation, and firm dynamics; and to impulses from taxes and public spending.

Some of these shocks we can interpret as fundamentally unpredictable variations in economic behavior under a fixed set of institutions, that is, of the explicit rules that govern individual decisions. Other shocks will be partly predictable responses to impulses that originate in domestic or international politics and shift unpredictably the "rules of the game."

Political shocks that change institutions can be crucial to economic growth because the quality of governance is typically highly persistent. England is a good example of a country that has been relatively well governed since the Magna Carta; Singapore provides a rare counterexample of a radical and rapid improvement in institutional quality. Development also appears to correlate with culture, i.e., with the unwritten rules that shape private economic behavior. Evidence of this link comes from a variety of sources ranging from Hofstede (1984) to more recent work by Tabellini (2010).

Culture is typically a very slow-changing "deep" parameter. If DSGE models help us establish a causal link between institutions, culture, and per capita income, we will have gone a long way toward understanding both the engines of economic growth and the reasons why some countries take so long to improve

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[9] Beaudry, Galizia, and Portier (2016) is a recent contribution to this tradition.

their standard of living. "Understanding" institutional change requires a sensible and parsimonious explanation of political equilibria that maintain institutional quality in some countries, improve it in others, and worsen it in still others.

10.3 *What Do Policy Makers Maximize?*

Last, but not least, on our to-do list are core issues of economic policy. Explicit descriptions of who our policy makers are and what goals they pursue are essential to endogenizing monetary and fiscal policy and understanding where policy shocks come from. Do we assign policy decisions to a benevolent and fully informed Ramsey planner, as we normally assume in macroeconomics? To an equally benevolent and somewhat uninformed mechanism designer who converts private messages to public policy, as in Hurwicz (1972)? To a self-perpetuating elite, as in more recent work by Acemoglu and his collaborators (Acemoglu, Johnson, and Robinson 2005)? Or do we need to go for the realism of political science where selfish politicians jostle to control agendas and remain in power for as long as possible, as in Tsebelis (2002)?

We do not know which of these routes will prove most useful. What we do know, from Mountford and Uhlig (2009) for example, is that fiscal shocks correlate strongly with GDP and some fiscal multipliers can be as high as five. Looking for the origins of those shocks in the political arena may help us understand past policy mistakes, like the inexplicable reluctance of the Federal Reserve System to act as a lender of last resort in the early 1930s. The same endeavor may help us deal with large coordination failures, by which we typically mean man-made rare events like the Japanese and Italian growth recessions in the last twenty years, the US Great Recession in 2008–10, and the long recent slides in the national incomes of Argentina, Greece, and Venezuela.

These events cannot be coordination failures if they are unique responses to deteriorating fundamentals. As a first approximation, we will take the fundamentals in all these cases to be stable. To continue this thought experiment, let us fix technology, preferences, endowments, and culture but allow politics to change unexpectedly because of unpredictable shifts in the identity of the ruling elite, the agenda setters, or even the median voter.

What circumstances could prevent markets, or the "invisible hand," from working to the benefit of society in this environment? One is confidence-sapping panics and crashes that deflate stock markets, diminish collateral, and hold back loans. Events of this sort can start innocently enough from financial mischief by a relatively small firm like Lehman Brothers, or from self-fulfilling rumors of commercial bank insolvency. Cultures, on the other hand, are more persistent than panics and, therefore, more effective triggers of long-lasting coordination failures.

Part of our culture comes in the form of religions, which often start as doctrines that exalt poverty and condemn enrichment. When these values dominate in economic life, as they did in medieval Europe, incentives to explore, innovate, and invest are dulled. Society, science, and technology can become prisoners of inherited traditions in a self-perpetuating low-level equilibrium. A symptom of this phenomenon is the decline of European mathematics in the Middle Ages which preserved Rome's Pantheon as the largest dome in the West from its erection in the second century of the Common Era to the building of the Cathedral of Florence in the fifteenth century.

It is hard to understand, and even harder to model, how traditional societies escape these straightjackets. Luckily, we have some practical knowledge about ways to avoid or manage bank panics, bubbles, and other short-term coordination failures. Standard

recipes dating back to the Napoleonic era call for lending of last resort, deposit insurance and, if needed, massive increases in central bank liabilities. This is how the Federal Reserve System dealt with the 2008–10 recession in the United States.

Our understanding of bubble management does not carry over to explicit DSGE models—theoretical or econometric—that would provide information about the origin of stock market and credit bubbles, guidance about their management, or a quantitative assessment of the good or harm they may cause to our economy. Intuitively, we know a few things about coordination failures, but we have no explicit rules that dictate with any degree of precision how we should behave when expectations get out of control.

Why don't we already have an explicit playbook for bubbles and panics? Why have we not arrived at a widely acceptable accounting of the shocks driving business cycles and the engines powering economic growth? The concluding section goes out on a thin limb to take a stab at these three questions.

11. *Afterword*

Dynamic general equilibrium is a parsimonious description of a private ownership economy in a small space of physical goods and household types. "Parsimony" means that institutional details and exogeneity assumptions should be as few in number and as simple in structure as the goals of the model allow. By combining first-order conditions for all actors with clearing in all markets, DSGE models derive laws of motion describing how economic aggregates evolve over time.

An exacting standard for exogeneity would put institutions, the quality of governance, and policy on the list of endogenous variables, restricting the set of predetermined

and exogenous variables to political constitutions, culture, tastes, some forms of technology, and a few initial conditions. To extract an operational econometric model from DSGE stories at this level of theoretical purity seems at worst a fantasy, and at best a daunting task, much harder than the Klein and Goldberger (1955) transformation of a simple IS–LM structure into a successful macroeconometric model.

It took Keynesian economists less than twenty years to start that transformation, about thirty years to complete it. Almost fifty years after Lucas's (1972) path-breaking piece on *Expectations and the Neutrality of Money*, DSGE advocates are far from matching Klein's success. One way to interpret this failure is to admit that DSGE models, including neo-Keynesian variants, are so far removed from descriptive realism that their forecasts and policy advice are irrelevant to practical people. The alternative is to believe that our profession is capable of matching Klein's achievement on a grander scale with more refined models that are both theoretically coherent and policy relevant.

What would it take for DSGE to succeed in this particular sense? Success seems far from assured, given the many questions and puzzles left open by the DSGE approach. In the wise words of Solow: "A foolish theory digs itself a hole and calls it a paradox" (correspondence 07/10/2017). In this writer's opinion, all we need to do to climb out of whatever hole we dug is to draw the attention of the younger generation of macroeconomists and econometricians; to hope that several of them will volunteer to try ideas outside their own and their friends' comfort zones; and to pray that a few of them will feel the almost irrational attraction to intellectual risk taking that goes against conventional wisdoms and that's evident in the careers of Keynes, Lucas, and Prescott. Risk taking of that magnitude seems to have sustained three generations of the world's best physicists in a

ninety-year-old, and still unsuccessful, quest to unify relativity and quantum mechanics under a common theoretical platform. The physicists keep trying because they value the advantages of speaking a common language. So should we.

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