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Business Cycle Theory and Econometrics

Allan W. Gregory
Queen's University

Gregor W. Smith
Queen's University

Department of Economics
Queen's University
94 University Avenue
Kingston, Ontario, Canada
K7L 3N6

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Allan W. Gregory and Gregor W. Smith*

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Abstract

We outline in turn criticisms made by econometricians of the methods used in empirical business-cycle research and then criticisms made by business-cycle researchers of some methods used by econometricians. The aim is to clarify and in some cases correct these criticisms. Overall there is no conflict in using rigourous statistical procedures to study modern dynamic stochastic general equilibrium models. We also provide a concise bibliography of recent research on statistical methods for business-cycle models.

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* Department of Economics, Queen's University, Kingston ON Canada K7L 3N6. We thank the Social Sciences and Humanities Research Council of Canada for support and Huw Dixon, Allen Head, James Nason, and Danny Quah for helpful suggestions.

Controversy about the methods used in empirical research on business cycles is not new. But the current controversy is new, partly because advances in economic theory, computing, statistics, and data collection have changed the nature of research on business cycles. The main reason for the controversy is that 'quantitative theory' has replaced 'macroeconomic theory' to a large extent over the past 15 years. To see what we mean, recall that in the 1970s academic journals contained numerous articles in macroeconomic theory which derived properties of models but did not study data. Today, such articles are much rarer, and aggregate studies typically include a comparison of predictions from theory with properties of data. This change certainly constitutes progress, but it has led to some debates as the gap between macroeconomics and econometrics has narrowed.

What then is at issue in the controversy about empirical work? This is a question often heard from applied researchers who find that there are several sets of unwritten rules for applied research. In trying to answer this question, we outline in turn criticisms of business cycle research made by econometricians, and criticisms of econometrics made by business-cycle researchers. Our method is itself empirical. We attended numerous seminars and conferences, assiduously noting the observations and comments of our colleagues. Then we listed (prudently without attribution) the most common criticisms heard from each side.

The disadvantage of this method is that we had to classify remarks as coming from one of the two camps — the econometricians and the business-cycle theorists. However, once we have done that, the advantage is that we do not need to cite sources for the remarks or criticisms. This method is best for our own personal safety, and it also means that we do not have to define completely what we mean by 'an econometrician' or 'business-cycle theorist'. Instead, our representative econometrician is simply a (frequency-weighted) collection of criticisms of the practices in business-cycle theory. The criticisms listed are not straw men, because they have been collected in this way. Nevertheless, we shall suggest that in some cases these criticisms and claims are oversimplified in certain respects.

I. AN ECONOMETRICIAN'S CRITIQUE OF QUANTITATIVE THEORY

We use the words 'quantitative theory' or 'calibration' to refer to the empirical method used by business-cycle theorists. Their applied work often is criticised by econometricians, and in this section we list some of the most common criticisms. The econometricians' lament is simple: business-cycle theorists use the same theory as in other branches of economics (such as public finance and international trade) and yet when it comes to comparing their models to data, they apply entirely different empirical tools. They seem to do without the methods of statistical inference that pervade applied economics and have been used in other sciences for more than a century (see Stigler, 1986). We shall outline several examples of this general complaint, while we reserve for Section II a discussion of the counter-attacks often heard from business-cycle modelers.

I.i. Business-cycle Models Based on Unobservable Shocks are Not Testable

Unlike many econometric models, most real business-cycle models are based on exogenous variables or shocks which cannot be observed directly. The principal example is the technology shock which plays a leading role in many models. In contrast, econometricians — along with researchers in growth, public finance, labour, and other fields — seek explanatory variables which are observable, and so object to this practice. They say that the specification of the stochastic process for the shocks affects all predictions of the model, yet this specification cannot be tested because it applies to an unobservable variable.

This argument seems to be wrong in theory but correct in practice. In theory, the stochastic process followed by technology shocks can be parameterised using measured Solow residuals. Moreover, even without such direct measurement of the shocks, the parameters of the shock process can be estimated and tested using simulation methods (see Ingram and Lee (1991), Gregory and Smith (1990), Smith (1993), and Duffie and Singleton (1993)). The idea behind this simulation estimation is simple. Precisely because the parameters assigned to the shock process affect the predictions, they can be estimated by varying them until the predictions 'match' actual history. We shall discuss how a 'match' might be determined below. However, the general message here is that there is nothing to prevent a business-cycle theorist from parameterising the unobservable shocks of a model based on data.

