

**Theory Drift in Economic Voting Models of Public Opinion: Perhaps the Economy Doesn't  
Always Matter**

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## **Abstract**

We contend that the application of economic voting theory to explain public support for political institutions represents an example of “theory shift” where scholars have presumed that a proven theory of incumbent support also has relevance in “peripheral” contexts. Yet, traditional specifications of peripheral economic voting models to explain democratic satisfaction and public support for European Union perform remarkably well despite their weak theoretical foundations. This poses an empirical puzzle. We demonstrate that the empirical puzzle is a product of poor model specification and systematic measurement error. More specifically, we contend that the systematic measurement error in national economic evaluations – economic assessments that are unrelated to the economy as a policy outcome – produce a spurious relationship between economic evaluations and support for political institutions due to the direct effect of this systematic measurement error on attitudes toward these institutions. We present a method for estimating economic voting models that accounts (controls) for measurement error. The empirical analysis is based on 1984 Eurobarometer (Study 21) survey data from France, West Germany, Italy and the United Kingdom. We demonstrate that in the case of the incumbent vote models (i.e., the conventional economic voting models), there is no change in the estimated effect of national economic evaluations on vote choice when controlling for measurement error in national economic evaluations. In the case of the “peripheral” economic voting models—democratic satisfaction and EU support—we find that controlling for measurement error in economic evaluations significantly weakens, if not eliminates, the evidence of a relationship with economic evaluations.

## Introduction

Economic voting is a firmly established and widely recognized model in political science (Lewis-Beck and Stegmaier 2006; Duch and Stevenson 2006). Reflecting this status, studies of public opinion and political behavior have applied economic voting hypotheses in a wide variety of substantive contexts. In some instances, scholars seem to assume economic voting by default without providing well-reasoned theoretical arguments for why it should have relevance in the specific context being analyzed. This application of economic voting by association rather than reason represents “theory drift” in that broad acceptance of a theory leads scholars to presume its applicability in a wide variety of “peripheral” contexts. Research on public support for political institutions provides a case in point.

In its original formulation, the economic voting model assumes that voters evaluate the current government in terms of the outcomes produced by its policies, with the economy being a policy outcome of particular interest. Essentially, economic voting is a type of policy voting because, as Kramer (1983) points out, voters are responding to the politically relevant portion of macro-economic outcomes (see also the discussions of Erikson 2004 and Hibbs 2006). This formulation is reflected in Kramer’s (1971) seminal study as well as in Stigler’s (1973) critique of it. Note that Stigler debated the logic of Kramer’s theory on the grounds that the U.S. House of Representatives exerts greater influence on the distribution of wealth than over macroeconomic policies and hence should not be held accountable for the economy. Clearly, both Kramer and Stigler presumed that voters hold governments responsible for their policies rather than simply reacting in an emotional fashion to shifts in the economy.

Some recent studies have applied the economic voting model to explain cross-national variation in democratic satisfaction (e.g., Karp, Banducci and Bowler 2003; Anderson and Guillory 1997; Weatherford 1984, 1987) and public support for the European Union (e.g., Hooghe and Marks 2005; Eichenberg and Dalton 1993; Gabel and Whitten 1996; Anderson and Kaltenthaler 1996). We believe that these studies represent examples of “theory drift” since they adopt “peripheral” economic voting models. When modeling democratic satisfaction, we seriously question the logic of expecting *short-run* policy outcomes to influence support for political institutions, especially in mature democracies. In the case of European Union (EU) support, we question why voters should be expected to hold the EU responsible for domestic

economic outcomes (especially during the 1970s and 1980s) when its policies affect those outcomes only indirectly and much more modestly than domestic politics and the world economy.

The central puzzle though is not so much the rationale of assertions associated with “theory drift” but the existence of empirical evidence that appears to confirm these assertions. In the context of “peripheral” economic voting models, the aforementioned studies have found that economic evaluations have significant effects on democratic satisfaction and EU support. Why would such evidence emerge if the underlying theory is suspect? The empirical validation is particularly surprising given that these studies have relied on evaluations of the general economy that represent rather blunt measures of policy-related economic performance. This essay attempts to address this empirical puzzle.

Building upon Palmer’s (1999) analysis of pocketbook economic voting, we posit that evidence supporting peripheral economic voting models might be an artifact of poor model specification and measurement error associated with assessments of the national economy. While theory has posited the existence of a pocketbook relationship at the individual level, empirical evidence has largely failed to confirm it. Palmer (1999) finds though that proper modeling of the systematic measurement error in evaluations of personal financial situation strengthens the evidence of pocketbook economic voting in U.S. presidential elections. More generally, Palmer’s analysis demonstrates that the failure to properly model measurement error can confound empirical tests of a well-reasoned, convincing theoretical argument.

In the present context, we adapt the logic of Palmer’s (1999) approach in order to make the opposite argument: the failure to properly model measurement error strengthens the evidence that the economy influences democratic satisfaction and EU support. We contend that evaluations of the national economy include measurement error as well as policy-related concerns. Furthermore, we believe that the measurement error is systematic rather than random (see Duch, Palmer and Anderson 2000) and stems from distinct sources that are independently correlated with support for political institutions (e.g., government partisanship). Hence, the nature of the economic voting relationship is distorted in models that do not account for measurement error in economic evaluations. More specifically, evidence of “peripheral” economic voting relationships is potentially spurious due to the failure to control for the distinct

sources of systematic measurement error. At the very least, models that “decompose” economic evaluations into their policy-related and measurement error components will produce more precise estimates of the relationship between the economy and public support for political institutions.

The remainder of this essay is organized into four sections. We begin with a discussion of “theory drift” and how it applies to “peripheral” economic voting models of democratic satisfaction and EU support. The next section discusses the nature of heterogeneity in national economic evaluations, giving particular attention to the distinct sources of systematic measurement error. Then we present a method for estimating economic voting models that accounts (controls) for measurement error. The third section applies this method to estimate economic voting models of incumbent vote, democratic satisfaction, and EU support. Separate models are estimated for France, West Germany, Italy and the United Kingdom using the 1984 Eurobarometer (Study 21) data. The incumbent vote analysis demonstrates that the evidence of economic voting improves when the model controls for systematic measurement error and thereby generates an estimate of the *policy* relationship that is “cleaner” from both a theoretical and empirical perspective. We also show though that the opposite holds for democratic satisfaction and EU support. This finding suggests that these “peripheral” economic voting relationships are largely spurious and attributable to measurement error rather than policy concerns (as theorized). Finally, we conclude with a section discussing the broader implications of our theoretical argument and empirical findings for economic voting research.

### **“Theory Drift” and “Peripheral” Economic Voting Models**

What we label “theory drift” represents the application of theory to a substantive context that is only loosely related (or peripheral) to that for which it was developed. We include models in this classification that employ national economic evaluations to explain phenomena that are only tangentially linked to government economic policy. Moreover, we argue that this theoretical weakness characterizes efforts to explain support for political institutions, such as support for democratic institutions and for the EU.

The strength of a theoretical argument rests on its ability to clearly and logically connect the outcome behavior to the motivations of the political actors. For this reason, it is necessary to

be quite clear as to what the economic voting theory states. Recall that in the sanction model, economic voters are confronted with a moral hazard problem when deciding on voting for the incumbent versus opposition parties (Barro 1973; Ferejohn 1986; Hibbs 2006). If voters do not sanction economic performance, they risk signaling to incumbents that poor economic performance would be tolerated and hence invite rent seeking on the part of self-interested political candidates. In this model, voters are not engaging in the comparative assessment of utility income streams from competing political candidates or outcomes— rather they simply establish a threshold performance level and re-elect incumbents that satisfy this requirement and punish those that do not (Ferejohn 1986). This leads to the *sanctioning* feature that characterizes most accounts of the economic vote. It is concern about future re-election prospects that motivates incumbents to avoid shirking their responsibilities. They anticipate that voters will sanction them if they under-perform. And in order to maintain the credibility of this threat, voters punish incumbents at the polls when retrospective economic performance is substandard. And as we noted above, voters are expected to respond to only the politically relevant portion of macro-economic outcomes.

Rather than preferences over political candidates, the dependent variable in theory drift models is preferences over political institutions. Political institutions, as part of a larger political system, constitute the process by which government policies are formulated. The strength of public support for a country's political institutions reflects the extent of legitimacy for its political system. Is it reasonable to think of the electorate as sanctioning political institutions by withholding this legitimacy in response to economic outcomes?

