

# Economic Distress and Voting: Evidence from the Subprime Mortgage Crisis\*

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## Abstract

We combine nationwide deed-level public records data on home foreclosures with election data and administrative voter data to examine the effects of home foreclosures on electoral outcomes and on individual voter turnout. County-level difference-in-differences estimates show that counties that suffered larger increases in foreclosures did not punish or reward members of the incumbent president's party more than less affected counties. Linking the Ohio voter file to individual foreclosure records, difference-in-differences estimates show that individuals whose homes were foreclosed on were slightly less likely to turn out to vote, rather than being mobilized. However, we do find evidence for a Trump effect; in 2016, counties more exposed to foreclosures supported Trump at substantially higher rates. Taken together, the evidence suggests that the effect of local economic distress on incumbent performance is generally close to zero and only becomes substantial in cases involving unusual candidates who offer heterodox policy views.

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# 1 Introduction

The subprime mortgage crisis had wide-ranging effects on the American economy (e.g., Mian, Sufi, and Trebbi 2015) and, like other large-scale economic crises in the past, has led many to speculate about its role in subsequent political upheaval and the rise of populism in American politics and across the world. A Fox News opinion piece, for example, recently declared that “poor access to housing in America, and the lingering aftereffects of the 2008 housing crash, remains a chief motivator of anger and dissatisfaction amongst an electorate that views the economy and the mortgage market as rigged against ordinary people.”<sup>1</sup> Did the Americans most affected by the housing crash—those whose homes were foreclosed, and those who live in areas where foreclosures were most prevalent—punish incumbents electorally? Has the economic hardship they experienced during and after the Great Recession activated them, politically? These are the key questions we study in this paper.

We use nationwide public records data on individual home foreclosures, combined with county-level election data for the U.S. House, Senate, and President, to study the effects of foreclosures on electoral outcomes. We also use administrative data on all registered voters in the state of Ohio, which allows us to link home foreclosures to individual voter records to examine effects on turnout.<sup>2</sup> To address concerns that the people and places where foreclosures are more likely to occur are unlike the people and places where they are less likely to occur, we employ a series of difference-in-differences designs comparing over-time changes in electoral outcomes and foreclosure rates.

We find precisely estimated null effects on county-level vote share outcomes. In general, increases in local foreclosure rates do not lead the incumbent party to perform worse electorally. This is true for both presidential and legislative elections, and it is true whether we consider 2-year shifts in foreclosure rates or whether we zoom in on foreclosures in the last 6

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<sup>1</sup><http://www.foxnews.com/opinion/2016/07/16/want-to-understand-voter-anger-in-2016-don-t-overlook-housing.html>

<sup>2</sup>Although both home foreclosure information and the Ohio voter file are matters of public record, we are aware of the sensitive nature of this information. After merging records, we remove all personally identifying information from the dataset. Our procedure was approved by Stanford’s IRB.

months before the election.<sup>3</sup> Also, we find small but detectable *decreases* in the propensity to turn out to vote after a home foreclosure.<sup>4</sup> Rather than mobilizing disaffected individuals, economic hardship from the housing crisis appears to have discouraged individuals from participating in politics, on average.

However, follow-up analyses suggest that the effect of foreclosures are substantial in rare cases. Specifically, we find large effects in 2016 when Donald Trump offered a populist departure from the conventional economic policies of both the Democrats and Republicans. These effects are particularly large when we examine foreclosures in the final six months before the election in 2016. Although we cannot definitively establish a particular mechanism, we consider this evidence in favor of what we call the “Stigler hypothesis,” after Stigler’s (1973) claim that, since the parties generally offer the same economic policies there is no reason for economically affected individuals to change votes or to become more politically active.<sup>5</sup> These findings are also consistent with recent work that suggests that, in the United States, adverse economic conditions lead to support for extremists (Autor et al. 2016*a,b*), and for right-wing populist parties across Europe (e.g., Dehdari 2018; Malgouyres 2017).

These findings also comport with Healy and Lenz (2017), which uses zipcode-level mortgage delinquencies in California and relates these to changes in support for the Democratic presidential candidate between 2004 and 2008, finding marked increases in support for Obama in places where the housing crisis was more severe, perhaps in part because Obama

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<sup>3</sup>We estimate effects for presidential, U.S. House, and U.S. Senate elections, but voters could be represented by a different party at the national level than at the state or local level. Recent work has not found a relationship between the state of the local economy and incumbent performance in state (Rogers 2018) or local (de Benedictis-Kessner and Warshaw 2019) offices, perhaps because voters are much less likely to know who the incumbent party is for these offices (Rogers 2018).

<sup>4</sup>This is consistent with the finding that turnout decreases among highly leveraged homeowners when their home prices decline (McCartney 2017).

<sup>5</sup>In general, we might suspect that voter behavior in response to crises, particularly their decision to punish incumbents electorally, depends on the strategic response of politicians (Ashworth and Bueno De Mesquita 2014). In the United States, for example, the mortgage default crisis seems to have influenced the behavior of members of Congress. Representatives in areas with large increases in mortgage defaults were more likely to support the Foreclosure Prevention Act (Mian, Sufi, and Trebbi 2010). This suggests that differences in economic policy, particularly as it pertains to financial regulation and the housing market — between Democrats and Republicans and between incumbents and challengers — are not always so clear, and may adapt to changing economic conditions.

represented an important—if less dramatic than Trump—break from the two parties’ previous economic platforms. In the Appendix, we replicate the California finding from Healy and Lenz (2017); if we zoom in on California in the years 2004 and 2008, we, too, find a modest but positive link between foreclosures and changes in support towards Obama. Although Healy and Lenz (2017) also find a general relationship between local economic conditions and incumbent vote share, their estimates are substantively tiny. Similarly, we find null results when we look beyond California 2004-2008, again suggesting that the effects are only present in unusual circumstances when a challenger offers a sharp break from past policy. In the Appendix, we provide a set of analyses to help unify our study and Healy and Lenz (2017).<sup>6</sup>

Lastly, a growing body of literature seeks to distinguish between economic and cultural explanations for voting behavior in the 2016 election. Economic arguments focus on the role of economic distress or insecurity to explain Trump support (e.g., Autor et al. 2016*b*; Healy and Lenz 2017). Racial or cultural arguments, meanwhile, suggest that negative reactions to progressive value change (Inglehart and Norris 2016), status threat (Mutz 2018), racial resentment (Hochschild 2018), or rural identity politics (Cramer 2016) might explain the shift towards Trump. We do not claim that these explanations are mutually exclusive. In fact, Donald Trump made appeals *both* to working-class concerns and white identity in the 2016 campaign, where economic concerns are activated in conjunction with racial grievances (McCall and Orloff 2017). Given this, and our expectation that different groups of voters will weigh these factors differently (Green and McElwee 2018), we should be hesitant to ascribe either explanation as being the sole cause of increased Trump support. Nonetheless, in this paper we do find that counties experiencing greater economic distress shifted towards Trump at higher rates, suggesting that the 2016 election may have been an uncommon

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<sup>6</sup>Hill, Herron, and Lewis (2010) also estimates some relationships between mortgage delinquencies and support for Obama, finding mixed results, including some evidence for a positive relationship but only conditional on high wage areas.

case where the effects of adverse economic conditions on incumbent party performance were substantial.

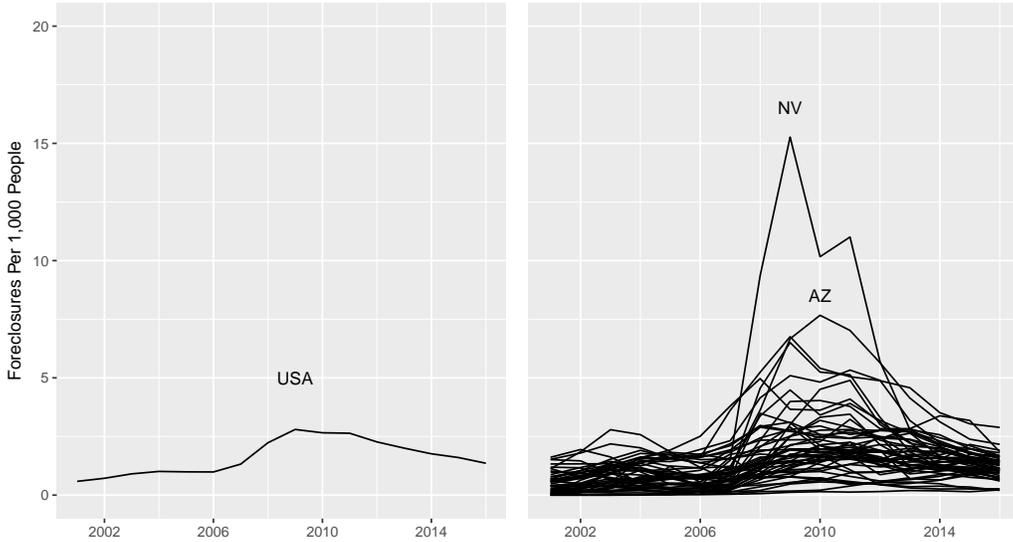
## 2 Studying the Effects of Home Foreclosures

Our data on home foreclosures comes from CoreLogic, a company that compiles information on the housing market culled from public records. The dataset covers foreclosures in the years 2000–2014. After limiting our attention only to deeds held by individuals, we define a foreclosure as any deed that is recorded as entering into a foreclosure, marked in varying cases by a real-estate owned transfer or sale, or by a variety of foreclosure certificates. We do not count a property as foreclosed based on pre-foreclosure events like notices of default or lis pendens. The Appendix provides detailed information on how we process the CoreLogic data. Because CoreLogic data is only available for about half of all U.S. counties, in the Appendix, we also discuss reasons to believe that our results generalize to the full set of U.S. counties. In particular, Table A.3 shows that CoreLogic counties look just like the full set of U.S. counties in terms of important variables like median household income, education, and unemployment.