However, many studies do not calibrate the shock process using either of these methods. Despite its appeal to some econometricians, estimation by simulation has not been widely adopted by business-cycle researchers. This could be so because it is computationally difficult or because there may be identification problems if many parameters are treated as free. But the infrequent use of simulation estimators probably is best accounted for by researchers' preference for the former method: relating the shock process to actual Solow residuals. Here the criticism has some practical force, because little time is spent on this aspect of calibration.

Often researchers adopt the same parameter values used in a previous study, arguing that this gives continuity to the research programme and facilitates comparisons with earlier work. A potential pitfall is that a theoretical model may have implications for how technology shocks should be measured (*e.g.* if it uses a novel production technology) which may make it inappropriate to use shocks parameterised from traditional Solow residuals

I.2. Moments are Easier to Match than Sample Paths or Dynamics, and Parameters Can be Set to Match Anything

Even if the moments of shocks are estimated (directly using measured shocks or indirectly using simulation and other data) using moments as inputs restricts studies to using moments as outputs. In itself, this is not a criticism, for the idea in much business-cycle research is to seek results which apply to more than one business cycle. Shocks which take widely different values across countries and time periods, say, nevertheless may have similar time-series properties. Hence building those properties into a model may make its implications more general than they would be if based on a particular realisation of the shock process. Still, econometricians object that models with many parameters should do more than restrict a small number of moments.

The emphasis on moments is criticised by econometricians who think that empirical work should seek to explain specific cycles and, perhaps more importantly, to make forecasts. Imagine an econometrician who has extensively studied the relationship between aggregate consumption expenditures and aggregate income for a certain country. Then imagine a business-cycle researcher who constructs a cycle model and, for those two time series, studies the variance of consumption growth, the variance of income growth, and their covariance. The cycle researcher claims that these moments in the simulation model are close to those in the data. This conclusion is unsatisfactory to the econometrician, whose own research focuses on accounting for the complete sample path of aggregate consumption. While the business-cycle modeler's investigations often are directed entirely within the sample, the econometrician is in a position to forecast out of sample, something that often is viewed as a real test of a theory.

We discuss the question of how closeness is measured in part I.4 below. Meanwhile the business-cycle researcher has two defences against the charges in I.2. The first defence involves observing that, while the ratio of the consumption variance to the income variance may not tell us much about the two series, economic theory may not even match this. A good example of a failure to match simple properties of data is the equity premium puzzle. Most intertemporal asset-pricing models fail to account for the mean difference between equity and bond returns. Admittedly, they may also fail to mimic time-series properties such as the conditional heteroskedasticity found in financial time-series data, but the mean premium seems a weaker property on which to focus first.

The second defence the business-cycle modeler has is that the model also makes predictions for moments of other time series, in addition to income and consumption. The econometrician is asked to forgive the weak information used in those two time series in exchange for seeing predictions for other time series as well. For this exchange to be convincing, the number of moments studied must exceed the number of free parameters; in other words, there must be over-identification. One sometimes sees business-cycle studies which introduce a number of free parameters and then adjust them in order to match a lesser number of moments. But most studies do not do this; they have fewer free parameters than moments, partly because some parameters are set on the basis of microeconomic evidence.

Suppose then that quantitative theorists use over-identified models, so that they cannot automatically match everything and hence there are some restrictions remaining to test against the data. There are still several aspects of the typical empirical exercise in business-cycle research that are questioned by econometricians. In first-rate applied econometrics, there is a sequence of specification, estimation, testing, and respecification. In calibration exercises, this sometimes seems to stop at the testing step. When a model does not fit some moments, the business-cycle theorist identifies this as model failure that requires modifying the theory in some way. For the most part, these modifications are restricted, in the sense that one does not seek a different modification for each empirical failure. Certainly there is an element of convention involved in the modifications which occur, but that is true in applied econometrics and time series modelling also.

