

# 1 Introduction: The State of Macroeconomics

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## 1.1 Aims and Themes

Capitalist economies fluctuate and periodically experience large disruptions in economic activity. The peculiar nature of these fluctuations is that, unlike in earlier times, they typically fail to have straightforward natural causes such as harvest failures, with the pandemic of the 2020 and the ensuing recession as an exception in this respect. A central focus of economics has been on understanding the sources of these fluctuations and recommending policies that might mitigate their harmful consequences. This book is an attempt to contribute to these tasks but, along the way, much effort will be devoted to a critique of dominant contemporary macroeconomic theories, which – strikingly – denies even the possibility that fluctuations and crises may be generated endogenously.

With its emphasis on stable equilibria and the self-regulating potential of market mechanisms, the contemporary macroeconomic orthodoxy represents a reversion to pre-Keynesian positions. Unlike the empiricist defense of free markets associated with Milton Friedman, however, it has devolved into a scholastic emphasis on ‘microeconomic foundations,’ demanding that core macroeconomic relations be derived directly from intertemporal optimization by representative agents. These microeconomic foundations are far less secure than advertised. Furthermore, empirical failures (some of them revealed by the financial crisis of 2008 and associated with the inadequacy of a worldview of inherent stability) have led to a great deal of patching up; *ad hoc* modifications have increasingly complicated the models and undermined the coherence of the approach.

The orthodoxy still maintains great hold over the levers of power in both an academic and policy making context. After the financial crisis of 2008, however, there has been growing disillusionment. The theory is becoming widely perceived as irrelevant, at best, to real-world issues, spurring a move in the direction of purely data-driven approaches. And indeed, such approaches can bring real progress in the context of expansions of available data and advances in econometrics. This had been the case with the pioneering work by Ragnar Frisch, Jan Tinbergen, Lawrence Klein, and others in the mid-twentieth century, and we may be going through another period of rapid progress, driven by the emergence of ‘big data’ and ever-expanding computer power. Big data and sophisticated econometric techniques do not, however, obviate the need

for a conceptual and theoretical framework to select and structure empirical studies and to interpret the statistical results that have been collected.

The alternative theoretical approach presented here departs from the dogmas of the prevailing orthodoxy in academic macroeconomics, drawing on recent developments in behavioral economics as well as older literatures from the Keynesian and Marxian traditions. The intention is to give readers a multifaceted view of contemporary macroeconomics as well as a path forward.

Macroeconomic models, first, tell stories about the interactions between myriad decision-makers operating within a particular structural setting. The microeconomic behavior of decision-makers is an essential part of these stories. The current orthodoxy, however, bypasses a series of aggregation problems and relies on assumptions about microeconomic behavior that are simplistic and misleading. The alternative approach in this book uses microeconomic assumptions that are informed by behavioral evidence, integrating these assumptions into a macroeconomic environment that has far more correspondence to present-day realities than can be found in orthodox models. There will be no infinite-horizon optimization by representative agents, but microeconomic behavior will be central to the analysis.

Macroeconomic theories, second, must be structuralist as well as behavioral: Economies in which households own the capital stock directly may perform differently in some respects than economies in which the capital stock is owned indirectly in the form of financial assets; increasing inequality cannot be understood without attention to institutional change; fiscal policy faces different challenges in advanced and developing economies. Focusing almost exclusively on technology and preferences as the basic parameters of an economy, the current orthodoxy is largely blind to these and other social, institutional, and structural contingencies.

Macroeconomics, third, is general as opposed to partial. As recognized by microeconomic theorists in the 1970s, there can be no presumption of stability in Walrasian models of general equilibrium. Using a different framework of analysis, Keynes had reached a similar conclusion years earlier: The *General Theory* does not deny the existence of a full-employment equilibrium, focusing instead on the stability properties of this equilibrium. The main message was that flexible prices and wages cannot be relied upon to eliminate involuntary unemployment. Taking into account interactions across markets, the full-employment equilibrium may not be stable. The current orthodoxy does not even consider these stability questions.

Going beyond the short run, fourth, the analysis challenges mainstream views of the growth process as involving stochastic fluctuations around a stable full-employment trajectory, with movements along a smooth neoclassical aggregate production function guiding the economy toward steady growth. The theoretical and empirical justifications for the aggregate neoclassical production function are flimsy, and this function is unnecessary to account for the empirical patterns. There are good reasons, furthermore, to think that steady growth paths will be locally unstable and that business cycles would exist in the absence of shocks. In this sense, the cycles become endogenous.