A plausible foundation for these arguments – although one that clearly does not conform to the moral hazard features of the sanctioning model of economic voting – is the notion that popular legitimacy is tied to the outputs of political institutions. The classic statement of this relationship is Easton's notion that citizen satisfaction with government outputs can build diffuse support for a political system (Easton 1965). Unsatisfied demands or poor outputs could compromise public support for the institutions. The "theory drift" literature addressed here assumes that popular support for political institutions – or preferences over different types of institutional arrangements – will respond to (short-run) fluctuations in macro-economic outcomes.

There is evidence that economic performance can play an important role in determining the success or failure of nascent political institutions in the case of nations at the formative periods of the institution-building process (Lipset 1959; Londregan and Poole 1990, 1996; Haggard and Kaufman 1995). The inference frequently made in the literature is that these successes or failures result because of the impact that economic performance has on public support for political institutions (Pye 1971). But evidence regarding the link between the economy and support for political institutions is even ambiguous in these transition contexts. Duch (1993, 1995) has presented evidence suggesting that even at the formative stages of democratic consolidation (specifically in post-Soviet countries for his empirical analyses) public support for democratic institutions is at best weakly related to economic outcomes. In fact, Duch (1995) finds that for the most part sanctioning resulting from perceived economic performance tends to be confined to incumbent governments.

While political legitimacy may be responsive to policy outcomes in contexts with fragile or nascent political institutions, mature democracies generally benefit from a reservoir of support for their political institutions, which causes voters to blame particular governments rather than the political system for poor policy outcomes.<sup>1</sup> And while there clearly is considerable debate as to how best to measure popular feelings of legitimacy towards political institutions, there is a substantial body of empirical work suggesting that in fact the public in mature democracies have quite stable attitudes supporting democratic institutions (Sniderman et al 1975; Sniderman 1981; Weatherford 1984).<sup>2</sup> In other words, voters respond to negative outcomes by expressing their dissatisfaction with those actors that have gained predominance in the policy-making process rather than with the process itself.

The theoretical confusion here is that the “theory drift” literature has employed the sanctioning model of economic voting to explain preferences in a fashion that simply does not meet the central premise of the model. To illustrate, consider a typical economic model of support for democratic institutions specified as follows:

$$(1.1) \quad Y_i = \beta_0 + \beta_1 X_i + \phi_1 Z_i$$

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<sup>1</sup> An example of this is what Persson and Tabellini (2005) label as a reservoir of “democratic capital”.

<sup>2</sup> Much of this controversy has concerned the measurement of political trust (Citrin 1974; Abramson and Finifter 1981).

In this notation,  $Y$  indicates support for democratic institutions by individual  $i$ .  $X_i$  are national economic evaluations (NEE) measured at the individual level and  $Z_i$  are other characteristics of individuals that shape attitudes toward democratic institutions. The national economic evaluations in these models are typically the retrospective socio-tropic variety – respondents’ assessment of changes in national economic conditions over the past 12 months. Hence, the logic of these arguments is that changes in assessments of short term policy outcomes ( $X_i$ ) will have significant effects on attitudes toward democratic institutions – voters “sanctioning” these institutions.

This strikes us as implausible. First, the relationship between policy and legitimacy in mature democracies only emerges after persistent trends in poor outcomes demonstrate a failure in the policy-making process (Weatherford 1987). We should expect cross-national variation in political legitimacy to emerge only as a result of *persistent* national differences in government performance (e.g., Almond and Verba 1963; Powell 1982; Almond and Powell 1988). Hence, we take issue with models that purport to establish an ongoing relationship between short-term fluctuations in economic performance or economic evaluations (which are typically measured over a 12 month period) and attitudes toward mature political institutions. We cannot conceive of any convincing theoretical reason to expect a persistent policy relationship to exist well beyond the early period of institution building. While policy concerns might gain relevance periodically, perhaps during periods of political crisis, the notion of an ongoing causal relationship seems fundamentally inconsistent with the stabilizing role of political institutions.

A second and related concern is that this makes little sense given the moral hazard or sanctioning theoretical foundation of economic voting models. These models, even in their “sociotropic” versions, essentially posit that self-interested citizens respond to the policy initiatives (and outcomes) of political decision-makers by punishing or rewarding those responsible. But institutions do not make policy decisions. These actions are the purview of elected politicians, government administrators, and other political actors with visible roles in the policy-making process. Moreover institutions are not rent seeking actors that create moral hazard incentives for citizens.

A more plausible interpretation of these peripheral economic voting models is to interpret



them as a simple distortion of the underlying rational policy relationship that is based on a broad emotional response by voters to short-run shifts in the economy. Voters do not try to induce good behavior when they respond to the economy; rather they simply get mad, disappointed, or annoyed when the economy is performing poorly and this gets reflected in a broad range of political attitudes and behavior (Lodge, McGraw and Stroh 1989; Clarke, Stewart, and Whiteley 1998; Zaller 1992; Marcus, Neuman, and MacKuen 2000). This emotional response implies that voters either hold everyone and everything accountable or simply shift their political attitudes to reflect their economic mood. Either explanation is clearly inconsistent with the rational policy-oriented core of the economic voting argument.

We now explore two examples of peripheral economic voting models of political institutions: democratic satisfaction and support for the European Union.

**Democratic Satisfaction.** It is widely accepted that the proper functioning of democratic institutions is somehow linked to public attitudes toward these institutions. A citizenry that is discontented with how democratic institutions perform or function will undermine the functioning of these institutions or possibly represent a threat to their longevity. Accordingly a considerable literature has developed over the past two decades devoted to explaining why levels of democratic satisfaction vary. And there has been a proliferation of studies purporting to demonstrate that attitudes toward democratic institutions or satisfaction with democratic institutions are linked to economic performance.<sup>3</sup> These “economic models” of support for democracy have been particularly popular given the simultaneous transition of post-communist nations to democracy and market economies. The significant economic dislocation that occurred at the same time these countries adopted nascent democratic institutions was generally considered a serious threat to support for democracy (see Mishler and Rose 1996, although see Duch 1995). But the hypothesized relationship has not been confined to transition democracies. For example, Anderson and Guillory (1997) argue that in the wake of the recent fall of the Iron Curtain and poor economic performance in Western Europe, European support for democracy has declined somewhat. Clarke, Dutt, and Kornberg (1993) and Karp, Banducci

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<sup>3</sup> Again, its important to recognize that the economic evaluation variables in these peripheral models are short-term economic evaluations that typically are based on questions referring to the performance of the overall national economy over the past 12 months (or the next 12 months in

and Bowler (2003) make similar claims regarding perceptions of economic performance and levels of democratic satisfaction. Typical of claims regarding the correlation between the economy and support for democratic institutions is the argument made by Anderson and Guillory (1997, 67): “Furthermore, economic difficulties...have magnified the loss of enthusiasm for democratic politics among people in Western Europe because the shortcomings of democratic governance have been put in sharper relief than previously.”

We find it implausible that the average citizen would lose enthusiasm for democratic institutions simply because of a downturn in the economy – or would “punish” democratic institutions to use the sanctioning terminology. Nevertheless, many of these scholars report individual-level equations of “democratic support” or “democratic satisfaction” with significant positive correlations between NEE and support for democracy.<sup>4</sup>

**Support for European Integration.** The link between economics and political attitudes has played a central role in the history of European unification over the past five decades. Part of the mythology of European unification is the notion that narrow economic agreements, with positive economic pay-offs for the average citizen, would expand and eventually evolve into a much broader political union. A number of students of European unification have proposed an individual level interpretation of these dynamics. European citizens, they argue, condition their support for further efforts at European unification in terms of their evaluations of current or future economic performance.

The link between macroeconomic outcomes at the national level and support for the European Union (EU) has been made on a number of fronts. On the one hand, there is a wealth of impressionistic accounts arguing that the European mass public is more receptive to the EU, presumably both political and economic, during favorable economic times. But when economic performance falters, it seems that public support for the European Union also declines. For example, during the early 1990s which was a period of weak economic performance in Europe,

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the case of prospective economic evaluations).

<sup>4</sup> Some have challenged this argument on the grounds that democratic satisfaction is actually measuring evaluation of incumbent performance. While others have provided convincing evidence regarding the construct validity of this measure (Clarke and Kornberg 1992). We are agnostic on the appropriateness of this measure. Our reservations concern the plausibility of the argument that short-term fluctuations in the economy shape attitudes toward democratic institutions. Our criticism of the literature concerns the inclusion of economic evaluations in these equations that are plagued by measurement error which we contend inflates the hypothesized correlations.

particularly with respect to job creation, support for European unification declined, from a high of 81% in 1991 to 73% in the spring of 1994 (1996). More rigorous efforts at establishing this link have also been proposed. One approach builds on the economic voting literature, arguing that fluctuations in national macro-economic conditions will affect EU support. This approach was originally adopted by Eichenberg and Dalton (1993) but has been applied more recently by other students of European integration (e.g., Gabel and Whitten 1996; Anderson and Kaltenthaler 1996; Hooghe and Marks 2005). And some have even argued that domestic economic outcomes have shaped the support for the EU in prospective member countries (Christin 2005).

