

Presidential Voting and the Local Economy

Evidence from Two Population-based Datasets

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Abstract:

We show that standard economic measures based on samples and richer newly available ones based on populations lead to different conclusions about democratic accountability. Previous research, which has primarily relied on sample-based measures, has mostly missed an important determinant of presidential election outcomes: the local economy. We detect the local economy's impact with two unique datasets, one of which includes data on all consumer loans made in California and the other a census of businesses. In contrast to measures subject to sampling error, these population-based measures indicate that economic conditions at the zip code and county level have an impact on presidential election outcomes. Presidents therefore face incentives to focus on electorally important geographic regions.

Keywords: presidential election, economy, retrospective voting, democratic accountability

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When American voters evaluate presidential performance, a large body of evidence suggests that they focus on national economic performance (Kramer 1971; Markus 1988), making it arguably the single most important factor in determining presidential election outcomes (e.g., Erikson 1989; Fair 1978; Hibbs 1987; Lewis-Beck and Stegmaier 2000; Tufte 1978; Zaller 2004). In this paper, we seek to answer a related question that has received much less attention: Do local economic conditions substantially influence presidential election outcomes?

The answer to this question has important implications for democratic accountability. If voters respond to local economic conditions, then presidents face incentives to focus on growth in politically pivotal regions rather than on broadly shared prosperity. For example, presidential candidates treat federal corn subsidies as untouchable in part due to Iowa's importance in the presidential nomination process (e.g., Krauss 2011). In a political system where some regions have greater electoral importance than others, voter responsiveness to local conditions will incentivize presidents to pursue policies that will likely help the economy in those areas but may reduce the welfare of citizens overall (Kriner and Reeves 2015).¹

Despite these implications for presidents' incentives and despite abundant examples of presidents appearing to respond accordingly, few papers have considered the impact of the local economy on presidential elections. The few that have considered it generally find the local economy to have at most a small impact. The earliest papers to look at presidential voting and

¹ This idea accords with research finding that presidents may direct spending particularly strongly to swing states (e.g., Kriner and Reeves 2013), since voters appear to reward presidents for that spending.

the local economy were by Gosnell and his co-authors, who examined the influence of county economic and social factors on presidential vote share in the late 1920s and 1930s in Iowa (Gosnell and Pearson 1941) and Pennsylvania (Gosnell and Colman 1940), finding weak evidence for relationships. Later work has found small effects of economic changes in the U.S. (Eisenberg and Ketcham 2004; Hill, Herron, and Lewis 2010; Kim, Elliott, and Wang 2003; Kriner and Reeves 2012; Wright 2012) and in other countries (Auberger and Dubois 2005; Elinder 2010; Johnston and Pattie 2001; Leigh 2005; Veiga and Veiga 2010).² Other studies have considered the influence of the local economy on perceptions of national conditions.³ For example, Books and Prysby (1999) found no such effect. Rogers (2014) found a small effect of the local unemployment rate on perceptions, but none for the change in the unemployment rate. Altogether, existing work generally suggests at most a small role for the local economy in presidential election outcomes.

Earlier research, however, could have missed the local economy's influence on voting because of poor measures. In the U.S., many economic measures are based on sample surveys that measure national or even state conditions accurately, but contain sampling error when used to estimate local conditions, and especially the change in those conditions. This noise can cause a county-level unemployment change to deviate from the truth by several percentage points.

² Studies have also failed to find relationships between changes in local economic conditions and congressional voting (e.g., Owens and Olson 1980). Several papers do find that higher *levels* of unemployment favor Democrats (Rees et al. 1962; Wright 2012), but these results may be vulnerable to omitted variable explanations.

³ Hansford and Gomez (2015) use these local conditions to instrument for national perceptions, implicitly assuming that local conditions do not have their own direct impact on elections.

Random measurement error will generally bias regression coefficients towards zero (Angrist and Pischke 2009). Indeed, Hausman (2001) refers to the downwards bias from random error as the “Iron Law of Econometrics.”⁴ Noisy estimates of local economic conditions could therefore render undetectable their true effect on election outcomes. Moreover, the null effects produced by measurement error could account for the comparatively few papers on the local economy, given publication bias against null results (e.g., Gerber and Malhotra 2008).

The Great Recession provides an example. Between November 2007 and November 2008, U.S. unemployment increased from 4.7% to 6.8%. At the national level, voters appeared to punish the Republican Party for the economic downturn, shifting substantially to the Democrats in the 2008 presidential vote as compared to 2004. At the local level, however, they apparently did not. The counties hardest hit by the recession—according to unemployment or income measures—exhibited little tendency to be more likely to vote Democratic, as Figure 1 shows for unemployment. The top part of the figure presents the relationship between Democratic vote share and the growth in county-level unemployment for all counties; the bottom shows it just for California counties. Unemployment is from the Local Area Unemployment Statistics program of the Bureau of Labor Statistics (BLS), which produces county-level estimates using a model that is based on data from the Current Population Survey (CPS) and other datasets. Nationwide, the figure suggests that local unemployment growth in the year before the 2008 election had little

⁴ Researchers have known about the downwards bias, or attenuation bias, that measurement error causes in regression estimates since Adcock (1878).

impact on vote totals, with counties shifting an average of five percentage points towards the Democrats regardless of changes in unemployment.⁵

Should we really believe that rising unemployment in 2008 failed to hurt the Republican ticket at the local level? This is the conclusion implied by these data and by previous studies in the U.S. These local economic measures, however, may be too poorly measured because of sampling error to uncover even a relatively strong relationship. As we explain below, this is primarily true when studying the effects of changes in economic measures, as doing so amplifies the noise and reduces the signal.

To determine whether voters respond to the local economy, we consider two population-based measures. First, we use zip-code level credit bureau data on all consumer loans in California—a 100 percent sample—to examine loan delinquencies, including those for mortgages, leading up to the 2008 election. We find strong evidence that voters hold the president's party accountable for local economic conditions as measured by delinquencies. The California communities hardest hit by the recession, as measured by delinquency rates across types of loans, shift against the Republican ticket in 2008 by about 6-10 percentage points more than we would otherwise expect. These results survive a host of robustness checks.