The events leading up to the Great Recession are well known. Starting in 2007, a large number of home owners defaulted on their mortgages, triggering the failure of several mortgage originators and high-profile financial institutions. During the subprime crisis, foreclosure rates skyrocketed. Figure 1 plots the national and state-by-state average foreclosure rates per 1,000 people over time. The years 2007–2009 saw an unprecedented spike in foreclosures, with especially dramatic increases in Nevada and Arizona, but with increases in many other states as well.

These foreclosures had tremendous consequences. Individuals who experience foreclosure obviously experience significant economic distress, both in the lead-up to foreclosure and in its aftermath. But foreclosures also have substantial spillover effects on local economies.

**Figure 1 – U.S. Home Foreclosures Over Time, CoreLogic Counties.** The left panel shows the national home foreclosure rate over time; the right panel shows the foreclosure rate by state. Both are measured using CoreLogic data, which cover roughly half of U.S. counties, so trends and state levels may vary from nationwide foreclosure data.



Mian, Sufi, and Trebbi (2015) estimate that foreclosures lead to substantial decreases in home prices, residential investment, and consumer demand at the local level. By studying the effects of foreclosures on political behavior, we are therefore able to study two closely related issues. First, we can study the local effects of foreclosures on aggregated vote choice. Here, foreclosures may exert their own force on electoral outcomes, and they are also likely a proxy for the more general economic downturn that local areas experienced. This is important because, much like other forms of economic distress like unemployment, foreclosures on their own do not affect nearly a large enough number of people to change overall electoral outcomes.<sup>7</sup> If foreclosures drive aggregate political outcomes, it likely depends on the more widespread economic effects that foreclosures catalyze. Second, within counties, we can study the effects of home foreclosures on individual voter participation.

<sup>7</sup>Considering the case of unemployment, (Stigler 1973: 162) offers a hypothetical 3 percentage-point change in unemployment, and writes: “The 3 percent of unemployed, largely concentrated in the young, unmarried, and less educated, has substantially less than 3 percent of the vote.”

For the aggregate analysis on vote share, we sum up the total number of individual foreclosures in a given county in each year, and we divide by the county’s population in that year, as estimated from the Census. We linearly interpolate county population for non-Census years. We then take the average foreclosure rate within an electoral cycle—a four-year period, when we study presidential elections, and a two-year period, when we study legislative elections (House and Senate). This information is then merged, by county and election period, with data on county-level vote share for these offices, which comes primarily from Dave Leip’s Atlas of U.S. Presidential Elections.<sup>8</sup>

Table 1 presents descriptive statistics for these county-level datasets. Panel A covers the presidential elections dataset, where each time period is a four-year presidential term; Panel B covers the legislative elections dataset, where each time period is a two-year congressional term. Because we do not have foreclosure data past 2014, we do not use 2016 legislative elections in our analyses, but we keep the 2016 presidential elections in the analyses by using foreclosures from 2012–2014. In addition to raw means, the panels also show population-weighted means, which accounts for the well-known fact that Republicans dominate the low-population rural counties of the U.S. while Democrats dominate the high-population urban counties.

On average, counties experience 1.10–1.11 foreclosures for every 1,000 people (column 1, Panels A and B). As column 2 shows, these averages go up when we weight by population—foreclosures are somewhat more common in more populous, urban areas. As these numbers show, foreclosed individuals are a small fraction of the population, an important fact when considering potential electoral effects at the aggregate level. Foreclosure rates run from a minimum of 0—in certain low population counties—to a maximum of 18.76 over a four-year period (Panel A). This maximum value comes from Archuleta County, Colorado, over the four-year period from 2008 to 2012. When we zoom in on two-year periods in Panel B, we

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<sup>8</sup>County-level vote shares for the November 2016 election are not yet available through Dave Leip’s Atlas of U.S. Presidential Elections, so 2016 vote shares for presidential races come from Politico’s reported results (see [https://github.com/Prooffreader/election\\_2016\\_data](https://github.com/Prooffreader/election_2016_data)).

**Table 1 – Descriptive Statistics, County Level, 2004–2016.**

	Mean (1)	Pop-Weighted Mean (2)	Standard Deviation (3)	Minimum (4)	Maximum (5)	Observations (6)
A. Presidential Elections						
Foreclosure Rate	1.27	1.60	1.41	0.00	19.97	9,541
Dem Vote Pct	39.26	52.28	15.05	4.83	95.75	9,541
County Population	120,497	–	354,202	110	10,139,013	9,541
B. Legislative Elections						
Foreclosure Rate	1.29	1.61	1.50	0.00	21.94	18,187
Dem Senate Vote Pct	42.14	53.39	19.14	0.00	100.00	12,282
Dem House Vote Pct	39.25	50.87	21.94	0.00	100.00	18,187
County Population	124,393	–	361,912	95	10,200,720	18,187

*Note:* Panel A presents descriptive statistics for the merged dataset on presidential elections. In this panel, every observation is a county within a four-year presidential election period. Panel B presents descriptive statistics for the merged dataset on legislative elections. In this panel, every observation is a county within a two-year congressional election period. The foreclosure rate is calculated as the mean of annual total foreclosures divided by county population in thousands of people, where the mean is computed over the relevant time period (either the four-year presidential term or the two-year congressional term.) Vote percentages are the Democratic percentage of the two-party vote in a given county. County population is linearly interpolated for non-Census years. The second column provides means that are weighted by county population.

find an even higher maximum (19.25 foreclosures per 1,000 people), which reflects Archuleta County in the period 2010–2012.

Panel A also shows average Democratic presidential vote share. Columns 1 and 2 show the urban-rural divide quite clearly. The raw average Democratic vote share is only 40%; but when we weight by county population, this average increases to over 50%. We see the same phenomenon for the U.S. House in Panel B, though it is more muted for U.S. Senate elections. House and Senate elections, as Panel B shows, include cases where the Democrats received 0 or 100% of the vote—these are uncontested races.

For the individual analysis on voter turnout, we focus on the state of Ohio. Ohio is a good test case because it is a closely contested state, electorally, which also happens to offer administrative data on individual voter turnout at no cost. We obtained the Ohio voter file from the Ohio Secretary of State website, and we joined it to CoreLogic’s individual-level foreclosure data using the full name and county of each individual. The voter file provides four important pieces of information. First, anyone who appears in the voter file is, by

definition, registered to vote. Second, the voter file shows who, among registered voters, actually turns out to vote in each election. Third, the voter file records the party with which each voter has chosen to register, if the voter has elected to register with a party. And fourth, the voter file shows the date on which each individual registered to vote.

In studying the voter file, we face a difficult post-treatment problem. Individuals only appear in the voter file when and if they register to vote, but the choice to register could itself be influenced by home foreclosure. If we study the effect of foreclosures only for the set of people who choose to register, we risk missing many people who either (a) experienced foreclosure, but did not register to vote, and so did not turnout or (b) did not experience foreclosure and did not register to vote, and so did not turnout. We address this issue in two ways. First, we include anyone from the CoreLogic data who experiences foreclosure, whether or not they appear in the voter file; anyone who does not appear in the voter file is recorded as not turning out to vote, as are any individuals who register to vote but do not turn out. This ensures that we do not eliminate people who were foreclosed on but did not vote. Second, we also perform analyses where we include only people who were registered to vote before the period of our study—thus avoiding post-treatment issues. In particular, for these analyses, we study only people who registered to vote in 2003 or earlier.

Table 2 presents summary statistics for this individual-level analysis. Panel A provides these statistics for the full voter file + CoreLogic dataset, while Panel B provides them for the set of people registered to vote before 2004 (we do not provide minimums or maximums in this table because all variables run from 0 to 1). In both panels, the unit of analysis is the individual-year. The probability that any individual is foreclosed on in any given year is 0.004, in the full dataset, and 0.002 among people registered to vote before 2004—this makes sense since we have reasons to suspect that long-time registered voters are relatively more affluent than non-voters or non-habitual voters (Verba, Schlozman, and Brady 1995).