The other aspect of calibration often questioned by econometricians is that business-cycle researchers sometimes take it for granted that certain moments are of interest. In extreme cases, there seems to be a hierarchy of moments, with first place given to unconditional means and variances. How do we know which moments to study? Gallant and Tauchen (1992) give some statistical basis for choice, but in practice convention plays a prominent role. This is a worrying question because one's ruling on the empirical adequacy of a model of course may depend on the moments which are studied. For example, a business-cycle model may mimic unconditional moments but not explain the dynamics of cycles well (see Cogley and Nason, 1995a).

One encouraging development is that many studies do present impulse response functions, showing the dynamic response of endogenous variables to shocks, and do compare these with empirical response functions. Even if other shocks are omitted (and hence unconditional moments cannot be matched) these response functions allow the model to be evaluated (see for example Cogley and Nason, 1995a). As Kim and Pagan (1994) observe, business-cycle models can be expressed as restricted vector autoregressions which can be compared to data using conventional statistics. A second encouraging development is that, where cycle models do succeed in matching unconditional first and second moments, researchers are studying complete sample paths (for examples, see Hansen and Prescott (1993) and Kollmann (1993)).

I.3. Parameter Values are Assigned from Other Studies, and Parameter Uncertainty is Ignored

Carrying parameter values from microeconomic studies to aggregate ones adds to over-identification, and hence defends against criticism I.2. But it also leads to other criticisms. For example, if the microeconomic model does not aggregate then this procedure will be misleading. If it does aggregate, then the parameters also could be estimated at the macroeconomic level as a test.

Business-cycle modelers frequently argue that using the same parameter values as in other studies makes the findings readily comparable. Suppose that a theorist makes an innovation to a standard business-cycle model and wishes to demonstrate that this new insight 'solves' empirical failings in earlier studies. The new model is parameterised (where possible) in an identical manner to earlier studies so that the impact of the new feature can be assessed easily. This approach may be too conservative. From the econometricians' perspective, it also ignores well-defined and useful tools (see Rothenberg, 1973) for combining sources of information in estimating parameter values.

Another frequently heard criticism is that calibration exercises ignore parameter uncertainty; they take only point estimates from panel data studies. In seminar presentations, business-cycle researchers are sometimes heard to say that to simulate data they need to 'take a stand' on parameter values. Then, just as one visualises a list of parameters being nailed to a church door, a different set of parameter values — also treated as fixed — is studied as part of sensitivity analysis. Even those econometricians who can suspend disbelief while studying several fixed parameterisations may wonder how the admissible ranges for the parameters are determined.

Parameter uncertainty may arise from pooling a number of studies, from estimating a parameter in a single data set or from the beliefs of the researcher. Fortunately, there are a number of studies which carefully map uncertainty about parameter values into uncertainty about predicted moments. Eichenbaum (1991) describes the effect of such uncertainty on estimates of the share of U.S. output variance explained by technology shocks. Canova (1994) and Dejong *et al.* (1994) have shown how to simulate business-cycle models by drawing from densities for both shocks and parameters.

I.4. Models are Not Tested

From the econometrician's perspective, one of the most perplexing aspects of many calibration exercises is the absence of formal statistical testing. Usually, researchers present a table of simulated moments beside a table of historical moments, and then comment on which disparities are large and which are not, without supplying any metric by which closeness can be judged. For example, one is often told that the first real business-cycle models 'fit surprisingly well', which also seems confusing.

However, there is nothing in calibrated business-cycle models which precludes standard statistical tests (for references see Smith (1995) or the detailed survey by Kim and Pagan (1994)). For example, the population moments of a simulation model may be compared to the historical, sample moments simply by estimating the sampling variability in the latter. Diebold *et al.* (1994) carefully construct goodness-of-fit measures in this way.

An alternative method involves estimating the sampling variability using the calibrated cycle model itself. Business-cycle modelers often take a series of draws from their model, then average the moments across draws. This wastes information because the dispersion in Monte Carlo draws can readily be used to estimate sampling variability. Cecchetti *et al.* (1990) and Gregory and Smith (1991) described ways to use this information to test closeness. Testing by resampling techniques should not be part of this controversy since even proponents of calibration methods advocate their use. Kydland and Prescott (1994) note:

> In the case of uncertainty, the computer can be used to generate any number of independent realizations of the equilibrium stochastic process, and these realizations along with statistical estimation theory are used to measure the sampling distribution of any finite set of statistics to any degree of desired accuracy (p. 2). Sometimes we may say that the model mimics well on some dimension and point out that the value of some statistic for the actual economy is not far from the center of support of the sampling distribution of the corresponding statistic for the model economy (p. 16).