To determine whether these results generalize, we consider a second population-based dataset that has received little attention from voting researchers: the Quarterly Census of Employment and Wages (QCEW). Those data provide measures—without sampling error—of total wages and employment at the county level that we utilize to determine the impact of the local economy nationwide from 1992 through 2012. With these data, we find effects of similar

⁵ Using county-level median income growth rates instead of unemployment produces similar results.

magnitude for the entire country over those six elections as we found for California zip codes in 2008. The results suggest that local economic conditions have been influencing presidential elections consistently over time and nationwide, with those effects previously rendered undetectable.⁶

An alternative interpretation of these findings is that the population-based measures of the local economy—loan delinquency and QCEW data—predict vote because they capture a different aspect of the economy, one that the sample-based measures miss. Indeed, loan delinquency and employment and wage data have rarely appeared in retrospective economic voting papers. Although we cannot rule it out, several findings point to sampling error, including that these measures all capture similar aspects of the economy and all predict presidential vote equally well at the national level. Moreover, we show that the government's own reported error in its local economic measures would lead to a large downwards bias when they are used in a standard economic voting regression. We then confirm with simulations that our results with population-based measures would not be visible in the kinds of sample-based economic datasets that have ordinarily been available to researchers. Nevertheless, it remains possible that these population-based measures are capturing a different aspect of the economy at the local level. Regardless of the mechanism, our findings show that the local economy matters.

⁶ Evidence on the mechanism underlying voters' responses to local economic conditions due to pocketbook concerns or through making inferences about the national economy is contested (Ansolabehere, Meredith, and Snowberg 2011; Kramer 1983), though research in other countries with richer data is making progress (Christiansen, Sønderskov, and Dinesen 2013). We provide some suggestive evidence on the mechanism in the Supporting Materials (SM), where we utilize survey data to control for pocketbook concerns (section 7).

In contrast with previous work, the findings from two population-based datasets indicate that the local economy influences presidential election voting. Consequently, presidents face incentives to ensure that growth is distributed geographically—at least to the regions that matter the most electorally.

The Credit Data

We first solve the measurement problem in most economic measures by considering delinquency rates on consumer loans—the share of individuals who are 90 days or more behind on their loan payments.⁷ The delinquency rates rely not on a random sample, but the entire population of loans to California consumers (reported to credit agencies, and collected by Equifax). We observe information on all classes of consumer loans, ranging from credit cards to mortgages. We acquired these data for California for five years, 2006-2010. They capture the status of all loans as of November 1 in each year. We chose California for three reasons. First, the credit data are expensive and so we were restricted to purchasing a single state. Second, California has data on election returns that we can aggregate to the zip code level, which is the reporting unit for the credit data.⁸ Third, California is a large and diverse state.

These loan data capture the financial lives of California residents before and during the recent economic crisis in remarkable detail, covering a yearly average of over 400 million loans with scheduled payments of \$21.9 billion per year over the five years. That number includes an

⁷ Hill, Herron, and Lewis (2010) also examine loan delinquency data and voting at the county level in their thoughtful analysis of the 2008 presidential election.

⁸ Voters may use a variety of geographic contexts to make judgments about politics, some of them smaller than zip codes and some of them larger (Wong et al. 2012).

average of over 35 million mortgage and home equity loans, with associated annual average payments of \$11.8 billion. We observe many details of these loans at close to the individual loan level (see Supporting Materials section 1), and so can calculate how far behind people are on their payments (30 days, 60 days, 90 days or more) for various types of loans. We start by examining economic distress through the number of first mortgages that are at least 90 days delinquent. We choose this threshold because families are unlikely to pass it accidentally by forgetting a payment and because other institutions, such as the New York Federal Reserve Bank, use this standard (NYFED 2013). Other loan types and delinquency thresholds produce similar results, which we discuss in the robustness checks below and in the SM. The data capture the collapse of the California housing market and the more general economic crisis that began in 2007. For each year, Figure 2 shows zip-code level histograms of the percent of first mortgages in delinquency for 90 days or more (we refer to these as “delinquent mortgages”). From 2006 to 2008, the median delinquency rate increased from 0.8 percent to 5.0 percent. The share of zip codes with over ten percent of mortgages in delinquency increased from zero percent to 17.9 percent. The crisis peaked in 2009, when 41.5 percent of zip codes had at least 10 percent of mortgages in delinquency. These histograms reflect the impact that the crisis had, to widely varying degrees, on almost all zip codes in California. In fact, delinquencies increased between 2006 and 2009 in all but seven of the 1422 zip codes with more than 250 registered voters.

Delinquency rates for most other classes of consumer loans also increased substantially before the 2008 election. For example, the 90-day delinquency rate on bank-issued credit cards increased from a median of 1.38 percent in 2006 to a median of 2.27 percent in 2008. Likewise, the delinquency rate for auto finance loans increased from a median of 0.79 percent in 2006 to a

median of 1.30 percent in 2008. In percentage terms, the delinquency rates for bank cards and auto finance each increased by roughly 65 percent.

Loan Delinquencies and Voting

When we switch from sample-based data to our population-based economic measures, do we find that local economic distress hurts the incumbent president's party? Figure 3 shows that we do. It presents the relationship between the 2006-2008 change in the share of zip-code mortgages that are delinquent and the change in Democratic presidential vote share from 2004. We look at the change in delinquencies between 2006 and 2008 because the crisis emerged in these two years (the change between 2007 and 2008 produces similar findings, see below). The figure shows strong evidence that local economic conditions influenced voting. Zip codes exhibiting the largest increase in mortgage delinquency rates were considerably more likely to vote for Obama. They shifted about eight percentage points more towards the Democratic ticket from 2004 to 2008 than did zip codes with little or no delinquencies. Figure 3 shows vote share after controlling for variables outlined below, but the scatterplot looks very similar without those adjustments (see SM section 2).

In our regression model, the dependent variable is the Democratic share of the two-party vote for president in 2008 in each zip code. Our regression equation is:

$$DemVote_{i, 2008} = \beta_0 + \beta_1 DemVote_{i, 2004} + \beta_2 \Delta Loan Delinquencies_{i, 2006-08} + \gamma Controls_{i, 2000-01} + u_{i, 2008}$$

As described in the equation, we include 2004 Democratic vote share to account for the different tendencies of zip codes to vote for the Democrats in general and for regression to the mean. By including it, we are estimating a flexible model of change in Democratic vote share

between 2004 and 2008 (Finkel 1995).⁹ The coefficient of interest is β_2 . It captures the effect of the local economy—as measured by change in loan delinquency rates—on the change in Democratic vote share. In each regression, we rescale the mortgage delinquency variables to vary from zero to one and weight by the number of registered voters in the zip code (the results are similar without weights). Finally, we correct the standard errors to account for clustering at the county level, a more conservative choice than clustering at the zip code level (which yields even more statistically significant results).

