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Progress in the Study of Nonstationary Political Time Series: A Comment

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Cointegration was introduced to our discipline by Renée Smith and Charles Ostrom Jr. and by Robert Durr more than two decades ago at political methodology meetings in Washington University–St. Louis and Florida State University. Their articles, along with comments by Neal Beck and John T. Williams, were published in a symposium like this one in the fourth volume of *Political Analysis*. Keele, Lin, and Webb (2016; hereafter KLW) and Grant and Lebo (2016; hereafter GL) show how, in the years that followed, cointegration was further evaluated by political scientists, and the related idea of error correction subsequently was applied.

Have the last twenty-plus years witnessed significant progress in modeling nonstationary political time series? In some respects, the answer is yes. The present symposium represents progress in understanding equation balance, analyzing bounded variables, and decomposing short- and long-term causal effects. In these respects KLW's and GL's articles deserve wide dissemination. But KLW and GL leave important methodological issues unresolved. They do not address some critical methodological challenges. From a historical perspective, the present symposium shows that we have made relatively little progress in modeling nonstationary political time series.

1 Some Methodological Progress

It now is clear that equation balance is not understood by political scientists.[1] As a result, as GL's Supplementary Appendix F suggests, a remarkable number of scholars apparently employed the incorrect critical values in their applications of error correction models. Who is responsible for the confusion is a matter of debate.[2] Beck's (1993) comment in the original symposium recommended the use of the Error Correction Model (ECM) for both stationary and nonstationary data; he did not go into much detail about how estimation procedures differ with each kind of variable. In the opening of their article, DeBoef and Keele (2008) clearly state that their focus is on stationary series. Unfortunately, later, in their application section, DeBoef and Keele do not perform any pretests for stationarity. And, the lessons they draw in their conclusion are confusing—especially the passage in which they urge researchers to use the ECM for "stationary and integrated series

Author's note: The author thanks Patrick Brandt for discussions about the original and current symposia and also about the strengths and weaknesses of the Bayesian approach to this subject. He also thanks Janet Box-Steffensmeier and Jim Granato for their comments. The author alone is responsible for the paper's content.

[1]According to the book that figures prominently in DeBoef and Keele's (2008), KLW's, and GL's articles, unbalanced regression is one in which "the regressand is not of the same order of integration as the regressors, or any linear combination of the regressors" (Banerjee et al. 1993, 164; see also Maddala and Kim 1998, Section 7.3.).

[2]As regards estimation procedures, GL's presentation is a bit confusing. They seem to advocate the use of the autoregressive distributed lag model rather than the multi-step Engle–Granger method (Grant and Lebo 2016). But this is not entirely clear.

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alike" and to not worry about tests for stationarity (2008, 199).[3] Interestingly, the contributors to the 1993 symposium went to some lengths to show that the series in their illustrations were of the same order of integration. But they did not explicitly discuss balance and its methodological imperatives. So, in this regard, the present symposium makes a contribution.[4]

This said, KLW's claim that unbalanced equations are "nonsensical" (16, fn. 4) and GL's recommendation to "set aside" unbalanced equations (7) are a bit overdrawn. Banerjee et al. (1993) and others discuss the estimation of unbalanced equations. They simply stress the need to use particular nonstandard distributions in these cases.[5] Sims, Stock, and Watson (1990) showed that it is possible to fit reduced form models containing unbalanced equations and to draw conventional inferences about *some* of the coefficients in them. The issue is whether the equations can be transformed in a way that allows the use of standard distributions. If not, nonstandard distributions have to be derived for inferences on coefficients on nonstationary variables, distributions which take into account nuisance parameters. Enders (2010, 320–21) provides an illustration of how the Sims, Stock, and Watson approach is applied to an unbalanced equation.[6]

A second contribution is the demonstration of how inferences are affected by the use of bounded variables. Bounded variables indeed are widely used in political science. In 1993, scholars disagreed about whether or how such variables are nonstationary (Smith 1993; Williams 1993b). GL's Case 2 casts new light on this issue. It alerts us to an important statistical problem with bounded variables—for example, the tendency of Dickey Fuller tests to overreject the null of nonstationarity for such series. GL also point out an important conceptual distinction about what is driving error correction for these kinds of variables—a movement away from a bound by a dependent variable versus a movement of equilibration between a dependent and an independent variable. The substantive sources of "bounded behavior" need to be clarified here. But GL's Case 2 is progress in my view.[7]

