OXFORD REVIEW OF ECONOMIC POLICY, VOL. 13, NO. 3

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-

[1] This paper is based on the introduction to Hoover, Hartley, and Salyer (eds), *Real Business Cycles: A Reader*, forthcoming from Routledge, London.

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ness cycle conjecture is advanced jointly with a claim that models should be assessed using a novel

This is not to say that workers and producers prefer slumps to booms. We all prefer good luck to bad (cf.

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

productivity (so-called *technology shocks*). Thus, in the midst of a slump (i.e. a bad draw), given the

(ii) The Novelty of the Real Business Cycle Model

models none the less have precursors. The primary antecedent is Robert Solow's (1956, 1970) neoclas-

vidual, and the economy as a whole, would choose

(Y) is produced according to a constant returns to

fail (K, aggregate labour (L), and a production (adjustments to a new steady-state) but the economy

Lucas (1975) employed the Solow growth model to

consumption follows a simple Keynesian consump- solve a difficulty in his own analysis of business

tion function cycles. Lucas (1977, 1973) explained the business

cycle as the reaction of workers and producers to

get from necessarily short-lived expectational er-

cerned earlier Keynesian economists and assumed *mechanisms* that perpetuate a cycle. Expectational

corrected, the economy was returned to an equilib-

capital depreciates at rate δ and grows with investment:

adjusting capital in order to regain the steady state

gation mechanism.

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

Labour grows exogenously at a rate n per cent per tions, Lucas replaced the stripped-down demand

unit time and labour-augmenting technology (A) behaviour of the Solow growth model with the more realistic

improves at a rate ζ per cent per unit time, so that assumption that the behaviour of the aggregate

choices of a *representative* agent, who chooses

consumption and labour supply by solving a dy-

capital after compensating for depreciation is just rate demand fluctuations are still impossible, be-

so is output itself. about the future course of the economy are neces-

In the Solow growth model we used to distinguish labour responds elastically to temporarily high real

always in equilibrium, because *ex ante* savings

the model need not be in steady-state (i.e. growing ness cycle model is a direct outgrowth of Lucas's

at $n + \zeta$). Anything that drives the economy away monetary growth model. It differs from Lucas's

[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

time with subscripts indicating the relevant time, but this would simply clutter the notation unnecessarily.

trend) supply the impulse to business cycles. The model does not rely on expectations. There is no need for unanticipated changes in real aggregates regardless of whether or not it is anticipated.

Kydland and Prescott (1997, p. 210) state, 'We have no use for the concept of the natural rate of unemployment.' Seen in context, this is misleading. Historically, it is for the use of the natural rate hypothesis that Kydland and Prescott criticize Friedman and Phelps. 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). $\{C_t\}$ is the set 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]

$$l = \gamma_{31} z + \gamma_{32} k, \tag{6.3}$$

$$i = \gamma_{41} z + \gamma_{42} k, \tag{6.4}$$

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

(iv) The Limits of Idealization

economists have found the source of business cy-

is a common feature of many economic models, but ness cycle models is appear to deal with disaggregated

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

some statistics for actual data and data generated

Model I; while, in the actual data, hours worked are

supplied in indivisible 8-hour units. Model II was created in part as an attempt to add realism to

Model I. In fact, it succeeds rather too well: hours are nearly three times as variable as productivity 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 ex-

than that of Table I. The standard is what might be called 'aesthetic *R*²': whether Models I or II in enough is a purely subjective judgement without a good metric.

One response might be that no formal metric is

ation would go some way to providing the missing

econometric macromodel. King and Plosser simu-

dures developed by Burns and Mitchell (1946) at the National Bureau of Economic Research. Both the business cycle model are processed using Burns

serve 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

real business cycle models, it is still wedded to aesthetic *R*².

In a similar vein, Hartley *et al.* (1997) examine the

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

A real business cycle modeler, 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 [illegible] nothing more than that it works for the actual economy. To check this interpretation, Hartley *et* [illegible] Fair model (particularly, the cross-correlations stressed by real business cycle analysts) behave [illegible] 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 [illegible] business cycles may be substantially different from [illegible] institutional differences, real business cycle models [illegible] particular lineal relationships; (ii) state variables and [illegible]

A second barrier to declaring the triumph of the real [illegible] model would be unwarranted. Calibration might be [illegible] 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 asymptotical measure of goodness of fit that is useful for real business cycle models, 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 explana-

Kydland and Prescott's (1991) objection to traditional econometrics is that an idealized model will not survive 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 [illegible] eliminate the need for calibration, Canova *et al.* examine the implications of a previously calibrated [illegible] *autoregression* (VAR). If the real business cycle model is an accurate description of the actual data, estimated parameters of the VAR.