Regression Results for Loan Delinquencies

In the first column of Table 1, we report the results from regressing Democratic vote share in 2008 on the change in the share of first mortgages that are at least 90 days delinquent and the lagged dependent variable. The ordinary-least-squares estimates imply that a shift from the zip code with the lowest change in delinquency rate to the one with the highest (about 20 points, see Figure 3) corresponds with a 7.7 percentage point increase in Democratic vote share, a substantial increase. This effect is almost the same size as the overall shift in California towards the Democratic Party from 2004 to 2008 in the presidential vote. Put another way, the results suggest that a one-percentage point increase in mortgage delinquencies increases Democratic vote share by 0.33 percentage points. This relationship is unlikely to be due to chance ($p < 0.001$, $t = 8.78$).

Of course, the zip codes most afflicted by the crisis are likely different from those that were not—they tended to be lower income and have more minority residents. These groups may

⁹ The results remain similar when we instead use change in incumbent party vote share as the dependent variable.

have disproportionately shifted to Obama for other reasons, a tendency the lagged dependent variable would fail to account for. To further address these alternative explanations, column 2 adds pretreatment controls for the share of black, white, and Hispanic residents in the zip code from the 2000 census, long before the crisis. Including these variables leaves the coefficient for mortgage delinquencies essentially unchanged.

Another concern is baseline income: poorer zip codes could also have disproportionately shifted to Obama for other reasons and been especially afflicted by the crisis. In column 3, we add a third-degree polynomial in income to flexibly account for this concern. To avoid sampling error in our measure of baseline income, we utilize data from the IRS that is available for some recent years to measure zip code-level income based on the population of all individuals who filed a tax return. We employ 2005 income in the zip code to get a pretreatment measure. The delinquency estimate does not substantially change.

We present a series of additional robustness checks in Table 2. In each specification, unless otherwise noted, we refer to the most saturated version of the specification, as reported in Table 1, column 3. We first show that the result holds up when we examine the change in delinquency from 2007 to 2008 (instead of 2006 to 2008). Next, the table shows that the finding is robust to using 30+ days as the threshold for delinquency instead of 90+ days. The finding holds up, the table next shows, when we control for the migration of blacks, whites, and Hispanics to or from the zip codes (using change in percent between the 2000 and 2010 censuses). It holds up in each quartile of 2004 Democratic presidential vote share except the most Democratic one (probably because of a ceiling effect). It holds up without weights in the bottom and top halves of zip codes in terms of population (though the estimate is smaller in the bottom half). Finally, Table 2 shows a key placebo test: post-election changes in delinquency

rates (2008-2009) do not predict higher Obama vote share. We present more robustness checks and further details on these tests in SM section 2.

Types of Loans and Borrowers

Our contention is that mortgage delinquencies matter because they are capturing the local economy, particularly the economic distress caused by the recession. With mortgage delinquencies reaching historically unusual levels in 2008 and the housing crisis receiving considerable media attention, voters may have been punishing Republicans for the housing crisis, not for general economic distress. However, we find that delinquencies on other types of loans, such as auto loans, are just as predictive of the shift against the Republicans (see SM section 3.4).

Consistent with the view that delinquencies are capturing general economic distress, we also find that delinquencies influence voting much more among prime borrowers than subprime ones. Given the greater degree of foreclosures before Election Day for subprime borrowers (see SM section 4), we might expect that delinquencies among subprime borrowers—borrowers with credit scores below 660 at the time of loan origination (Mian and Sufi 2010)—would better predict vote shifts against the incumbent. In fact, however, we find that delinquencies by prime borrowers—those above 660 at the time of loan origination—matter considerably more (see SM section 4). Delinquencies by prime borrowers may especially capture distress because their credit histories show that they have rarely missed loan payments previously.

Generalizing to Other States and Years: The QCEW

While the 2007-2009 recession in California offers the opportunity to analyze the impact of a severe economic shock on election outcomes, that shock was also historically unusual. Was

the electoral impact also unusual? To examine whether the results generalize, we consider a dataset that provides monthly measures of county-level economic conditions based on the population of business establishments in the United States: the Quarterly Census of Employment and Wages (QCEW). The Bureau of Labor Statistics produces the QCEW based on employers' Unemployment Insurance filings, and it covers 98 percent of all jobs in the United States. In contrast with the datasets that voting scholars have generally used to measure economic conditions, the population-based QCEW measures local conditions without sampling or modeling error, albeit with a five-month delay. The QCEW are available back to 1990 (Konigsberg et al. 2005).

The QCEW reports total and average wages along with total employment at the county level. Since both wage and employment increases should benefit the incumbent, we measure local economic conditions with the mean of the percentage change for these two variables. We take the mean to better capture the overall state of the labor market (e.g., workers in counties with strong employment growth but weak wage growth probably have an oversupply of labor). The results that we obtain are primarily driven by the wage growth part of the average, as we show below. We take this average for the six months before each presidential election, finding a 1990-2012 mean of 2.1% and standard deviation of 3.3%. Since some counties grow much more consistently than do others, we mean deviate the local economic measure by subtracting the 1990-2012 mean for the county. The results are robust to other coding decisions, as we show below. As with the loan delinquency measures, we rescale the measure (0.1 percentile = 0, 99.9 percentile = 1). We then model the Democratic Party's share of the two-party presidential vote as a function of the local economic conditions. Since we expect improving economic conditions to benefit the incumbent, we interact the local economic conditions with a variable coded 1 for

Democratic incumbent and -1 for Republican incumbents. We then expect a positive coefficient so that a strong economy helps the Democrats when they are the incumbent party and hurts them when they are not. In each regression, we cluster the standard errors at the county level. We also include year effects and a series of control variables.