The link between short-term policy outcomes and public support for the EU strikes us as more plausible than their association with public support for more mature democratic political institutions. EU institutions might not benefit from broad legitimacy and hence the European publics may in fact blame EU institutions rather than policy-makers for short-run policy failures. Nevertheless, there is only a weak connection between EU policy and domestic economic performance. Until recently, the EU was responsible for neither fiscal nor monetary policies (and this certainly was the case for the period covered by our survey data testing this argument). And while EU membership represents a constraint on national economic policies, historically, this effect has been indirect and hence we seriously doubt that the average citizen has the sophistication and knowledge to recognize this linkage.

In sum, our general criticism of peripheral economic models of EU support is that until recently the policy initiatives of the EU have had very little effect on economic outcomes. Arguably, domestic politics and the global economy shape a country's economic performance much more than EU policy does. Hence, the theoretical justification for a traditional economic voting model of EU support is weak at best.

### **Speculations on the Appropriate Specification of “Peripheral” Economic Voting Models**

Our discussion of the peripheral economic voting models of democratic satisfaction and EU support lead us back to the empirical puzzle that motivates this essay. If the theory is suspect, then how do we account for the empirical evidence of correlation between national

economic evaluations and public support for political institutions? A critical assumption in peripheral economic voting models is that the economic evaluation variables on the right-hand side of these equations are free of measurement error that might bias the results. As we (Duch, Palmer and Anderson 2000) and others (Kramer 1981; Erikson 2004; Evans and Andersen 2006; Hibbs 2006) have demonstrated, this is a questionable assumption. What is this measurement error and why should we expect it to be particularly troublesome in the case of models derived from “theory drift”? Much of the insights into the nature of this measurement error have been developed in the study of U.S. economic voting models.

Error in the measurement of personal financial situation (PFS) is frequently identified as contributing to the weak relationship between pocketbook concerns and vote choice or evaluations of incumbents. Markus (1988) characterized the measurement error as essentially random – any systematic component of PFS is not considered measurement error even if it captures highly personal factors unrelated to government policy making. Kramer (1983), on the other hand, suggested that the measurement error in PFS is systematic, incorporating factors that are unrelated to government-induced economic changes – such as partisanship. By adopting Kramer’s specification of the measurement error, Palmer (1999) has shown that there is in fact a relationship between pocketbook assessments and reported vote in presidential and congressional elections. In controlling for the systematic factors that shape PFS evaluations but are unrelated to government-induced policy outcomes (e.g., partisanship and life-cycle circumstances), Palmer’s equations essentially estimated a “clean” pocketbook effect, i.e., one that is directly attributable to government-induced policy outcomes.

In this essay, we posit that NEE suffer from the same sources of systematic measurement error that plague PFS evaluations. This suggests that our measure of NEE incorporates measurement error,

$$(1.2) \quad \begin{aligned} X_i &= X_i^O + X_i^S + \varepsilon_i \\ X_i^S &= f(W) \end{aligned}$$

where,  $X_i^O$  is the objective economic evaluation,  $X_i^S$  captures systematic differences due to information and subjective factors (i.e.,  $W$ ), and  $\varepsilon_i$  is the stochastic component. In this formal definition, individual-level evaluations contain two forms of “noise”: subjective considerations

and random fluctuations. Both forms of noise constitute sources of non-attitudes. In previous research (Duch and Palmer 2002), we modeled  $X^S$ , leaving the objective evaluation and the stochastic component in the disturbance.

But we contend that the systematic measurement error in NEE affects estimation of the economic voting relationship in exactly the opposite manner as for PFS evaluations. The systematic factors contributing to NEE measurement error could in fact artificially inflate the correlation between NEE and the dependent variable. An example developed elsewhere (Duch, Palmer and Anderson 2000) considers the case of economic voting in U.S. presidential elections where NEE is strongly shaped by partisan pre-dispositions. If we include NEE in an economic voting model without controlling for this systematic measurement error ( $X_i^S$ ), NEE will “pick up” the direct effect of partisan pre-dispositions, thereby producing an inflated estimate of the relationship between NEE and vote choice.

We expect this problem to be exaggerated in models derived from “theory drift”. Models derived from “theory drift” have dependent variables that we contend are only tangentially linked to economic policy outcomes. Nevertheless, the NEE variables exhibit a high correlation with the dependent variable primarily because of the systematic component of measurement error in NEE ( $X_i^S$ ). In the conventional economic voting model, systematic measurement error in NEE is defined as factors that shape NEE but are essentially unrelated specifically to economic policy outcomes – in other words the distinction between  $X_i^S$  and  $X_i^O$ . We expect that much of the systematic measurement error that shapes NEE but is not specifically related to economic policy outcomes ( $X_i^S$ ) is highly correlated with dependent variables in “theory drift” models. The reason is simply that the dependent variables in most of these “theory drift” models tend to be rather diffuse political evaluations that have the same psychological antecedents as economic evaluations – the  $Z$  control variables in equation (1.1) are very similar to the  $W$  variables predicting  $X_i^S$  in equation (1.2). For example, there is considerable evidence suggesting that economic evaluations are shaped by partisanship. Because partisanship tends also to be strongly correlated with institutional evaluations (a typical dependent variable in these theory drift models), not addressing the measurement error problem results in an inflated

correlation between economic evaluations and the dependent variable. We now explore this problem with respect to democratic satisfaction and support for European unification.

*Systematic measurement error as a source of augmentation bias.* The argument that systematic measurement error inflates the correlation between economic evaluations and support for political institutions is based on two sets of expectations. First, the economic evaluation measures in these models typically are not exogenous. Second, some of the systematic error associated with the measurement of economic evaluations is also correlated with democratic satisfaction and EU support. Our model of variation in economic evaluations can be broken into the three categories identified in equation (1.2): 1) random (i.e., unsystematic) measurement error ( $\varepsilon_i$ ); 2) systematic measurement error ( $X_i^s$ ); and 3) factors contributing to meaningful fluctuations in economic evaluations ( $X_i^o$ ). By controlling for systematic and random measurement error in economic evaluations, we expect to be able to better evaluate the actual link between economic perceptions and public support for political institutions.

Essentially, systematic measurement error makes NEE subjective rather than purely objective. While purely objective NEE may vary across individuals, this variation is “meaningful” with respect to the relationship between the economy as a policy outcome and the political actor/institution held responsible for that outcome. In other words, variation in the objective, or “politically relevant” (Hibbs 2006), component of NEE is attributable to policy-related factors that differentiate among individuals in terms of their preferences over competing policy goals and initiatives. An example here would be factors that differential individuals in terms of the importance they give to controlling inflation rather than promoting job creation.

The systematic component of the measurement error in economic evaluations consists of three broad factors. First, economic evaluations are shaped by partisanship. Duch and Palmer (2002) have demonstrated that NEE is to a large extent shaped by partisan pre-dispositions – this is the case in both the American and European contexts. In addition to political partisanship, respondents may rely on personal experiences and regional economic circumstances to formulate an evaluation of the national economy. Citizens who infer national conditions from personal and local experiences are effectively evaluating the economy in a subjective rather than objective manner. Similarly, NEE may vary across individuals due to differences in levels of information

and sophistication about government policy and economic outcomes. Third, social class differences may systematically shape economic evaluations. Individuals in different socioeconomic circumstances might view the same economy in a very different light (MacKuen and Mouw 1995). Similarly, citizens of different sexes and races may perceive the economy differently due to biases in their general attitudes toward the economic and political systems (on gender and economic voting, see Welch and Hibbing 1992).

As posited earlier, measurement error in economic evaluations is likely to represent a problem for models of support for European Union and democratic satisfaction because the systematic components of the measurement error are also correlated with these attitudinal variables. We argued above that partisanship shapes economic evaluations. There is also strong evidence suggesting that partisanship influences attitudes toward European Union. The classic statement of this is Reif's (1984) notion that European elections are "second order elections." By second order effect we mean that support for European institutions is strongly correlated with voter's evaluations of national incumbents. Also, contributing to the correlation between partisanship and support for the EU is the fact that national parties having varying levels of enthusiasm for the EU which tends to get reflected in the attitudes of their partisans.

