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Household Finance Shapes Political Participation: Evidence from Mortgage Refinancing*

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Abstract

We study mortgage refinancing during the Great Recession, a period marked by a dislocated housing market, major government programs, and large potential gains from refinancing. Using quasi-experimental variation from movements in mortgage rates and eligibility cutoffs in HARP, we show that borrowers who refinanced between 2009 and 2012 were more likely to vote in the 2012 general election than otherwise similar borrowers who did not refinance. The increase in turnout is concentrated among households that experienced larger payment reductions and among politically independent voters. Our findings show that policy-induced mortgage relief can extend beyond household balance sheets into the political sphere and highlight the deep connections between finance and politics.

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

Keywords: Household Finance, Mortgages, Interest Rates, Political Participation, Voter Turnout

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1 Introduction

This paper studies the link between household finance and politics, analyzing the impact of policies that increase the ease and benefits of mortgage refinancing on households' political participation or civic engagement. Government interventions in mortgage markets have featured prominently in the political economy of the U.S. and most democracies for over a century. The dramatic economic and financial consequences of these policies are well documented (Agarwal et al., 2023; Calomiris and Haber, 2014; Fieldhouse et al., 2018; Green and Wachter, 2005; Mian et al., 2013). And a large literature further emphasizes that political economy factors influence the supply of mortgage credit, household balance sheets, and the broader financial system (Carvalho, 2014; Dinç, 2005; La Porta et al., 2002; Rajan and Ramcharan, 2011; Sapienza, 2004).

Despite this extensive literature, there is almost no evidence on how voters themselves respond to these government interventions in credit markets. Mortgages are the largest liability for many households, and lower mortgage rates along with reduced refinancing frictions can greatly improve the financial health of homeowners (Di Maggio et al., 2017; Keys et al., 2016). Standard rational models of voter turnout would thus predict increased political participation after policies that enable households to refinance and increase their disposable income (Blais, 2000; Smets and Van Ham, 2013). On the other hand, a large body of work documents persistent inertia, inattention, and other frictions that limit households' responses to even sizable financial incentives, raising doubts about whether voters respond to such policy-induced gains (Campbell and Ramadorai, 2025).[1]

To study the impact of mortgage refinancing on political participation, we compile a new individual-level dataset that jointly describes the mortgage and voting behavior of millions of households. We focus on the U.S. mortgage market between 2009 and 2012, a period marked by severe housing market distress and an unusually broad set of government interventions aimed at lowering borrowing costs and expanding access to refinancing. Policy rates fell sharply, the Federal Reserve's large-scale asset purchases further reduced long-term mortgage rates, and fiscal programs such as the Home Affordable Refinance Program (HARP) relaxed refinancing constraints for many borrowers

[1] A large literature has shown for example that household behavior is often characterized by inertia, so that households can ignore even valuable opportunities to refinance—see (Andersen et al., 2020). The ideas in (Gabaix and Laibson, 2006) also apply in our setting, as political parties can shroud or conflate economic and non-economic factors in order to make it difficult for voters to respond to only economic factors. (Wolf, 2023) provides examples of this "shrouding" in the case of race and redistribution in the U.S.. (Lewis-Beck et al., 2008) consider these issues econometrically. (Kamada and Kojima, 2014) and (Boleslavsky and Cotton, 2015) develop information based models of voter choice with a similar intuition.

(Abel and Fuster, 2021; Agarwal et al., 2017, 2023; Amromin et al., 2020; Cloyne et al., 2020).

These interventions to reduce interest rates and refinancing frictions in the mortgage market created significant opportunities for homeowners to improve their financial situations. For those who refinanced, monthly mortgage payments typically fell by over $300, generating present-value savings in the tens of thousands of dollars and boosting consumption, voluntary deleveraging, and credit scores (Abel and Fuster, 2021; Fuster and Willen, 2017). We ask whether households that refinanced, reducing their mortgage rates by an average of 161 basis points, differed in their likelihood of voting in the 2012 election relative to otherwise similar households that did not refinance. Turnout is a central political outcome in this context: it is the primary channel through which households influence public policy, elected officials respond to voters rather than citizens more broadly, and participation is widely viewed as a barometer of civic engagement and democratic legitimacy (Dalton, 2017; Griffin and Newman, 2005; Leighley and Nagler, 2013; Lijphart, 1997; Norris, 2014; Schlozman et al., 2012).

Identifying the effects of mortgage relief on turnout is challenging. Voting is the outcome of a complex decision-making process that remains difficult to model empirically (Margalit, 2019; Smets and Van Ham, 2013), and the same socioeconomic characteristics that predict refinancing also predict participation. In our context, for example, poorer and less-educated citizens are both less likely to vote (Brady et al., 1995; Lijphart, 1997) and less likely to refinance (Andersen et al., 2020; Keys et al., 2016). To address these endogeneity concerns, we begin with a benchmark specification that conditions on rich observables and prior voting behavior, and then employ three complementary quasi-experimental research designs built around government interventions in the mortgage market.

