

Americans Vote with their Pocketbooks: Evidence from Mortgages*

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This Version: April 21, 2023

Abstract

How do shocks to households' financial circumstances affect election outcomes? We find that registered Democrats who benefited from automatic rate resets on their adjustable-rate mortgages (ARMs) were relatively more likely to vote in the 2012 presidential election than Democrats who did not. Republicans who benefited, on the other hand, were relatively less likely to vote than Republicans who did not. Consistently, the greater the share of households in a county with ARMs resetting before the 2012 election the greater the relative share of the vote for the incumbent president. These results are consistent with a pocketbook voting mechanism where voters of both parties reward the incumbent for their lower monthly mortgage payments: Democrats by turning out and Republicans by staying home. Our findings shed new light on the complex relationships between economics and politics.

JEL Classification: D12, D14, D72, E43, H31, R20

Keywords: Household Finance, Pocketbook Voting, Political Participation, Electoral Outcomes

*We thank Manuel Adelino, John Cochrane, Olivier Coibion, Jim Conklin, Andreas Fuster, Bob Hunt, Stephanie Johnson, Lauren Lambie-Hanson, and Toni Whited; and seminar participants at the Junior Household Finance Seminar for helpful comments and suggestions. We are very grateful to Ethan Kaplan and Haishan Yuan for sharing their Ohio Voter Data. Claire Nelson provided excellent research assistance. All errors are our own.

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1 Introduction

How do Americans decide whether to vote and whom to vote for? How does the functioning of the country’s financial system affect the political process? The retrospective pocketbook hypothesis contends that voters reward or punish incumbents when their personal financial situations improve or deteriorate, respectively. On the one hand, the pocketbook hypothesis is almost accepted truth by pundits and amateurs. And the effort politicians spend securing resources for their constituents and the time they spend posing the question – “Are you better off than you were four years ago?” – suggests that they too think pocketbook considerations are important (Dahlberg and Johansson, 2002; Levitt and Snyder Jr, 1997; Lewis-Beck and Stegmaier, 2007). On the other hand, and not for lack of trying, empirical evidence consistent with the hypothesis is scant (Healy et al., 2017; Lewis-Beck and Stegmaier, 2013; Margalit, 2019).

A clean identification of the retrospective pocketbook hypothesis has been stymied by a lack of detailed data and appropriate research designs. At issue is that available individual-level data tends to be survey based, introducing concerns about recall and partisan bias. Aggregated data solves these problems, but is subject to its own limitation, namely the inability to infer individual-level relationships from group-level data. Furthermore, appropriately analyzing individual-level data requires some level of random assignment to a treated condition, but politicians’ choices of where to allocate resources are non-random. The final empirical challenge to overcome is that not all random assignment is useful for identifying pocketbook voting. Households must at least partially credit or blame the government for the change to their pocketbooks. To overcome these challenges, we build an individual-level data set that merges voter registration records and household mortgage details. We shed new light on the age-old pocketbook hypothesis by exploring how changes to households’ mortgage payments, which voters might plausibly attribute to the incumbent government, affect voter turnout and electoral outcomes.

The cost of mortgage credit is both economically important – lower rates protect from default (Fuster and Willen, 2017) and spur investment, consumption, and voluntary deleveraging (Di Maggio et al., 2017) – and salient to households. While there is variation in the mortgage rates households are charged, much of this variation is due to differences in households’ credit-worthiness. And since their credit-worthiness is likely correlated with their political beliefs and voting decisions, a

naive comparison of the two suffers from the standard endogeneity problems. To overcome this identification challenge, we use the natural experiment implied by the widespread use of adjustable-rate mortgages (ARMs) at the onset of The Great Recession. A key feature of ARMs is an initial fixed-interest-rate period. After the fixed-rate period expires, interest rates automatically reset, and continue to reset periodically. ARMs differ in the length of their fixed-rate period, though. Consequently, the large decline of interest rates between the 2008 and 2012 U.S. presidential elections automatically passed-through to some, but not all, ARM-borrowers. It is this quasi-random variation we use for identification.

As in [Di Maggio et al. \(2017\)](#) and [Fuster and Willen \(2017\)](#), we compare households who originated two- or five-year ARMs in 2006 and 2007 (the treatment group) to otherwise similar households who originated seven- or ten-year ARMs during the same period (the control group). Treated households saw their mortgage rates automatically reset to lower rates, and their monthly mortgage payments consequently decline significantly relative to control households, in the years leading up to the 2012 election. We confirm that the participation decisions of treated and control households were indistinguishable in the 2008 election, and we can therefore implement a well-behaved difference-in-differences research design. We find that households' responses to lower mortgage payments depended a great deal on their political affiliations. Among voters who did not participate in the 2010 midterm election, treated Democrats are 6.3 percentage points more likely to vote in 2012 than control Democrats conditional on their 2008 participation decision. Treated Republicans, on the other hand, are 4.6 percentage points *less* likely to vote in 2012 than control Republicans. An interacted framework that compares these estimates confirms a "wedge" of 10.6 percentage points in favor of Democratic voter turnout relative to Republican voter turnout in 2012.

An important limitation of the voter registration data is that, while we can observe whether registered voters turned out or not, we cannot observe for whom they voted. In other words, while we infer something about election results in the paragraph above, our individual-level data set limits our ability to speak to election outcomes. However, by zooming out and using aggregated data, we *can* observe election results. We construct a cross-sectional data set of counties in 2012 and then compare the share of the voting eligible population in the county that took out two- or five-year ARMs in 2006 or 2007 to the change in the share of the county that voted for President Obama in 2012 versus 2008. In almost all counties, Obama's vote share declined between 2008 and 2012, but in

those counties where a higher share of potential voters received a positive cash-flow shock via their ARMs, the decrease is much smaller. This relationship is, of course, purely correlational. But, when taken together with our individual-level results, allows us to interpret our findings as evidence that voters reward the incumbent government for improvements to their personal finances.

A vast literature on economic voting investigates whether households consider their personal finances (egotropic) or the broader economy (sociotropic), whether they look backwards (retrospective) or to the different futures proposed by the candidates (prospective), and how these considerations affect whether they vote and whom they vote for (Healy and Malhotra, 2013; Lewis-Beck and Paldam, 2000; Lewis-Beck and Stegmaier, 2007; Margalit, 2019). The general consensus from the academic literature is that American voters consider macroeconomic conditions, like unemployment and inflation, and not their own pocketbooks, when voting (Levitt and Snyder Jr, 1997; Tilley et al., 2018); that the relative importance of retrospective voting depends greatly on the context of the election (Lewis-Beck and Stegmaier, 2013); and that the effect of households' personal economic situations on turnout is small (Smets and Van Ham, 2013). Overall, though, limited data and imperfect research designs means few of these questions have been answered without dissent.

The literature that specifically examines pocketbook voting has not been able to provide conclusive evidence of households voting retrospectively based on their personal finances. The reason for this is not clear. It could be because households do not actually vote based on their finances, or it could be because we lack the means to identify it in the available data. This is due to the difficulty in gathering individual-level data that accurately captures how households' financial situations and voting choices change over time, particularly in cases where households attribute credit or blame to the government (Powell Jr and Whitten, 1993; Tilley et al., 2018), and where changes in financial circumstances affect some households randomly, but not others. Our primary contribution is to overcome these challenges. We use the automatic resetting of the rates on adjustable-rate mortgages to quasi-randomly, via plausibly orthogonal mortgage choices made years prior, improve some households' financial situations relative to others. Changes to households' mortgage payments are, to be clear, *not* a direct function of choices made by elected officials in this case. However, many Americans do not understand how or why interest rates change but do know, broadly, that some quasi-governmental agency is involved. They might therefore mistakenly credit the government for their good fortune (Skinner and Binder, 2021; Weber et al., 2022). This paper's primary contribution

is to take a careful research design to detailed individual-level data and contribute new evidence in support of the retrospective pocketbook voting hypothesis.