Finally, both of the current articles help us understand the distinction between short- and long-term causal effects. This distinction eludes many users of dynamic models in political science. The KLW article is especially useful in this regard insofar as it makes clear how, for stationary variables, the total causal effect of a shock to an independent variable is embodied in the long-run multiplier. The earlier paper by DeBoef and Keele (2008) introduced a measure of precision in this total effect, a measure of precision that is employed by GL (Grant and Lebo 2016, fn. 41).

2 Methodological Issues Still to Be Resolved

The articles by KLW and GL raise four issues. The first is whether one can use, for stationary variables, either an ECM or a dynamic regression model to calculate long-run multipliers. KLW show how this is done. Their demonstration is consistent with the analysis in Banerjee et al. (1993, Chapters 2, 6). GL challenge this claim. GL's third case supposedly shows that for stationary variables the two models do not produce the same results. But both of the examples in this section of GL's article are problematic. The first is contrived. Why, after pretesting, would one build an ECM with a white noise series as an independent variable? No substantive justification for

[3] The rest of this sentence is "analysts need not enter debates about unit roots and cointegration to discuss long-run equilibria and rates of equilibration." DeBoef and Keele's (2008) footnotes 11 and 21 are also unclear about balance and the procedures required for fitting ECMs for nonstationary series.

[4] Ostrom and Smith (1993, 158–59) showed that their inflation series was $I(1)$, unemployment series was trend stationary, and approval series was possibly but not conclusively $I(1)$. Durr demonstrated (1993a, 215–16) that all three of his series were $I(1)$. Both described the procedures used to model nonstationary series but they did not highlight the differences between these procedures and those used to build error correction models of stationary series. Ostrom and Smith's purpose was to show that the causality tests performed in vector autoregressive (VAR) models were faulty because of the failure to include error correction terms in the respective equations.

[5] For instance, Banerjee et al. produce nonstandard distributions for a collection of spurious regressions (1993, 79–80). Maddala and Kim (1998, 252) are more equivocal. They recommend against estimating unbalanced equations. But Maddala and Kim quote Banerjee et al. to say that unbalanced equations are "valid tools of inference as long as the correct critical values are used."

[6] The Sims, Stock, and Watson (1990) approach is also discussed in Banerjee et al. (1993, Chapter 6). Freeman et al. (1998) evaluate its usefulness in political science.

[7] What is missing in this part of GL's discussion is some substantive explanation for the movement away from bounds? For instance, does presidential approval invariably move away from lower(upper) bounds because of some enduring partisan division in the electorate.

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such a model is provided or imagined by GL. GL's second example ignores the results in their own Case 2. The series in this example are both measures of approval; hence the series are bounded. The authors who originally produced these series apparently never tested for bounded unit roots. But neither do GL. GL perform no new pretests on the approval variables. And they apply the results in their Table 4 which are for stationary variables, not bounded unit roots. It therefore is difficult to know what to make of GL's results for the autoregressive distributed lag (ADL) model and ECM in their Table 5. For these reasons, the results in KLW (and Banerjee et al. 1993) are not overturned.

Second, KLW and GL disagree about what pretests should be used. Neither KLW nor GL develop a meaningful *pretest design*.[8] Both KLW and GL support the practices of prewhitening and pretesting. The former filter the series into different parts. GL allude to the conceptual problem of working with filtered series (keeping straight the fact that the series of interest are certain components of the original series). But they ignore this problem when they present their results, referring to the original unfiltered level of their variables rather than to the filtered component (Grant and Lebo 2016).[9] As illustrated by Erikson, MacKuen, and Stimson (1998), there is a theoretical rationale for filtering variables, for isolating particular components of political time series. The present articles teach us little about how theory informs filtering (prewhitening) or about the need to be clear conceptually about the natures of filtered political variables.

