BAYESIAN APPROACHES TO THE 'UNIT ROOT' PROBLEM: A COMMENT

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1. INTRODUCTION

Peter Phillips has produced a most provocative and useful article that will, I suspect, stimulate further research and debate both on unit roots in macroeconomic time-series and, more generally, on Bayesian techniques in time-series econometrics. Phillips' main results are that the inferences drawn from Bayesian analyses of unit roots are highly sensitive to the choice of the prior and, when the prior is chosen by a Jeffreys ignorance criterion, the evidence against the unit root hypothesis is much weaker than found in flat-prior Bayesian work. I am largely in sympathy with both conclusions and have no important criticism of Phillips' article. I also applaud his willingness to examine previous Bayesian analyses of the unit root problem that have produced considerable confusion among empirical macroeconomists. These remarks therefore raise some additional issues not emphasized by Phillips concerning the papers by, in Sims and Uhligs' (1988) terminology, the Bayesian unit rooters (Sims, 1988; Sims and Uhlig 1988; DeJong and Whiteman, 1989a,b,c; and Schotman and van Dijk, 1989).

My argument has three main points. First, as Phillips aptly points out, from the classical (frequentist) perspective the flat-prior analysis of unit roots has some unsettling features, such as severely biased estimators and interval estimates with grossly incorrect asymptotic confidence levels. These puzzling results arise not just because of choice of priors, but more fundamentally because the classical and Bayesian techniques answer different questions.

Second, as Sims (1988) and Sims and Uhlig (1988) emphasized, because of this discrepancy between the Bayesian and classical results, researchers must take a stand on whether they are classical or Bayesian statisticians. My personal view is that the classical framework is more satisfying and useful, for two reasons: I find the axiomatic development based on the likelihood principle uncompelling; and the Bayesian paradigm does not provide a useful way to communicate information to scientific audiences. Phillips and the Bayesian unit rooters provide one of the best examples of this second point: their disagreement over priors (of which there appears to be no ready resolution) leads to different posteriors, quite different results, and a general failure in information transfer. These arguments are made in the second section of this discussion.

Third, one of Phillips' key arguments for his priors is that they come closest to producing classical confidence sets and produce unbiased (in a frequentist sense) point estimates. This raises the question: why adopt the artifice of a random coefficient and the additional burden of constructing a prior, if the goal is simply to produce results close to the classical ones? Why not simply use classical techniques instead? This is, as it turns out, not particularly difficult to do using recent research by Phillips and others in this area, and I discuss this in Section 3.

Before proceeding, it is useful to review the mechanics of the flat-prior procedure leading to the 'biased' point estimates and intervals criticized by Phillips. Following Zellner (1987, examples 4 and 5), consider the standard Gaussian regression model with k nonstochastic regressors, $Y = X\beta + \varepsilon, \varepsilon_t$ i.i.d. $N(0, \sigma^2), t = 1, ..., T$. With a diffuse ('flat') conjugate prior $p(\beta, \sigma)$ proportional to $1/\sigma$, the posterior p.d.f. of β (conditional on Y, X, and σ) is normal with mean $\hat{\beta}$ (the OLS estimator of β) and variance $\sigma^2 (X'X)^{-1}$, and the marginal posterior p.d.f. for β is a Student-t. Thus, in large samples, 95 per cent Bayesian interval estimates are constructed as $\hat{\beta} \pm 1 \cdot 96 \mathrm{SE}(\hat{\beta})$, where $\mathrm{SE}(\hat{\beta})$ is the usual OLS standard error.

This is the framework applied by Sims and Uhlig (1988) and DeJong and Whiteman (1985b) to the AR(1) model, modified in some applications by restricting the parameter space (a detail inconsequential for this discussion). Thus Sims (1989) urged the members of the Brookings panel to evaluate their Dickey–Fuller (1979) statistics using critical values of ± 2 rather than those in Fuller's (1976) tables. Similarly, DeJong and Whiteman (1989b) concluded that, for almost all of the Nelson–Plosser (1982) series, $\rho = 1$ is not contained in their 95 per cent confidence regions.