The benchmark specification uses rich, matched mortgage-voter data to compare otherwise similar borrowers who differ only in whether they refinanced. We condition on detailed demographic, housing, and mortgage characteristics, location-by-time fixed effects, and, crucially, each individual's participation in the 2008, "pre-period," general election. This absorbs the effects of even unobserved characteristics that drive both participation and refinancing decisions, at least to the extent that these characteristics and their effects are time invariant. When we take this baseline model to our sample of more than ten million borrowers who originated fixed-rate mortgages between 2005 and 2008 and were eligible to vote in both the 2008 and 2012 general elections, we find that those who refinanced were approximately 2.3 percentage points more likely to vote in the 2012 election than otherwise similar borrowers who did not refinance. This estimate is robust to additional controls for income and combined loan-to-value ratios. And intuitively, the estimates increase when the realized savings from refinancing are larger.

While these basic correlations suggest that government policies that promoted refinancing increased political participation in 2012, the benchmark research design may not fully address endogenous selection. We thus exploit three sources of quasi-experimental variation. First, we use a rate-gap instrument that leverages variation in borrowers' ex-ante exposure to subsequent interest rate declines arising from the timing of mortgage origination (Fonseca and Liu, 2024). This approach builds on the fact that borrowers with higher initial rates, who would thus experience larger debt savings by refinancing, are more incentivized to refinance and thus to actually refinance (Agarwal et al., 2013; Keys et al., 2016; Stanton, 1995). Consistent with the relevance of this instrument, we document a strong first stage: households with higher initial rates are substantially more likely to refinance when rates fall during the 2009-2012 period. We then use this instrument to predict refinancing and find that refinancing increases turnout by about 3.8 percentage points.

A related strategy exploits refinancing incentives generated by the Federal Reserve's first large-scale asset purchase program (QE1). QE1 led to a rapid and substantial decline in mortgage rates and triggered a surge in refinancing, with effects concentrated among borrowers with conforming loans (Di Maggio et al., 2020). We use conforming-loan eligibility—determined by whether a borrower's mortgage balance at origination was below the conforming loan limit—as an instrument for exposure to QE1-driven refinancing incentives. Because conforming loan limits were set prior to QE1, this exposure is predetermined with respect to subsequent political participation. Borrowers eligible for QE1-induced refinancing were substantially more likely to refinance, yielding a strong first stage, and instrumenting refinancing with conforming-loan eligibility implies that refinancing increased turnout in the 2012 election also by about 3.8 percentage points. This estimate is robust to varying the bandwidth around the conforming loan limit in ways that exclude endogenous shifts below the cutoff. While this design contrasts borrowers across segments of the mortgage market rather than exploiting a sharp cutoff, the verisimilitude of the rate-gap and QE 1 estimates provides independent evidence that policy-driven reductions in borrowing costs increased political participation.

Our third research design uses a fundamentally different source of variation, exploiting the quasi-random variation in access to refinancing generated by the Home Affordable Refinance Program (HARP), following Abel and Fuster (2021) and Karamon et al. (2017). HARP expanded refinancing access for highly leveraged borrowers by allowing eligible homeowners to refinance into lower-rate mortgages, generating substantial reductions in monthly payments and default risk. Eligibility for HARP depended on whether a borrower's mortgage had been purchased by Fannie Mae or Freddie Mac on or before May 31, 2009, a timing decision outside the borrower's control. We therefore use this cutoff-based eligibility as an instrument for refinancing.

Focusing on GSE-eligible loans originated around the cutoff, eligible and ineligible borrowers are balanced on observable characteristics, including prior turnout, and HARP eligibility strongly predicts refinancing. Instrumenting refinancing with HARP eligibility implies that refinancing increased turnout in the 2012 election by about 4.4 percentage points. The larger magnitude relative to our other designs likely reflects the characteristics of the compliers in this "intention to treat" setting. Borrowers who "complied" with HARP – those who both required HARP to refinance and would not have refinanced without it – were precisely those facing the largest refinancing frictions and experiencing the greatest improvements in financial conditions when refinancing became feasible. This interpretation suggests that the political response to refinancing is mediated by the magnitude and broader economic significance of the financial relief it delivers.

We next turn to heterogeneity by baseline political engagement, proxied by party affiliation. Some theories of political participation emphasize that voters with strong partisan or ideological attachments, who are more likely to be affiliated with a major party rather than unaffiliated, participate largely for expressive, habitual, or identity-based reasons, implying that short-run changes in material well-being may play a limited role in their turnout decisions (Achen and Bartels, 2017; Feddersen and Sandroni, 2006; Gerber et al., 2003). Consistent with this view, we find relatively muted responses among voters registered with either major political party. In contrast, refinancing substantially increases participation among unaffiliated borrowers, who are more likely to be infrequent or "sometimes" voters.

In sum, we have used four different research designs to help connect household finance and politics. The estimates across these designs are broadly similar, indicating that voters who refinanced during the 2009-2012 period were also more likely to vote in the 2012 election, especially if they were not affiliated with a political party. To our knowledge, this is the first paper to document that voters, and especially independent voters, appear responsive to government interventions in the mortgage market. Let us now put these results in context.