More important, while they endorse strongly pretesting, KLW and GL disagree about how pretesting ought to proceed. Both KLW and GL stress the importance of testing for the order of integration. KLW appear to favor familiar unit root tests. Beyond this they make several calls for testing for structural change, presumably because structural change can confound unit root tests (Perron 1989; Enders 2010, 229ff). It is not clear if KLW are referring to permanent or recurring changes, however. The two types of changes imply different modeling strategies (Frühwirth-Schnatter 2006; Brandt 2009; Park 2010). GL appear to prefer tests for fractional integration rather than for unit root tests. However, GL say nothing here about how (if) structural change complicates tests for fractional integration.[10] And, GL paper over the problem of estimation uncertainty. The d parameter is estimated with uncertainty. Yet GL treat the d estimate as a knife-edge result (24, fns. 43, 44). The estimation uncertainty in it means we should perform robustness checks for draws from the distribution of d. How are such robustness checks best accomplished? Still another important issue here is the treatment of deterministic variables. Nowadays political scientists often add time polynomials to their models. But as GL indicate, the nonstandard distributions from which critical values for ECM parameters are derived depend, in part, on the inclusion of such "nuisance parameters." In general, there is no guidance in the two articles about how to treat deterministic variables in models with stationary as opposed to nonstationary data. Last, the important assumption of weak exogeneity is not addressed (see below). And the possibilities of nonlinearity (Freeman 2012) and chaotic behavior (Williams and Huckfeldt 1996) are ignored. The assumption seems to be that political series always are stationary or (difference and trend) nonstationary. This assumption needs some defense.

In fact, KLW and GL make numerous claims about the nature of all(!) political time series. For instance, GL are convinced that most political time series are fractional integrated and that unit roots are rare.[11] KLW (2016) counter that autoregressive fractionally integrated moving average (ARFIMA) models are rare. Interestingly, the earlier symposium contained similar claims. Williams (1993b) argued unit roots are rare in political science and that error correction models were "overused."

[8]The idea of a pretest design is not new. Granato (1991), on the basis of work by Hendry (1995) and others, made a strong case for such designs.

[9]Framing an argument in terms of changes in variables ($d = 1$) is conceptually less cumbersome than framing it in terms of fractional differences. Agents may think in terms of changes and behave strategically in ways that suggest equilibration in the changes in variables. What it means to say that agents think in terms of fractional differences and strive to reduced errors in the same kinds of differences is less clear.

[10]Elsewhere, in a study of strategic party government, Lebo, McGlynn, and Kroger (2007, 468, 471) are sensitive to the problem of structural change in fractional cointegration processes. But they do not draw any larger *methodological* lessons for pretesting in the present symposium.

[11]GL (2016, fn. 7) talk of political data that are "close" to one year and three-month interest rate data, but it is not clear what they mean by this term.

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Put simply, we do not yet have the evidence to back up such claims. While GL's investigation is a valuable first step in this direction, we have not yet produced catalogues of the properties of political time series. An illustration of what such a study might look like is DeVries's (1992) study of exchange rate series. His investigation is relevant for work in international political economy such as Bernhard and Leblang's analysis of the politics of financial markets (2006). Work in this branch of political science also has revealed evidence of *recurring* structural breaks in the impact of politics on such series (Hays, Freeman, and Nesseth 2003).

The claims KLW and GL make about the typical sample size of political time series also are inaccurate. More than one hundred temporal observations often are available to international political economists. Thanks to advances in event data analysis, series of length 500 or more now are available to students of international conflict (Zeitzoff 2011). For the respective political scientists, the Monte Carlo experiments of KLW, therefore, are more useful than those of GL. And the finite sample issues that plague GL's study may be less serious than they appear. The larger point is that claims about what is and is not rare about political time series are premature.[12]