By using an economically important context, mortgage payments and changes to them, we also contribute to the literature on the real effects of mortgage contract choices. Using a research design similar to ours, [Fuster and Willen \(2017\)](#) show that households’ risk of becoming delinquent decline substantially following decreases in mortgage payments. They also show evidence consistent with households not anticipating the upcoming decline in their mortgage payments. We use this to argue that the seven- and ten-year ARM borrowers in our sample are therefore unlikely to reward incumbents for their *upcoming* mortgage payment declines. [Di Maggio et al. \(2017\)](#) take the literature one step further and, also using a similar design, show that treated households increase their consumption while simultaneously voluntarily deleveraging. Given the importance of mortgage payments to households and the pocketbook voting hypothesis to our understanding of the democratic process, our work is a natural next question in this literature.

2 Data

We merge the publicly available North Carolina Voter Data with deeds data collected, processed, and sold by CoreLogic Real Estate Solutions. This same merged data set has been used in other papers to evaluate the effect of house price declines on voter participation ([McCartney, 2021](#)) and the effect of political polarization on home sales ([McCartney et al., 2021](#)). We briefly describe the two main data sets below and direct readers looking for a more detailed description of the underlying data sets to those two papers.

2.1 Data Sources

North Carolina Voter Data. We obtain voter registration and historical voting data from the North Carolina State Board of Elections (NCSBE). The registration data includes the full name of each person registered to vote along with the political party they are registered with, complete mailing address, age, race, sex, and state of birth. Unlike many other states, historical snapshots of the registration data are also available. We can therefore observe everybody who was registered to vote in 2008, 2010, and 2012 without concerns about survivorship bias. The companion historical

voting data lists all the elections in which each individual participated (of course, we cannot observe for which candidate each individual voted). These data are free to download and are available at <https://dl.ncsbe.gov/>.

CoreLogic Real Estate Solutions Data. Information on adjustable-rate mortgages used in our paper is from county deeds records collected and processed by CoreLogic. Partly because changes in monthly payments in ARMs could trigger defaults, county offices collect much more information on ARMs than fixed-rate mortgages (FRM). For example, less than 10% of the FRMs in county records by CoreLogic have valid interest rates, whereas most of the ARMs have all detailed contract parameters available, including the initial interest rates, the initial fixed-rate period, reset frequency, the interest rate index used in the adjustable-rate period, spread relative to the index, look-back period, caps on life time rate and each reset. CoreLogic also provides termination dates for ARMs no longer active. With these contract parameters and termination dates for mortgages, we are able to calculate monthly payment over the life cycle of ARMs. The only assumption we make is of no curtailment by borrowers on top of the amortization schedule. One crucial advantage of using ARMs from county deeds records as opposed to credit data is that we observe borrowers' full names and addresses. This allows us to merge the ARM data with the voting records and study how changes in mortgage payments affect voter participation.

2.2 Describing the Merged Data Set

To construct our sample, we first merge the ARM data with the voter data using just property address. To ensure that voters are either the borrowers or borrowers' family members, we then restrict the sample to registered voters whose last name (in the voter file) matches a last name on the deed (from CoreLogic). We focus this investigation on borrowers who originated ARMs between 2005 and 2007 and whose ARMs were active until after the 2012 election. We then restruct the sample to borrowers whose ARMs reset at some point after the 2008 election, but not in the two months immediately following the 2012 election. From this sample, we keep only those voters who were registered in both the 2008 and the 2012 general elections, were affiliated with either the Democrat or Republican party in both elections, and did not switch parties. We further drop voters with mortgage amounts in the top and bottom 1% of the sample. Our final sample includes just over 15,000 voters.

[TABLE 1 HERE]

Table 1 describes the sample. In columns (1) and (2) we describe the full sample. On average, the election participation rate decreased from 90 percent to 86 percent between 2008 and 2012. Just under half of the voters are affiliated with the Democratic party. By construction, the remainder are affiliated with the Republican party. The sample includes only those who originated ARMs between 2005 and 2007. Of these, 83% had ARMs whose rates adjusted prior to the 2012 election. Since interest rates fell dramatically between 2006 and 2012, these voters automatically, costlessly, and unexpectedly received sizeable decreases in their monthly mortgage payments. These voters make up our treatment group. The rest of the sample makes up our control group.

In columns (3) and (4) of Table 1 we describe a particular subgroup of the full sample, those voters that did not participate in the 2010 November midterm election. The voters who did overwhelmingly also voted in both 2008 and 2012. These “always voters” are not particularly interesting to analyze when considering marginal participation decisions, and we consequently focus most of our attention on the “sometimes voters” as identified by their abstention during the 2010 midterms. These voters vary from the main sample in predictable ways. They are much less likely to vote in the 2008 and 2012 presidential elections. They borrow less (i.e., are poorer) and are several years younger. However, with respect to the length of their ARM’s fixed-rate period and CLTV, their mortgage contract decisions are similar.

2.3 Describing the Estimation Samples

[TABLE 2 HERE]

In Table 2 we compare our treated and control groups in finer detail. We begin with unconditional comparisons of our treated and control groups – registered voters who (i) took out ARMs in 2005, 2006, or 2007, (ii) had ARMs that were still active as of 2012, (iii) were registered with a party and the same party in both elections, and (iv) did not participate in the 2010 midterms – in columns (1) and (2). Treated households, those with ARMs automatically resetting before the 2012 election, had higher CLTVs and smaller mortgage amounts. They are younger, more likely to be Black, and more likely to have been born in North Carolina.

An important prediction of the pocketbook voting hypothesis is that, as far as participation is considered, Democrats and Republicans behave differently. That is, to support the incumbent (Democratic) president, Democrats are more likely to go vote while Republicans are less likely to do so. The assumption here is that voters affiliated with a party will vote for that party if they go vote. So if a Republican wants to support a Democrat, he does not actually go vote for the Democrat – that would be a bridge too far – but rather abstains. To test this prediction, we further split the sample by party affiliation in columns (3) and (4), and (5) and (6).

Within both parties, there are similarities between treated and control. Treated households have higher or similar CLTVs and much smaller loan amounts. Within both parties, they are younger, more likely to be Black, and more likely to have been born in North Carolina. A striking difference between treated and control households for Democrats relative to Republicans is that, while they all had similar likelihoods of voting in 2008, their 2012 participation decisions diverge wildly. We plot these eight participation rates – 2008 vs 2012, Dem vs Rep, Treated vs Control – in [Figure 1](#).

[FIGURE 1 HERE]

The most obvious first finding is that everybody was less likely to vote in 2012 than 2008. But striking differences emerge when we start to split the sample. Previewing our main result, [Figure 1](#) shows that treated Democrats (solid blue line), those Democrats whose mortgage payments go down between the 2008 and 2012 elections, become dramatically more likely to participate in 2012 than control Democrats (dashed blue line). Treated Republicans (solid red line), on the other hand, behave the opposite and become relatively *less* likely to participate. In the following sections, we explain our research design in more detail and formalize our comparisons of treated and control groups within parties.

3 Research Design

3.1 Features of Adjustable Rate Mortgages

Borrowers with ARMs are initially charged interest at pre-determined rate that is fixed for some period of time (typically 2, 3, 5, 7, or 10 years). After the expiration of the fixed-rate period, the mortgage rate they are charged adjusts periodically, often once per year. How the rates adjust de-

depends on the terms of the contract, but typically the adjusted rate will be some base rate – the prime rate, LIBOR, rate on U.S. Treasury bills – plus an additional spread, called the margin. ARMs often offer initial rates somewhat below comparable FRMs. This so-called “teaser” rate is used to attract borrowers to the product. Consequently, borrowers who took out ARMs in the late 1990s and early 2000s saw their interest rates rise once the teaser periods started expiring in the mid-2000s. But this paper is not about them. Instead, we focus on the borrowers who took out adjustable rate mortgages in 2006 and 2007. Once their fixed-rate periods ended, their mortgage rates adjusted *downward* and their required mortgage payments shrank, without them having to refinance, fill out a form, or, indeed, do anything at all.