Table 3 reports the results. Following the specifications in Table 1, the first column shows the effect of economic conditions—measured as the mean of employment and wage growth in the county—controlling only for lagged Democratic presidential vote share and year effects. The estimate on economic conditions is 9.7 and is highly statistically significant. It implies that an increase from the 0.1 percentile to the 99.9 percentile in employment wage growth corresponds with an increase in vote share of 9.7 percentage points, which is similar in size to the estimates for delinquencies. The next column adds state fixed effects and a cubic in baseline county income, where we take the baseline to be 1988. These controls reduce the estimate to about 6.3, still strongly significant. In column 3, the specification adds controls for the percentage of black, white, and Hispanic residents (the excluded category consists mostly of Asian residents). The estimated effect of economic conditions with those demographic controls is similar to the one obtained without them in column 2.¹⁰

Table 3 also shows the very different results obtained when we examine the sample-based measures of county economies. It does so with the two sample-based measures available from government agencies: county unemployment measure (from BLS) and the county median income measure Census Bureau's Small Area Income and Poverty Estimates (SAIPE). The

¹⁰ If we also include separately the state-level growth in economic conditions, that variable enters with a *t*-statistic of 1.90. The coefficient on county-level conditions drops from 5.88 to 5.36 (*t* = 6.64).

unemployment measure is available for the same elections as QCEW, 1992-2012; the income measure is only available for the 2000-2012 elections. We adopt the same coding procedures for these variables (calculate the change from the previous period, mean deviate that change, and rescale to 0-1), though the results do not depend on those decisions, as we show below.¹¹ In contrast with the QCEW results, the sample-based measures fail to find an effect of the local economy, as shown in Table 3, columns 4 and 5. These columns only show the specifications with controls, but the estimates are essentially the same without controls. The unemployment estimate should be negative, as rising unemployment should hurt the incumbent, but its estimate has the wrong sign (positive). The median income growth estimate has the correct sign (positive), but is close to zero. These models estimate these effects with sufficient precision to rule out meaningful effect sizes. The top end of the 95% confidence intervals for the maximum possible, correctly-signed effects are small— -1.8 and 3.1 for unemployment and income, respectively—especially when we consider that the maximal changes in these variables are large—13 percentage points for unemployment and 25 percent for income. The difference between the QCEW measures and the sample-based measures are also highly statistically significant in models that contain sets of both variables. These estimates therefore confirm earlier research that generally failed to find that the local economy influences presidential voting, a null result that now appears incorrect.

To further examine whether the findings in Table 3 are robust, Table 4 presents a series of checks. It shows only the key coefficient and standard error for the economic conditions

¹¹ One difference is that we calculate the QCEW growth for the six months before the election, but cannot do so for the sample measures as they are yearly. As Table 4 (col. 1, sec. D) shows, however, the one-year QCEW effect is identical, so this difference is inconsequential.

measures from Table 3. In each specification, unless otherwise noted, we refer to the most saturated version of the specification, as reported in column 3. The first column examines the robustness of the QCEW wage and employment growth measure. It first shows that that the estimate is insensitive to excluding any one of the six elections—the smallest coefficient occurs when we exclude the 2000 election and reestimate the model, but the effect remains (4.1) and is highly statistically significant. (The SM reports the estimates for each year in section 5.3.) The next row of Table 4 shows that the effect is robust to interacting the control variables with the year dummies. The estimate is also robust to controlling for the level of wages (using the lagged annual wage from the QCEW), and to controlling for the lagged annual wage squared and cubed, as shown in the next two rows. The following row shows that it is also robust to controlling for population growth, as measured from the Census.

Next, we show that the results are also not sensitive to weighting the data by the number of registered voters—instead we present unweighted estimates for counties with 25,000 voters or more and 50,000 voters or more. The effect is somewhat smaller when we do not mean deviate the economic conditions measure, as shown in the next row, where the estimate falls to 2.4, indicating that economy’s estimated effect comes from variation within the county over time. When we separate out employment and wage growth, we also see that it is primarily wage growth that drives the estimated effects of the county economy. Employment growth appears to have little impact by itself, while wage growth has a similar impact as that of the mean of employment and wage growth.

The effect of the economic conditions measure is also robust to considering average employment and wage growth over the year before the election, instead of just the six months before, as shown in the last row for the pre-election economy measures. Finally, we include a

series of placebo tests at the bottom of Table 4. Those tests show that post-election economic conditions fail to predict presidential voting, providing evidence that the conditions in the six months before the election causally impact election outcomes. The SM presents additional robustness checks (section 5).¹² Columns 2 and 3 of Table 4 repeat these robustness tests on the two sample-based measures, showing that the estimates generally remain small or incorrectly signed (in the case of unemployment growth).

Why Did Previous Studies Find Small Local Effects?

Our findings differ from earlier research suggesting little to no role for the local economy in determining presidential election outcomes. Two explanations likely account for why researchers have failed to find these effects. First, the population-based measures may capture a different aspect of the local economy than the sample-based measures, and that different aspect may be what matters for voting at the local level. Second, the sample-based measures may contain too much measurement error to detect effects on voting. Several results support the measurement-error explanation over the different-aspect explanation, but both remain possible.

To start, the population and sample-based measures appear to capture the same underlying state of the economy. The strongest evidence comes from the national level where sample-based measures are precisely estimated. At this level, the sample-based measures correlate highly with the equivalent population-based ones. For example, the sample-based unemployment rate and population-based employment growth correlate at -0.83 at the national level (see SM section 2.1). These two measures also correlate highly with real per capita GDP

¹² We also consider some results using earlier data collected by the BLS but not published as part of the QCEW. Those results are similar to the ones in Table 3 (see SM section 5.3).

growth (at -0.85 and 0.79, respectively) and with the growth rate in mortgage delinquencies (at 0.79 and -0.76). These strong correlations imply that, at the national level, these measures all capture similar phenomena.

Not only do these measures all correlate highly at the national level, but they also all predict incumbent presidential vote at the national level. In the national-level retrospective voting literature, researchers often focus on real disposable income growth (Achen and Bartels 2016) or GDP growth (Wlezien 2015). All the measures discussed above, however, including the population-based measures in this paper, predict national-level vote with similar strength as real disposable income growth and GDP growth (see SM section 6.2).

The high correlation between the sample-based and population-based measures at the national level, as well as their similar abilities to predict the national-level vote, suggest that these measures capture the same construct, at least at the national level. This finding argues against population-based measures predicting local level voting because they measure something different at the local level, though we cannot rule out the possibility with certainty. Instead, the correlations suggest that measurement error may account for sample-based measures failing to predict county-level voting, as the high national level correlations deteriorate markedly at the local level (see SM sections 6.3 and 6.4). For example, the -0.83 national-level correlation between the sample-based unemployment rate growth and the population-based nonfarm employment growth falls to only -0.24 at the county level. Many of the other high national correlations for sample-based measures fall even further, to near zero, at the local level. Those lower correlations at the local level suggest the potential presence of measurement error in the

sample-based measures, error that would lead to downwards bias if used to predict county-level election results.¹³

Could Measurement Error Explain Earlier Findings?