Last, neither KLW nor GL are clear about how the study of nonstationary time series contributes to theory building. Since they never mention forecasting, both sets of authors presumably believe their work has significant theoretical value.[13] Are KLW's and GL's aim to chart the empirical battlefield for theory building—to illuminate stylized facts about particular political time series—facts which political theories must explain? Or is their goal to confirm that theoretically implied mechanisms of error correction exist in particular political processes? Take the idea of equilibrium. Banerjee et al. (1993) define equilibria as a relation *between* variables. A static equilibrium, for instance, is one in which all changes have ceased to occur; the long-run multiplier is defined in these terms (Banerjee et al. 1993, 48–50). Cointegration refers to a "stationary relationship" among variables, a relationship that implies two nonstationary variables are in a "stable equilibrium state [that is] stochastically bounded and, at some point, diminishing over time" (Banerjee et al. 1993, 136).[14] DeBoef and Keele (2008, 191) expressly employ the notion of static equilibrium. And they and Webb argue here (2016) that both a zero-valued adjustment parameter for an error correction model and the absence of balance implies no equilibrium exists between the respective variables. But KLW do not explain what this means for theory building. If an error correction mechanism is found (can be shown to be mathematically equivalent to a distributed lag formulation), should theorists reformulate their verbal arguments in terms of a stable equilibrium state that is stochastically bounded? Will doing so facilitate reformulation/development of better and sounder notions of political equilibration? What does the *absence* of error correction mean for the same political theory? What is an example of a theory that implies no stable relationship between nonstationary variables? GL make a curious claim about the equilibrating properties of a single variable (Grant and Lebo 2016, 12, fn. 24). It is not clear what it means to say a random variable has reached its own equilibrium, let alone what such a finding means for theory building. Presumably, the sounder inferences GL produce about error correction inform our theoretical debates. However, at no point do GL revisit a theoretical controversy and show that the controversy was resolved incorrectly because of mistaken inference about error correction.

As regards the idea of using the study of nonstationary time series in a confirmatory fashion, for some time, rational choice theorists have predicted unit root processes and error correction (in political science, see Williams and McGinnis 1988; in economics, see Nickell 1985). In fact, in the earlier symposium both Ostrom and Smith (1993) and Durr (1993a) provided theoretical

[12]KLW treat any sample less than 250 as a small sample (27), but they consider samples sizes of 500 and 1000. GL essentially focus on the series used by students of macro-American politics, assuming sample sizes of less than 100. With such small sample sizes, estimation bias could be a serious problem. See, for instance, Banerjee et al. (1993, Section 7.4, 222ff). For a brief review of the time series used in different branches of political science, see Box-Steffensmeier et al. (2015, Chapter 1).

[13]In introducing it, Enders (2010, 359) talks of causal, behavioral, and reduced form interpretations of cointegration (equilibration). But he does not explain what distinguishes behavioral and reduced form interpretations exactly. Presumably these interpretations have value in forecasting a topic not included in Chapter 6 of his book.

[14]Banerjee et al. (1993, 7) note that models of stationary relationships the departures from which have nonconstant variance could be useful but they do not explain how.

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motivations for their error correction models. Ostrom and Smith advanced a Presidential Approval Equilibrium Hypothesis based on a mechanism that connects public evaluations (rewards and punishments) of the chief executive. And Durr actually developed a formal model of (error correcting) policy sentiment. Later Erikson, MacKuen, and Stimson (1998) developed a theoretically motivated error correction model of macro-partisanship. What we lack—and what neither KLW nor GL help us develop here—is a methodology for using the results of studies of cointegration to inform these types of theorizing. How one translates the results of KLW and GL into formal theoretical models is not explained.[15] The Empirical Implications of Theoretical Models (EITM) project, for example, has not developed models that predict cointegration (error correction).[16]

3 Methodological Challenges

The current symposium highlights important methodological challenges we are yet to meet. For a single equation ECM, like that discussed by KLW and GL and applied by many political scientists, a key assumption is that the independent variables are weakly exogenous. Consider the following reduced form model ECM for two $I(1)$ variables, y_t and z_t:[17]

$$\Delta y_t = \alpha_1(y_{t-1} - \beta z_{t-1}) + e_{1t}$$

$$\Delta z_t = \alpha_2(y_{t-1} - \beta z_{t-1}) + e_{2t},$$

where $\alpha_{1,2}$ and β are parameters and $e_{1t,2t}$ are the reduced form errors. These errors are related to the structural shocks, $\epsilon_{yt}, \epsilon_{zt}$, by the equation:[18]

$$\begin{bmatrix} e_{1t} \\ e_{2t} \end{bmatrix} = \begin{bmatrix} c_{11} & c_{12} \\ c_{21} & c_{22} \end{bmatrix} \begin{bmatrix} \epsilon_{yt} \\ \epsilon_{zt} \end{bmatrix}.$$