The second row of each panel shows the turnout rate. Among the full dataset, 44% of person-year observations show that the individual turns out to vote (Panel A). This rate is

**Table 2 – Descriptive Statistics, Individual Level, Ohio, 2004–2016.**

	Mean (1)	Standard Deviation (2)	Observations (3)
A. Full Voter File			
Prob of Foreclosure	0.004	0.064	72,845,379
Turnout Rate	0.48	0.50	72,845,379
Dem Registration %	0.16	0.37	71,099,289
Rep Registration %	0.26	0.44	71,099,289
Ind Registration %	0.58	0.49	71,099,289
B. Registered Before 2004			
Prob of Foreclosure	0.002	0.040	25,999,794
Turnout Rate	0.80	0.40	25,999,794
Dem Registration %	0.23	0.42	25,999,794
Rep Registration %	0.41	0.49	25,999,794
Ind Registration %	0.36	0.48	25,999,794

*Note:* Panel A presents summary statistics for all individuals who appear either in Ohio’s voter file or in the Ohio foreclosure records. Panel B presents these same summary statistics only for individuals who registered to vote prior to the timeframe of our study. The unit of analysis for all rows is an individual-year pair. An individual is marked as foreclosed on if she experienced at least one foreclosure at any time in the year up to the election date in early November. Turnout rate is measured as the percentage of all individuals in the dataset who are marked as turning out in the general election. Dem, Rep, and Ind registration percentages are calculated using party registration as provided in the Ohio voter file, and reflect the proportion of registered voters who register with each party. A negligible number of Ohio voters register for third parties. The number of observations in Panel A is slightly larger for foreclosures and turnout rate than for the party registration variables because we include foreclosed individuals who do not register to vote. These individuals are not included in Panel B, so all sample sizes in Panel B are equal.

substantially higher among long-time registered voters (Panel B), as we might expect. The final three rows of each panel show the breakdown of party registration, among registered voters. The majority of registered voters do not affiliate with a party (Panel A), but the majority of long-time registered voters do (Panel B). Among both sets of people, Republican registration is significantly more common than Democratic registration. In the top panel, we drop roughly 1 million person-year observations when we calculate the party registration percentages—these are individuals in the CoreLogic data who do not register to vote, and

thus do not have party registrations. This drop does not occur in Panel B because here we have subset to only people appearing in the voter file.

### 3 Electoral Non-Effects of Home Foreclosures

We first estimate the effects of home foreclosures on incumbent vote share at the county level. Specifically, we estimate equations of the form

$$Dem\ Vote\ Pct_{it} = \alpha Foreclosures_{it} + \beta Foreclosures_{it} \cdot Dem\ Inc_{it} + \gamma_i + \delta_t + \epsilon_{it}, \quad (1)$$

where  $Dem\ Vote\ Pct_{it}$  measures the Democratic candidate's percentage of the two-party vote in county  $i$  at time  $t$ , running from 0 to 100. The variable  $Foreclosures_{it}$  measures total foreclosures per capita in county  $i$  during the period from the last election to the election at time  $t$ . This variable is also interacted with  $Dem\ Inc_{it}$ , which takes the value 1 when the Democratic party held the office in the previous cycle and -1 when the Republican party held it. Finally,  $\gamma_i$  and  $\delta_t$  stand in for county and year fixed effects, respectively. In many specifications,  $\delta_t$  is made more flexible, either using state-by-year fixed effects or population decile-by-year fixed effects. For many cases, including when studying the presidency, the main effect on  $Dem\ Inc_{it}$  is omitted because it is absorbed by the time fixed effects.

As the above specification makes clear, the analysis is a difference-in-differences design in which we compare within-county changes in incumbent and non-incumbent party vote shares over time across within-county changes in home foreclosures. For the resulting estimate to be causal, the trends in vote share in counties with smaller increases (or decreases) in foreclosures must provide valid counterfactuals for the trends we would have observed in counties with larger increases (or decreases) in foreclosures, had these counties instead had smaller increases (or decreases). The main advantage of the difference-in-differences design is that this assumption can be tested. One way to test it, which we do throughout the results below, is to relax the assumption of parallel trends in a variety of ways and see if the results

change. Because the results do not change meaningfully, we conclude that the parallel trends assumption appears to be valid.

Given that we want to study how foreclosures affect incumbent performance, it might seem more logical to use incumbent party vote share, rather than Democratic party vote share, as our dependent variable. This would allow us to forego the interaction term between foreclosures and Democratic incumbency. However, it seems unlikely that counties trend in terms of their general support for incumbents, and far more likely that they might trend in terms of their partisanship. As such, it makes more sense to use the interactive specification with Democratic vote share as the dependent variable, so that we can account for these trends directly. The main quantity of interest is therefore the interaction between foreclosures and the incumbent party variable.

Table 3 presents the estimates for four possible specifications, and we weight each of the estimates using county population weights. The first column is the most simple, in which we use county fixed effects and state-by-year fixed effects—implicitly doing a separate difference-in-differences for each state and averaging the estimates together. Here we find a substantively small, but not statistically significant, *benefit* to the incumbent party in presidential elections when a county suffers more foreclosures. Specifically, an increase of one foreclosure for every 1,000 people in the county is estimated to increase the incumbent party’s presidential vote share by roughly a tenth of a percentage-point. Put another way, it is estimated to take an increase of 10 foreclosures per 1,000 people to move the incumbent party’s vote share by a full percentage-point—roughly 6 standard deviations in the foreclosure rate variable (see Table 1).

We have reasons to be skeptical of this specification. The state-year fixed effects mean that we compare the changes over time in counties with more foreclosures to the changes over time in counties with fewer foreclosures in the same state. These counties may not give us the best counterfactual trend. Counties that experienced big spikes in foreclosures tend to be urban or suburban counties with high populations—the same types of counties

**Table 3 – Effects of Housing Foreclosures on Presidential Elections, County Level, 2004–2016.**

	Dem Pres Vote Pct (0-100)			
	(1)	(2)	(3)	(4)
Foreclosures Per 1,000 People	-0.51 (0.15)	-0.19 (0.11)	0.09 (0.13)	0.01 (0.12)
Foreclosures × Inc Party	0.11 (0.08)	-0.11 (0.07)	-0.02 (0.07)	-0.10 (0.07)
N	9541	9541	9541	9541
# Counties	2840	2840	2840	2840
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.

that have become increasingly Democratic over this same time period. As such, it might be better to use similarly populous counties as counterfactuals, even if they are in different states. We do this in column 2, where we create fixed effects for every population decile-year. That is, we chunk cities into deciles based on their population as of 2003 (before the treatment window), and we create year fixed effects within each population decile, so that our difference-in-differences counterfactual trends come from counties in the same population decile who vary in their foreclosure rates over time. When we do this, the estimates are again substantively small. As column 2 shows, we now estimate that an increase of 1 foreclosure per 1,000 people in a county decreases incumbent vote share by about 0.11 percentage points, or 11 basis points. In this specification, it would take an increase of almost 10 foreclosures per 1,000 people to swing incumbent vote share by a full percentage point—an increase that spans over half of the entire range of the data, where foreclosures per 1,000 people range from 0 to roughly 19.

Besides being substantively tiny, this null result is relatively precisely estimated. The 95% confidence interval for this estimate ranges from roughly -0.33 to +0.11, which mean we can reject any substantively meaningful relationship between changes in foreclosure rates and changes in incumbent electoral support.

The remaining specifications explore the robustness of this null result. We include county-specific linear time trends, and in the Appendix we also explore differences in the results when we do not weight by county population (see Table A.6). In all cases, we continue to find precisely estimated, substantively small effects.<sup>9</sup>

### 3.1 Evidence for a “Trump Effect” of Home Foreclosures

Why don't foreclosures alter localities' aggregated vote choices? One possibility is that voters don't react to foreclosures in their area by altering their support for one party or the other because the parties' positions on policies related to the housing market and to financial regulation are relatively similar, most of the time. One way to investigate this possibility, though it is crude, is to see whether effects seem to be different in the 2016 election. Donald Trump deviated sharply from policy positions traditionally held by both parties, particularly related to free trade, infrastructure spending, student loans, and other economic issues that voters might care about. Whether any of these issues linked to home foreclosures is unclear, but as we have mentioned earlier, upticks in home foreclosures in an area are likely signs of other forms of economic distress. Trump's overall separation on economic policy might make the 2016 election different from previous ones.

In Table 4, we reestimate the effects adding an interaction of the foreclosures variable with a dummy variable for the 2016 election cycle. The results suggest that Trump benefited from foreclosures, electorally. Across specifications, an increase of 1 foreclosure per 1,000 people

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<sup>9</sup>One explanation for the null finding could be that voters anticipate changes in the state of the economy, and that voters would only punish incumbents for *unanticipated* increases in foreclosures (Alt, Barfort, and Lassen 2017). While we cannot measure voters' ex ante expectations about changes in the economy in our data, we expect that most voters would not have anticipated the extent of the subprime mortgage crisis during the Great Recession.

**Table 4 – Effects of Housing Foreclosures on Presidential Elections, County Level, 2004–2016: Testing for Trump-Clinton Effects.**

	Dem Pres Vote Pct (0-100)			
	(1)	(2)	(3)	(4)
Foreclosures Per 1,000 People	-0.31 (0.10)	-0.18 (0.07)	-0.02 (0.10)	-0.12 (0.10)
Foreclosures $\times$ 2016	-1.76 (0.33)	-1.36 (0.20)	-1.81 (0.41)	-0.99 (0.23)
N	9541	9541	9541	9541
# Counties	2840	2840	2840	2840
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.

is estimated to decrease the Democratic vote share (the Clinton vote share) by 0.99 to 1.81 percentage points more in 2016 than in past years (this the range of the interaction coefficient estimates, not their confidence intervals). We should note, however, that these effects are much smaller when we do not weight by county population (see Table A.7). The larger effects in the weighted specifications suggest that, if foreclosures led voters to punish Clinton (and the incumbent Democratic party), and to reward Trump, this behavior was concentrated in more populous counties.