The analysis of complex, dynamic interactions between decision-makers and across markets, fifth, requires the use of formal mathematical models. The problem with

the current orthodoxy is not so much that it uses excessive formalization but that it makes the wrong basic assumptions. The straightjacket of full intertemporal optimization misrepresents real-world decision-making, but also has another negative effect: It reduces the ability of the theory to incorporate important aspects of reality in a tractable manner, including mechanisms that can lead to local instability and endogenous fluctuations.

Unlike macroeconomic orthodoxy, finally, the analysis in this book is not justified on an *a priori* basis, following instead traditional approaches to scientific methodology, including requirements of logical coherence and consistency with empirical observations. Present-day economic orthodoxy is an outlier in this respect: The claim that macroeconomic theory must be founded on extreme versions of individual rationality and intertemporal optimization represents a peculiar scholastic admonition, unknown and without parallel in any other discipline.

## 1.2 Background

In the period after the Great Depression, and especially after the Second World War, a broad-based Keynesian consensus had emerged. The post-War boom had helped give credence to this consensus; the disruption of the boom in the 1970s set the stage for the breakup of the consensus.

By the early 1980s, challenges from traditional monetarist ideas had given way to Robert Lucas's more radical critique: The reduced-form relations in Keynesian macroeconomic models reflect economic behavior that will not, he argued, be invariant to changes in economic policy. Following this 'Lucas critique,' a methodological imperative gradually gained general acceptance: Macroeconomic theories, it was suggested, must be based explicitly on microeconomic optimization.

The Lucas critique was valid in principle and, potentially at least, had practical significance. But the doctrines that emerged in the wake of the critique presumed extravagant levels of intertemporal rationality on the part of the public, ignored aggregation problems, and reasserted pre-Keynesian beliefs that the capitalist macroeconomy was inherently self-equilibrating. The initial formulations of the new theories faced serious empirical problems, necessitating a range of modifications and extensions, and coalescing in the contemporary 'dynamic stochastic general equilibrium' (DSGE) models, the flagship macroeconomic theory of mainstream macroeconomics. Despite successive modifications, the DSGE orthodoxy retains central elements of the earlier models – including the presence of optimizing representative agents with infinite horizons and rational expectations, and the presumption of a stable equilibrium linked to a natural rate of unemployment.

By the beginning of the 2000s, a self-congratulatory consensus had taken hold among macroeconomists. The desirability of microeconomic foundations had become generally accepted, and the Lucas-inspired research program was dominant in academic macroeconomics. Undergraduate textbooks still discussed Keynesian models, but the takeover was complete at the graduate level, and policymakers increasingly

relied on the new theory. Traditional Keynesian economics had been displaced, and Chari and Kehoe (2006, p. 4) could declare victory: “Macroeconomists now take policy analyses seriously only if they are based on quantitative general equilibrium models in which the parameters of preferences and technologies are reasonably argued to be invariant to policy.”

Inadequate theoretical foundations and a reliance on ephemeral empirical correlation allegedly rendered Keynesian models unreliable and misleading. The new models, by contrast, were seen as firmly grounded in economic theory. Woodford (1999, p. 31) saw convergence, not just within macroeconomics but also in relation to the rest of economics: “Modern macroeconomic models are intertemporal general equilibrium models derived from the same foundations of optimizing behavior on the part of households and firms as are employed in other branches of economics.” Blanchard (2000, p. 1375) suggested that “progress in macroeconomics may well be the success story of twentieth century economics,” with Chari and Kehoe (2006, p. 26) claiming that the theoretical advances had great practical value: “Macroeconomic theory has had a profound and far-reaching effect on the institutions and practices governing monetary policy and is beginning to have a similar effect on fiscal policy. The marginal social product of macroeconomic science is surely large and growing rapidly.”

The ‘great moderation’ and a dynamic American economy in the 1990s (along with the breakup of the Soviet Union) formed the background to this broadly shared sentiment. Business fluctuations had become milder after the mid 1980s, and Lucas pronounced the problem of depression prevention as “solved, for all practical purposes.” He went on to argue that “the potential for welfare gains from better long-run, supply-side policies exceeds by far the potential from further improvements in short-run demand management” (Lucas 2003, p. 1).