3.2 The Research Design

In this paper, we leverage the fact that borrowers who took out ARMs between 2005 and 2007 might have chosen two-, three-, five-, seven-, or ten-year ARMs. We focus on ARMs originated in between 2005 and 2007 since almost all of these borrowers were still in their fixed-rate period during the 2008 selection.¹ By 2012, however, those with two-, three-, or five-year ARMs and some with seven-year ARMs had seen their rates reset, while those with ten-year ARMs or who got seven-year ARMs in 2006 or 2007 were still in their fixed-rate period. We define the treated group as those borrowers whose ARMs reset prior to the 2012 election. The key difference between our treatment and control groups, therefore, is that they experience their interest rate resets on different sides of the 2012 election. Specifically, due to the unexpected 2008 global financial crisis, treated households saw their mortgage payments fall substantially relative to control households. We use this unexpected positive cash-flow shock to investigate the pocketbook voting hypothesis.

In evaluating the electoral consequences of pocketbook voting, previous research has documented seemingly contradictory results. For example, two designs using lottery winners (Brunner et al., 2011) and oil shocks (Charles and Stephens Jr, 2013) show that positive shocks to households’ economic conditions *decrease* turnout. On the other hand, when Romanian households are given 200 Euros to buy computers (Pop-Eleches et al., 2012) and when Mexican households are the beneficiaries of a cash transfer program (De La O, 2013) they are *more* likely to vote. Tilley et al. (2018)

¹Some two-year ARM borrowers are not included in the final sample. Two-year ARM borrowers are especially likely to be subprime, a fact we revisit later in the paper.

reconciles these results: People respond to shocks to their personal finances when they attribute the shocks to the government (see also, e.g., (Lewis-Beck and Stegmaier, 2013)). In our setting, then, we need registered voters to plausibly attribute some of their lower mortgage payment to the incumbent government. Though declines in treated households’ mortgage rates are largely *not* a function of choices made by elected officials in this case, we consider it reasonable that some Americans might mis-attribute their windfall to the government since a body of evidence illustrates that they do not understand how or why interest rates change but do know, at a very high level, that some quasi-governmental agency is involved (Skinner and Binder, 2021; Weber et al., 2022).

Specifically, we use the sample of registered voters who originated ARMs in between 2005 and 2007, still had active ARMs as of 2012, were affiliated with either the Democratic or Republican party, and were registered to vote in both 2008 and 2012, to estimate the following model:

$$\text{Participation}_{i, 2012} = \beta \times \text{Rate Reset Before 2012}_i + \text{Controls}_i \times \Theta + \eta_{\text{County} \times \text{Originaion Year}} + \epsilon_i, \quad (1)$$

where $\text{Participation}_{i, 2012}$ is a dummy equal to 1 if registered voter i voted in the 2012 federal elections. $\text{Rate Reset Before 2012}_i$ is a dummy equal to 1 if voter i ’s ARM had a two-year or five-year fixed rate period. Our most important control variable is the voter’s participation decision in 2008, that serves as a baseline for how likely each voter is to participated. We also control for registered voter i ’s combined loan-to-value (CLTV) ratio, interest rate, and mortgage amount at the time of origination, the purpose of the mortgage (purchase, refinance, or unknown), age, race, ethnicity, and state of birth (a dummy equal to 1 if born in NC and 0 otherwise), the median income and unemployment rate in their census tract, and the delinquency rate in their ZIP-Code. Finally, η indexes county-by-mortgage origination years.

The granular fixed effects help account for how economic conditions, like home prices or unemployment, have changed in the county since mortgage origination. And the control variables absorb some of the differences between treated and control variables that might be correlated with 2012 participation, like race, age, and size of mortgage (our proxy for wealth). Our most important control variable is participation decisions in 2008. While there may exist unobserved differences along, for example, religiosity, income, or risk aversion, between the treated and control groups, participation in 2008 should control for a great deal of this. Indeed, we show in the results section that after con-

trolling for party affiliation, county, and 2008 participation, the inclusion of all the other mortgage and demographic control variables has no effect on the results.

Our specification implies the following experiment. We consider two similar households who live in the same county and take out a new adjustable-rate mortgage at the same time. One of these borrowers chooses an ARM with a five-year fixed rate period while the other chooses an ARM with a ten-year fixed rate period. Our assumption is that this assignment is exogenous to the change in their participation decision between 2012 and 2008. To be clear, borrowers who chose five-year ARMs may be different than those who chose ten-year ARMs. But the panel nature of the data set means we do not have to rely on just cross-sectional variation in participation in 2012 and can instead compare registered voters' 2012 participation decision to their 2008 decisions. We present evidence consistent with the validity of our test's identifying assumption in a later section.

We also estimate models that allow the effect of the treatment to vary as a function of characteristics of registered voter i and their mortgage. For example, we investigate if Democrats and Republicans respond differently to the treatment by estimating the following model:

$$\begin{aligned} \text{Participation}_{i, 2012} = & \beta \times \text{Rate Reset Before 2012}_i + \gamma \times \text{Democrat}_i \\ & + \lambda \times \text{Democrat}_i \times \text{Rate Reset Before 2012}_i + \text{Controls}_i \times \Theta + \eta_{\text{County} \times \text{Originaion Year}} + \epsilon_i, \quad (2) \end{aligned}$$

where Democrat_i is a dummy equal to one if registered voter i is affiliated with the Democratic party and 0 if they are affiliated with the Republican party.

4 Main Results

[TABLE 3 HERE]

In [Table 3](#) we present a first look at our results. We begin by asking how receiving a rate reset before the 2012 election affected all voters in the sample. Column (1) includes an interaction term of treatment by party affiliation. Columns (2) and (3) restrict the sample to Democrats and Republicans respectively and estimate [Equation 1](#). The main coefficient of interest in column (1) is the interaction term between receiving a reset before the 2012 election and being a Democrat. It shows that relative to a Republican who received a reset before the election, the same reset makes a Democrat a relative

4.1% more likely to vote on average.

In columns (2) and (3), we ease interpretation by estimating Equation 1 for Democrats and Republicans separately. We find that receiving a reset before the election increases Democrats' turnout rate by 2.1 percentage points whereas the same reset reduces the Republicans' turnout rate by 2.1 percentage points. Column (1) formally compares these two estimates and yields the 4.1 percentage point "wedge" discussed above. These results are consistent with our hypothesis that improved pocketbook makes voters more supportive of the incumbent. Democrats support the (Democratic) incumbent by turning out and voting while Republicans support that same incumbent by staying home.

The sample used in Table 3 includes voters who did not vote in the 2010 midterm election *and* those who did. Perhaps not surprisingly, 97% of voters who voted in the 2010 midterm election also voted in the 2012 general election. These "always voters" are, again not surprisingly, not especially responsive to changes in the mortgage payments. After all, the kinds of voters who vote in the 2010 midterm will vote in the presidential elections almost no matter what. We therefore turn our attention to the registered voters who did not vote in the 2010 midterm election in Table 4.

[TABLE 4 HERE]

First, to better motivate the sample split, we analyze the subsample of voters who *did* vote in the 2010 midterm election in columns (1) through (3). We can see that all the relevant coefficients are almost precisely 0, consistent with a lower monthly mortgage payment being unlikely to affect the participation decisions of those registered voters who were almost surely going to vote no matter what.