Sometimes these comparisons also may be constructed to allow for the approximation error in solving models.

Business-cycle theorists can and do subject their models to some form of testing. The real objection from econometricians therefore seems to be that business-cycle theorists rarely reject their models. Many revisions to theory tend to be modest refinements rather than the wholesale changes econometricians think are warranted in light of the evidence.

Econometricians also think that applying weak and unsystematic tests tends to slow the pace of revision in the theory.

I.5. Detrending Methods are Arbitrary

Much of the recent business-cycle research takes as its data macroeconomic time series which have been filtered by removing a symmetric, two-sided moving average – the Hodrick-Prescott (HP) filter. Econometricians have frequently asked: Wouldn't a different conclusion on the match between model and data be found if the data were filtered in some other way? Econometricians are even more amazed to learn that the cycle theorists apply the same filter to the data simulated from business-cycle models. Those data are already stationary, so what is the purpose of this filtering? The actual data may have been seasonally adjusted, yet researchers do not apply X-11 to the simulated data.

Three responses are most common. First, some researchers say that they use this method for comparability with other studies. This defence based on convention of course leads to one of the other two responses. Second, then, some cycle researchers argue that the method of measuring cycles does not matter to the central findings of their studies. However, there are now many studies which show that even qualitative business-cycle properties, such as volatility ranking, depend on the detrending method. King and Rebelo (1993), Harvey and Jaeger (1993), and Cogley and Nason (1995b) find that cycle properties (such as persistence) and properties of moments depend on the filter used to define cycles.

Third, business-cycle researchers sometimes agree that their measurement of cycles is arbitrary. They do not claim that time series are generated by additive trend and cycle components and that their filtering measures the true cycle. The idea is that the theoretical models treat growth and cycles in an integrated way, and not necessarily as the sum of two components. Then the intent of filtering is to isolate certain frequencies in both data and theory, and compare fluctuations in the two. From this point of view, the cycles are indeed arbitrary, but the same arbitrary cycle is measured in historical and simulated time series. It is not clear that the HP filter really achieves this goal, given that the cycle models typically are already stationary (though see King *et al.* (1988a,b) for some models with growth and cycles).

This third defence partly de-claws one of the standard criticisms of HP filtering. In several studies, econometricians have written non-stationary time series models in which the trend component is not generated by the inverse of the HP filter. For example, the data might be generated by a linear time trend plus a stationary, persistent cycle. Then when one applies the HP filter to this model, *voila*, the measured cycle component will differ, perhaps dramatically, from the true cycle component. This demonstration seems to miss the point that we do not know the 'true' trend component and yet meanwhile would like to study business cycles.

Researchers sometimes can avoid controversies over detrending by comparing the historical and theoretical moments using filter-invariant goodness-of-fit statistics (see Cogley and Nason, 1995b). Unfortunately, the comparison is more often informal, and hence may be affected by the filter transformation. The combination of the HP filter and simple visual comparisons of historical and simulated moments makes econometricians uneasy. Moments that appear to be close to one another may not be, once the Jacobian of the filter is applied to standard errors of the sample moments. Econometricians suspect that the visual comparison of moments leads quantitative theorists to accept inadequate models (type II error).

It is not necessarily the case that informally comparing HP-filtered moments from theory and data is a weak test, though. It also is possible that allowing for sampling variability or studying other filters might reveal that apparent discrepancies between the theory and the evidence are significant. Meanwhile, a research programme focused on one conventional filter may have taken the theory off in a specific direction, as a result of a fragile discrepancy. Thus, type I error also is a danger.

An alternative to arbitrarily filtering both historical and simulated data is to use some of the moments of the simulated data to extract the cyclical component of the historical time series. This matching uses business-cycle models to measure business cycles, and is described by Gregory and Smith (1994). We choose a filter that leads to the best fit between the business-cycle model and stationary components of data. For example, suppose that the theory describes only cycles (not growth) and predicts that output has a first-order autocorrelation of 0.9. Then this moment condition can be applied to extract a stationary component from the data with this same property. Of course, the match between predicted and actual moments then cannot serve as a test of the theory, but the many other predictions of typical cycle models can do so.

