

Presidential Voting and the Local Economy

Evidence from Two Population-Based Datasets*

Andrew Healy
Loyola Marymount University
ahealy@lmu.edu

Gabriel S. Lenz
University of California, Berkeley
glenz@berkeley.edu

February 2014

We show that standard economic measures based on samples and richer newly available ones based on populations lead to strikingly different conclusions about democratic accountability. Previous research, which has primarily relied on sample-based measures, has largely missed an important determinant of presidential election outcomes: the local economy. We demonstrate the local economy's impact with two unique population-based economic measures: (1) missed payments (delinquencies) on *all* loans made to California consumers before and during the 2007-2009 recession, as reported to a credit bureau, and (2) employment and wage data based on unemployment insurance contributions for almost all businesses in the United States from 1990-2012. In contrast to measures subject to sampling error, these population-based measures indicate that economic conditions at the zip code and county level have a clear impact on presidential election outcomes. Presidents therefore face incentives, not to distribute prosperity widely, but to target specific geographic regions.

* We thank Henry Korytkowski and Lori Pete of Equifax for their help with the credit score and consumer loan data. We also thank Tejas Dave, Stephanie Khoury, and Sam Syde for excellent research assistance. We are grateful for helpful comments to James Alt, Shigeo Hirano, Yotam Margalit, Jennifer Merolla, Helmut Norpoth, Randy Stevenson, and seminar participants at Claremont Graduate University, the Stanford Graduate School of Business, the 2013 Midwest Political Science Association meetings, and the 2013 American Political Science Association meetings.

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 election outcomes (e.g. Fair 1978; Tufte 1978; Hibbs 1987; Erikson 1989; Lewis-Beck and Stegmaier 2000; 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. Local economic voting would push presidents 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).¹ In a political system where some regions have greater electoral importance than others, local economic voting will incentivize presidents to pursue policies that will likely help the economy in those areas but may reduce the welfare of citizens overall.²

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

¹ Consider, for example, President Carter's actions shortly before the 1980 election. Viewing Iowa as vital to the election, Carter's campaign chairman Richard Strauss urged Carter to reverse an earlier position and impose tariffs on imported ethanol. Just five days before the election, Carter sent a letter to Treasury Secretary G. William Miller calling for the implementation of tariffs "immediately, by administrative means if possible" (Farnsworth 1980). Two days later, Archer Daniels Midland fulfilled a promise to the Carter campaign to announce a new ethanol-producing plant in Iowa if tariffs were imposed (the plant was later cancelled). The decision to impose the tariff came in the face of objections from the Special Trade Representative, the Treasury Department, and the Justice Department, who warned of damage to the national economy through inflation, harm to consumers, and a potential trade war (Farnsworth 1980; Staff 1985; Lawrence 2010).

² 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.

economy on presidential elections. When using standard economic aggregates, that research has generally found the local economy to have little impact.³ For example, when survey-based estimates of county-level income fall or unemployment increase, voters do not generally appear to shift against the incumbent party (e.g., Eisenberg and Ketcham 2004). However, data limitations may have obscured the true impact of the local economy. In the US, economic aggregates have generally been based on sample surveys that measure national conditions accurately, but have massive sampling error when used to estimate local conditions. This noise can cause a county-level unemployment estimate to be off by several percentage points. Measurement error will usually, and often severely, bias regression coefficients towards zero (Angrist and Pischke 2009).⁴ Therefore, noisy estimates of local economic conditions could render undetectable their true effect on election outcomes.⁵

To determine how voters do or do not respond to the local economy, we consider two sources of data that eliminate sampling error. First, we use zip-code level credit bureau data on all consumer loans in California—a 100% sample—to examine loan delinquencies, including 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

³ The earliest papers to look at presidential voting and 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. More recent studies reach similar conclusions (Kim, Elliott, and Wang 2003; Eisenberg and Ketcham 2004). Studies examining the influence of the local economy on perceptions of national conditions have also failed to find a relationship at the city level (Books and Prysby 1999).

⁴ Hausman (2001) refers to this frequent downwards bias as the “Iron Law of Econometrics.”

⁵ 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).

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. Moreover, we show with simulations that our results would not be visible in the kinds of sample-based economic datasets that have ordinarily been available to researchers.

In addition, we consider the extent to which our results generalize. While the foreclosures and loan delinquencies in California provide an unusually powerful economic shock to analyze, they were also unusually salient in the 2008 election, and so the effects from that context may or may not generalize to other states and years. To determine whether they do 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 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 by data limitations.

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

Graphical Overview of Findings

As unemployment rose sharply and incomes fell nationwide before the 2008 election, voters appeared to punish the Republican Party for the economic downturn at the national level, shifting substantially to the Democratic Party in the 2008 presidential vote as compared to 2004.⁶ At the local level, however, they did not appear to respond similarly, at least according to standard economic aggregates. Counties that appeared to be worst hit by the recession according to unemployment or income exhibited little tendency to be more likely to vote Democratic, as we show in Figure 1. The top part of the figure shows the relationship between Democratic vote share and the growth in county-level unemployment for US counties; the bottom shows it just for California counties. Since the 2008 election had the first African-American presidential candidate and the subprime crisis was especially severe in minority neighborhoods, these graphs control for the percent black, Hispanic, and white, as well as for pre-crisis income.⁷ Nationwide, the figure shows that counties shifted Democratic by about 5%, on average, regardless of local unemployment growth before the 2008 election. In fact, many of the counties with the largest increases in unemployment appear to punish the Republican ticket somewhat less harshly, shifting less than five percentage points.⁸ For California, we likewise see no clear relationship

⁶ Between November 2007 and November 2008, unemployment increased from 4.7% to 6.8%. From just June 2008 to November 2008, national income as measured by GDP, fell by 2.9%.

⁷ That is, we present "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.

⁸ Two recent papers have examined local economic voting in the 2008 presidential election using different data sources. Examining county-level change in unemployment rates, gas prices, and accumulated foreclosures, Cho and Gimpel (2009) find opposite than expected relationships overall, e.g., unemployment increases in fact helped the Republican ticket, but they also find considerable variation by region. Their geographically weighted regression estimates imply considerable variation in effect, finding for example that rising unemployment may have hurt the

between Democratic vote share and unemployment growth. Using income growth rates, as estimated by sample-based models, as the economic measure produces similar results.

[Figure 1 about here]

As noted earlier, however, standard measures of local economic conditions such as the unemployment rate suffer from sampling error. The model that the Bureau of Labor Statistics (BLS) uses to estimate county-level unemployment, for example, is based mainly on the Current Population Survey (CPS), which interviews about 0.1% of the US population in a given year.⁹ Although the sample is large enough to estimate the national unemployment rate with reasonable precision, it is too small to precisely estimate local unemployment rates.

We first solve this measurement problem by considering delinquency rates on consumer loans—the share of individuals who are 90 days or more behind on their loan payments. The delinquency rates rely not on a random sample, but the entire population of loans to California consumers. We observe information on all classes of consumer loans, ranging from credit cards to mortgages. We discuss the data in detail in the next section and provide additional description in Section 1 of the Supporting Information (SI).

Figure 2 shows the relationship between mortgage delinquencies and the change in Democratic presidential vote share at the zip-code level in California, again controlling for demographics and pre-crisis income. The figure shows strong evidence that local economic

Republican presidential ticket in some areas, such as the Ohio River Valley. Hill, Herron, and Lewis (2010) conclude that local unemployment rates, also measured at the county level, had relatively little effect on support for Obama. They do find a small effect for the average mortgage delinquency rate, this time in the expected direction, but only for higher-wage counties.

⁹ The CPS interviews approximately 60,000 households per month. A household is interviewed a total of eight times over the course of a year in the sample. Therefore, roughly 90,000 unique households are in the sample per year, giving 0.1% as the estimated sampling share.

conditions influenced voting. Areas exhibiting the highest mortgage delinquency rates were considerably more likely to vote for Obama. Zip codes experiencing the highest rates of 90-day mortgage delinquencies shifted more than 16 percentage points towards the Democratic ticket from 2004 to 2008, whereas areas with little or no delinquencies only shifted nine percentage points, a seven percentage-point difference. We find similar effects if we aggregate our data up to the county level to make a comparison with Figure 1. Please see Section 2.1 of the SI for details.

[Figure 2 about here]

The collapse of housing prices played a key role in the great recession, but our findings are not driven just by mortgage delinquencies. In fact, delinquencies on non-mortgage loans, such as credit cards and auto loans, also robustly predict changes in vote share. If we take the simple average of the delinquency rates across nonmortgage loans, we obtain a similar relationship to the one observed for mortgage delinquencies.¹⁰ Figure 3 presents this finding. It shows that a shift from the zip code with the lowest to the highest delinquency rate corresponds with about an eight-point increase in Obama's vote share, similar to the seven percentage-point effect for mortgage delinquencies.

[Figure 3 about here]

In the following sections, we expand on the results in Figures 2 and 3. We describe the data, consider alternative explanations, conduct simulations as if we only had a sample of the

¹⁰ We include the six nonmortgage loans in our data other than student loans to create this measure. These include auto loans from banks or from auto retailers, credit cards from banks and retailers, unsecured consumer loans, and other consumer loans. If student loans are included, the graph looks similar (please see Section 2.5 of the SI). We do not include student loans in our measures of nonmortgage delinquency because research on such loans concludes that “economics plays a modest role in repayment behavior” (Flint 1997, 344).

data, consider the roots of the recession in loans made years before the election, and finally test whether the findings generalize to other states and elections.

