Macro vs. Micro-Level Perspectives on Economic Voting: Is the Micro-Level Evidence Endogenously Induced?

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Abstract

Many of the findings regarding economic voting derive from the micro-level analyses of survey data, in which respondents' survey evaluations of the economy are shown to predict the vote. This paper investigates the causal nature of this relationship and argues that cross-sectional consistency between economic evaluations and vote choice is mainly if not entirely due to vote choice influencing the survey response. Moreover, the evidence suggest that apart from this endogenously induced partisan bias, almost all of the cross-sectional variation in survey evaluations of the economy is random noise rather than actual beliefs about economic conditions In surveys, the mean evaluations reflect the economic signal that predicts the aggregate vote. Following Kramer (1983), economic voting is best studied at the macro-level rather than the micro-level.

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The idea that economic conditions affect election outcomes is firmly embedded in the lore of electoral politics. In the case of US presidential elections, an overwhelming body of evidence indicates that the economy influences the vote. At the macro-level, objective measures of economic growth predict electoral support for the incumbent presidential party (Tufte, 1978, Hibbs, 1987; Fair, 2002; Lewis-Beck and Rice, 1992; Erikson, 1989). Macro-level measures of the subjective economy (e.g., the Index of Consumer Sentiment) also predict the vote (Erikson, MacKuen, and Stimson, 2002).[1] At the micro-level, the consensus of numerous survey analyses is that voters are influenced by their subjective views of the national economy, even though they are not much swayed by their personal economic standing (Fiorina, 1981; Kinder and Kiewiet, 1979; Kiewiet, 1983; Kinder, Adams, and Gronke, 1989).

It is micro-level research that provides most of the findings regarding the psychological mechanisms connecting the economy to the voter. Evidently, economic voting is "sociotropic," with voters responding to their beliefs about the state of the overall economy rather than to their personal pocketbooks. But doubts can be raised about what we can learn about economic voting from individual survey respondents.

[1] In extreme, it has been argued not just that the economy matters but that the economy is virtually all that matters, apart from incumbency, trumping other variables in the political environment (Fair, 2002). Within political science, a common interpretation is that political variables such as the candidates themselves and the quality of their campaigns are of little importance as voter preferences march toward the electoral outcome that is largely destined by the state of the national economy. (See Gelman and King, 1993).

Micro-level evidence regarding economic voting must be regarded with some suspicion, because of the dubious assumption that evaluations of the economy are exogenous to vote choice. To the extent that the presumptive dependent variable—vote choice—also influences survey responses about the state of the economy, the evidence for economic voting is biased and exaggerated.

The most notable critique of micro-level analysis of economic voting is Kramer (1983). Kramer reminds the reader of two difficulties with micro-level research on economic voting.

- First, to the extent people might vote according to the state of their personal pocketbook, they should consider only government-induced changes to their economic fortunes. Since only a small portion of personal income is attributable to government actions, peoples' reports of net change in personal fortune present a distortion of the small impact of government on their lives. Because survey reports of net income change are poor measures of government-induced change, the effect of government-induced change may be underestimated by surveys.
- Second, Kramer reminds us that cross-sectional variation in perceptions of the economy represent variation in survey responses regarding perceptions of a constant. When these variations in perceptual error correlate with respondents' reported vote decisions, the strong possibility exists that vote preferences influence economic perceptions. When this happens, survey evidence of economic voting becomes biased upward.

Kramer offers the radical advice that for an understanding of economic voting, political scientists should abandon the potentially misleading micro-level analysis of survey responses and instead pursue the study of economic effects at the macro-level. In short, he turns the usual advice on its head—to understand economic voting, aggregate data is preferred to individual-level analysis.

Two decades after Kramer's warning, the seriousness of the endogeneity problem remains uncertain. In particular how much does the purported survey-based evidence of economic voting actually represent people voting based on their economic evaluations and how much does it represent respondents offering evaluations based on their vote choice? Research since Kramer reinforces the warning. Conover, Feldman, and Knight's (1986. 1987) papers on the public's understanding of economics describe economic citizens who, while surprisingly ignorant of economic facts, do show some knowledge of economic trends (especially regarding unemployment) and who are influenced by their partisan predispositions. More recent research by Hetherington (1996) and Duch, Palmer, and Anderson (2000) confirm that partisanship affects economic evaluations. A related stream of research finds that partisan economic evaluations can be primed by placement of the economic question after a battery of political items (Sears and Lau, 1982; Lau, Sears, and Jessor, 1990; Palmer and Duch, 2001). In recent years, the endogeneity debate has even been extended to the macro-level as some scholars (e.g., Norpoth, 1996; Freeman, Hauser, Kellstedt, and Williams, 1998; de Boef and Kellstedt, n.d.) suggest that macro-level political attitudes affect macro-level economic attitudes—e.g., that presidential approval affects economic expectations as well as the other way around.

The present paper is intended as a thorough empirical evaluation of Kramer' warning. By applying some simple assumptions and simple tests of the data, this paper argues that cross-sectional variation in respondent's reported perceptions of national economic conditions are largely random noise that has no bearing on political evaluations. At the same time, some variance in these responses is endogenously induced, with vote choice influencing survey responses regarding the economy. Furthermore, the problem is so severe that virtually all of the observed statistical relationship between respondent perception of the economy and vote choice is due to vote choice influencing perceptions and not the other way around. Nonetheless, the aggregate perceptions of various cross-sections capture the economic signals and present a meaningful time series of differential economic evaluations that help account for the presidential vote.

The main focus is on survey respondents' assessment of the current economy—more specifically, perceptions of the current economy compared to a year ago. This focus on the subjective economy revealed by survey responses (rather than the objective economy) allows the movement back and forth between the macro- and micro-levels of analysis. In some fashion, the micro-level data must "add up" to account for the macro-level data.

The Data

Starting in 1980, the National Election studies have asked, with only slight variation,[2] the following question about current economic conditions.

[2] In 1984, the NES presented the middle choice as "'about' the same." In 2000 it rotated the order of "gotten better" and "gotten worse." For our 1980-1996 period, the National Election Studies also include measures of the respondent's current personal economic circumstances and the respondent's expectations

"*Would you say that over the past year the nation's economy has gotten better, stayed the same or gotten worse?"*

This question is very similar but not identical to the question measuring current economic conditions in the University of Michigan's Consumer Sentiment monthly surveys. :

"*Would you say that current business conditions are better or worse than they were a year ago?*"

The differences are two-fold. The NES asks about "the nation's economy" whereas the Consumer Sentiment item asks about "business conditions." And only the NES item invites the response of "the same," although volunteered responses of "same" conditions (typically about 10 percent) are recorded in the Consumer Sentiment series.

As Figure 1 shows, despite the mild disparity in question wording, the two series track each other in the aggregate: The biennial NES measure of the perceived "economy" tracks the Consumer Sentiment measure of "current business conditions." Each is an average of three possible evaluations: that the economy (business) has gotten better (+), stayed the same (neutral) or gotten worse (-) over the previous year. Figure 1 norms the two measures on the ICS's familiar 200 point scale where 0 is all negative, 100 is neutral and 200 is all positive. This is analogous to individual survey responses varying on a scale from -1 (worse) to 0 (same) to +1 (better). One point on the former aggregate scale is equivalent to a one percent difference on the individual-level scale.

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about the economy and expectations about their personal pocketbook, with questions roughly analogous to those in the Consumer Sentiment surveys. The NES included measures of respondents' personal economic circumstances going back to 1968. In 1972 and 1976, NES included questions about perceptions of the current economy that were closer to the Consumer Sentiment items but not directly comparable to the 1980-2000 item that is the subject of attention here. The 1976 items lack comparability in another respect, that they are asked in the post-election survey rather than the pre-election interview.