Columns (4) to (6) estimate the same specifications, but for voters who did *not* participate in the 2010 midterm election. The coefficients for the "sometimes voters" are much larger than those in Table 3. Specifically, treated Democrats are 6.3 percentage points more likely to vote and treated Republicans 4.6 percentage points less likely to vote than their same-party counterparts. Column (4) formalizes this wedge at just over ten percentage points. We conclude that the results presented in Table 3 are driven exclusively by registered voters who skipped the midterm election, those marginal voters that so often determine election outcomes.

5 Robustness

In this section, we conduct a number of robustness, falsification, and external validity tests.

5.1 Selection into the Treatment versus Control Groups

We begin with an investigation of the concern that households in 2005, 2006, and 2007 were non-randomly choosing the lengths of their ARM’s fixed-rate periods. Indeed, much evidence suggests that those households who chose 2-year ARMs were more likely to be subprime. Our descriptive statistics are consistent with borrowers who chose shorter fixed-rate periods also borrowing much less and paying higher interest rates. They are younger, more likely to be Black, more likely to have been born in NC, and live in neighborhoods with lower median incomes and higher delinquency and unemployment rates. The concern in our setting is that households’ participation decisions in 2012 are not being caused by their interest-rate reset or lack thereof, but rather their correlated demographic characteristics. In this subsection, we address this concern in four ways.

5.1.1 (Lack of) Importance of Control Variables

[TABLE 5 HERE]

First, we re-estimate the exact same specifications estimated to create Table 4, but without any of the control variables. We present these results in Table 5. The key result of this analysis is that the estimates are statistically and economically indistinguishable from those in the version of the table that includes the controls. This helps address the concern about omitted variable bias because, after conditioning on party, participation in 2008, and county-by-origination year fixed effects, the inclusion of those variables that we *do* observe – CLTV, mortgage amount, interest rate, transaction type (purchase, refinance, or unknown); borrower age, race, ethnicity, birth state; and neighborhood median income, unemployment rate, and zip code-level delinquency rate – do not affect the magnitude of the estimated treatment effect. We argue that the “null” effects of these control variables suggest that the omission of other variables might be similarly innocuous.

5.1.2 Parallel Trends

The fact that we observe participation in 2008 is helpful not just because we can include it as a control variable, but because we can look at 2008 participation as the outcome of interest and, in this way, test for parallel trends. It is this logic that motivates [Figure 1](#), where we showed that all groups had similar participation rates in the 2008 election. In this subsection, we conduct this test more rigorously.

[TABLE 6 HERE]

In [Table 6](#), we document no effect of when households' ARMs reset on participation in 2008. Columns (2) and (3) confirm that this null result is not obscuring differential differences between Democrats and Republicans. To the extent that borrowers whose ARMs reset before the 2012 election are different from those borrowers whose ARMs reset after the 2012 election, these differences are *not* reflected in their participation decisions in 2008. We take this as evidence in support of our assumption that there would also not have been differences in their 2012 decisions but for the unexpected moves to their interest rates.

Simply, we do not argue that there are no differences between our treated and control groups. Rather, we claim that so long as any differences that affect voting decisions affect those decisions in the same way in 2008 and 2012, our design will be valid. While we cannot prove this assumption, for the standard reasons, we can observe households' conditional participation decisions in 2008 (in this subsection) and their unconditional participation decisions in 2012 (in the previous subsection). If the two groups were very different in a way that was correlated with voter participation, we might expect to see some of that difference reflected in these tests. But, in both cases, we fail to document such a difference.

Using similar logic, we next analyze participation decisions in 2016. On the one hand, by this point, everybody's ARMs have reset to lower rates and the wedge in mortgage payments between treatment and control groups should have greatly diminished. On the other hand, the treatment group will still have experienced many more years of lower mortgage payments. Our evidence presented in [Table 6](#) is consistent with both stories. That is, no longer do we find *significant* differences between households that made different mortgage contract choices in 2005, 2006, and 2007, consistent with the treatment no longer having a meaningful economic impact on households. But we also

document a relatively large magnitude and can therefore not rule out fairly large treatment effects on 2016 participation.

5.1.3 Subsampling on Length of Fixed-Rate Period

Our final strategy for addressing endogenous assignment to the treatment group is to analyze subsets of ARM borrowers whose ARM choices might be especially likely to have been randomly assigned. We do this by limiting our sample to specific categories of fixed-rate period length. For example, two-year ARM borrowers are well known to have been relatively worse credit risks. So, we simply omit them and compare within just 5-, 7-, and 10-year ARM borrowers. The choice between 5- and 7-year ARMs seems especially likely to have been random, so, in column (2) of [Table 7](#), we compare just those two groups.

[TABLE 7 HERE]

In all cases, we find that the effect of the treatment, having an ARM reset in the months or years leading up to the 2012 election, makes Democrats relatively more likely to vote relative to Republicans treated in the same way.

5.1.4 Variation in Reset Timing within 7-Year ARM Borrowers

Our final test in this subsection takes the logic used in the previous subsection to its natural end point and compares *just* within those borrowers who chose 7-year ARMs in either 2005 or 2006. This sample is relatively small, but has the distinct advantage of comparing borrowers who made the *same* fixed-rate length decision and therefore means we limit our variation to *just* timing with respect to origination. Those who originated their 7-year ARMs early in this period (the first ten months of 2005) had seen their rates automatically reset before the 2012 election while those who made the same mortgage decision in 2006 had not yet received an automatic rate reset by the time of the 2012 election.

[TABLE 8 HERE]

The main result can be found in column (2) of [Table 8](#) where we show that Democratic 7-year ARM borrowers whose interest rates had reset prior to the 2012 election were relatively more likely

to have participated in 2012 than their Republican counterparts. There might, of course, still be non-random assignment to the treatment group if borrowers who made originated their mortgages later in the pre-period were systematically different from those who borrowed earlier. But, importantly, the threat to this assumption is different than the threats to the validity assumption of our main research design. This adds another hurdle that any alternative explanation to ours – that Democrats who received positive cash flow shocks were relatively more likely to go vote in 2012 than in 2008 as compared to Republicans – needs to clear.

Columns (1) and (3) of [Table 8](#) replicate the same test of the parallel trends assumption conducted in [Table 6](#). We find, consistent with the validity of this research design, that both treated and control households made similar participation decisions in 2008, when they were both still in their fixed-rate period. As before, the treated group is more likely to participate in in the 2016 election, but not significantly so.

5.2 Selection into the Estimation Sample

Next, we address a second kind of selection, selection into the estimation sample. By requiring voters to still have an active ARM in 2012, we drop from our sample those ARM borrowers who voluntarily refinanced, foreclosed, or moved away in the years preceding the 2012 election as in [Di Maggio et al. \(2017\)](#). This sample restriction leaves a little under half of all voters who originated ARMS in 2005 through 2007. A limitation of the voter data is that we cannot consistently follow households as they move. But, of those who stayed put, approximately 60% refinance their mortgages. And, not surprisingly, refinancing activity is much higher for the control group, those borrowers with 7- or 10-year ARMs whose rates would *not* have automatically resetting. This makes sense. They wanted to take advantage of lower rates and had to refinance to do so.

The implication for us is that that those borrowers with longer fixed rate periods who *were* still in the sample were either unable to refinance or not aware that refinancing was an option. This makes the control group, relative to the treatment group, either especially liquidity constrained or especially unaware of the benefits of refinancing ([Keys et al., 2016](#)). Importantly for the validity of our main results, this sort of selection into the sample is reflected in both the pre- and post-periods. So, to the extent that the control group is different from the treatment group, this should be addressed with the same analyses conducted in the previous section. This type of non-random

attrition does, however, raise questions about one kind of external validity. Namely, how does *having* an automatically adjusting interest rate affect households who voluntarily refinance? This is surely an interesting question, but, in the interest of focusing this paper on the role of cash-flow shocks to voter participation via pocketbook voting mechanisms, we leave it to future work.