Data and Summary Statistics

Equifax Credit Trends Data

The Equifax Credit Trends Data provide measures of loan payments and delinquencies at the zip-code level using a 100% sample of consumer loans. We acquired these data for California for five years, 2006-2010. The data capture the current status of all loans as of November 1 in each year. We chose California for three reasons. First, the Equifax 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. 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 \$22 billion per year over the five years. We observe these loans at close to the individual loan level. Each row of the data describes the number of loans and their payment status for a given loan type (e.g. credit card, auto loans, first mortgages). The example below shows one line of the data for a subset of our variables:

	Loan category					Number of loans in each payment status category		
Date	Product	Vintage	Original Score	Current Score	ZIP	Current	30 days delinquent	90 days delinquent
200811	Bank Card	1997Q1	4	3	*****	1	0	1

This line describes the current standing as of November 2008 (date) of two bank-issued credit card accounts opened in the first quarter of 1997 (vintage) in a particular zip code. At the time of opening the accounts, both borrowers scored 4 for credit status (original credit score of 700-850), but scored 3 in November 2008 (current credit score of 660-699). Of those borrowers, one is current on her account in November 2008 and the other is 90 days delinquent. The line shows only two credit card loans because only two existed in the zip code with all of these characteristics (e.g., opened in the first quarter 1997, with original risk 4, etc.). Besides these variables, the data also contain information on the number of loans delinquent at least 60 days, at least 120 days, currently in bankruptcy, and recently closed. A separate set of variables also describes the balances in each of these categories. On this same line, for example, we could observe that loan repayments of \$1000 are current with \$1500 being 90 days delinquent.

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 show below and in the SI.

Summary statistics

The Equifax data capture the collapse of the California housing market and the more general economic crisis that began in 2007. For each year, Figure 4 shows zip-code level histograms of the percent of first mortgages in delinquency for 90 days or more.¹¹ From 2006 to 2008, the median delinquency rate increased from 0.8% to 5.0%. The share of zip codes with over 10% of mortgages in delinquency increased from 0% to 17.9%. The crisis peaked in 2009, when 41.5% of zip codes had at least 10% 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.

[Figure 4 about here]

Delinquency rates for other classes of consumer loans also increased substantially before the 2008 election. We see such a pattern for nine of the eleven loan categories, with the exceptions being student loans and retail credit cards, where we see a small increase for the latter category. For example, the 90-day delinquency rate on bank-issued credit cards increased from a median of 1.38% in 2006 to a median of 2.27% in 2008. Likewise, the delinquency rate for auto finance loans increased from a median of 0.79% in 2006 to a median of 1.30% in 2008. In percentage terms, the delinquency rates for bank cards and auto finance each increased by roughly 65%.¹²

¹¹ For simplicity, we will refer to mortgages that are at least 90 days delinquent simply as “delinquent mortgages” from this point.

¹² The 30-day delinquency rate is larger for each type of loan and also shows the increase from 2006 to 2008. For example, the median share of auto finance loans that were at least 30 days delinquent increased from 4.85% in 2006 to 7.15% in 2008.

Local Economic Voting: Regression Results

In this section, we build on Figures 2 and 3 by presenting regression results that consider whether alternative explanations could account for the patterns in the graphs. We examine the role of chance, underlying demographic differences, and migration. To do so, we rely on regression models in which the dependent variable is the Democratic share of the two-party vote for president in 2008. Our regression equation is:

$$DemVote_{it} = \beta_0 + \beta_1 DemVote_{it-1} + \beta_2 Economy_{it} + \gamma Controls_{it} + u_{it} \quad (1)$$

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. The coefficient of interest is β_2 , the effect of the local economy—as measured by loan delinquency rates—on the change in Democratic vote share. In each regression, we rescale the mortgage delinquency and demographic variables to vary from 0 to 1 and weight by the number of registered voters in the zip code.¹³ Finally, we correct the standard errors to account for clustering at the county level.

Chance and Underlying Demographic and Income Differences

We start by considering whether the relationships seen in Figures 2 and 3 could be due to chance. In the first column of Table 1, we regress Democratic presidential vote share in 2008 on the share of first mortgages that are at least 90 days delinquent and the lagged dependent

¹³ We also considered specifications where the economy was measured by the change in delinquency rates from the previous year. These regressions lead to similar results. In addition, weighting by the number of votes or not weighting the regression has little effect on the estimates. Please see the SI for details (sections 2.3-2.4).

variable. Using ordinary least squares, the estimates imply that a shift from the ZIP code with the lowest delinquency rate to the one with the highest corresponds with a 6.8 percentage point increase in Democratic vote share, a substantial effect. 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 0.3 percentage points. This relationship is highly unlikely to be due to chance ($p < 0.001$, $t = 8.58$).

[Table 1 about here]

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. To start to 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 may have shifted more towards the Democratic presidential candidate and also have been more 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.¹⁴ We employ 2005 income in the zip code to get a pretreatment measure. The delinquency estimate does not substantially change.

¹⁴ In principle, we could use these data to examine the crisis, but the economic downturn fails to appear clearly in the IRS income data. Income stays mostly flat from 2007 to 2008. This might occur because 2008 income is the average over the course of the year, while we observe a

In the SI, we present additional regressions and plots to illustrate these relationships, demonstrate that delinquencies are not concentrated in minority neighborhoods, and show that changes in turnout do not drive the effect (see Section 3). In sum, race, ethnicity, and baseline income do not account for the effect that the economic crisis had on voting at the zip code level.

Migration

Another alternative explanation for the relationship between voting and our measures of financial distress is outmigration of whites. Whites may be more able, on average, to leave zip codes with rising delinquencies and foreclosures. If so, the departure of whites—who are more likely to vote Republican—from afflicted neighborhoods could make it appear as if rising delinquencies lead to more Democratic votes.

To address this concern, we would ideally measure white migration between the 2004 presidential election, before the crisis, and the 2008 presidential election. Since white percent is unavailable in those years, we instead measure the percentage change between 2000 and 2010. In column 4 of Table 1, we add variables for black, white, and Hispanic migration into the zip code. White migration into zip codes fails to significantly hurt Obama's vote share—compared to the omitted category which consists mostly of Asian-Americans—though black migration into zip codes does correspond with higher Obama vote share. Most importantly, including these variables gives a similar result for the key delinquency coefficient, indicating that white outmigration does not give rise to the delinquency effect.

snapshot on November 1 in our loan delinquency data. Average income over the year is thus adequate for assessing baseline income but not for assessing the short-run change before the election.

Ceiling Effects

Another alternative explanation for these findings arises from ceiling effects. Some urban areas of California, such as San Francisco, voted Democratic at higher rates before 2008 and also largely avoided the housing crisis. If strongly Democratic areas could not shift much more in 2008 because they were up against a vote share ceiling and these areas *also* happened to avoid the housing crisis, then ceiling effects could make it appear as if delinquencies helped the Democratic ticket.

To address this concern, Figure 5 reproduces the Figure 2 scatterplot—change in Democratic presidential vote share by the percent of mortgages 90 days delinquent—but does so separately for each quartile of 2004 Democratic presidential vote share. The picture shows that delinquencies corresponded with a greater shift towards the Democrats across all quartiles, with the effect being of similar strength in the first three. As expected, the slope is less steep in the fourth quartile (the most Democratic zip codes), though still present. The figure also reveals that this quartile experienced similar delinquencies to other areas, on average. The mean delinquency rate in the fourth quartile of 2008 Democratic vote was 8.28, compared to 8.34 for the others. In fact, the least Democratic zip codes (quartile 1) experienced the lowest level of delinquencies, with a mean of 7.63. In the SI, we show that the delinquency-vote relationship also holds in each household-income quartile and each white-percent quartile (see SI Section 3). In short, ceiling effects in heavily Democratic neighborhoods do not appear to be driving our results.

[Figure 5 about here]

Placebo tests

Although the delinquency effect survives the inclusion of a variety of controls and does not result from ceiling effects, we remain concerned that delinquencies could be capturing the effect of an omitted variable. To address this possibility further, the SI reports placebo tests which show that post-election delinquencies fail to explain increased Democratic vote share (see Section 4). These results further confirm that the pre-election delinquencies drive election outcomes rather than picking up the effect of omitted variables.

Variation across Types of Loans and Borrowers

Mortgage delinquencies reached historically unusual levels in 2008 and 2009 and the housing crisis received considerable media attention. Consequently, voters' ability to hold the president's party accountable for local economic conditions may be unusual in this election. As we showed in Figure 3, however, our findings are not unique to mortgage delinquencies. In fact, they hold for all types of loans except student loans (see footnote 10). To further show this finding, Table 2 replicates the analyses in Table 1, except we now use the average of nonmortgage loans from Figure 3 as the independent variable. The results are similar to those in Table 1. Shifting from zip codes with the lowest delinquency rate to those with the highest corresponds with a Democratic gain from 2004 of about five to eight percentage points in the presidential vote. In the SI, we also show similar findings when we use the average of all loans (mortgage and nonmortgage) with equal weight to all loan types and when we use an index based on principal component factor analysis (see Section 5).

[Table 2 about here]

To show just how consistently the different types of loan delinquency predict vote share, Figure 6 presents the effect of 90-day delinquencies in 2008 for each of the 11 types of loans. Each coefficient in this figure shows the effect estimated in a separate model that includes controls for demographics and baseline income (the model in column 3 of Tables 1 and 2). Since we have rescaled each variable to vary between zero and one, the coefficients show the effect of shifting from the zip code with the lowest to the highest rate of delinquencies. For all loan types except student loans, the effects are statistically significant and similar in size, ranging from 5 to 10 percentage points. Home-equity revolving loans have the largest coefficient. Since these provide consumers with liquidity at lower rates than credit cards, consumers will often continue to make payments on them even after they have defaulted on first mortgages (Goodman et al. 2010, 28).¹⁵ They thus may have the largest estimate because they capture severe financial distress even better than delinquencies on other loans. The results also are robust to other thresholds for loan delinquency such as 30+, 60+, or 120+ days. In the SI, we show the results for 30+ day delinquencies (see Section 5).

