THE LIMITS OF BUSINESS CYCLE RESEARCH: ASSESSING THE REAL BUSINESS CYCLE MODEL

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The real business cycle model dominates business cycle research in the new classical tradition. Typically, real business cycle modellers both offer the bold conjecture that business cycles are equilibrium phenomena driven by technology shocks and also novel strategies for assessing the success of the model. This article critically examines the real business model and the assessment strategy, surveys the literature supporting and opposing the model, and evaluates the evidence on the empirical success of the model. It argues that, on the preponderance of the evidence, the real business cycle model is refuted.

I. THE REAL BUSINESS CYCLE CONJECTURE

The philosopher of science, Karl Popper (1959, 1972), argued that science progresses through a series of bold conjectures subjected to severe tests. Most conjectures are false and will be refuted. The truth, by definition, will survive the ordeal of testing and emerge unrefuted at the end of enquiry in an infinitely distant future. The boldest conjectures are often the most fruitful, because, making the strongest claims, they are the most readily refuted and their refutation narrows the universe of acceptable conjectures most rapidly. We argue that real business cycle models are bold conjectures in the Popperian mould and that, on the preponderance of the evidence, they are refuted. It is not, however, straightforward to see this, because the real busi-

ness cycle conjecture is advanced jointly with a claim that models should be assessed using a novel strategy. We must, therefore, evaluate the conjecture and the assessment strategy simultaneously.

Since the publication of Kydland and Prescott's 'Time to Build and Aggregate Fluctuations' (1982), the paradigm real business cycle model, a large and active group of new classical macroeconomists has elaborated and developed the real business cycle model. As important as these developments are to the real business cycle programme, none of them fundamentally affects the critical points that we shall make.[2] Our assessment will, therefore, focus on the original Kydland and Prescott model and its successor models in a direct line. We will also refer frequently to the programmatic statements and methodological reflections of Kydland, Prescott, and Lucas, the most articulate defenders of the aims and methods of equilibrium business cycle models.

(i) Equilibrium Business Cycles

To common sense, economic booms are good and slumps are bad. Economists have attempted to capture common sense in disequilibrium models: full employment is modelled as an equilibrium; that is, as a situation in which each worker's and each producer's preferences (given his or her constraints) are satisfied, while anything less than full employment represents a failure of workers or employers or both to satisfy their preferences. The real business cycle model is an extraordinarily bold conjecture in that it describes each stage of the business cycle—the trough as well as the peak—as an equilibrium (see, for example, Prescott, 1986a, p. 21). The source of the fluctuations is objective changes in aggregate productivity (so-called *technology shocks*). Thus, in the midst of a slump (i.e. a bad draw), given the objective situation and full information, every individual, and the economy as a whole, would choose to be in a slump.

This is not to say that workers and producers prefer slumps to booms. We all prefer good luck to bad (cf. Lucas, 1978, p. 242). Rather it is to deny that business cycles represent failures of markets to work in the most desirable ways. Slumps represent an undesired, undesirable, and unavoidable shift in the constraints that people face; but, given those constraints, markets react efficiently and people succeed in achieving the best outcomes that circumstances permit.

Contrary to the claims of some real business cycle proponents (e.g. Hodrick and Prescott, 1997, p. 1), there is no pre-Keynesian historical precedent for viewing business cycles as equilibria. Kydland and Prescott (1991) see such a precedent in the business cycle models of Ragnar Frisch (1933), while Lucas (1977, p. 215; 1987, p. 47 *inter alia*) sees such a precedent in the work of Hayek (1933, 1935) and other members of the Austrian School. Hoover (1988, ch. 10; 1995) demonstrates that these precedents are, at best, superficial. Frisch's business cycle models are aggregative and do not involve individual optimization. Some Austrians reject the notion of equilibrium altogether. Hayek, who is not among these, accepts dynamic equilibrium as an ideal case, but sees business cycles as the result of mismatches of capital type and quantity to the needs of production, transmitted to unemployment through a failure of wages and prices to adjust to clear markets in the short run—clearly a disequilibrium explanation.[3] The real business cycle model advances a novel conjecture as well as a bold one.

(ii) The Novelty of the Real Business Cycle Model

Novel in their bold conjecture, real business cycle models none the less have precursors. The primary antecedent is Robert Solow's (1956, 1970) neoclassical growth model. In this model, aggregate output (Y) is produced according to a constant-returns-to-scale production function $\Phi(\cdot)$ using aggregate capi-

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[2] As will become clear below, our focus is on real business cycles narrowly construed as perfectly competitive representative agent models driven by *real* shocks. A number of recent developments have extended models with roots in Kydland and Prescott (1982) to include monetary factors, limited heterogeneity among agents, non-Walrasian features, and imperfect competition. These models are ably surveyed in chs 7–9 of Cooley (1995b). One way to view this literature is as a constructive response to some of the difficulties with the narrow real business cycle model that we evaluate.

[3] Lucas (in Snowden *et al*., 1994, p. 222) accepts that his previous characterization of the Austrians as precursors to new classical business cycle theory was incorrect.

tal (K), aggregate labour (L), and a production technology indexed by Z:[4]

$$Y = \Phi(K, L, Z). \qquad (1)$$

Consumption follows a simple Keynesian consumption function:

$$C = (1 - s)Y, \qquad (2)$$

where s is the marginal propensity to save. Since Solow was interested in long-term growth, he ignored the aggregate demand pathologies that concerned earlier Keynesian economists and assumed that people's plans were coordinated so that savings (S) equalled investment (I) *ex ante* as well as *ex post*:

$$I = S. \qquad (3)$$

Capital depreciates at rate δ and grows with investment:

$$\dot{K} = I - \delta K = sY - \delta K, \qquad (4)$$

where $\dot{K}$ indicates the rate of change of capital. Labour grows exogenously at a rate n per cent per unit time, and labour-augmenting technology (Z) improves at a rate ζ per cent per unit time, so that *effective labour* grows at $n + \zeta$.

Under these circumstances, the economy will converge to a *steady state* in which the growth of capital after compensating for depreciation is just enough to match the growth of effective labour. Along the steady-state growth path, both capital and effective labour grow at a rate $n + \zeta$ and, since both inputs to production are growing at that steady rate, so is output itself.

In the Solow growth model we need to distinguish between equilibrium and steady state. The model is always in equilibrium, because *ex ante* savings always equal *ex ante* investment (equation (3)). But the model need not be in steady-state (i.e. growing at $n + \zeta$). Anything that drives the economy away from the steady state (for example, a change in s or n) will also produce changes in capital and output

(adjustments to a new steady state), but the economy remains in continuous equilibrium along the adjustment path.

Lucas (1975) employed the Solow growth model to solve a difficulty in his own analysis of business cycles. Lucas (1972, 1973) explained the business cycle as the reaction of workers and producers to expectational errors induced by monetary policy. To get from necessarily short-lived expectational errors to longer cycles, Lucas distinguished, in Ragnar Frisch's (1933) useful terminology, between *impulses* that begin a business cycle and *propagation mechanisms* that perpetuate a cycle. Expectational errors were the impulses, driving the economy away from steady state. *Ex post*, the economy was seen to be in disequilibrium until the expectational errors were corrected. But even when they had been corrected, the economy was returned to an equilibrium away from the steady state. The process of adjusting capital in order to regain the steady state would be a relatively slow one. This was the propagation mechanism.