The frequentist's 'bias' in this approach is self-evident: if in fact $\rho = 1$, then over repeated samples the mean of the flat-prior posterior will tend to fall well below one. With a constant and time included as regressors, in large samples Sims' (1989) 5 percent two-sided Dickey–Fuller test will reject approximately 61 percent of the time, and DeJong and Whiteman's (1989b) 95 percent interval estimates will contain the true value of ρ only 39 percent of the time.[1] The reason for this 'bias' is that the Bayesian and classical statisticians are asking different questions, the former concerning repeated draws of ρ from a prior distribution, conditional on the data, the latter concerning repeated draws of the data, conditional on ρ. Whether this 'bias' strikes the reader as disturbing—as it does this discussant—seems to me to be a good gauge of whether the reader is likely to be receptive to wider applications of Bayesian techniques in time-series econometrics.

One of Phillips' main arguments for adopting the Jeffreys 'ignorance prior' is that it overcomes these frequentists' defects of flat-prior unit-root analysis. According to Phillips, the Jeffreys prior comes closest to producing unbiased estimates of ρ and, citing Perks (1947) and Welch and Peers (1963), the resulting Bayesian interval estimates come the closest to classical confidence intervals for many problems when Jeffreys priors are used.[2] While I am sympathetic to these two arguments, they appeal to classical criteria; Bayesians need not be concerned by biased estimation or confidence sets that do not have a classical interpretation. The difference arises because the Bayesian and classical approaches address fundamentally different problems.

2. DIFFICULTIES WITH THE BAYESIAN APPROACH

Phillips makes considerable progress towards reconciling the Bayesian and classical empirical results, and his impressive effort is to be congratulated. But, although such a goal has merit,

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[1] These percentages are based on a Monte-Carlo simulation (10 000 replications) of the Dickey–Fuller t-statistic in the AR(1) case, $\rho = 1$, $T = 100$, and a constant, time, and lagged level of the series included as regressors. In the AR(k) case with $k \geq 2$, Sims (1989) and DeJong and Whiteman appear to differ. Sims' use of critical values of ± 2 implies a test with size 61 percent for all k. However, DeJong and Whiteman's (1989b) inference is on the largest autoregressive root λ, which has a prior that increases in λ for $k \geq 2$, so their 'bias' diminishes as k increases.

[2] Kim and Maddala (1991) recently examined the performance of Phillips' prior from a frequentist perspective when the true root is far from one. For $\rho = 0 \cdot 5$ they found that the posterior mean computed using Phillips' prior is substantially biased towards one, while for the flat prior it exhibits little bias. Taken at face value, these results suggest that neither the flat nor Jeffreys prior effectively solves the bias problem when both stationary and nonstationary values of ρ are considered.

it is important not to lose sight of the underlying differences in the two approaches. As the calculations in Section 1 make clear, in the 'unit root' problem the large differences between flat-prior Bayesian and classical inferences remain asymptotically. It is therefore important to confront directly the issue of why a researcher might prefer the classical or Bayesian methodology.

There is a large and thoughtful literature on the merits and drawbacks of Bayesian techniques in statistics and econometrics, and I make no pretence of making a profound new contribution on these matters.[3] Yet it seems appropriate to summarize my main qualms about Bayesian methodology as an approach to econometric data analysis. These remarks are limited to two topics: the axiomatic development based on the likelihood principle that leads to the conclusion that statisticians must adopt Bayesian techniques; and the difference between the role of statistics in scientific discourse and its role in decision theory. Although the issue at hand is inference about large autoregressive roots, the examples used in this section concern simpler problems for which the essential differences between Bayesian and classical treatments can be considered at a more elementary and intuitive level.

2.1. The Likelihood Principle

Modern proponents of Bayesian techniques often start with the likelihood principle (LP) and derive Bayesian inference as a logical consequence (e.g. Berger and Wolpert, 1988; Poirier, 1988). The LP states that if two different experiments pertaining to some parameter θ result in likelihoods that, given the data, are proportional functions of θ, then the inferences drawn from the two experiments must be the same. This sounds plausible—after all, if the scores are the same for each experiment, should not the inferences be the same as well?