[Figure 6 about here]

Of course, delinquencies on nonmortgage loans may only predict vote because they correlate with delinquencies on mortgage loans. To examine this possibility, we estimated a model with both the index of mortgage delinquencies and the index of nonmortgage and nonstudent delinquencies. Both measures of delinquency predict a higher Democratic vote share. In models without clustered standard errors, both enter significantly. In the more conservative models with clustered standard errors, only mortgage delinquencies are statistically significant.

¹⁵ They may also do so because these “second liens” rarely allow for borrowers to simply walk away from the loans without the lender being able to seek full payment through the legal system.

Finally, we note that placebo tests confirm that post-election delinquencies for nonmortgage loans enter with a near-zero coefficient, further suggesting that the estimates in Table 2 reflect the effect of the local economy on presidential voting rather than omitted variables (see SI Section 5).

Another finding also suggests that the effect of the local economy comes from economic distress, not just from neighborhoods being blighted by foreclosures. Given the greater degree of foreclosures before Election Day for subprime borrowers (see below), 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 incumbent. In fact, however, we find that delinquencies by prime borrowers—those above 660 at the time of loan origination—matter considerably more. Moreover, prime delinquencies matter more even in zip codes where prime loans are less common. We present this intriguing result in the SI (Section 9, especially 9.5). It implies that the delinquency-vote relationship arises, not because of the severity of the subprime crisis in hard-hit neighborhoods, but because delinquencies capture financial distress. Delinquencies by prime borrowers may especially capture distress because their credit histories show that they have rarely missed loan payments in the past. These results are also consistent with Bartels’s (2008, ch. 4) finding that higher-income individuals hold the president more accountable for election-year income growth than do lower-income individuals.

Measurement Error: Simulation Evidence

These results from the population of all loans suggest that the local economy had an important effect on 2008 presidential voting. Our hypothesis is that measurement error explains

the striking difference between these findings and the results obtained with standard local economic aggregates such as the county unemployment rate. To examine this issue, we now turn to the following question: Would we be able to detect the effect of the local economy if, instead of the 100% sample of loans, we had only a sample with relatively few observations in each zip code? We address this question by randomly sampling shares of the loan data and repeating our analyses. We find that the effects we identify for the population of all loans would be rendered undetectable in a dataset that sampled only a small share of the population.

As described earlier, sampling error in local economic aggregates will usually bias the coefficient on local unemployment or income towards zero. The problem of measurement error (of which sampling error is one important case) biasing regression coefficients towards zero is well understood by econometricians (e.g., Angrist and Pischke 2009).¹⁶ In the extreme, small samples of respondents from a geographic area will yield estimates of local economic conditions that are largely noise. Regressing vote share on a *measured* unemployment rate that is mostly noise will give a near-zero coefficient even if we would observe a strong relationship between election results and the *actual* unemployment rate.¹⁷

To investigate the impact of sampling error, we conducted a simulation. Instead of using all the data to estimate the models, as we did above, we randomly sampled a fraction of the loan

¹⁶ As Angrist and Pischke (2009, 114) describe, “One of the most important results in the statistical theory of linear models is that a regression coefficient is biased toward zero when the regressor of interest is measured with random errors (to see why, imagine the regressor contains only random error; then it will be uncorrelated with the dependent variable and hence the regression of y_i on this variable will be zero).”

¹⁷ Looking at changes over time in noisy indicators of economic performance such as income growth will exacerbate any measurement error in an economic measure at any one point in time (Angrist and Pischke 2009), since measurement error comes from two different sources. Please see Section 6 of the SI for details.

data, attempting to mimic a survey randomly sampling a small percentage of Americans. We do so at the individual-loan level for various percentages of all loans in the data, such as 0.1%, 1%, etc. After we randomly drew each sample, we then examined how large of an effect mortgage delinquencies would appear to have on voting by re-estimating the model in Table 1 (column 3).

Figure 7 presents the results. On the horizontal axis, it shows the percentage of loans we sampled, ranging from 0.1% to 25%. For each percentage, we randomly drew 200 samples, estimating the effect in each one—the vertical axis plots the median and interquartile ranges for these estimates. As the figure reveals, a sample of 0.1%, which approximates the CPS, finds barely any effect, with a median estimate of only 0.016. The estimate rises rapidly as the sample size increases. The results imply that surveys would have to be truly massive to detect local economic voting. Figure 7 suggests that a 2% sample (2 million households in the US) would allow researchers to find about half of the effect, and a 4% sample (4 million households) would allow researchers to detect about two-thirds of the effect. In sum, a sample of even several hundred thousand households would be too small. To see the local economy’s impact clearly requires datasets much larger than standard samples.

[Figure 7 about here]

Local Economic Voting and the Origins of the Recession

Before examining whether the main results generalize to other elections and other states, we vet our findings in another way by considering the long run of decisions that households made in the years before the 2008 election. In a series of provocative articles, Mian and Sufi examine the origins of the Great Recession, using data largely unavailable for earlier recessions (Mian and Sufi 2009; Mian and Sufi 2010a; Mian and Sufi 2010b; Mian and Sufi 2011). One of

their key results is that the recession began first in counties with the greatest increases in household debt relative to income between 2002 and 2006 (Mian and Sufi 2010b). These counties showed a sharp relative decline in durable consumption, falling house prices, rising delinquencies, and rising unemployment before Election Day in 2008. The recession then spread to other counties, they find, but did so primarily after the election.

If Mian and Sufi's findings hold up at the zip-code level in California, we should therefore be able to predict which zip codes experienced the recession by Election Day: those with the largest increases in leverage between 2002 and 2006. We can then test whether these zip codes shifted their votes away from the Republican Party in 2008 more so than did others. If they did, it would further confirm our findings because these predictions stem from an understanding of the origin of the recession and rely on data from long before the election.

The data make it possible to measure increases in the debt-to-income ratio in the loan data because, as described above, the data include loan origination dates. For example, we observe the number of loans in a zip code originating in the first quarter of 2002. Consequently, we can measure the increase in debt across all loan types from 2002 to 2006. We combine this debt increase with zip-code income from the IRS to estimate the change in the debt-to-income ratio during that time (for details on these calculations, see SI Section 7).

Figure 8 reveals that the Mian and Sufi result for delinquencies successfully replicates at the zip-code level in California. It shows a strong relationship between the 2002-2006 increase in leverage and mortgage delinquencies in 2008 (weighted by zip-code population). In fact, the leverage increase explains more than 40% of the variation in delinquencies. In zip codes with the smallest increases in leverage, only about 2.5% of mortgages were delinquent in 2008. In zip codes with the largest increases, however, the mortgage delinquency rate averaged 15%. As we

show in the SI, a zip code's demographics or baseline income does not account for this pattern, consistent with Mian and Sufi's (2009) finding that the credit expansion was driven by increases in credit availability rather than credit demand. Moreover, we also find that leverage increases before 2002 have little effect, suggesting that only increases during the unusual 2002-2006 period matter (see SI Section 8.1).

[Figure 8 about here]

Did zip codes with the greatest leverage increases also shift their vote most against the Republican ticket and towards the Democratic ticket in 2008? To answer this question, we reestimate the models in columns 1, 3, and 4 of Table 1, but replace the delinquency variable with the change-in-leverage variable. We report these reduced form estimates in Table 3. When we control for demographics as well as immigration, the zip codes with the largest increases in leverage do indeed punish the Republican ticket more in 2008, shifting an additional 4.9 percentage points towards the Democratic presidential ticket, an effect that is highly statistically significant.¹⁸ Increases in household debt from 2002 to 2006 hurt the incumbent party later when people started to default on those loans.¹⁹

[Table 3 about here]

¹⁸ Since the relationship between the leverage increase and later mortgage delinquencies is so strong, the leverage increase could serve as an instrument for mortgage delinquencies in the voting regression. However, while the first stage *t*-statistic is over the threshold of roughly 10 that puts instruments in the "safe zone" with regards to strength (Stock, Wright, and Yogo 2002; Angrist and Pischke 2009), the leverage increase is not randomly assigned. As a result, we treat the instrumental variables (IV) estimates with extreme caution. Reassuringly, the IV results are similar to those reported in Table 1 (see Section 8.2 of the SI).

¹⁹ While the increase in debt from 2002 to 2004 strongly predicts 2008 presidential voting, it does not have an effect on 2004 presidential voting in the zip code, which took place before the debt increase had led to economic downturn in many areas. This result suggests that the debt increase impacted 2008 voting through its effect on later economic distress. Please see the SI for details (section 8.3).

There are at least three explanations for the surge in debt, each of which leads to a different interpretation of the impact of loan defaults on voting decisions. One is that residents began in 2002 to expect their incomes to rise, leading them to borrow more. Mian and Sufi (2010b) cast doubt on this explanation, however, by showing that incomes were actually falling, not rising, in counties with the greatest debt increases, a result we have replicated for California zip codes. A second explanation is that residents in these areas experienced self-control problems. When finance innovations led to an increase in the credit supplied to subprime borrowers, many borrowed money they were unlikely to ever repay.²⁰ In this scenario, voters may have in part brought the crisis on themselves, then punished the Republican ticket for misery of their own making. A third explanation is that lenders exploited the lax regulatory environment to deceive consumers into borrowing beyond their means. In this scenario, voters may have rightly punished the incumbent Republican Party for its failure to police the lending industry. One subprime lender estimated that at least half of borrowers in default did not fully understand what they were getting into, but that the other half did (Bitner 2008, 133), suggesting that both of the latter two explanations played a role.