If $\alpha_2 = 0$ and $c_{21} = 0$, $E(e_{1t}e_{2t}) = 0$ and z_t is weakly exogenous to y_t. Weak exogeneity allows us to avoid simultaneity bias in estimating the model as well as to identify its parameters. Testing for weak exogeneity, therefore, is a critical part of building ECMs.[19] Fully modified OLS is one way to test for violation of the weak exogeneity assumption. If the assumption is violated, for example there is correlation between the reduced form disturbances ($c_{21} \neq 0$), a Choleski decomposition of the reduced form errors can be performed. But this introduces a new restriction with regard to the ordering of the reduced form shocks.[20]

We have made little progress in assessing weak exogeneity. In the symposium of 1993, Ostrom and Smith (1993, 148–49, 151) repeatedly stressed the importance of this assumption. Smith (1993, 250–51) did the same in her response to Beck's Comment (1993). But neither Ostrom and Smith nor

[15]In the work cited above in fn. 10, Lebo, McGlynn, and Kroger (2007) make a serious attempt to provide some theoretical motivation for their fractionally cointegrated model of strategic party government. But again, GL do not offer here any larger *methodological* lessons from this effort.

[16]Personal communications from Janet Box-Steffensmeier and Jim Granato. The idea of error correction resonates with the notion of incomplete information. But, in political science, the equilibria derived for the respective games are static in nature; it is not clear how these equilibria imply an error correction mechanism. Evolutionary game theory may produce conceptions of equilibration that are closer to what we call error correction. But most applications of evolutionary game theory rely on calibration rather than estimation methods. [Nickell's (1985) mathematical model shows how an ECM emerges from a dynamic optimization problem facing a single agent. I have not found a game theoretic analysis in economics that produces an ECM.]

[17]This example comes from Enders (2010, 406–7). See also Banerjee et al. (1993, especially 67–68 and 204–5).

[18]The structural form of the model contains contemporaneous relationships between the levels of the variables.

[19]The assumption of weak exogeneity also plays an important role in estimating the cointegrating vector in the multi-step Engle–Granger procedure. On this point see Enders (2010, 376–77).

[20]A brief sketch of FM-OLS is provided by Enders (2010) in Appendix 6.2. FM methods also are discussed in Maddala and Kim (1993, Section 7.3) and Banerjee et al. (1993, Chapter 7). A discussion of these methods along with applications from the study of international relations and American politics can be found in Freeman et al. (1998) and in Wood and Jordan (2012). Enders explains how to use a Choleski decomposition to orthogonalize the reduced form errors (2010, 406–7).

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Durr tested for weak exogeneity. KLW (2016) acknowledge its importance. However, they do not review any tests for weak exogeneity. They do not mention such tests in their summary guidelines (Table 5).[21] Near the end of their article, GL make a passing reference to the importance of assessing the direction of causality in ECM models (28). And GL do consider the possibility of error correlation in their first supplementary appendix. But the importance of the weak exogeneity assumption is never mentioned in their review of alternative forms of ECMs (2016). More important, GL do not tell us if any published works tested for the weak exogeneity, let alone how (if) those works suffer from simultaneity bias and the identification problem. Weak exogeneity may be a reasonable assumption in micro-level and perhaps some meso-level political analyses. However, we lack theoretical justification for this assumption in many of our macro-level political analyses. We must start performing tests for weak exogeneity.

If these tests show that certain of our variables are not weakly exogenous, we should start building vector error correction models (VECMs). It is remarkable that in 22 years since the first symposium in *Political Analysis* there are virtually no applications of the Johansen method, for instance. Ostrom and Smith acknowledged the possibility of testing for the number of cointegrating relationships in their three-equation system (1993, fn. 14). But they did not conduct such tests; they assumed a single cointegrating vector. Brandt and Williams (2007, Section 2.7.3) mention Johansen's and related methods. However, they too do not apply them. With such a method we can allow for multi-cointegration and, in turn, for multiple equilibria (Banerjee et al. 1993, 5). Tests for the number of cointegrating relationships in clusters of political time series would enrich our catalogues of the properties of political time series and thereby present new empirical challenges for political theorists.[22] From a confirmatory standpoint, the idea of competing theories as alternative *clusters* of causal claims is not new (Freeman and Alt 1994; Brandt, Colaresi, and Freeman 2008; Sattler, Freeman, and Brandt 2010). But the idea that theoretical controversies are based on claims about different *sets* of cointegrating relationships is, to my knowledge, novel. If we dig deeper into our theoretical debates, we may find they are based on competing claims about the existence of (multiple) cointegrating relationships. VECMs therefore can help resolve them. For example, we may find that debates about the existence of political business cycles amount to competing claims about how certain sets of cointegrating relationships governing presidential approval equilibration are connected to demand and supply and(or) financial equilibration in the macroeconomy.[23]