What is consciously excluded from the LP is the notion that inference must depend on samples that might have been realized but were not. An old, simple example is that of two different experiments with Bernoulli trials; one experiment uses binomial sampling—say, until 10 samples were drawn—while the other uses negative binomial sampling—say, until four successes are achieved. If it turns out that both experiments produce four successes and six failures, the likelihoods will be proportional as a function of the unknown success probability π, and the LP instructs us that the 'evidence' is the same, even though classical techniques result in different inferences in the two circumstances. This example is commonly used by Bayesians (e.g. Berger and Wolpert, 1988; Poirier, 1988) as an example of how classical procedures yield counterintuitive proscriptions. Yet, in a real sense the evidence provided by these two experiments with the same outcomes *is* different; as LeCam (1988) emphasizes, the way an experiment is designed (here, the stopping rule) plays an important role in the practical evaluation of experimental evidence.[4]

An associated implication of the LP is that the sample space—the realizations that might have occurred but didn't—is an artificial structure that should be abandoned. Instead, inference should proceed conditional on the data. However, while the sample space is an intellectual device, in many circumstances it is not far-fetched. For example, one can think of polling a large population on their preference in a Presidential race; the idea of samples that

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[3] A good starting point is Rothenberg (1974). For more recent discussions, see Berger and Wolpert (1988) and the comments by Le Cam (1988); Poirier (1988) and the comments by Pagan (1988) and Rust (1988); Lindley (1990) and the comments by Lehmann (1990); and the references to earlier debates contained therein.

[4] For several years the San Francisco *Chronicle* has run a column where a reporter asks several individuals on the street a probing personal question; almost invariably the column reports one or more outlandish response. In interpreting these data on the willingness of individuals to admit embarrassing things in public, I suspect the discerning reader would want to know what stopping rule the reporter uses.

might have been drawn but were not has concrete meaning, while what seems artificial is the treatment of the population probability π as a random object.

The concept of a sample space seems more strained in the analysis of economic time-series. One can imagine drawing a time-series on the price of an individual stock and repeating this experiment. Speaking loosely, with stationarity and sufficient limitations on temporal dependence, we can extend this reasoning to repeated samples of the same series, taken over time. But in many circumstances we might be unwilling to accept stationarity; for example, it is reasonable to suspect that the relation between money and output was different during the nineteenth and twentieth centuries. In this case we must retreat to a thought experiment in which there are other realizations of the US economy during the twentieth century that might have occurred but did not. That this leaves some students perplexed might have a message: if we are truly interested in US GNP from 1909 to 1970, perhaps it is necessary to condition on the data. If so, and if one wants to do formal inference, then we must adopt a second artifice, that the parameters are random. This leads to the Bayesian approach.

It must be emphasized, however, that recognizing that the sample space is an artificial construct does not make the idea of random parameters any less so. Thus, the purist is left with conditioning on the data and, at the same time, introducing no randomness via a model. The result is a type of data analysis that has a long history in economics. The analysis typically consists of a nonstatistical (but not necessarily atheoretical) examination of the historical record, providing a blow-by-blow account of each movement in the series. Such an approach can yield valuable insights; good examples are Friedman and Schwartz's (1963) analysis of the Great Depression, and Gordon's (1980) synopsis of the evolution of the postwar US economy. But this nonprobabilistic approach—economic story-telling in the best sense—leaves little room for extrapolation (prediction) or for discerning statistically among theories. I am unwilling to abandon the pursuit of these key goals of modern time-series econometrics, and I consider the sample-space construct a useful way to proceed with econometric inference.[5]

2.2. Communication vs. Decision Theory

If the Bayesian approach to econometrics is not compelling on axiomatic grounds, then its success must hinge on its practical ability to advance scientific knowledge. This leads to the second issue raised at the beginning of this section, the distinction between scientific discourse and decision analysis.

Consider a simple example. A politician, under financial pressure and considering dropping out of a race, commissions a poll; 40 per cent support (60 per cent oppose) his candidacy. If the politician has strong prior beliefs that his support is, say, 55 per cent, his posterior mean might remain close to 50 per cent and he might stay in the race. But a newspaper reporting the results of such a poll would have other considerations. For example, over years of reporting many such polls the publisher might want to be right on average, and she might want to minimize, say, the average squared error of the published estimates. For the politician, Bayesian techniques are appropriate, but the newspaper publisher's choice of what to publish can be made on purely classical grounds. Indeed, a sensible policy for the newspaper would

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[5] These remarks on the sample space are heavily influenced by Lehmann (1990). Lehmann also notes that the standard construction of the posterior does in fact use the sample space, in the sense that the likelihood assigns probabilities to various elements of the sample space. Although they use different terminology, Slutsky (1937), Haavelmo (1944) and Koopmans (1947) provide articulate arguments for the usefulness of the data-space concept in the analysis of economic time-series.

be to publish a range of proportions, where the range has the property that 95 per cent (say) of the ranges it publishes over the years contain the true proportions; that is, it would report a 95% confidence interval. This is, of course, what major newspapers actually *do* report; Bayesian analysis is left to the editorial pages.