This paper contributes to the literature on how government-led reductions in mortgage borrowing costs, whether through monetary easing or targeted refinancing programs, affect household behavior (see, e.g., Agarwal et al. (2023); Andersen et al. (2020); Beraja et al. (2019); Campbell (2006); Di Maggio et al. (2017); Ganong and Noel (2020); Keys et al. (2016)). Especially related to our paper are Di Maggio et al. (2017) and Abel and Fuster (2021). Di Maggio et al. (2017) take advantage of variation in the timing of when households with adjustable rate mortgages saw their rates reset to find that reductions in the size of mortgage payments increases car purchases and voluntary deleveraging. Abel and Fuster (2021), off of whose HARP research design we build, find that refinancing reduces borrower default rates while also inducing borrowers to access other types of debt, like auto loans, consistent with increases in consumption.

Our work adds to this important literature by showing that the bundled treatment received following mortgage refinancing has effects not just on households' pocketbooks, but on households' participation in the political process and thus society more broadly. These results also extend the growing literature in finance on the political economy of credit booms and busts (Guriev and Papaioannou, 2022; Gyöngyösi and Verner, 2022). We show that government policies aimed at mitigating credit crunches can enhance civic engagement. In this way, our findings underscore how consumer financial markets and democratic politics are deeply intertwined, and helps to explain the political economy of mortgage credit (Akey et al., 2025).

We also add to the large literature examining the political origins of housing policy and mortgage credit in the U.S., especially in the wake of the 2008-2009 financial crisis (see, for example, Calomiris and Haber (2014), Fieldhouse et al. (2018), Green and Wachter (2005), Mian et al. (2010), and Rajan (2011)). Our paper turns this perspective around to study the political consequences of mortgage relief. We show that policies improving households' access to mortgage credit and reducing borrowing costs, via both monetary easing and targeted refinance programs, can increase voter participation.

Our paper also contributes to the large literature in economics and political science that investigates how changes to individuals' financial situations affect turnout (see reviews of this literature in Blais (2000), Margalit (2019), and Smets and Van Ham (2013)). Because refinancing during this period was heavily shaped by government interventions, the setting provides unusually rich policy-driven variation for causal inference. We also broaden the mechanisms commonly studied in this literature, focusing on policy-induced changes in the cost of consumer credit and mortgage relief. In doing so, our study complements existing evidence on the political effects of other shocks to households' financial situations by showing that policies affecting mortgage markets and households' borrowing costs shape civic engagement.

In what follows, Section 2 describes the data and sample construction. Section 3 presents our benchmark results. Section 4 introduces the three quasi-experimental research designs and reports our results. Section 5 examines heterogeneity in the effects with respect to baseline civic engagement. In section Section 6, we set our findings in a broader context of the drivers of voter turnout. We conclude in Section 7.

2 Data

2.1 Data Sources

2.1.1 Voter Data

We obtain voter registration and historical voter participation data from the non-partisan data provider Labels & Lists, Inc. (hereafter "L2").[2] The voter registration data is a snapshot of all people registered to vote and includes each voter's full name, complete mailing address, party affiliation, date of registration, date of birth, and sex.[3] The historical voter participation data lists each election in which the voter cast a ballot.

One important limitation of the voter registration data is that it cannot be used as a census of current residents. There are two reasons for this. First, not all people are registered to vote. They may not be legally allowed to vote or may have never registered. Second, voter rolls are rarely purged which means that many people who have long since passed away or moved to different states are still included in the list of registered voters. If L2 were our only data source, then this would be a crippling limitation since we would not know whether a voter does not participate because they abstained, because they died, or because they were living, registered, and participating somewhere

[2] Our access to this data comes via a licensing agreement between L2 and the University of Virginia. For more information on L2, visit https://www.l2-data.com/datamapping/.

[3] Party affiliation is state reported except in the following states where it is modeled by L2: AL, GA, HI, IL, MI, MN, MO, MT, ND, SC, TX, VA, VT, WA, WI.

else. But, by using deeds data to track home ownership, we can make relatively safe assumptions about who is truly registered and eligible to participate in any election.

A second limitation of the voter data is what we observe regarding participation. Specifically, we know only whether or not a voter submitted a ballot for the election that took place on a given day. We see, for example, that a voter submitted a ballot on November 4, 2008. But, since the ballots themselves are confidential, neither we nor anyone else can legally know which specific elections – presidential, congressional, local – each voter voted in nor for whom they voted. In this paper, we focus on participation decisions, i.e., whether a voter cast a ballot, in the 2008 and 2012 general elections. A benefit of focusing on presidential elections is that, across all jurisdictions, the presidential candidates are the same on each election date, and the elections take place at the same time. This helps ensure a level of standardization across the sample.

Despite these limitations, the voter data have useful attributes for answering the questions we ask in this paper. First, with few exceptions, the coverage of registered voters and their complete participation histories is universal, allowing us to examine participation decisions of the entire electorate in the United States over time. Second, since the data is at the individual-level, we can reach much more precise causal conclusions than allowed by aggregate data. And, finally, the public availability of the data, which includes voters' full names and addresses, means we can merge in information from other public data sources, like county deeds and assessors' offices, to paint detailed pictures of households' financial situations and civic engagement.