5.3 Renters – A Natural Falsification Group

An advantage of the voter registration data is that it includes all North Carolina residents registered to vote, not just homeowners. Our main dataset of homeowners is constructed with by matching the sample of owners/borrowers in the deeds and mortgage data to registered voters using name and address. In those cases where we can confidently assign a voter to a property but they have a last name that is not on the deed, we assume they are renters. This, in all likelihood, mis-classifies some homeowners as renters, especially spouses with different last names, but nevertheless suggests a natural falsification test. One potential concern is that voters living in places where certain types of ARMs were especially utilized might be partially driving our main result. By looking at renters whose *landlords* had ARMs resetting before or after the 2012 election, we can address this concern head on.

[TABLE 9 HERE]

We show in [Table 9](#) that renters seem completely unaffected by the ARM choices of their landlords. This null result allows us to rule out a number of potential environmental endogeneity concerns that might be raised in objection to our main results.

5.4 External Validity

Our analysis focuses on North Carolina largely for data availability reasons. Being able to track households' registration and participation decisions is something unique to the North Carolina voter data. In an effort to speak to the generalizability of our results, though, we turn to Ohio, another state with good voter data.² An important limitation of Ohio for our purposes is that voters do not

²Unfortunately, Ohio does not publish historical snapshots of voter registration. The only voter information available they provide relates to voters currently³ registered to vote. In other words, we only observe the 2008 and 2012 participation decisions of those Ohio voters who are still registered to vote today. Fortunately, we were able to locate an old voter file from 2014 very graciously shared with us by Professors Ethan Kaplan and Haishan Yuan ([Kaplan and Yuan, 2020](#)).

register with a party. Consequently, the only way to determine party affiliation is to observe which primaries voters vote in. If they vote in the Democratic party, we assume they are Democrats, and if they vote in the Republican party, we assume they are Republicans. For many purposes, this assumption is fairly innocuous. In our case, though, the kinds of voters who participate in primaries are *also* the kinds of voters who participate every presidential election, similar to the voters in North Carolina who participate in the 2010 Midterm.

[TABLE 10 HERE]

We re-estimate our main specifications using Ohio voter data and find results with similar magnitudes, though the standard errors are much larger. These large standard errors are the result of a sample made small by jointly requiring voters to have a party affiliation in the data – only possible if the voted in a primary – and to have *not* voted in the 2010 midterm. The intersection of voters who vote in primaries and do not vote in midterms is small.

6 County-Level Evidence

An important limitation of the previous analysis is that we observe only participation at the individual level. That is, we do not see whom each participating voter votes for. Our working assumption that we use to reach our conclusions is that registered Democrats, when they vote, are likely voting for the Democratic candidates and registered Republicans likely vote for the Republican candidate. But of course we cannot know this with certainty. One option is to turn to survey data. But survey data is plagued by recall bias, partisan bias, and, especially when voting decisions are being surveyed, dishonesty. Instead, we look to aggregated data. Specifically, we compute the share of the vote in each county that voted for the Democratic incumbent in 2008 and 2012. We relate that share to an aggregated measure of exposure to resetting adjustable-rate mortgages. To create this measure we divide the total number of two-year and five-year ARMs issued in each county in 2006 or 2007 by the Citizen Voting Age Population from the 2006-2010 5-Year American Community Survey.⁴

[FIGURE 2 HERE]

⁴Source: <https://www.census.gov/programs-surveys/decennial-census/about/voting-rights/cvap.2010.html>.

In [Figure 2](#), we plot a binscatter with twenty bins of counties based on the measure of exposure to resetting ARMs leading up to the 2012 election. Looking at the y-axis, we see that all county bins saw lower shares won by the Democrat in 2012 than in 2008. However, this decline in the share won by the Democrat decreases as the share of the voting eligible population with resetting ARMs increases. This finding is entirely consistent with our individual-level evidence: Americans vote with their pocketbooks. At the individual-level we see this in the increased turnout of Democrats and the decreased turnout of Republicans. At the county-level we see this in the relatively greater share won by the incumbent in places where more of the electorate received the positive cash-flow shock via lower mortgage payments.

7 Potential Channels

Our primary contribution is to demonstrate a causal relationship between positive shocks to households' balance sheets, their participation decisions, and electoral outcomes. The relevance of the pocketbook hypothesis and the lack of clean evidence either supporting or refuting it makes this paper's contribution important. In this section we ask a question that is challenging to answer conclusively: Through what precise channels does our documented relationship operate? We propose two possibilities and discuss the evidence in favor of each of them.

One possibility is a resource constraint channel. [Hall et al. \(2021\)](#); [Margalit \(2019\)](#); [McCartney \(2021\)](#) find that negative shocks decrease turnout, and argue this is due to resource constraints. In all three papers, this result is shown regardless of party affiliation. However, our opposite findings for Democrats and Republicans does not square with a pure resource constraint relaxation channel. A second possibility is that households reward the incumbent for positive shocks to their pocketbooks. This channel is consistent with our main results and the one we have been arguing for throughout the paper. In conclusion, it is largely the divergence between Democrat and Republican responses to the cash flow shock that pushes us towards a reward-the-incumbent and away from a relaxed-resource-constraint channel.

Our work also speaks to the debate on the relative importance of retrospective versus prospective voting. Since we use changes to interest rates in the years leading up to the election, our evidence suggests households must be voting retrospectively. That said, we face the standard problem that

we cannot be sure whether households are using past changes to infer future likelihoods. But, by comparing households who live in the same places and likely share exposure to the same overall labor markets ([Burden and Wichowsky, 2014](#)) and news media ([Prior, 2013](#)), our results are best explained by households voting with their personal pocketbooks and personal financial experiences in mind.

8 Conclusion

In this paper, we show that plausibly exogenous reduction in mortgage payment makes voters more supportive of the incumbent president, providing evidence for pocketbook voting. Our paper opens the door for future work on how mortgage contracts affect election outcomes.

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Figure 1: Unconditional Participation Rates of Treated and Control Democrats and Republicans

This figure plots the participation in the 2008 and 2012 election of registered voters who did not participate in the 2010 midterm election. Treated voters are those whose ARMs reset (resulting in lower payments). The control group is voters whose ARMs did not reset until after the 2012 election. Registered voters are split into four groups: treated and control Democrats and Republicans. The data is based on the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set and is described in [Table 2](#).