An additional, long-recognized difficulty of communicating using Bayesian techniques is that the results of the inference depend on the prior. The Bayesian unit root papers illustrate this general principle. I suspect that the Bayesian unit rooters will long be arguing among themselves about which prior is least informative or most 'objective' for their problem. I do not have a strong opinion on this matter, but rather view this disagreement as evidence of their inability to convey information efficiently.

As Rothenberg (1974) has eloquently argued, one way to understand why Bayesian techniques are not more widely used is to think about the nature of scientific discussions. A useful concrete model is the interaction in a lively seminar in empirical economics. The author presents the empirical evidence and a theory explaining it; the challenge for the audience is to develop other theories that are consistent with the same statistical evidence, that is, which the data fail to reject. Broadly interpreted, this activity amounts to finding models that are in a classical confidence set constructed with the observed data.

Examples of this behaviour abound, even if attention is restricted to the unit roots literature. For example, West (1988) developed a new-Keynesian model with the explicit objective of producing a root in output sufficiently close to one that Dickey-Fuller t-tests have neglible power against his alternative. Thus, loosely speaking, real business cycle models were forced to share the 'near-unit root' confidence interval with new-Keynesian models in which shocks to monetary policy provide the only source of aggregate instability. Christiano and Eichenbaum (1989) can also be interpreted as arguing that both a trend-stationary and a difference-stationary model of real GNP belong in some reasonable but unspecified confidence set. Indeed, they go so far as to compute the distribution of certain statistics under both difference-stationary and trend-stationary models (point hypotheses), and find that the statistics computed using the data fail to reject either model. Thus they started the formal construction of a confidence interval for ρ, but stopped after examining only two points.

This view of discourse and discovery among econometricians readily lends itself to the techniques of classical inference and confidence intervals. To the extent that it produces different inferences than the classical approach, it is far less clear how Bayesian analysis with its parametric priors fits with this type of interaction. These observations, along with the evident disagreement among the Bayesian unit rooters about how best to express their ignorance, suggest the relevance of classical confidence intervals for the unit root problem.

3. THE CLASSICAL ALTERNATIVE: CONFIDENCE INTERVALS

One of Sims' (1988) arguments for the flat-prior Bayesian analysis, quoted by Phillips in 2(c), is that classical asymptotic theory generates confidence regions of 'disconcerting topology'; namely, they can be disjoint. Phillips carefully details this argument, which is based on first-order asymptotic theory. Even if disjoint confidence sets are 'disconcerting'—it is not clear why they should be—first-order asymptotic theory is arguably not the best tool to use in developing these sets. Certainly, no asymptotic theory is needed at all if one maintains (as do the Bayesian unit rooters) that the sample size is fixed and the process is Gaussian. Then in principle exact sampling theory can be used to construct confidence intervals, although in

practice this is complicated in the presence of nuisance parameters describing short-run dynamics.

Rudebusch (1990) and Andrews (1990) have recently used finite-sample techniques to construct classical median-unbiased estimates of ρ. Rudebusch (1990) used Monte-Carlo techniques and considered the AR(k) model. Andrews (1990) used exact finite sample theory for the AR(1) model and constructed finite-sample central confidence intervals for ρ in addition to median-unbiased point estimates. This approach formally maintains that the series is Gaussian and, in Andrews' case, AR(1). These two assumptions can be relaxed by developing asymptotic confidence intervals based on recent work for nearly-nonstationary processes as studied by, among others, Cavanagh (1985), Chan and Wei (1987), and Phillips (1987). The construction of these asymptotic confidence intervals is summarized here; for details, see Stock (1990).

Consider the AR($k + 1$) model,

$$y_t = \mu_0 + \mu_1 t + v_t, \quad a(L)v_t = \varepsilon_t, \quad a(L) = b(L)(1 - \rho L), \quad t = 1, ..., T,$$

where $b(L)$ has order k (so that $a(L)$ has order $k + 1$), $b(1) \neq 0$, and ε_t is a martingale difference sequence with $E\varepsilon_t^2 = \sigma^2$ and $\sup_t E|\varepsilon_t|^4 < \infty$, with (μ_0, μ_1) nonzero in general. Suppose that ρ, the largest root of the autoregression polynomial $a(L)$, is nested as local to one so that $\rho = 1 + c/T$. It is shown in Stock (1990) that the Dickey–Fuller t-statistic, $\hat{\tau}^\tau$, in a regression of Δy_t on to $1, t, y_{t-1}, \Delta y_{t-1}, ..., \Delta y_{t-k}$, has a distribution that depends on c but not on any of the other nuisance parameters in (1). Thus classical confidence intervals can be constructed by inverting $\hat{\tau}_\tau$, that is, by finding the set of c which are not rejected by the observed value of $\hat{\tau}_\tau$.