Altogether, the data on loan origination show that much of the crisis's impact on the election outcome was rooted in loans made years earlier. Before examining whether our main findings on delinquencies generalize to other elections and local economic measures, we briefly summarize the key results. In contrast with previous research, we find that voters hold the

²⁰ Mian and Sufi (2011) find that home-equity borrowing was not on average used to pay down expensive credit card balances, even among those with a heavy dependence on credit card borrowing, nor was it used to purchase new homes or investment properties. "Given the high-cost of keeping credit card balances," they conclude "this result suggests a high marginal private return to borrowed funds," meaning that borrowers used home-equity borrowing to finance purchases of consumption goods.

president's party accountable for local economic conditions as measured through delinquencies. The finding is robust to controls for demographics, migration, and ceiling effects—and placebo tests help rule out concerns about any omitted variables. The effect holds for almost all loan types and is larger for revolving mortgage delinquencies, for prime borrowers, and in prime neighborhoods. Additionally, simulations showed that the findings would be smaller or even undetectable in the kinds of samples ordinarily available to researchers. Last, by shedding light on the origins of economic distress in 2008, the findings on leverage increases long before the election further confirm that financial hardship lies behind the delinquency-vote relationship.

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% of all jobs in the United States. In contrast to the datasets that voting scholars have generally used to measure economic conditions, the population-based QCEW measures local conditions without sampling error. The QCEW are available back to 1990 (Konigsberg et al. 2005).

QCEW: Elections from 1990-2012

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 growth rates for these two variables. We do so for the six months before each presidential election. The average of this economic measure between 1990 and 2012 is 2.1% with a 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 transform the measure so that zero corresponds to roughly the minimum value and one to the maximum. Because of outliers, we set the 0.1 percentile, which is -28%, to zero, and the 99.9 percentile, which is 34%, to one. 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 4 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.

[Table 4 about here]

To examine whether this effect is robust, Table 5 presents a series of checks. It shows only the key coefficient and standard error for the economic conditions measure—the mean of employment and wage growth—from Table 4. In each specification, unless otherwise noted, we refer to the most saturated version of the specification reported in column 4. 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 next row of Table 5 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.

[Table 5 about here]

Next, we show that the results are also not sensitive to weighting the data by the number of registered voters by presenting 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 5. 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 SI presents additional robustness checks (Section 11).

Incorporating earlier years: Elections from 1976-2012

The forerunner to the QCEW, the Universe Database (UDB) is available with less detail and some data problems back to 1975, problems that led to the development of the QCEW (see Konigsberg et al. 2005). While the earlier data lack information on six month changes in wages and employment, they do have nearly complete yearly employment and wage totals for the years after 1977. To estimate the impact of the local economy back to 1976, we pool the two data sets and calculate the change in average wages and employment in the year before the election rather than in the six months before. The results are broadly similar. If we estimate the regression in

column 3 of Table 4, we obtain a coefficient of 4.95 (standard error of 0.85), similar to the estimate for the later data.

In Figure 9, we present the estimates for each of the ten elections from 1976-2012, using the regression model in column 3 of Table 4. To obtain these estimates, we interact the economic measure with the year effects. We also include state-year effects so that the estimates are identified by variation within states in a given year. Despite potential problems with the earlier data, we find a reasonably consistent effect of the local economy, with the exceptions of 1976 (wrong sign) and 1992 and 1996 (essentially zero). The wrongly-signed estimate for 1976 may arise because the data are incomplete at the county-level until 1977, and approximately 23% of counties are missing in 1976. The confidence intervals narrow in later years when data quality is higher. Over the last ten elections, the data reveal a consistent pattern of the local economy having an impact on presidential voting.

[Figure 9 about here]

Conclusion

Until recently, researchers have lacked the data needed to detect economic voting in all its forms. Two datasets new to voting researchers—the status of payments on all consumer loans in California and the Quarterly Census of Employment and Wages—provide insights into presidential voting that were not visible in standard sample-based economic measures. In particular, we find that the local economy has an impact on presidential election outcomes, an effect that required uniquely-detailed data to properly see.

These findings have important implications for democratic accountability. They suggest that presidents face incentives to boost the economy in politically important regions even when

doing so harms the economy overall. Examples abound, from long-standing agricultural subsidies to the recent 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.

The two otherwise very different datasets that we analyze share one crucial feature: they are based on the population of all (or nearly all) economic actors rather than just a sample of individuals. In fact, we show that sampling error would render the impact of the local economy close to undetectable in sample-based datasets commonly used by scholars. Much as physicists and cosmologists needed sufficiently-sensitive measurement tools to test fundamental theories about the natural world, the results suggest that economic voting only comes into complete view once we have the data needed to see it.

Moreover, the variety in the two datasets speaks to the generality of the results. Even though scholars have argued that the 2007-2009 housing shock offers a window into consumer and political behavior more generally (Mian and Sufi 2010a; Mian, Sufi, and Trebbi 2010), findings based on this shock may or may not generalize to other elections because it was so unusual.²¹ For example, media coverage of the subprime crisis and foreclosures may have helped

²¹ There are several reasons to believe the results from the loan delinquencies generalize, in addition to our results based on the QCEW. 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 Figures 3 and 6). Second, media coverage focused on subprime areas devastated by foreclosures, with abandoned homes and dark streets, but our findings show that it was particularly delinquencies amongst prime borrowers, not subprime borrowers, that cost the Republicans votes on Election Day. Third, recent work using precisely-measured local data has found that a variety of other local factors influence presidential election outcomes, including

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 results do generalize.

How large are the effects? They are probably about one-third the size of the national economy's effect on presidential vote share. Examining elections between 1952 and 2004, Bartels (2008, 103) finds that a shift from the worst (1980) to the best (1984) election-year economy corresponds with about a 30 percentage point increase in the incumbent party's vote margin. In contrast, our estimates are generally shy of a 10 percentage point effect, despite substantially more variation at the local level than at the national level. Consequently, even though the local economy matters, the national economy matters considerably more. At the same time, national economic effects may to a significant degree reflect the influence of local factors that are correlated with national ones.

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 blame the president for poor regulation (Hill, Herron, and Lewis 2010). In other years, however, regional variation seems largely haphazard, and yet voters nevertheless continue to hold the president accountable. Such behavior accords with evidence suggesting that voters hold the president accountable for events beyond the president's control, ranging from

distributive spending (Healy and Malhotra 2009; Chen 2013), trade-induced layoffs (Margalit 2011), and war casualties (Grose and Oppenheimer 2007; Karol and Miguel 2007).

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 personal 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 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, we showed, had its roots in household debt increases that began as much as six years before the election. As we discussed earlier, these increases may occur because lenders duped people 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.

References

- Achen, Christopher H., and Larry M. Bartels. 2004. "Blind Retrospection: Electoral Responses to Drought, Flu, and Shark Attacks." Manuscript. Princeton University.
- Angrist, Joshua David, and Jorn-Steffen Pischke. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton: Princeton University Press.
- Ansolabehere, Stephen, Marc Meredith, and Erik Snowberg. 2011. "Macro-Economic Voting: Local Information and Micro-Perceptions of the Macro-Economy." Manuscript.
- Bartels, Larry M. 2008. *Unequal Democracy: The Political Economy of the New Gilded Age*. Princeton, NJ: Princeton University Press.
- Bitner, Richard. 2008. *Confessions of a Subprime Lender: An Insider's Tale of Greed, Fraud, and Ignorance*. Hoboken, N.J.: John Wiley & Sons.
- Books, John, and Charles Prysby. 1999. "Contextual Effects on Retrospective Economic Evaluations the Impact of the State and Local Economy." *Political Behavior* 21 (1): 1-16.
- Chen, Jowei. 2013. "Voter Partisanship and the Effect of Distributive Spending on Political Participation." *American Journal of Political Science* 57 (1): 200-17.
- Cho, Wendy K. Tam, and James G. Gimpel. 2009. "Presidential Voting and the Local Variability of Economic Hardship." *The Forum* 7 (1): 1-21.
- Eisenberg, Daniel, and Jonathan Ketcham. 2004. "Economic Voting in US Presidential Elections: Who Blames Whom for What." *Topics in Economic Analysis & Policy* 4 (1): 1-23.
- Erikson, Robert S. 1989. "Economic Conditions and the Presidential Vote." *American Political Science Review* 83(2): 567-73.

- Fair, Ray C. 1978. "The Effect of Economic Events on Votes for President." *Review of Economics and Statistics* 60 (2): 159-73.
- Farnsworth, Clyde. 1980. "Washington Watch: Farm Lobby' S Gasohol Victory." *New York Times*, December 1, D2.
- Flint, Thomas A. 1997. "Predicting Student Loan Defaults." *Journal of Higher Education* 68(3): 322-54.
- Gerber, Alan, and Neil Malhotra. 2008. "Do Statistical Reporting Standards Affect What Is Published? Publication Bias in Two Leading Political Science Journals." *Quarterly Journal of Political Science* 3 (3): 313-26.
- Goodman, Laurie S., Roger Ashworth, Brian Landy, and Ke Yin. 2010. "Second Liens: How Important?" *Journal of Fixed Income* 20 (2): 19-30.
- Gosnell, Harold F., and William G. Colman. 1940. "Political Trends in Industrial America: Pennsylvania an Example." *Public Opinion Quarterly* 4 (3): 473-86.
- Gosnell, Harold F., and Norman M. Pearson. 1941. "Relation of Economic and Social Conditions to Voting Behavior in Iowa, 1924–1936." *The Journal of Social Psychology* 13 (1): 15-35.
- Grose, Christian R., and Bruce I. Oppenheimer. 2007. "The Iraq War, Partisanship, and Candidate Attributes: Variation in Partisan Swing in the 2006 US House Elections." *Legislative Studies Quarterly* 32 (4): 531-57.
- Hausman, Jerry. 2001. "Mismeasured Variables in Econometric Analysis: Problems from the Right and Problems from the Left." *Journal of Economic Perspectives* 15 (4): 57-68.