The government agency that produces the sample-based income measure of the local economy provides information on the amount of error in the measure. Based on the levels they report, would we expect to fail to find effects on vote choice?

The answer appears to be yes. To show this, suppose that a county-level economic measure at time t , x_t , is the true value, x_t^* , plus some additive error, ε_t :

$$x_t = x_t^* + \varepsilon_t.$$

The error in the observed economic measure can come from sampling or other sources. If we consider the bivariate regression of a variable y_t (e.g., incumbent party vote share) on x_t , the expected coefficient estimate depends on the true coefficient, β , the variance of economic conditions, $\sigma_{x^*}^2$, and the variance of the error, σ_{ε}^2 :

$$E(\hat{\beta}) = \left(\frac{\sigma_{x^*}^2}{\sigma_{x^*}^2 + \sigma_{\varepsilon}^2} \right) \beta.$$

Measurement error will be higher, for example, for sample-based variables when the sample size is smaller. It also worsens considerably when we consider the change in a measure over time. When income is measured with error at the current and previous point, the independent variable becomes:

$$x_t - x_{t-1} = (x_t^* - x_{t-1}^*) + (\varepsilon_t - \varepsilon_{t-1}).$$

¹³ Also consistent with the measurement error interpretation, we find that the sample-based, unemployment rate growth becomes a better predictor of vote when aggregated to the Metropolitan Statistical Area (see SM section 6.6).

If the measurement error is independently and identically distributed at different points in time, so that $\sigma_{\Delta\varepsilon}^2 = 2\sigma_\varepsilon^2$, then we have:

$$E(\hat{\beta}) = \left(\frac{\sigma_{\Delta x^*}^2}{\sigma_{\Delta x^*}^2 + 2\sigma_\varepsilon^2} \right) \beta.$$

For most economic measures, such as unemployment or income, the variance within county of the change in income is smaller than the variance across counties of income, so that $\sigma_{\Delta x}^2 < \sigma_x^2$. Consequently, using the change worsens the signal-to-noise ratio: it reduces the variance of the signal while increasing the variance of the noise.

The sample-based measure for which the government provides estimates of error at the county level is median income, from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE).¹⁴ To derive their estimates, the Census uses a model based on the American Community Survey, a survey of approximately 295,000 households each month. They provide 90 percent confidence intervals that are meant to capture uncertainty coming from two sources: 1) sampling variation within counties, and 2) lack of fit in the model. According to Census (2016), "the sampling variance is large compared with the lack of fit variance."

Given the Census Bureau's confidence intervals for its county-level income estimates, we can estimate the signal-to-noise ratio in yearly income growth. Assuming that the measurement error at different points in time is independently and identically distributed, the Census Bureau's

¹⁴ The BLS provides estimates of error for their state-level unemployment estimates, but not their county-based ones. Those estimates are based on a model drawing on several datasets, including the Current Population Survey (CPS). We thank BLS economist Susan Campolongo for helpful correspondence about the unemployment data.

confidence intervals suggest that the attenuation in a bivariate regression using income growth would be large. The reliability is far lower than a perfect score of one:

$$\left(\frac{\sigma_{\Delta x^*}^2}{\sigma_{\Delta x^*}^2 + \sigma_{\Delta \varepsilon}^2} \right) = 0.39.$$

The ratio implies an attenuation bias of income's effect on vote of $(1-0.39) \times 100 = 61$ percent. In a bivariate regression, the Census Bureau's own error estimates for county-level income are thus large enough to explain the small-to-null effects found by previous researchers. Much of this low reliability arises, we emphasize, because we are examining change in the income measure. The Census Bureau's claims to estimate county income levels relatively precisely (average reliability of 0.91), but examining year-to-year change reduces the signal substantially while doubling the noise. In simulations, we find that we would be unable to detect the apparent effect of QCEW wages on vote with the error levels in the Census county-level income data (see SM section 9).

Conclusion

These findings from these unique data have important implications for democratic accountability. They suggest that presidents face incentives to boost the economy in politically important regions even when doing so may harm the economy overall. Examples abound, from long-standing agricultural subsidies to tariffs on Chinese tires, which may have saved several hundred jobs in politically important states but cost consumers billions (Hufbauer and Lowry 2012). Moreover, these results suggest that national leaders may face similar perverse incentives in any region-based electoral system.

These findings are also robust. The delinquency findings in California hold up when controlling for prior Democratic vote, prior demographics, migration, and prior income. They hold up separately in areas with low Democratic vote share (in prior elections). They also survive

placebo checks (changes in delinquencies after the election fail to predict change in vote share). The findings are therefore not the result of Democratic regions being more likely to get into debt.

Moreover, the consistency in the results across two datasets speaks to the generality of the findings. Given the unusual nature of the 2007-2009 housing shock, findings based on it may or may not generalize to other elections. For example, media coverage of the subprime crisis and foreclosures may have helped voters connect their personal circumstances with the incumbent president, not a connection that voters readily make on their own (Mutz 1998). Our analysis of the QCEW, which covers the entire country over multiple elections, however, leads to remarkably similar conclusions and implies that the delinquency results do generalize.¹⁵

How large are the effects of the local economy on presidential voting? Previous work (e.g., Bartels 2008) has argued that each percentage point of election-year disposable income growth increases the incumbent party's expected vote margin by about four percentage points. With the standard deviation of yearly real disposable income growth since 1980 being about 1.4 percentage points, the estimate translates to a one-standard-deviation improvement in national

¹⁵ There are several reasons to believe the loan-delinquency findings generalize, in addition to our QCEW results. First, delinquencies predict voting, not just on mortgages, but on types of loans that received minimal media coverage, such as credit cards and auto loans (as shown in SM section 3.4). Second, media coverage focused on subprime areas devastated by foreclosures, but our findings show that it was particularly delinquencies amongst prime borrowers, not subprime borrowers, that cost the Republicans votes. Third, recent work using precisely-measured local data has found that a variety of other local factors influence presidential election outcomes, including distributive spending (Chen 2013; Healy and Malhotra 2009), trade-induced layoffs (Margalit 2011), and war casualties (Grose and Oppenheimer 2007; Karol and Miguel 2007).

conditions equaling about 5-6 percentage points more for the incumbent presidential party. For local economic conditions, the results across both datasets suggest that a one-standard-deviation improvement adds about 1-2 points to the incumbent party's vote margin. While significantly smaller than the national economy's impact, it arguably provides a lower bound. Since voters appear to respond to local economic conditions, the national economy may influence voters in part through local conditions (which year fixed effects exclude in the QCEW analysis).

