Economics and Elections Revisited

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ABSTRACT

The economics and elections connection has been heavily investigated, although mostly through single-country studies. The first comparative, survey-based research on economic voting, by Lewis-Beck, found serious effects. Subsequently, other comparative scholars have explored this terrain. The most recent, and most ambitious, examinations are by Duch and Stevenson, and by van der Brug et al. These impressive efforts arrive at opposing conclusions about the importance of economic voting. We carry out another major examination, with an eye to reconciling these differences. A carefully specified model of vote choice is estimated on a balanced survey pool (N > 40,000), from ten Western European nations. Special pains are taken with issues of economic measurement, estimation and endogeneity. The finding is that economic perceptions are formed from economic reality, and importantly influence vote choice. Besides enhancing our understanding of comparative political behavior, the strong result speaks to the functioning of government accountability in advanced democracies.
Economics and elections have become a major research area in political science. Initially, systematic economic voting investigations were single-country, focusing mostly on the United States, Britain, and France. [See reviews in Anderson (1995, chp.3), Lewis-Beck and Stegmaier (2000, 2007), Monroe (1984), Nannestad and Paldam (1994), Norpoth (1996).] Innovative as these studies were, they “provide only very crude evidence of whether economic voting exists, or does not, in particular survey contexts” (Duch & Stevenson, 2008, p.23). The remedy, according to Duch and Stevenson (2008, p.2), is the approach begun by Lewis-Beck, who “conducted the first cross-national individual-level study of economic voting.” His examination of special Eurobarometer surveys (Britain, France, Germany, Italy, and Spain) found pervasive and strong economic voting effects. However, with respect to these effects, he raised a scholarly caution: “Will they stand the test of time? Will they stand the test of other researchers?” (Lewis-Beck, 1988, p.162).

Since then, a number of comparativists have taken up these queries, none so fully as the current and ambitious volumes of Duch and Stevenson (2008), and van der Brug, van der Eijk, and Franklin (2007). [See also the earlier, important comparative economic voting survey investigations of Anderson (2000), Hellwig (2001), and Nadeau, Niemi, and Yoshinaka (2002).] The former team declares their work “unambiguously confirms Lewis-Beck’s assertion that economic voting in the Western democracies is widespread and forcefully contradicts the most pessimistic assessments” (Duch & Stevenson, 2008, p.32). The latter team, in contrast, finds “that effects of economic conditions on individual voters are rather modest… significant though not especially powerful” (van der Brug et al., 2007, p.118, p.137). In the study at hand, we plow these same fields,
using comparably ambitious datasets and analytic techniques. Our guiding question is the importance of the economic vote, according to a large set of Western European election surveys. Below, we describe the data. Then we offer a model, and preliminary estimates. As appropriate, we go on to complicate the model in useful ways. In the end, we are able to calibrate the impact of the “real” economy on the vote choice. As shall be seen, it is not small.

Before turning to the empirics, it seems worthwhile to mention what is at stake here, to put the issue in a larger context. The first thing, already mentioned, is establishing the existence of an economic voting coefficient, across a number of advanced democracies. The second thing is what its existence (or lack thereof) says about democratic accountability. If the economics-elections link is found to be negligible, then the ability of the electorate to hold the government accountable for a bad economy is called into question. The implication would be that leaders are free to follow whatever policies they choose, without regard for how they benefit the people. That would hardly be a sanguine outcome for the democracies under study.

THE DATA AND MEASURES

The comparative survey analysis of economic voting imposes strong design demands. First, the relevant items of the instrument should be equivalent, if not equal, from one election survey to the next. In particular, it should contain acceptable economic voting question(s). Second, the administration of the instrument in different countries is required. Ideally, a researcher would have a sound and complete instrument, applied to
multiple elections in multiple countries. This ideal is not easily met. In the two most thorough studies, already mentioned, Duch and Stevenson (2008, p.31) followed an exhaustive harvest strategy, ending with 163 cross-sections from 18 democracies, 1979-2001. Van der Brug et al. (2007, p.66) focused on the European Election Studies (EES), using fifteen democracies and three survey years, for a total of 42 cross-sections (13 in 1989, 13 in 1994, 16 in 1999, individual total N = 32,950). Our data approach approximates the latter, with certain necessary changes.

We draw on the EES, but over four surveys (1988, 1994, 1999, 2004). Analysis confines itself to the original countries surveyed in 1988 (minus Luxembourg and Belgium). That is, ten countries (Denmark, France, Germany, Greece, Ireland, Italy, the Netherlands, Portugal, Spain, and the United Kingdom) each surveyed four times, thereby permitting a complete and balanced data pool (4 x 10). These surveys meet our above criteria: the same instrument with the same economic items, administered in multiple surveys and multiple countries, yielding a total of forty surveys. In particular, it should be stressed that it permits great real variation in political and economic conditions, of special interest here. These data have design advantages over van der Brug et al. (2007), in that they are more current and more efficient for pooling. They have design advantages over Duch and Stevenson (2008), in that the instrument, and its time of administration, is everywhere the same. Of course, there is some trade-off in sample size, although with an individual total N of 44,014, this is not a major concern. [It is larger than the van der Brug et al. (2007) individual total N.]