2.1.2 Mortgage Data

We use information on property transactions and mortgage originations from county deeds and assessors' records. These publicly available data are collected and processed by CoreLogic Solutions, LLC (hereafter "CoreLogic") and come in three main parts.[4] First, we observe the complete history of ownership changes. We see the date of the transaction, the nature of the transaction, the sale price (if applicable), and the names of the new and previous owners. Second, we observe every time a mortgage that uses the property as collateral is originated. Here we see the amount of the mortgage, the date the mortgage was originated, and the names of the borrowers. Third, we see parcel-level

[4]Our access to this data comes via a licensing agreement between CoreLogic and the University of Virginia's White Ruffin Byron Center for Real Estate. For more information on CoreLogic, visit https://www.corelogic.com/360-property-data/. Note that CoreLogic has since rebranded to "Cotality."

characteristics used by local tax assessors' offices including full address, square footage, and year built.

These data are ideally suited to understanding the effects of mortgage refinancing on civic engagement for two reasons. First, we observe not only original purchase transactions and details of any purchase mortgage, but also all subsequent refinancing activity. Second, the ownership transfer data includes not just arms length sales, but also quitclaims, affidavits of death, and inter-family deed transfers among many other transaction types. This wealth of information allows us to create a very accurate and detailed picture of who owns a given parcel at any point in time.[5] We can therefore overcome the central limitation of the voter data – the often stale nature of registration data that does not reflect when individuals pass or move away.

2.1.3 Loan Ownership Data

One of our research designs uses HARP eligibility, which requires that we know whether the mortgage is held by either of the GSEs as of the end of May 2009. Since there could be unobservable differences in loan characteristics that affected a bank's decision of whether to sell a loan or keep it on balance sheet, especially during this period of falling home prices, we restrict the sample we use for this research design to just those loans that were owned by the GSEs as of the end of December 2009. Restricting the sample to loans sold to the GSEs guarantees that, within our sample, HARP eligibility is simply a function of timing rather than loan characteristics. To create these two dummy variables – held by a GSE as of May 31st, 2009 and held by a GSE as of December 31st, 2009 – we use loan performance data from ICE, McDash. We present more details on how we incorporate this data in Section 4.3.

2.1.4 Other Data

We do not observe the rate on each loan, so we assign each loan a rate equal to the prevailing mortgage rate during the month it was originated. We use publicly available current mortgage rates data from Freddie Mac.[6] To measure borrower's income, we match loans in our sample to mortgage

[5] A limitation of the deeds data is that we observe only the names of each property's owners, not the names of its residents. We therefore focus our paper on owner-occupants, defined as property owners who are also registered to vote at that address, leaving an analysis of the political behaviors of landlords and renters to future work.

[6] https://www.freddiemac.com/pmms, accessed September 17, 2024.

applications in the publicly available Home Mortgage Disclosure Act (HMDA) data.[7] We use the crosswalk described in Bayer et al. (2022) and achieve an overall match rate of roughly 60%. We determine if a loan is under the conforming loan limit by comparing it to the publicly available conforming loan limit values published by the Federal Housing Finance Agency (FHFA).[8] Finally, we collect monthly ZIP code level median house prices (ZHVI all homes, seasonally adjusted) from Zillow.[9]

2.2 The Final Sample

Most simply, our empirical strategy compares the participation decisions of people who refinanced to those who did not. Because our focus is on the effect of refinancing on political participation, selecting a sample of homeowners with fixed rate mortgages is economically appropriate. To this end, we use the deeds data to identify all individuals that meet the following three criteria. First, we limit the sample to those households who have a mortgage to refinance. Specifically, we include only those borrowers whose most recent first lien mortgage as of January 1, 2009 was originated some time after January 1, 2005. We include both purchase mortgages and refinances of previous mortgages. Second, we require that this mortgage have a fixed interest rate. Fixed-rate mortgages represent the majority of the mortgage market and provide a more consistent basis for analyzing the effects of refinancing than adjustable-rate mortgages due to their predictability and standardization.

Our third criteria limits the sample to just those borrowers who had not sold their house or otherwise transferred ownership as of the 2012 election. This third requirement is needed in order to match homeowners to state voting records. Moving can be for random reasons, such as divorce or death of a family member. But foreclosure, job loss, or other economic factors can also precipitate moving and possibly cause non-random attrition out of the sample. In Section 4.3, we discuss how non-random attrition leads to sample selection bias and what we do to address these concerns. These sample restrictions leave us with a final sample of just over 14 million mortgages whose borrowers we attempt to find in L2.

