Five Questions about Business Cycles*

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This article considers five broad questions about the fundamental nature of business cycles and surveys relevant recent research. It is a slightly revised version of the introductory chapter to our book, *Business Cycles: Durations, Dynamics, and Forecasting* (Diebold and Rudebusch 1999). Both the book and this article attempt to place recent empirical business cycle research, and especially our own work, in a broader perspective. In particular, we focus on research that analyzes the durations or lengths of expansions and contractions, the co-movement and dynamics of cyclical variables, and the prediction of macroeconomic fluctuations.

Introduction

This article examines five questions about business cycles. They are difficult questions, and we do not provide definitive answers. Instead, we focus on the range of relevant evidence and discussion provided in recent research. These are the five questions we consider:

1. Have business cycles moderated recently? The possible postwar stabilization of the economy has been the subject of much controversy. After reviewing the evidence, our tentative conclusion is that the economy has undergone somewhat shorter, shallower, and less frequent recessions in the postwar period.

2. Do expansions (or contractions) die of old age? We consider whether business cycle regimes are more likely to end as they get longer. Contrary to popular wisdom, we find little supporting evidence in the postwar period for the notion that expansions become more fragile as they age. However, there is ample evidence that postwar contractions are quite likely to end after just a few quarters—perhaps because they are curtailed by countercyclical policy.

3. What are the defining characteristics of the business cycle? We focus our discussion on two important issues. The first is how economic variables move together, or covary, over the cycle, which is closely related to how broadly business cycles are spread throughout the various sectors of the economy. The notion of co-movement—and particularly accelerated and delayed co-movement—leads naturally to definitions of coincident, leading, and lagging business cycle indicators. Second, we consider the timing of the slow alternation between expansions and contractions. In particular, we examine the persistence of business cycle regimes using both linear and nonlinear models.

4. How can secular growth be distinguished from cyclical fluctuations? Understanding the difference between the economy's trend and its cycle is crucial for business cycle analysis. A long debate has raged on the appropriate separation of trend and cycle; we summarize recent elements in this debate and sift the relevant evidence. In the end, a great deal of uncertainty remains; however, it appears to us that some traditional trend/cycle decompositions with quite steady trend growth are not bad approximations in practice.

5. How can business cycles be forecast? There are a variety of important issues associated with the problem of business cycle forecasting, especially regarding forecast methodology and forecast evaluation. We pay special attention to the problem of forecasting business cycle turning

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points instead of merely predicting the level of future economic activity. Overall, it is fair to say that macroeconomic forecasting has a fairly poor reputation in the popular press (see Granger 1996). Consistent with this view, we find the well-known index of leading indicators is a weak predictor of economic activity, especially when evaluated in a real-time setting. Still, even with the recognition that forecasting business cycles is a very difficult task, we find some hopeful signs for future progress.

Question 1: Have Business Cycles Moderated Recently?

It would not be too surprising if the empirical characteristics of business cycles varied secularly over time. There have been important changes in the economy in the postwar era, including, for example, changes in the composition of production and of the labor force, in the technology of inventory management, and in the importance and behavior of government. All these developments might plausibly affect the nature of economic fluctuations, and in the 1960s and 1970s, the common wisdom was that the U.S. economy had become more stable in the postwar period than before. Indeed, this conclusion fairly leapt from the data: Conventional measures of prewar real output have a variance around trend that is about 70 percent higher than in the postwar period (Baily 1978). However, the other side of the debate on volatility stabilization was taken by Romer (1986), who argued that the diminished volatility displayed by macroeconomic aggregates spuriously reflected changes in the methods used to construct those aggregates in the postwar period rather than an actual stabilization of the economy.

The course of this debate on stabilization is described in Diebold and Rudebusch (1991a, 1992). In addition, our analysis introduces a new element, namely a business cycle duration perspective. Whereas the earlier debate focused only on the relative volatility of the economy in the prewar and postwar periods, we considered the relative duration of expansions and contractions in the two periods. Specifically, duration stabilization would be reflected by longer expansions and shorter contractions in the postwar period. This new perspective shifts the focus from the relative amplitude of recessions and economic fluctuations to their relative frequency.

In providing evidence for duration stabilization, we employ a different type of data from that examined in the previous volatility stabilization literature because we look directly at durations of expansions and contractions based on the National Bureau of Economic Research (NBER) chronology of business cycle turning points. This business cycle chronology is constructed from examining the concordance of a large number of business indicators—a much greater variety of series than those included, for example, in the components of real aggregate output. Thus, besides adding a new dimension on which to evaluate postwar stabilization, we also implicitly bring new information to this debate.

To test for duration stabilization, Diebold and Rudebusch (1991a, 1992) examine whether the distributions of expansions and contractions have shifted from the prewar period to the postwar period. The null hypothesis is that there has been no shift in these distributions, that is, no postwar stabilization. The alternative hypothesis of duration stabilization states that contractions have shortened and expansions have lengthened in the postwar period. If the distributions of expansion and contraction durations were normal, then a simple *t*-test would suffice. Because these distributions are distinctly non-normal, we use a distribution-free analog of the *t*-test, known as the Wilcoxon test, which uses the ranks of the observations rather than the values of the observations themselves.[1]

Our empirical results are striking. They clearly reject the null hypothesis of no postwar duration stabilization. Expansions have been significantly longer and contractions significantly shorter since World War II.

A crucial issue, of course, given the earlier debate on volatility stabilization, is the historical comparability of prewar and postwar turning point dates in the NBER chronology. For our results to be valid, it is important to date recessionary episodes the same way in the prewar and postwar periods. Diebold and Rudebusch (1992) discuss this topic in some detail and explore many variations to the basic chronology that exclude various dubious episodes. Our results are robust across these variations.