We also suspect that government partisanship is correlated with democratic satisfaction. Just as partisans are less likely to criticize the economic performance of incumbent governments we expect that they will also have more positive views of the institutional status quo when their party is in government. For this reason, we expect partisanship as a source of systematic measurement error in NEE to inflate the statistical relationship between the (perceived) economy and both measures of support for political institutions.

Additionally, we expect socioeconomic and demographic factors to contribute to the inflation of estimated relationships in peripheral economic voting models. Again this augmentation bias is due to the direct effects these sources of systematic measurement error have on the dependent variables in peripheral models. For instance, the research of Gabel and Palmer (1995; Gabel 1998a, 1998b) demonstrates that support for European integration varies with individual-level differences in job skills and financial assets as reflected in the citizen's socioeconomic situation and life-cycle stage. These same variables also are correlated with the systematic component of measurement error in economic evaluations ( $X_i^S$ ).

**Statistical Hypotheses.** Our theoretical arguments can be summarized in three statistical hypotheses. First, we argue that measures of national economic evaluations contain systematic measurement error. We demonstrate this by specifying models of national economic evaluation in four European countries. Our expectations regarding the model of national economic evaluations are summarized in Table 1. We expect two sets of variables to affect variation in national economic evaluations: a set of variables that reflect “Policy Related Variation” and a set of variables capturing systematic measurement error. The “Policy Related Variation” represents differences in economic evaluations that are grounded in self-interested differences in opinion regarding measures of economic performance. Individuals who value reducing inflation more than maintaining low unemployment (e.g., retired respondents on fixed incomes) might emphasize price stability more than job creation when evaluating the national economy.

**Table 1: Assumptions Dictating the Measurement Error Specification for National Economic Evaluations**

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<i>Sources of Policy-Related Variation</i>	
Unemployment Concern	Self-Employed
Reduced Government Role	Farmer
Family Income	Retired
Manual Worker	
<i>Distinct Sources of Systematic Measurement Error</i>	
Government Partisanship	Age
Retrospective Personal Financial Situation	Sex
Personally Unemployed	Race
Education	Region
Cognitive Mobilization	

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On the other hand, “Distinct Sources of Systematic Measurement Error” consist of variations in economic evaluations that are unrelated to policy concerns. For example, evaluations of the national economy that are shaped by partisan attitudes are certainly not grounded in any “objective” assessment of economic performance, nor do they reflect a policy-related weighting of different economic outcomes (e.g., a preference for job growth rather than price stability). The variables listed in the bottom half of Table 1 constitute systematic measurement error because they contribute to variation in national economic evaluations that cannot plausibly be associated with preferences for particular economic policy goals or initiatives. Older respondents might be unhappy because they have not benefited from robust economic growth but this in itself does not reflect an obvious policy orientation. These older respondents are unhappy because they are net losers but this does not reflect a differential weighting of economic policy goals.

A second hypothesis simply posits that in traditional specifications of conventional economic voting models (with incumbent government support as the dependent variable) and of “theory drift” models (with democratic satisfaction and support for the EU) national economic evaluations will be statistically significant. In the former case, the significant statistical relationship reflects the existence of a causal relationship between the economy as a policy outcome and political support for the incumbent government. But in the latter case, we contend that the significant statistical relationship is an artifact of systematic measurement error whose sources produce a spurious relationship due to their direct effects on support for political institutions.

Our third (and critical) hypothesis here is that controlling for the systematic measurement error in national economic evaluations should produce two distinct outcomes. First, in the case of the incumbent vote models (i.e., the conventional economic voting models), we expect to observe no change or an improvement in the estimated effect of national economic evaluations on vote choice. Second, in the case of the “peripheral” economic voting models—democratic satisfaction and EU support—we expect to find that controlling for measurement error in NEE significantly weakens, if not eliminates, the evidence of an economic voting relationship.

## Results

As Table 1 indicates, we hypothesize that there is heterogeneity in national economic evaluations – some of it policy related and some reflects measurement error. Table 2 presents estimated ordered probit equations of national economic evaluations in each of the four European countries. Three variables that we suspect are the primary source of systematic measurement error in NEE are highly significant across the four samples: government partisanship (i.e., favoring a party in the governing coalition), retrospective personal financial situation, and whether the respondent was unemployed. With the exception of partisanship in West Germany and the unemployed variable in Italy, these variables have statistically significant effects in all four equations. The other hypothesized sources of systematic measurement error are less uniformly significant in the model. The United Kingdom might be the exception since education, sex and race all achieve statistical significance. Overall, there is clear evidence of systematic measurement error in national economic evaluations.

In addition to the systematic measurement error, heterogeneity in NEE is also driven by policy-related factors. The two variables making the most significant contribution in this regard are unemployment concerns (significant in all but France) and support for a reduced government role in the economy (significant in France and Italy). Once again the UK seems to exhibit the most policy-related heterogeneity in retrospective evaluations of the economy. But on balance, there are reasonable levels of policy-related heterogeneity in all four of these NEE equations. The results in Table 2 confirm that national economic evaluations are shaped by both policy-related factors and systematic measurement error.

**Table 2: Ordered Probit Models of Retrospective National Economic Evaluations, EuroBarometer 21 (1984)**

	France		W.Germany		Italy		UK	
	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat
Government Partisanship	.211**	6.35	.066	1.68	.162**	5.36	.288**	10.51
Retrospective PFS	.318**	8.09	.526**	11.14	.431**	9.83	.340**	10.49
Personally Unemployed	-.61*	-2.36	-.45*	-2.53	.47	1.63	.49*	2.35
Unemployment Concern	.027	.62	-.317**	-7.69	-.158**	-3.85	-.223**	-5.92
Reduced Government Role	-.303**	-7.07	.031	.77	-.128**	-3.29	-.029	-.76
Family Income	.010	.54	.003	.21	-.021	-1.39	.0021	.10
Manual Worker	-.32	-1.61	-.29*	-2.00	.11	.54	.40*	2.40
Self-Employed	-.07	-.29	-.32*	-2.12	.17	.84	.45*	2.43
Farmer	-.27	-1.15	.32	1.12	-.10	-.35	.26	.57
Retired	-.10	-.47	-.18	-1.14	.33	1.69	.39*	2.26
Education	-.026	-1.55	.013	.92	.032*	2.20	.067**	3.39
Cognitive Mobilization	-.036	-.89	.043	1.04	.050	1.20	-.003	-.07
Age	-.0019	-.65	-.0004	-.16	-.0025	-1.00	.0049	1.94
Female	-.10	-1.36	-.18*	-2.33	-.05	-.72	-.21**	-2.86
White	-.28	-1.41	-.18	-1.32	.28	1.43	.60**	3.42
Region 2	-.04	-.33	-.04	-.27	-.08	-.75	.05	.40
Region 3	-.07	-.42	.28	1.84	-.07	-.60	.12	1.25
Region 4	-.35**	-2.81	.29	1.78	-.12	-1.17	-.09	-.63
Region 5	-.08	-.65	.33*	2.10	-.27*	-2.22	-.01	-.06
Region 6	-.20	-1.26	.41**	2.62	~~~		-.16	-1.29
Region 7	-.24	-1.82	~~~		~~~		~~~	
Constant	.80**	3.16	.71**	2.59	.02	.07	-.02	-.09
$\mu_1$	1.17**	21.93	1.21**	15.30	1.15**	22.79	.93**	17.64
$\mu_2$	2.54**	29.17	2.72**	29.69	1.88**	30.41	1.83**	27.54
$\mu_3$	~~~		4.56**	29.46	3.38**	26.51	3.57**	32.02
$\chi^2$ statistic for entire model	304.9**		284.7**		184.6**		432.3**	
% Predicted Correctly	49.1		53.2		42.0		44.7	
% Error Reduction	19.2		10.6		6.6		22.5	
N	969		955		1019		1009	

Note: Dependent variable is retrospective assessment of the general economy over the last 12 months. See appendix for further details. \*\*p<.01; \*p<.05

Our second hypothesis is that national economic evaluations prove highly significant in all three models – support for the incumbent, democratic satisfaction, and support for membership in the European Union—when there are no controls for systematic measurement error in the equation. Table 3 reports the results for binomial probit models of incumbent support (where the dependent variable indicates whether the respondent would vote for a party in the governing coalition). Clearly other factors in this model contribute to the explanation of vote choice, but nevertheless retrospective evaluations of the economy are consistently significant in all four equations. As we would expect, positive retrospective evaluations of the economy generate a greater likelihood that respondents will vote for an incumbent party. In a similar fashion the ordered probit results in Tables 4 and 5 are consistent with other estimations of models derived from “theory drift” in that they show statistically significant and positive coefficients for the NEE variable (with one exception out of eight). It is results like those reported in Table 4 and 5 that have generated widespread consensus regarding the empirical validity of these “peripheral” economic voting models.