Is it reasonable for voters to hold the president accountable for the local economy? In some years, it may be. Since the federal government regulates banking and mortgages, voters in 2008 who lived in areas impacted by foreclosures may have had more access to information needed to appropriately or inappropriately blame the president for poor regulation. In other years, however, regional variation seems largely haphazard, and yet voters nevertheless continue to respond. Such behavior accords with evidence suggesting that voters hold the president accountable for events beyond the president's control, ranging from shark attacks to football games (Achen and Bartels 2004; Healy, Malhotra, and Mo 2010; Huber, Hill, and Lenz 2012).

An important question is whether the local economy matters for pocketbook reasons or for sociotropic reasons, that is, whether voters care primarily about themselves or primarily about the nation. The local economy may matter primarily because it correlates with voters' pocketbooks. If so, the findings would provide long-sought evidence for pocketbook voting, since the evidence from voter surveys supporting sociotropic voting over pocketbook remains controversial (Kramer 1983). Our findings, however, are also plausibly interpreted as sociotropic voting. Voters may simply be drawing inferences about the national economy based on their local circumstances (Ansolabehere, Meredith, and Snowberg 2011). For instance, voters in hard-hit counties may be voting against the president, not because of their own circumstances, but

because they in fact believe the national economy is worse than it actually is. Another related interpretation is that voters might particularly care about their friends and neighbors, voting not according to their own interest but instead according to that of their area. Future research may be able to disentangle these alternatives, which cast voters' motives in very different lights.

Another area for future research is on the origins of the 2007-2009 recession. The impact of the economic downturn on voting had its roots in household debt increases that began as much as six years before the election. These increases may have occurred because lenders duped people—both affluent and not—into taking out loans that they could ill afford, but also in part due to borrowers suffering from self-control problems. A fascinating question for future research, one with important implications for democratic competence, is whether some voters may have punished incumbents for misery of their own making.

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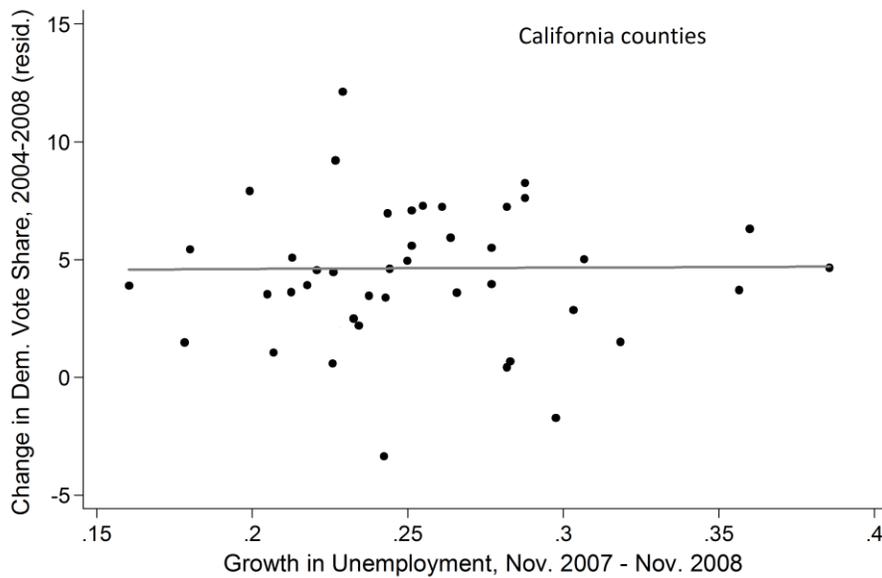
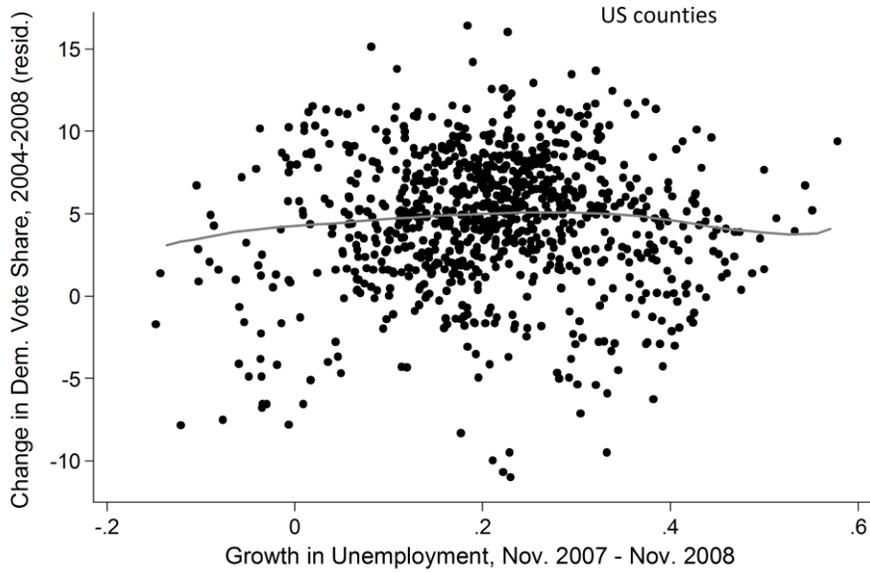


Figure 1: Democratic Vote Share and Unemployment Growth

Note: Counties with at least 50,000 residents in 2008 according to the Census bureau are included in the figure. To control for demographics, vote is residualized using percent white, percent black, percent Hispanic, and income, income squared, and income cubed (see the SI for details). That is, it presents "component plus residual" plots that residualize the dependent variable for variation explained by the percent white, black, and Hispanic as well as for income, income squared, and income cubed. For details and the full models, see the supporting information. The lpolyc curve is weighted by votes cast in the county. Two outliers for unemployment are dropped from the top plot.