In keeping with the new classical agenda of reducing macroeconomics to microeconomic foundations, Lucas replaced the stripped-down demand behaviour of the Solow growth model with the assumption that the behaviour of the aggregate economy can be described by the utility-maximizing choices of a *representative* agent, who chooses consumption and labour supply by solving a dynamic, intertemporal optimization problem. Aggregate demand pathologies are still impossible, because in Lucas's model the same agents make both the savings and the investment decision, which insures *ex-ante* coordination, and the agents have *rational expectations*, which ensure that mistakes about the future course of the economy are necessarily unsystematic. Furthermore, the supply of labour responds elastically to temporarily high real wages: workers make hay while the sun shines.

Kydland and Prescott's (1982) seminal real business cycle model is a direct outgrowth of Lucas's monetary growth model. It differs from Lucas's model in that there is no monetary sector; technology shocks (i.e. deviations of Z in equation (1) from

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[4] Equation (1) is a snap-shot of the economy at a particular time. In fact, variables in the model are growing. We could indicate this with subscripts indexing the relevant time, but this would simply clutter the notation unnecessarily.

trend) supply the impulse to business cycles. The model does not rely on expectational errors. There is no need; technological change has real effects regardless of whether or not it is anticipated.

Kydland and Prescott (1997, p. 210) state, 'We derive the business cycle implications of growth theory.' Seen in context, this is misleading. Historically, it is not the use of the growth model that distinguishes the real business cycle model from earlier business cycle models. Rather it is finding the impulses in technology shocks and modelling the economy in continuous equilibrium. In a realistically parameterized Solow model, technology shocks would be propagated rather slowly, for the convergence time to steady state is long (cf. Sato, 1966). The characteristic business cycle behaviour in real business cycle models comes from the shocks and from the optimizing model itself (of which more presently) rather than from the fact that these are embedded in a growth model.

(iii) A Quantified Idealization

Real business cycle models are implemented by giving specific functional forms to the equations of the optimal growth model. For example, equation (1) is replaced by the Cobb–Douglas production function:

$$Y = ZL^{\theta}K^{1-\theta}, \tag{1'}$$

where θ is the share of labour in national output. An equation such as (1') could be estimated as it stands, or jointly with the other equations in the model to determine the value of θ. Real business cycle proponents do not typically estimate the parameters of their models. Instead, they assign values to them on the basis of information from sources outside the model itself. This is known as *calibration* of the model. The value chosen for θ is usually the average value that the labour share takes in suitably adapted national accounts. The value of the depreciation rate (δ) is calibrated similarly. (Cooley, in this issue, discusses the issues related to establishing an appro-

priate correspondence between the real business cycle model and the national accounts, to permit the calibration of the model.)

Equations (2) and (3), which represent aggregate demand in the Solow growth model, are replaced in real business cycle models by an optimization problem for a representative agent who is both consumer and producer. The representative agent maximizes a utility function

$$U = U(\{C_t\}, \{L_t\}), \tag{5}$$

subject to current and future production constraints given by (1') and linked together by equation (4). The set $\{C_t\}$ is the set of current and future levels of consumption and $\{L_t\}$ is the set of current and future supplies of labour. The utility function must be calibrated as well. This is usually done with reference to the parameters estimated in unrelated microeconomic studies.[5]

The calibrated model is non-linear. To solve the model, its equations are typically reformulated as linear approximations around the unknown steady state. This is the technical sense in which real business cycle models abstract from the concerns of traditional growth theory; for no explanation of the steady state is sought; the focus is on (equilibrium) deviations from the steady state. The solution to the linearized model is a set of linear equations for output, consumption, labour, and investment of the form:

$$y = \gamma_{11}z + \gamma_{12}k, \tag{6.1}$$
$$c = \gamma_{21}z + \gamma_{22}k, \tag{6.2}$$
$$l = \gamma_{31}z + \gamma_{32}k, \tag{6.3}$$
$$i = \gamma_{41}z + \gamma_{42}k, \tag{6.4}$$

where the lower-case letters are the deviations from steady state of the logarithms of the analogous upper-case variables. The coefficients γ_{ij} are combinations of the calibrated parameters determined by solving the model.

[5] It is actually a debated question whether microeconomic studies do in fact provide the necessary parameters. Prescott (1986*a*, p. 14) cites Lucas's (1980, p. 712) argument that we have 'a wealth of inexpensively available data' of this sort. However, Hansen and Heckman (1996, pp. 93–4) argue that in this regard Prescott is wrong. As evidence they point to Shoven and Whalley (1992, p. 105) who rather candidly admit that 'it is surprising how sparse (and sometimes contradictory) the literature is on some key elasticity values. And although this procedure might sound straightforward, it is often exceedingly difficult because each study is different from every other.' (Cf. the debate between Summers (1986) and Prescott (1986*b*) about whether the parameters used in Prescott (1986*a*) are the appropriate ones.)

The right-hand variables in (6.1)–(6.4) are called state variables. They summarize the past evolution of the model economy and are exogenous in the sense that the representative agent takes them as given data, and conditions his choices upon them (z is exogenous and k is determined from choices made in previous periods). Equations (6.1)–(6.4) detail the outcomes of those choices—i.e. how the preferences of the representative agent interact with the constraints he or she faces, including the current state of z and k, to determine output, capital, labour, and investment.

In the original Kydland and Prescott (1982) model, the technology shock, z, was modelled as a random process with parameters chosen to cause the model to mimic the variance of GNP in the US economy. However, as Lucas (1987, pp. 43–5; cf. Prescott, 1986b, p. 31) noticed, *constructing* the predicted output series to mimic actual output does not provide an independent test of the model. Beginning with Prescott (1986a), real business cycle models have taken a different tack (cf. Kydland and Prescott, 1988). Typically, real business cycle models use the Cobb–Douglas production function (equation (1')) as follows:

$$\log(Z) = \log(Y) - \theta\log(L) - (1-\theta)\log(K). \quad (7)$$

This empirical measure of the technology parameter is known as the *Solow residual* (Solow, 1957). When estimated using actual data, the Solow residual, like the series used to compute it, has a trend (implying $\zeta \neq 0$), and so must be detrended before being used as an input to the real business cycle model. Detrended $\log(Z)$ is the state-variable z.[6]

(iv) The Limits of Idealization

The real business cycle model does not present a descriptively realistic account of the economic process, but a highly stylized or idealized account. This is a common feature of many economic models, but real business cycle practitioners are bold in their conjecture that such models nevertheless provide useful quantifications of the actual economy. While idealization is inevitable in modelling exercises, it

does limit the scope of the virtues one can claim for a model.

In particular, the real business cycle programme is part of the larger new classical economics, which is argued to provide satisfactory microfoundations for macroeconomics in a way that Keynesian models conspicuously fail to do (e.g. Lucas and Sargent, 1979). The claim that new classical models in general, and real business cycle models in particular, provide microfoundations is largely based on their use of a representative agent who solves a single dynamic optimization problem on behalf of all the consumers, workers, and firms in the economy. However, the claim that representative agent models are innately superior to other sorts of models is unfounded. There is no a priori reason to accord real business cycle models a presumption of accuracy because they look as though they are based on microeconomics. Rather, there are several reasons to be theoretically sceptical of such models.[7]

Most familiar to economists is the problem of the fallacy of composition, which Samuelson's (1948) introductory economics text prominently addresses. It is difficult to deny that what is true for an individual may not be true for a group, yet, representative agent models explicitly embody this fallacy of composition. The central conceptual achievement of *political* economy was to analogize from the concerns of Robinson Crusoe—alone in the world—to those of groups of people meeting each other in markets. The complexities of economics from Adam Smith's invisible hand to Arrow and Debreu's general equilibrium model and beyond have largely been generated from the difficulties of coordinating the behaviour of millions of individuals. Indeed, some economists have found the source of business cycles precisely in such coordination problems.