Rapid US growth demonstrated the benefits of free markets and an economic policy focused on low inflation, economic incentives, and liberalization. At least that was the claim. Historically, the US performance after the 1970s was not, in fact, particularly successful. Average growth rates had been higher in 1950–1975, and the distribution of the gains became highly skewed after 1975. The rich (and especially the super rich) got richer; the rest experienced stagnating or falling real incomes. Women and minorities experienced some material improvements, but the median wage of a male worker was virtually unchanged between 1973 and 2020, and at the low end of the distribution, male workers have seen declining real wages. Macroeconomists, however, have traditionally given little weight to distributional issues; Lucas (2004, p. 14) famously commented that of “the tendencies that are harmful to sound economics, the most seductive, and in my opinion the most poisonous, is to focus on questions of distribution.”

The financial crisis of 2008 took the profession by surprise. The timing of financial crises will always be difficult to predict, but the problem for economists ran deeper. The macroeconomic consensus had converged on a theoretical model in which finance played no significant role and in which a financial crisis could not occur: Any meaningful treatment of finance and financial crises has, as a prerequisite, the existence of

distinct agents with different financial positions; the standard model, by contrast, was built around a single, representative household.

Once the financial crisis did occur, the model also proved useless as a guide to economic policy. It had nothing to say about financial issues; it implied that although price stickiness could lead to temporary unemployment, these aggregate demand-related unemployment problems would be short-lived; it suggested that adjustments in household saving would largely offset fiscal policy, leaving fiscal policy with little or no effect on aggregate demand; it pointed to monetary policy as the preferred instrument for stabilization, but the traditional monetary instruments ceased to be available when interest rates hit the zero lower bound.

The poverty of the new models led to a resurgence of crude empiricism, a resurgence that is deeply ironic. A profession that had touted the Lucas critique and the necessity of economic theory and solid microeconomic foundations now extrapolated from past evidence to suggest that, for unspecified reasons, financial crises must lead to prolonged recessions. Meanwhile fiscal retrenchment was advocated on the basis of evidence suggesting a correlation between high levels of public debt and slow growth (Reinhart and Rogoff 2009, 2010).

The implications have been devastating. The American Recovery and Reinvestment Act of 2009 (a fiscal stimulus package) and aggressive quantitative easing attenuated the downturn in the US but were insufficient in scale to prevent a painful and prolonged recession. Matters have been worse on the other side of the Atlantic where policymakers, with a predominant influence of German ordoliberal ideas, pursued austerity policies that have been ripping Europe apart. Millions of citizens have suffered needless hardship as a result of soaring unemployment and cuts to pensions and social benefits. Political turmoil, social unrest, and the rise of extremist nationalist movements have followed, threatening the democratic fabric.

Not all of these ills can be attributed to poor economic policy, but it would be hard to deny the influence of worsening economic conditions on social and political events. And the economics profession and macroeconomic theory in particular must accept its share of responsibility for the policies that have been pursued. The insistence on markets as self-regulating and fiscal policy as ineffective or harmful provided theoretical justification for neoliberal policies before the crisis and for inadequate and misguided policies after the crisis.

Economic theory is not the only influence on policy. Opportunistic attempts to ‘starve the beast’ and cut back the welfare state have undoubtedly contributed to policy formation. But macroeconomic theory has acted to disguise the political nature of these attacks. It has also done little to confront and dispel misleading analogies between Swabian housewives and sound macroeconomic policy.<sup>1</sup> On the contrary,

<sup>1</sup> According to the *Economist* (2014, Feb. 1), the Swabian hausfrau as an archetype was invoked by Angela Merkel when she suggested failing banks “should have consulted a Swabian housewife because she could have told them how to deal with money.” The same article quotes the prime minister of Baden-Württemberg as saying:

viewing the macroeconomy as essentially a single representative household plays into that same mindset.

Applied economists cannot afford to ignore empirical evidence, especially in a deep recession. Not surprisingly, therefore, a body of policy research has questioned the presumptions and implications of the current orthodoxy, sometimes explicitly and sometimes implicitly. The International Monetary Fund (IMF) and Organisation for Economic Co-operation and Development (OECD) studies, for instance, have debunked claims about ‘expansionary austerity’ and documented strong fiscal multipliers during recessions. More generally, a booming body of applied research has addressed a number of important issues; Nakamura and Steinsson (2018) discuss some of this recent work.