We merge in information from the L2 voter data using the borrower/registered voter's complete

[7] https://www.consumerfinance.gov/data-research/hmda/, accessed April 23, 2024.
[8] https://www.fhfa.gov/data/conforming-loan-limit-cll-values, accessed November 17, 2021.
[9] https://www.zillow.com/research/data/, accessed March 3, 2025.

mailing address, first name, and last name.[10] In most cases, we require a match on complete address and full name, but we also allow some flexibility to pick up spouses who might not match perfectly. We do so in two ways. First, we include in the sample voters whose first name is on the deed/mortgage at that address if that address also has another person with a full name match. This captures cases where, for example, John Doe is on the deed and registered to vote and his wife is on the deed as Jane Doe and registered to vote as Jane Smith. Second, in some cases one spouse is left off the mortgage. We therefore include voters whose last name is the same as the voter with the full name match to the deed/mortgage and whose age is within fifteen years of the voter with a full name match. By including this age restriction, we aim to capture spouses, but not children or parents.[11]

Our merged data set includes nearly 11 million distinct registered voters representing 7 million distinct mortgages (half of the 14 million qualifying mortgages described above). There are three explanations that likely cover most of the unmatched mortgages. First, these mortgages might not be for owner-occupied housing, as in the case of vacation homes or rental properties. Note that this lack of match is actually a strength of our data, not a limitation, since it means our final dataset correctly restricts itself to owner-occupants. Second, while the homes might be owner-occupied, the owners themselves might be either unable to register, as is the case for non-U.S. citizens, or not interested in registering. Third, we require voters to have been registered and eligible to vote in both the 2008 and 2012 elections which drops voters that either registered for the first time after the 2008 election or unregistered before 2012. Our final dataset is conservatively built along a number of dimensions but nevertheless consists of well over ten million borrowers for whom we observe a very complete picture of mortgage and election participation decisions.[12] We describe this sample in detail in Table 1.

TABLE 1 HERE

The first two sections of Table 1 summarize our dependent and independent variables of interest. We see first that among this population of homeowners with recently originated mortgages who then did not move for at least four years, participation in general elections is very high. This is consistent

[10]For merging purposes, we define a parcel by its house number, unit number (if applicable), street direction, street name, street mode, street quadrant, and ZIP code.

[11]Our final sample consists of 93.42% full name, or type 1, matches, 1.24% type 2 matches, and 5.34% type 3 matches. Our conclusion are unchanged if we use only type 1 matches.

[12]We visualize the creation of this dataset in Appendix A.

with prior work; (DeSilver, 2022) and (Hall and Yoder, 2022) both document participation rates in presidential elections of nearly 95% among homeowners registered to vote. Also consistent with prior work, less than half of these borrowers refinanced, but those that did decreased their rate by an average of 161 basis points (Keys et al., 2016). The next three sections of Table 1 describe the voters' demographic information, the characteristics of their properties, and the characteristics of the mortgages they had as of 2009.

3 The Baseline Effect of Refinancing on Voter Participation

3.1 Empirical Framework

We begin by estimating the relationship between mortgage refinancing and voter participation using a linear probability model with a comprehensive suite of controls. The richness of our matched borrower–mortgage–property–voter data allows us to account for many observable factors that plausibly shape both refinancing decisions and political engagement, including demographics, housing and mortgage characteristics, and local market conditions.

A key advantage of our panel structure is that we observe the same individuals' electoral participation both before and after the refinancing window. This allows us to control directly for prior participation in the 2008 general election, which is strongly predictive of future participation and plausibly captures persistent individual traits—such as education, social capital, civic habits, and broader engagement—that may jointly correlate with refinancing propensities. While controlling for 2008 turnout does not eliminate all sources of endogeneity, it provides a powerful way to absorb a broad set of time-invariant differences between borrowers who refinance and those who do not.

Specifically, we estimate the following specification:

$$\begin{aligned}\text{Voted in the 2012 General Election}_i = \beta \times \text{Refinanced Between 2009 and 2012}_i \\ + \gamma \times \text{Voted in the 2008 General Election}_i + \text{Controls}_i \times \Phi \\ + \text{Party-by-County-by-Origination Quarter}_{p,c,t} \quad (1)\end{aligned}$$

where our baseline individual-level control variables include personal demographics (sex, birth year, and registration year), property characteristics (year built and property size), and characteristics of the outstanding mortgage (purpose, amount, whether or not conventional, and whether or not under the conforming loan limit). Our preferred models also include political party (Democrat, Republican, or Unaffiliated) by County by Quarter of Origination (ranging from 2005Q1 to 2008Q4) fixed effects. These fixed effects absorb broad geographic and partisan differences and ensure that identification comes from comparisons among otherwise similar borrowers who originated mortgages in the same place and time and differ only in whether they subsequently refinanced.

3.2 Baseline Result

TABLE 2 HERE

Table 2 begins with a specification that includes only party affiliation-by-county-by-origination quarter fixed effects. This result, presented in column (1), shows a strong correlation between having refinanced between 2009 and 2012 and voting in the 2012 election. Borrowers that refinanced are nearly four percentage points more likely to vote than those who did not.

In column (2), we introduce a control for whether or not the individual voted in the general election in 2008. Prior participation is highly predictive of future participation, capturing persistent traits such as education, social capital, and family influences. Not surprisingly, 2008 participation loads very strongly, with those who participated in 2008 being 37.3 percentage points more likely to vote in 2012 than those who did not. The inclusion of this control, by controlling for many of the time invariant characteristics that predict both voting and refinancing, decreases the magnitude of the estimate of interest from .0385 to .0284.