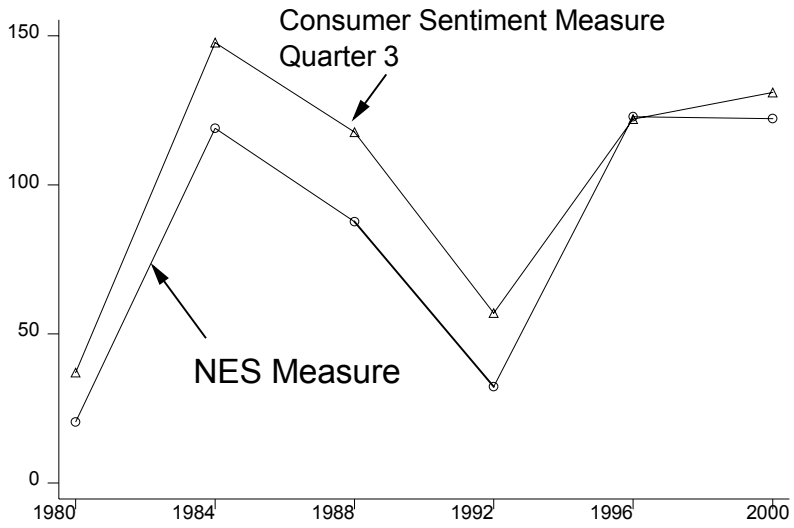

Figure 1. *Means of Economic Perceptions, NES and Consumer Sentiment.*

For the survey analysis of the following sections, it is convenient to scale respondent estimates of economic conditions as -1 (worse) to 0 (same) to +1 better rather than on the Consumer Sentiment 200 point index. Using this -1 to +1 scale, we can conduct a mini-time series analysis of the relationship between the mean responses of voters in the NES samples and their mean vote choices. Over our six presidential elections, the coefficient predicting the survey respondents' mean vote choice (on a 0-1 scale) from their mean perception of the economy is 0.14. The relationship is shown graphically in Figure 2. This estimate of the macro-level effect becomes relevant in the analysis that follows.

The analysis that follows examines the individual-level relationship between the NES item regarding current economic conditions and the reported vote, for six presidential elections, 1980-2000. In each of the six NES surveys, the economic conditions item is presented to respondents during the pre-election survey, while the

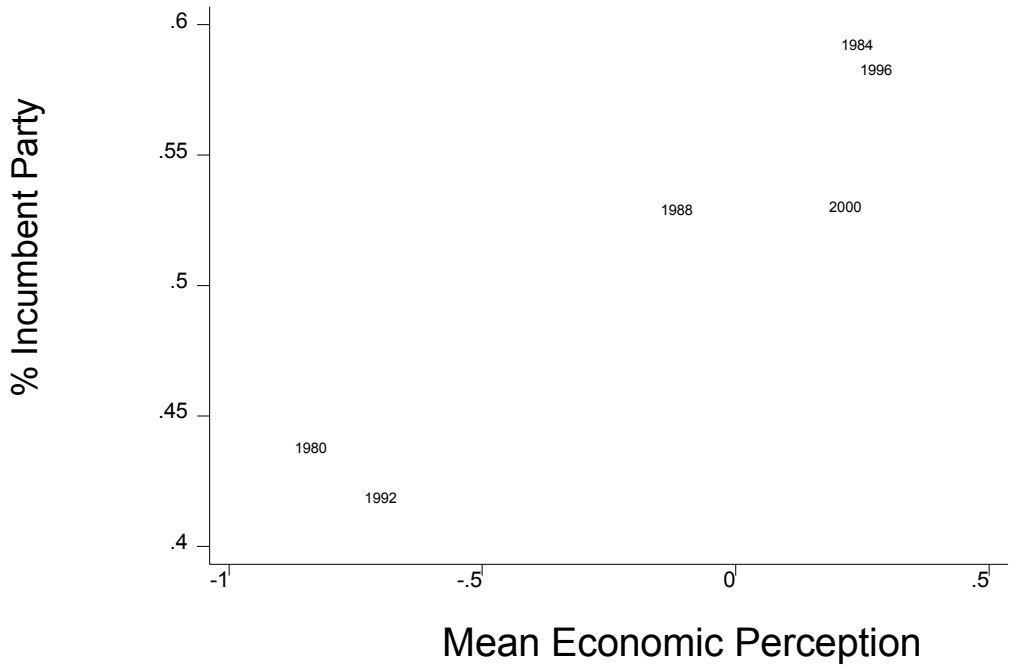

Fig. 2. *NES Mean Incumbent Party Vote by Mean Perception of Economic Conditions.*

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reported vote is naturally from the post-election survey. Although the "vote" question is not asked in the same survey wave as the economic conditions item, the pre-election survey contains a heavy dose of political items that respondents must realize are designed to tap their partisan sentiment and vote intention. The question is, how serious is the problem of respondents adjusting their verbal perceptions of the economy to fit their revealed vote intentions?

Economic Perceptions and the Vote, as seen with Micro-Level Data

Recall that for the survey analysis, respondent estimates of economic conditions are scaled as -1 (worse) to zero (same) to +1 better. Table 1 presents some probit equations predicting the vote from economic perceptions plus dummy variables for the

Table 1. Presidential Vote for Incumbent Party by Economic Perceptions, 1980-2000 NES (Probit coefficients)

	(1) ***4 Predictors***	**(2)** ***Dropping Pocketbook Expectations***	**(3)** ***National Economy Only***	**(4)** ***Retrospective Only***
Current Economic Conditions	0.51 (0.03)	0.53 (0.03)	0.58 (0.03)	0.60 (0.03)
Economic Expectations	0.23 (0.03)	0.23 (0.03)	0.24 (0.03)	
Current Personal Conditions	0.18 (0.02)	0.18 (0.02)		0.20 (0.02)
Personal Expectations	0.03 (0.03)	--		
Pseudo R Squared	.11	.11	.10	.10
N	(6300)	(6459)	(6492)	(6828)

Note: Year effects not shown.

specific election years. In four equations shown in four columns, perceptions of the current economy trump all other economic perceptions as predictors of the incumbent-party vote. Seemingly, the respondent's perception of the current economy is more electorally relevant than recent changes in the respondent's pocketbook, the respondent's expectations about future pocketbook conditions, and the respondent's expectation about change in the nation's economy.

Table 2 shows the relationship between perceptions of the current economy and the incumbent-party vote in greater detail. With some variation from election to election, it shows a considerable difference between the vote choice of those who say the economy is improving and those who see it deteriorating. Averaged over elections, this gap is 43 points while averaged over respondents and controlling for the election it is 48 percent. In 3 of the 6 elections (1984, 1992, and 1996) the gap was 50 percentage points or more.

Table 2. Presidential Vote for Incumbent Party by Perceptions of Economy, 1980-2000

Perception of Economy	*1980*	*1984*	*1988*	*1992*	*1996*	*2000*
Improving	58% (33)	80% (646)	77% (249)	82% (66)	76% (454)	69% (437)
Same	71% (102)	53% (413)	53% (568)	62% (314)	46% (138)	45% (517)
Worse	39% (732)	21% (282)	35% (348)	32% (968)	35% (136)	33% (160)

Note: Incumbent party vote = Percent of Two-Party Vote for President Party Candidate.

A natural but possibly incautious imagery is of a fluid electorate with voters readily switching sides with the economic winds. A plausible causal inference from Table 2 would be that small shifts in economic perceptions generating major shifts in the vote. The corollary would be that vote choices must be rather fluid from election to election to accommodate the churning of economic perceptions from one election to the next. But such projection would be wrong. As we will see, while conceding that the economy influences the aggregate vote, a straightforward reading of the individual-level relationships shown in Table 2 exaggerates the influence of the economy both for individuals and projected to the aggregate.[3]

As researchers have long been aware, evidence of the sort shown in Tables 1 and 2 is subject to simultaneity bias. The association between economic evaluations and vote choice can be fueled not only by economic evaluations causing votes, but also by

[3] It may seem surprising that this discussion is couched entirely within the context of a bivariate relationship between economic perceptions and the vote without resort to possible "control" variables like party identification and the like. To simplify the discussion, we can avoid consideration of controls because other plausible predictors of the vote are not plausible causes of economic perceptions except indirectly by affecting the vote choice which may in turn feed economic perceptions. In short, we need not worry that the relationship between economic perceptions is spurious, due to both variables being caused by some other exogenous variables.

vote choices influencing the survey responses that comprise the observed evaluations. For instance, in-party and out-party partisans could hold sincere and different attitudes about the state of the economy. But it is not required that vote choices systematically distort peoples' attitudes regarding the economy away from the objective economic facts as if people regularly see the world with partisan blinders. This bias can also arise due to short-term rationalization as respondents find it easy to answer vague survey questions with responses that show consistency with their partisan behavior. Partisans of the presidential party, for instance, might choose to avoid the discomfort of revealing that they plan to vote for the party presiding over an objectively bad economy by telling their interviewers that the economy is actually okay.[4]

How bad is this problem? Multivariate controls for variables like party identification can shrink the coefficients for the economy in the vote equation but will not make them disappear. Attempting to solve the problem by imposing two-stage least squares (e.g., Kinder, Adams, and Gronke, 1989) are hampered by the lack of good instruments. Imposing instrumental variables for vote choice in the equation explaining economic perceptions suggest that the impact of the vote on economic perceptions is considerable (Wlezien, Franklin, and Twiggs. 1997). The degree of bias in estimating economic effects on the vote with surveys remains unclear.