In terms of measurement, we would like to highlight that of economic conditions. The survey literature on comparative economic voting largely follows the item guidelines
laid down by the pioneering Lewis-Beck (1988, chp.3) battery. The key item measures the “sociotropic” (collective) assessment of the national economy, with respect to its performance retrospectively (over the past year): “How do you think the general economic situation in this country has changed over the last 12 months? (better, same, worse.)”. Such an item is available for all these surveys. With respect to measurement of the national macroeconomic conditions, we principally use economic growth, inflation, and unemployment, as do van der Brug et al. (2007, p.71). [Measures on our other variables are discussed below.]

A GENERAL ECONOMIC VOTING MODEL

In estimating comparative economic models of voting, a common baseline specification across Western European democracies is as follows:

\[ \text{Vote} = f (\text{social cleavages, left-right ideology, the economy}) \]  \hspace{1cm} \text{Eq. 1}

This basic specification first appeared, in the context of comparative economic voting studies, in the Lewis-Beck (1988, p.60) classic. Furthermore, it is the basic specification employed by Duch and Stevenson (2008, pp.113-121). It has several strengths. We begin with consideration of the dependent variable, vote intention for or against the national government. Importantly, it allows consistency across nations. As Duch and Stevenson (2008) explicitly argued, a simple dichotomous dependent variable, incumbent v. opposition, is the viable coding option when a pooling strategy is used: “these models demand that we define the choice set similarly across the studies to be pooled (i.e., we
can estimate a pooled model in which the dependent variable is a vote for or against an incumbent but not when it is for or against each of the available parties – which differ from survey to survey)” (Duch & Stevenson, 2008, p.50). [That said, we did experiment with logistic analysis of a three-way dependent variable (main incumbent party, main opposition party, other parties). The results show the current dichotomous definition of the dependent variable exhibits stronger performance. These findings are available upon request.]

Now consider the strengths of the specification on the independent variable side. First, economic evaluations are treated as a short-term force, acting apart from the controlled long-term forces of social cleavage and ideological identification. Such a distinction is fundamental to any adequate model of political behavior, as Campbell et al. (1960, chp.2) have demonstrated. [See also Lewis-Beck et al. (2008, chp.2).] Second, the specification generalizes easily to most European democracies, thus avoiding different conceptual variables for different countries. Third, we can measure all these variables in the same manner, for a large sample of countries. Last, but not least, its theoretical underpinning rests on the standard reward-punishment model of incumbency voting that girds this literature, regardless of national context (Fiorina, 1981; Key, 1966; Kiewiet, 1983). In terms of what Lewis-Beck (1988, p.34) calls “traditional economic voting theory,” it reads: “When voters approve (disapprove) of past economic conditions, they vote for (against) the governing party (or parties).”

To illustrate the workings of the model and the measures, we first estimate it as a simple additive logistic regression, across all ten countries, utilizing the four available surveys in each. The model variables are operationalized as follows:
Vote = \( b_0 + b_1 \text{Class} + b_2 \text{Religiosity} + b_3 \text{Ideology} + b_4 \text{Economy} \)

\[ + b_5 \text{Months} + b_6 \text{Country} + e \quad \text{Eq. 2} \]

where vote = 1 if the respondent intends to vote for a party in the government coalition, 0 otherwise; ideology = self-placement on a seven-point scale (from left to right, rescaled 0-1, and first multiplied by -1 (then +1 added to it) if the government is left-wing); class = subjective identification (working class, lower middle class, middle class, upper middle class, upper class, rescaled 0-1, and first multiplied by -1 (then +1 added to it) if the government is left-wing); religiosity = service attendance (no religion, never, once a year, few times a year, once a week, several times a week, rescaled 0-1, and first multiplied by -1 (then +1 added to it) if the government is left-wing); economy = sociotropic evaluation (worse, same, better for 1988, 1994, 2004, rescaled 0-1; and very dissatisfied, somewhat dissatisfied, somewhat satisfied, very dissatisfied for 1999, rescaled 0-1); months = months from the last election (to prevent a biased estimate of incumbent support); country = (G-1) country dummies (to correct for differing baseline support for the incumbent); e = error. The wording of the questions appears in the Appendix.

The estimates of Eq. 2 appear in Table 1 (column 1). The substantive results from the non-economic variables are as expected. Higher social class standing significantly increases vote for an incumbent, when it is on the right. More intense religious practice yields the expected positive sign for incumbent right support, and is statistically significant. Left-right ideology itself easily achieves statistical significance, with right-wing identifiers much more likely to favor a ruling right coalition. But it is economic
evaluation that wants our attention. The sociotropic retrospective item represents the pivotal measure, asking the respondent to evaluate the condition of the national economy over the past year. We observe, initially, that its coefficient firmly registers statistical significance, with $p < 0.01$.