The real business cycle model implies three sets of [illegible] restrictions on the VAR of two distinct types. First, [illegible] variables (time-series should be *cointegrated*. Two [illegible] are cointegrated when a particular linear [illegible] even though the series are not separately stationary.

[illegible] tions: (i) the state variables (the analogues of Z and [illegible] variables in equations (1)–(4)) must stand in [illegible] predicted values for various time-series (e.g. the [illegible] 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 produc-

Canova *et al.*'s imposition of a specific numerical calibration of the real business cycle model might limit the generality of their results. Imagine, for example, that the real business cycle model is correct in principle, but Canova *et al.* have failed to calibrate its parameters correctly. In defence of their test, their choice of parameters is not arbitrary but is drawn from a wide range of existing studies. Their results against these rules, 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 [illegible] model are simply estimates of the true values. We [illegible] depreciation rate or the variance of the shock to the [illegible] from sample data, and there is a sampling error [illegible] little to alter the behaviour of the real business cycle model, the notable exceptions are the parameters associated with the Solow residual, which have [illegible] but even if real business cycle models had no other [illegible] 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 *shown superior to his model with divisible labour* [illegible] Other models have included a monetary sector [illegible] (Cushman and Eichenbaum, 1997), 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 [illegible] with nine different principles of construction is [illegible] increasing returns to scale and shocks to animal [illegible] equilibria, and where the economy ends up depends [illegible] lations of actual data. He also claims that his model [illegible] captures the dynamics of the economy more accu- [illegible] tacly. He estimates vector autoregressions for the [illegible]

[13] Solow (1957, pp. 314, 320) explicitly observes that idle capacity biases the measure and that the measure hinges on the

[illegible]

business cycle models follow one of two strategies. Harvey and Jaeger also show that the HP filter and

[illegible]

the logarithm of the series, and so is known as *linear* in the case of U.S. prices and the monetary base

[illegible]

with detrending methods produce similar cyclical [illegible] Jaeger's analysis they demonstrate that the HP

[illegible]

$$\min_{\{\tau_t\}} \left\{ \sum_{t=1}^{T} c_t^2 + \lambda \sum_{t=2}^{T-1} \left[(\tau_{t+1} - \tau_t) - (\tau_t - \tau_{t-1}) \right]^2 \right\}$$

[illegible]

cycles and also severely decreases the correlation [illegible] among the predicted values for output, consumption

[illegible] the real business cycle model. The model itself. [illegible] use of the HP filter against critics who have argued the propagation mechanism, does little of the work [illegible] that it induces spurious cycles by stating that devia- [illegible] the HP filter [illegible] the HP filter measurement [illegible]

[illegible] [illegible] growth and cycles as [illegible] [illegible]

[illegible] document the intercorrelations among [illegible] talking about the resulting statistics as measur- [illegible] filtered time-series. These correlations are held by [illegible] spurious cycles makes no sense. The tone of [illegible] real business cycle modellers to provide strong [illegible] Kydland and Prescott's position escapes us. It is

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 [illegible] Jaeger (1993) not only show that the HP filter can [illegible] correlations they use to show various [illegible] stylized [illegible] are [illegible] [illegible] [illegible]