The timing of foreclosures might also influence voters’ responsiveness to economic performance. To construct our foreclosure rate measure, we take the average foreclosure rate within an electoral cycle. It could be, however, that voters are myopic in their evaluation of incumbents, considering only economic circumstances in the time leading up to the election. The voter myopia literature generally suggests that it is the past six months or year that are most salient to voters (e.g., Achen and Bartels 2004; Alesina, Londregan, and Rosenthal 1993; Fair 1978; Healy and Lenz 2014). Accordingly, in Table 5 we consider the effects of

**Table 5 – Effects of Recent Housing Foreclosures on Presidential Elections, County Level, 2004–2016: Testing for Trump-Clinton Effects.**

	Dem Pres Vote Pct (0-100)			
	(1)	(2)	(3)	(4)
Foreclosures Per 1,000 People	-0.24 (0.14)	-0.12 (0.11)	0.21 (0.10)	0.05 (0.13)
Foreclosures $\times$ 2016	-4.31 (0.60)	-4.13 (0.46)	-4.32 (0.68)	-3.31 (0.56)
N	9186	9186	9186	9186
# Counties	2819	2819	2819	2819
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.

recent foreclosures on the 2016 election, where the foreclosures variable is the foreclosure rate in the six months leading up to election day, instead of over the past election cycle like in the main analysis before. Here, we find massive effects of recent housing foreclosures on the Democratic vote share in 2016. An increase of 1 foreclosure per 1,000 people in the last six months before the election is estimated to decrease the Democratic vote share by 3.31 to 4.32 more in 2016 than in previous years.

It is difficult to be confident in the identification of these effects, relying as they do on changes within only one four-year period, but they are at least suggestive. Voters may rarely switch parties or punish incumbents in response to personal economic distress not only because they do not care about politics, but also because most of the time the two parties offer relatively similar economic policies. However, the results, particularly the effects of foreclosures in the six months before the election, provide suggestive evidence in favor of this claim in 2016, perhaps because of Trump’s populist appeal.

## 4 Home Foreclosures Reduce Individual Turnout

Having documented the non-effects of foreclosures on election outcomes, we now estimate effects of foreclosure on individual voter turnout. Specifically, we estimate equations of the form

$$\textit{Turns Out}_{it} = \beta \textit{Foreclosed}_{it} + \gamma_i + \delta_t + \epsilon_{it}, \quad (2)$$

where  $\textit{Turns Out}_{it}$  is a binary variable that takes the value 1 if individual  $i$  turned out to vote in the election at time  $t$ , and 0 otherwise. The variable  $\textit{Foreclosed}_{it}$  is an indicator for whether individual  $i$  experienced a foreclosure during election period  $t$ ;  $\gamma_i$  stands in for individual fixed effects, while  $\delta_t$  stands in for county-by-year fixed effects. In some specifications, we also add interactions of the  $\textit{Foreclosed}$  variable with an indicator for 2016.

Table 6 presents the results. The first two columns use the full sample, while the second two use only the individuals registered to vote before 2004, as discussed along with Table 2. In column 1, we find a modest but highly precise negative estimate. Individuals who suffer foreclosure are less likely to turn out to vote—as much as, perhaps, 2 percentage points (column 1). Interestingly, this estimate is quite similar to those in a previous study of residential mobility. Gay (2012) studies individuals who randomly received the opportunity to move out of public housing and into private apartments during the 1994 Moving to Opportunity for Fair Housing Demonstration Program. Experimental estimates of the effect of this opportunity on turnout suggest that it decreased turnout by roughly 2–4 percentage points. The similarity in our estimates further supports the idea that home foreclosures have generally not galvanized political activity; instead, foreclosures seem to affect individuals in much the same way other forms of residential mobility—even those that are the result of an intentional opt-in program and that are not associated with a political crisis—do. We find a null effect in column 3, likely because the set of people who are in the voter file and registered to vote before 2004 are habitual voters who are correspondingly less sensitive to any stimuli that might reduce their propensity to vote.

**Table 6 – Difference-in-Differences Effects of Housing Foreclosures on Individual Turnout in Ohio, 2004 to 2016.**

	Turn Out in General Election (Yes/No)			
	Full Voter File (1)	(2)	Registered Before 2004 (3)	(4)
Home Foreclosed	-0.019 (0.001)	-0.009 (0.001)	0.002 (0.002)	-0.006 (0.002)
Home Foreclosed * 2016		-0.079 (0.002)		0.073 (0.005)
N	72,845,379	72,845,379	25,999,794	25,999,794
Individual Fixed Effects	Yes	Yes	Yes	Yes
County-Year Fixed Effects	Yes	Yes	Yes	Yes

Robust standard errors clustered by individual in parentheses. Each observation is a person-election year.

Earlier, we provided evidence that foreclosures may have caused voters to punish the incumbent more in 2016 than in previous years. Were foreclosed individuals particularly motivated to turn out in response to foreclosure in 2016 relative to past years? To investigate this, we interact the foreclosures variable with an indicator for whether the foreclosure occurred in the two years prior to the 2016 election. The estimates reveal interesting heterogeneity. Experiencing foreclosure is estimated to decrease an individual’s turnout likelihood by about 8.8 percentage points in 2016 (summing the coefficients in column 2), which suggests that the demobilizing effect of foreclosures was much larger in 2016 than in previous years. Column 4, however, reveals that foreclosures in 2016 discouraged some types of voters and galvanized others. When we subset to individuals registered before our period of study, we estimate that experiencing foreclosure *increases* an individual’s turnout likelihood by about 6.7 percentage points (summing the coefficients in column 4). Voters registered before 2004 are habitual voters—Table 2 shows that the turnout rate from 2004 through 2016 among this group is 80%, compared to under 50% for the full voter file—so the results illustrate how, in an uncommon election where Trump’s economic policy deviated sharply from previous presidential candidates, individual experiences of economic distress can have larger effects on voter behavior. While habitual voters are particularly motivated to turn out

in 2016 in response to experiencing foreclosure, voters on average became much less likely to participate.

In sum, experiencing a home foreclosure makes an individual in Ohio less likely to turn out and vote, on average. This effect is not massive but is very precisely estimated, which means we can rule out positive effects. It does not appear to be true that, on average, home foreclosures galvanize individuals to participate in politics, in Ohio at least. The result in column 4, however, suggests that foreclosures might motivate habitual voters to participate even more in unusual circumstances, like the 2016 election.

Why don't foreclosures galvanize people in general, the way some popular accounts have suggested they might? At the individual level, there are probably a number of reasons, but the results for the full voter file versus those registered before 2004 suggest one in particular. Foreclosures no doubt upset people, and this may encourage them to become politically active, but they also cause stress and many new obligations. Foreclosed individuals have complicated financial situations to deal with, not to mention that they must find a new residence. More prosaically, simply being forced to change addresses may reduce their probability of registering to vote (e.g., Squire, Wolfinger, and Glass 1987). Strains like these raise the opportunity cost of participating in politics, and likely reduce participation as a result. We should probably be skeptical of posited links between other forms of economic distress and individual political behavior for the same reasons. In general, the people who experience economic distress may have the greatest incentives to pay new attention to the political process, but they are also the people experiencing life events that reduce their ability or desire to spend time involved in politics.

## 5 Why Don't Foreclosures Affect Voter Behavior, Generally?

We now consider possible reasons why the finding that, in general, foreclosures do not affect incumbent vote shares, could be biased toward finding null results. We conclude that none of these possibilities explains the results.

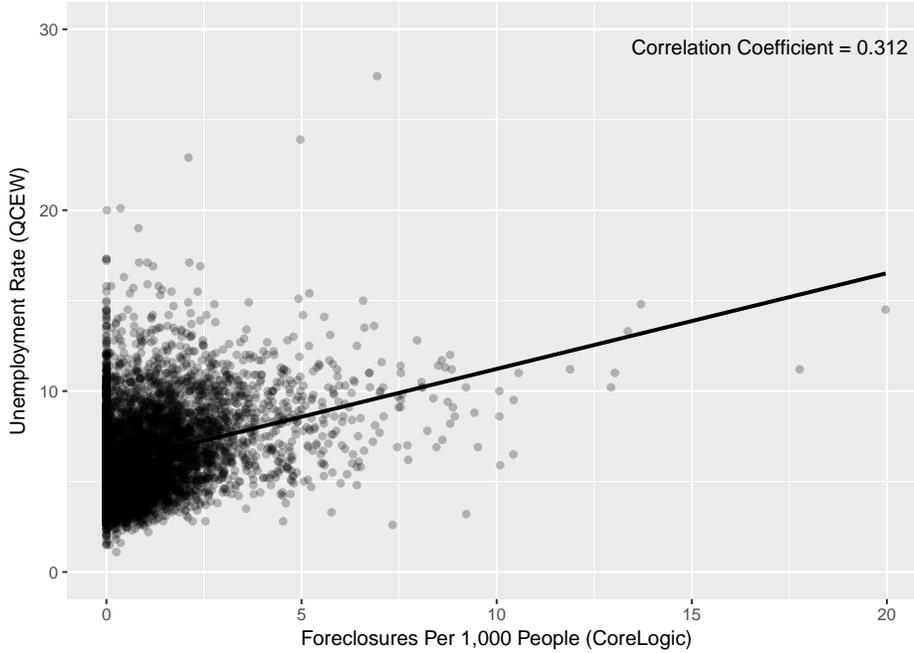
### **Foreclosure Rates as a Measure of the Economy**

First, foreclosure rates might not characterize a county's general economy. To examine this concern, we correlate our foreclosure rate measure with other county-level economic indices (Figure 2). In line with our expectations, our CoreLogic foreclosure rate positively correlates with the unemployment rate from the Quarterly Census of Employment and Wages (QCEW). Moreover, the CoreLogic foreclosure rates in Figure 1 accord with accounts of which states were hit hardest by the recession (e.g., Yagan 2016), and we use our data to replicate the result from Healy and Lenz (2017) on the effects of the housing crisis in California (see the Appendix). All of this suggests that our foreclosure rate measure gives a good sense of a county's general economy. Relatedly, one might worry about measurement error in the CoreLogic data on foreclosures, which would attenuate our estimated effects. In the next section, however, we find that home foreclosures at the individual level reduce turnout among voters in Ohio—an effect which we would not find if there were substantial measurement error in the CoreLogic data.