At a theoretical level, by contrast, the DSGE approach has met little resistance within the mainstream of the profession. The work by George Akerlof and several coauthors represents an exception, challenging both the behavioral assumptions and key notions such as the ‘natural rate of unemployment.’<sup>2</sup> Another example could be the recent revival by Larry Summers of the notion of ‘secular stagnation’ and the closely related suggestions by Paul Krugman that liquidity traps may have become increasingly relevant. But neither Summers nor Krugman has proposed a full-fledged alternative to the DSGE consensus.

The failures of current versions of the DSGE models are hard to ignore, however, and their limitations have been widely and increasingly acknowledged, even if the methodology goes unquestioned by many of those who are critical. Blanchard (2018a), for instance, sees “current DSGE models as seriously flawed, but they are eminently improvable and central to the future of macroeconomics” (p. 44). Moreover, he argues, “starting from explicit microfoundations is clearly essential: where else to start from? Ad hoc equations will not do” (p. 47). Thus, he concludes, “DSGE models can fulfill an important need in macroeconomics, that of offering a core structure around which to build and organize discussions” (p. 48). Christiano et al. (2017) present a more extreme version of this position in their defense of DSGE models. Macroeconomists, they argue, cannot perform experiments on actual economies in order to learn the relative strengths of competing forces. Experiments are necessary, however, and “[t]he *only* place that we can do experiments is in dynamic stochastic general equilibrium (DSGE) models” (abstract; italics in original) – a claim that is both audacious and blatantly false: We need formal models, but not all logically consistent macroeconomic

“Yes, she’s a cliché, but much more than a cliché,” says Winfried Kretschmann with some pride, because “the Swabian housewife represents the starting point” in German thinking on the euro and fiscal management.

<sup>2</sup> Other behavioral macroeconomists have also chipped away at the standard DSGE models (e.g., De Grauwe 2012), and Robert Solow has been another consistent critic of the Lucas paradigm (e.g., Solow 1986, 2008). See also two special issues of *Oxford Review of Economic Policy* (2018, Vol. 34(1–2); 2020, Vol. 36(3)) on ‘Rebuilding macroeconomic theory,’ which contain a range of papers that discuss, defend, or critique the DSGE approach.

models with dynamic and stochastic elements follow the particular ‘DSGE’ approach to macroeconomic theory.<sup>3</sup>

Outside the mainstream, (post-)Keynesian, (neo-)Marxian, and institutionalist critics have always been scathing in their critique, even before the onset of the financial crisis.<sup>4</sup> As an example, Dutt and Skott (2006) argued that “what has happened in macroeconomics since the late 1960s has been a wasteful detour. A generation of macro-economists has grown up learning tools that may be sophisticated, but the usefulness of these tools is questionable. Moreover, a great deal of damage may be, and has been, done when the tools are applied to real-world situations.”

After the crisis, similar conclusions have been voiced by a number of economists who were previously seen as part of the mainstream. Paul Krugman is quoted as saying that most of modern macroeconomics is “spectacularly useless at best, and positively harmful at worst” (*Economist*, July 16, 2009); Willem Buiter (2009) referred to the last 30 years of macroeconomics training at US and UK universities as a “costly waste of time,” and Brad DeLong (2009) commented on the Chicago school’s “intellectual collapse.” More recently, Paul Romer (2016) has berated the DSGE models for their use of “incredible identifying assumptions to reach bewildering conclusions” (p. 1). Macroeconomics, he suggests, has been guided by deference to the leaders in the field and “progress in the field is judged by the purity of its mathematical theories, as determined by the authorities” (p. 16). Ferocious as these comments may be, it is often unclear what is being promoted by these critics as an alternative to the orthodoxy. And an alternative is needed.

Blanchard sees the DSGE model as providing a core structure for understanding capitalist economies. The flawed DSGE approach is a poor choice, but Blanchard is correct, in my view, that a core structure or theoretical vision is necessary, even if one’s goal is ‘merely’ sound policymaking rather than grand system building for its own sake – as Keynes observed, “practical men who believe themselves to be quite exempt from any intellectual influence, are usually the slaves of some defunct economist”

<sup>3</sup> Some DSGE models incorporate traditional, pre-Lucas elements and insights. The large ‘indirect effect’ of monetary policy identified by Kaplan et al. (2018) and highlighted by Christiano et al. as an example of cutting-edge research basically identifies (as they acknowledge on p. 20) a standard Keynesian multiplier. The attempts to include deviations from rational expectations point in the same direction, and perhaps the term DSGE will lose its distinctive meaning sometime in the future. But as of today, DSGE is not a simple synonym for the macroeconomic model. The Euler equation for optimizing, representative agents with infinite horizons is at the center of analysis for DSGE models, which are guided by particular rules of the game and a particular underlying vision.