Problems of aggregation are similar to problems arising from the fallacy of composition. Real business cycle models appear to deal with disaggregated agents, but in reality they are aggregate models in exactly the same way as the Keynesian models upon which they are meant to improve. The conditions under which a representative agent could

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[6] Although we refer to z as 'the technology shock,' this terminology is not universal. Generally, z will be a persistent process; for example, $z_t = \rho z_{t-1} + \varepsilon_t$, with $\rho > 0$ and ε_t an independent, identically distributed random variable. Some economists identify ε_t as 'the technology shock.' Similarly, some economists identify z_t rather than Z_t as the 'Solow residual.'

[7] These reasons are elaborated in Hartley (1997).

legitimately represent the aggregate consequences of, and be deductively linked to, the behaviour individuals are too stringent to be fulfilled: essentially all agents must be alike in their marginal responses (Gorman, 1953; Stoker, 1993). Because it is impracticable, no one has ever tried to derive the aggregate implications of 260m people attempting to solve private optimization problems. The real business cycle model thus employs the formal mathematics of microeconomics, but applies it in a theoretically inappropriate circumstance: it provides the simulacrum of microfoundations, not the genuine article. It is analogous to modelling the behaviour of a gas by a careful analysis of a single molecule *in vacuo* or, of a crowd of people, by an analysis of the actions of a single android. For some issues, such models may work well; for many others, they will miss the point completely.

Kydland and Prescott argue that the models are designed to capture some features of the economy while ignoring or even distorting other features, holding this to be one of their virtues, and argue that their failure to capture features that they were not designed to model should not count against them (Kydland and Prescott, 1991). We take this claim seriously. It should, nevertheless, be noted that it undermines the argument that we trust the answers that the models give us on some dimensions because they have been successful on other dimensions (Lucas, 1980, p. 272). Kydland and Prescott (1996, p. 72) make exactly this claim with regard to using the Solow growth model to explore the business cycle. However, if the dimensions on which we need answers are ones on which, because of their idealized natures, the models are false, the success on other dimensions is irrelevant. As a point of logic, rigorous deductions are useful only if they start with true premises.[8] Idealized models are useful because they are tractable, but only if they remain true in the features relevant to the problem at hand. Kydland and Prescott want idealized real business cycle models to provide quantitative conclusions about the economy. There is nothing in their construction that ensures that they will succeed in doing so.

Thus, part of the boldness of the real business cycle conjecture is the seriousness with which it takes the idealization of a representative agent. Although economists, at least since Alfred Marshall, have sometimes used representative agents as a modelling tool, new classical (and real business cycle) models expect the representative agent to deliver far more than earlier economists thought possible. For example, Friedman's (1957) explication of the permanent-income hypothesis begins with something that looks like a representative agent, but Friedman uses the agent only as a means of thinking through what sorts of variables belong in the aggregate consumption function. He makes no attempt to derive an aggregate consumption function from his agent; in fact, he takes pains to note how different the aggregate function will look from the individual's function.

Real business cycle models, on the other hand, take the functions of the representative agent far more seriously, arguing that 'we deduce the quantitative implications of theory for business cycle fluctuations' (Kydland and Prescott, 1997, p. 211). However, for the reasons described above, these deductions are not the rigorous working out of microeconomic principles combined with a serious analysis of heterogeneity and aggregation. Indeed, it is surprising to see Cooley (this issue) point to the Sonnenschein–Mantel–Debreu results as a justification for using real business cycle methods. In an article published in the *Journal of Economic Perspectives*, Kirman (1992) ably pre-empted the use of this defence.

There is nothing in the construction of real business cycle models that ensures that they will succeed in providing accurate quantitative conclusions. There is nothing that guarantees a priori their superiority. The proof of the pudding is in the eating: the real business cycle model must be tested and evaluated empirically.

II. TESTING

(i) What are the Facts about Business Cycles?

Before real business cycle models can be tested, we must know precisely what they are meant to ex-

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[8] This seems to be Friedman's (1997, p. 210) point when he criticizes Kydland and Prescott's (1996) standards of empirical evaluation for calibrated models, saying 'There is a world of difference between mimicking and explaining, between 'can or may' and 'does'.'

plain. Following Prescott (1986*a*), advocates of real business cycle models have redefined the *explanandum* of business cycles. Business cycle theory has traditionally tried to explain what causes output to fall and then rise again. To be sure, when output declines one expects employment, income, and trade to decline as well. Nevertheless, the central fact to be explained was believed to be the decline and the subsequent recovery, and not the comovements of aggregate time-series.

Even before the first real business cycle models, new classical macroeconomics shifted the focus to the comovements. Lucas (1977, p. 217) argues that the movements of any single economic aggregate are irregular, and '[t]hose regularities which are observed are in the *comovements* among different aggregative time series'.[9] Real business cycle modellers view the business cycle in precisely the same way. The things to be explained are the correlations between time-series, and the typical assessment of the success or failure of a model is to compare the correlations of the actual time-series to those that result from simulating the model using artificially generated series for the technology shock (z). Formal statistical measures of the closeness of the model data to the actual data are eschewed. Prescott (1986*a*), for example, takes the fact that the model approximates much of the behaviour of the actual aggregates as an indicator of its success. In the case in which the model data predicts an empirical elasticity of output to labour greater than the theory, Prescott (1986*a*, p. 21) argues '[a]n important part of this deviation could very well disappear if the economic variables were measured more in conformity with theory. That is why I argue that theory is now ahead of business cycle measurement.'

Kydland and Prescott (1990) make similar arguments in opposing 'business cycle facts' to 'monetary myths.' For example, the real business cycle model predicts that the real wage is procyclical, while monetary business cycle models (Keynesian and monetarist) predict counter-cyclical real wages.

Kydland and Prescott (1990, pp. 13–14) argue that a correlation of 0.35 between money lagged one period and current output is too low to support the view that money leads output; while a correlation of 0.35 between the real wage and output is high enough to support the view that the real wage is procyclical. They argue that if measurements were made in closer conformity to theory, the second correlation would be higher. But, even as it stands, they take the 'business cycle facts' as supporting the real business cycle model.

Theory may be ahead of measurement. Given the best data in the world, however, simply mimicking the data is a weak test. One learns very little from knowing that a theory mimics the data—especially if it was designed to mimic the data. One also needs to know that the data cannot be mimicked by rival theories. Although real business cycle models are often shown (without any formal metric) to mimic actual data, they have rarely been tested against rivals.[10]

It is usually regarded as a more stringent test of a model that it performs well on a set of data different from the one used in its formulation. Most often this means that models are formulated on one sample and then tested against a completely different sample. Kydland and Prescott (1997, p. 210) offer a different argument: real business cycle models are formulated using the 'stylized facts' of *long-run growth theory* and are then tested, not against a completely different data set, but for their ability to mimic the *short-run business cycle behaviour* of the same data. While there is clearly merit in deriving empirically supported implications of one set of facts for another, this particular test provides very weak support for the real business cycle model. Many models that are fundamentally different from the real business cycle model, in that they posit neither continuous equilibrium nor impulses arising from technology shocks, are consistent with the 'stylized facts' of growth (e.g. the constancy of the labour share in national income or the constancy of the capital–output ratio).