**Table 3: Binomial Probit Models of Incumbent Support, EuroBarometer 21 (1984)**

	France		W.Germany		Italy		UK	
	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat
Retrospective Eval. of Economy	.185**	2.90	.371**	5.79	.232**	5.75	.488***	9.82
Left-Right Ideology	-.601**	-15.03	.415**	12.76	.040	1.86	.354**	10.94
Strength of Religious Faith	-.044	-1.07	.153**	3.16	.205**	4.81	.028	.81
Personally Unemployed	.25	.90	.02	.09	.10	.34	-.42	-1.56
Family Income	-.026	-1.02	.017	.96	.024	1.40	.064**	2.66
Manual Worker	.12	.95	-.06	-.52	-.065	-.64	-.39**	-3.44
Education	-.026	-1.13	.026	1.27	-.012	-.71	.009	.36
Age	.0035	1.04	.0101**	3.37	.0075**	2.77	-.0026	-.81
Female	.28**	2.69	.06	.66	.13	1.60	.13	1.28
Region 2	-.21	-1.19	-.46*	-2.11	-.06	-.53	.15	1.05
Region 3	.16	.69	-.33	-1.62	-.03	-.25	.14	1.08
Region 4	.04	.22	-.39	-1.81	.14	1.19	.12	.60
Region 5	-.14	-.78	-.19	-.85	.01	.09	-.35*	-2.17
Region 6	.21	1.01	-.23	-1.09	~~~		-.05	-.27
Region 7	-.13	-.72	~~~		~~~		~~~	
Constant	1.64**	5.06	-3.61**	-10.64	-1.70**	-6.97	-3.37**	-10.61
$\chi^2$ statistic for entire model	471.7**		392.8**		107.9**		417.1**	
% Predicted Correctly	83.4		78.6		62.2		78.8	
% Error Reduction	49.5		47.3		13.0		36.7	
N	1009		992		1060		1042	

Note: Dependent variable indicates whether respondents would vote for a party in governing coalition if a general election were held tomorrow. See appendix for further details. \*\*p<.01; \*p<.05

**Table 4: Ordered Probit Models of Democratic Satisfaction, EuroBarometer 21 (1984)**

	France		W.Germany		Italy		UK	
	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat
Retrospective Eval. of Economy	.399**	8.29	.348**	7.60	.242**	6.87	.286**	8.09
Left-Right Ideology	-.129**	-6.01	.125**	6.15	.033	1.86	.070**	3.02
Extremity of Left-Right Ideology	-.085**	-2.84	-.054	-1.75	-.135**	-5.13	.006	.18
Family Income	.046**	2.69	.026	1.75	.034*	2.41	.025	1.43
Cognitive Mobilization	.066	1.78	-.069	-1.62	.048	1.13	-.075	-1.89
Education	.047**	3.08	-.005	-.35	-.039**	-2.74	.024	1.29
Age	.0084**	3.75	.0071**	3.07	-.0018	-.79	.0022	.97
Female	-.08	-1.03	-.01	-1.10	.05	.69	.060	.83
Constant	.45*	2.23	.48*	2.26	.11	.58	.37*	1.96
$\mu_1$	1.19**	21.52	1.21**	13.81	1.36**	26.07	1.05**	18.02
$\mu_2$	2.90**	30.79	3.29**	31.69	2.77**	25.34	2.70**	34.94
$\chi^2$ statistic for entire model	218.1**		139.9**		103.9**		130.9**	
% Predicted Correctly	47.7		65.5		48.7		52.3	
% Error Reduction	15.1		2.8		3.0		0.8	
N	935		922		1017		984	

Note: Dependent variable is 4-category measure of respondents' level of satisfaction with how democracy works in their country. See appendix for further details. \*\*p<.01; \*p<.05

**Table 5: Ordered Probit Models of Support for EC Membership, EuroBarometer 21 (1984)**

	France		W.Germany		Italy		UK	
	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat
Retrospective Eval. of Economy	.217**	4.14	.094	1.87	.133**	3.04	.262**	7.12
Left-Right Ideology	.035	1.51	-.022	-.97	.022	1.04	.048*	2.20
Extremity of Left-Right Ideology	-.046	-1.36	.055	1.59	-.080**	-2.66	-.037	-1.21
Family Income	.045*	2.24	-.004	-.24	.049**	2.87	.035*	2.03
Cognitive Mobilization	.091*	2.05	.026	.52	.049	.99	.053	1.32
Education	.078**	4.13	.049**	3.02	.014	.83	.086**	4.59
Age	.0050	1.89	-.0011	-.40	-.0009	-.31	.0082**	3.52
Female	-.33**	-3.90	.07	.78	-.10	-1.15	.04	.54
Constant	.82**	3.54	1.28**	5.45	1.39**	5.93	-.97**	-5.21
$\mu_1$	1.31**	18.03	1.34**	19.24	1.14**	14.89	.91**	20.58
$\chi^2$ statistic for entire model	77.9**		18.5*		40.0**		133.2**	
% Predicted Correctly	67.7		59.2		75.0		47.0	
% Error Reduction	4.1		0.3		0		18.7	
N	938		887		985		986	

Note: Dependent variable is a 3-category measure of respondents' evaluation of whether their country's membership in the European Community is a good thing. See appendix for further details. \*\*p<.01; \*p<.05

As discussed above, we contend that the economic voting models of democratic satisfaction and EU support in Tables 4 and 5 are fundamentally flawed due to their failure to account for measurement error. More specifically, we hypothesize that the estimated economic voting relationships in these models are spurious—attributable to the systematic measurement error in retrospective evaluations of the national economy rather than the existence of a causal relationship between policy outcomes and support for political institutions. Hence, an estimation method that properly models this measurement error will reduce the strength of the economic voting relationships in Tables 4 and 5. This is the second part of the third statistical hypothesis specified above.

Note that in the traditional “errors in variables” model, controlling for the measurement error in a regressor eliminates the attenuation of that regressor’s estimated effect. The traditional setup, though, assumes that the measurement error is random. As discussed above and shown in Table 2, as well as elsewhere (Duch and Palmer 2002), retrospective evaluations of the national economy contain systematic measurement error attributable to government partisanship, personal experiences and demographic characteristics. We contend here that the sources of this systematic measurement error have independent effects on democratic satisfaction and EU support. Hence, economic voting models that do not control for this systematic measurement error, like those in Tables 4 and 5, will potentially produce augmented estimates of the relationship between economic evaluations and support for political institutions.

In order to test this contention, we re-estimated the models in Tables 3-5 using a method developed by Palmer (1999). In his analysis of pocketbook economic voting, Palmer (1999) adapted a method proposed by Rivers and Vuong (1988) for (recursive) simultaneous probit models. Their estimator is a two-stage conditional maximum likelihood (2SCML) estimator that applies directly to the case of random measurement error. Palmer adapted the Rivers-Vuong method to the case of systematic measurement error. The adaptation involves the construction of an *augmented residual* that includes both systematic and random components of measurement error.

More specifically, the adapted Rivers-Vuong method consists of two steps (discussed in the specific context of the present analysis). The first step is to regress national economic evaluations on a set of exogenous explanatory variables in order to model the measurement



error. This set of variables should capture “meaningful” or policy-oriented variation as well as theorized sources of measurement error in national economic evaluations. Table 1 reviews our assumptions about the nature of the heterogeneity in national economic evaluations, which serves as the theoretical basis for our specification of the systematic measurement error. The first-stage least squares (LS) regressions used to model the measurement error are presented in Table A5 in the Appendix.

Under the assumption of random measurement error, the estimate of the measurement error is simply the first-stage LS residuals. But in the present context, we constructed an augmented residual that incorporates the sources of systematic measurement error. More specifically, the augmented residual ( $\hat{X}_i$ ) is derived by subtracting the policy-related portion of the first-stage LS fitted value (i.e., the portion explained by the variables listed in the top half of Table 1) from the national economic evaluation.

$$(1.3) \quad \hat{X}_i = X_i - X_i^o$$

Hence, the augmented residual combines random variation ( $\varepsilon_i$ ) and “explained” variation attributable to theorized sources of systematic measurement error ( $X_i^s$ ).