### **Non-Effects of Foreclosures in Legislative Elections**

Second, it could be that voters attribute blame for adverse economic conditions to offices other than the president (de Benedictis-Kessner and Warshaw 2019). To explore this possibility, we examine effects of foreclosures on legislative elections. Table 7 presents the results for House and Senate elections, respectively, using the same specifications from Table 3. We

**Figure 2 – CoreLogic Foreclosure Rate Validation** We compare our foreclosure rate measure to the unemployment rate from the Quarterly Census of Employment and Wages (QCEW). Each point in the scatterplot represents a county-year observation. As expected, we observe a positive relationship.



continue to define party incumbency based on the presidency (in the Appendix, we present results using party incumbency of senators, also.) In the interest of brevity, we do not walk through each individual estimate; but they are largely null. The only arguably large effects are for effects on Senate races in the last two columns and on House races in some columns. However, these estimates are primarily positive, not negative, which would indicate that incumbent party candidates may benefit, slightly, from an increase in foreclosures. However, these estimates are less imprecise, and they are all still substantively small.

### **Voter Myopia and the Timing of Foreclosures**

Third, as discussed earlier, the timing of foreclosures might affect voters' responsiveness to economic performance. In Table 8, we consider the effects of recent foreclosures on presidential elections, where the foreclosures variable is the foreclosure rate in the six months leading up to election day, instead of over the past election cycle like in the main analysis

**Table 7 – Effects of Housing Foreclosures on Legislative Elections, County Level, 2002–2016.**

	Dem Senate Vote Pct (0-100)			
Foreclosures Per 1,000 People	-0.55 (0.15)	-0.85 (0.35)	-0.27 (0.18)	-1.31 (0.51)
Foreclosures $\times$ Inc Party	0.10 (0.10)	-0.20 (0.26)	-0.27 (0.13)	0.37 (0.40)
N	12282	12282	12282	12282
# Counties	2829	2829	2829	2829
	Dem House Vote Pct (0-100)			
Foreclosures Per 1,000 People	-0.48 (0.19)	0.13 (0.16)	-0.06 (0.19)	0.08 (0.16)
Foreclosures $\times$ Inc Party	0.28 (0.14)	-0.04 (0.11)	0.24 (0.14)	0.16 (0.10)
N	18187	18187	18187	18187
# Counties	2839	2839	2839	2839
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.

before. Interestingly, the estimates range from -0.59 to -0.11, suggesting that an increase in foreclosures in the last six months before election day decreases incumbent vote share. However, as we will show later, this effect is particularly large for the 2016 election, and the estimates from this specification are all null when we do not include 2016.

Table 9 estimates the effect of recent foreclosures on House and Senate races. The results are null across most specifications. Although these estimates are somewhat less precise, we generally find no evidence of an effect of recent housing foreclosures on incumbent performance in House or Senate elections.

**Table 8 – Effects of Recent Housing Foreclosures on Presidential Elections, County Level, 2004–2016.**

	Dem Pres Vote Pct (0-100)			
	(1)	(2)	(3)	(4)
Foreclosures Per 1,000 People	-0.73 (0.16)	-0.66 (0.12)	0.10 (0.15)	-0.20 (0.16)
Foreclosures × Inc Party	-0.50 (0.10)	-0.59 (0.09)	-0.11 (0.11)	-0.34 (0.11)
N	9186	9186	9186	9186
# Counties	2819	2819	2819	2819
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.

These analyses also raise a distinction between testing for the systematic effects of the foreclosure crisis, itself, versus testing theories of retrospective voting. Our main results on the electoral effects of foreclosures (Table 3) are designed to test the former, while the results for the effects of recent housing foreclosures might be better suited to test the latter. We focus primarily on the systematic effects of the foreclosure crisis—where issues of timing are not relevant—so for the main results we use all of the information on foreclosures in the election cycle. The timing of foreclosures is crucial, however, for testing theories of retrospective voting.

### County Size and Economic Perceptions

Fourth, it could be that the county economy does not reflect the experiences of the typical person in that county. If foreclosures are felt only by a small number of people in a large county, it might be difficult to pick up any effects of foreclosures on election outcomes. We do observe foreclosures at the individual level in the CoreLogic data, but we cannot observe

**Table 9 – Effects of Recent Housing Foreclosures on Legislative Elections, County Level, 2002–2016.**

	Dem Senate Vote Pct (0-100)			
Foreclosures Per 1,000 People	-0.82 (0.21)	-1.45 (0.55)	-0.56 (0.30)	-1.63 (0.90)
Foreclosures × Inc Party	-0.10 (0.16)	-0.75 (0.51)	-0.56 (0.26)	-0.08 (0.77)
N	12059	12059	12059	12059
# Counties	2823	2823	2823	2823
	Dem House Vote Pct (0-100)			
Foreclosures Per 1,000 People	-0.70 (0.29)	0.04 (0.25)	-0.08 (0.30)	0.19 (0.27)
Foreclosures × Inc Party	0.18 (0.21)	-0.05 (0.18)	0.32 (0.22)	0.23 (0.17)
N	17829	17829	17829	17829
# Counties	2836	2836	2836	2836
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.

vote choice at the individual level. In order to test for these effects on presidential elections in counties where the foreclosure rate might be a better measure of the typical person’s experiences in that county, we subset the analysis to small counties, defined as those with a 2003 population at or below the median (Table A.4). We find null effects across all specifications. Similarly, we find no evidence that voters in small counties reward or punish House or Senate incumbents based on housing foreclosures (Table A.5).

**Table 10 – Effects of Housing Foreclosures on Presidential Elections, County Level, 2004–2016: Including Only Competitive Counties.**

	Dem Pres Vote Pct (0-100)			
	(1)	(2)	(3)	(4)
Foreclosures Per 1,000 People	-0.54 (0.22)	-0.16 (0.15)	0.22 (0.18)	0.03 (0.17)
Foreclosures $\times$ Inc Party	0.11 (0.10)	-0.08 (0.10)	-0.09 (0.09)	-0.08 (0.09)
N	5826	5826	5826	5826
# Counties	1718	1718	1718	1718
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.

### Competitive Counties

Lastly, it is also possible that voters might not typically react to foreclosures because of strong ideological views and/or strong partisan loyalties. Even if historically voters did punish incumbents for a host of economic outcomes, polarization may have dulled this response in more recent elections. Although there is no perfect way to test this claim, we try to get at it indirectly by examining if the effects of foreclosures are different in counties that are more politically competitive—counties where voters, as a whole, have not consistently supported one party in the past. This is not a perfect test since a county might be competitive only because it contains partisan or ideological voters of each party in equal proportion, rather than containing more swing voters, but it is at least a possibility, and one which would be consistent with empirical research that suggests incumbents offer more moderate positions in more competitive places (Ansolabehere, Snyder, and Stewart 2001). Specifically, we compute each county’s “normal vote” as the average Democratic vote share for president in the county

in all elections from 1972–2000 (thus avoiding any post-treatment issues). We then re-estimate the regressions from Table 3 focusing only on competitive districts, which we define to be districts where the normal vote is between 40% and 60%. As Table 10 shows, we continue to find null results. If even more politically competitive districts do not appear to punish incumbent party candidates for foreclosures, it seems less likely that the null results are driven by ideological voters or partisan loyalty.

## Summary

In summary, looking across presidential, U.S. House, and U.S. Senate races, we do not find a systematic link between home foreclosures and the punishment of incumbents. Counties that have suffered disproportionate home foreclosures do not appear to have voted against incumbents at a different rate than counties experiencing fewer foreclosures. These null results are not the result of noise; in many cases we have very precise estimates which can rule out any substantively meaningful effects. The results hold when subsetting to counties with small populations, and when subsetting to electorally competitive counties. The only case where these results do not hold is when we consider only the effects of foreclosures in the six months before election day on presidential elections, and these estimates are particularly sensitive to the inclusion of the 2016 election. Despite the salience of home foreclosures, they do not seem to alter vote choices across localities in general.

## 6 Conclusion

The political aftermath of the Great Recession has raised a number of questions about the possible links between the housing crisis and electoral politics. In this paper, we report three relevant empirical patterns. First, local areas more affected by home foreclosures do not appear to have changed their voting patterns, in general. Second, one of the reasons for this lack of change may be that individuals are, if anything, less likely to participate in elections

after experiencing foreclosure. And third, areas with more home foreclosures in 2016 appear to have voted for Trump at substantially higher rates, on average.