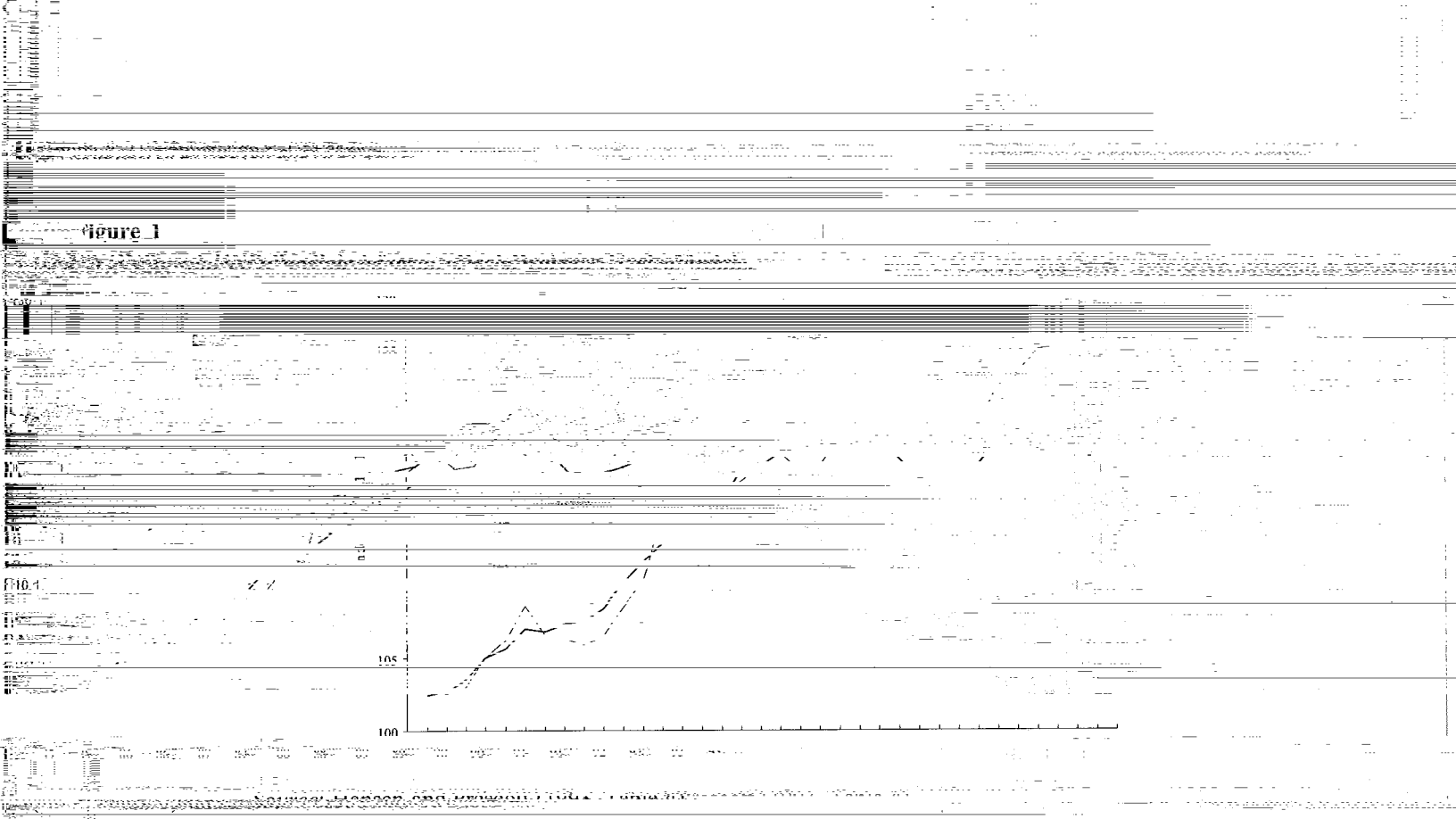

Figure 1

Despite having reflected in earlier work the ... sceptical thought in the fourth quarter of 1990, in reality GNP rose the entire time. The actual fall ... in GNP in the 1990–1 recession is only 1.6 per cent ... Prescott, 1986*a*), real business cycle models have recently made precisely such comparisons. Hansen ...

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 ... 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 ... 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 ...

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 busi... model in which we abstract from time ... given by

$$\log(Y_t) = \log(a_t) + (1-\theta)\log(K_t) + \theta\log(L_t) \qquad (0)$$

where the subscripts refer to variables determined in the model. Substituting (0) into (0) ...

$$\log(Y_S) = \log(Y_t) - (1-\theta)\log(K_t) - \theta\log(L_t)$$
$$+ (1-\theta)\log(K_t) + \theta\log(L_S)$$

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

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 propo-

II. REFUTATION?

he history of real business cycle models illustrates

to be to make the real business cycle model behave

ently observed. If measured as the Solow residual

produce business cycles [and an unemployment

sidual in every sense of the word: it is whatever it has

[illegible]

[illegible]

[illegible]

problems, combined with the fact that the highly

limits the ambitions that their advocates have for the

The degree of confidence in the answer depends

The real business cycle model has

On balance, however, there is little convincing empirical evidence that favours it over alternative

may not be of too much concern, for Kydland and Prescott (1991, p. 174) argue that

one places in a model to answer economic questions cannot 'be resolved by computing some measure of

' But anyone who believes that

reason to date to place much confidence in real business cycle models.

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