Following our work on duration stabilization, several studies re-examined the evidence, focusing on the comparability of the prewar and postwar dates. Romer (1994) provides a comprehensive historical analysis of the prewar and postwar business cycle chronologies. Following the theme of her work on volatility stabilization, she attempts to construct a consistent chronology of turning points over the two periods.[2] She finds much less—but still some—

1. The key insight underlying the Wilcoxon test is that, despite the nature of the duration distribution, under the null hypothesis of no stabilization, the average rank of prewar and postwar durations should be the same. Because exact finite-sample critical values are available for the test, we are assured of correct test size, even in the small samples that are available. In addition, it has been shown that the test has very good power properties.

2. Much of Romer's criticism of the NBER's chronology focuses on inconsistent treatment of the trend in dating turning points. Romer's new business cycle chronology is not without critics. For example, Watson (1994) suggests that her procedure may have biases.

evidence for postwar duration stabilization.[3] Her results suggest that the average lengths of prewar and postwar contractions are roughly equal but that prewar expansions are shorter than postwar expansions. Thus, her results may weaken but do not destroy the evidence for duration stabilization.

Watson (1994) takes a somewhat different approach to the same issue. In a review of the underlying source data, Watson finds little evidence of duration stabilization in the postwar period relative to the period before the Great Depression. For example, he dates turning points in a series on "plans for new buildings" from 1869 through 1929 and in the "building permits" series from 1947 through 1990. As for the many other series he examines, Watson finds little change in average contraction and expansion length across these two series. Of course, there are questions about the dating procedures for individual series and about finding comparable variables in the prewar and postwar periods, especially at a monthly frequency. Still, it is surprising that so little duration stabilization is apparent in individual series, given the aggregate evidence. Watson also finds little evidence that duration stabilization could reflect changes in the composition of aggregate output (say, a shift toward a service economy) assuming no duration stabilization in individual sectors. Overall, Watson provides a further cautionary note with regard to the adequacy of the NBER prewar business cycle chronology.

How do we summarize the debate on both postwar volatility and duration stabilization? Although the evidence is not nearly as strong as it seems at first glance, it does appear to us that there has been some moderation in economic fluctuations in the postwar period.[4] The evidence has held up surprisingly well for volatility stabilization (although volatility has not perhaps fallen as much as previously believed). Even Romer's reconstructed measures of aggregate output, which were not uncontrovertible improvements, display a volatility stabilization of about 20 to 30 percent. Watson, too, finds considerable evidence of reduced variances in the postwar era. For duration stabilization, the evidence is probably just as strong, especially when the joint hypothesis of both longer expansions and shorter contractions is considered. That is, it appears that the *proportion* of time that the economy spends in recession has clearly diminished.[5] This evidence would be further reinforced by the addition of the most recent observations: the short (8-month-long) 1990–1991 recession and the very long subsequent expansion.

Several issues regarding macroeconomic stabilization deserve further scrutiny. An obvious but important step for further research would be a careful reconstruction of the prewar business cycle chronology, building on the analyses of Romer and Watson.[6] There are other dimensions along which macroeconomic performance should be compared in the prewar and postwar eras. For example, the average severity of recessions (as in Romer 1994) and the variability of their duration are interesting objects for study. Of course, understanding the causes of the moderation of business cycles remains a crucial issue. Diebold and Rudebusch (1991a, 1992) provide some discussion of this issue. Zarnowitz (1992a) catalogs fifteen different hypotheses on why the postwar period may have become more stable, and Watson provides some evidence on a few of these. Still, much more analysis is required.

Finally, we should note that much recent work in this area has focused on the macroeconomic stabilization that may have occurred in the United States during the past two decades.[7] In particular, McConnell and Perez Quiros (2000) note that the standard deviation of fluctuations in quarterly U.S. output growth from 1953 to 1983 is twice as large as the standard deviation from 1984 to 1999. Besides documenting this decline in U.S. output volatility, they also provide some suggestive evidence that better management of durable goods production and inventories is responsible for the decline. Taylor (1998), instead, makes the alternative case that better monetary policy was the crucial stabilizing influence.

Question 2: Do Expansions (or Contractions) Die of Old Age?

As an ongoing business cycle expansion endures, questions inevitably arise as to when it will end. The general issue of predicting business cycle turning points is examined in question 5 below, but here we deal with a distinct, though related, question: Are expansions (or contractions) more likely to end the longer they last? In the popular press, such increasing mortality rates are usually assumed. For example, in the *Wall Street Journal,* Malabre (1988) cautioned that the ongoing expansion

> *endures on a very large amount of borrowed time. Six years old this month, it already has gone on 39*

3. Formal tests of duration stabilization using the Romer chronology are conducted by Parker and Rothman (1996).

4. This is also the conclusion of Zarnowitz's (1992a) comprehensive review of the issue.

5. This is particularly true if one includes the Great Depression in the prewar period (unlike Watson, for example). It would seem that avoidance of Great Depressions is one of the manifestations of postwar macroeconomic stabilization.

6. Boldin (1994) provides a useful review of some methods for dating peaks and troughs of the business cycle.

7. For a related discussion of the dating of duration stabilization, see Cover and Pecorino (1999).

> *months longer than the average for the previous 30 business-cycle upswings... Business-cycle experience suggests, in brief, that the present expansion has become exceedingly long in the tooth... Moreover, there's a broad consensus that the current upswing, regardless of the next recession's arrival date, is at long last manifesting its considerable age in ways bound to complicate economic policy making.*

The exact rationale for such pronouncements of caution is rarely stated, but the analogy to human mortality is straightforward. As the expansion ages, the accumulation of assorted stresses and strains engenders a macroeconomic fragility; thus, the economy is susceptible to and can be jeopardized by ever smaller shocks.