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[9] See also Sargent (1979, p. 256). Prescott (1986*a*, p. 10) argues that the noun 'business cycle' should be avoided as it encourages people to believe incorrectly that there is an entity to be explained independently of economic growth. Instead, 'business cycle' should be used as an adjective, as in 'business cycle phenomena', that points to the volatility and comovements of various economic series. Lucas (1987, section V) recognizes that, to the extent that one is interested in questions of unemployment, models that aim to explain the comovements alone are silent on an important question; although, he argues that this is a limitation, not a fault.

[10] Farmer (1993) is an exception, see section II(ii) below.

Table 1
Summary Statistics for Actual US Data and for Two Real Business Cycle Models

Series	Actual US data 1953Q3–1984Q1		Artificial economy: divisible labour		Artificial economy: indivisible labour	
	Standard deviation	Correlation with output	Standard deviation	Correlation with output	Standard deviation	Correlation with output
Output	1.76	1.00	1.35 (0.16)	1.00 (0.00)	1.76 (0.21)	1.00 (0.00)
Consumption	1.29	0.85	0.42 (0.06)	0.89 (0.03)	0.51 (0.08)	0.87 (0.04)
Investment	8.60	0.92	4.24 (0.51)	0.99 (0.00)	5.71 (0.70)	0.99 (0.00)
Capital stock	0.63	0.04	0.36 (0.07)	0.06 (0.07)	0.47 (0.10)	0.05 (0.07)
Hours	1.66	0.76	0.70 (0.08)	0.98 (0.01)	1.35 (0.16)	0.98 (0.01)
Productivity	1.18	0.42	0.68 (0.08)	0.98 (0.01)	0.50 (0.07)	0.87 (0.03)

Source: Hansen (1985), Table 1. Standard deviations in parentheses.

(ii) Do Real Business Cycle Models Fit the Facts?

Although it is a weak test to check whether models mimic the facts, it is a useful starting point. The fact that real business cycle models are idealized presents some difficulties in judging them even on such a standard. As Kydland and Prescott (1991, p. 169) stress, the real business cycle model is unrealistic in the sense that it aims only to capture certain features of the data rather than to provide a complete explanation. There is no claim that it will do well in explaining correlations it was not designed to capture; nor is there any claim that its errors will be truly random.

The dilemma is this: theories are interpretable, but too simple to match all features of the data; rich econometric specifications are able to fit the data, but cannot be interpreted easily. The coefficients of a statistically well-specified econometric equation indicate the effects on the dependent variable *ceteris paribus* of a change in the independent variables. In general, these effects depend in a complicated way on the parameters of the deep relations that connect the variables together and generate the observed data. Lucas (1976) in his famous 'critique' of policy analysis noticed the lack of autonomy of econometrically estimated coefficients and argues, in particular, that the values of the coefficients would not remain stable in the face of changes in monetary and fiscal policy regimes.

One solution to the Lucas critique might be to identify the complex structure of the estimated coefficients. Hansen and Sargent (1980) map out a strategy for doing this. Essentially, the model is taken to be true and used to disentangle the 'deep' parameters (i.e. the parameters of the theory) from the estimated coefficients. The central difficulty with this strategy as a means of providing support for real business cycle models is that it does not work. In the case in which the model imposes more relationships among the parameters than there are parameters to identify, the model is said to be overidentified. Statistical tests can be used to assess whether the 'overidentifying restrictions' can be rejected empirically. Altug (1989) estimated an econometric version of the real business cycle model and tested its overidentifying restrictions. They were clearly rejected. This should not be surprising. An idealized model abstracts from too many of the features of the world for the resulting specification to meet the econometric ideal. Not only is it likely that the errors will not show irreducible randomness and the appropriate symmetry, but they are unlikely to be independent of omitted variables.

Kydland and Prescott advocate a second solution: eschew econometric estimation altogether. They believe that the advantage of the calibrated model is that it refers to theoretically interpretable parameters, so that counterfactual experiments can be given precise meanings: for example, the effects of

a change in the persistence of the technology shock or in the relative risk-aversion of consumers have precise analogues in the calibrated model. A good model in Kydland and Prescott's view is unrealistic in the sense that it will not fit the data in the manner of a statistically well-specified econometric model, but it will fit with respect to certain features of interest. Calibration and model structure are adjusted until the models do well against those features of the data that are of interest.

The development of the labour market in early real business cycle models provides an illustration of the strategy. Table 1 reproduces from Hansen (1985) some statistics for actual data and data generated from simulating two real business cycle models. Model I is a simple model similar to Kydland and Prescott (1982) in which labour is supplied in continuously variable amounts. The standard deviations of hours worked and productivity are nearly equal in Model I; while, in the actual data, hours worked are over 50 per cent more variable. Model II is a modification of Model I in which labour must be supplied in indivisible 8-hour units. Model II was created in part as an attempt to add realism to capture a feature that was not well described in Model I. In fact, it succeeds rather too well: hours are nearly three times as variable as productivity in Model II. Further developments of the real business cycle model (see Hansen and Wright, 1992) aim in part to refine the ability to mimic the data on this point.

A serious case can be made for choosing Kydland and Prescott's strategy for dealing with the Lucas critique and favouring idealized models at the expense of achieving the econometric ideal of complete description of the data (see Hoover, 1995). The gain is that one preserves theoretical interpretability—though only at the cost of a limited understanding of the actual economy. Real business cycle modellers might respond that the choice is between limited understanding and no genuine understanding at all. But this would be too glib. There are at least two barriers to declaring the triumph of the real business cycle approach on the basis of the methodological virtues of idealization.

First, most of the assessments of the success or failure of real business cycle models have been made in the casual manner exemplified by our

discussion of Hansen's (1985) divisible and indivisible labour models, using data no more precise than that of Table 1. The standard is what might be called 'aesthetic R^2': whether Models I or II in Table 1 are too far from the actual data or close enough is a purely subjective judgement without a good metric.

One response might be that no formal metric is possible, but that a more rigorous subjective evaluation would go some way to providing the missing standards. King and Plosser (1989) take this tack. They revive the method of Adelman and Adelman (1959), first used to evaluate the Klein–Goldberger econometric macromodel. King and Plosser simulate data from a real business cycle model and evaluate it using the business cycle dating procedures developed by Burns and Mitchell (1946) at the National Bureau of Economic Research. Both the actual data and the simulated data from the real business cycle model are processed using Burns and Mitchell's procedures. King and Plosser observe that it is difficult to discriminate between these two sets of data. But they note that the results 'leave us uncomfortable,' because the same claims can be made on behalf of the Keynesian Klein–Goldberger model. (Pagan, in this issue, provides an explanation for why all these models keep generating the same results.) Despite the greater detail in King and Plosser's study compared to typical assessments of real business cycle models, it is still wedded to aesthetic R^2.

In a similar vein, Hartley *et al*. (1997) examine the ability of the standard informal methods of assessment of real business cycle models to discriminate between alternative accounts of the actual economy. Hartley *et al*. use the Fair (1990) macroeconometric model of the US economy to simulate data for a 'Keynesian' economy in which demand shocks and disequilibrium are important. Calibrating a real business cycle to be consistent with the relevant parameters of the Fair model, they ask whether a real business cycle model, driven by technology shocks and continuous equilibrium, can mimic this 'Keynesian' economy. They find out that it can, to at least as high a degree as it mimics the actual economy on the usual standards used by real business cycle modellers. One interpretation of this result is that it is very bad news for the real business cycle model, because it shows that it has no power

of discrimination; its key assumptions do not restrict the sort of economies it can fit.