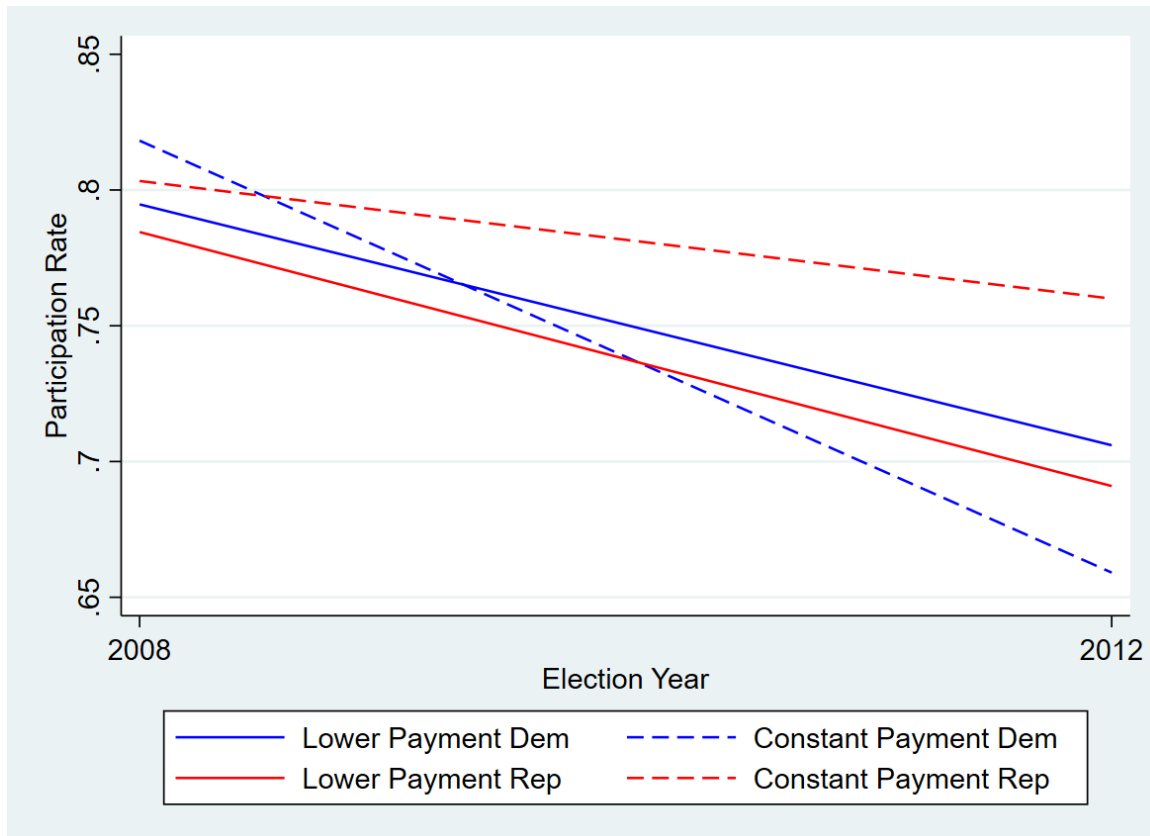


Figure 2: United States County-Level Evidence

This figure plots a bin scatter showing the relationship between a county's share of ARMs that reset by 2012 with the change in the county's Democrat vote share between 2008 and 2012. State fixed effects are included. The share of ARMs that reset by 2012 is calculated by dividing the number of two- and five-year arms originated in 2006 and 2007 by the voting age population from the 2010 Census. The sample contains the 1,338 counties covered by CoreLogic's deeds data in both 2008 and 2012.

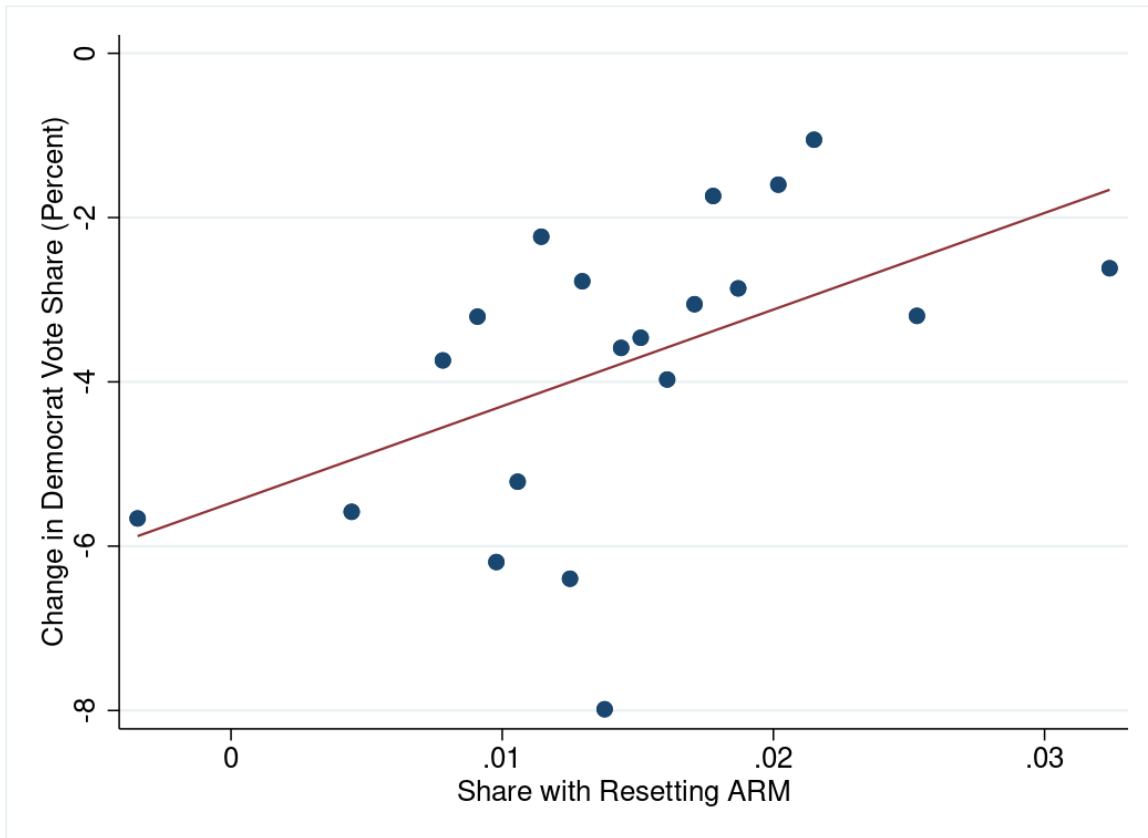


Table 1: Summary Statistics

This table presents summary statistics for the sample of registered voters who took out an ARM in 2005, 2006, or 2007 that was still active as of 2012, whose rate reset followed the 2008 November election, and whose rate reset was not in the two months following the November, 2012 election. To be included in the sample, we further require voters to be registered as either Democrat in both 2008 and 2012 or Republican in both 2008 and 2012. The data is based on the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set. Columns (1) and (2) describe the full sample. In columns (3) and (4), we limit the sample to those that did not vote in the 2010 midterm election in November. The participation dummies are dummies for if the individual voted in the November election of that year. Whether the ARM reset before the 2012 election is our treatment variable. CLTV is the combined-loan-to-value ratio at the time the mortgage was originated. Mortgage amount is in 100,000s and reflects the amount borrowed of the primary ARM loan. The interest rate is at the time of origination. We classify loan type as either purchase, refinance, or unknown. The demographic variables are all from the voter file. Census-tract tract-level median income and unemployment rate is from the US Census, and the ZIP Code-level delinquency rate is from McDash.

<i>Sample:</i>	All		Non-2010 Voters	
	Mean	Std Dev	Mean	Std Dev
<i>Participation</i>				
Voted in 2012	0.86	0.35	0.70	0.46
Voted in 2010	0.59	0.49	0.00	0.00
Voted in 2008	0.90	0.30	0.79	0.41
<i>Party</i>				
Democrat	0.49	0.50	0.51	0.50
<i>Mortgage</i>				
Reset before the 2012 election	0.83	0.37	0.84	0.37
CLTV	0.88	0.26	0.89	0.25
Mortgage Amount	2.45	1.72	2.28	1.61
Interest Rate	6.93	1.55	7.04	1.60
Purchase	0.39	0.49	0.42	0.49
Refinance	0.40	0.49	0.37	0.48
<i>Demographic Characteristics</i>				
Age	47.12	12.48	43.56	11.98
Black	0.23	0.42	0.19	0.39
Hispanic	0.02	0.12	0.03	0.16
Born in NC	0.36	0.48	0.34	0.47
<i>Neighborhood Characteristics</i>				
Zip Code Median Income	68,300	29,500	67,800	29,300
Census Tract Delinquency Rate 08-12	0.13	0.07	0.14	0.07
Census Tract Unemployment Rate	0.09	0.04	0.09	0.04
Count	15,240		6,242	

Table 2: Summary Statistics - Comparing Treated and Control Registered Voters

This table compares various subsamples of the full sample described in [Table 1](#). The first two columns restricts to just those voters who did not participated in the 2010 November election and compares the treated and control registered voters within that group. Within this sample, columns (3) and (4) compare treated and control registered voters affiliated with the Democratic party and columns (5) and (6) further compare treated and control registered voters affiliated with the Republican party. The data is based on the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set.