- Healy, Andrew J., Neil A. Malhotra, and Cecilia H. Mo. 2010. "Irrelevant Events Affect Voters' Evaluations of Government Performance." *Proceedings of the National Academy of Sciences* 107 (29): 12804-809.
- Healy, Andrew, and Neil Malhotra. 2009. "Myopic Voters and Natural Disaster Policy." *American Political Science Review* 103 (3): 387-406.
- Hibbs, Douglas A. 1987. *The American Political Economy: Macroeconomics and Electoral Politics*. Cambridge: Harvard University Press.
- Hill, Seth J., Michael C. Herron, and Jeffrey B. Lewis. 2010. "Economic Crisis, Iraq, and Race: A Study of the 2008 Presidential Election." *Election Law Journal* 9 (1): 41-62.
- Huber, Gregory A., Seth J. Hill, and Gabriel S. Lenz. 2012. "Sources of Bias in Retrospective Decision Making: Experimental Evidence on Voters' Limitations in Controlling Incumbents." *American Political Science Review* 106 (4): 720-41.
- Hufbauer, Gary Clyde, and Sean Lowry. 2012. "US Tire Tariffs: Saving Few Jobs at High Cost." *Peterson Institute for International Economics Policy Brief* 12 (9): 1-14.
- Karol, David, and Edward Miguel. 2007. "The Electoral Cost of War: Iraq Casualties and the 2004 US Presidential Election." *Journal of Politics* 69 (3): 633-48.
- Kim, Jeongdai, Euel Elliott, and Ding-Ming Wang. 2003. "A Spatial Analysis of County-Level Outcomes in U.S. Presidential Elections: 1988–2000." *Electoral Studies* 22 (4): 741-61.
- Konigsberg, Sheryl, Merissa Piazza, David Talon, and Richard Clayton. 2005. Quarterly Census of Employment and Wages (QCEW) Business Register Metrics. Paper read at Joint Statistical Meetings. Minneapolis, MN, August 2005.
- Kramer, Gerald H. 1971. "Short-Term Fluctuations in U.S. Voting Behavior, 1896-1964." *American Political Science Review* 65 (1): 131-43.

- Kramer, Gerald H. 1983. "The Ecological Fallacy Revisited: Aggregate- Versus Individual-Level Findings on Economic and Elections, and Sociotropic Voting." *American Political Science Review* 77 (1): 92-111.
- Krauss, Clifford. 2011. "Ethanol Subsidies Besieged." *New York Times*, July 8, B1.
- Kriner, Douglas L., and Andrew Reeves. 2013. "Presidential Particularism and Divide-the-Dollar Politics." Manuscript.
- Lawrence, Robert. 2010. How Good Politics Results in Bad Policy: The Case of Biofuel Mandates. Discussion paper 2010-10. Belfer Center for Science and International Affairs. CID working paper No. 200, Center for International Development, Cambridge, MA: Harvard University, September 2010.
- Lewis-Beck, Michael S., and Mary Stegmaier. 2000. "Economic Determinants of Electoral Outcomes." *Annual Review of Political Science* 3 (1): 183-219.
- Margalit, Yotam. 2011. "Costly Jobs: Trade-Related Layoffs, Government Compensation, and Voting in US Elections." *American Political Science Review* 105 (1): 166-88.
- Markus, Gregory B. 1988. "The Impact of Personal and National Economic Conditions on the Presidential Vote: A Pooled Cross-Sectional Analysis." *American Journal of Political Science* 32 (1): 137-54.
- Mian, Atif, and Amir Sufi. 2009. "The Consequences of Mortgage Credit Expansion: Evidence from the US Mortgage Default Crisis." *Quarterly Journal of Economics* 124 (4): 1449-96.
- Mian, Atif, and Amir Sufi. 2010a. "The Great Recession: Lessons from Microeconomic Data." *American Economic Review* 100 (2): 51-56.

- Mian, Atif, and Amir Sufi. 2010b. "Household Leverage and the Recession of 2007–09." *IMF Economic Review* 58 (1): 74-117.
- Mian, Atif, and Amir Sufi. 2011. "House Prices, Home Equity-Based Borrowing, and the US Household Leverage Crisis." *American Economic Review* 101 (5): 2132-56.
- Mian, Atif, Amir Sufi, and Francesco Trebbi. 2010. "The Political Economy of the US Mortgage Default Crisis." *American Economic Review* 100 (5): 1967-98.
- Mutz, Diana Carole. 1998. *Impersonal Influence: How Perceptions of Mass Collectives Affect Political Attitudes*, Cambridge Studies in Political Psychology and Public Opinion. Cambridge: Cambridge University Press.
- NYFED. 2013. Quarterly Report on Household Debt and Credit: Report from Microeconomic Statistics Group.
- Staff. 1985. "Cash, Connections Fuel Cause." *Washington Post*, December 8, H7.
- Stock, James H., Jonathan H. Wright, and Motohiro Yogo. 2002. "A Survey of Weak Instruments and Weak Identification in Generalized Method of Moments." *Journal of Business & Economic Statistics* 20 (4): 518-29.
- Tufte, Edward R. 1978. *Political Control of the Economy*. Princeton University Press.
- Zaller, John. 2004. "Floating Voters in U.S. Presidential Elections, 1948-2000." In *Studies in Public Opinion: Attitudes, Nonattitudes, Measurement Error, and Change*, edited by Willem E. Saris and Paul M. Sniderman, 166–212. Princeton, NJ: Princeton University press.

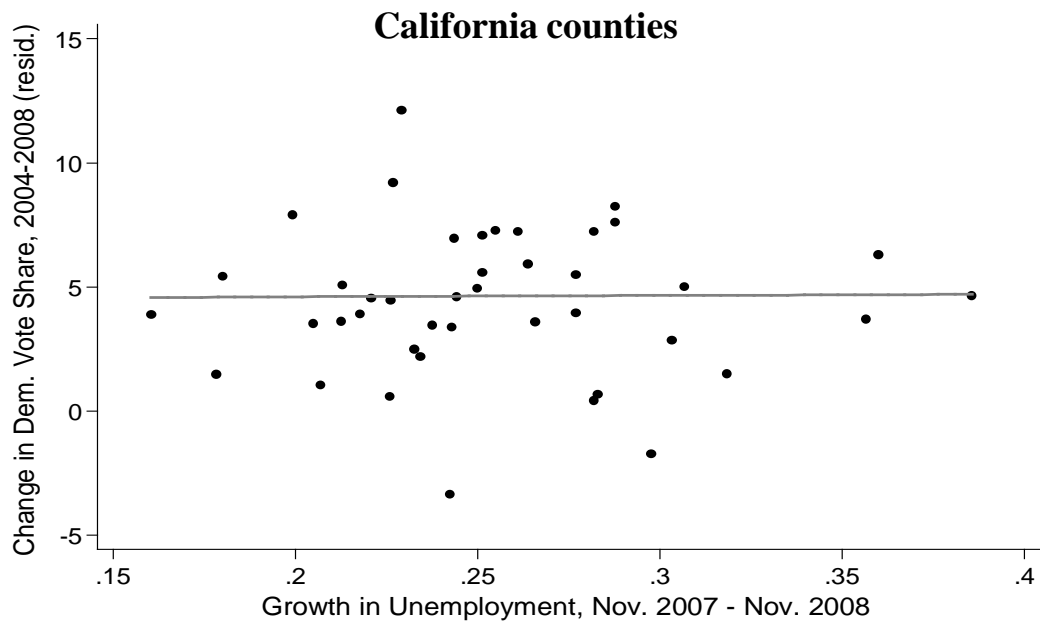
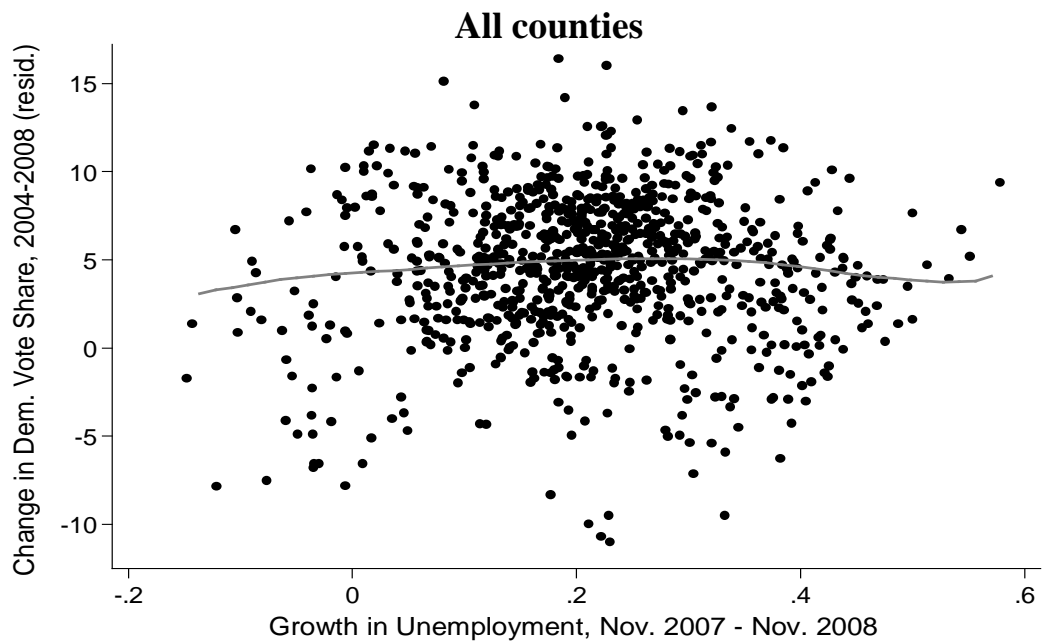


Figure 1: Democratic Vote Share and Unemployment Growth

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). The lpolyc curve is weighted by votes cast in the county. Two outliers for unemployment are dropped from the top plot.