A real business cycle modeller, however, might riposte that the Fair model is a typical Keynesian model with so many free parameters that it gives a good statistical description of the economy even as it fails to model the true underlying mechanisms. Thus, the fact that the real business cycle model 'works' for simulations from the Fair model means nothing more than that it works for the actual economy. To check this interpretation, Hartley *et al.* alter two key parameters—those governing the interest elasticities of money demand and investment—changes which makes the simulations of the Fair model (particularly, the cross-correlations stressed by real business cycle analysts) behave more like European economies (see Backus and Kehoe, 1992). The real business cycle model is poor at mimicking the data from the altered Fair model. One might conclude that the real business cycle model is, in fact, discriminating. However, for a modelling strategy that takes pride in its grounding in fundamental and universal economic theory (the Solow growth model is not country-specific), this is hardly an attractive conclusion. Although European business cycles may be substantially different from American business cycles because of important institutional differences, real business cycle models typically seek to explain business cycles abstracting from those very institutional details.

A second barrier to declaring the triumph of the real business cycle model on the basis of the methodological virtues of idealization is that, even if idealized models cannot be expected to fit as well as traditional econometric specifications under the best of circumstances, the conclusion that econometric estimation is irrelevant to the real business cycle model would be unwarranted. Calibration might be regarded as a form of estimation (Gregory and Smith, 1990, 1991). The problem is how to judge the performance of calibrated models against an empirical standard. Watson (1993) develops an asymmetrical measure of goodness of fit that is useful for real business cycle models precisely because their idealized nature makes it likely that the errors in fitting them to actual data are systematic rather than random. Even using his goodness-of-fit measure, Watson fails to produce evidence of high explanatory power for real business cycle models.

Kydland and Prescott's (1991) objection to traditional econometrics is that an idealized model will not provide the necessary restrictions to permit the accurate estimation of its own parameters on actual data, because of the many features of the data that it systematically and intentionally ignores. Canova *et al.* (1994) undertake a somewhat less demanding test. Where Altug (1989) had tested restrictions that were strong enough to identify all the parameters of the real business cycle model and, therefore, to eliminate the need for calibration, Canova *et al.* examine the implications of a previously calibrated real business cycle model for the dynamic behaviour of various time-series. They observe that the various time-series can be described by a *vector autoregression* (VAR). If the real business cycle model is an accurate description of the actual data, then a number of restrictions must hold among the estimated parameters of the VAR.

The real business cycle model implies three sets of restrictions on the VAR of two distinct types. First, various time-series should be *cointegrated*. Two series are cointegrated when a particular linear combination of them is stationary (i.e. when its mean, variance, and higher moments are constant) even though the series are not separately stationary. There are two sets of such cointegration restrictions: (i) the state variables (the analogues of Z and K, the non-detrended counterparts to the state variables in equations (6:1)–(6:4)) must stand in particular linear relationships; (ii) state variables and predicted values for various time-series (e.g. the left-hand variables in equations (6:1)–(6:4)) must also stand in particular linear relationships. Finally, once one has accounted for the cointegrating relationships among these time-series and concentrates on their behaviour about their common trends, there is a third set of restrictions (second type), which are the particular implications of the real business cycle model for the parameters of the VAR.

Canova *et al.* use a calibrated real business cycle model with a considerably richer specification than Kydland and Prescott's early models to derive the necessary restrictions on the VAR. These restrictions are then compared to the data. Canova *et al.* show that the restrictions do not hold. A particularly interesting finding is that the real business cycle model imputes too much importance to the productivity shock.

Canova *et al.*'s imposition of a specific numerical calibration of the real business cycle model might limit the generality of their results: it might be said that the real business cycle model is correct in principle, but Canova *et al.* have failed to calibrate it correctly. In defence of their test, their choice of parameters is not at all atypical and they do examine a limited range of alternative parameters. Their results along these lines, however, are not nearly as comprehensive as they would need to be to close the case.

Eichenbaum (1991) examines the issue of parameter choice more systematically. He begins by noting that the numerical values of the underlying parameters used to calibrate a real business cycle model are simply estimates of the true values. We do not *know* the true values of things such as the depreciation rate or the variance of the shock to the Solow residual. Instead, we estimate these numbers from sample data, and there is a sampling error associated with every estimate.

Eichenbaum finds that altering most of the parameters within the range of their sampling error does little to alter the behaviour of the real business cycle model. The notable exceptions are the parameters associated with the Solow residual, which have large standard errors. He finds that at standard levels of statistical significance (5 per cent critical values), technology shocks may account for as little as 5 per cent and as much as 200 per cent of the variance in output. Eichenbaum's results suggest that, even if real business cycle models had no other problems, we cannot reject the view that technology shocks in conjunction with a real business cycle model explain only a small fraction of aggregate fluctuations.

Although not decisive, conventional econometric tests of real business cycle models are not kind to the theory. The qualifications surrounding any one of the tests described above remind us that no test of the real business cycle is likely on its own to provide a decisive Popperian refutation. The very fact that the models are idealized implies that the actual data alone provide at best a weak standard. More important than simply fitting the data, is the *relative* performance of alternative models. Canova *et al.*

and Hartley *et al.* push in the right direction, though not terribly far. Of course, the advocates of real business cycle models have always judged them relatively against other models in their class. Hansen's (1985) model with indivisible labour was judged superior to his model with divisible labour. Other models have included a monetary sector (Cooley and Hansen, 1989), a government sector (Christiano and Eichenbaum, 1992), household production (Benhabib *et al.*, 1991) or variable capacity utilization (Greenwood *et al.*, 1988).

All of these models, however, retain the common core of the original Kydland and Prescott real business cycle model. The only substantial comparison between a real business cycle model and one with quite different principles of construction is found in Farmer's (1993) model of an economy with increasing returns to scale and shocks to 'animal spirits'. In Farmer's model there are multiple equilibria, and where the economy ends up depends upon the self-fulfilling expectations of consumers. Farmer argues that his model performs better than the real business cycle model using Kydland and Prescott's standard of mimicking the relative correlations of actual data. He also claims that his model captures the dynamics of the economy more accurately. He estimates vector autoregressions for the actual economy and uses the estimated equations to generate the path the economy would follow after shocks to the various variables (i.e. *impulse response functions*). He then compares the impulse response functions of the real business cycle model, his model with multiple equilibria, and the estimated VARs. He finds that the impulse responses of the real business cycle model are very different from his model and that his model is more like those from the VAR. Once again, the appeal is to aesthetic R^2. Further work on standards of comparison is much to be desired.[11]

(iii) Testing the Elements of the Real Business Cycle Model: The Impulse Mechanism

Rather than assessing the performance of the real business cycle model directly against the data, we can ask how well its fundamental components succeed. As we noted earlier, one of two distinguishing features of the real business cycle model is

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[11] A strategy for the assessment of idealized models is discussed in Hoover (1994).

that it locates the impulse to business cycles in technology shocks. The overarching question is, what evidence do we have that technology shocks are the principal impulse driving the business cycle?

Before we can answer that question, however, another more basic one must be answered: what are technology shocks? This question has plagued real business cycle research from the beginning (see, for example, Summers, 1986). At the formal level, technology shocks are just the deviations of the parameter Z in the aggregate production function (e.g. equations (1) or (1') above) from its steady-state growth path: we represented these shocks earlier as z. By analogy to the microeconomic production function for individual products, one might naturally interpret z as a change in physical technique or organizational ability.

An aggregate measure should average out shocks to particular technology, so that z should measure shocks that have widespread effects across the economy. Such averaging should reduce the variability of the aggregate shocks relative to the underlying shocks to individual technologies. However, in order to make the real business cycle model match the variability of US output, the technology shocks must be relatively large and persistent: Kydland and Prescott (1982) model z as an autoregressive process with a half-life of about 14 quarters and a standard deviation of 2.9 per cent of trend real per capita GDP. Our calculations for the period 1960Q1–1993Q1 are similar, yielding a standard deviation for z of 2.8 per cent and for GDP *per capita* of 4.4 per cent about trend.