The notion of the increasing fragility of an aging expansion had wide currency among business cycle theorists in the prewar period. Gottfried Haberler's (1937) classic synthesis of prewar business cycle theory devotes an entire section to this topic with the title "Why the Economic System Becomes Less and Less Capable of Withstanding Deflationary Shocks After an Expansion Has Progressed Beyond a Certain Point." Additionally, there is a section entitled "Why the Economic System Becomes More and More Responsive to Expansionary Stimuli After the Contraction Has Progressed Beyond a Certain Point." In both sections, Haberler finds the reasoning, which is based on the inelasticity of the supply of money and of the factors of production, compelling. Indeed, the fact that an economic expansion or contraction gave rise to "maladjustments in the economic system (counterforces) which tend to check and reverse" itself was usually accepted by early writers as "dogma, at least so far as the expansion is concerned."[8]

Diebold and Rudebusch (1990) and Diebold, Rudebusch, and Sichel (1993) provide empirical evidence on whether expansions and contractions become progressively more fragile with age. This evidence is obtained from the same duration perspective used above to examine postwar shifts in expansion and contraction durations. For this second question, however, we focus on the *shape* of the duration distributions, using novel techniques drawn from the literature on hazard and survival analysis. These techniques had been most prominently employed in analyses of individual mortality rates in medical trials and of failure and reliability rates in engineering problems, although also in certain microeconomic contexts, including, for example, examinations of the lengths of individual unemployment spells.[9]

The techniques of survival (or duration or reliability) analysis are extremely well-suited to our investigation. Question 2 essentially asks whether the conditional probability that an expansion will end, given that it has lasted x months, changes as the expansion endures, that is, as x gets larger. If this probability does not change, then a 30-month-old expansion has the same chance of ending, or *hazard rate,* as a 60-month-old expansion. In contrast, an expansion that exhibits a hazard rate with (positive) *duration dependence* will be more likely to end in any given month as it grows older. The crucial insight of survival analysis is that it allows inferences about unobservable hazard rates to be made on the basis of the observed distribution of the actual durations (i.e., the unconditional distribution). In the same way, for example, a demographer can calculate the mortality rates at various ages by examining the age distribution of all deaths.

Diebold and Rudebusch (1990) and Diebold, Rudebusch, and Sichel (1993) take nonparametric and parametric approaches, respectively, to investigating business cycle duration dependence. In the parametric approach, a statistical model of business cycle durations is postulated, parameters are estimated using the available duration data, and inferences are drawn about the shape of the distribution of durations and equivalently the presence or absence of duration dependence. The nonparametric approach eschews a particular parametric specification and instead provides a very general test of the shape of the distribution and of the extent of duration dependence. Each approach has certain advantages under specific circumstances.

Reassuringly, both the parametric and the nonparametric approaches give very similar answers.[10] The evidence indicates somewhat different results for the prewar and postwar samples. In the period before World War I, it appears that business expansions were more likely to end as they grew longer (positive duration dependence) but that long contractions were no more likely to end than short ones (no duration dependence). This is true for the United States as well as for France, Germany, and Great Britain.[11] For the postwar period in the United States (the only country for which consistent data are available), the results are dif-

8. In the 1939 revised edition (but not in the original), Haberler does admit to "the possibility of a more or less stationary state with unemployment and fairly stable prices as envisaged by Mr. Keynes and his followers"; that is, an enduring contraction.

9. See Kiefer (1988) for an excellent introduction to econometric duration analysis. Hazard analysis is also used in a macroeconomic setting in Rudebusch (1995) to model the durations between changes in the monetary policy instrument.

10. Largely consistent results are also obtained with an alternative nonparametric procedure in the appendix to Diebold and Rudebusch (1991d) and with an alternative parametric approach in Sichel (1991).

11. See Mudambi and Taylor (1995) for confirming prewar British results with yet another nonparametric methodology.

ferent. The evidence indicates that expansions show no effects of aging or duration dependence, whereas contractions are increasingly likely to end with age. Thus, while prewar business cycle analysts were perhaps accurate in their assessment of expansion duration dependence, postwar commentators are not justified in suggesting that a business cycle peak is more likely to occur as an expansion ages.

Accordingly, Diebold and Rudebusch (1990) provide evidence for a postwar structural change in the process governing business cycle durations; however, as noted above, the source of this change is an open question. One obvious candidate hypothesis is that the pattern of duration dependence has been altered by the greater influence of the federal government.[12] The highly significant duration dependence of postwar contractions may reflect the commitment of the government to end recessions decisively once they have started (after some recognition lag). Much work, however, remains to be done to understand the pattern and source of structural change. Hansen (1993) provides a useful step in this direction by applying formal tests of structural stability to our parametric model. He confirms our finding of structural change but does stress that the source of this change is unknown.

Much other recent work has explored the question of duration dependence with a regime-switching model of the type pioneered by Hamilton (1989), which is considered in some detail in question 4 below. Diebold, Lee, and Weinbach (1994) essentially present a variant of this model that allows for variable hazard rates. Similarly, Durland and McCurdy (1994) also modify this model to allow for duration dependence, and they calculate hazard rates that are broadly similar to the postwar estimates in Diebold, Rudebusch, and Sichel (1993), viz., significant duration dependence for contractions and very little for expansions. Kim and Nelson (1998) also confirm our results with a multivariate regime-switching model. In contrast, Lahiri and Wang (1994), who use a Markov-switching model for the leading index, find no evidence of duration dependence.

It should be noted that much of this research examines whether hazard rates depend on duration alone without taking into account variation in other factors, such as inventories, balance sheets, and asset prices, which may be important for determining hazard rates.[13] The amount of duration dependence in hazard rates after conditioning on other information is unknown, and further research on this issue remains to be done. The scanty available evidence is from studies that attempt to forecast cyclical turning points, an issue discussed in question 5 below. For example, one careful study is Stock and Watson (1993), which finds some evidence that, even after conditioning on other variables, the duration of the current regime is helpful in forecasting the next turning point. We conjecture that their results reflect the strong duration dependence of postwar contractions.