Column (3) replaces the prior participation control with our full set of demographic, property, and mortgage characteristics. While the estimated association between refinancing and turnout declines relative to column (1), the reduction is substantially smaller than when conditioning solely on prior participation in column (2). This pattern suggests that a single pre-period measure of civic engagement absorbs more of the baseline correlation than the entire set of observable financial and housing characteristics. Column (4) includes both prior participation and the full set of controls. The estimated association remains positive, economically meaningful, and precisely estimated, at 0.0236.

3.3 Variation in Treatment

We next investigate if borrowers who experienced the biggest welfare gains when they refinanced, i.e., lowered their monthly payments the most, responded with the greatest increase in political participation. To evaluate this hypothesis, we compute for each refinancing borrower the difference between the prevailing rate at the time they originated their initial mortgage and the prevailing interest rate on the date that they refinanced. We use this difference to proxy for the size of the financial gains from refinancing and then test whether borrowers who experienced a larger rate reduction are indeed more likely to vote.[13]

TABLE 3 HERE

Specifically, instead of comparing those who did not refinance to those who did, we split the refinancers into three mutually exclusive groups as a function of how much their rate decreased when they refinanced. In Table 3, we compare those who did not refinance (61% of the sample) to those who saved less than 99 basis points (5% of the sample), those who saved between 100 and 199 basis points (22% of the sample), and those who saved more than 200 basis points (12% of the sample).[14] In column (1) of Table 3, we document that the greater the savings due to refinancing, the larger the effect refinancing had on voting. And these differences are not small. Borrowers who saved fewer than 100 basis points were 1.75 percentage points more likely to vote than those who did not refinance. This treatment effect is 43% larger, at 2.51 percentage points, for those borrowers who saved 200 basis points or more. In column (2), we show, even conditional on income, an increasing relationship between dollars saved and participation likelihood.[15]

This result pushes back against the notion that unobservable differences between those who refinanced and those who did not explains the main result. That is, if it were simply the case that our main results are being driven by differences between borrowers who ever refinance and those who do not, perhaps because those who refinance are savvier or have deeper ties to their communities, then we would *not* expect to see the effect on voting vary by the dollars saved. But we do. An

[13] Note that later in the paper, we use variation in when people *originated* their loan to get variation in their *potential* savings. Here, we fix time of origination and use variation in *when* they refinanced (or did not refinance) to get variation in *realized* savings.

[14] We choose round number cutoffs for ease of exposition, but the results are robust to other cutoffs.

[15] Our conclusions are unchanged if we further limit our sample to purchase borrowers and add the CLTV bin to the fixed effect. We show these results in Table B2.

alternative story thus has to say, for example, that borrowers who timed their refinancing to save 225 basis points instead of 150 basis points are especially savvy and thus especially more likely to vote in 2012. Few theories of voting posit such a refined participation strategy.

A determined skeptic can argue that since rates were mostly falling between 2009 and 2012, the results of our first analysis reflect the *salience* or recency of the savings rather than the *amount* of the savings. That is, it might be that voters who refinanced more recently have their lower monthly payment front and center in their mind. And it is this sort of recency effect that makes them more likely to vote, and not actual dollars saved.

The ideal way to address this would be to control for both the time the initial mortgage was originated and the time that the mortgage was refinanced. But, if we control for both of these times, then we have no variation left in the rate drop itself. Instead, we leverage an econometrically useful feature of the 2010-2011 period: Rates on 30-year fixed-rate mortgages were *not* uniformly falling. Indeed, during one spell, prevailing rates increased nearly a full percentage point in just three months starting at the end of 2010. These large swings mean that the precise savings each household got when they refinanced was somewhat outside of their control, shutting down an endogenous timing channel. And since rates were not uniformly decreasing, we can horse-race the dollars saved mechanism with a salience story by comparing people who saved more money further in time with those who saved less money more recently.

FIGURE 1 HERE

To do so, we estimate a model that allows the refinancing treatment effect to vary monthly by adjusting our main model slightly. Instead of comparing those who did not refinance to those who did, we compare those who did not refinance to those who refinanced in the first month of 2009, to those who refinanced in the second month of 2009, and so on. We then plot these monthly effects. At the same time, we plot the prevailing rate each month. We present our findings in Figure 1 and document a striking negative correlation between the prevailing rate in a given month and the effect of refinancing in that month on 2012 participation. That is, in periods when rates were relatively high, and the benefits from refinancing relatively low, the treatment effect of refinancing on future participation was also low.

3.4 Robustness Tests

3.4.1 Controlling for Borrower Financial Characteristics

A household's equity position in the years following purchase reflects two factors: their initial down payment (the combined loan-to-value ratio at origination) and the evolution of local house prices. One potential concern is that our results are not driven by refinancing, but rather by differences in housing wealth. Households with lower current LTVs, those who made larger down payments or benefited from rising home values, may be systematically more likely to both refinance and vote.