This type of detrending is based directly on a 'quantitative theory' model, rather than requiring arbitrary auxiliary assumptions for measuring cycles. A further advantage, relative to the HP filter, is that multivariate restrictions (such as balanced growth) are readily incorporated. Thus output and its components can share a common trend.

II. A BUSINESS-CYCLE THEORIST'S CRITIQUE OF ECONOMETRICS

In the same format as in Section I, we now list some frequently heard observations, made by theorists, about econometrics. Econometricians may find it surprising that business-cycle theorists object to the manner in which econometricians do empirical research. Theorists complain that econometricians are not really interested in the theoretical model but seem to care only about whether the theorist's model has been subjected to the 'appropriate battery of diagnostic tests'. They think econometricians may have discouraged quantitative analysis by establishing a complicated, strict code by which applied work should be conducted. In seminars and conferences, econometricians may be poised to strike at the theorist for a failure to test some subsidiary statistical aspect of a model. While this point of view may be an exaggeration, there is sufficient merit in this concern that econometricians should consider it.

II.1. The Model is Wrong and so will be Rejected

This is a rather old argument. Since the model is an abstraction from reality, it is false, and therefore will be rejected with probability one, given sufficient data. Theorists suggest that calibration is simply a tool to isolate the largest anomalies with respect to the model (since by assumption we know there are always some). Moreover, unlike many formal statistical methods, their methods provide helpful information regarding model respecification (see Watson (1993) for example).

Business-cycle theorists should, however, keep in mind that there is a world in which the model they postulate is true; it is the one they build in their calibration exercise. Their method studies the properties of artificial data generated under a parameterised business-cycle model against actual data. In the language of the econometrician, these laboratories are used to study the properties of data under the null hypothesis and actual data are employed as (pseudo) critical values in determining correspondence. Econometricians also recognise that the world is complicated and that their models are in some absolute sense false. However, they also have no difficulty discussing linear regressions, estimators, and test statistics under the tentative assumption that a model is true and also under the assumption that it is false.

In Section I we repeated the econometrician's complaint that theorists do not test their models or respecify them on the basis of test results. But theorists also complain that econometricians sometimes reject models without providing guidance about respecification. Business-cycle theorists find this process inadequate for their purposes. Since the distribution theory for the tests is derived under the null hypothesis, standard tests often do not point to explicit alternative models. In cases where there is some obvious implicit alternative hypothesis, the theorist may find this direction uninteresting. Even in situations in which the implicit alternative hypothesis does represent a worthwhile modelling alternative, the econometrician reminds the theorist that the tests applied have power in other directions as well and that these too should be checked.

Econometricians have long appreciated that applied research is difficult and that it may be easier and less controversial to develop new econometric theory than it is to do empirical work. In econometric seminars in which someone is doing applied research in some area other than with calibration methods, there seems to be a recognition of this difficulty and criticisms are softened accordingly. However, despite the statistical foundations of calibration methods, the same understanding is not always extended to the business-cycle theorist.

II.2. Theory Provides Discipline which Econometrics Lacks

All researchers would agree that we need economic theory to interpret data. Without theory, empirical analysis would simply be a series of correlation exercises with little interpretative value. What is at issue is: What should be the relative contributions of theory and empirical determination in modelling? In some econometric studies, dynamic specification and functional form are chosen by statistical criteria, where theory is said to be silent. This method has of course been extensively criticised in the *Economic Journal*. For example, one theorist wrote of this method:

> To the best of my understanding, Professor X is not presented with his time lags, as he is with qualitative analysis, by his economist friends, but invents them for himself.

The theorist was Keynes (1939, p. 565), and X = Tinbergen.

One reason for theorists' criticism is that they take an unduly narrow definition of econometrics. Some cycle theorists see econometrics as being the classical linear regression model or VAR modelling. Given their own emphasis on general equilibrium restrictions, theorists have little interest in the single-equation or VAR specifications that are common in 'standard' econometric work. These empirical models are typically developed from the general to the specific, without a great deal of attention to the underlying economic theory. When econometricians do model interdependencies, it is often in the simultaneous linear equation models which are best exemplified by some large-scale macroeconomic models of the 1960s and 1970s. Again, such models hold little appeal for business-cycle theorists. However, a broader definition of econometrics — as the application of statistical methods to economic data — would remove much of the controversy.