Let us continue to explore more fully the idea of this general economic voting coefficient, $b_4 = 1.04$. These pooled data offer considerable statistical leverage, beyond the very large sample size itself. In particular, real economic conditions exhibit considerable variation, namely voters within a nation are exposed to four different sets of economic conditions, and the nations themselves offer ten different economic settings. Thus, voters are no longer trapped by the “Kramer problem,” observing one fixed economic value which, in theory, generates no variance at all (Kramer, 1983). Instead, with the pooled data, there is now real economic variance in abundance available for voter observation, so increasing chances of reliably recovering the actual economic voting parameter. The economic variable, which for purposes of subsequent comparison we label National Economic Perception (NEP), holds substantive as well as statistical significance. We observe, for example, that moving from a perception of last year’s economy as “worse” to “better” makes that voter almost three times more likely to vote for the incumbent (i.e., the odds ratio = 2.8). This is a large effect in an absolute sense. Further, it is large compared to the estimated social cleavage effects, i.e., the odds ratios for social class and religious practice are, respectively, 1.4 and 1.8.
Economic effects seem present. However, the case is by no means open-and-shut. Several issues still need to be addressed, among them the parsimonious specification of the model. The sparse specification of Eq. 1 – cleavages, ideology, economics – comes from an established literature, and can be consistently applied across the countries sampled. Other specifications are possible, of course. Van der Brug et al. (2007, p.26) argue for more elaborate controls. Duch and Stevenson (2008) take on this argument, comparing a “sparse” specification with a “rich” specification. They conclude that “the estimate of economic voting is fairly stable to even drastic revisions in the list of other controls” (Duch & Stevenson, 2008, p.120). The control variables used in our Eq. 2 appear at least as important as those they used. That is, in the surveys they examined, left-right ideology was used in 97 percent, religiosity in 92 percent, and class in 88 percent of the cases (Duch & Stevenson, 2008, p.114). Other controls were generally available much less often and, on the basis of their reported empirical tests, made virtually no difference in terms of assessing economic effects (Duch & Stevenson, 2008, chp.4).

Thus, sound evidence can be marshaled in defense of the sparse specification. Still, judicious exploration of certain other controls, in the name of more rigorous testing, has merit. We begin with a very strong test. In particular, it is possible to place a past (recalled) vote on the right-hand side for the full four-survey ten-country pool. The disadvantage of such a strategy is that it may again “over-control” for other forces acting on the vote intention, thereby suppressing, say, the true impact of economic evaluation
(Greene, 1997, pp.586-588). The advantage, however, is that it picks up the influence of other independent variables that, for various reasons, could not be included (as is the case here). In that way, as a proxy variable for omitted independent variables, it allows a vigorous extension of the model specification. [Another example of the successful implementation of this strategy in the political economy literature appears in Burkhart and Lewis-Beck (1994).]

In Table 1 (column 2) such an equation, which includes past vote recall, is estimated. One observes, first, that the pseudo-R-squared more than doubles in magnitude, to 0.71. Moreover, the percentage correctly predicted increases greatly, to 89.6 percent. The past vote coefficient itself is large, and highly significant. Furthermore, as expected, the coefficients on the original control variables are diminished. Surprisingly, though, the NEP coefficient declines only slightly in magnitude (down from 1.04 to 0.93), and its standard error rises only slightly. With this strong result, it seems possible to dismiss the charge that effects of economic perception were much inflated, due to the lack of adequate model controls. Moreover, our model fits compare favorably to those arrived at by Duch and Stevenson (2008) and van der Brug et al. (2007).²

Impressive as these results might be, they are still open to another challenge. That is, there may be a concern about the long-term socio-psychological controls in this voting behavior model. While left-right identification, some would argue, could be a preferred cross-national measure of this psychological anchor, it is certainly not the only measure. Party identification, of course, stands as an alternative. [Two leading Western European cases where both party identification and ideological identification are contenders for this anchoring role are France and Portugal, both countries in our study. See the analysis and
A control on party identification would seem especially important, given the serious argument about economic perceptions being largely a product of partisan bias. Therefore, we control on party identification, alongside ideological identification (see Table 1).

Note that party identification is measured only in three of the four surveys, with 1994 the absent survey year. Therefore, for purposes of proper baseline comparison, we first replicate the above equations, in Table 1 (columns 3 and 4) on the three available surveys – 1988, 1999, and 2004. We observe that the new NEP coefficients – 1.14 and 1.01 – are quite similar to the originals of Table 1 (columns 1 and 2). Now we add party identification to the specification. Even using the most inclusive measure of party identifiers (i.e., those “close” to a party, “fairly close” or just “a mere sympathizer”), our main finding does not really alter (Table 1, column 5). The coefficient of NEP, at 1.05, is down only a bit from the comparable value (of 1.14) without the party identification control. Furthermore, party identification proves to be a less strong control than past vote recall which, when included in the equation of Table 1 (column 4), reduces the NEP coefficient to 1.01. A final exercise, which does appear open to the charge of an “over-control,” is to include all three of these variables – ideological identification, party identification, and past vote – on the right-hand side. This is done in Table 1 (column 6). Even in the face of this most stringent test, the NEP coefficient still registers a highly significant 0.87.
The above considerations suggest existence of the comparative economic voting coefficient as a population parameter. However, before embarking on further interpretation, its statistical significance must be questioned. In brief, the reported standard errors may be too small, due to the clustering of individual observations within countries. For example, the respondents of Spain tend to be more like each other than would be a random draw of Europeans, because they all live within the same boundaries. This clustering could make them less than fully independent observations, at the same time exaggerating the significance values. To test this assumption, we take as our baseline the specification of Table 1 (column 2), which imposes the very strong control of past vote, while at the same time maintaining the full sample size available from all four surveys, and explaining a large portion of the variance. For purposes of comparison, this equation reappears in Table 2 (column 1).