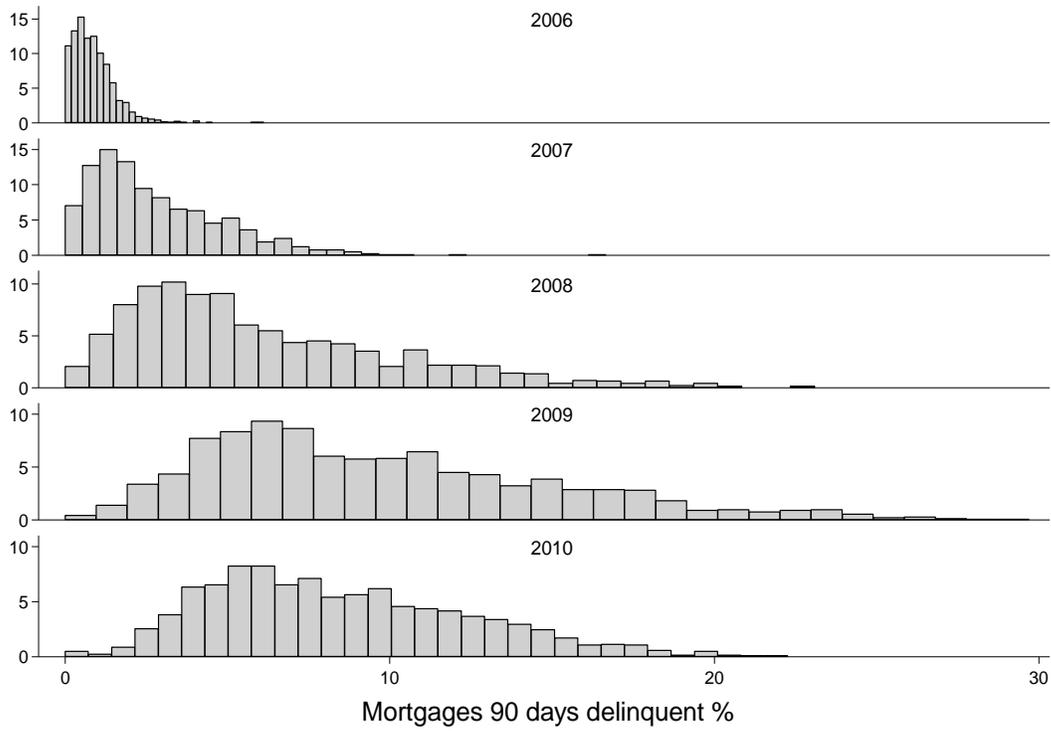


Figure 2: Histograms of the Mortgage Delinquency Rates for California Zip Codes

Note: Plot shows zip codes with approximately 250 registered voters or more.

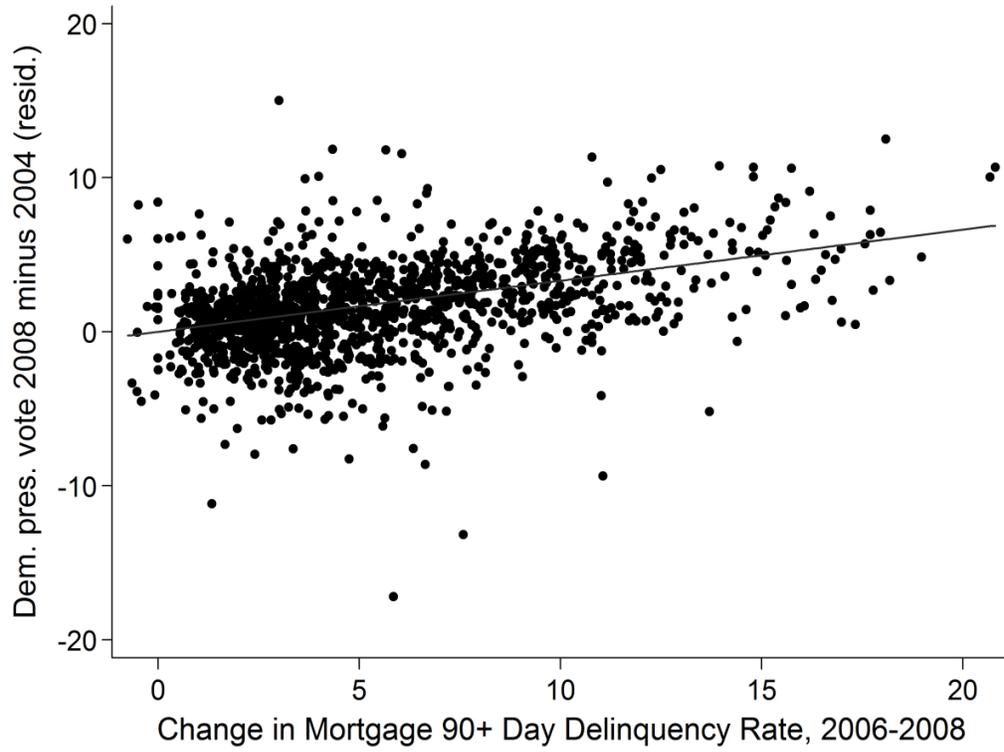


Figure 3: Democratic Vote Share and Change in Mortgage Delinquency

Note: To control for demographics, vote is residualized using a regression of the change in Democratic vote share on a set of demographic variables (see Table 1, Column 3). Stata's `lpoly` command produced the best-fit curve and is weighted by the number of zip-code registered voters.

Table 1: ZIP Code Mortgage Delinquencies and Presidential Voting, 2008*Dependent variable: Democratic share of two-party presidential vote, 2008**Level of analysis: Zip code*

	(1)	(2)	(3)
Change in share of mortgages 90+ days delinquent, 2006-2008	7.68*** (0.87)	6.73*** (1.01)	7.68*** (1.08)
Democratic vote share, 2004	0.90*** (0.01)	0.90*** (0.011)	0.89*** (0.010)
Percent black, 2000		0.011 (0.018)	0.026 (0.018)
Percent white, 2000		0.003 (0.014)	-0.0017 (0.013)
Percent hispanic, 2000		0.011 (0.011)	0.030** (0.014)
Average income, 2001			0.062*** (0.012)
Average income squared, 2001			-0.00024*** (0.000062)
Average income cubed, 2001			2.3e-07*** (6.8e-08)
Constant	9.87*** (0.82)	9.86*** (1.20)	7.12*** (1.42)
Observations	1,386	1,386	1,386
R-squared	0.983	0.983	0.985

Note: Since we control for Democratic vote share in 2004 (the lagged dependent variable), this is a model of change in Democratic vote share (see Finkel 1995). Robust standard errors clustered at the county level.