Survey respondents vary in their reported perceptions of the economy that they all share. But are these variations real? Are they mere random measurement error? Are they tinged by partisanship? If response variation is random error in the independent

[4] Some might see such behavior as instances of the partisanship content of the survey "priming" respondents to alter their short-term attitudes about the economy. But the artificial nature of such "primed" effects suggest that it would be misleading to consider these survey responses as changes in actual attitude.

variable, the imputed economic effect is actually underestimated. If response variation is induced by partisan choice—the intended dependent variable—the imputed economic effect is biased upward.

A Micro-Level Model

Economic perceptions are measured on a three-point scale. Vote choice in our treatment is a dichotomy: for the in-party or the out-party. Although neither economic perceptions nor the vote are measured as interval-level variables, it is helpful for the discussion to treat them as if they were. In other words, let us suspend belief and model the observed variables as if they are interval in nature. It is proposed that the gain from this indulgence outweighs the possibility of major mistakes of inference due to methodological laxity.

Consider the variable we will call X_{it} the ith survey respondent's economic perception of the economy at time t. Our starter model is:

$X_{it} = E_t + u_{it}$.

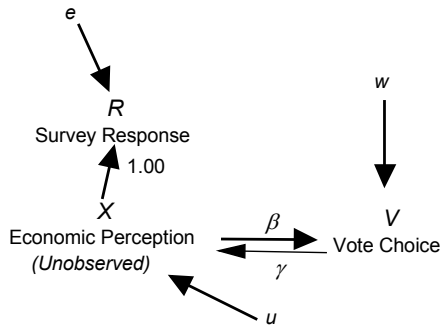
where E_t is the economic signal at time t, shared by all and u_{it} is a random disturbance with zero mean. By this model, economic perceptions will be a function of the actual economy (a signal shared by all) plus idiosyncratic variance in perceptions of the signal. Although the variation in X_{it} is random, it is real in the sense that this variation affects vote choices. An interesting question is whether the variance of X_{it} is large or small. As this variance approaches zero, finding evidence of economic voting using individual level data becomes impossible. Another interesting question is the depiction of u_{it} as a time series. Is it long-memoried? If so, people observe the economy as long-term optimists or pessimists. These tendencies would presumably influence the vote; e.g., people who

persist in the view that the economy is rosy would be a regular source of support for the incumbent party.

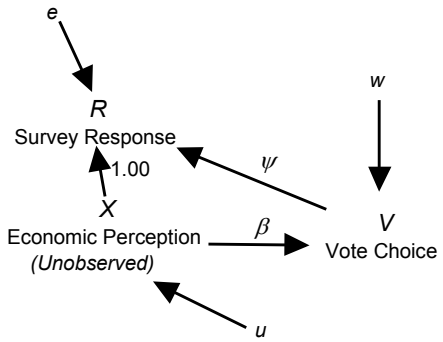
The variable X_{it} is not observed. Instead we observe the survey response, R_{it}, which is a function of X_{it}, the respondent's perception of the economic signal, plus an error term e_{it}. This error is the random noise of measurement error. Introducing e_{it} serves to distinguish the survey response R_{it} from the latent perception, X_{it} As we will see, an interesting matter is the variance of e_{it}. When it is large, many respondents could be answering randomly or guessing haphazardly regarding economic conditions. The difference between u_{it} and e_{it} is that the latter is an ephemeral response that does not affect votes. When the variance in e_{it} is large, especially relative to u_{it}—the variation in underlying latent perceptions—the statistical relationship between true perceptions and the vote becomes attenuated.

Our central interest is on the effect of the respondent's latent perception of the economy, X_{it} on the respondent's vote. Let us call this causal parameter β. A further problem beyond measurement error is that the vote choice might sway peoples' actual latent perceptions of the economy. We signify this causal feedback by the parameter γ. Or, in a problem limited to survey responses but not actual attitudes, respondents might succumb to an urge to display consistency between their vote choice and economic assessments, nudging their reported economic assessments (R_{it}) into greater agreement with their vote choice. We signify this rationalization by the parameter ψ.

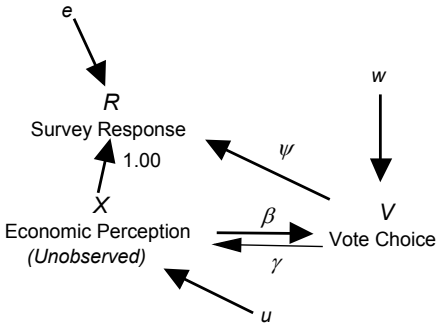
To put these parts together, Figure 3 depicts the causal model underlying our discussion. The Figure presents three versions of the model for a single election year. (At this point we drop the *i* and *t* subscripts) In Model A, vote choices are allowed to



Model A. Vote Affects Economic Evaluations



Model B. Vote Affects Survey Responses



Model C. Vote Affects Economic Evaluations and Survey Responses.

Fig. 3. *Modeling Economic Perceptions and the Vote.*

influence economic perceptions in a feedback loop, so that economic optimism can influence voting for the incumbent which fuels further optimism, etc. In model B, vote choices do not influence actual attitudes but they do influence survey responses, as respondents rationalize their reports of economic conditions to conform to their partisan preferences. Both models also allow for a random error component to reported subjective perceptions. Of course the components of both models might be present. The two models are presented separately for ease of exposition. The truth may be hybrid Model C where the causal parameters for the vote influencing both perceptions and survey responses are nonzero.

These models help us understand the sources of bias when one estimates economic effects using OLS or other methods (e.g., probit, logit) that ignore problems of endogeneity and measurement. With model A, the OLS estimate of the effect of economic evaluations on the vote is presumably a function of both the economy-on-vote effect plus the vote-on-economy effect, with no further source of covariation (e.g., the zero covariance restriction). The zero-covariance restriction (here, no correlation between the disturbances of *V* and *X*) allows the following expectation: [5]

$$\hat{\beta}_{\mathbf{OLS}} = \left(\frac{\sigma_X^2}{\sigma_R^2}\right)\frac{\left[\beta + \left(\frac{\sigma_V^2}{\sigma_X^2}\right)\gamma\right]}{(1+\beta\gamma)} \qquad \text{Equation 1}$$

[5] The zero covariance restriction is a useful form of identification in simultaneous equation systems, discussed briefly in many econometric tests. Two useful sources are Hausman and Taylor (1983) and Hausman, Newey, and Taylor (1987). The specification of Equation 1 follows from the fact that when the covariance of two variables is a function solely of the two variables' effects on each other, the correlation equals the sum of the two standardized effects divided by one plus their product. See Erikson, (1982) and Erikson and Palfrey (1998) for more on the algebra of the derivation. For earlier discussions, see Rothenberg (1972, chapters 4, 5) and Heise (1975; pp. 181-182). In the absence of measurement error, estimation of one causal parameter allows one to solve for the second parameter.

where the variable and parameter labels are as signified in Figure 3. By model A, the OLS estimate of the economy-on-vote effect is roughly a composite of the true effect of X on V (β) plus the reciprocal effect of V on X (γ), all attenuated by any measurement error in X. For Model B, where vote choice influences responses rather than attitudes, the statistical challenge is technically not simultaneity exactly.. Rather, the statistical challenge is best described as measurement error in the independent variable confounded by an effect of the dependent variable on the independent variable's proxy indicator. .. Here, the expectations for the OLS estimate are straightforward. Again assuming no source of XV covariance from outside the model, we get:

$$\hat{\beta}_{\mathbf{OLS}} = \left(\frac{\sigma_X^2}{\sigma_R^2}\right)\beta + \left(\frac{\sigma_V^2}{\sigma_R^2}\right)\psi \qquad \text{Equation 2}$$

Equation 2 has two components. The first represents the true effect (β), possibly attenuated by measurement error. The second represents the effect of the vote on the survey response (ψ). Measurement error in economic perceptions biases the estimate downward. Simultaneity bias pushes the estimate upward.