Finally, although the discussion thus far has been in terms of the duration dependence of either expansions or contractions, Diebold and Rudebusch (1990) and Diebold, Rudebusch, and Sichel (1993) also investigate the closely related question of the duration dependence of whole cycles—measured either from peak to peak or from trough to trough. The question of whole-cycle duration dependence is one way of asking whether business cycles are periodic. Business cycle periodicity often refers to a regularity in time intervals between similar phases of the business cycle.[14] The existence of such periodicity has long been debated. Mitchell (1927) was an early skeptic, arguing that the term "periodicity" should not be used "with reference to business cycles, or with reference to crises. For the time intervals between crises are far from regular" (p. 378). On the other hand, certain political business cycle theorists (e.g., Klein 1996) and others (e.g., Britton 1986) have argued that periodicity is an important consideration.

Diebold and Rudebusch (1990) provide some much-needed structure to this debate and offer some empirical evidence on the existence of business cycle periodicity. The key insight is recognizing the equivalence between positive cyclical duration dependence and the clustering of business cycle durations around a specific length. If business cycles are regularly close to four years in length, then cycles that are longer than four years are more likely to end immediately than cycles that are shorter than four years; that is, the cycle must have positive duration dependence. Thus, the nonparametric and parametric techniques described above can be applied to peak-to-peak and trough-to-trough business cycle durations. Overall, our investigations find little consistent evidence for even weak business cycle periodicity, a result that has been confirmed and amplified by Mudambi and Taylor (1991).

Question 3: What Are the Defining Characteristics of the Business Cycle?

We argued above that there is little evidence for periodicity or regularity in the timing of business cycles. Instead, there are two widely acknowledged key characteristics of the

12. Zarnowitz (1992b) provides some discussion of this issue.

13. Another caveat stems from possible methodological problems, and Hansen's (1993) caution of possible deep identification problems with this approach should be noted.

14. This refers to the *weak* periodicity of Diebold and Rudebusch (1990). It differs from the *strong* periodicity that would be evident in spectral analyses as in, e.g., Howrey (1971).

cycle. First, a large number of macroeconomic variables appear to move together; i.e., there is a co-movement of economic series over the cycle. Second, fluctuations in economic activity exhibit *persistence*; deviations from the average or trend level of activity are typically maintained for a considerable length of time. Taken together then, the business cycle alternations between expansion and recession are fairly slow and are broadly diffused throughout the economy.

The prewar literature focused on the first characteristic, co-movement among macroeconomic variables, as the defining attribute of the business cycle; the work of Burns and Mitchell (1946) remains the classic example. Diebold and Rudebusch (1996) provide a modern interpretation of the Burns and Mitchell focus, drawing heavily on the idea that some shocks are sector-specific, whereas others are common, and that the common shocks naturally produce co-movement. Formal models of this co-movement are said to display a factor structure. Dynamic factor models have strong intuitive appeal for business cycle analysis: We observe hundreds of business cycle indicators, each of which fluctuates in part because of dependence on a common macroeconomic factor, which represents aggregate macroeconomic shocks, and in part for idiosyncratic reasons.

Static factor models have a long history in multivariate statistical analysis, but dynamic factor models are a more recent construct. Dynamic factor models can be traced to Sargent and Sims (1977) and Geweke (1977) and underlie a variety of recent and ongoing developments in business cycle analysis, such as the construction of business conditions indicators, as in Stock and Watson (1989, 1997), and the analysis of macroeconomic panel datasets, including cross-country, cross-region, and cross-state business cycle data, as in Quah and Sargent (1993) and Gregory, Head, and Raynauld (1997).

Co-movement is much but certainly not all of the story of business cycles. There are also important considerations regarding the persistence of macroeconomic fluctuations over the cycle. In the response to question 2, we considered the importance of duration in regulating the transition between expansions and contractions. However, persistence requires a more complete discussion of empirical analyses of business cycle dynamics. As noted in Diebold and Rudebusch (1996), there is a clear contrast between postwar and prewar conceptions of business cycle persistence. Most of the postwar literature focuses on cyclical models composed of one or more linear stochastic difference equations. In contrast, the prewar literature, again well represented by Burns and Mitchell (1946), often focused on the separation of expansions from contractions and the associated idea of turning points. Both of these perspectives, the dynamic aspects of linear models and nonlinear regime switching, have been the focus of much recent research.

The postwar perspective on business cycle persistence emphasizes linear models of aggregate output and its components. The ideas can be traced at least to the 1920s, when Slutsky (1927) and Yule (1921) recognized that simple linear stochastic difference equations, or autoregressive processes, converted serially uncorrelated shocks into persistent outputs whose dynamics closely resembled those of many business cycle economic indicators. Frisch (1933) put the Slutsky-Yule framework to work in formulating the idea of impulse and propagation mechanisms in economic dynamics. The idea remains very much alive in modern macroeconomic analyses, whether univariate as in Nelson and Plosser (1982) or multivariate as in the vector-autoregressive tradition initiated by Sims (1980).

The prewar perspective on business cycle dynamics and persistence, in contrast, has a nonlinear flavor associated with emphasis on regime switching between successive periods of expansion and contraction. The idea is also manifest in the great interest in the popular press, for example, in identifying and predicting turning points in economic activity, because it is only within a regime-switching framework that the concept of a turning point has intrinsic meaning.[15] Hamilton (1989) provides an elegant and modern interpretation of the old idea of regime switching. In Hamilton's model, the dynamics of the economy differ in expansions and contractions, with transitions governed by a first-order Markov process. Hence the name "Markov-switching" models.