Although technology is improving over time, Kydland and Prescott's assumptions about the variability of z imply that technology must sometimes regress. But as Calomiris and Hanes (1995, pp. 369–70) write: 'Technological regress does not appear to correspond to any event in Western economic history since the fall of the Roman Empire.' Elsewhere they point to the large literature on

the introduction and diffusion of particularly important technologies through history: even for such crucial technological developments as the steam engine, the electric motor, and the railroad, the speed of diffusion is relatively slow, so that new technologies take decades rather than quarters to spread through the economy. Calomiris and Hanes (1995, p. 369) conclude that

the diffusion of any one technological innovation could not increase aggregate productivity by more than a trivial amount from one year to the next. If no single innovation can make much of a difference, it seems extremely unlikely that the aggregate rate of improvement could vary *exogenously* over cyclical frequencies to an important degree.

In the face of such objections, proponents of real business cycle models have broadened the scope of technology to include 'changes in the legal and regulatory system within a country' (Hansen and Prescott, 1993, p. 281). Fair enough, such changes may be important to the economy and may plausibly be negative; but are they likely to justify quarter-to-quarter variation in z of the required amount? Furthermore, as Calomiris and Hanes (1995, p. 370) point out, regulatory and legal intervention in the US economy was substantially smaller before the First World War, when business cycles themselves were more variable.[12]

Debates over the size and frequency of technology shocks are difficult to resolve because the shocks are not directly observable. Real business cycle models have generally used the Solow residual (equation (7) above) as a proxy for technology shocks. The Solow residual attributes to technology any change in output that cannot be explained by changes in factor inputs. Jorgenson and Griliches (1967) and Griliches (1996) point out that the Solow residual measures more than underlying technological change (a fact recognized by Solow, 1957, p. 312, himself), picking up, among other things, variability in capital utilization and labour hoarding.[13] Summers (1986) reiterates these points in the con-

[12] This claim is controversial. Romer (1986*a,b*, 1989) argues that post-war business cycles are not substantially less variable than those of the 19th century. Weir (1986) and Balke and Gordon (1989) challenge Romer's revisionism. The debate is updated and assessed in Siegler (1997), which, on the basis of better estimates of 19th century GNP, supports the traditional view that modern business cycles are in fact smoother than those of the 19th century.

[13] Solow (1957, pp. 314, 320) explicitly observes that idle capacity biases the measure and that the measure hinges on the assumption of factors being paid their marginal products. Solow (1990, p. 225) argues that he never intended the Solow residual as a suitable measure of anything but the *trend* in technology: 'the year-to-year behavior of the residual could be governed by all sorts of "technologically" irrelevant short-run forces. I still think that . . .'

text of real business cycle models. Hall (1986, 1990) notes that calibrating the parameters of the Cobb-Douglas production function (equation (1')), θ and $(1-\theta)$, as the shares of labour and capital in output in the calculation of the Solow residual (as in equation (7)), requires the assumption of perfect competition so that firms and workers are paid their marginal products and factor shares exactly exhaust output. But if firms have market power so that price exceeds marginal cost, factor shares will no longer coincide with the technological parameters θ and $(1-\theta)$ and z will reflect variations in mark-ups across the business cycle as well as true technology shocks. Hall (1990) also demonstrates that if there are increasing returns to scale, the Solow residual will move with things other than pure technology shocks.

Jorgenson, Griliches, and Hall conclude that the Solow residual captures a great deal besides technology. Hartley (1994) provides evidence that the Solow residual may not reliably capture even genuine technology shocks. The evidence is found in simulated economies constructed using Hansen and Sargent's (1990) flexible, dynamic linear-quadratic equilibrium macromodel, which permits a relatively rich specification of the production technology: there are multiple sectors, including intermediate and final goods, and parameters representing multiple aspects of the production process. Hartley generated series for output, capital, and labour after shocks to specific parts of the production process. Because these were simulations, the variability in these series is known to arise only from technology shocks and not market power, labour hoarding, and the like. For a range of plausible parameters, Hartley found an extremely low correlation between his controlled technology shocks and the Solow residuals calculated from the simulated series. Often, the correlation was not even positive. The failure of the Solow residual accurately to capture the underlying process appears to reflect the fact that the Cobb–Douglas production function, implicit in the calculation of Solow residuals, is a poor approximation to the rich production details of Hansen and Sargent's model: the quarterly Solow residuals largely reflect specification error rather than technological change. Hansen and Sargent's model is rich relative to the typical idealized real business cycle model, but is itself an extreme idealization of the real production process. Hartley's simulation results *a fortiori* call

the Solow residual into question as a measure of actual technology shocks.

(iv) Testing the Elements of the Real Business Cycle Model: The Propagation Mechanism

The propagation mechanism of a business cycle model should transmit and amplify the impulses to the various cyclical aggregates. Together with the shocks themselves it should account for the pattern of fluctuations in each series and for their comovements. Real output is generally taken as the marker series for the business cycle. The balance of evidence is that real business cycle models add relatively little to the pattern of fluctuations in real output beyond what is implicit in the technology shocks themselves. Watson (1993) uses spectral analysis to decompose the power of the real business cycle model to match movements in output at different frequencies or (equivalently) time horizons. He finds that the spectral power of the real business cycle model is high at low frequencies (corresponding to trend or long-term growth behaviour), but low at business cycle frequencies (approximately 2–8 years). Cogley and Nason (1995*b*) compare the dynamic pattern of the technology shocks fed into the real business cycle model with the predicted time-series for output generated by the model. Again, they find that it is the dynamic properties of the exogenous inputs that determine the properties of the output with the model itself contributing almost nothing. In one sense, these results should not be surprising: the Solow growth model, the foundational model of the real business cycle model, was originally meant to capture secular change. It is bold to conjecture that, unaltered, it would also model the business cycle. What is more surprising is that it took relatively long to document its low value-added with respect to business cycles.

Part of the reason that the real business cycle model has appeared to do well is that its proponents have relied on standards of assessment that are not particularly discriminating (see section II(ii) above). Part of the reason has to do with the standard practices of real business cycle modellers with respect to handling data. The real business cycle model predicts values for output, consumption, investment, and other time-series expressed as deviations from the steady state. In order to compare these with actual data, an estimate of the steady

state must be removed from these variables, which typically are trending. The Solow growth model suggests that all these variables should grow at rates related to the steady-state growth rate. Unfortunately, that is not observable. In practice, real business cycle models follow one of two strategies to generate detrended data. They sometimes remove a constant exponential trend, which is linear in the logarithm of the series, and so is known as *linear* detrending (e.g. King *et al*., 1988). This would be accurate if the rate of growth of the labour force (*n*) and of technology (ζ) were constant over time. But there is no reason to think that this is so. An alternative strategy is to use a slowly varying trend that effectively allows the steady-state growth rate to be variable. This is the most popular option and it is typically implemented using the Hodrick–Prescott (HP) filter (Hodrick and Prescott, 1997).[14] The HP filter is a non-linear regression technique that acts like a two-side moving average. As we noted, and as Prescott (1986*a*) asserts, one should, in principle, model growth and cycles jointly (see also Kydland and Prescott, 1996). In practice, however, real business cycle models express the interrelationships of data as deviations from the steady state. So, in effect, the HP filter provides an *atheoretical* estimate of the steady-state growth path.