Together, these results suggest that localized economic hardship does not guarantee any sort of electoral backlash against incumbents—in the case of foreclosures, at least, such backlash is the exception rather than the rule. However, we find that backlash appears in a particularly salient case where a populist candidate stands for office and breaks with economic orthodoxy. As is often the case, to understand the links between economic distress and political behavior, it is important to consider the interplay of economic conditions, candidate responses, and voter behavior. In general, localized economic hardship may not affect observed electoral outcomes because candidates react and, in the case of policies like foreign trade, converge in the platforms they offer. But in cases where a new candidate emerges offering views sufficiently radical that establishment candidates cannot match them, there may be substantial effects of local economic conditions on electoral outcomes.

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# Online Appendix

Intended for online publication only.

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## A.1 Unifying Our Results with Existing Research

Because Healy and Lenz (2017) studied the effects of local economic conditions with different data, and because the authors of the study have made their replication data public, we have a somewhat unusual opportunity to consider evidence across the two studies and aggregate them together to obtain more information about local economic conditions and incumbent electoral fortunes.

In this section, we compare our results to the two main findings from Healy and Lenz (2017), and we use these comparisons to offer an aggregated view of the estimated links, considering both datasets in context. Specifically, the two main findings from Healy and Lenz (2017) are: (1) that an index of local wage and employment growth predicts incumbent electoral performance at the county level between 1990 and 2012; and (2) that mortgage delinquencies at the zip-code level in California predicted Obama vote gains in 2008.

We draw two main conclusions from the analyses below. First, using our preferred specification, which is the difference-in-differences design, we estimate precise null results of local economic conditions on incumbent electoral fortunes using both our dataset on home foreclosures and the Healy and Lenz (2017) dataset on local economic conditions. Second, using the estimates from the difference-in-differences design and the lagged dependent variable model preferred by Healy and Lenz (2017), applying both strategies to both datasets after rescaling the explanatory variables, we bracket the true effect and argue that it is quite modest in either case. In sum, while there may be unusual cases where effects exist—like California in 2008, a case that Healy and Lenz (2017) identifies and for which we, too, find an effect (see later discussion below)—in general, there does not appear to be a substantial link between local economic conditions and incumbent electoral performance.

### A.1.1 Evaluating General Effects of the Local Economy

To start, we compare the nationwide analysis from Healy and Lenz (2017) to our nationwide results. The main explanatory variable that Healy and Lenz (2017) uses in the nationwide analysis is an index of local economic growth—the simple average of wage and employment growth over the past 6 months before the election—that has been de-measured by county, and then rescaled so that it is 0 for the .001 percentile and 1 for the .999 percentile. Healy and Lenz (2017) uses this variable in a lagged dependent-variable framework to estimate its effect on incumbent vote share.

A key difference between the main results presented in this paper and those from Healy and Lenz (2017) is that they come from different empirical designs.<sup>10</sup> While Healy and Lenz (2017) uses the lagged dependent variable model, the main results in our paper come from the difference-in-differences framework. We prefer the difference-in-differences framework to the lagged dependent variable setup because its key identifying assumption, parallel trends,

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<sup>10</sup>The reader might also note a second difference, which is that the Healy and Lenz (2017) regressions are weighted by total votes while our results are weighted by county population. We have examined these two approaches and found that it makes only negligible differences in the results. That said, the Healy and Lenz (2017) results do attenuate considerably when the counties are not weighted, as do our 2016/Trump estimates in Table 4. We do not focus too much on this issue, because it seems reasonable to down-weight low population counties.

is clear and testable. In the body of our paper, we test the validity of the difference-in-differences design by showing that our conclusions do not change when we add county linear time trends, or when we add several different types of time fixed effects. Both of these strategies are ways to relax the parallel trends assumption of the difference-in-differences design, and the fact that the estimates are similar across them suggests that the assumption is sound. These kinds of robustness checks are not possible in the lagged dependent variable framework, which is one reason we prefer the difference-in-differences. Moreover, we know that the lagged dependent variable approach is biased when parallel trends, the difference-in-differences assumption, is met (Angrist and Pischke 2009)—and, as we have just discussed, we have reasons to believe the parallel trends assumption is met in this case.

All that said, both strategies require assumptions and neither is a silver bullet. In cases where the parallel trends assumption is violated, like in cases where there is substantial year-to-year variability in treated and control potential outcomes, the lagged dependent-variable model can perform better. Accordingly, it is instructive to look at estimates for both designs on both datasets. To try to make estimates comparable, we rescale the main explanatory variables, foreclosures per 1,000 people and local wage and employment growth, so that each takes the value 0 at the median and 1 at the .99 percentile.<sup>11</sup> Finally, to make the estimates go in the same direction, we take the negative of the point estimates from our foreclosure data (since an increase in foreclosures is the opposite of an increase in economic growth). Figure A.1 plots the results along with 95% confidence intervals from robust standard errors clustered by county.

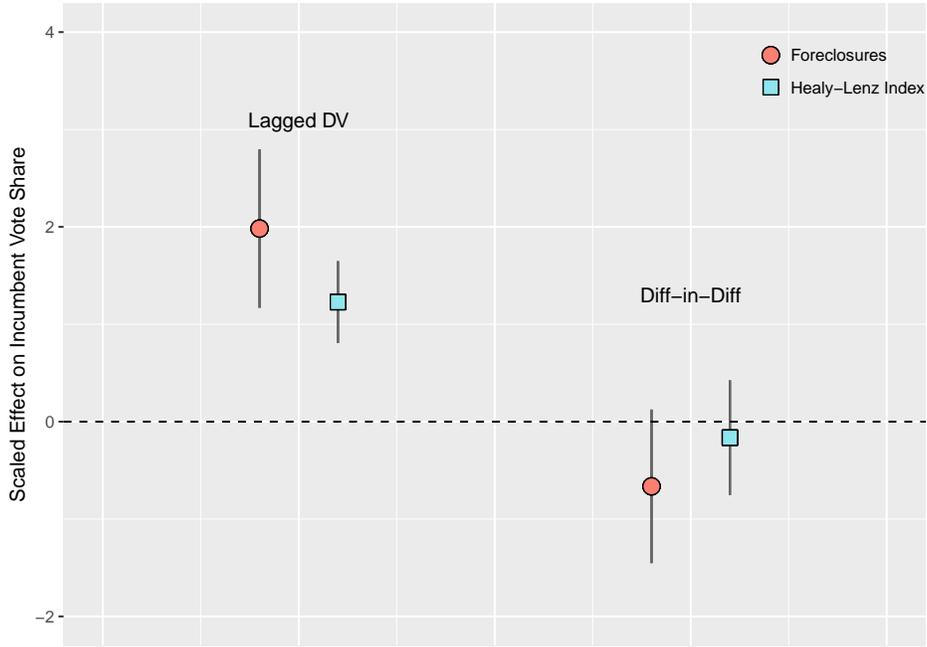
As the figure shows, the estimates from the two datasets are surprisingly similar, once variables are rescaled to be comparable and the same empirical design is used. This is despite differences in the time period and the economic measures used, which makes the similarity all the more remarkable. How do we interpret these similar effects? As the plot shows, when we use the lagged dependent variable setup, we find positive effects, but these effects are quite modest in size. A move from the median to the 99th percentile is an extreme shift, and even this shift is estimated to increase the incumbent’s vote share by fewer than 2 percentage points in both datasets.

These effects become very close to zero (and in fact slightly negative), when we use the difference-in-differences design on either dataset. These null results are precisely estimated; as the figure shows, the 95% confidence intervals rule out effects even as large as +1 per-

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<sup>11</sup>This is not the one-unit shift that Healy and Lenz (2017) uses; instead, the paper reports the effect for a shift from the .001 percentile to the .999 percentile. This is a very large shift, because both the .001 and the .999 percentile are extreme outliers in the distribution of the economic growth index variable. And because the index variable is already a growth variable, i.e., a measure of changes in the data, this resulting scaled variable reflects the difference between what is one of the most negative changes observed in the data (the .001 percentile of growth) and one of the most positive observed changes in the data (the .999 percentile of growth). As a result, the coefficient on this scaled variable corresponds to the change in incumbent vote share for a very extreme and unusual change in local economic conditions. Specifically, the distance between the .001 percentile and the .999 percentile of the growth variable, after de-meaning by county but not rescaling, is roughly 0.4. The .01 and .99 percentiles of the growth variable are -0.08 and +0.08, respectively. This means that changes of 0.08, like that from the median to the .99 percentile, which are more extreme than 98% of all the data, are smaller than the hypothetical shift from the .001 percentile to the .999 percentile by a factor of 5.

**Figure A.1 – Comparing Estimates to Healy and Lenz.** We compare estimates from our foreclosures dataset to estimates from the Healy and Lenz data by estimating effects in each dataset using the same two designs after rescaling the explanatory variables so that the regression coefficient of interest represents a shift from the median to the 0.99 percentile of each variable. Foreclosures estimates are multiplied by -1 so that they are comparable to the Healy and Lenz results (since a decrease in foreclosures is positively correlated with economic growth).



centage point, for the foreclosures data, and 0.42 percentage points (42 basis points) for the local economic growth data.