We can of course produce a composite model (Model C) whereby vote choice influences both the respondents' true perception (via γ) and the respondent's rationalization for the interviewer (ψ). The resultant OLS estimate of β becomes:

$$\hat{\beta}_{\mathbf{OLS}} = \left(\frac{\sigma_X^2}{\sigma_R^2}\right)\frac{\left[\beta + \left(\frac{\sigma_V^2}{\sigma_X^2}\right)\gamma\right]}{\left(1+\beta\gamma\right)} + \left(\frac{\sigma_V^2}{\sigma_R^2}\right)\psi \qquad \text{Equation 3}$$

Equation 3 reveals two potential sources of bias in the survey estimate of economic voting. One is measurement error. Noise in the survey responses (the intended

independent variable) will attenuate the OLS coefficients representing the effect of economic perceptions and the vote. The second is simultaneity. To the extent vote choice influences survey responses (and perhaps the latent, unmeasured "true" perceptions of respondents), this process inflates the OLS coefficients for economic voting. From Equation 3, the clear goal is to gain an understanding of the parameters γ, ψ and σ_X^2.

Our micro-level expectations are anchored, by the macro-level estimate of the economic effect. Recall that the coefficient predicting the aggregate vote from economic perceptions is 0.14. A working assumption is that the macro-level effect is a function of simple composition—that the macro-level effect in an election year is the sum of the responses to individual-level perceptions of the economy. Thus, with no measurement or simultaneity problems, the micro-level parameter β should be 0.14, matching the macro-level estimate.

The question remains, how can we go beyond suspicion to empirical test? Are we able to verify the presence of a serious endogeniety problem regarding the relationship between the vote and survey respondents' reports regarding economic conditions? Or can we certify perceptions of economic conditions as exogenous measures with a clean bill of health?

The current paper offers a four-pronged strategy. First, it explores the option of controlling for other causes of the vote to see if this makes the relationship between perceived conditions and the vote disappear. Second, it attempts an accounting of the macro-micro connection, asking whether the micro-level results plausibly add up to create the evident macro-level connection. Third, the paper engages in some

simultaneous equation modeling. And fourth, it examines the panel of NES respondents who were interviewed during both the 1992 and 1996 campaigns.

Imposing controls

When we examine the effect of one variable (*X)* on another (Y) while suspecting that *Y* might also affect *X*, there is a natural tendency to control for other causes of *Y* to see if the purported *X*-on-*Y* effect survives the challenge. The problem of course is that even if control variable *Z* causes the coefficient for *X* to decline, the new coefficient will still be biased if an endogeniety problem exists. Further unmeasured causes of *Y* will continue to cause *X*, biasing the *X*-on-*Y* effect estimate.

Still, we can explore what happens. Table 3 presents some probit equations regarding the "effect" of perceived economic conditions on the vote for both president (1980, 1984, 1988, 1992, 1996, 2000) and midterm elections for the House of Representatives (1982, 1986, 1990, 1994, 1998). The first column shows the "effect" of current economic conditions on the incumbent-party presidential vote, with no controls other than year effects. The second column shows the equation where respondent party identification is added as a right-hand-side variable. The coefficient goes down, but is still highly significant. Added controls for party or candidate "thermometer" scores (not shown) do not make the economic effect go away. Even if it did, we would have a different problem sorting out the effects, as variables like party identification and candidate or party thermometer scores can themselves be suspected to pose endogeneity problems.

Table 3. Probit Equation Predicting Vote from Perceived Economic Conditions, Controlling for Party Identification

	Presidential Vote		*Midterm Congressional Vote*	
Perceived Economic Conditions	0.64 (0.03)	0.40 (0.03)	0.20 (0.03)	0.02 (0.03)
Party ID (Pres. Party)	--	0.56 (0.01)	--	0.38 (0.01)
Pseudo R Sq.	.09	.48	.01	.27
N	(6863)	(6840)	(3937)	(3934)

Note: Year Effects Not Shown.

The third and fourth columns of Table 3, however, show that controlling for added variables can sometimes be revealing. Adding the simple control for respondent party identification is sufficient for the once significant relationship between congressional voting and economic perceptions to disappear. Congressional elections arguably are not much affected by the state of the economy, especially at midterm (Erikson, 1990; Erikson, MacKuen, and Stimson, 2002). If so, perceptions of the national economy should have little bearing on the congressional midterm vote. The simple control for party identification confirms this. People tend to give economic responses consistent with their partisanship (thus the false positive in column 3), but a control for party identification makes it go away (column 4).

A difference between the presidential and congressional analyses in Table 3 is that the congressional vote choice is only one of many indicators (and perhaps a minor one) of the respondent's partisanship at midterm. Midterm respondents are not likely to rationalize their economic perception by their congressional vote alone.[6] In presidential

[6] The micro-level evidence for the null hypothesis in the case of congressional elections is for illustrative purposes only. Based on the analysis ahead, it is possible that while partisan bias can be controlled in the

years, the presidential choice is the major choice. Although the respondents' economic perceptions are asked in the pre-election survey and vote choice is ascertained after the election, the pre-election surveys center on presidential choice. It is conceivable that these responses regarding the national economy are influenced by causes of the vote beyond party identification. For instance, even though pure "Independents" divide in their vote in a way consistent with their verbalized economic evaluations, it is plausible that vote choice influences the reported economic perception rather than the other way around.

The Micro-Macro Connection

We have seen both micro-level and macro-level evidence for economic voting in the six elections, 1980-2000. In principle, the macro result is an exact function of the micro-data that comprise its foundation. What are the accounting rules? Tables 4 and 5 below present various forms of a pooled time-series cross-section equation predicting individual votes, where the economy is divided into its between-year and within-year components. The between-year component is simply the election year mean perception of the economy. The within-year component is the residual perception of the economy, or the respondent perception minus the mean perception for the year. As an algebraically equivalent setup, we can measure the individual response as the observed perception rather than as a deviation from the mean. The aggregate measure indicates the signal of a common economy that varies by year but not by individual within year. The individual-level measure indicates the presumed variation in the received signal within the year (variation in perception of a constant).

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case of midterm elections, the cross-sectional variance in true perceptions of the economy is so small relative to the response error that the estimate of a real effect attenuates toward zero.

What should we expect? The simplest case would be straightforward composition effects, where perceptions of the economy are exogenous, measured without error, and uncorrelated with other causes of the vote. Voters who see the economy as improving would vote in roughly identical proportions for the in-party no matter which election is observed. Similarly, voters who see the economy as declining would vote at a roughly constant and lower level of support for the in-party in al elections. Meanwhile, voters who see the economy as the "same" would persistently show a roughly middling level of support for the in-party.

If composition effects were to account for the macro-level relationship, we would see the result in the pooled equation as follows. With mean perceptions and residual perceptions on the right side, the two coefficients would be identical. With mean perceptions and observed individual perceptions on the right hand side, the individual-level coefficient would dominate. Its magnitude would be identical to the time-series macro-level coefficient, while the parameter for mean perceptions would approach zero. With macro level economic effects fully accounted for by the composition of perceptions for the election year, there would be no added room in the equation for mean perceptions to explain vote choices.

Composition effects are not the only possible state of affairs. A second possibility is for the micro-level (cross-sectional) estimates of the economic effect to vanish at the expense of a dominant macro-level (time series) effect. One instance would be if all economic effects were contextual rather than operating via individual perceptions. For instance, it could be that a good economy stimulates the media to present favorable news about the incumbent party, which cause voters to support the

incumbent party independent of their individual perceptions about the economy. More likely, individuals are responding to the signal of the real economy, but with individual variation in perceptions of the economy so corrupted by measurement error that the individual differences in survey responses fail to predict differences in reported perceptions. We know in advance, that minimal within-year "effects" are not likely, due to the large within-year "effects" observed in Tables 1, 2, and 3. However, following equation 1, measurement error can attenuate the coefficient estimate even as the coefficient is expanded by the reverse effect of vote choice on economic perceptions.

This leads to a third possibility—that the estimation of a vote-on-perceptions effect generates a larger micro-level coefficient than found in the aggregate. Consider the case when votes cause perceptions. With mean perceptions and residual perceptions on the right hand side, the parameter for residual perceptions would exceed the parameter for mean perceptions, while the presumptive effect of mean perceptions would turn negative. This would be as if for voters within every category of survey response about the economy, the better the mean estimate of the economy, the fewer votes for the incumbent party.

This third possibility would be a strong symptom of the reverse effect of partisan choice on economic perception. It is not plausible that the true effect of economic perceptions would be larger cross-sectionally when the variation in perceptions is due to different views of a constant economy, than time-serially. A surplus "effect" at the micro-level indicates that the micro-level estimate is biased upward by voter rationalization.