On the linear side, our own work makes three main contributions to the study and characterization of persistence in economic time series. First, we sound a warning regarding difficulties associated with attempts based on "unit root tests" to determine whether macroeconomic shocks have a permanent component. Rudebusch (1992, 1993) shows at least for some key macroeconomic series, the data are simply not informative regarding the existence of a permanent component. At the same time, the analysis of Diebold and Senhadji (1996) makes clear that uncritical repetition of the "we don't know" mantra is just as scientifically irresponsible as blind adoption of the view that "no macroeconomic series have a permanent component," or the view that "all macroeconomic series have a permanent component." Taken together, this work promotes more careful use and interpretation of tests for macroeconomic unit roots.

Second, we emphasize that the *importance* of permanent components in macroeconomic series is a question

15. In linear frameworks, by way of contrast, there are no turning points, or switch times, in probabilistic structure. One can, of course, define turning points in terms of features of sample paths, but such definitions are fundamentally ad hoc.

distinct from the *existence* of permanent components, and we propose a refined measure of persistence. The essence of the refinement is to measure the effects of shocks to macroeconomic series not in the infinite future, as in the important earlier work of Campbell and Mankiw (1987) on which we build, but rather at the horizons of economic interest, which might be from one to one hundred years into the future. Diebold and Rudebusch (1989, 1991b) implement the ideas for aggregate output and consumption, respectively.

Finally, running throughout Diebold and Rudebusch (1989, 1991b, 1991c) is the idea that long-memory models, which display fractional integration, provide a rich and flexible framework for the analysis of macroeconomic persistence. This work has been followed by an explosion of theoretical and applied research on long-memory dynamics. Baillie (1996) provides a fine survey of recent theory, as well as applications to both macroeconomics and finance, and Michelacci and Zaffaroni (2000) provide an interesting recent macroeconomic contribution.

A number of contributions to the analysis of persistence also can be made from the nonlinear regime-switching perspective. The research in Diebold and Rudebusch (1990, 1993) on business cycle duration dependence can be viewed from the vantage point of regime-switching models; it effectively amounts to asking whether the transition probabilities are constant. Our finding of strong duration dependence in postwar U.S. contractions made us wary of regime-switching models that exclude from the outset the possibility of time-varying transition probabilities; hence, Diebold, Lee, and Weinbach (1994) propose formal models of regime switching that relax the constant transition probability constraint. Contemporaneous and independent work of Filardo (1994) pursues similar goals. Time-varying transition probabilities in regime-switching models also form a dominant theme, for example, in the 1994 *Journal of Business and Economic Statistics* symposium on Markov-switching models (e.g., Durland and McCurdy, 1994) and are featured prominently in a number of powerful recent developments, such as Kim and Nelson (1998, 1999).

As noted above, the analysis of business cycles has focused thinking about co-movement and linear and nonlinear forms of persistence. The dynamic factor structure provides a useful framework for combining all of these ideas. In particular, Diebold and Rudebusch (1996) suggest interpreting business cycles, and the history of business cycle analysis, through the lens of a dynamic factor model with a Markov-switching factor, possibly with time-varying transition probabilities. The factor structure incorporates linear dynamics and co-movement in the usual way, and it also incorporates nonlinearity via the Markov-switching factor, resulting in a multivariate dynamic regime-switching model with commonality in the timing of turning points across business cycle indicators.

A number of authors have provided rigorous econometric estimation of this model with business cycle data. Kim (1994) provides the key filtering theory for a very general class of state space models and shows how to obtain approximate maximum-likelihood estimates. Kim and Yoo (1995) and Chauvet (1998) use the Kim algorithm to obtain approximate maximum-likelihood estimates of the dynamic factor/Markov-switching business cycle model; they extract estimates of the factor (the "coincident index"), using both quarterly and monthly data and a variety of detrending procedures.

Most recently, a number of authors, including Filardo and Gordon (1999) and Kim and Nelson (1998, 1999), have exploited recent advances in Markov Chain Monte Carlo to perform Bayesian analyses of the Diebold-Rudebusch model and comparisons to other models. Kim and Nelson (1998, 1999), in particular, use a multi-move Gibbs sampler to estimate the Diebold-Rudebusch model, allowing for time-varying transition probabilities. They find that both co-movement and regime switching are empirically relevant features of the cycle, and moreover, they confirm in a multivariate framework the univariate Diebold-Rudebusch finding that U.S. contractions display clear positive duration dependence, whereas expansions do not. Finally, they extract an estimate of the latent factor from the model, which is effectively a composite index of coincident indicators, and find that its turning points coincide remarkably closely with those designated by the NBER.

Question 4: How Can Secular Growth Be Distinguished from Cyclical Fluctuations?

Macroeconomic analysis and research has generally maintained an important distinction between the trend and cyclical components of macroeconomic time series. The former described the long-run growth path of the economy, whereas the latter represented the short-run fluctuations about this trend. Indeed, theories of economic growth, stressing real human and physical capital accumulation and productivity, and theories of business fluctuations, often emphasizing nominal rigidities, have been typically constructed without reference to one another. In empirical work, a similar dichotomy was assumed in that detrended data were often employed for business cycle analysis.[16]

16. The use of such detrended data is exactly analogous to the use of seasonally adjusted data, which is done on the assumption that the seasonal cycle is independent of business fluctuations (see, e.g., Miron and Beaulieu 1996).

If indeed there is little interaction between the trend growth of the economy and its short-run fluctuations, then analysts are justified in removing the trend from the data in order to bring the business cycle into better focus. However, even assuming that the trend and cycle are largely independent from one another, instability in the trend component can complicate the separation of trend from cycle. If the economy is fluctuating around a steady growth path, it is relatively easy to discern its cyclical deviations. In fact, in the 1960s and 1970s, a common practice among almost all economists was to assume that the long-run growth of the economy followed a simple linear deterministic trend. Although perhaps a useful approximation, the assumption of a constant deterministic trend seems implausible over long historical periods where there are likely structural changes in the economy as well as varying rates of factor accumulation and technical progress. Thus, it is plausible to entertain the notion of shifts or breaks in the trend or even period-by-period random or *stochastic* trends. Such variability in the trend complicates its separation from the cycle.