To address this concern, we focus on the subset of borrowers who purchased their homes during the 2005–2008 period. For this group we observe precise combined loan-to-value (CLTV) ratios at origination, since we know both the purchase price and the exact amounts of all loans used. It is crucial to use combined loan-to-value ratios here since second or "piggyback" loans were common during this time period. Indeed, 20.5% of our sample of purchasers used more than one loan to finance the purchase of their home. By focusing on purchase loans, and excluding those who are in the sample because they refinanced a mortgage during the 2005-2008 period, we avoid the well-known appraisal biases that can contaminate LTV measures (Agarwal et al., 2015; Kruger and Maturana, 2020).

TABLE 4 HERE

In Column (1) of Table 4, we first show that the refinancing effect in this purchase sample is virtually identical to our main result. Column (2) then introduces four bins of CLTV at origination, motivated by industry conventions: (i) <80%, (ii) exactly 80%, (iii) 80–95%, and (iv) 95–120%. Consistent with the idea that smaller down payments capture financial constraints, higher CLTV borrowers are modestly less likely to vote. Importantly, however, the estimated effect of refinancing on turnout remains unchanged.

Next, in Column (3), we allow the effect of CLTV to vary locally by including fixed effects that interact county, party, and quarter of origination with CLTV bins (the same four bins as defined in column (2)). This specification forces comparisons to be made among borrowers who share not only location, timing, and party affiliation, but also equity position at origination. Even under this demanding specification, the refinancing effect remains steady at roughly 2.5 percentage points.

Finally, Column (4) goes further by incorporating party-by-ZIP code-by-origination quarter-by-

CLTV bin fixed effects. This specification absorbs both the initial down payment decision *and* subsequent local house price dynamics, shutting down the primary channels through which households may differ in their current equity positions. Even here, we continue to estimate nearly the same refinancing effect. Taken together, these results demonstrate that our main finding is not explained by differences in borrowers' down payment behaviors, local house price dynamics, or expected current equity positions.

In Table B3, we further control for income at the time of origination among the subset of our sample we can match to HMDA. Unsurprisingly, we find that high income borrowers are much more likely to vote than low income ones. The main takeaway from this table, however, is that adding a control for income to our baseline model (in column (2)) and adding a control for income to our model with a CLTV Bin fixed effect (in column (4)) does not change the main conclusion that refinancing in between 2009 and 2012 is associated with significantly higher likelihoods of voting in 2012.

3.4.2 Register or Not

A natural limitation of our main tests is that they use a sample of registered voters who exhibit high turnout rates and might not be representative of the broader population. Of course, homeowners are an important bloc in the American electorate, and understanding how their political behavior changes following rate refinancing is important. But the question remains if the extensive margin of political participation – deciding to register at all – is affected by refinancing. To measure this dimension of civic engagement, we re-run our analysis on the mutually exclusive complement of our main sample: borrowers who were *not* registered in 2008, and we study whether they appeared on the voter rolls by 2012.

TABLE 5 HERE

One feature of this exercise is worth highlighting. Registration occurs with a very low likelihood of about five percent in our sample. In other words, of those borrowers who were not registered to vote in 2008, 95% were still not registered to vote in 2012. Non-registration can occur for a variety of very different reasons – lack of interest, high perceived costs, out-of-state registration (as is the case among investor or second-home owners), or ineligibility (e.g., non-citizenship) – that our data cannot fully disentangle. Nevertheless, Table 5 shows a clear and precisely estimated association between refinancing and subsequent registration. We show in columns (1) and (2) that among the 2005–2008 borrower sample, refinancers are 1.66 to 2.19 percentage points more likely to register by 2012 relative to comparable non-refinancers. Relative to a 5 percent base rate, this is a roughly 30–40 percent increase in the probability of entering the voter rolls. In columns (3) through (5), we restrict to purchase borrowers for whom we can measure CLTV and again find that refinancing increases registration rates.

These estimates document effects at the extensive margin, addressing the concern that high turnout among registered homeowners might mask meaningful changes in overall political engagement. That is, refinancing is associated with higher participation both by increasing the participation of registrants *and* by bringing new voters onto the rolls. While registration decisions may reflect additional mechanisms—such as residential stability or administrative updating—the presence of a positive association on the extensive margin reinforces the broader conclusion that refinancing is linked to increased political engagement, not merely higher turnout among habitual voters. In short, the positive relationship between refinancing and political participation that we document above is not an artifact of conditioning on a high-turnout registered sample: it also appears on the margin of becoming a voter in the first place.

4 Evidence from Quasi-Experimental Variation in Refinancing

A central concern in our setting is that households who refinance are likely to differ systematically from those who do not along unobservable dimensions that also predict voter participation. These include factors such as education, cognitive ability, financial sophistication, or broader engagement with civic and political institutions. Even after conditioning on a rich set of observable borrower, loan, and neighborhood characteristics, our baseline estimates may therefore remain biased. To address this concern, we employ three different instrumental variables strategies that exploit plausibly exogenous variation in borrowers' refinancing incentives generated by mortgage market conditions and policy-driven eligibility rules.