<sup>4</sup> The marginalization of these traditions within the profession will be seen by the mainstream as a reflection of their weaknesses, rather than as an indication of the close-mindedness of the mainstream itself. In the words of the *Economist* (July 16, 2009, “What went wrong with economics?”), “[t]oday’s economists tend to be open-minded about content, but doctrinaire about form. They are more wedded to their techniques than to their theories. They will believe something when they can model it.” There is some truth in this claim. But the distinction between content and the modeling technique breaks down in the case of contemporary macroeconomics. It is reasonable to demand that an argument be clearly articulated, logically coherent, and consistent with relevant empirical evidence, but the prevailing orthodoxy in macroeconomics has a particular methodology, demanding explicit intertemporal optimization as a central element of any acceptable macroeconomic model.

(*General Theory*, p. 383). But in order for it to be useful and relevant for real-world applications, we need a core structure that is quite different from what is being offered by the research program on DSGE models.

### 1.3 Overview

#### 1.3.1 Behavior

Macroeconomic models incorporate pure accounting equations, but on their own these equations do not take us very far. Behavioral elements must be added, and the specification of these elements requires assumptions about microeconomic behavior. This recognition of the importance of microeconomic behavior does not imply that macroeconomic equations must be derived directly from the intertemporal utility maximization of a representative agent.

Chapter 2 examines the Lucas critique and the way it has been addressed through the introduction of an optimizing representative agent. The message of this chapter is simple: The Lucas *critique* is unexceptionable, but the Lucas *solution* developed by mainstream macroeconomics represents an abject failure. Heroic aggregation assumptions are embodied in the creation of a representative agent: Even if individual preferences could be taken as well defined, exogenous, and stable over time, the celebrated Sonnenschein–Debreu–Mantel results show that microeconomic rationality imposes only very weak constraints on the properties of aggregate excess demand functions.

Chapter 2 also questions the utility function of the representative agent as the basis for welfare analysis. This approach to welfare analysis has been hailed as a strong and distinctive advantage of the contemporary approach because, supposedly, it uses a ‘correct’ and ‘objective’ welfare criterion. This claim is false: Using a ‘descriptive’ representative agent’s utility function imparts a systematic bias against the poor and in favor of the rich. The derivation of macroeconomic relations from the optimization of a representative household “is not simply an analytical convenience as often explained, but is both unjustified and leads to conclusions which are usually misleading and often wrong” (Kirman 1992, p. 117).

Even if we put aggregation issues to one side, the standard behavioral assumptions are questionable. Much of economic activity is goal oriented in a relatively clear way. This applies most obviously to capitalist firms. Given the complexity of the decision problem and the pervasive clouds of uncertainty in which these decisions must be made, firms cannot ‘maximize profits’ in a strict sense, but formal models embodying profit maximization can be useful for many purposes. The optimization approach is more questionable with respect to households, which are at the center of DSGE models. Indeed, in light of much behavioral evidence, the microeconomic assumptions of contemporary macroeconomics appear mechanical, primitive, and misleading. Behavioral economics has demonstrated systematic deviations from the simple assumptions of perfect instrumental rationality and rational expectations. These deviations from



predicted behavior have important implications for key elements of macroeconomic theory, including wage formation and saving.

Chapter 3 considers households' saving decisions. Some deviations of actual behavior from that of an idealized 'homo oeconomicus' are quite trivial and irrelevant for macroeconomics: Households make mistakes in their daily activities and sometimes fail to choose consumption baskets that could have made them better off, but if the mistakes are random, the implications for macroeconomic theory are limited. Matters are different when the mistakes are systematic and occur in areas that affect aggregate economic outcomes.