There are different ways to correct for this clustering problem, otherwise known as intraclass correlation. A direct, and demanding, way is the clustered robust standard errors technique, which appropriately inflates standard error estimates to adjust for the clustering (Raudenbush & Bryk, 2002). We employ this robust standard errors correction (Table 2, column 2) and find the NEP coefficient still registers the same magnitude, of 0.93. [Recall that the issue now is not one of biased parameter estimation, but rather biased standard errors.] More pointedly, we also observe that with the adjusted standard
error NEP continues to register a very high level of statistical significance (at 0.01 or better).

Another difficulty to address is the possibility of interaction effects. So far, we have modeled only main (additive) effects. However, since the sample stretches across ten nations, it seems reasonable to entertain the possibility that the effects of any of the independent variables might vary over space. As a test, then, we allowed the impact of the explanatory variables to vary across countries, yielding 45 interaction terms (five variables times nine country dummies). The economic coefficient, however, was not allowed to vary, so as to estimate its potential impact in the face of all possible other interactions. While these 45 interaction coefficients are not reported in Table 2, the NEP coefficient is (in column 3). The addition of these many interactions increased the variance explained only slightly (up two points). Further, the NEP coefficient itself, at 0.93, remained unchanged.

The above analysis purposefully fixes the economic coefficient, while allowing the other coefficients to vary across countries. But what about variation in the economic coefficient itself? Looking at the economic voting coefficient country-by-country, or even within an interaction framework, is not the same thing as directly testing for the possibility of its randomness. That is best done through the use of a multi-level model. Therefore, we carry out this test, with logistic estimation of a multi-level model where all the slope coefficients, including the economic coefficient, are permitted to vary randomly. In Table 2 (column 4), the fixed effects estimates for each independent variable are reported. The coefficient for NEP is a significant 0.94, almost exactly the same value as in previous columns of the table. What is the meaning of this result? The
interpretation is straightforward. The general population parameter of the economic voting coefficient is estimated to be 0.94, the statistically significant estimated fixed component of the NEP coefficient.

Of course NEP also has a random component, as do all the other explanatory variables. However, the random coefficient for NEP (of 0.46) is not the most volatile. All the explanatory variables (save class) display a significant random component. Moreover, the size of this component in proportion to the fixed effect is greater for ideology, religion and the month variable (smaller for past vote and class). Assuming the fixed effects component estimate for NEP follows a normal distribution, then we can set a confidence interval around it, to assess estimated coefficients from, say, separate country samples. For example, taking multiples of its standard deviation estimate (of 0.46), we obtain the following confidence intervals: for 68% of the cases, [0.94 +/- 0.46 (1)] = [0.48, 1.4]; for 80% of the cases, [0.94 +/- 0.46 (1.28)] = [0.35, 1.53]; for 95% of the cases, [0.94 +/- 0.46 (1.96)] = [0.04, 1.84].

What do we observe when we examine the NEP coefficients estimated from the ten separate countries (using the baseline model of Table 1, column 1)? We see that eight of the ten coefficients (80%) distribute themselves symmetrically within the 80% confidence interval, as expected, with four higher and four lower than the value of the fixed effect (0.94). Further, as expected, the more extreme cases also distribute themselves symmetrically, with Denmark (at 0.27) in one tail, and Greece (at 1.91) in the other tail. [This interpretation finds further support with a classic interaction analysis, where the NEP variable is multiplied by nine country dummies using Portugal as the reference case (picked because the value of its coefficient is closest to the fixed effect –
1.01 v. 0.94) and allowing all explanatory variables to vary (54 interactives including NEP x countries); we can observe that only two interactive terms are significant at the 0.05 level, again the one for Denmark (negative) and Greece (positive).

The foregoing analyses have challenged the presence of a general economic coefficient, in various ways. Still, the economic coefficient persists, as a rather strong and stable estimate. However, before accepting such results, we need to establish the actual economic contents of these perceptions.

**THE ECONOMIC PERCEPTIONS: “REALITY TESTS”**

There is scattered work on the topic of whether economic perceptions are based on the “real” economy. Some country studies – most notably on France, the United Kingdom, the United States – have shown that economic perceptions at the macro level can correlate with macroeconomic indicators, such as growth or unemployment (Kiewiet & Rivers, 1984; Bélanger & Lewis-Beck, 2004; Nadeau & Lewis-Beck, 2001; Sanders, 2003). These findings provide useful background evidence in favor of the economic voting hypothesis. But they are only preliminary. If we fail to uncover a systematic link between subjective economic perceptions and their objective counterparts, at the aggregate level of behavior, then the case for an economic vote rests on shaky ground. Hence, for each of the forty surveys, we record its average NEP score. Also, we record the country’s scores on macroeconomic performance at that time: economic growth, unemployment, unemployment change, inflation, and interest rate.