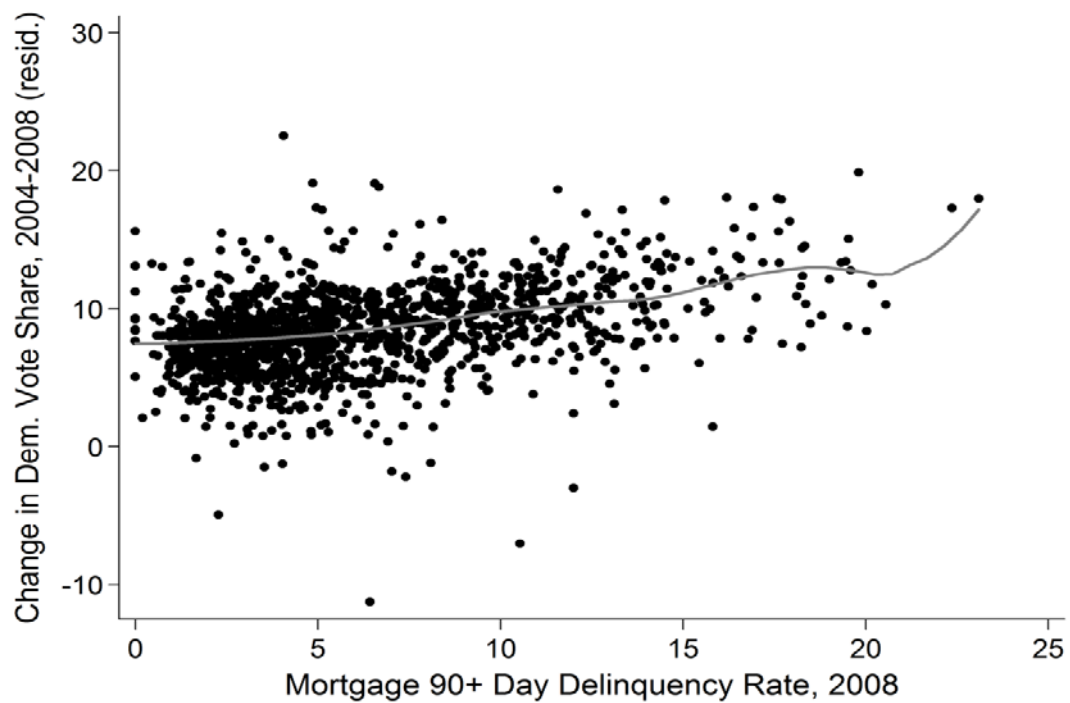


Figure 2: Democratic Vote Share and Mortgage Delinquency

Note: To control for demographics, vote is residualized using the model from Table 1, Column 3. Stata's `lpolyc` command produced the best-fit curve and is weighted by the number of zip-code registered voters.

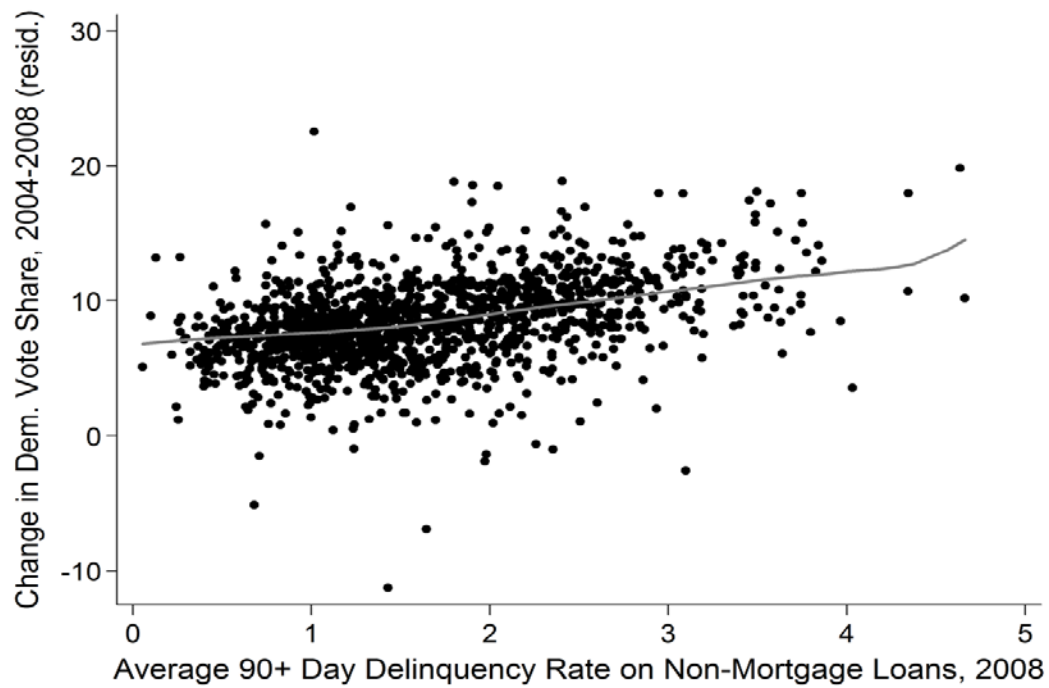


Figure 3: Democratic Presidential Vote and Average Delinquency for Non-Mortgage Loans

Note: To control for demographics, vote is residualized using the model from Table 2, Column 3. Stata's `lpolynomial` command produced the best-fit curve and is weighted by the number of zip-code registered voters.