In their 2008 article DeBoef and Keele make no distinction between modeling time series for a single unit and modeling time series for multiple units (time-series-cross-sections). In fact, their first illustration is a panel reanalysis of taxation in OECD countries. It is not surprising then their article also produced confusion among panel data analysts. Leading scholars of political economy and other subjects applied ECMs without first pretesting for (panel) unit roots and cointegration and, if necessary, using nonstandard distributions. As in the earlier symposium, KLW and GL do not illuminate any special challenges for modeling nonstationary panel data. GL appear not to have reviewed the respective body of applications. What does balance mean in the panel context? Does unit heterogeneity pose issues for causal inference about error correction? How so? Why?

Unfortunately, recent works in political methodology do not provide answers to these questions. Consider Beck and Katz's (BK) piece, "Modeling Dynamics in Time-Series-Cross-Section Data" (2011). BK's article is useful insofar as it reviews, for stationary data, the equivalence of panel ADL and ECM models, illustrates the use of associated impulse and unit responses, and summarizes some results about the estimation of models with fixed effects and (stationary) lagged dependent

[21]DeBoef and Keele (2008, 186, 193) only acknowledge this key assumption. Banerjee et al. (1993, Chapter 2) stress that violations of the weak exogeneity assumption create estimation issues for $I(0)$ as well as $I(1)$ variables.

[22]Once more the treatment of "nuisance variables" is important in the construction of VECMs. On this point, see Enders (2010, Section 6.7).

[23]GL (fn. 35) mention a companion study that analyzes "how multiple endogenous variables re-equilibrate to each other" (Lebo, McGlynn, and Kroger 2007). As I understand it, this investigation studies fractionally stationary (bounded) time series; it does not test for the number of fractionally cointegrated vectors but rather stipulates them. Instrumental variable methods are used by Lebo, McGlynn, and Kroger (2007) to produce 3SLS estimates. This article is a major contribution but not an application of a VECM.

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variables. But when they discuss the modeling of *nonstationary* panel data, BK further the same confusion as the DeBoef and Keele's original (2008) article. BK essentially ignore the case of panel regression composed of variables with different orders of integration.[24] There is a literature on nonstationary panel data analysis. It is composed of separate veins of research for nonstationary panels in which N and T are both large and in which N is large and T is small. An example of the latter is Binder, Hsiao, and Hashem Pesaran (2005). Binder et al. demonstrate, in the context of panel vector autoregressions, the virtues of random and fixed effects quasi-maximum likelihood estimators over various generalized method of moments estimators. In so doing they show how fixed effects quasi-maximum likelihood estimators can be used to test for panel unit roots and panel cointegration. We need to study works like Binder et al. and apply them in our research.[25]

In closing, it is important to note that, like all but one of the articles in the original symposium, the KLW and GL pieces are written from a frequentist perspective. Williams (1993b, 233–34) was the exception. He pointed out that from a Bayesian point of view the tests of the 1980s placed too great a prior probability on the possibility of a unit root, and also more probability on the possibility that the first own lag coefficient in such tests was greater rather than less than one. Brandt and Freeman (2006, 2009) explored the usefulness of the Bayesian approach to modeling multiple political time series. They used hyperparameters—specifically the λ_1 and $\mu_{5,6}$ hyperparameters in the Sims–Zha prior—to incorporate beliefs about the nonstationarity of political time series.[26] Brandt and Freeman showed how this Bayesian approach, through its use of impulse response functions, produces assessments of short- and long-term causal effects of shocks in political variables, and how it avoids pretest biases of various kinds. How the Bayesian approach can be used to generate theoretically significant stylized facts about political dynamics and to test theoretically competing clusters of causal claims was demonstrated subsequently by Brandt and his associates (Brandt, Colaresi, and Freeman 2008; Sattler, Freeman, and Brandt 2010).