Table 4. OLS Regression Predicting Incumbent Party Presidential Vote (0,1) from Economic Perceptions

	(1) *Micro With Year Effects*	(2) *Macro & Micro*	(3) *Macro & Residual Micro*
Observed Economic Perceptions	0.24 (0.01)	0.24 (0.01)	--
Mean Economic Perceptions	--	-0.10 (0.02)	0.14 (0.01)
Residual Economic Perceptions (OBSERVED MINUS MEAN)	--	--	0.24 (0.01)
YEAR EFFECTS (2000=Base)			
1980	0.16		
1984	0.06		
1988	0.08		
1992	0.11		
1996	0.04		
Constant	0.47	0.53	0.53
Adjusted R sq.	.121	.120	.120

N=6863. Note: OLS is inappropriate. Standard errors for mean economic perceptions aree inflated.

Tables 4 and 5 present the results of the pooled analysis. Table 4 presents the results as a set of OLS regression equations. While technically inappropriate for a binary dependent variable, these equations are relatively easy to interpret. Table 5 presents the equivalent results in terms of probit. [7]

In each table, column 1 presents an equation predicting the vote from year effects (dummy variables) plus observed economic perceptions. In the OLS equation of Table 4, the parameter estimate for observed economic perceptions is 0.24, roughly consistent

[7] In these equations, the individual rather than the election year is the unit of analysis. By treating each respondent as a separate case, the estimated "standard error" for the mean economy is underestimated compared to its value when the number of cases is 6 (years). In a multi-level model with fixed year effects, the standard error for mean economic evaluations doubles from 0.01 to 0.02.

Table 5. Probit Equations Predicting Incumbent Party Presidential Vote from Economic Perceptions

	(1) *Micro With Year Effects*	(2) *Macro & Micro*	(3) *Macro & Residual Micro*
Observed Economic Perceptions	0.64 (0.02)	0.64 (0.02)	--
Mean Economic Perceptions	--	-0.27 (0.04)	0.37 (0.04)
Residual Economic Perceptions (OBSERVED MINUS MEAN)	--	--	0.64 (0.02)
YEAR EFFECTS (2000=Base)			
1980	0.44		
1984	0.16		
1988	0.22		
1992	0.30		
1996	0.11		
Constant	-0.09	0.08	0.08
Pseudo R sq.	.092	.090	.090

N=6863. Standard errors for mean economic perceptions are inflated.

with Table 2.[8] The second column shows the effect of one unit of cross-sectional perception when year dummies are replaced by a single variable representing mean economic evaluations. Table 5's OLS parameter estimate of 0.24 is unchanged from column 1. Interestingly, with individual perceptions in the equation, the effect of mean perceptions becomes negative, as if aggregate perceptions of prosperity work against the incumbent in order to offset the "effect" of cross-sectional differences in perception.

One way to understand this is that the original 0.14 macro-level effect is less than the cross-section effect (0.24) so that the aggregate effect must be -0.10 to make the

[8] In Table 1, the average effect per election year is 21.5 percent of the vote associated with one unit of perception. In Table 2, column 1, the average effect per respondent is a .24 difference in the proportion of the vote per one unit of perception.

accounting add up. Still another perspective is column 3, which shows the same equation as column 2, but with perceptions measured as the residual from the mean perception for the election year. Now we see the time-serial effect to be 0.14 as in the binary time-series equation while the cross-sectional effect is 0.24 as always.

The magnitude of the evident effect of a unit of cross-sectional variation in the perception of a constant economy exceeds by about two-thirds the estimated time series effect representing changing responses to real changes in the economy. This should disturb one's belief that the cross-sectional analysis is free of bias. But how serious is this bias?

Figure 4 provides further clues. This figure shows the reported incumbent-party vote for each of the three categories of economic perception as a function of the mean perception for the election year. We see that for all elections—across all levels of mean economic perception—the incumbent-party vote for those who see the economy as declining stays constant at about 33 percent. No matter what the other circumstances of the election, only one voter in three who sees the economy in decline will vote for the incumbent party. At the other extreme, we see that except for 1980, the incumbent party vote for those who see the economy as improving stays constant at about 75 percent. Regardless of the circumstances (perhaps 1980 excepted), three voters in 4 who see the economy as growing will vote for the presidential party.

Left to consider are the voters in the middle, those who report that the economy has stayed the "same." This is the one set for whom the vote depends on aggregate perceptions of the economy. But the direction of the relationship is not what one might think. The more positive the aggregate perception of the economy, the more negative

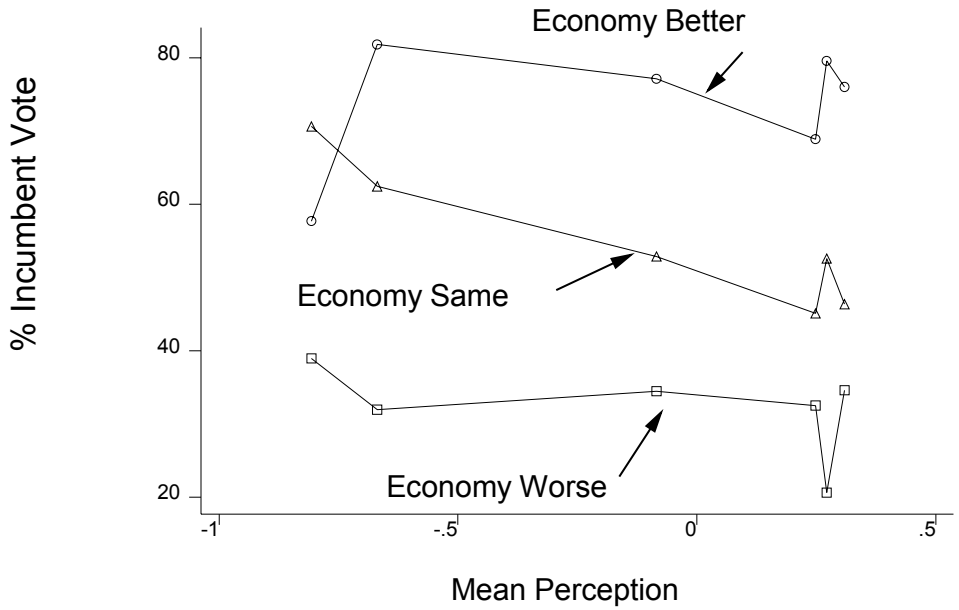

Figure 4. Incumbent Vote by Mean Economic Perception, Holding Constant Individual Perceptions

toward the in-party is the vote of those who see the economy to be the "same" as the previous year. Moreover, the macro-level relationship (six elections, six cases) is very strong, with a correlation coefficient of -.95. What does this mean?

One interpretation is that the response option that the economy has stayed "the same" provides a useful parking place for respondents who might be voting inconsistent with their true perceptions of the economy (for the incumbent, seeing a bad economy or against the incumbent, seeing a good economy). For instance, by asserting that the economy is actually about the same, G.H.W.Bush voters in 1992 could rationalize their vote in the face of an objectively poor economy. Similarly, by asserting that the economy is actually about the same, Mondale voters in 1984 could rationalize their vote in the face of an objectively good economy. Meanwhile, if some voters who go against the grain with their vote act this way, so too will a lesser number of voters going with the

grain who for some reason misperceive the economy. For instance, in 1984 a subset of Reagan voters reported the economy as poor. We can surmise that there were also a number of Reagan voters who were inclined to report the economy as poor but rationalized their Reagan vote by saying it was "the same."

The question we explore further is whether these distortions were severe enough to compromise individual level evidence of economic voting. We must keep in mind that while partisan rationalization inflates the coefficient for economic voting, measurement error depresses it. In the most severe case, all seeming evidence of economic voting could be rationalization: By this possibility, voters observe a constant economy with little true variance in what they objectively see. Meanwhile, the inevitable measurement error generates responses that are not entirely random. They are influenced by vote choice. The result is a false-positive estimate of economic voting. For illustrative purposes, Table 6 shows the relationship between economic perceptions and the vote with economic perceptions now comprising the presumptive dependent variable.