Determining whether we think there are positive effects thus requires choosing between the two empirical strategies. As Angrist and Pischke (2009) explains, we can use the two estimators to bracket the true effect, if we assume that one of the two identifying assumptions is correct. The two foreclosures estimates give us the most extreme estimates in either direction, positive and negative; as such, using the bracketing principle, we can offer an estimated effect range from -0.25 to +1.63, where again these are effects on incumbent vote share from increasing the foreclosure rate from the median rate to the .99 percentile rate. If we use the bracketing principle with the 95% confidence intervals, we bound the effect of good local economic changes, defined using the shift from the median to the .99 percentile, between -1.50 and +2.71 percentage points. Again, these are estimated effects scaled to what is among the most extreme plausible swings in economic conditions that we could see in the data. That such large swings are only estimated to move incumbent vote by at most a few percentage points—and that what we think are the most plausible estimates even go in the opposite direction—suggests to us that the link between local economic conditions and incumbent electoral fortunes is in general very modest, if it exists.

### A.1.2 Evaluating Effects of the Housing Crisis in California

Having considered overall effects, we now zoom in on estimating the effect of foreclosures on support for Obama, studying California in the period 2004-2008, mirroring the Healy and Lenz (2017) analysis on mortgage delinquencies in California. Given the overall results, the question now is whether we have evidence for a salient case producing a larger link between local economic conditions, specifically measured via the housing collapse, and incumbent electoral fortunes.

We start by replicating the Healy and Lenz (2017) finding using our foreclosure data. We use our preferred difference-in-differences design, like in the body of this paper.<sup>12</sup> Following Healy and Lenz (2017), we relate changes from 2006 to 2008 in foreclosures per 1,000 people at the county level to changes in Democratic vote from 2004 to 2008. The results are presented in the first two columns of Table A.1; in the first column, we do not weight by population county, in the second, we do. Consistent with Healy and Lenz (2017), we find a positive and statistically significant link between foreclosures and support for Obama in California in 2008. To make the estimates comparable to those from the previous section, we again rescale the foreclosures per 1,000 people variable so that it is 0 at the median foreclosure rate and 1 for the 0.99 percentile. The increase from the median to the 0.99 percentile is estimated to increase Obama vote in 2008 by 4.42 or 3.57 percentage points, depending on the specification. These effects are much larger than those estimated overall—indeed, the difference-in-differences estimates on the full sample sometimes even have the opposite sign—and, while they are not huge, they are clearly discernible.<sup>13</sup>

Our main results (Table 3) suggest that this relationship does not generalize to other times and places. But might we find it in other highly salient contexts, like we seem to in California during the heyday of the subprime crisis? To investigate this, in the last two columns of Table A.1, we apply the design to the 5 other states who experienced the largest changes in foreclosures per 1,000 people between 2006 and 2008 in the CoreLogic data. Specifically, these states are: Arizona, Florida, Michigan, Nevada, and Rhode Island. As the final two columns show, we find null results for these cases. Although the estimates here are not precise, the magnitude is much smaller than the California case, and we cannot reject the null of no effect. Combined with the results in the body of the paper, it appears that home foreclosures do not affect incumbent electoral fortunes in general. We cannot rule out that there are special cases where they do—perhaps including California in 2008 and certain areas of the U.S. in 2016 (see Table 4 in the body of our paper)—but there does not seem to be a general phenomenon underlying these effects.<sup>14</sup>

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<sup>12</sup>Because we only have two time periods for this replication, we difference the  $y$  and  $x$  variables rather than using county and year fixed effects.

<sup>13</sup>We mean opposite sign in the sense that here we find incumbents doing worse when the economy does worse. Mechanically the sign is positive in both cases, but that’s because here we are just using Democratic vote share without an indicator for incumbency, because the Democrats are the non-incumbent party in 2008.

<sup>14</sup>In considering these more specific effects, we should keep in mind that zooming in on cases with only two time periods does raise the probability of finding false positives. With only one difference to compute over time, resulting standard errors are highly suspect, so many patterns that result from noise may look statistically significant. This could be an issue for the California analysis as well as for the 2016/Trump analysis in Table 4.

**Table A.1 – Effects of Housing Foreclosures on Presidential Elections, 2004–2008**

	$\Delta$ Dem Pres Vote (0-100)			
	California		Top 5	
	(1)	(2)	(3)	(4)
$\Delta$ Foreclosures Per 1,000 People	4.19 (0.53)	3.57 (0.64)	2.50 (1.10)	0.23 (0.88)
N	58	58	127	127
State Fixed Effects	–	–	Yes	Yes
Population Weights	No	Yes	No	Yes

Robust standard errors clustered by county in parentheses.

## A.2 Senate Results Using Senate Incumbency

Here, we re-estimate the results on legislative elections, defining the incumbent party for the election based on the incumbent senator rather than president. Because the data is at the county level, we do not repeat this exercise for the U.S. House, where there is not a one-to-one correspondence of incumbent members of congress to counties. As the table shows, we continue to find null results with this alternate specification.

**Table A.2 – Effects of Housing Foreclosures on Legislative Elections, County Level, 2002–2016.**

	Dem Senate Vote Pct (0-100)			
Foreclosures Per 1,000 People	-0.48 (0.11)	-0.97 (0.30)	-0.36 (0.18)	-1.01 (0.34)
Foreclosures $\times$ Inc Party	-0.00 (0.15)	-0.64 (0.25)	-0.47 (0.26)	-1.50 (0.19)
N	12282	12282	12282	12282
# Counties	2829	2829	2829	2829
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Inc Party is 1 for two Dem Senators, -1 for two Rep Senators, and 0 for split party. Main effect for Inc Party is absorbed by fixed effects.

## A.3 Processing the Deed-Level Data

Each row in the CoreLogic Deed data represents a deed-level event for a property. Deed-level events include typical transactions like sales and foreclosures, as well as lesser-known transactions like the submission of a Notice of Default. Our research concerns itself with foreclosure events (e.g., sale of property by a real estate organization) and pre-foreclosure events (e.g., Notice of Default, lis pendens); thus, we seek to narrow the dataset of all transactions into a dataset of only foreclosure records.

We begin by sorting every row in the Deed data into one of three categories: foreclosure event, pre-foreclosure event, or no indication of this being a foreclosure-related event. Four fields in each row provide us the necessary information to divide the rows into categories.

- First, we inspect the Secondary Deed Cat Codes in each row. The contents of these character positions are “detailed category codes providing additional deed information” per the CoreLogic documentation. If the field contains the value “O” (meaning Real-Estate Owned transfer) or “P” (meaning Real-Estate Owned sale), then we classify the row as a foreclosure event, since the property is under real estate control.
- If the Secondary Deed Cat Codes are inconclusive, we continue to the character positions in the row corresponding to the Mortgage Document Type, which is the “Type of Deed Used for Recording” per CoreLogic documentation. Various states signify foreclosures in different ways, so, if any of the following flags appears, we classify the transaction as a foreclosure event: “CO” (Commissioner’s Deed (foreclosure)), “FD” (Foreclosure Deed), “MF” (Mortgage Foreclosure Deed), “NT” (Notice of Trustee’s Sale), “SC” (Sheriff’s Certificate of Foreclosure), “TE” (Trustee’s Deed Upon Sale (Foreclosure)), “U” (Foreclosure Deed).
- If the Mortgage Document Type is inconclusive, we inspect the character positions in the row corresponding to Document Type, which is the “Type of Transfer Document Recorded” per CoreLogic documentation. If this field takes the value “U” (a Foreclosure event), then we classify the row as a foreclosure event. Else, if this field takes the value “N” (Notice of Default (NOD)) or “L” (Lis Pendens), we classify the row as a pre-foreclosure event.
- If the Document Type is inconclusive, we examine the character positions in the row corresponding to Foreclosure Field, an “Indicator Showing the Transaction is a Foreclosure” per CoreLogic documentation. If the value of this indicator is “O” or “Y” (REO-Nominal transaction or a Transfer Between Bank and FNMA, FHA, etc.) or “P” (REO Sale - Sale from Government to Third Party), then we classify the event as a foreclosure event.
- If none of the above described criteria are met, we classify the row as having no indication of being a foreclosure event and omit it from the dataset.

Following this narrowing to just the foreclosure and pre-foreclosure transactions, we employ a three-step filter to further narrow these rows into the most relevant data points.

First, we filter out transactions that correspond to businesses or trusts and not to individual homeowners. Because we seek to observe relationships between individuals' voting behavior and their home foreclosures, information about the foreclosures of enterprise properties does not provide us useful information. One character in the Corelogic Deed rows corresponds to an indicator of whether the property in question is owned by a corporation or trust. If the character assumes the value of "C" (for "Corporation"), "T" (for "Trust"), or "Y" (for "Yes-Corporation"), then we omit the transaction from further consideration. If the character position is vacant (i.e. occupied by whitespace), we assume the property does not belong to a corporation or trust and retain the record.

Next, we filter out transactions for which no home address numbers are available. We do so because, in order to link foreclosure information to voter files, we require a name and address of residence. Simply knowing that an anonymous foreclosure occurred in a county does not provide us with information we can use to model the relationship between foreclosures and voting behavior. To filter these anonymous transactions, we check the character positions in the Corelogic Deed data row corresponding to the house number; if they are occupied by whitespace, we omit the record.