These twin issues—the exogeneity and the variability of the economy's trend—have been the subject of heated debate in macroeconomics over the past two decades. This debate was initiated by Nelson and Plosser (1982), who argued that macroeconomic time series, such as real output and employment, were better represented as stochastic processes that have no tendency to return to a deterministic linear trend (so-called difference stationary or DS processes) than as processes that do have such a tendency (that is, trend stationary or TS processes). In essence, Nelson and Plosser argued that the trends in many macroeconomic series were stochastic, and they supported their position with the results of a "unit root" statistical test for each time series. These test results could find no evidence for variables such as real output and employment that disagreed with a DS representation. This suggestion of a stochastic rather than a deterministic trend had a profound influence on macroeconomic theory and empirical work. Many embraced Nelson and Plosser's results as confirmation of a stochastic trends view of macroeconomic dynamics. Notably, Campbell and Mankiw (1987) argued that postwar real output followed a DS process; therefore, a shock to the level of output in any period shifted the entire future path of output. In this way, the effects of a shock were not eliminated through reversion to trend but persisted indefinitely, and much of the variation that had previously been considered business cycles would actually be permanent shifts in trend. This stochastic or variable trend view of the world began to predominate among researchers (e.g., Stock and Watson 1988).

To a large extent, however, the pendulum has swung back from the stochastic trends consensus. In our research, we argue that at the very least there is considerable uncertainty regarding the nature of the trend in many macroeconomic time series, and that, in particular, assuming a fairly stable trend growth path for real output—perhaps even a linear deterministic trend—may not be a bad approximation.

Rudebusch (1992) confronts the Nelson and Plosser data set with a different methodology for drawing inferences about unit roots. This methodology focuses on two issues. First, by using the small-sample distributions of the test statistics, the precise amount of evidence that is contained in the relatively short data samples available can be determined. Second, by investigating the distributions of the test statistic under *both* the DS model and the TS model, the evidence regarding the validity of each of these models can be considered. Both of these issues turn out to be crucial. Rudebusch (1992) shows that the Nelson and Plosser sample of data does not support the proposition that unit roots or stochastic trends are a pervasive element in real macroeconomic time series. Indeed, the Nelson and Plosser data do not appear to be able to differentiate between plausible DS and TS representations for many series.

Rudebusch (1993) and Diebold and Senhadji (1996) apply this small-sample testing methodology to other real output data series and again find little decisive evidence for the DS model.[17] For postwar quarterly real GNP, the unit root tests say little about the relative likelihood of plausible DS and TS models of the data, even though these models display distinctly different dynamics at cyclical horizons (of, say, less than five years). Thus, typical point estimates of dynamic persistence using this data set, say, in Campbell and Mankiw (1987), are misleading because they ignore the large uncertainty regarding the estimates. Alternatively, with several annual U.S. output series that stretch over a hundred years to 1875, the TS model is distinctly favored over the DS model. That is, the deterministic trend model appears to fit the past century of data fairly well.[18]

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17. Also see Cheung and Chinn (1997).

18. Although using such a long span of data, even at an annual frequency, holds the potential for illuminating the importance of a unit root (Perron 1989), some caveats should be noted. The U.S. prewar real output data are of distinctly lower quality than the postwar data, which may be cause for some concern (though Cheung and Chinn (1997) argue that it is not). In particular, Murray and Nelson (2000) argue that spurious data outliers will prejudice the evidence against DS models. In addition, studies of countries (e.g., Kormendi and Meguire 1990), some of which have better long-span data than the United States, generally favor DS specifications over TS ones.

Diebold and Rudebusch (1989, 1991b, 1991c) provide a different perspective on the trend-cycle decomposition and the persistence of macroeconomic shocks. This work introduces and explores a general statistical model of *fractional integration* (introduced as the ARFIMA model) that nests the DS and TS models as special cases. Here, too, the conclusion is that, for many real time series, there is a great deal of uncertainty about any estimate of macroeconomic persistence and about the decomposition of trend and cycle.[19]

What can we conclude then about the nature of trend and cycle decompositions? As a blanket statistical statement, painfully little. The safest recommendation is to approach each time series with an open mind and consider the sensitivity of the results to a variety of assumptions about the variability of the trend. For U.S. real output, however, it appears that a trend representation that is very smooth, even if not exactly linear, is a viable candidate.[20]

Question 5: How Can Business Cycles Be Forecast?

Our earlier discussions of the moderation of the postwar business cycle, duration dependence in expansions and contractions, business cycle co-movement and persistence, and the separation of secular from cyclical fluctuations, all have clear implications for macroeconomic forecasting. However, Diebold (1998) is a more explicit assessment of the past, present, and future of macroeconomic forecasting. The discussion—which is both descriptive and prescriptive—argues that, broadly defined, macroeconomic forecasting is alive and well. Nonstructural forecasting, which is based largely on reduced-form correlations, has always been useful and continues to improve. Structural forecasting, which aligns itself with economic theory and hence rises and falls with theory, receded following the decline of Keynesian theory in the 1970s but is poised for possible resurgence in the wake of the powerful new dynamic stochastic general equilibrium theory developed subsequently.

Still, even if the new structural models are off to a start, they have a long way to go to become valuable for macroeconomic forecasting. Workable estimation strategies, for example, are still in their infancy and need further development.[21] Moreover, the models will have to be fleshed out with richer impulse and propagation dynamics, and the possibility of parameter non-constancy will have to be taken seriously because, for a number of reasons discussed subsequently, the allegedly "deep structural parameters" of the new models may not be immune to the Lucas (1976) critique. Still, some of the most exciting new research has used a simplified version of these models to examine a wide variety of interesting issues related to monetary policy (see, e.g., Taylor 1999 or Rudebusch 2000). In particular, the successful operation of a preemptive monetary policy requires good forecasts of business conditions, and the inflation targeting strategy that was explicitly adopted by so many central banks in the 1990s depends crucially on good inflation forecasts (see Bernanke, et al. 1999).