We then correlate average NEP with these macroeconomic indicators. The correlations (Pearson’s r) are in the expected direction, and moderate to strong:
unemployment (-0.21), inflation (-0.27), interest rate (-0.27), unemployment change (-0.49), Gross Domestic Product growth (0.65). Impressively, GDP growth renders the strongest correlation, and stands regarded by some as the foremost macroeconomic indicator for use in economic voting studies (Lewis-Beck, 1991, p.5). These results, to be expanded upon below, imply real connections between the subjective and objective economies. However, these correlational results are introductory, in that they are bivariate. Moreover, there is high collinearity between unemployment rate change and GDP growth ($r = -0.60$), and interest rate and inflation ($r = 0.87$).

Therefore, we predict (ordinary least squares) average NEP from the three central macroeconomic variables: growth, inflation, unemployment:

$$
\text{NEP} = 0.41^{***} (0.07) + 0.06^{***} \text{GDP} (0.01) - 0.02^{***} \text{INFL} (0.01) - 0.01^{*} \text{UNEMP} (0.01) + e
$$

Eq. 3

$R$-sq. = 0.56 ;  adj. $R$-sq. = 0.52 ;  $N$ = 40

where NEP = country-level means for individual economic perceptions (based on perceptions originally scaled to 1, 0.5, 0 for positive, neutral and negative perceptions); GDP = GDP growth; INFL = inflation; UNEMP = unemployment rate; $e$ = error; figures in parentheses = standard errors; $^*$ = statistical significance at 0.10, $^{**}$ = statistical significance at 0.05, $^{***}$ = statistical significance at 0.01, all tests one-tailed; $N$ = 40 surveys.

Overall, the model fits the data well, as the adjusted $R$-squared of 0.52 indicates. [A United States example of such a result appears in Nadeau and Lewis-Beck (2001).] Clearly, real economic conditions propel subjective national economic assessments.
These perceptions are not based on illusion. Rather, to a large extent they come from measurable national economic activities.

As impressive as these “reality” tests may seem, two difficulties remain. First, there is the risk of the ecological fallacy, since the data are aggregate-level. Indeed, the early move to individual-level survey data to test theories of economic voting was motivated precisely by the fear that the macro-links between the economy and elections were spurious. As Lewis-Beck (1988, p.30) observed: “What gives meaning to the foregoing statistical associations between economics and elections is the underlying belief that individual citizens react systematically to economic stimuli at the ballot box.” On similar grounds, Duch and Stevenson (2008, p.22) make a current case for individual-level, survey analysis of comparative economic voting.

Second, there is the risk of the endogeneity of economic perceptions, once analysis returns to the individual level (Anderson et al., 2004; Evans & Andersen, 2006; Wlezien, Franklin, & Twiggs, 1997). In particular, economic perceptions may be a “mirage,” with no real economic life of their own. Instead, they could be formed by political preferences. For example, because John likes the party in power, he perceives the economy to be doing well, while Sally perceives the economy to be doing badly because she dislikes the party in power. In such a situation, individual economic perceptions are largely devoid of economic reality. Thus, the effect of economic perception on the expressed vote, as estimated by the usual regression techniques, comes out heavily biased. As van der Brug et al. (2007, p.26) argue: “an adequate research design… avoids using subjective indicators of economic conditions, since these are
strongly contaminated and subject to severe endogeneity problems.” We address these difficulties below.

Since individual voters may simply be seeing their national economy as better (or worse) because they like (or do not like) the current government, the estimated NEP coefficient (in Table 2) does not necessarily tell us much about real economic effects. Put another way, to the extent the individual voter’s national economic perception (NEP), standing on the right-hand side of the equation, is endogenous, its slope estimate is necessarily biased. [It amounts to the single-equation equivalent of simultaneous equation bias.] To reduce, if not eliminate, that bias, it is necessary to render the NEP variable effectively exogenous. Instrumental variables methodology can be employed for this purpose. This instrumental variable, NEP’, has exogenous status because it is constructed from truly exogenous variables. This instrumental variable stands in as a proxy for the original variable, NEP, in a re-estimation of the equation under evaluation (i.e., the equation of Table 1, column 1).

The results using this instrumental variable are shown in the equation of Table 2, column 5, which also applies robust standard errors. [Note that, for comparability, NEP’ is scaled to a 0-1 interval.] The pattern of statistical significance persists, and model fit sustains itself (i.e., the pseudo-R-squared = 0.71, the percentage correctly predicted stands at 89.6 percent). Of specific interest is the economic coefficient itself, whose magnitude actually increases, to 1.22. These results reveal that the coefficient using the original uncorrected economics variable, NEP, contains bias. However, the bias is somewhat downward. When NEP is properly transformed, into an instrumental variable, NEP’, and the model re-estimated, the economic voting coefficient grows in magnitude.
Observe, finally, that it continues to dominate the effects from social class and religious practice. [A final note is that the effect of NEP’ seems even stronger when the variable is introduced into a multi-level estimation, where slopes are allowed to vary. We accept this slope estimate of 1.92, but cautiously, because of all the assumptions required. We do believe in its implication that the underlying population parameter for economic effects is greater than that suggested by employing an uninstrumented NEP. Support for the notion that this multi-level instrument retains some stability comes from observing that the ratio of the random component to the fixed component is roughly similar for NEP (at 0.49) and NEP’ (at 0.64).]