Simultaneous Equation Modeling

Can we resort to simultaneous equation techniques like two-stage least squares to solve our estimation problem? Recall the causal model of Figure 3. To directly estimate the β parameter for economic voting, we would need some instruments that predict economic perceptions but do not directly affect the vote. Despite some attempts in the literature, for example using personal pocketbook considerations as a key instrument (Kinder et al, 1989), no convincing instruments for economic perceptions are available in the US context. However, consider the reverse effect of the vote on the survey response. Label this net effect α, where (in the notation of Figure 3),

Table 6. OLS Regression Predicting Micro-Level Economic Perceptions from Presidential Vote

	(1) *With Year Effects*	(2) *Mean Economic Perceptions Only*	(3) *Presidential Vote Only*	(4) *Macro Only*
Presidential Vote ('In' Pty =1, 'Out' Pty = 0)	0.44 (0.02)	--	0.54 (0.01)	0.44 (0.02)
Mean Economic Perceptions	--	1.00	--	0.94 (0.02)
YEAR EFFECTS (2000=Base)		--		
1980	-1.01			
1984	-0.00			
1988	-0.33			
1992	-0.87			
1996	0.04			
Constant	0.02	0.02	-0.39	-0.23
Adjusted R sq.	.380	.020	.145	.380

N=6863. Dependent Variable =-1 if "Worse," 0 if "Same" and 1 if "Better"

$\alpha = \gamma + \psi.$

Parameter α is a composite of any direct effect of the vote on respondent perceptions (γ in Figure 3) plus the effect of the vote on the survey response as rationalization (ψ). While there are no good instruments for economic perceptions, there is no shortage of instruments for the vote that would allow the estimate of the less interesting effect of the vote on economic perceptions. Thus, we can estimate the reverse vote-on-perceptions effect (α) using two-stage least squares, exploiting the many instruments for vote choice that presumably do not directly affect economic perceptions. For example, if being Catholic pushes one to vote Democratic, it is not likely that Catholics see the economy differently from others except via the possible path that being Catholic makes one Democratic, which leads to a pro-Democratic perception of the economy.

The value of estimating α is that it allows the estimation of β, the parameter of interest. If we assume the covariance between the two variables is caused only by the reciprocal effects of each variable on the other, we have what is known in the econometric literature as the zero-covariance restriction. Knowing one effect via the use of instrumental variables allows one to calculate the reverse effect which actually might be more interesting. (See, especially, Hausman and Taylor, 1982; Hausman, Newey, and Taylor, 1987). The key is the assumption that the disturbances to the vote and perceptions (*V* and *R* in Figure 3) are uncorrelated. This means simply that there are no omitted variables causing both variables For instance, anticipating one possible result, if most of the observed covariance between vote choice and economic perceptions can be accounted for α, by the reverse effect (estimated via two-stage least squares), there is little room left over for the effect of interest, from the economy to the vote, to do its work.

In the first stage (not shown), the instruments include measures of race, education, income, sex, age, religious denomination, and region.[9] Separate results are obtained for each of the six election years, and shown in Table 7.[10] The table presents OLS and 2SLS estimates of the effect of vote choice on economic evaluations, side by side. In general, the 2SLS estimates are actually higher than the OLS estimates. The one

[9] Exact variables are dummies for black, no high school degree, college graduate, poor, rich, female, young, old, Catholic, Jewish, None or Other Religion, black, and southern white. Base categories are nonblack, some college, middle income, male, middle age, Protestant, and not a southern white.
[10] For all equations of Table 7 and elsewhere, economic evaluations are measured on the three-point scale of better-same-worse, ignoring responses to the follow-up question of whether the economy has gotten "much" or "somewhat" better (worse). The pattern of Table 7 is replicated using the 5-point measure.

Table 7. OLS vs. 2SLS Estimates of Effect of Vote on Perception of Economic Conditions.

	OLS	*2SLS*
1980 (*N*=867)	0.17 (0.03)	0.19 (0.08)
1984 (*N*=1341)	0.74 (0.04)	1.16 (0.10)
1988 (*N*=1165)	0.43 (0.04)	0.61 (0.10)
1992 (*N*=1348)	0.36 (0.03)	0.43 (0.07)
1996 (*N*=1028)	0.45 (0.04)	0.09 (0.10)
2000 (*N*=1114)	0.37 (0.04)	0.46 (0.11)
Mean	*0.42*	*0.49*

Instruments = Dummies for rich, poor, no high school, college graduate, Catholic, Jewish, No or Other Religion, Black, Woman, young, old.

exception is 1996, where the 2SLS estimate is quite small.[11] Even with the 1996 anomaly, on average, the two methodologies produce virtually identical results.[12]

The implications are interesting and compelling. The effect of the vote on the economy is sufficient to account for virtually all of the covariance between the vote and economic perceptions. To deny this fact requires faulting the instrumental variable model. Further, for certain instruments to be creating mischief with the estimates, it would require that apart from indirectly via vote choice, certain demographic groups are optimists under one party's administration but not the other's for reasons other than their electoral attachments. A pattern whereby different demographic groups see the economy

[11] The 1996 coefficient is restored somewhat by allowing income and education to have independent effects on economic perceptions. For other years, allowing income and education to directly affect the vote had little impact on the 2SLS estimates. Perhaps in 1996 and 1996 only, perceptions of the net economy were distorted by one's place on the economic ladder.

[12] I repeat the analysis of Table 7 substituting the respondent's pre-election preference for post-election report of vote choice. The estimates of α are slightly higher (expected, since the economic questions are in the same survey wave) and, as for Table 7, consistently higher using 2SLS than OLS except for 1996.

differently is what drives the results, but this is presumably because they are influenced by their vote choice.

The estimated effect of the vote on survey responses regarding economic retrospections is just as high (actually a bit higher) using 2SLS as OLS. Normally when OLS and 2SLS provide the same result, the conclusion is that no simultaneity bias exists, and that the OLS estimate is the most sufficient. If we assume no simultaneity bias, the reverse effect of economic perceptions on the vote would seem to be zero. Yet that cannot be exactly right, because otherwise how do we account for the aggregate relationship? The complication is the degree of error in the measure of economic perceptions and whether there exist much meaningful cross-sectional variation at all.[13] We return to this problem in the section after next.

Panel Data

As a final test, we can examine the relationships between economic perceptions and the vote in the 1992-1996 NES panel. We can compare how the 1992 and 1996 elections are predicted from economic responses in one election campaign and then the other. For instance, we can see how 1992 vote choices are related to 1996 as well as 1992 economic perceptions. Are 1996 perceptions of the economy already ordained by the respondents' vote choices in 1992? And we can see how 1996 vote choices are related to economic perceptions in the two election years. Are 1996 choices as predictable from 1992 choices as from 1996? A twist is that positive economic perceptions predict Republican voting in

[13] If it were not for measurement error, one could estimate β from 2SLS-estimated α and the OLS estimate of β. We could do so substituting numbers from Equation 3. (The unknown variance of X makes this impossible.) An equivalent method would be to treat residual R as an instrument. That is, first estimate α via 2SLS using the instruments employed for Table 7. Second, from this equation compute residual R. Third, perform a 2SLS equation predicting the vote from R, using residual R as the instrument while including the instruments for V. See Hausman, Newey, and Taylor, 1987, p. 854.

1992 (with GHW Bush as president) but Democratic voting in 1996 (with Clinton as president).

The various relationships are shown in Tables 8-11. As an approximation, the economic perceptions in the two election years are interchangeable as predictors of vote choice in the two election years. This is clear evidence that vote choices influence perceptions.[14]

Table 12 shows 2SLS estimates of both the presidential vote equation, where the vote is a simple function of the lagged vote and current economy, and the economic perception equation, where perceptions are a function of the current vote and lagged perceptions. Because the data do not conform strictly to the assumptions of interval level data, these results can only be taken as illustrative. Still, the results are consistent with the findings so far. The vote affects evaluations, but not the other way around.

As a final presentation, Table 13 shows the pattern of shifts of economic perceptions by respondents who shifted from Democratic to Republican or the reverse with their presidential vote, 1992-1996. If economic voting was responsible for vote shifts *or* if the reverse effect were true, we would see that vote shifts from Republican to Democratic would be accompanied by positive economic perceptions in both elections while vote shifts from Democratic to Republican would be accompanied by negative economic perceptions both times. The table shows a faint trace of such a pattern, but based on very few cases. The remarkable fact about Table 13 is the rarity of party switchers. A scant 11 percent of major party voters in the two presidential elections switched their vote from one party to the other.

[14] Similar patters are found in the NES panel study over 1972-1976, when a different question-wording was employed about retrospective evaluations. Substituting vote choice or retrospective evaluations in one year for the other makes only a modest difference in the size of the economic perception-by-vote cross-tabs

Table 8. 1992 Presidential Vote by 1992 Perception of the Economy

	1992 Perception		
1992 Vote	*Improving*	*Same*	*Worse*
GHW Bush	100%	64%	35%
Clinton	0%	36%	65%
	(19)	(92)	(276)

Table 9. 1992 Presidential Vote by 1996 Perception of the Economy

	1996 Perception		
1992 Vote	*Improving*	*Same*	*Worse*
GHW Bush	31%	56%	67%
Clinton	69%	44%	33%
	(183)	(158)	(49)

Table 10. 1996 Presidential Vote by 1996 Perception of the Economy

	1996 Perception		
1996 Vote	*Improving*	*Same*	*Worse*
Clinton	75%	46%	23%
Dole	25%	54%	77%
	(179)	(156)	(48)

Table 11. 1996 Presidential Vote by 1992 Perception of the Economy

	1992 Perception		
1996 Vote	*Improving*	*Same*	*Worse*
Clinton	12.5%	42%	65%
Dole	87.5%	58%	35%
	(16)	(98)	(265)

Table 12. OLS and 2SLS Equations Predicting 1996 Presidential Vote and 1996 Economic Perceptions from Each Other, 1992-1996 NES Panel Data.