This procedure was used to construct central confidence intervals for four major economic time-series for the postwar US. The results are summarized in Table I for $k = 4$. These confidence intervals are for quarterly roots; for annual roots (the sampling frequency of the Nelson–Ploser data), the roots and confidence intervals should be taken to the fourth power. For each of the series the 90 per cent confidence interval includes one. Importantly, however,

Table I. Ninety percent asymptotic confidence intervals and asymptotically median-unbiased estimates for the largest autoregressive root ρ in selected US postwar quarterly economic time series

Series	$\hat{\rho}_{\text{med}}$ (90% CI)	$\hat{c}_{\text{med}}$ (90% CI)
Real disposable personal income	1·014 (0·966, 1·027)	2·2 (−5·5, 4·3)
Real personal consumption expenditures— nondurables and services	1·015 (0·983, 1·028)	2·4 (−2·7, 4·5)
Real GNP	0·910 (0·835, 1·013)	−14·6 (−26·7, 2·1)
Stock prices (S&P 500)	0·963 (0·894, 1·020)	−6·0 (−17·2, 3·3)

Notes: 90 percent central confidence intervals appear in parentheses below the median-unbiased estimator. The first column reports values for ρ, while the second column reports values in terms of c, where $\rho = 1 + c/T$. The estimates and confidence intervals were obtained by computing the Dickey–Fuller (1979) t-statistic with regressors $1, t, y_{t-1}, \Delta y_{t-1}, ..., \Delta y_{t-4}$. The estimation period for the regression was 49:III–89:IV ($T = 162$), with earlier values taken for initial conditions. The data were taken from the CITIBASE data base. For computational details see Stock (1990, Appendix A).

the intervals for GNP and stock prices are large: on an annual basis, the confidence interval for real GNP is (0·49, 1·053). These wide intervals are consistent with the results reported in Stock (1990) for the Nelson–Plosser data set. These confidence intervals are more informative than traditional tests for a unit root: they not only report whether the observed data are consistent with $\rho = 1$, but rather provide a range of ρ which the data are unable to reject. The range is wide, indicating substantial sampling uncertainty.

The methodological message of this section is that classical confidence intervals are relatively straightforward to compute for the unit root problem, particularly if local-to-unity asymptotics are used to sidestep the nuisance parameter problem. Although this technique can (rarely) yield a disjoint confidence set, there is no particular reason to find this disconcerting.

4. CONCLUSIONS

The foregoing comments are not intended to imply that Bayesian techniques have no place in time-series econometrics. On the contrary, the Bayesian vector autoregressions (BVARs) developed by Doan, Litterman, and Sims (1984) and Litterman (1986a) have enjoyed considerable success in the reduced-form forecasting of economic time-series (see Litterman, 1986b and McNees, 1986 for a quantitative analysis). The classically trained econometrician can appreciate BVARs as a convenient functional form in which the parameters of the priors (the 'hyperparameters') can be estimated from the data. This use of a data-based prior is a long way from the likelihood principle, but in forecasting it is ultimately out-of-sample performance—not philosophy or axiomatic reasoning—that counts. And, of course, if BVARs perform well (as they often have), neither the validity of the likelihood principle nor the usefulness of Bayesian techniques to communicate in-sample information logically follows.

In the problem at hand, I fear that the posteriors reported by the Bayesian unit rooters shed no new light on the question of whether there are unit roots in economic time-series. It is hard to argue with Bayesian decision analysis: if the flat prior used by DeJong and Whiteman (1989a) actually reflects their prior beliefs, then I assume that they will use their finding that stock prices are trend-stationary to guide their personal financial investments. However, the sharp sensitivity of the posterior to priors which Phillips so expertly documents cuts to the core of the limitations of Bayesian techniques to communicate results to a scientific audience. Fortunately, classical confidence intervals provide a convenient and appropriate alternative.

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