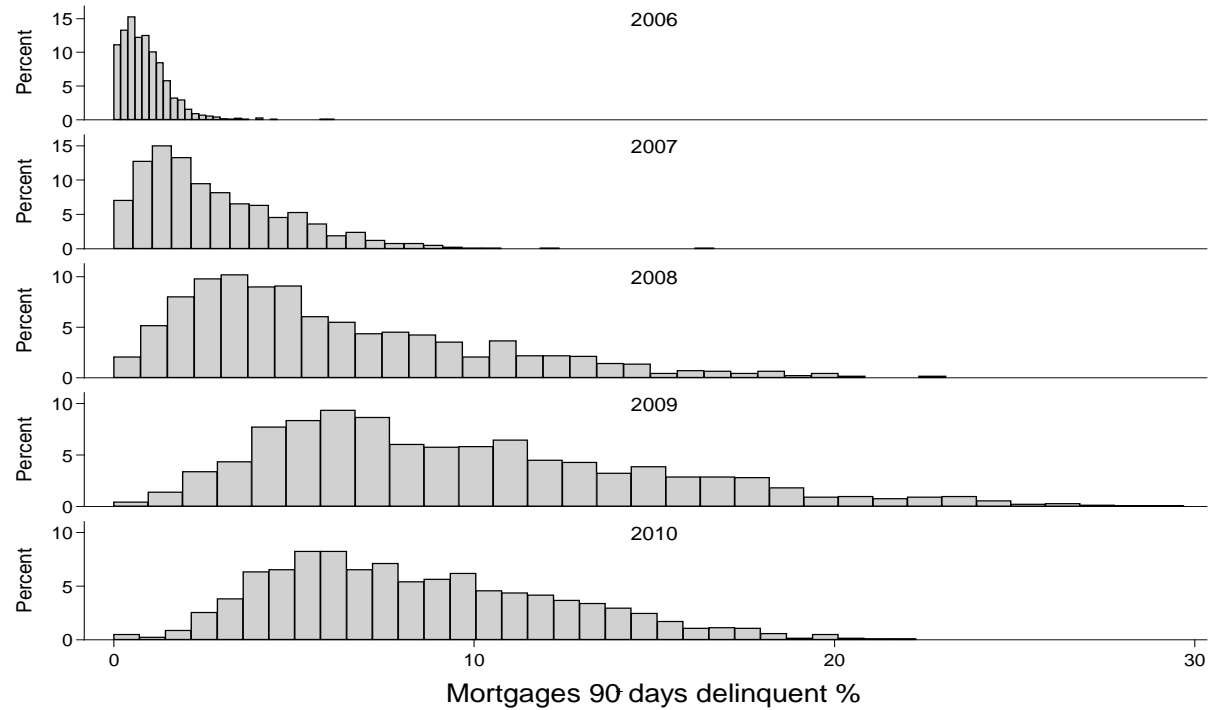


Figure 4: Histograms of the Mortgage Delinquency Rates for California Zip Codes

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

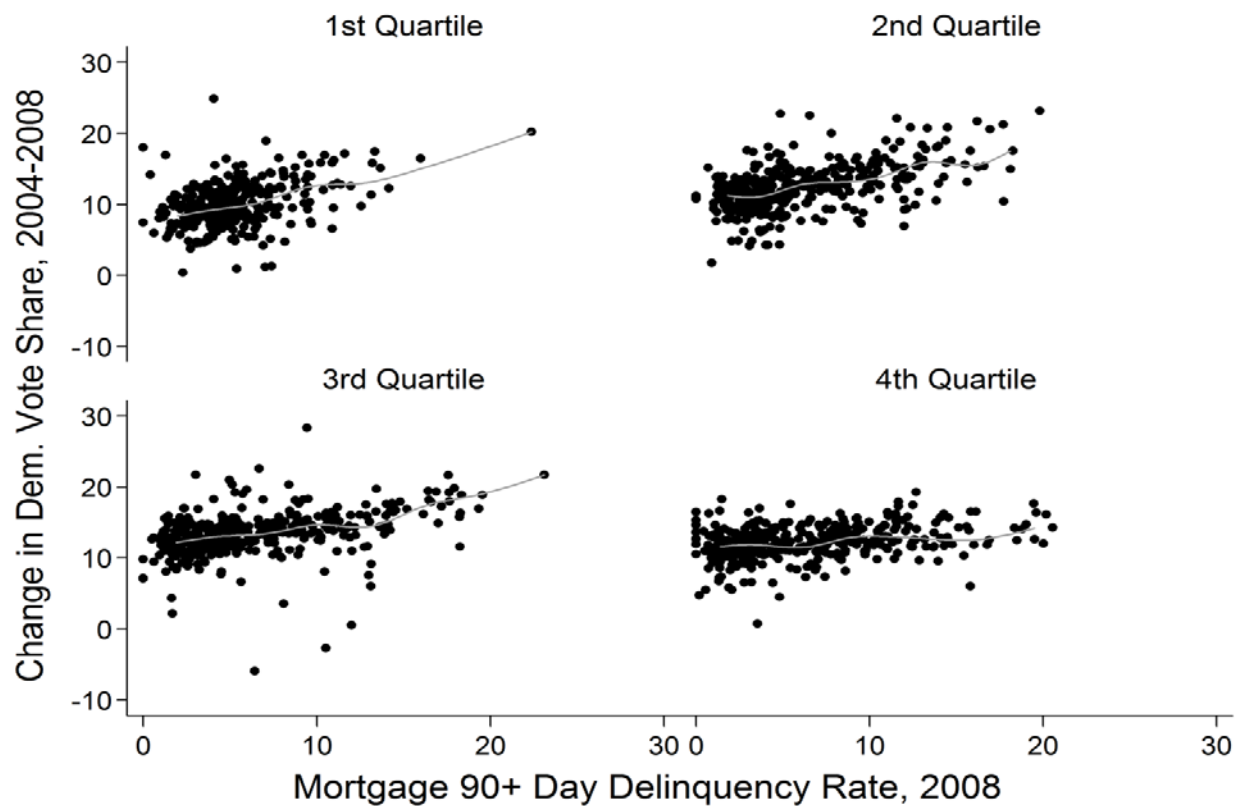


Figure 5: Presidential Voting and Mortgage Delinquency Rates, by 2004 Vote Share Quartiles

Note: Stata's `lpolys` command produced the best-fit curves and is weighted by the number of zip-code registered voters.

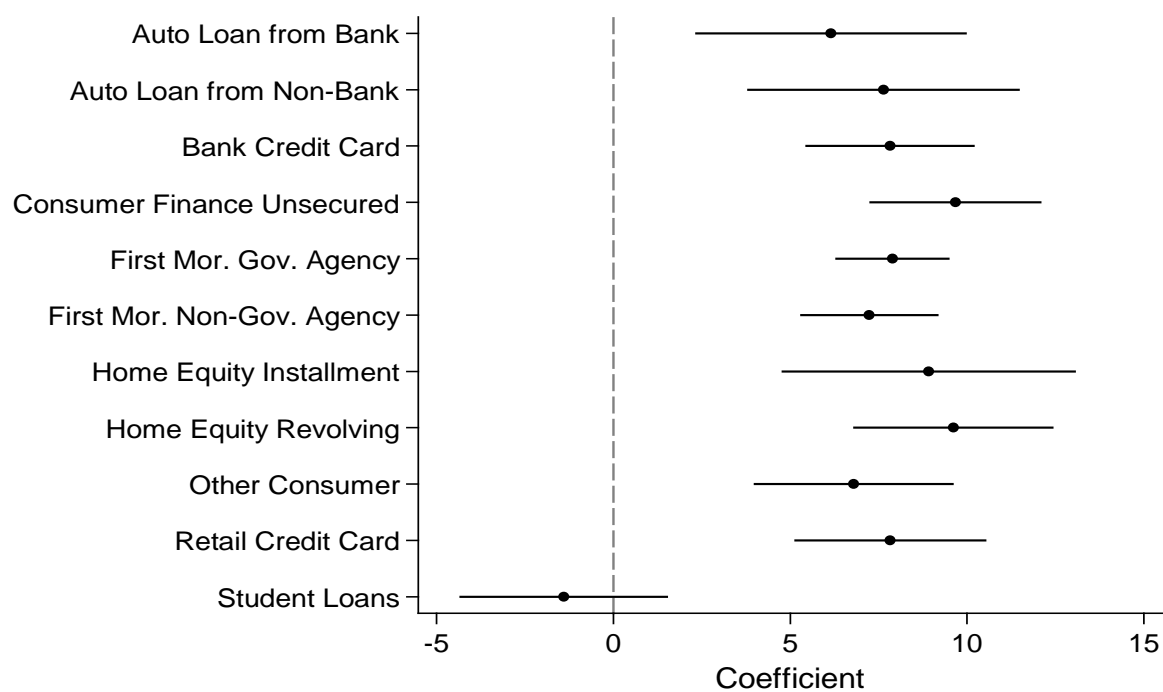


Figure 6: Effect of Mortgage Delinquency on Presidential Voting for Each Loan Type, 2008

Note: Each coefficient is estimated separately using the model in Table 1, Column 3.

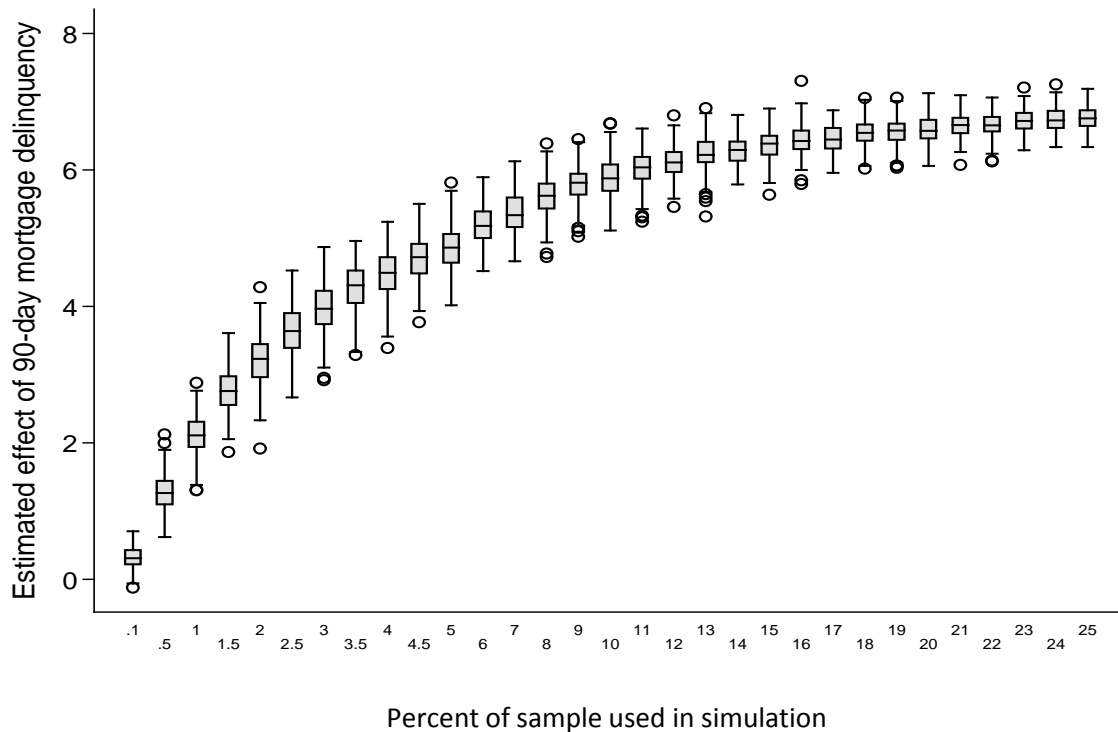


Figure 7: Simulations Showing the Attenuation Bias in Estimates Due to Measurement Error

Note: This figure presents the effect of mortgage delinquencies on the change in vote share (y-axis)—using the model in Table 1, column 3—by the percent of loans sampled in the simulation (x-axis). It shows that sample sizes typically used to estimate county economic statistics, such as the approximately 0.1% sample of the CPS, lead to estimates that vastly underestimate the effect from the 100% sample, which is 6.9. From bottom to top, it shows the lower adjacent value, 25th percentile, median, 75th percentile, and upper adjacent value. It also shows any values outside the lower and upper adjacent values (outliers).

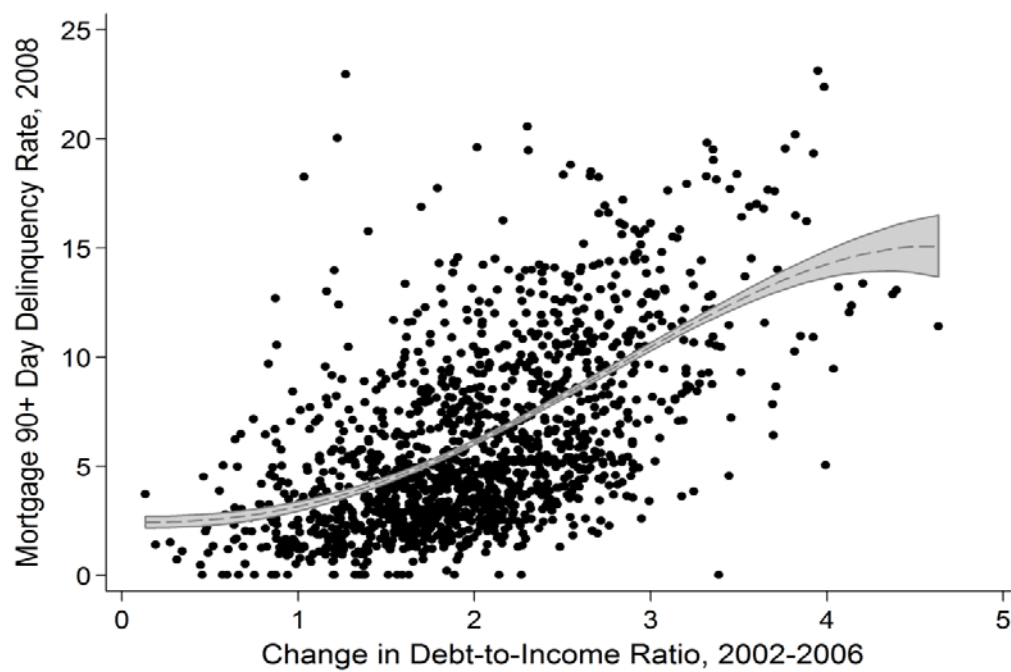


Figure 8: Mortgage Delinquencies and the Increase in Household Debt from 2002-2006

Note: Stata's `fpfitci` command was used to create this graph, weighted by zip-code population. One outlier each on left and right dropped.