The evidence shows that a large proportion of households have saved very little by the time they reach retirement. There may be several reasons for low saving, including impatience, lack of self-discipline, and peer effects on consumption. But pervasive uncertainty also poses questions for the general notion that long-term decisions like household retirement saving can be based on meaningful notions of optimization. Technical change can make skills obsolete and create new job opportunities, and alterations in economic policy can have major effects on household finances. Obvious examples include uncertainties concerning future social security, health benefits, and the cost of sending children to college. Even if it successfully identifies these contingencies, the household still faces the daunting task of incorporating them into optimal plans.

These complications and the systematic deviations of household behavior from the postulates of the model do not merely add random errors that cancel out on aggregation. And they affect the core of the DSGE model: It is precisely the intertemporal utility maximization under perfect foresight (or rational expectations) that supposedly establishes the superiority of the model. As noted by Blanchard (2016, p. 1), the derivation of consumption demand in DSGE models is "strongly at odds with the empirical evidence" with respect to both "the degree of foresight and the role of interest rates in twisting the path of consumption." One can try to patch up the model in various ways – by introducing a subset of 'hand-to-mouth' consumers or adding 'habit formation,' for instance – but these are "repairs, rather than convincing characterizations of consumers" (Blanchard 2016, p. 2). Thus, we have here a research program which, 40 years after its inception and after at least 20 years of near-total dominance, has to admit that its key innovation does not fit the facts (Chapter 3).

The treatment of wage formation and the labor market is another example of failure. The existence of a well-defined 'natural rate of unemployment' informs much of economic policy, but the evidence is weak: Strong prior beliefs are required to justify interpretations of the evidence as supportive of a natural rate of unemployment. Natural-rate theory, second, is fragile even on its own terms: Minor changes in the Barro–Gordon (1983) analysis of inflation bias can eliminate the natural rate of unemployment and generate policy conclusions that are radically different, even when assumptions of perfect foresight and well-defined preferences over inflation and output are retained (Chapter 5).

The behavioral assumptions underlying wage formation in the current orthodoxy also exclude forces that have a systematic influence on outcomes in the labor market.

Abundant evidence suggests that notions of fairness are important for wage setting and labor relations. These behavioral findings fit badly within the current orthodoxy. Norms of fairness, moreover, are likely to have strong historical and conventional elements: Reductions in wages relative to a previously established level are typically seen as unfair. These systematic, behavioral deviations from homo oeconomicus have macroeconomic significance. ‘Fair wages’ can be a source of unemployment, as suggested by Akerlof and Yellen (1990), and money illusion (Akerlof et al. 1996), while conventional elements in fairness can lead to path dependencies (hysteresis) in both the rate of unemployment and the structure of relative wages (Chapters 6 and 7).

The deviations from homo oeconomicus with respect to both consumption and wage setting do not deny the importance of microeconomic behavior for macroeconomics. On the contrary, DSGE models fail because they have been based on misleading assumptions about microeconomic behavior. Macroeconomic relations should indeed reflect microeconomic behavior, and macroeconomics must be ‘behavioral.’ But the core assumptions of the DSGE approach represent a poor approximation to real-world behavior.

### 1.3.2 Structure

Microeconomic behavior takes place within a macroeconomic environment, and macroeconomic theory should ignore neither the structures that define the environment nor the individual agency within these structures. All theories have, implicitly or explicitly, a structural setting that is macroeconomic in nature – even the Walrasian general equilibrium model with its simple set of abstract assumptions about property rights and markets. Thus, the critique of the current orthodoxy is not the absence of any structural assumptions, but the poor choice of assumptions actually made. The underlying position appears to be that preferences and technology dominate and that, otherwise, institutions are largely irrelevant or simply reflect preferences and technology.

Discussing the labor market and the time patterns of work, Lucas (1981) expresses this view explicitly. Social convention and institutional structures affect the time patterns, he argues, but

conventions and institutions do not simply come out of the blue, arbitrarily imposing themselves on individual agents. On the contrary, institutions and customs are designed precisely in order to aid in matching preferences and opportunities satisfactorily.

Theories that take into account the complicated institutional arrangements in actual labor and product markets

would have to explain why, given their opportunities, people *prefer* arrangements involving erratic employment patterns. Ignoring this simple point seems to me simply bad social science: an attempt to explain important aspects of human behavior without reference either to what people like or what they are capable of doing. (Lucas, 1981, p. 4; italics in original)

This reductionist argument is unconvincing. Institutions change over time, and the aggregate and cumulative effects of individual behavior undoubtedly play a part in the generation of such changes. But collective action problems and simple game theory