The second step in the adapted Rivers-Vuong method is to include the augmented residual ( $\hat{X}_i$ ), as an estimate of the measurement error, in the (binomial or ordered) probit model along with national economic evaluations.

$$(1.4) \quad Y_i = \beta_0 + \beta_1 X_i + \beta_2 \hat{X}_i + \phi_1 Z_i$$

Essentially, the augmented residual obtained from the first-stage regression controls for the effect(s) of the measurement error. If the sources of systematic measurement error do not have direct effects on the dependent variable, then the augmented residual simply controls for the “noise” in the measure of national economic evaluations, thereby eliminating the attenuation bias. However, if the sources of systematic measurement error directly influence the dependent variable (as theorized here for democratic satisfaction and EU support), then the augmented residual also captures these direct effects, potentially eliminating a spurious relationship.

Tables 6A-6D present 2SCML models of incumbent vote, democratic satisfaction and EU

**Table 6A: Comparison of Economic Voting Models that Account for Measurement Error, France**

	Incumbent Vote		Demo. Satisfaction		EU Support	
	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat
Retrospective Evaluation of Economy	.675*	1.97	.708**	3.46	.242	.98
Augmented Residual	-.501	-1.46	-.318	-1.55	-.026	-.11
Left-Right Ideology	-.561**	-11.67	-.105**	-3.99	.037	1.23
Extremity of Ideology	~~~		-.086**	-2.85	-.046	-1.36
Cognitive Mobilization	~~~		.061	1.64	.091*	2.00
Family Income	-.031	-1.23	.043*	2.51	.045*	2.22
Education	-.030	-1.29	.043**	2.77	.077**	4.01
Manual Worker	.21	1.50	~~~		~~~	
Personally Unemployed	.22	.79	~~~		~~~	
Strength of Religious Faith	-.041	-1.01	~~~		~~~	
Age	.0039	1.15	.0084**	3.71	.0050	1.88
Female	.27**	2.59	-.08	-1.11	-.33**	-3.90
Region 2	-.22	-1.26	~~~		~~~	
Region 3	.16	.68	~~~		~~~	
Region 4	.03	.20	~~~		~~~	
Region 5	-.16	-.93	~~~		~~~	
Region 6	.19	.90	~~~		~~~	
Region 7	-.15	-.83	~~~		~~~	
Constant	1.52**	4.56	.42*	2.06	.82**	3.52
$\mu_1$	~~~		1.19**	21.48	1.31**	18.03
$\mu_2$	~~~		2.91**	30.56	~~~	
$\chi^2$ statistic for entire model		473.8**		220.4**		77.9**
% Predicted Correctly		83.5		58.9		67.7
% Error Reduction		49.8		33.3		4.1
N		1009		935		938

**Table 6B: Comparison of Economic Voting Models that Account for Measurement Error, West Germany**

	Incumbent Vote		Demo. Satisfaction		EU Support	
	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat
Retrospective Evaluation of Economy	.840**	2.60	.390	1.79	.451	1.84
Augmented Residual	-.483	-1.48	-.044	-.21	-.372	-1.47
Left-Right Ideology	.376**	9.06	.121**	4.30	-.054	-1.71
Extremity of Ideology	~~~		-.054	-1.76	.053	1.54
Cognitive Mobilization	~~~		-.068	-1.60	.031	.61
Family Income	.015	.89	.026	1.74	-.0045	-.30
Education	.027	1.31	-.006	-.36	.048**	2.91
Manual Worker	.02	.18	~~~		~~~	
Personally Unemployed	.01	.04	~~~		~~~	
Strength of Religious Faith	.149**	3.08	~~~		~~~	
Age	.0103**	3.41	.0071**	3.05	-.0013	-.51
Female	.05	.56	-.01	-.11	.05	.62
Region 2	-.47*	-2.15	~~~		~~~	
Region 3	-.33	-1.64	~~~		~~~	
Region 4	-.41	-1.87	~~~		~~~	
Region 5	-.20	-.92	~~~		~~~	
Region 6	-.23	-1.09	~~~		~~~	
Constant	-3.42**	-9.47	.50*	2.07	1.48**	5.47
$\mu_1$	~~~		1.22**	13.70	1.34**	19.19
$\mu_2$	~~~		3.29**	31.46	~~~	
$\chi^2$ statistic for entire model		395.0**		140.0**		20.7*
% Predicted Correctly		78.7		65.6		59.2
% Error Reduction		47.5		3.1		0.3
N		992		922		887

**Table 6C: Comparison of Economic Voting Models that Account for Measurement Error, Italy**

	Incumbent Vote		Demo. Satisfaction		EU Support	
	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat
Retrospective Evaluation of Economy	-.182	-.66	.056	.25	.185	.67
Augmented Residual	.423	1.52	.190	.83	-.053	-.19
Left-Right Ideology	.047*	2.14	.036*	2.00	.021	.96
Extremity of Ideology	~~~		-.136**	-5.13	-.080**	-2.65
Cognitive Mobilization	~~~		.045	1.07	.049	.99
Family Income	.015	.82	.030*	2.04	.050**	2.80
Education	-.014	-.85	-.039**	-2.73	.014	.83
Manual Worker	-.09	-.90	~~~		~~~	
Personally Unemployed	.04	.01	~~~		~~~	
Strength of Religious Faith	.20**	4.76	~~~		~~~	
Age	.0085**	3.05	-.0012	-.52	-.0010	-.35
Female	.13	1.52	.05	.66	-.10	-1.14
Region 2	-.06	-.50	~~~		~~~	
Region 3	-.02	-.20	~~~		~~~	
Region 4	.14	1.22	~~~		~~~	
Region 5	.01	.09	~~~		~~~	
Constant	-1.69**	-6.93	.10	.55	1.40**	5.92
$\mu_1$	~~~		1.36**	26.08	1.14**	14.83
$\mu_2$	~~~		2.77**	25.15	~~~	
$\chi^2$ statistic for entire model		110.2**		104.5**		40.0**
% Predicted Correctly		62.6		48.5		75.1
% Error Reduction		14.1		2.6		0
N		1060		1017		985

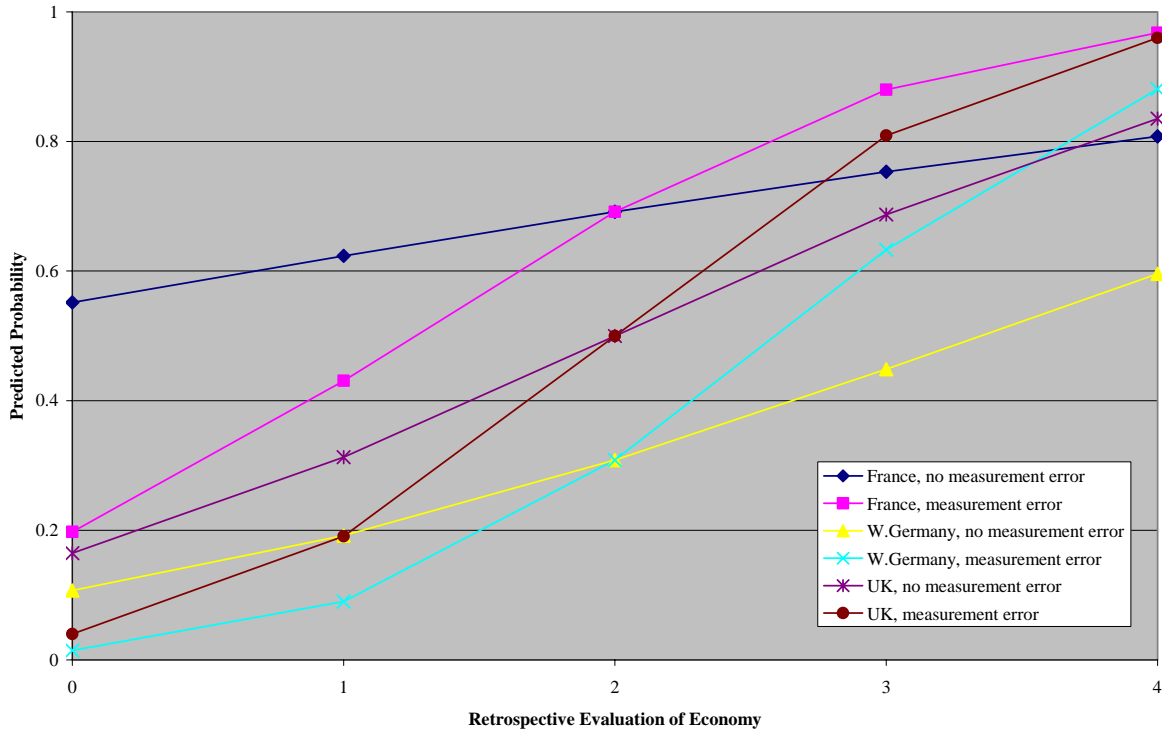
**Table 6D: Comparison of Economic Voting Models that Account for Measurement Error, United Kingdom**