ASSESSING THE MAGNITUDE OF THE ECONOMIC VOTING EFFECT

In terms of the probability of an intended vote for the incumbent, how important are changing economic perceptions? Compare two groups of voters, who are alike in terms of social class, religiosity, left-right ideology (all scores set at their means). They differ only with regard to how they see the economy: one sees it as “the same” (score = 0.5), the other sees it as “better” (score = 1). Given these conditions, what is their probability increase in incumbent vote intention? Different scenarios are spelled out in Table 3. In the first column is the baseline model (without the past vote control), and using uncorrected NEP. We see that the probability increase in incumbent support for that group is 0.13. Contrast this to the third column and the estimate of 0.18, from the baseline model with past vote recall added, and using the instrumented NEP’. Other scenarios are possible, as Table 3 makes clear. In any case, changes in the probability
impact rendered by changes in perception of the economy are not small. Overall, there are serious economic effects on vote intention in these democracies, and they appear robust against multiple methodological and statistical challenges.

[TABLE 3 ABOUT HERE]

SUMMARY AND CONCLUSION

A classic Western European model of political behavior, vote = f (social cleavages, ideology, economics), provides the framework. Estimating that model against a massive dataset, in multiple ways, sheds considerable light on the “reality” of economic voting. For purposes of exposition, the first estimations follow traditional practice. These analyses yield the standard positive result: sociotropic retrospective economic evaluations produce statistically significant effects. Model estimation with this balanced and large pooled dataset, on ten Western European nations, achieves formidable statistical efficiency.

However, this work does not fully satisfy the concerns of some, who express doubts over the endogeneity of individual economic perceptions. To answer this question, we exogenized the sociotropic retrospective measure, employing the instrumental variables approach, including it finally in a multi-level context. From these statistically more desirable results, economic perceptions appear strongly shaped by the objective economy. Indeed, once the usual distortions and error are removed from them, by a proper exogeneity strategy, the economy appears to weigh even more heavily on vote intention. This conclusion is not unique. Instead, it has surfaced elsewhere, in other
investigations of the endogeneity problem and economic voting. [On the United States, the United Kingdom, and Canada, see Lewis-Beck, Nadeau, and Elias (2008); on Spain, see Fraile and Lewis-Beck (2010).] According to Lewis-Beck, Nadeau and Elias (2008, p.93), “with exogeneity conditions given full consideration, the strength of economic perception on the vote choice heightens further.” One implication is that the findings from conventional cross-sectional analyses of economic voting are biased, but in the unexpected downward direction.

Thus, the economy is not a mirage. Voters see it, and see it rather clearly, when they exercise their choice. We believe, with Duch and Stevenson (2008, p.92), that we have provided strong “evidence in favor of the conventional wisdom. Yes, there is economic voting in developed democracies.” But we take that conclusion one step further. Economic perceptions, properly understood, have a greater impact than previously imagined. To extend this proposition to other democracies, and other elections, more cross-national survey modeling needs to be done, always with careful attention to issues of economic measurement and estimation bias. [While our conclusion, then, supports the earlier work of Duch and Stevenson (2008), it departs from that of van der Brug et al. (2007). This departure is discussed further in the endnote.6]

We began our study by noting two things at stake in this investigation. The first we have amply addressed: the economic voting coefficient stands strong across this set of advanced democracies. What about the second – accountability? Are elected officials called to account for a poor economy? Our findings show that voters reward governments for good economic performance and punish them for bad. Thus, parties that want to win
re-election must deliver valued goods, rather than empty rhetoric. Otherwise, they risk being voted out in the time-honored fashion democracy makes possible.
References


Appendix

I. Data Sources

A. Political Data

The source for the political data is the European Election Study. This survey is a sample-based survey of the member states of the European Union. The number of countries surveyed varied from 12 (1988, 1994) to 24 (2004). The surveys were performed in October and November of 1988, November and December of 1994, and in June of 1999 and 2004. The 10 countries selected were member states of the European Union throughout the period. Belgium and Luxembourg were not included for reasons spelled out in the text. The sample for Germany in 1988 is West Germany and “unified” Germany thereafter. The individual country’s weighting schemes were applied throughout. The countries selected and the sample sizes are presented below. The variable “number of months after general elections” was computed from the European Journal of Political Research’s Political Data Yearbook (mean: 0.36; sd: 0.20). Detailed information about these surveys can be found at the European Election Studies (EES) website: http://www.ees-homepage.net/.
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B. Economic Data

The economic data for the different countries were taken from the OECD Statistics Directorate  
(http://www.oecd.org/department/0,3355,en_2649_33715_1_1_1_1_1,00.html) and the Statistical Office of the European Commission  
(http://epp.eurostat.ec.europa.eu/portal/page/portal/eurostat/home/).