<i>Sample:</i>	Non-2010 Voters					
<i>Subsample:</i>	All		Democrats		Republicans	
	Treated	Control	Treated	Control	Treated	Control
<i>Participation</i>						
Voted in 2012	0.70	0.72	0.71	0.66	0.69	0.76
Voted in 2010	0.00	0.00	0.00	0.00	0.00	0.00
Voted in 2008	0.79	0.81	0.79	0.82	0.79	0.80
<i>Party</i>						
Democrat	0.53	0.40	1.00	1.00	0.00	0.00
<i>Mortgage</i>						
Reset before the 2012 election	1.00	0.00	1.00	0.00	1.00	0.00
CLTV	0.89	0.87	0.90	0.87	0.88	0.87
Mortgage Amount	2.13	3.08	1.88	2.69	2.41	3.35
Interest Rate	7.18	6.29	7.53	6.34	6.79	6.26
Purchase	0.42	0.44	0.43	0.48	0.41	0.41
Refinance	0.38	0.34	0.37	0.31	0.39	0.35
<i>Demographic Characteristics</i>						
Age	43.42	44.33	43.66	45.02	43.14	43.88
Black	0.21	0.08	0.38	0.17	0.01	0.01
Hispanic	0.03	0.02	0.04	0.05	0.02	0.01
Born in NC	0.36	0.28	0.37	0.24	0.33	0.30
<i>Neighborhood Characteristics</i>						
Zip Code Median Income	65,900	77,900	61,800	74,000	70,700	80,400
Delinquency Rate 08-12	0.14	0.12	0.16	0.13	0.13	0.11
Unemployment Rate	0.09	0.08	0.09	0.08	0.08	0.07
Count	5,248	994	2,791	395	2,457	599

Table 3: Effect of Rate Resets on Voter Participation

This table presents the coefficient estimates of the effect of having an ARM reset prior to the 2012 election on participation in the 2012 election. The outcome variable is a dummy equal to 1 if the voter participated in the 2012 election. The sample is described in [Table 1](#). Control variables include CLTV, mortgage amount, interest rate, transaction type (purchase, refinance, or unknown), age, race, ethnicity, birth state, and census tract-level median income and unemployment rate (from Census), and zip code-level delinquency rate (from McDash). Standard errors, adjusted for clustering at the county level, are reported in parentheses. The data is based on the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Participated in the 2012 Election (=1)		
<i>Sample:</i>	All	Democrats	Republicans
	(1)	(2)	(3)
Reset before 2012 Election × Democrat	0.041*** (0.015)		
Reset before 2012 Election	-0.021* (0.010)	0.021** (0.010)	-0.021* (0.011)
Democrat	-0.042*** (0.015)		
Participated in the 2010 Election	0.179*** (0.013)	0.176*** (0.012)	0.176*** (0.015)
Participated in the 2008 Election	0.389*** (0.012)	0.430*** (0.015)	0.346*** (0.015)
Controls	X	X	X
County × Year FEs	X	X	X
N	14,849	7,313	7,516
Adj R2	.25	.27	.24

Table 4: Main Result – Heterogeneity by 2010 Participation

This table presents the same analysis as [Table 3](#), but restricts the sample in two ways. Columns (1) - (3) uses just the sample of voters who participated in the 2010 midterm while columns (4) - (6) use just those who did not. Control variables include CLTV, mortgage amount, interest rate, transaction type (purchase, refinance, or unknown), age, race, ethnicity, birth state, and census tract-level median income and unemployment rate (from Census), and zip code-level delinquency rate (from McDash). Standard errors, adjusted for clustering at the county level, are reported in parentheses. The data is based on the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set and is described in [Table 1](#) and [Table 2](#). *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Participated in the 2012 Election (=1)					
<i>Sample:</i>	Did Participate in the 2010 Election			Did Not Participate in the 2010 Election		
<i>Subsample:</i>	All	Democrats	Republicans	All	Democrats	Republicans
	(1)	(2)	(3)	(4)	(5)	(6)
Reset before 2012 Election × Democrat	-0.006 (0.008)			0.106*** (0.030)		
Reset before 2012 Election	-0.002 (0.006)	-0.009 (0.007)	-0.002 (0.007)	-0.043* (0.023)	0.063*** (0.022)	-0.046* (0.026)
Democrat	0.001 (0.006)			-0.103*** (0.031)		
Participated in the 2008 Election	0.062*** (0.018)	0.088*** (0.030)	0.045* (0.026)	0.440*** (0.013)	0.471*** (0.017)	0.404*** (0.018)
Controls	X	X	X	X	X	X
County × Year FEs	X	X	X	X	X	X
N	8,739	4,188	4,522	6,081	3,087	2,964
Adj R2	0.01	0.01	0.01	0.19	0.20	0.17

Table 5: Table 4 Without Controls

This Table replicates Table 4 and differs only in that no control variables are included in the specifications except for participation in 2008 and county-by-year fixed effects.

Dependent Variable:	Participated in the 2012 Election (=1)					
<i>Sample:</i>	Did Participate in the 2010 Election			Did Not Participate in the 2010 Election		
<i>Subsample:</i>	All	Democrats	Republicans	All	Democrats	Republicans
	(1)	(2)	(3)	(4)	(5)	(6)
Reset before 2012 Election × Democrat	-0.004 (0.008)			0.114*** (0.028)		
Reset before 2012 Election	-0.005 (0.005)	-0.010 (0.006)	-0.006 (0.006)	-0.054** (0.022)	0.060*** (0.020)	-0.054** (0.023)
Democrat	0.005 (0.006)			-0.109*** (0.028)		
Participated in the 2008 Election	0.065*** (0.016)	0.087*** (0.029)	0.052** (0.025)	0.446*** (0.013)	0.475*** (0.018)	0.418*** (0.017)
Controls						
County × Year FEs	X	X	X	X	X	X
N	8,968	4,310	4,628	6,242	3,172	3,038
Adj R2	0.00	0.01	0.00	0.18	0.20	0.16

Table 6: Testing for Parallel Trends – Participation in 2008 and 2016

This table presents results of an analysis similar to that presented in [Table 3](#), but changes the outcome variable to be a dummy equal to 1 if a voter participated in the 2008 general election in columns (1) - (3) and a dummy equal to 1 if a voter participated in the 2016 general election in columns (4) - (6). The sample is limited to those who did not participate in the 2010 election. Control variables include CLTV, mortgage amount, interest rate, transaction type (purchase, refinance, or unknown), age, race, ethnicity, birth state, and census tract-level median income and unemployment rate (from Census), and zip code-level delinquency rate (from McDash). In columns (4) - (6), we further control for participation in 2008. Standard errors, adjusted for clustering at the county level, are reported in parentheses. The data is based on the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Participated in the 2008 Election (=1)			Participated in the 2016 Election (=1)		
	All	Democrats	Republicans	All	Democrats	Republicans
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Sample:</i>						
Reset before 2012 Election × Democrat	-0.004 (0.040)			0.073 (0.057)		
Reset before 2012 Election	0.010 (0.028)	0.002 (0.022)	0.008 (0.027)	-0.044 (0.041)	0.024 (0.029)	-0.046 (0.042)
Democrat	0.002 (0.034)			-0.039 (0.048)		
Controls	X	X	X	X	X	X
County × Year FEs	X	X	X	X	X	X
N	6,081	3,087	2,964	5,328	2,704	2,593
Adj R2	0.03	0.04	0.03	0.03	0.04	0.04