	Incumbent Vote		Demo. Satisfaction		EU Support	
	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat
Retrospective Evaluation of Economy	.875**	3.42	.277	1.59	-.103	-.57
Augmented Residual	-.392	-1.54	.008	.05	.369*	2.04
Left-Right Ideology	.329**	9.07	.070**	2.64	.075**	2.88
Extremity of Ideology	~~~		.006	.18	-.040	-1.31
Cognitive Mobilization	~~~		-.075	-1.87	.045	1.11
Family Income	.065**	2.70	.025	1.43	.037*	2.16
Education	.009	.36	.024	1.29	.083**	4.44
Manual Worker	-.46**	-3.76	~~~		~~~	
Personally Unemployed	-.33	-1.19	~~~		~~~	
Strength of Religious Faith	.028	.83	~~~		~~~	
Age	-.0036	-1.10	.0022	.97	.0089**	3.75
Female	.14	1.42	.06	.81	.02	.31
Region 2	.15	1.00	~~~		~~~	
Region 3	.15	1.15	~~~		~~~	
Region 4	.11	.58	~~~		~~~	
Region 5	-.34*	-2.07	~~~		~~~	
Region 6	-.04	-.25	~~~		~~~	
Constant	-3.28**	-10.14	.37	1.93	-1.05**	-5.42
$\mu_1$	~~~		1.05**	18.01	.92**	20.56
$\mu_2$	~~~		2.70**	34.89	~~~	
$\chi^2$ statistic for entire model		419.5**		130.9**		137.4**
% Predicted Correctly		79.5		52.3		47.1
% Error Reduction		38.7		0.8		18.8
N		1042		984		986

support for France, West Germany, Italy and the United Kingdom. *Augmented Residual* controls for the measurement error in *Retrospective Evaluation of Economy*. According to our “theory drift” argument, the 2SCML models should provide weaker empirical support for peripheral economic voting models of democratic satisfaction and EU support than do the “naïve” specifications in Tables 4 and 5. In other words, the inclusion of *Augmented Residual* should account for the spurious relationship due to systematic measurement error. In contrast, the 2SCML models of incumbent vote should produce stronger estimates of the economic voting relationship since they control for the confounding effect of measurement error in retrospective evaluations of the national economy. By controlling for measurement error, the 2SCML models produce more precise or “cleaner” estimates of the relationship between politically relevant macro-economic outcomes and political support for the incumbent government.

A comparison of Table 3 with the incumbent vote models in Tables 6A-6D reveals that controlling for measurement error strengthens the economic voting relationship considerably for France, West Germany, and the United Kingdom. Figure 1 illustrates the differences in the estimated economic voting relationship for these three countries. The “no measurement error” lines plot the estimated relationships from Table 3, while the “measurement error” lines plot those from Tables 6A, 6B and 6D. Clearly, proper modeling of the measurement error increases the marginal effect of retrospective evaluations on the probability of an incumbent vote.

Figure 1: Comparison of Estimated Effects of Retrospective Economic Evaluations on Incumbent Vote



In contrast, the 2SCML models of democratic satisfaction produce weaker evidence of an economic voting relationship for West Germany, Italy, and the United Kingdom (though the economic effect for France is considerably stronger). While Table 4 indicates that retrospective evaluations of the national economy significantly influence the likelihood of an incumbent vote at better than the 1% level, the estimated sociotropic effect in these countries becomes insignificant at the 5% level once the model controls for systematic measurement error. This finding supports our claim that measurement error produces the spurious relationships reported in Table 4 and hence undermines the validity of peripheral economic voting models of democratic satisfaction.

Similarly, the 2SCML models of EU support produce weaker evidence of an economic voting relationship for France, Italy, and the United Kingdom. Again, the “naïve” model specifications in Table 5 for these countries produce estimated economic voting relationships that are statistically significant at better than the 1% level. However, the models in Tables 6B, 6C and 6D that account for the systematic measurement error in retrospective evaluations of the economy produce estimated effects that do not prove significant at the 5% level. In sum, the

contrast between core and peripheral economic voting models is greatest for West Germany and the United Kingdom where the sociotropic effect is only significant in the incumbent vote equation once the estimation method accounts for measurement error (see Tables 6B and 6D). Furthermore, between these two countries, the United Kingdom produces results most consistent with our arguments about theory drift and measurement error in that the “naïve” specifications suggest that the economy matters in explaining democratic satisfaction and EU support among the British public but it clearly does not in the estimates controlling for measurement error.

### **Discussion**

In this essay, we address an empirical puzzle associated with economic voting models of democratic satisfaction and public support for European Union. We contend that the application of economic voting theory to explain public support for political institutions represents an example of “theory shift” where scholars have presumed that a proven theory of incumbent support also has relevance in “peripheral” contexts. Yet, traditional specifications of peripheral economic voting models perform remarkably well despite their weak theoretical foundations. This poses an empirical puzzle.

We characterize traditional specifications of peripheral economic voting models as “naïve” since they fail to account for measurement error in evaluations of the economy. We then demonstrate that the empirical puzzle is a product of poor model specification and systematic measurement error. More specifically, our analysis shows that controlling for the systematic measurement error in national economic evaluations largely eliminates any evidence of a relationship between the economy and public support for democratic institutions at the national and supranational levels.

This finding has several broader implications for economic voting research as well as theory building in political science more generally. First, the individual-level evidence presented here that systematic measurement error can augment as well as attenuate estimates of economic voting relationships complements recent aggregate-level research. This is consistent with our earlier findings that measurement error due to partisanship, personal financial experiences, and information can systematically bias relationships between incumbent popularity and aggregate series of citizens’ economic evaluations (Duch, Palmer and Anderson 2000).



Second, our analysis calls into question the validity of all peripheral economic voting models, not only those associated with public support for democratic institutions. Essentially, any “economic voting” argument that relies on shifts in the public’s economic mood or on ad hoc emotional responses to short-run economic circumstances is theoretically suspect. Rather than blindly adapting the economic voting paradigm, studies of peripheral contexts should develop original theories that incorporate the specific incentives faced by the relevant political actors. If the economy does matter, it should motivate behavior in a sensible way specific to the context being analyzed. Explanations of political behavior should rest on sound theoretical logic rather than theory by association or analogy.

## Appendix

The *demographic* variables employed in our analysis include a set of dummy variables measuring occupation and employment status: farmer, manual, retired, unemployed, and self-employed. Income is measured by income categories (or ranges) that vary between 10 and 12 depending upon the specific European country. Education is measured by the age at which respondents completed their studies (adjusted for those respondents who indicated that they were still studying). Descriptive statistics for all of the explanatory variables are reported in Table A2.

*Regional dummy variables.* We speculate that perceptions of the national economy vary regionally given variations in regional economic circumstances. The analysis includes a series of regional dummy variables based on the regional codes in the EuroBarometer study. These codes are defined in Table A1.

We measure *political sophistication* with the same two items that comprise Inglehart's cognitive mobilization measure: "When you yourself hold a strong opinion, do you ever find yourself persuading your friends, relatives or fellow workers to share your views? Is so, does this happen often, from time to time, or rarely?" and "When you get together with your friends, would you say you discuss political matters frequently, occasionally or never?"

*Measures of economic circumstances.* The central explanatory variable in our analysis (and the dependent variable in the first-stage regression), *retrospective evaluation of general economy*, is based on the following question: "How do you think the general economy has changed over the last 12 months?" The response set is: got a lot worse, got a little worse, stayed the same, got a little better, and got a lot better. *Retrospective personal financial situation* has the same response set with the following question wording: "How does the financial situation of your household compare with what it was 12 months ago?"

*Measures of economic policy preferences.* Perceptions of the economy may simply reflect economic policy priorities held by individuals – for example individuals concerned with unemployment issues may significantly discount improvements in economic fundamentals such as growth rates (or possibly not even notice them) if there is no significant improvement in the unemployment rates. Five items were included in EB21 that measure respondents' economic policy priorities:

- A measure of concern for the issue of unemployment was constructed from responses to the following question: “During the last year, have you (or some one in your household) worried about losing a job or not finding a job?” Responses were coded as follows: A lot (3); a little (2); not at all (1); and don’t know (1).
- Respondents were also asked a series of agreed/disagreed questions that touched on economic policy issues. We employed two of these: “Unemployment is distressing”; and “The government should intervene less in the management of the economy.” Responses were coded as follows: 1) disagree completely; 2) disagree to some extent; 3) agree if anything; 4) broadly agree; 5) completely agree. Missing values were set to the sample mean.
- Respondents were asked the following question: “Here are some kinds of fears which are sometimes expressed about the future (say 10 to 15 years) of the world we live in. I would like you to tell me which of the following really concern you or you worry about? Three of the “fears” concerned job issues: “foreign workers”; “unemployment amongst the young”; and “loss of job”. A variable measuring concern about job security was constructed by simply adding up how many of these items were mentioned by the respondent.
- Identification with either the Left or Right represents an expression of economic policy preferences. To measure Left-Right placement, respondents were asked to place themselves on a scale from 1 to 10 with 1 representing the Left and 10 representing the Right (“In political matters, people talk of ‘the Left’ and the ‘Right’. How would you place your views on this scale?”). Missing values are set to the sample mean.