GDP = Gross domestic product annual growth (mean: 3.4; sd: 1.8).

Inflation = Annual growth rate of the consumer price index (mean: 3.0; sd: 2.6).

Unemployment: Annual rate of unemployment (mean: 8.8; sd: 3.4).
II. Question Wordings

All variables were recoded on a 0-1 scale. The coding for the ideology, class, and church attendance variables are adjusted according to the ideology of incumbent governments (first multiplied by -1 then +1 added to it). For example, a score of 10 on the [original, 10 integer number points] ideology scale (extreme right) is coded 0 [on the operational 7-point scale] under leftist governments, and coded 1 [on the same operational 7-point scale] under rightist governments. The wordings for the different questions used in this study are as follows:

**Vote:** If there were a general election tomorrow, which party would you vote for?
Incumbent parties are coded 1, others are coded 0 (mean: 0.42; sd: 0.49).

**Past Vote Recall:** Which party did you vote for at the General Election of [Year]? Same coding as the vote variable (mean: 0.43; sd: 0.49).

**Ideology:** In political matters people talk about “the left” and “the right”. What is your position? Please indicate your views using any number on a 10-point scale. On this scale, where 1 means “left” and 10 means “right” which number best describes your position? (this original, 10 integer number points scale was first recoded into an operational 7-point scale by regrouping the center and the extreme categories: [1-2 = 0] [3 = 0.17] [4 = 0.33] [5-6 = 0.5] [7 = 0.67] [8 = 0.83] [9-10 = 1]) (mean of operational 7-point scale: 0.50; sd: 0.28).

**Class:** If you were asked to chose one of these five names for your social class, which would you say you belong to – the working class, the lower middle class, the middle class, the upper middle class or the upper class? (mean: 0.51; sd: 0.35).
Church attendance: How often do you attend religious services: several times a week, once a week, a few times a year, once a year or less, or never? (mean: 0.54; sd: 0.33).

National Economic Perceptions (NEP): What do you think about the economy?
Compared to 12 months ago, do you think that the general economic situation in this country is: a lot better, a little better, stayed the same, a little worse, or a lot worse? (1988, 1994, 2004). How about the state of the <Country> economy? Very satisfied, somewhat satisfied, somewhat dissatisfied, very dissatisfied? (1999). (Mean: 0.51; sd: 0.41.)

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
<th>Model 5</th>
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<td>3.00**</td>
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<td>(0.09)</td>
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¹Percentages correctly predicted for multilevel models were generated using the Expected Percentage Correctly Predicted (“epcp”) Stata plug-in developed by Christopher N. Lawrence.

**p≤0.01; *p≤0.05 (two-tailed tests). Country dummies are not shown. The dependent variable takes the value of 1 if respondents intend to vote for the sitting coalition in general election, 0 otherwise (don’t knows and refusals are excluded). Entries are unstandardized logistic regression coefficients, with standard errors in parentheses. See the Appendix for a description of the variables.

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1 Percentages correctly predicted for multilevel models were generated using the Expected Percentage Correctly Predicted (“epcp”) Stata plug-in developed by Christopher N. Lawrence.

**p ≤ 0.01; *p ≤ 0.05 (two-tailed tests). Country dummies are not shown. The dependent variable takes the value of 1 if respondents intend to vote for the sitting coalition in general election, 0 otherwise (don’t knows and refusals are excluded). Entries are unstandardized logistic regression coefficients, with standard errors in parentheses. See the Appendix for a description of the variables. NEP’ is the prediction from ordered logistic regression with NEP as the dependent variable (see endnote 5).


<table>
<thead>
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<th>Impact:</th>
<th>(1) Without past vote NEP</th>
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<th>(3) With past vote NEP’</th>
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<tr>
<td>Same to better</td>
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<td>Worse to better</td>
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<td>[0.24 - 0.27]</td>
<td>[0.17 - 0.21]</td>
<td>[0.28 - 0.41]</td>
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</table>

Entries are measures of the effects of changes for categories of NEP and NEP’ on the dependent variable (voting intention for or against a party in the sitting coalition), with 95% confidence intervals in square brackets. These effects were computed using the software CLARIFY.
Luxembourg was excluded because its national sample size was so much smaller than the other nations. Belgium was excluded because the vote intention question was not asked in 2004. In any case, Belgium would pose complications stemming from the linguistic divisions. [To deal with this problem of Belgium, van der Brug et al. (2007) divided Belgium in two and treated it as two countries. We preferred to avoid that strategy.]

Duch and Stevenson (2008, p.122), looking across their models, report “the typical pseudo-$R^2$ is about 0.35.” With respect to van der Brug et al. (2007, pp.84-88), they offer different adjusted R-squared (from OLS), across relevant models of increasingly elaborate specification. Their most naïve model, holding party preference to be based on “parties’ government or opposition status and on the state of the economy,” yields an adjusted R-squared of 0.03 (van der Brug et al., 2007, p.84). However, this increases to 0.32 when previous national vote is added (van der Brug et al., 2007, p.84). Under the fullest specification, with left-right ideology and social cleavages included, along with previous vote, the adjusted R-squared tops out at 0.46 (van der Brug et al., 2007, p.88). Unfortunately, neither team of researchers looks at prediction error measures of fit, so we can make no comparisons there.