Weighted by the number of zip-code registered voters. *** p<0.01, ** p<0.05, * p<0.1

Table 2: Robustness of Local Economy's Effect on Presidential Elections for Delinquencies

	Coefficient estimate (SE)
<i>Baseline Specification</i>	
Change in share of mortgages 90+ days delinquent, 2006-2008 (Table 1, col. 3)	7.7 (1.1)
<i>A. Different Delinquency Measures in Zip Code</i>	
Change in share of mortgages 90+ days delinquent, 2007-2008	15.3 (2.3)
Change in share of mortgages 30+ days delinquent, 2006-2008	8.1 (1.2)
Change in share of mortgages 30+ days delinquent, 2007-2008	18.0 (3.1)
<i>B. Controlling for Migration</i>	
Controlling for black, hispanic, and white migration	5.4 (1.1)
<i>C. Breaking the Effect Down by Previous Vote in the Zip Code</i>	
1st quartile of 2004 Democratic vote	10.1 (2.1)
2nd quartile of 2004 Democratic vote	10.0 (1.6)
3rd quartile of 2004 Democratic vote	5.0 (1.2)
4th quartile of 2004 Democratic vote	-0.5 (1.1)
<i>D. Breaking the Effect Down by High and Low Population Zip Codes</i>	
Below the median zip-code population (no weights)	3.1 (1.4)
Above the median zip-code population (no weights)	8.1 (1.4)
<i>E. Placebo Tests Using Post-Election Delinquencies in Zip Code</i>	
Change in share of mortgages 90+ days delinquent, 2008-2009	1.0 (2.8)

Note: All estimates are based on Table 1, column 3 model except where noted. Robust standard errors clustered at the county level, except where noted. Weighted by the total number of voters, except where noted. Change in mortgage delinquencies is rescaled to vary between zero and one. All estimates are statistically significant at $p < 0.01$ except for the "below the median zip-code population" and "placebo test" estimates.

Table 3: The Local Economy and Presidential Vote, 1992-2012*Dependent variable: Democratic share of two-party presidential vote**Level of analysis: County*

	(1)	(2)	(3)	(4)	(5)
	Population measure	Population measure	Population measure	Sample measure	Sample measure
Expected sign:	+	+	+	-	+
QCEW wage and employment growth*Democratic incumbent	9.82*** (1.26)	6.42*** (0.99)	5.95*** (0.97)		
BLS Unemployment growth*Democratic incumbent				1.09 (1.49)	
SAIPE Income growth*Democratic incumbent					0.86 (1.12)
Controls					
Previous election Democratic vote share	1.03*** (0.0051)	1.00*** (0.0057)	0.91*** (0.0071)	0.91*** (0.0072)	0.92*** (0.0083)
1988 per-capita income		-3.25 (4.84)	6.29 (5.34)	7.34 (5.47)	6.30 (6.93)
1988 per-capita income squared		0.014 (1.53)	-3.05* (1.76)	-3.46* (1.81)	-3.41 (2.29)
1988 per-capita income cubed		0.10 (0.15)	0.40** (0.17)	0.44** (0.18)	0.46** (0.23)
Percent black			5.61*** (1.23)	5.47*** (1.27)	8.15*** (1.43)
Percent white			-5.78*** (1.22)	5.99*** (1.25)	4.40*** (1.34)
Percent hispanic			4.88*** (1.00)	4.76*** (1.02)	4.60*** (1.08)
Constant	11.0*** (0.69)	15.3*** (4.46)	15.0*** (4.48)	11.1** -4.71	-0.27 -5.68
Year effects?	Y	Y	Y	Y	Y
State fixed effects?	N	Y	Y	Y	Y
Observations	18,511	18,510	18,510	18,510	12,357
R-squared	0.932	0.94	0.946	0.946	0.959

Note: The first three columns of this table show that the population-based QCEW measure of the local economy strongly predicts incumbent presidential vote share, while columns 4 and 5 showed that sample-based measures do not. We rescale the local economy measures to vary between zero and one. Since we control for Democratic vote share (the lagged dependent variable), this is a model of change in Democratic vote share (see Finkel 1995). We do not include the main effect of Democratic incumbent because its effect is captured by the year fixed effects. Robust standard errors clustered at the county level. Weighted by the total number of voters. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Robustness of Local Economy's Effect on Presidential Elections

	Estimate (SE)		
	QCEW wage and employment growth	BLS Unemployment growth	SAIPE Income growth
	Population measure	Sample measure	Sample measure
Expected sign:	+	-	+
<i>Baseline Specification</i>			
From Table 3, Columns 3, 4, and 5	6.0 (1.0)***	1.1(1.5)	0.9(1.1)
All estimated in same sample (2000-2012)	9.2 (1.1)***	0.7(1.4)	0.9(1.1)
<i>A. Testing for Sensitivity to Individual Elections</i>			
Largest effect excluding elections individually and reestimating	7.3(1.1)***	-3.9(2.0)*	2.1(1.4)*
Smallest effect excluding elections individually and reestimating	4.1(0.8)***	-0.8(1.6)	-0.3(1.1)
<i>B. Controlling for Additional Variables</i>			
All controls interacted with election-year effects	5.6(0.9)***	0.7(1.1)	1.0(1.0)
Controlling for lagged annual wage	7.1(1.1)***	-0.7(1.5)	0.8(1.1)
Controlling for lagged annual wage, squared, and cubed	6.7(1.1)***	-0.4(1.6)	0.6(1.1)
Controlling for population growth	5.7(1.0)***	1.2(1.5)	0.8(1.1)
<i>C. Dropping Smaller Counties and Not Using Weights</i>			
Counties with 25,000 voters or more (no weights)	5.0(1.1)***	-0.5(1.5)	-0.5(1.1)
Counties with 50,000 voters or more (no weights)	5.6(1.3)***	0.4(1.9)	-0.7(1.4)
<i>D. Varying the Economic Measure</i>			
Not mean deviated	2.4(0.9)***	2.4(1.6)	1.6(1.2)*
Employment growth	0.8(1.0)		
Wage growth	4.7(0.9)***		
Average of wage and empl. growth for year before election	5.9(1.0)***		
<i>E. Placebo Tests Using Post-Election Economic Conditions</i>			
Regression from Table 3, Column 2	0.8(0.6)	-0.0(0.1)	0.1(0.0)
Regression from Table 3, Column 3	0.5(0.6)	-0.1(0.1)	0.1(0.0)

Note: All models are based on Table 3, columns 3, 4, and 5 except where noted. Growth measures are rescaled to vary between zero and one. Robust standard errors clustered at the county level. Weighted by the total number of voters in the county, except where specified. *** p<0.01, ** p<0.05, * p<0.1 only noted for expected direction (signs).