We included these five items in a factor analysis (for the entire EuroBarometer sample), which generated two factor dimensions – a primary dimension tapping concern with unemployment issues and job security, and a second dimension that suggesting a high degree of support for reducing government management of the economy. These results are presented in Table A3. Both factor scales are employed as proxies of economic policy preferences.

*Measure of Partisanship.* There is no direct measure of partisanship in the EuroBarometer surveys. Nevertheless, the EB21 survey asks a series of questions asking whether respondents would or would not consider voting for a particular partisan *tendency*. The question wording is as follows: “I will now mention a few political movements. Please tell me each time if it is possible or impossible for you to vote one day for a party which will correspond to the description.” The response set was 1) possible or 2) impossible. We used these questions to construct an incumbent partisanship measure that ranges from –2 for strong anti-incumbent

partisanship to +2 for strong pro-incumbent partisanship. This measure was coded as follows:

- As Table A4 indicates, each of the political tendencies was associated with the incumbent government (G) or with the opposition (O).
- Respondents who indicated that they would vote for a political group associated with the incumbent government but not for one associated with the opposition were assigned a score of 2.
- Respondents reporting that they would vote for a political group associated with the government but also for any of the other non-government groupings were assigned a score of 1.
- Respondents indicating that they would vote for a grouping not associated with the government but do **not** report that they would **not** vote for a grouping associated with the government (these essentially are respondents who reported “don’t know” to the question) were assigned a score of –1.
- Respondents reporting that they would not vote for a grouping associated with the government but would vote for a grouping associated with a non-government grouping were assigned a score of –2.

*Support for European Unification.* Support for European unification is based on the responses to the following question: “Generally speaking do you think that “France’s” membership in the European Community is a 1) good thing, 2) bad thing, 3) neither good nor bad.”

*Democratic Satisfaction.* The measure of democratic satisfaction is based on responses to the following question: “On the whole, are you very satisfied, fairly satisfied, not very satisfied or not at all satisfied with the way democracy works in “France”?”

**Table A1: Region Codes**

Country	Code	Region	% of Country's Cases
France (N=1009)	Region 1	Northwest	16.5
	Region 2	Southwest	17.3
	Region 3	North	6.7
	Region 4	Paris Basin	17.9
	Region 5	Paris Region	18.9
	Region 6	East	7.6
	Region 7	Southeast	15.0
Germany (N=992)	Region 1	Schleswig-Holstein, Hambur	6.5
	Region 2	Lower Saxony, Bremen, West Berlin	14.8
	Region 3	Northrhine-Westphalia	27.7
	Region 4	Hesse, Rhineland-Palatinate, Saarland	16.5
	Region 5	Baden-Wuerttemberg	15.2
	Region 6	Bavaria	19.3
Italy (N=1060)	Region 1	Northwest	27.6
	Region 2	Northeast	18.5
	Region 3	Central	19.1
	Region 4	South	23.3
	Region 5	Islands	11.5
United Kingdom (N=1042)	Region 1	North, Yorkshire-Humberside, Northwest	29.4
	Region 2	North Midlands, West Midlands	15.3
	Region 3	East Anglia, Southeast	22.7
	Region 4	Southwest	7.4
	Region 5	Wales, Scotland	14.3
	Region 6	Greater London	10.9

Note: Region 1 is used as the baseline for comparison in our econometric analysis (i.e., excluded from specification). Respondents in Northern Ireland were excluded from the analysis.

**Table A2: Descriptive Statistics**

Explanatory Variables	Mean	S.D.	Min.	Max.
Government Partisanship	.24	1.27	-2	2
Retrospective PFS	1.73	.86	0	4
Personally Unemployed	.045		0	1
Unemployment Concern	0	1.00	-3.52	3.41
Reduced Government Role	0	1.00	-3.76	3.04
Family Income	5.81	2.63	0	11
<hr/>				
Manual Worker	.274		0	1
Self-Employed	.122		0	1
Farmer	.028		0	1
Retired	.199		0	1
Education	2.36	2.56	0	9
Cognitive Mobilization	1.25	.94	0	3
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Age	42.6	17.7	15	91
Female	.521		0	1
White	.274		0	1
Left-Right Ideology	4.38	1.90	0	9
Extremity of Ideology	1.41	1.28	0	4.5
Strength of Religious Faith	2.38	1.33	0	4

Note: Standard deviations are not reported for dummy variables.

**Table A3: Factor Analysis of Economic Policy Preferences**

Factors	Factor Loadings	
	Unemployment Concern	Reduced Government Role
Less Government	-.182	.797
Left-Right Ideology	-.486	.403
Unemployment Distress	.494	.524
Worry about Unemployment	.549	.031
Job Concern Count	.658	.098
Eigen Value	1.25	1.08
Variance Explained	24.9	21.7

**Table A4: Coding of Government and Opposition Parties, April 1984**

	Extreme Left	Extreme Right	Nationalist	Socialist	Christian Democrat	Communist	Fascist	Conservative	Ecologists	Liberal	Regionalist
France	O	O	O	G	O	G	O	O	O	O	O
UK	O	O	O	O	G	O	O	G	O	O	O
Germany	O	O	O	O	G	O	O	G	O	G	O
Italy	O	O	O	G	G	O	O	G	O	G	O

**Table A5: OLS Models of Retrospective National Economic Evaluations**

	France		W.Germany		Italy		UK	
	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat	Coeff	T-stat
Government Partisanship	.144**	6.51	.044	1.69	.139**	4.97	.254**	11.38
Retrospective PFS	.216**	7.44	.356**	10.06	.361**	8.84	.281**	9.34
Personally Unemployed	-.39*	-2.20	-.31*	-2.29	.37	1.21	.37*	2.14
Unemployment Concern	.018	.59	-.210**	-7.37	-.133**	-3.52	-.188**	-5.53
Reduced Government Role	-.199**	-6.83	.018	.66	-.107**	-3.00	-.025	-.81
Family Income	.008	.61	.002	.25	-.015	-1.10	.0007	.05
Manual Worker	-.22	-1.71	-.18	-1.90	.08	.40	.33*	2.31
Self-Employed	-.06	-.39	-.21	-1.93	.15	.69	.36*	2.26
Farmer	-.18	-1.20	.21	1.01	-.08	-.30	-.02	-.05
Retired	-.07	-.54	-.11	-1.05	.29	1.39	.32*	2.17
Education	-.018	-1.57	.008	.80	.030*	2.32	.059**	3.69
Cognitive Mobilization	-.016	-.59	.022	.78	.054	1.48	-.003	-.09
Age	-.0009	-.44	-.0002	-.11	-.0023	-1.07	.0042*	1.96
Female	-.07	-1.33	-.12*	-2.45	-.06	-1.01	-.17**	-2.77
White	-.20	-1.55	-.11	-1.21	.24	1.15	.49**	3.36
Region 2	-.02	-.23	-.04	-.33	-.06	-.61	.06	.69
Region 3	-.05	-.44	.18	1.71	-.04	-.47	.12	1.44
Region 4	-.23**	-2.74	.18	1.65	-.09	-.99	-.04	-.37
Region 5	-.05	-.61	.21	1.87	-.21	-1.90	-.02	-.21
Region 6	-.11	-1.04	.27*	2.43	~~~		-.13	-1.30
Region 7	-.15	-1.68	~~~		~~~		~~~	
Constant	1.17**	6.62	1.13**	6.26	.64*	2.47	.74**	3.72
F-statistic for entire model	16.2**		16.2**		9.9**		26.9**	
R-squared	.264		.258		.159		.352	
N	969		955		1019		1009	

Note: Dependent variable is retrospective assessment of the general economy over the last 12 months. See appendix for further details. These models were employed to construct the augmented residuals included in Tables 5-8 to control for systematic measurement error. \*\*p<.01; \*p<.05



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