Dependent Var.=1996 Pres. Vote (1=Clinton, 0=Dole)		
	OLS	*2SLS*
Pres. Vote 1992 (1=Clinton,0=Bush)	0.73 (0.04)	0.79 (0.05)
Economic Perceptions, 1996	0.10 (0.03)	-0.04 (0.08)
Constant	0.15 (0.03)	0.17 (0.03)
Adj. R Squared	.629	.595
N	(311)	(304)

Instruments for Economic Perceptions 1996 = Economic Perceptions in 1992 and 1994.
Note: OLS and 2SLS are inappropriate for binary dependent variables.

Dependent Variable=1996 Economic Perceptions		
	OLS	*2SLS*
Pres. Vote 1996 (1=Clinton,0=Dole)	0.45 (0.06)	0.44 (0.10)
Economic Perceptions, 1992	0.04 (0.06)	0.04 (0.06)
Economic Perceptions, 1994	0.34 (0.04)	0.32 (0.05)
Constant	0.03 (0.05)	0.06 (0.06)
Adj. R Squared	.591	.586
N	(376)	(304)

Instrument for 1996 Preidential. Vote=1992 Presidential Vote.

Table 13. Vote Shifts (Dem. to Rep. or Rep. to Dem.) by Change in Perception of Economy, 1992-1996

	Change in Perception of Economy								
	Worse, 1992			*Same, 1992*			*Better, 1992*		
.	Worse 1996	Same 1996	Better 1996	Worse 1996	Same 1996	Better 1996	Worse 1996	Same 1996	Better 1996
Dem. to Rep.	2	4	4	0	0	0	0	0	0
Rep. to Dem.	1	4	8	0	4	4	0	0	2

Switchers=33
Stand-Patters= 273

Putting the Parts Together

Here is what we know. If we estimate the perception-on-vote effect at the macro-level, we obtain an estimate of 0.14. A working assumption would be that this aggregate relationship represents the sum of individual effects. That is, ignoring complications from non-interval data, among those moving from one category to the next (e.g., saying the economy stayed the same to saying it was improving), about one seventh will shift their vote accordingly. But when we estimate this effect with OLS using micro-level data, we obtain an estimate of 0.24, which is too large by the amount 0.10. The suspicion was that this inflated coefficient was due to respondent vote choices influencing their responses about the economy. In fact, when we tested the reverse hypothesis of a vote-on-perception effect, the OLS and 2SLS coefficients were similar, suggesting no simultaneity problem and thus, seemingly no actual effect of economic perceptions on the vote. But how could the cross-sectional estimate be zero while the aggregate estimate is not? We need to deal with one further complication, the measurement error in economic perceptions.

Let us distinguish again the difference between variation in true economic perceptions and the error. True perceptions represent real, latent (unobserved) perceptions that impact peoples' vote and that people hold independent of the interview situation. Error represents the detritus from the slippage between the true perception and the observed survey response. When the vote affects the survey response, the effect can be a feedback effect on true perceptions, an effect that persists independent of the survey context (γ). Or it can be an effect on the survey response, an effect that is temporary without any feedback on the vote (ψ). We cannot directly assess the degree to which the

net effect α represents components γ and ψ. But it makes a difference in the interpretation.

Consider again Figure 3 and Equation 3. Our interest is in β, the effect of (true) economic perceptions (X) on the vote. We can set it to its macro-level value (0.14) and contrast that with its estimated (via OLS) value (0.24). We know the variances of the vote (V) and observed perceptions of the economy (R).[15] We also have an estimate of α but not its division into γ and ψ. Unknown is the variance of X, true perceptions. With this information, we have sufficient information for at least a rough estimate of the variance of latent economic perceptions.

To see this, Equation 4 restates Equation 3, this time substituting observed or estimated numerical values for all but the variance of X plus parameters γ and ψ, where γ and ψ sum to about 0.49 based on the 2SLS estimate of their composite, α.

$$\hat{\beta}_{\mathbf{OLS}} = \left(\frac{\sigma_X^2}{\sigma_R^2}\right)\frac{\left[\beta + \left(\frac{\sigma_V^2}{\sigma_X^2}\right)\gamma\right]}{(1+\beta\gamma)} + \left(\frac{\sigma_V^2}{\sigma_R^2}\right)\psi$$

$$0.24 = \frac{\sigma_X^2}{0.45}\frac{\left[0.14 + \left(\frac{0.25}{\sigma_X^2}\right)\gamma\right]}{(1+0.14\gamma)} + \left(\frac{0.25}{0.45}\right)\psi \qquad \text{Equation 4}$$

$$= \left(\frac{\sigma_X^2}{0.45}\right)\left(\frac{0.14}{1+0.14\gamma}\right) + \left(\frac{0.25}{0.45}\right)\left[\left(\frac{\gamma}{1+0.14\gamma}\right) + \psi\right]$$

One can now solve for σ_X^2, the variance of X, given alternative combinations of γ and ψ that sum to 0.49. It turns out that no matter what values one substitutes, the

[15] All measures of the micro-level variance are within-year averages.

estimated variance of X is negative, which we can interpret to be zero.[16] The intuition is that for the variance of latent perceptions to be positive, the estimated effect of the vote on perceptions should be smaller (0.49), or the OLS estimate of β (0.24) should be larger.

The negative estimate suggests that taking the mean of annual estimates (0.49) may be too high an estimate of α. Nudging the estimate of α slightly downward to the neighborhood of about 0.44 gives a cleaner result. If $\alpha = \psi$, then the estimated variance of X is zero. If $\alpha = \gamma$, the variance is positive, but only enough to accommodate the variance due to partisan influence (V on X). The exogenous sources of X are in either case non-existent.

In some respects, the details of this analysis are pushed to their limit, so it is not wise to press for an exact estimate of how much the observed variance in economic perceptions represents true underlying attitudes about the economy that could impact the vote. The best guess, however, is about zero. The burden of proof is on those who analyze cross-sectional survey responses about economic conditions to show that they represent anything beyond noise and partisan distortion.

Discussion

Most survey analyses of economic voting treat cross-sectional variation in economic perceptions as accurate representations of the respondent's belief about the economy. The notion that this response variation is real receives bolstering by the fact that these responses predict vote choice—as if people who report an objectively good economy to be bad actually vote against the incumbent due to a delusion that the

[16] To see this, substitute zero for the variance of X in Equation 4, plug in any positive values for γ and ψ that sum to 0.49 and note that the left hand side of the equation is slightly larger than the right hand side.

economy is poor. Should we take survey respondent reports regarding economic conditions at face value as their economic beliefs?

The analysis of this paper suggests that the answer is decidedly "no." Cross-sectional variation in perceptions of the economy is little more than measurement error plus partisan bias. Given this characterization, survey responses of economic perceptions should not be allowed to serve as independent variables in analyses of survey respondents' vote. This is not to say, however, that voters (respondents) are uninfluenced by the economy. And it does not suggest voters are incapacitated regarding evaluations of the economy. Rather, it must be the case that voters who respond to the economy see the economy through essentially identical lenses. For instance, with a thriving economy during the campaign, voters who take the economy into account presumably are capable of observing that the economy is strong. To the extent they let this observation influence their vote, the result would be uniformly positive toward the incumbent rather than against.

Survey responses regarding the economy do vary, but this variation is not a cause of vote choice. At any one time, voters hold differing perceptions of the thresholds separating better, same, and worse. Placement of these thresholds might even vary for the same individual over short periods of time. And responses are decidedly influenced by the partisan context of political surveys, yielding the illusion that survey responses cause vote choice. Finally, even though economic voters can observe the economy correctly, not all voters have the national economy on their political radar screen. When asked questions about the economy, respondents who ignore the economy will provide responses that are essentially random.