Our own research on forecasting has focused on two key themes: first, the meaning, use, and forecasting ability of leading economic indicators, and second, the evaluation and comparison of forecasters and forecasting models, often on a real-time basis, rather than on the usual ex post basis with the full sample of final revised data.

Much of our work on business cycle forecasting proceeds from the nonlinear business cycle perspective discussed above in our answer to question 3. In the 1980s, the work of Hamilton (1989) and Neftci (1982) clearly suggested that turning points are naturally defined in nonlinear models of regime switching. Similarly, our first research on the leading indicators, Diebold and Rudebusch (1989), viewed turning points as times of regime switches. We generated business cycle turning point forecasts using nonlinear methods for assessing whether the composite leading index was in an expansion or recession regime. As the evidence mounts for a turning point in the composite index, the probabilistic assessment of an imminent turning point in the business cycle (within the next six months, say) rises. The turning point forecasts are then evaluated using variants of a scoring rule originally proposed by Brier (1950).[22] Perhaps surprisingly, we judge the performance of U.S. turning point forecasts produced using the composite leading index to be quite poor.

The poor performance of the composite leading index in turning point prediction is a sobering finding, particularly

19. These conclusions are supported in further work by Sowell (1992) and Hassler and Wolters (1994).

20. Other work in the same spirit attempts to explore some middle ground between the TS and DS models. Notably, there are models that allow for some limited flexibility in trend through trend shifts and trend breaks (Perron 1989, 1997; Balke and Fomby 1991; Bradley and Jansen 1995; Cogley 1997). These allow for discrete breaks in the trend but only rarely and not in every period as in the DS model.

21. In particular, as noted in Oliner, Rudebusch, and Sichel (1995) the popular Generalized Method of Moments (GMM) strategy for estimating deep parameters may exhibit poor finite-sample performance.

22. The use of econometric probability forecasts and associated scoring rules has increased recently. For interesting recent contributions, see Lopez (1999) and Estrella and Mishkin (1998); for a survey, see Diebold and Lopez (1996).

when one considers that the poor predictive performance obtains even though the final revised leading index used in this exercise contains a wealth of information not available in real time. One would expect the turning-point forecasting ability of the leading index actually available in real time to be no better, and potentially much worse, because the index is subject to extensive revisions. We confirm that conjecture in the real-time U.S. turning point forecasting analysis of Diebold and Rudebusch (1991d).

Given the potential insights to be gained from ex ante or real-time forecast evaluations, as opposed to the conventional ex post evaluations, in Diebold and Rudebusch (1991e) we continue our inquiry into prediction with the composite leading index in real time, this time in a standard linear framework. Our analysis is motivated by Auerbach (1982), who performs a standard Granger-Sims causality test using the final revised leading index and concludes that it has strong predictive power. Again, the extensive revisions to which the leading index is subject and our other negative findings regarding turning point prediction in real time make us similarly doubtful about the real-time predictive ability of the leading index using linear methods. Hence, we study the ability of the composite leading index to predict industrial production in a linear model, taking care to use only the leading index data that were actually available in real time. We effectively do a real-time Granger-Sims causality test by comparing the forecasting ability of two models: In the first, industrial production is regressed only on lags of itself, and in the second, industrial production is regressed on lags of the composite leading index in addition to lags of itself. As it turns out, our real-time analysis produces a dramatic reversal of Auerbach's results: In real time, including lags of the leading index in an autoregression fails to improve forecasting performance.

Taken as a whole, our work casts substantial doubt on the effectiveness of what was often called "the government's primary forecasting tool" and leads one to question what, if anything, the leading index leads. Others, for example, Koenig and Emery (1991, 1994), have subsequently confirmed our results for the U.S. business cycle. In fact, since we delivered our pessimistic assessment of the track record of the composite index of leading economic indicators, the U.S. government has gotten out of the business, having transferred the rights to the Conference Board in the mid-1990s.

In much of the contemporaneous and subsequent work on the construction and use of composite indexes of economic indicators, including that of the Conference Board as well as Stock and Watson (1989, 1993, 1997), greater care is taken to implement statistically rigorous and replicable methods of leading index construction, with reduced emphasis on the periodic reweighting or redefining of the index that can make its predictive ability appear much better ex post than in real time.

Still, others have occasionally found more encouraging results, including Lahiri and Wang (1994), who use an alternative nonlinear procedure, and Hamilton and Perez Quiros (1996), who use an alternative *linear* procedure. Also, following Stock and Watson (1989), Estrella and Mishkin (1998) have some success in forecasting U.S. turning points using financial variables—in particular, the term structure "tilt" and the spread of corporate over Treasury bonds. Reinhart and Reinhart (1996) find that similar financial variables perform well in forecasting Canadian turning points, even though the Canadian composite leading index does poorly. The good performance of financial variables in forecasting many U.S. and Canadian recessions, however, is tempered by their miserable performance in forecasting the most recent U.S. recession of 1990. As for the key issue of whether this episode of poor predictive performance is merely a bad draw, or whether a structural break occurred, only time will tell. However, Stock and Watson (2001), who provide the definitive analysis of the forecasting ability of asset prices for inflation and output are generally pessimistic about the ability of any single indicator to consistently supply good forecasts.

We also look forward to more applications to forecasting variables besides the traditional NBER reference cycle. Artis, et al. (1995), for example, use leading indicators to forecast turning points in U.K. inflation and obtain good results. Kaminsky, et al., (1997) take a leading indicator approach to forecasting currency crises with good results, and their analysis could be extended in a variety of ways. For example, they use single indicators rather than combining them either via an index or by running a regression.