Finally, we filter out transactions for which no date information was available. Our goal is to tally the foreclosures that occurred between elections, so as to analyze their possible impact on vote behavior in the election cycle containing the foreclosures. For example, if a foreclosure occurred on January 3rd, 2001, we would tally it as a foreclosure relevant to the 2002 election. If no date information is present, we cannot arbitrarily assign the foreclosure record to a time period out of concern for biasing the results of our inquiry. To prevent mis-allocation of undated foreclosure records, we omit the Deed events that contain no date information. If the character positions in the Deed data row corresponding to the date are vacant, we disregard the record.

We now possess a list of transactions classified as either a foreclosure event or as a pre-foreclosure event. However, we still need to transform these to voter-level records. One voter owning multiple properties could appear multiple times within our current document. To overcome this, we build a dictionary; we group all transactions that share a unique owner; a unique owner is one with a distinct four-tuple comprising FIPS (county) code, city, owner's last name, and owner's first name. Using the transactions within each group, we tally the number of foreclosure-related transactions that occurred prior to the elections (November 7th) in 2004, 2006, 2008, 2010, 2012, 2014, and 2016 for each unique owner. For each of these election years, we record the number of foreclosure-related events for that unique owner since the last election year, the date of the earliest foreclosure-related event that occurred in that time interval, and the severity of the most severe foreclosure-related event that took place. Severity can assume three values: either an "I" (incomplete) foreclosure if only pre-foreclosure events (Notice of Default or *lis pendens*) occurred in that election cycle, or "C" (complete) if even one foreclosure event occurred. If neither is true, it assumes the value of "N" (no foreclosure).

Following this consolidation of transaction-level data into unique-owner-level data, each row in our data contains a FIPS, a city, the owner's last name, the owner's first name, and the three aforementioned items per election year. Equipped with this information, we seek to now associate as many homeowners in this file as possible to a voter file containing vote behavior for each election of interest. Each row in our voter file corresponds to an individual

voter; for each entry, it contains a FIPS, a city, a first name, and a last name. We use these fields to perform a left-join between our foreclosure data and the voter file, retaining all rows in our foreclosure data with appended voter information where available. Where unavailable, the voter information is merely populated as “NA”. We then proceed with further analysis.

## A.4 Assessing the Generalizability of the CoreLogic Data

The CoreLogic data only covers roughly half the counties in the United States. Although this does not induce any obvious bias into our statistical analyses, we would like to know how specific the resulting estimates are to the CoreLogic context. Accordingly, Table A.3 compares the CoreLogic sample to the full set of U.S. counties. Specifically, we compare means of a large number of covariates available in the American Community Survey (ACS). As we see, CoreLogic counties are a bit more populous, with an average population of about 130,000 versus about 100,000 for all counties in the U.S. However, on essentially every other covariate they look extremely similar. Perhaps most importantly, CoreLogic counties do not appear to have higher or lower unemployment rates, education rates, or household median incomes. As a result, we suspect our results generalize to non-CoreLogic counties.

**Table A.3 – County Covariates (ACS 2009-2014)**

Covariate	Mean (All Counties)	Mean (CoreLogic Counties)
Total Population	82586	82339
Percent Age 18 to 29	0.166	0.165
Percent Age 30 to 44	0.194	0.193
Percent Age 45 to 64	0.265	0.267
Percent Age 65 and up	0.139	0.140
Percent Female	0.507	0.507
Percent White	0.656	0.676
Percent Black	0.114	0.121
Percent Hispanic	0.156	0.139
Percent Asian	0.042	0.034
Percent Less than High School	0.135	0.131
Percent High School Degree	0.287	0.293
Percent Some College	0.298	0.297
Percent 4-year College Degree	0.177	0.176
Percent Post-Graduate	0.104	0.103
Unemployment Rate	0.092	0.090
Household Median Income	55134	54258
# Counties	2986	2661

## A.5 Effects in Small Counties

In this section, we examine the effect of foreclosures in small counties. We do not find effects of foreclosures on aggregated vote choice generally, but because foreclosures are felt by a small number of people, it could be difficult to detect effects of foreclosures in large counties. If foreclosures are a better measure of the typical experiences in small counties, we might be most likely to pick up an effect when we subset the analysis to small counties. In Table A.4, we estimate the effect of foreclosures just for small counties, defined as those with a 2003 population at or below the median. Much like the results using the full sample of counties, we find null results across specifications. In Table A.5, we estimate the effect of foreclosures in House and Senate races. Again, we find no evidence that voters in small counties reward or punish House or Senate incumbents based on housing foreclosures. The largest coefficient is in the fourth column for Senate incumbents, but the coefficient is in the opposite direction that the economic voting literature would predict.

**Table A.4 – Effects of Housing Foreclosures on Presidential Elections for Small Counties, 2004–2016.**

	Dem Pres Vote Pct (0-100)			
	(1)	(2)	(3)	(4)
Foreclosures Per 1,000 People	-0.27 (0.14)	-0.31 (0.17)	-0.19 (0.16)	-0.67 (0.28)
Foreclosures × Inc Party	-0.10 (0.11)	0.12 (0.11)	-0.09 (0.11)	0.22 (0.18)
N	4770	4770	4770	4770
# Counties	1551	1551	1551	1551
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.

**Table A.5 – Effects of Housing Foreclosures on Legislative Elections for Small Counties, 2002–2016.**

	Dem Senate Vote Pct (0-100)			
Foreclosures Per 1,000 People	-0.05 (0.09)	-0.54 (0.31)	-0.04 (0.11)	-0.91 (0.58)
Foreclosures $\times$ Inc Party	-0.11 (0.08)	0.46 (0.22)	-0.08 (0.09)	1.46 (0.36)
N	6188	6188	6188	6188
# Counties	1570	1570	1570	1570
	Dem House Vote Pct (0-100)			
Foreclosures Per 1,000 People	-0.10 (0.26)	0.29 (0.29)	-0.05 (0.37)	0.19 (0.30)
Foreclosures $\times$ Inc Party	0.04 (0.28)	-0.52 (0.28)	0.08 (0.35)	-0.30 (0.26)
N	9091	9091	9091	9091
# Counties	1577	1577	1577	1577
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	Yes	Yes	Yes	Yes

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.

## A.6 Effects Without County Population Weights

In this section, we report estimates without weighting by county population. In the main text, we weight all of our estimates by county population. As Table A.6 shows, the null effects of foreclosures on presidential elections (from Table 3) hold when we do not use county population weights.

**Table A.6 – Effects of Housing Foreclosures on Presidential Elections, County Level, 2004–2016.**

	Dem Pres Vote Pct (0-100)			
	(1)	(2)	(3)	(4)
Foreclosures Per 1,000 People	-0.50 (0.07)	-0.16 (0.07)	-0.11 (0.07)	-0.30 (0.10)
Foreclosures $\times$ Inc Party	0.16 (0.05)	0.00 (0.06)	-0.13 (0.05)	0.08 (0.06)
N	9541	9541	9541	9541
# Counties	2840	2840	2840	2840
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	No	No	No	No

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.

Next, we show that the Trump effects in 2016, which we show in Tables 4 and 5 for all foreclosures and recent foreclosures, respectively, are somewhat sensitive to the decision to weight by county population. In Table A.7, we test for Trump effects using all foreclosures in the electoral cycle and without weighting by county population. The estimates are all still negative, but some are null. Compared to 4, where the point estimates are more negative, we can conclude that if foreclosures led voters to punish Clinton and reward Trump, this behavior was concentrated in more populous counties.

There are also larger effects in the weighted specifications for recent housing foreclosures. In Table A.8, we test for a Trump effect using only foreclosures in the last six months before the election, but we do not weight by county population. The estimates, while still all negative and statistically significant, are much smaller in magnitude than in the corresponding Table 5, where we use population weights and find massive effects of foreclosures on punishing Clinton and rewarding Trump. Again, this suggests that, if foreclosures in the last six months before the election led to voters to support Trump, these effects were particularly concentrated in more populous counties.

**Table A.7 – Effects of Housing Foreclosures on Presidential Elections, County Level, 2004–2016: Testing for Trump-Clinton Effects.**

	Dem Pres Vote Pct (0-100)			
	(1)	(2)	(3)	(4)
Foreclosures Per 1,000 People	-0.33 (0.05)	-0.13 (0.05)	-0.27 (0.07)	-0.27 (0.07)
Foreclosures $\times$ 2016	-0.32 (0.12)	-0.16 (0.11)	-0.74 (0.16)	-0.23 (0.16)
N	9541	9541	9541	9541
# Counties	2840	2840	2840	2840
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	No	No	No	No

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.

**Table A.8 – Effects of Recent Housing Foreclosures on Presidential Elections, County Level, 2004–2016: Testing for Trump-Clinton Effects.**

	Dem Pres Vote Pct (0-100)			
	(1)	(2)	(3)	(4)
Foreclosures Per 1,000 People	-0.41 (0.08)	-0.14 (0.07)	-0.08 (0.08)	-0.20 (0.09)
Foreclosures $\times$ 2016	-0.85 (0.31)	-0.59 (0.24)	-1.32 (0.42)	-0.64 (0.33)
N	9186	9186	9186	9186
# Counties	2819	2819	2819	2819
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	No	Yes	No
Pop Decile-Year Fixed Effects	No	Yes	No	Yes
County Linear Trends	No	No	Yes	Yes
Population Weights	No	No	No	No

Robust standard errors clustered by county in parentheses. Inc Party is 1 for Dem, -1 for Rep. Main effect for Inc Party is absorbed by fixed effects.