Just as in the frequentist tradition, it is possible to use the Bayesian approach to estimate vector autogressive models with unbalanced systems of equations. Presumably, the respective hyperparameters incorporate the analyst's beliefs about imbalance and potentially also about bounded unit roots. However, Brandt and Freeman are not clear about exactly how this is done.

[24]BK state that "whether the [panel] series are integrated or stationary but slowly moving, they may be well modeled by the [panel] EC[M] specification (Equation 8), which, as we have seen, is just an alternative parameterization of the [panel] ADL model." They continue, "...if the series are integrated, either the EC[M] model (the series are said to be co-integrated) or the residuals will be highly correlated. Because our preferred methodology chooses specifications with almost uncorrelated residuals, it should never lead to choosing an incorrect EC[M] (or ADL)specification" (2011, 343–44). They then argue that since, as political economists, we usually use proportions, it is unlikely our series are integrated, economists have little or no theoretical justification for ECMs, and we know which variables are exogenous to others so there is no need to test for this aspect of our specifications. In a cryptic footnote (2011, fn. 6), BK say "Dickey–Fuller type distributions" can be used to test the statistical significance of (panel) ECM adjustment parameter. And, even though earlier in their article (2011, 338) they say most political economy data sets have 20–40 units and twenty annual observations, BK argue that "given the large n and T of TSCS data, in many cases it is clear that the EC[M] model is adequate or not, and if we incorrectly assume stationarity, consistent application of appropriate standard methods will indicate the problem." It is not clear whether the "Dickey–Fuller type distributions" they refer to are those used in an Engle–Granger multiple-step procedure (to test for unit root residuals and which employ nonstandard distributions) or whether the tests they have in mind employ a panel ADL setup and hence a different nonstandard distribution (which uses critical values like those produced by Ericsson and McKinnon).

[25]Two surveys of the analyses of nonstationary panel data are Baltagi and Kao (2000) and Phillips and Moon (2000).

[26]Briefly, the λ_1 hyperparameter is the standard deviation of the first own lag coefficient, a coefficient that is set to 1. A small λ_1 implies the belief the variable is a random walk. The $\mu_{5,6}$ hyperparameters scale a set of dummy observations or pre-sample information. The former is the sum of autoregressive coefficients hyperparameter; it weights the precision of the belief that the average lagged value of a variable i better predicts variable i than the averaged lagged values of a variable $i \neq j$. μ_5 thus reflects the belief that there may be as many unit roots as endogenous variables. The correlation of coefficients (initial condition) component is μ_6. It reflects the analyst's beliefs about whether the precision of the coefficients in the model is proportionate to the sample correlation of the variables. The possibility of common trends among the variables is reflected in the magnitude of μ_6. While Brandt and Freeman did not discuss it, beliefs about fractional integration can be captured through the λ_3 hyperparameter, the hyperparameter for the standard deviation of the coefficients on longer lags of own variables. For a fuller explanation of these and other hyperparameters, see Brandt and Freeman (2006, 2009).

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Nor do they show how (if) *misinformed* settings of these hyperparameters produce mistaken inferences about the posterior probability of a model, the impulse responses generated with the model, and the location, width, and skewness of the Bayesian shape error bands for these impulse responses.[27] Finally, Brandt and Freeman do not discuss Bayesian approaches to analyzing nonstationary panel data. These too are important directions for future research in political methodology.

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[27]If one knows the order of integration of all the variables and exactly which variables are cointegrated, in theory, given the nuisance parameters in the system, the appropriate multivariate nonstandard distribution could be derived and sound inferences could be made (Sims, Stock, and Watson 1990). If the orders of integration and cointegrating relationships are not known, one could pretest for these things and employ (simulate) the relevant nonstandard multivariate distribution. Alternatively, on the basis of one's beliefs about nonstationarity in the system, one could set the values of the hyperparameters such as $\lambda_1, \mu_{5,6}$ in the Sims–Zha prior to reflect those beliefs, and, in effect create departures from what would otherwise be a proper, multivariate t posterior distribution. What is at issue is whether mistaken inferences are more or less likely if one adopts this Bayesian approach.

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