A second key research theme is the careful evaluation of business cycle forecasts and forecasting models, for example the use of forecast accuracy comparisons to help separate true predictive value from the spurious effects of data mining, as illustrated in the evaluations of turning point forecasts in Diebold and Rudebusch (1991d, 1993). Another prominent example is Mark's (1995) study of foreign exchange rate fluctuations, in which he argues that economic "fundamentals" affect the determination of exchange rates by showing that the current deviation of an exchange rate from its fundamental value has predictive content for its long-term evolution.

Similarly, Oliner, Rudebusch, and Sichel (1995) examine the forecasting accuracy of older (e.g., accelerator) and newer (Euler equation) models of a key macroeconomic variable: business fixed investment. Many would regard the allegedly "deep structural" specification of the Euler equation model as likely to produce superior forecasts, but the forecasting competition indicates otherwise: The newer models fare much worse than the old. In light of the poor predictive performance of Euler equation investment models, Oliner, Rudebusch, and Sichel, (1996) assess the structural stability of the new models and find that they are no more stable than the older models, even though the new models were designed to be immune to the Lucas critique. Perhaps the structural instability of the new models is due to their reliance on the representative agent paradigm, which would be consistent with the work of Geweke (1985), Kirman (1992), and Altissimo (1997), who show that "the representative agent" can change when policy changes.

We also have developed formal statistical tests for assessing the significance of apparent accuracy differences across forecasters. Given the obvious desirability of a formal statistical procedure for forecast accuracy comparisons, one is struck by the casual manner in which such comparisons are typically carried out. The literature contains literally thousands of forecast accuracy comparisons; almost without exception, point estimates of forecast accuracy are examined, with no attempt to assess their sampling uncertainty. Our work owes a great debt to Granger and Newbold (1986), who make creative use of an orthogonalizing transformation pioneered by Morgan (1939–1940) in developing a forecast accuracy comparison test. Their test, however, depends on a number of restrictive assumptions, which we begin to relax in the context of assessing the results of the forecasting accuracy comparisons of Diebold and Rudebusch (1991e) and fully relax in Diebold and Mariano (1995). Our forecast accuracy comparison methods have been refined and extended by West (1996), West and McCracken (1998), and Harvey, Leybourne, and Newbold (1997, 1998).

Conclusion

As should be evident, although much has been done, many questions about business cycles remain unanswered. For example, a basic question is, What are the causes of business cycles? Can we formulate an explanatory model of economic fluctuations, instead of just a statistical or forecasting description of business cycles? In our judgment, there has been very little success in the literature in forging a consensus about the nature of such an explanatory model. For example, it is instructive, although dismaying, to examine the analyses of Blanchard (1993), Hall (1993), Hansen and Prescott (1993), and Walsh (1993), which try to determine the causes for one particular contractionary episode, the recession of 1990–1991. In these papers, a long list of explanations is considered and various models are estimated, but no clear causal driving force is uncovered. In the end, the leading causal candidates vary substantially across papers and include: (1) a spontaneous decline in consumer confidence (i.e., animal spirits); (2) a jump in oil prices; (3) a deliberate disinflation by the central bank; and (4) a negative technology shock, perhaps through a change to the legal and regulatory system. Indeed, Hall (1993, p. 278) gives this candid summary of his analysis: "I conclude that established models are unhelpful in understanding this recession, and probably most of its predecessors." This broad sentiment is shared by Blanchard and Fisher (1989, p. 277), who state, "there is little agreement as to the main sources of [business cycle] disturbances—monetary or real, and if real from changes in tastes or in technology, from the private sector, or from the government."

Another interesting question for future research is, What are the similarities in business cycles across different countries? The existing literature, although not extensive, suggests that there is a commonality in the cyclical behavior of real quantities across countries (e.g., Backus and Kehoe 1992). Thus, it seems that one could usefully pool cross-country evidence to ascertain business cycle characteristics, as, for example, in Diebold, Rudebusch, and Sichel (1993). A related international business cycle question is, What are the linkages across business cycles in different countries? The extent and nature of a "world" business cycle is largely unknown. There are clearly some global business cycle shocks, such as the oil price increases of the 1970s, as well as international propagation mechanisms acting through foreign trade and capital flows. Canova and Dellas (1993), Gregory, Head, and Raynauld (1997), and Artis, et al., (1997) provide useful introductions to some of these issues and make some progress toward exploring business cycle synchronization across countries.

A final question is, What is the appropriate role for countercyclical government policy? Especially since the Great Depression and the Keynesian Revolution, debate has raged about the proper role for monetary and fiscal policy in smoothing the business cycle. The Employment Act of 1946 legislated a government responsibility to promote full employment and production, and many policymakers and economists have continued to believe in the importance of active stabilization policy. For example, Yellen (1996) argues that monetary policy "is needed, and has succeeded, in smoothing the ups and downs of the business cycle." Other economists have been persuaded by old arguments about the long and variable lags of policy or new arguments about the Lucas critique and time inconsistency, and they doubt the effectiveness, and perhaps even the influence, of any active stabilization policy.

As noted above, government policy may have played a major role in achieving the postwar stabilization of the business cycle. Also, at a deeper level, however, the whole notion of countercyclical policy may need to be rethought in a world with stochastic trends. Regarding the narrower operational question of the appropriate conduct of policy, as noted above, any increasing fragility of aging business expansions, or duration dependence, complicates countercyclical policy. More generally, given the lags in the effects of policy on the economy, the issue of forecasting the business cycle is crucial to making good policy (see Rudebusch and Svensson 1999). This last fact provided the main motivation for much of the research summarized here on forecasting the business cycle and, in particular, on the real-time analyses with leading indicators. Still, many questions regarding policy and business cycles, which are arguably among the most important to be considered, remain unanswered.

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