Table 7: Restricting the Sample to Specific Fixed-Rate Period Lengths

This table presents results of an analysis similar to that presented in [Table 3](#), but restricts the samples those who did not participate in the 2010 election and to subsets of the full sample based on the length of the ARM's fixed rate period. Control variables include CLTV, mortgage amount, interest rate, transaction type (purchase, refinance, or unknown), age, race, ethnicity, birth state, and census tract-level median income and unemployment rate (from Census), and zip code-level delinquency rate (from McDash). Standard errors, adjusted for clustering at the county level, are reported in parentheses. The data is based on the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set. The sample used in column (2) is described in more detail in [Table A1](#). *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Participated in the 2012 Election (=1)			
<i>Sample:</i>	5 and 7 ARMs	5, 7, and 10 ARMs	5 and 10 ARMs	2, 3, 5, and 10 ARMs
	(1)	(2)	(3)	(4)
Reset before 2012 Election × Democrat	0.077* (0.041)	0.108*** (0.030)	0.131*** (0.041)	0.133*** (0.039)
Reset before 2012 Election	-0.032 (0.034)	-0.047** (0.023)	-0.061** (0.028)	-0.056** (0.026)
Democrat	-0.074 (0.045)	-0.102*** (0.033)	-0.133*** (0.042)	-0.133*** (0.040)
Voted in 2008	0.414*** (0.015)	0.405*** (0.015)	0.408*** (0.019)	0.448*** (0.013)
Controls	X	X	X	X
County × Year FEs	X	X	X	X
N	3,131	3,626	2,950	5,404
Adj R2	0.17	0.17	0.18	0.19

Table 8: Comparing 7-Year ARM Borrowers with Resets Before and After the 2012 Election

This table presents the result of a test that uses just voters who originated 7-Year ARMs in either 2005 and 2006 and did not participate in the 2010 election. The outcomes in columns (1), (2), and (3) are participated in the 2008, 2012, and 2016 elections, respectively. Control variables include CLTV, mortgage amount, interest rate, transaction type (purchase, refinance, or unknown), age, race, ethnicity, birth state, and census tract-level median income and unemployment rate (from Census), and zip code-level delinquency rate (from McDash). Standard errors, adjusted for clustering at the county level, are reported in parentheses. The data is based on the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Participated in the 2008 Election (=1)	Participated in the 2012 Election (=1)	Participated in the 2016 Election (=1)
	(1)	(2)	(3)
Reset before 2012 Election × Democrat	-0.027 (0.069)	0.142* (0.073)	0.076 (0.093)
Reset before 2012 Election	-0.023 (0.064)	-0.069 (0.045)	-0.043 (0.083)
Democrat	0.014 (0.058)	-0.123** (0.059)	-0.039 (0.070)
Voted in 2008		0.376*** (0.047)	0.228*** (0.045)
Controls	X	X	X
County FEs	X	X	X
N	441	441	378
Adj R2	0.08	0.15	0.08

Table 9: Falsification Group – Renters

This table uses the (falsification) sample of renters living in properties owned by landlords who had ARMs that reset prior to the 2012 election. We further require that these renters did not participate in the 2010 election. The outcomes in columns (1), (2), and (3) are participated in the 2008, 2012, and 2016 elections, respectively. Control variables include age, race, ethnicity, birth state, and census tract-level median income and unemployment rate (from Census), and zip code-level delinquency rate (from McDash). Standard errors, adjusted for clustering at the county level, are reported in parentheses. The data is based on the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

<i>Sample:</i> Dependent Variable:	Implied Renters		
	Participated in the 2008 Election (=1)	Participated in the 2012 Election (=1)	Participated in the 2016 Election (=1)
	(1)	(2)	(3)
Reset before 2012 Election × Democrat	0.042 (0.055)	0.041 (0.050)	-0.004 (0.065)
Reset before 2012 Election	-0.052 (0.042)	-0.012 (0.055)	-0.027 (0.060)
Democrat	0.039 (0.049)	-0.045 (0.045)	-0.027 (0.063)
Voted in 2008		0.487*** (0.019)	0.364*** (0.016)
Controls:	X	X	X
County × Year FEs:	X	X	X
N	3,154	3,154	2,665
Adj R2	0.09	0.28	0.16

Table 10: Replicating the Main Result in Ohio

This table presents the same analysis as **Table 4** using registered homeowner voters in the state of Ohio. Standard errors, adjusted for clustering at the county level, are reported in parentheses. The data is based on the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Participated in the 2012 Election (=1)					
<i>Sample:</i>	Did Participate in the 2010 Election			Did Not Participate in the 2010 Election		
<i>Subsample:</i>	All	Democrats	Republicans	All	Democrats	Republicans
	(1)	(2)	(3)	(4)	(5)	(6)
Reset before 2012 Election × Democrat	-0.016 (0.014)			0.101 (0.066)		
Reset before 2012 Election	0.008 (0.014)	-0.005 (0.017)	0.007 (0.013)	-0.061 (0.050)	0.033 (0.053)	-0.050 (0.053)
Democrat	-0.011 (0.013)			-0.081 (0.054)		
Participated in the 2008 Election	0.065*** (0.020)	0.097** (0.046)	0.051** (0.025)	0.166*** (0.040)	0.240*** (0.057)	0.100* (0.053)
Controls	X	X	X	X	X	X
County × Year FEs	X	X	X	X	X	X
N	2,886	1,411	1,460	1,026	670	335
Adj R2	0.01	0.01	0.04	0.04	0.06	0.02

A Online Appendix - Supplemental Figures and Tables

Table A1: Summarizing the Sample of 5-, 7-, and 10-year ARM Borrowers

This table is identical to **Table 2** but restricts to the subsample of those registered voters with 5-, 7-, and 10-year ARMs.

<i>Sample:</i> <i>Subsample:</i>	Non-2010 Voters with 5-, 7-, or 10-Year ARMs					
	All		Democrats		Republicans	
	Treated	Control	Treated	Control	Treated	Control
<i>Participation</i>						
Voted in 2012	0.71	0.72	0.70	0.66	0.71	0.76
Voted in 2010	0.00	0.00	0.00	0.00	0.00	0.00
Voted in 2008	0.80	0.81	0.80	0.82	0.80	0.80
<i>Party</i>						
Democrat	0.46	0.40	1.00	1.00	0.00	0.00
<i>Mortgage</i>						
Reset before the 2012 election	1.00	0.00	1.00	0.00	1.00	0.00
CLTV	0.83	0.87	0.85	0.87	0.82	0.86
Mortgage Amount	2.50	3.09	2.21	2.69	2.75	3.34
Interest Rate	6.22	6.29	6.40	6.34	6.07	6.25
Purchase	0.46	0.44	0.47	0.48	0.45	0.41
Refinance	0.34	0.33	0.34	0.31	0.35	0.35
<i>Demographic Characteristics</i>						
Age	43.29	44.31	43.29	44.91	43.28	43.92
Black	0.12	0.08	0.25	0.18	0.01	0.01
Hispanic	0.03	0.02	0.04	0.05	0.02	0.01
Born in NC	0.29	0.28	0.28	0.24	0.29	0.30
<i>Neighborhood Characteristics</i>						
Zip Code Median Income	74,400	78,000	70,000	74,000	78,200	80,600
Delinquency Rate 08-12	0.12	0.12	0.14	0.13	0.11	0.11
Unemployment Rate	0.08	0.08	0.08	0.08	0.08	0.07
Count	2,713	991	1,237	394	1,476	597