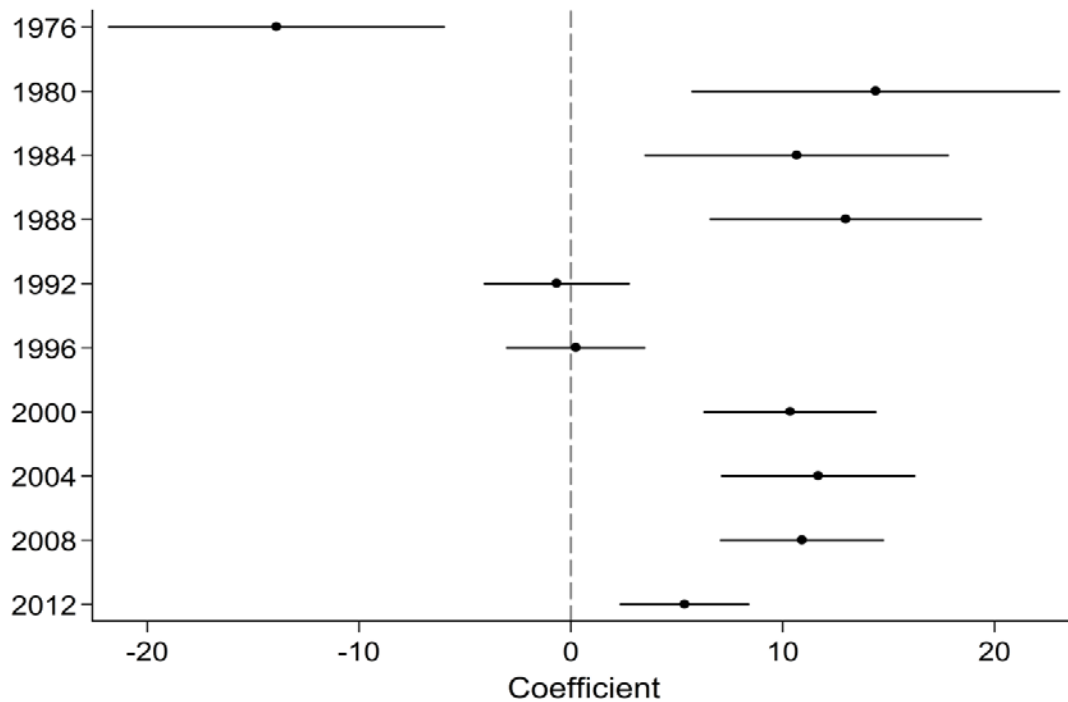


Figure 9: Effects of Wage and Employment Growth on Presidential Voting in Elections since 1976

Note: Coefficients are from a regression model like that in Table 4, Column 3, and includes state*year fixed effects, with standard errors clustered at the county level. As in Table 4, average wage and employment growth is interacted with the Democratic incumbent indicator (1 = Democrat, -1 = Republican). The wrongly-signed estimate for 1976 may arise because the data are incomplete at the county-level until 1977, and approximately 23% of counties are missing in 1976.

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)	(4)
Share of mortgages 90+ days delinquent, 2008	6.76*** (0.78)	5.82*** (0.88)	6.90*** (0.94)	4.68*** (0.96)
Democratic vote share, 2004	0.90*** (0.010)	0.90*** (0.011)	0.89*** (0.0100)	0.90*** (0.0100)
Percent black, 2000		0.0084 (0.019)	0.023 (0.019)	0.057*** (0.018)
Percent white, 2000		0.0027 (0.014)	-0.0020 (0.014)	-0.0052 (0.014)
Percent hispanic, 2000		0.012 (0.011)	0.031** (0.013)	0.047*** (0.014)
Average income, 2001			0.065*** (0.012)	0.064*** (0.011)
Average income squared, 2001			-0.00025*** (0.000062)	-0.00024*** (0.000058)
Average income cubed, 2001			2.4e-07*** (6.9e-08)	2.3e-07*** (6.4e-08)
Black migration percent, 2000-2010				0.27*** (0.048)
White migration percent, 2000-2010				-0.015 (0.019)
Hispanic migration percent, 2000-2010				0.040 (0.031)
Constant	10.6*** (0.78)	10.6*** (1.22)	7.70*** (1.45)	7.20*** (1.51)
Observations	1,386	1,386	1,386	1,386
R-squared	0.983	0.983	0.984	0.985

Note: 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: Delinquencies Across Nonmortgage Loans and Presidential Voting, 2008*Dependent variable: Democratic share of two-party presidential vote, 2008**Level of analysis: Zip code*

	(1)	(2)	(3)	(4)
Share of all nonmortgage loans 90+ days delinquent, 2008	6.55*** (0.85)	4.90*** (1.03)	8.13*** (1.16)	4.77*** (1.09)
Democratic vote share, 2004	0.90*** (0.012)	0.89*** (0.011)	0.89*** (0.011)	0.90*** (0.0099)
Percent black, 2000		0.0028 (0.020)	0.0027 (0.020)	0.046** (0.018)
Percent white, 2000		-0.0023 (0.014)	-0.0090 (0.014)	-0.0087 (0.014)
Percent hispanic, 2000		0.018 (0.012)	0.032** (0.013)	0.052*** (0.014)
Average income, 2001			0.081*** (0.013)	0.071*** (0.012)
Average income squared, 2001			-0.00031*** (0.000067)	-0.00026*** (0.000059)
Average income cubed, 2001			2.9e-07*** (7.4e-08)	2.5e-07*** (6.4e-08)
Black migration percent, 2000-2010				0.26*** (0.054)
White migration percent, 2000-2010				-0.031 (0.021)
Hispanic migration percent, 2000-2010				0.051 (0.032)
Constant	10.3*** (0.89)	10.9*** (1.22)	6.67*** (1.66)	6.60*** (1.61)
Observations	1,386	1,386	1,386	1,386
R-squared	0.982	0.982	0.984	0.985

Note: Nonmortgage delinquencies includes six-loan types and excludes student loans. 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 3: Earlier Increases in Household Debt and Presidential Voting*Dependent variable: Democratic share of two-party presidential vote, 2008**Level of analysis: Zip code*

	(1)	(2)	(3)
Change in debt-to-income ratio 2002-2006	8.65*** (1.61)	6.67*** (1.50)	4.86*** (1.39)
Democratic vote share, 2004	0.91*** (0.011)	0.89*** (0.0090)	0.90*** (0.010)
Percent black, 2000		0.036* (0.018)	0.062*** (0.019)
Percent white, 2000		-0.0024 (0.013)	-0.0080 (0.013)
Percent hispanic, 2000		0.053*** (0.014)	0.059*** (0.015)
Average income, 2001		0.039*** (0.011)	0.049*** (0.0095)
Average income squared, 2001		-0.00012** (0.000051)	-0.00016*** (0.000045)
Average income cubed, 2001		1.1e-07* (5.3e-08)	1.4e-07*** (4.7e-08)
Black migration percent, 2000-2010			0.32*** (0.055)
White migration percent, 2000-2010			-0.0078 (0.021)
Hispanic migration percent, 2000-2010			0.070** (0.028)
Constant	8.62*** (1.04)	7.55*** (1.45)	6.63*** (1.59)
Observations	1,386	1,386	1,386
R-squared	0.981	0.984	0.985

Note: 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 4: The Quarterly Census of Employment and Wages (QCEW) and Presidential Vote, 1992-2012*Dependent variable: Democratic share of two-party presidential vote**Level of analysis: County*

	(1)	(2)	(3)
Average of wage and employment growth*Democratic incumbent	9.74*** (1.26)	6.34*** (0.99)	5.88*** (0.96)
Previous election Democratic vote share	1.03*** (0.0051)	1.00*** (0.0057)	0.91*** (0.0071)
1988 per-capita income		-3.24 (4.84)	6.28 (5.35)
1988 per-capita income squared		0.011 (1.53)	-3.05* (1.77)
1988 per-capita income cubed		0.10 (0.15)	0.40** (0.17)
Percent black			5.60*** (1.23)
Percent white			-5.81*** (1.22)
Percent hispanic			4.88*** (1.00)
Constant	11.0*** (0.69)	15.2*** (4.46)	15.0*** (4.48)
Year effects?	Y	Y	Y
State effects?	N	Y	Y
Observations	18,576	18,575	18,575
R-squared	0.932	0.940	0.946

Note: 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 5: Robustness of Local Economy's Effect on Presidential Elections for QCEW

	Effect of mean of wage and employment growth (SE)
<i>Pre-Election Economy Measure</i>	
Smallest effect excluding elections individually and reestimating	4.1 (0.8)
All controls interacted with election-year indicators	3.6 (0.8)
With lagged annual wage	7.2 (1.1)
With lagged annual wage, squared, and cubed	6.7 (1.1)
With population growth	5.8 (1.0)
Counties with 25,000 voters or more (no weights)	5.0 (1.1)
Counties with 50,000 voters or more (no weights)	5.6 (1.3)
Employment plus wages not mean deviated	2.4 (0.9)
Employment growth	0.8 (1.0)
Wage growth	4.7 (0.9)
With lagged annual wage	5.9 (1.0)
<i>Placebo Tests Using Post-Election Economy Measure</i>	
Regression from Table 6, Column 2	0.8 (0.7)
Regression from Table 6, Column 3	0.5 (0.6)
All controls interacted with election-year indicators	0.7 (0.5)

Note: All models are based on Table 5, Column 2. Robust standard errors clustered at the county level. Weighted by the total number of voters.