Harvey and Jaeger (1993) analyse the usefulness of the HP filter in accomplishing this task. They compare the cyclical component for output generated from an HP filter to that from a structural time-series model in which the trend and the cyclical component are estimated jointly. (This is closer to what Kydland and Prescott advocate in principle than to what they actually practice.) For US GDP both detrending methods produce similar cyclical components. Harvey and Jaeger, however, demonstrate that the HP filter is wildly different from the structural time-series model for several other coun-

tries. This underscores the previously cited finding of Hartley *et al*. (1997) that the real business cycle model matches US data but not artificial data of a more 'European' character.

Harvey and Jaeger also show that the HP filter and the structural time-series model differ substantially when applied to other US time-series—particularly in the case of US prices and the monetary base. Given Kydland and Prescott's impassioned attack on the 'monetary myths' of the business cycle, it is obviously critical to know whether the facts about money and prices are independent of the filtering process. Furthermore, Harvey and Jaeger demonstrate that in small samples the HP filter can induce apparent cyclical fluctuations and apparent correlations between series even when the pre-filtered series are independent and serially uncorrelated. As they point out, these results are in the same spirit as Slutsky's and Yule's analyses of spurious cyclical behaviour (Yule, 1926; Slutsky, 1927/1937; more recently, see Nelson and Kang, 1981). This phenomenon has been long known if not fully appreciated. Simon Kuznets, for example, 'discovered' long cycles in US data that had first been transformed through two separate moving averages and first differencing. It can be shown that purely random data subjected to such transformations present precisely the same 20-year cycles that Kuznets reported: they are nothing but an artefact of the filtering (see Sargent, 1979, pp. 249–51). The analogy between the HP filter and Kuznets' transformations is close because the HP filter acts as a type of two-sided moving average.

Cogley and Nason (1995*a*) reinforce Harvey and Jaeger's analysis; they demonstrate that pre-filtered data do not generate cycles in a real business cycle model, while HP-filtered data do. Furthermore, when the input data are highly serially corre-

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[14] The HP filter is defined as follows: Let $x_t = \bar{x}_t + \hat{x}_t$, where $\bar{x}_t$ denotes the trend component and $\hat{x}_t$ denotes the deviation from trend. The HP filter chooses this decomposition to solve the following problem:

$$\min\left\{(1/T)\sum_{t=1}^{T}\hat{x}_t^2 + (\lambda/T)\sum_{t=2}^{T-1}\left[(\bar{x}_{t+1}-\bar{x}_t)-(\bar{x}_t-\bar{x}_{t-1})\right]^2\right\}.$$

T is the number of observations and λ is a parameter that controls the amount of smoothness in the series: if λ = 0, then the smooth series is identical to the original series; if λ = ∞, the smoothed series is just a linear trend. Hodrick and Prescott use a value of λ = 1,600 for quarterly data on the ground that this replicates the curve a business cycle analyst might fit free-hand to the data. With no better justification than this, λ = 1,600 has become the standard choice for the smoothing parameter in the real business cycle literature.

lated, the HP filter not only generates spurious cycles but also strongly increases the correlation among the predicted values for output, consumption, investment, hours of work, and other values from the real business cycle model. The model itself, i.e. the propagation mechanism, does little of the work in generating the cyclical behaviour; the HP filter does the lion's share.

The use of the HP filter calls into question the very facts of the business cycle. Kydland and Prescott (1990) document the intercorrelations among HP-filtered time-series. These correlations are held by real business cycle modellers to provide strong *prima facie* support for the real business cycle model (Kydland and Prescott's subtitle to their 1990 paper is 'Real Facts and a Monetary Myth'). For example, they show that the correlation between HP-filtered real GDP and HP-filtered prices is –0.50, and claim that this contradicts the prediction of Keynesian models that prices are procyclical. Harvey and Jaeger (1993) not only show that the HP filter can induce such correlations, they also show that it adds statistical noise, so that a genuine correlation would in a sample size of 100 have to exceed 0.40 before we could be sure that it was statistically significant at the standard 5 per cent critical value. If such correlations are really artefacts of a filtering procedure, with no particular grounding in the economics of the business cycle, then the support of the 'real facts' for the real business cycle model is substantially weakened.

Prescott (1986*a*, p. 10) wrote: 'If the business cycle facts were sensitive to the detrending procedure used, there would be a problem. But the key facts are not sensitive to the procedure if the trend curve is smooth.' The weight of evidence since Prescott wrote this suggests that he is incorrect: the facts are sensitive to the type of filtering that defines the trend.

While there is good reason to find some way to detrend the technology shock series used as an input into the real business cycle model, it is also standard practice to HP filter the predicted series generated by the real business cycle model before checking their intercorrelations and comparing them to the HP-filtered actual data. Harvey and Jaeger's and Cogley and Nason's analyses suggest that this practice raises the correlations among these series artificially.

Kydland and Prescott (1996, p. 76, fn 7) defend the use of the HP filter against critics who have argued that it induces spurious cycles by stating that deviations from trends defined by the HP filter 'measure nothing' but instead are 'nothing more than well-defined statistics'; and, since 'business cycle theory treats growth and cycles as being integrated, not as a sum of two components driven by different factors . . . talking about the resulting statistics as imposing spurious cycles makes no sense'. The logic of Kydland and Prescott's position escapes us. It is true that real business cycle theory treats the business cycle as the equilibrium adjustments of a neoclassical growth model subject to technology shocks. But, as we have previously noted, the real business cycle model does not, in practice, model the steady state. The HP filter is an atheoretical method of extracting it prior to the economic modelling of the deviations from the steady state. The implicit assumption is that the extracted trend is a good approximation of the steady state, for which no evidence is offered. This does not say that the steady state could not be modelled jointly with the deviations in principle. But that it is not actually modelled jointly in practice means that the objection to the HP filter raised by many critics remains cogent. The work of Harvey and Jaeger and Cogley and Nason, which Kydland and Prescott wish to dismiss, demonstrates that the choice of which *ad hoc* method is used to extract the balanced-growth path greatly affects the stochastic properties of the modelled variables and their relationships with the actual data.

One way of reading Watson (1993) and Cogley and Nason (1995*a*) is that, while a model driven by a technology shocks fits output well, it is the technology shocks, not the model, which are responsible for that fact. The picture painted is one of the real business cycle model as a slightly wobbly transmission-belt converting the time-series characteristics of the technology shocks into the model's predictions for real output. But in the end there is a good fit between the model and real output. King (1995) and Hoover (1997) suggest that if the Solow residual is taken as the proxy for technology shocks then this success is an illusion.

Figure 1
Real US GNP and GNP Predicted from a Real Business Cycle Model
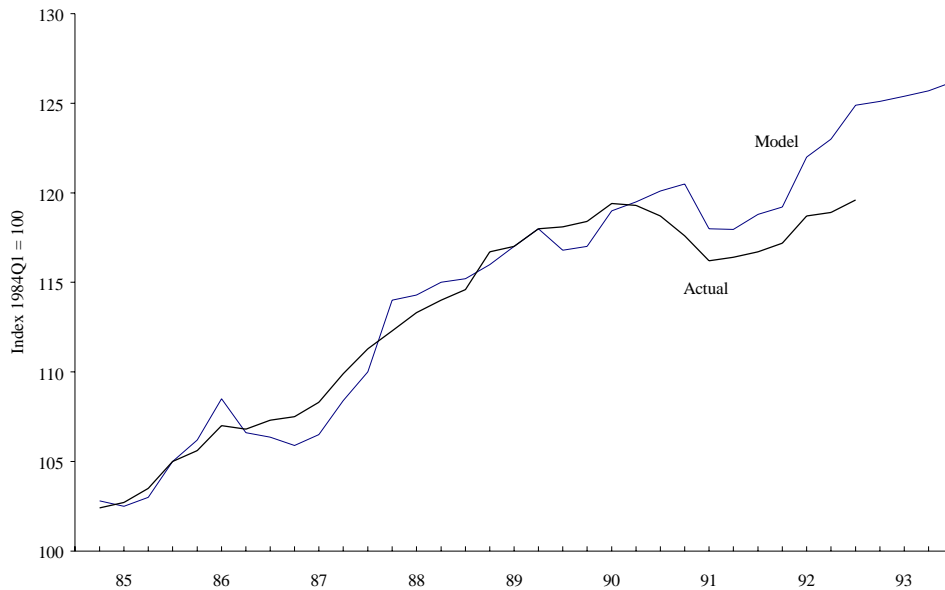
Source: Hansen and Prescott (1993, Table 4).