II.3. Econometrics is Curve-fitting or Data-mining

This observation is really an implication of II.1. Since models are by their very nature false, the business-cycle theorist does not attempt to fit the time-series observations closely for each variable in the model. Instead the aim is to find models that account for some of the variation or covariation in the data. There is no attempt to develop the same degree of fit that single-equation econometrics enjoys by having regression equations with many unrestricted explanatory variables.

We are not convinced that business-cycle theorists have less ambitious goals in fitting data than do, say, labour economists. Instead, the fact that many business-cycle models so far do not fit well may explain the theorists' lack of interest in this kind of criterion.

In any event, econometricians do not try to maximise R^2 either; this criticism to us seems misguided.

Some business-cycle theorists also criticise any attempt to estimate parameters from macroeconomic data. Even estimates of behavioural parameters, based on Euler equations from theory, are suspect. It is difficult to rationalise this point of view. One version of it holds that parameter uncertainty and parameter variation simply are not important. Interesting perturbations are in the model's assumptions and structure, not the parameter values.

However, it is not tenable to view 'quantitative theory' as focusing on general facts of measurement, for which the niceties of econometrics are of second-order importance. For example, Eichenbaum (1991) showed that allowing for parameter uncertainty in standard ways had a dramatic effect on the estimated share of post-war U.S. output volatility accounted for by technology shocks. As business-cycle models become more refined and focus on more detailed empirical evidence, the argument that econometrics is irrelevant becomes more tenuous.

II.4. Unit Root Trends in Output do Not Leave Realistic Business Cycles

For the most part, business-cycle models deal in stationary variables. Econometricians have been instrumental in the development of methods appropriate for determining whether a variable has a unit root or not. To a disinterested third party, this would seem to be a good match — the right tool for the right problem. But just as econometricians have complained about the arbitrary detrending methods used by business-cycle theorists, so too have theorists objected to the relevance of unit root test results.

In some parametric time series models, first-differencing isolates the stationary component. More generally, if one imagines a time series as the sum of permanent and transitory (cycle) components, then finding a unit root in the series does not mean that first-differencing yields the cyclical component; rather, it yields the sum of the first differences of the two components. However, whether econometricians conclude that first-differencing or a decomposition based on unobserved components is appropriate is of little consequence to most business-cycle theorists. Transitory components from these transformations do not resemble most definitions of cycles since they typically are not very autocorrelated. Even casual reflection would indicate that downturns and upturns in the economy are persistent and that modelling cycles as the first differences of output, for example, would be inconsistent with business-cycle history.

Although filtering output using polynomials of time does produce highly persistent data, it generally is the case that these data are still non-stationary. Perhaps the current research on unit root tests that permit structural breaks in trends will lead to stationary components which can be more readily identified with persistent business cycles. But quantitative theorists perhaps will continue to be suspicious of any scheme of cycle measurement based on estimated time series models alone.

III. CONCLUSION

There is a controversy in business-cycle research, and it seems to centre on one issue: how seriously economic theory is taken. To some extent, the varying weight given to theory reflects the range of goals in applied work, from developing theory, to measurement with existing theory, to short-term forecasting. Given this assortment of goals, there is in turn a range of methods in use, with a continuum between econometricians and business-cycle theorists. Certainly there are standard statistical tools that business-cycle theorists would find useful, but some methods developed in time-series econometrics are unlikely to be adopted in business-cycle research if they do not arise in the context of an economic theory.

References

Canova, F. (1994). Statistical inference in calibrated models. *Journal of Applied Econometrics*, 9, S123-144.

Cecchetti, S. G., Lam, P.-S. and Mark, N. C. (1990). Mean reversion in equilibrium asset prices. *American Economic Review*, 80, 398-418.

Cogley, T. and Nason, J. M. (1995a). Output dynamics in real business cycle models. *American Economic Review*, 85, 492-511.

Cogley, T. and Nason, J. M. (1995b). Effects of the Hodrick-Prescott filter on trend and difference stationary time series: implications for business cycle research. *Journal of Economic Dynamics and Control*, 19, 253-78.