The verdict that variation in survey responses within a given election survey lack substantive content may seem harsh. But let us consider the example of perceptions of change in personal family income, a variable that presumably varies as a function of objective family income growth. To see if the former correlates with the latter, I measured the actual income growth among panelists, 1994-1996, as the growth rate in family income reported in the two panel waves.[17] The same panelists were asked in 1996 for their perception of the change in their families' income over the previous year, with the usual choice of the three categories better, same, or worse. Although observed income growth is measured over two years and respondents are asked to recall their family's income growth over only the previous year, actual income change over two years should account for something around half the variance in the recall of family income over one year. But instead, recalled income growth and measured income growth correlate at only .03, which is decidedly nonsignificant even with 520 cases. Even allowing for the suspicion that respondents often report their income inaccurately, this statistic is astounding. Survey respondents' recollections of their family's recent economic fortunes have virtually no relationship to their family's actual income growth.

This is important side evidence. But what does it mean? Should it be taken as one more bit of evidence that voters are morons, this time that they cannot even remember their own family's recent economic history? More likely it is the case that voters "know" their family's economic history well enough but that they vary in their translations of this knowledge into categories of better, same, or worse. The difficulty in translation is sufficient that measurement error swamps the objective facts when

[17] In the 1994 and 1996 panel waves, respondents were offered a rich set of 24 income categories to choose from. For each interval of income (e.g., $20,000-21,000), I scored income at the midpoint. Respondents in the top income category (>$105,000) were arbitrarily scored at $125,000.

accounting for recalled family income change.[18] Recognition that individual-level assessments of family income change are next to worthless as a measure of objective income change should make it easier to accept that individual variation in reported perceptions of the current economy (a constant) represent essentially error, distorted further by partisan bias.

A seeming paradox is that on the one hand, the variance in survey responses about economic conditions contain mainly noise while on the other hand, true perceptions of the economy are fairly uniform. Many respondents, while ignoring the economy as a basis for their vote, will still offer survey responses to the interviewer. Others do consider the economy for their vote and do hold some reasonable information about the national economic direction, even if their survey responses are not all in the same survey categories Respondents who voice incorrect evaluations of the economy in surveys are almost certainly not casting their votes based on their survey responses, even though these responses are consistent with their vote choices. Meanwhile, voters who do take the economy into account when they vote should readily find sufficient information during the campaign to correctly asses the economy's direction.[19]

What can be salvaged from cross-sectional information about voter beliefs of the economy is not the individual variation, but the mean. The variance is noise. The election-year mean is the signal. Following Kramer (1983), the objective economy provides the signal that guides individual responses, aggregated to the mean.

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[18] Reported family income fares poorly as a predictor of the vote, but this seeming failure of the "pocketbook voting" hypothesis might be an artifact of measurement error that is not (unlike perceived business conditions) distorted by partisanship. The pocketbook voting hypothesis might not be getting a fair shake at the individual level.

[19] On this latter point, see Erikson, MacKuen, and Stimson, Chapter 3.

Survey Noise, Partisan Distortion, and the Macro-Economy

If Kramer's warning is validated for micro-level analysis, is it safe to conclude that macro-level analysis of economic voting should be spared? If micro-level analysis of economic voting is contaminated by measurement error and partisan distortion, is the same true for macro-level data? Of course we limit this concern to macro-level analysis of the subjective economy based on survey responses, and not objective indicators.[20] But the concern extends to beyond the voting literature to consider economic influences on presidential approval or party identification in the US context, or government approval of various kinds in the comparative context.

Macro-level analysis of survey data aggregates over individuals. This simple fact allows the concern about measurement error at the individual level to be not a concern at the macro-level. Errors cancel out at the individual level. But what about partisan influence, such as the impact of the vote on economic evaluations? If the partisan influence on the survey response consists is simply the influence on the short-term survey response rather than underlying latent attitudes, then there is no macro-level problem—as long as the survey upon which the macro-level analysis is based is not laced with provocative political content as well as economic content. The problem would lie with influences of politics on latent economic evaluations that exist independent of the survey context. We could imagine, for instance, observing a macro-level correlation between mean economic perceptions and the vote where the relationship is distorted because supporters of popular candidates see the economy consistent with their favored candidate's position. Should we worry?

[20] However some might claim that partisan or political influence on economic attitudes spurs economic activity, as if (for example) a popular president can generate not only the belief but also the actuality of prosperity.

We can return to equation 3 and consider the macro-level version, shown in Equation 5.[21] Taking our substantive example, suppose we worry that our estimate of β is biased due to the influence of aggregate vote choice on aggregate perceptions. Equation 5 plugs in the values, where β is now treated as unknown and 0.14 is our macro-level OLS estimate.

$$\hat{\beta}_{\mathbf{OLS}} = \frac{\left[\beta + \left(\frac{\sigma_V^2}{\sigma_X^2}\right)\gamma\right]}{(1+\beta\gamma)} \qquad \text{Equation 5}$$

$$0.14 = \frac{\left[\beta + \left(\frac{0.005}{0.25}\right)0.49\right]}{(1+0.49\beta)}$$

We plug in 0.49, as our (possibly high) estimate of the effect of the vote on perceptions. We also plug in the macro-level variances of the vote and perception, drawn from the macro-version of our micro-data, with 6 cases for 6 election years. Note that whereas the ratio of the variances of the vote and perceptions is 0.55 at the macro-level (see Equation 4), it is .a mere 0.021 at the macro level. Given our assumptions, the OLS estimate of β=0.14 is off slightly. It corresponds to a slightly lower "true" value of only 0.131. The 0.14 estimate inflates the true value by 6 percent.

The slight contamination in this exercise assumes the worst—that the impact of vote choice on the vote affects latent attitudes and not just the survey response. Even so, in this example, the distortion is slight. If the vote affects economic attitudes as it might according to the micro-level analysis, the bias in the estimate of economic voting is negligible. The macro-level β estimate is not attenuated by measurement error as it is at the individual level. Moreover, the ratio of the variances of the vote and economic

[21] This is Equation 1 without the measurement-error term.

perceptions is much lower at the macro level. When moving from the micro to the macro level, the variances of the variables decline, with the decline far greater in the vote than in economic perceptions.[22]

One way to understand why the variances matter is to consider that the variances affect how "large" the two effects (*X* on *V, V* on *X*) are relative to each other, for example when the relative metric is that of "standardized" rather than unstandardized effects. If the posited effects .131 and .49 are standardized according to the individual standard deviations, they convert to 0.10 and 0.66, respectively. With the units normed so that both variables are measured in (micro-level) standard deviation units, the vote on perceptions effect appears to loom large over the more interesting perceptions-on-vote effect. But when we standardized in terms of macro-level standard deviations, the tables turn. The perception-on-vote effect is a massive 0.98 and the reverse vote-on-perceptions effect is a paltry 0.07.

A reasonable conjecture is that the shift in variances from the micro-to-the macro level generalizes to other variables. In general, economic variables vary more in relation to political variables at the aggregate level than at the individual level. As a result, any possible reverse effect from the political to the economic offers little distortion of the economics-on-politics effect at the aggregate level.

Conclusions

This paper confirms Kramer's diagnosis that estimations of the effect of economic perceptions on the vote are highly contaminated when conducted at the micro-level where the unit of analysis is the survey respondent. In presidential election surveys,

[22] This discussion ignores the possibility that aggregate change can be different for different voter groups. See Duch, Palmer, and Anderson (2000) for a critique of macro-level analysis based on heterogeneous responses.

respondents' reports of their economic evaluations represent entirely or almost entirely random error plus some bias based on their presidential choice. What holds is the aggregate signal represented by the mean economic perception. Even if partisan bias is at its worst, this bias distorts the mean evaluations only to a slight degree.

An important substantive consideration is how one views the psychology of the individual voter. In an earlier era, led by the influential non-attitudes argument of Converse (1964), scholars began to view citizens as devoid of attention to policy issues and the nuances of ideological debate. A reasonable interpretation is that voters do not respond as a function of the issue positions they offer in surveys, but rather to positions on underlying latent issue dimensions that are difficult to measure.

The same scrutiny should apply to economic voting. From poring over survey crosstabs, one can become misled to believe that voters hold heterogeneous economic perceptions and are easily swayed to vote according to the latest economic news. According to the present analysis, survey variation in responses about the economy may be all noise and partisan bias, otherwise devoid of content. But this is to say that in the cross-section, latent attitudes about the economy are uniformly shared. Voters (or at least some of them) do assess the economy, with little variation in latent perceptions of the common signal. And their votes respond to these economic assessments, a fact we know because of the macro-level evidence. But the individual-level effect is considerably smaller than reading the cross-tabs would have one believe.

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