Despite having rejected in earlier work the relevance of direct comparisons to historical data (e.g. Prescott, 1986*a*), real business cycle models have recently made precisely such comparisons. Hansen and Prescott (1993) ask whether technology shocks can explain the 1990–1 recession in the United States, while Cooley (1995*a*) asks whether they can explain the 'Volcker recessions' of 1980–2. In each case, the predictions of a real business cycle model are compared directly to the historical path of real output.

Again, the standard is one of aesthetic R^2, and the pitfalls of this standard are easily seen in Figure 1, which reproduces Hansen and Prescott's (1993) Figure 4. Hansen and Prescott cite the fact that the output predicted from their real business cycle model tracks actual output as favourable evidence for its explanatory power. In particular, they note that the model catches the fall in output in 1990–1. But look more closely. Actual GNP peaks in the first quarter of 1990, while model GNP peaks in the fourth quarter; actual GNP bottoms out in the first quarter of 1991, while model GNP bottoms out in the second quarter. Furthermore, the model predicts two earlier absolute falls in GNP, while, in fact, there are no other recessions in the data. One of these predicted falls is actually on a larger scale than the genuine recession of 1990–1: the model shows that GNP peaks in first quarter of 1986 and falls 2.3 per

cent to a trough in the fourth quarter of 1986, where in reality GNP rose the entire time. The actual fall in GNP in the 1990–1 recession is only 1.6 per cent.

The difficulties of using aesthetic R^2 to one side, these graphical measures, or their more statistical counterparts (e.g. see Smith and Zin, 1997), offer no support for the real business cycle model. To see the difficulty, consider a simple version of a real business cycle model in which we abstract from time trends. The Solow residual (z_t) can be calculated in log-linear form:

$$\log(z_t) = \log(Y_t) - (1-\theta)\log(K_t) - \theta\log(L_t). \quad (8)$$

The log-linear version of the production function is given by

$$\log(Y_S) = \log(z_t) + (1-\theta)\log(K_t) + \theta\log(L_S), \quad (9)$$

where the S subscripts refer to variables determined in the model. Substituting (8) into (9):

$$\begin{aligned}\log(Y_S) &= \log(Y_t) - (1-\theta)\log(K_t) - \theta\log(L_t) \\ &\quad + (1-\theta)\log(K_t) + \theta\log(L_S) \\ &= \log(Y_t) - \theta[\log(L_t) - \log(L_S)]. \quad (10)\end{aligned}$$

There is an artefactual element to the correlation between predicted and actual output. How well

predicted output fits actual output is seen to depend on how well predicted labour fits actual labour. Actual output shows up on the right-hand side of equation (10) only because we put it there in the construction of the Solow residual, not because the model generated it by closely matching the structure of the economy.[15]

Of course, it would be a marvellous testament to the success of the model if the right-hand side of equation (11) turned out to be very nearly $\log(Y_t)$. That would occur because the model's predicted labour was very nearly the actual labour. However, the Solow residual also contains current information about labour by construction. Truly revealing tests of the success of the real business cycle model at capturing the true propagation mechanism based on comparisons of the predictions of the model against historical time-series should then concentrate on those series (e.g. consumption), the current values of which play no part in the construction of measures of the technology shocks. We know of no work to date that has systematically investigated the propagation mechanism of the real business cycle model independently of the Solow residual.

III. REFUTATION?

The history of real business cycle models illustrates a fact well known to philosophers and historians of science: it is rare for a conjecture—however bold— to be refuted *simpliciter* on the basis of a single experiment or a single observation, as in Popper's ideal case. Accumulated evidence may, none the less, render the intellectual cost of persisting in a particular conjecture (model or theory) higher than the cost of abandoning or modifying it. To some extent, it does not appear to be controversial that the evidence weighs against the real business cycle programme narrowly construed. Even the best-known advocates of real business cycle models, have tended to move away from models of perfectly competitive representative agents driven by technology shocks only (see fn. 1). While these models are direct descendants of the real business cycle model and remain in the broader class of equilibrium business cycle models, they represent an abandon-

ment of the strongest form of the real business cycle conjecture. The balance of the evidence presented here is that they are right to abandon it. Although there can be no objection to investigating just how far these new models can be pushed, there is little in the evidence with respect to the narrower real business cycle conjecture that would warrant much optimism about their success.

The case against the real business cycle conjecture has several parts. First, the propagation mechanism (i.e. the Solow growth model), while it provides, to a first approximation, a reasonable account of long-term growth, has virtually no value-added with respect to business cycles. The growth model will transmit fluctuations at business cycle frequencies from impulses that are already cyclical, but it will not generate them from non-cyclical impulses.

The putative impulse mechanism is the fluctuation of technology. In the model itself this amounts to shifts in a disembodied parameter (Z). The proponents of real business cycle models have given very little account of what features of the world might correspond to Z and fluctuate in the way needed to produce business cycles. Z is an unexplained residual in every sense of the word: it is whatever it has to be to make the real business cycle model behave in an appropriate manner, and it cannot be independently observed. If measured as the Solow residual, 'technology' means whatever bit of output that cannot be accounted for by capital and labour inputs. Using this residual output as an impulse cannot yield predicted values for output that provide a logically sound independent comparison between the model and the actual data on the dimension of output.

While valid comparisons might be made on other dimensions, the actual evidence in favour of real business cycles is weak in the sense that it does not provide discriminating tests: alternative models do as good a job in mimicking the data on the usual aesthetic standards as does the real business cycle model. Both the facts to be explained and the ability of the models to match those facts are themselves frequently distorted by the common data-handling techniques (particularly the HP filter). These data

[15] Hoover and Salyer (1996) provide simulation evidence that the Solow residual does not convey useful information about technology shocks and that the apparent success of real business cycle models in matching historical data for output is wholly an artefact of the use of current output in the construction of the Solow residual.

problems, combined with the fact that the highly idealized nature of the real business cycle models limits the ambitions that their advocates have for the matching the actual data, insulates the model from decisive refutation, but equally well undercuts the role of empirical evidence in lending positive support to it.

The real business cycle model has for 15 years dominated the agenda of business cycle research. On balance, however, there is little convincing empirical evidence that favours it over alternative models. To its advocates, the paucity of evidence may not be of too much concern, for Kydland and Prescott (1991, p. 171) argue that the confidence one places in a model to answer economic questions cannot 'be resolved by computing some measure of how well the model economy mimics historical data. . . . The degree of confidence in the answer depends on the confidence that is placed in the economic theory being used.' But anyone who believes that theories must be warranted by evidence has little reason to date to place much confidence in real business cycle models.

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