We are indebted to Robert Erikson (2009) for his felicitous concept of economic perception as “mirage.” It should be made clear, however, that he himself does not prescribe to the notion that the economy is a mirage.

As noted, van der Brug et al. (2007) avoid using individual-level, survey measures of economic perceptions, because of concern over the endogeneity problem. Duch and
Stevenson (2008) voice the concern, but do not find it fatal. After testing, they report “that endogeneity in our measure of economic evaluations has surprisingly little influence on the patterns of contextual variation in the economic vote” (Duch & Stevenson, 2008, p.126). However, on the basis of the tests they employ, this conclusion may be too sanguine. They attempt to “purge” the reported economic evaluations of distortions that might be induced by the endogeneity problem, and then re-estimate a model using these purged economic evaluations. From that analysis, they conclude “despite persistent worries in the literature about the endogeneity of economic perceptions, the practical impact of this problem is relatively small” (Duch & Stevenson, 2008, p.124).

The fly in the ointment concerns the variables they used to purge the economic evaluations, namely partisanship, personal experiences, and socio-economic circumstances (Duch & Stevenson, 2008, pp.127-128). From the viewpoint of instrumental variables estimation, these variables used may not be truly exogenous. If they are not, then the new purged economic variable is itself not, leaving the bias present. Of particular concern here is the exogenous status of partisanship; one could argue that it is endogenous, being influenced by economic evaluations (just as economic evaluations might be influenced by partisanship); indeed, that is the crux of the endogeneity problem that needs solving. [For a full discussion of this dilemma, and the need for an instrumental variables strategy, see Lewis-Beck, Nadeau, and Elias (2008).]

5 For a classic treatment, see Kmenta (1997). In the first stage, available exogenous variables (i.e., three macro-level variables of economic growth, inflation, and unemployment; three micro-level variables of age, gender, and class; and country dummies) are regressed on NEP, and a predicted value (labeled NEP’) is obtained. That
is to say, there are both aggregate-level and individual-level exogenous variables selected for use in the construction of the instrument. Exogenous variables are determined outside the system, and cannot be influenced by an individual’s immediate vote preference. Sometimes, applied instrumental variables estimation mistakenly assumes, rather than establishes, the exogenous status of the variables used to create the instruments. However, if they are not truly exogenous, then they only make matters worse. [See Woolridge (2006, pp.525-540).] Note in particular that the individual-level exogenous ones are available for all countries, and are in common use this way in the literature (see, e.g., Duch et al., 2000, note 14, pp.645-646). The instrumental variable, NEP’, is thus constructed from the following ordered logistic regression equation:

\[
\text{NEP} = 0.30^{**} \text{GDP} - 0.12^{**} \text{Inflation} - 0.18^{**} \text{Unemployment Change} \\
- 0.37^{**} \text{Age} + 0.29^{**} \text{Gender} + 0.29^{**} \text{Class} \\
+ \text{Constant and Country Dummies (coefficients not shown)}. 
\]

LR chi-squared = 4307.11
Percent correctly predicted = 64.1%
Percent error reduction = 50%
N = 39,110

Observe that NEP’ is also a good instrument, in the sense that it predicts NEP well, according to the percent correctly predicted and the percent error reduction figures. [Note further that the coding of Class, unlike with the vote equation model, has not been adjusted for the ideological orientation of the sitting government because we wish here to
account for the fact that well-to-do persons tend to be more optimistic about the economy whatever the government in place. Note also that Unemployment Change is used here (rather than Unemployment Level) because, unlike with the aggregate model, there is not a collinearity problem. All other variables are measured as listed in the Appendix.]

The general reason the study at hand differs from the van der Brug et al. (2007) study is methodological. Below, key methodological differences are pointed out. First, this study has a larger sample, with more surveys included (a total of forty in all). Second, the measurement of the dependent variable differs. The dependent variable used here – an incumbent/opposition dichotomy – is widespread in the economic voting literature. In contrast, the dependent variable used by van der Brug et al. (2007) – based on differing probabilities of party support – does not occur elsewhere in the economic voting literature. Third, there are different measures of the economy. Van der Brug et al. (2007) employ macroeconomic indicators directly, avoiding perceptual economic measures all together. In contrast, this study employs perceptual economic measures, then properly exogenized perceptual economic variables (instrumental variables based partly on macroeconomic indicators). Fourth, the goodness-of-fit of the models at hand tend to surpass that of the van der Brug et al. (2007) models. That difference must be due in part to the estimation procedures selected. While this study draws mostly on logistic regression, the van der Brug et al. (2007) one relies on ordinary least squares (OLS) regression. Logistic, rather than ordinary, regression would seem essential for comparative voting models, with their categorical choice sets. These are highlights of the differences. The serious student of the topic must of course read these different studies carefully and with an even-hand, as we have tried to do.