Dejong, D. N., Ingram, B. F. and Whiteman, C. H. (1994). A Bayesian approach to calibration. Mimeo, Department of Economics, University of Pittsburgh.

Diebold, F. X., Ohanian, L. E. and Berkowitz, J. (1994) Dynamic equilibrium economies: a framework for comparing models and data. Mimeo, Department of Economics, University of Pennsylvania.

Duffie, D. and Singleton, K. J. (1993). Simulated moments estimation of Markov models of asset prices. *Econometrica*, 61, 929-52.

Eichenbaum, M. (1991). Real business-cycle theory: wisdom or whimsy? *Journal of Economic Dynamics and Control*, 15, 607-26.

Gallant, A. R. and Tauchen, G. (1992). Which moments to match? *Econometric Theory*, forthcoming.

Gregory, A. W. and Smith, G. W. (1990). Calibration as estimation. *Econometric Reviews*, 9, 57-89.

Gregory, A. W. and Smith, G. W. (1991). Calibration as testing: inference in simulated macroeconomic models. *Journal of Business and Economic Statistics*, 9, 297-303.

Gregory, A. W. and Smith, G. W. (1993). Statistical aspects of calibration in macroeconomics. In *Handbook of Statistics*, vol. 11. (ed. G. S. Maddala, C. R. Rao and H. D. Vinod), ch. 25, 703-19. Amsterdam: North-Holland.

Gregory, A. W. and Smith, G. W. (1994). Measuring business cycles with business-cycle models. Discussion Paper No. 901, Institute for Economic Research, Queen's University.

Hansen, G. D. and Prescott, E. C. (1993). Did technology shocks cause the 1990-1991 recession? *American Economic Review(P)*, 83, 280-6.

Harvey, A. H. and Jaeger, A. (1993). Detrending, stylized facts, and business cycles. *Journal of Applied Econometrics*, 8, 231-47.

Ingram, B. F. and Lee, B.-S. (1991). Simulation estimators of time series models. *Journal of Econometrics*, 47, 197-206.

Keynes, J. M. (1939). Professor Tinbergen's method. *Economic Journal*, 49, 558-68.

Kim, K. and Pagan, A. R. (1994). The econometric analysis of calibrated macroeconomic models. Forthcoming in *Handbook of Applied Econometrics* vol. 1 (ed. M. Wickens and M. H. Pesaran). Oxford: Basil Blackwell.

King, R. G., Plosser, C. I. and Rebelo, S. T. (1988a). Production, growth, and business cycles. I. The basic neoclassical model. *Journal of Monetary Economics*, 21, 195-232.

King, R. G., Plosser, C. I. and Rebelo, S. T. (1988b). Production, growth, and business cycles. II. New directions. *Journal of Monetary Economics*, 21, 309-42.

King, R. G. and Rebelo, S. T. (1993). Low-frequency filtering and real business cycles. *Journal of Economic Dynamics and Control*, 17, 207-32.

Kollmann, R. (1993). Fiscal policy, technology shocks, and the U.S. trade balance deficit. Discussion Paper No. 9313, Département des sciences économiques, Université de Montréal.

Kydland, F. E. and Prescott, E. C. (1994). The computational experiment: an econometric tool. Staff Report 178, Research Department, Federal Reserve Bank of Minneapolis.

Pagan, A. R. (1994). Calibration and econometric research: an overview. *Journal of Applied Econometrics*, 9, S1 -S10.

Rothenberg, T. J. (1973). *Efficient Estimation with A Priori Information* New Haven: Yale University Press.

Smith, A. A., Jr. (1993). Estimating nonlinear time-series models using simulated vector autoregressions. *Journal of Applied Econometrics* 8, S63-84.

Smith, G. W. (1995). Discussion of the econometrics of the general equilibrium approach to business cycles. Forthcoming in *Macroeconometrics: Developments, Tensions, and Prospects* (ed. K. D. Hoover). Boston, MA: Kluwer.

Stigler, S. M. (1986). *The History of Statistics: The Measurement of Uncertainty before 1900*. Cambridge, MA: Harvard University Press.

Tinbergen, J. (1940). On a method of statistical business-cycle research. A reply. *Economic Journal*, 50, 141-54.

Watson, M. W. (1993). Measures of fit for calibrated models. *Journal of Political Economy*, 101, 1011-41.