4.1 Variation in Refinancing Incentives: The Rate-Gap Instrument

We look first to variation in refinancing incentives generated by movements in mortgage rates. Specifically, we use a rate-gap–based instrument that captures differences in borrowers' exposure to subsequent interest rate declines arising from the timing of mortgage origination.

4.1.1 Explaining the Instrument

Our strategy follows a growing literature that exploits the "rate gap" – the difference between a borrower's locked-in mortgage rate and prevailing market rates at later dates (Andersen et al., 2020; Fonseca and Liu, 2024). When market rates fall below the origination rate, borrowers with larger negative rate gaps face stronger financial incentives to refinance. This insight underpins the standard refinancing "gap rule" in household finance that borrowers refinance when the interest-rate savings exceed the fixed costs of refinancing (Agarwal et al., 2013; Keys et al., 2016; Stanton, 1995).

In our data we do not observe each borrower's exact contractual rate at origination, and therefore cannot measure individual-level rate gaps directly. Instead, we use the prevailing average market rate in the origination month to proxy for the initial contract rate. This approach parallels Fonseca and Liu (2024), who instrument household-specific rate gaps with origination-month aggregates, showing that such month-to-month variation is powerful and well-balanced on observables.

We apply this approach to the refinancing–turnout context, using the average market rate in the origination month as an instrument for post-period refinancing. The first stage requires that, conditional on Party-by-County-by-Origination Qtr-by-CLTV Bin fixed effects and a rich set of controls, households who originated in high-rate months face stronger incentives to refinance once rates decline. For this to generate a valid instrument, two conditions must hold: (i) month-to-month fluctuations in origination rates reflect aggregate credit market conditions outside the borrower's control, and (ii) the exact month within a quarter when a borrower closes is effectively random. Under these assumptions, this rate gap instrument provides us with a compelling source of quasi-experimental variation in refinancing activity.

4.1.2 Results using the Rate-Gap Instrument

In column (1) of Table 6, we reproduce our benchmark OLS estimate of the relationship between refinancing and voter turnout among the sample of purchase-loan borrowers, originally reported in column (3) of Table 4.

TABLE 6 HERE

Column (2) presents the first stage, showing that the rate at origination strongly predicts subsequent refinancing: a one percentage point higher origination rate increases the probability of refinancing in the post period by about 9.4 percentage points. The first-stage F-statistic exceeds 800, confirming the relevance of the instrument. Column (3) reports the 2SLS estimate, which uses the origination rate as an instrument for refinancing. The estimated effect of refinancing on voter turnout is a positive 3.8 percentage points, an estimate broadly consistent with our baseline OLS estimate. The similarity between the OLS and IV estimates suggests that selection into refinancing based on unobserved sophistication or political engagement is unlikely to explain our findings after conditioning on the various fixed effects and individual-level controls.

Column (4) reports the reduced form, which shows that higher origination rates — which shift refinancing incentives exogenously through subsequent rate gaps — are directly associated with higher turnout in 2012. Because the reduced form does not condition on actual refinancing, this result is informative about endogeneity: the effect of origination rates on turnout is evident even before considering individual refinancing decisions. In other words, households randomly exposed to higher initial rates subsequently exhibit higher participation, consistent with a causal refinancing channel rather than differential take-up by more sophisticated borrowers.

As a robustness check, we repeat the analysis using the broader sample that includes borrowers who had already refinanced prior to 2008. The results, reported in Table B4, remain positive and significant, with the 2SLS estimate if anything larger than in our main purchase-loan sample. The results of this robustness test suggests that our findings are not an artifact of restricting to the sample with a clean measure of CLTV. Furthermore, as in Fonseca and Liu (2024) who document that rate gaps are orthogonal to prior refinancing activity, these results which use the sample of prior-refinancers suggest that our instrument is not simply capturing differences in borrower sophistication. Instead, the results point to a robust causal effect of refinancing on voter turnout.

4.2 Monetary Policy and Refinancing: Evidence from QE1

We next exploit variation in refinancing incentives generated by the Federal Reserve's first large-scale asset purchase program (QE1). By sharply lowering mortgage rates over a short horizon, QE1 induced a surge in refinancing activity, particularly among borrowers with conforming loans (Di Maggio et al., 2020). This policy episode provides an additional source of plausibly exogenous variation in refinancing incentives that is distinct from the rate-gap variation used above.

4.2.1 Explaining the Instrument

Announced in late 2008 and implemented primarily between early 2009 and early 2010, QE1 involved large-scale purchases of agency mortgage-backed securities and agency debt. These purchases led to a rapid decline in mortgage interest rates and a substantial increase in refinancing, with effects concentrated among borrowers whose loans were eligible for conforming refinancing (Di Maggio et al., 2020).

We exploit this policy-induced variation by instrumenting refinancing with a borrower's exposure to QE1-driven refinancing incentives, as determined by whether the original mortgage balance fell below the conforming loan limit at origination. Because conforming loan limits are set by national policy and fixed prior to the implementation of QE1, eligibility for the refinancing opportunities created by QE1 is predetermined with respect to subsequent political participation.

The identifying assumption underlying this strategy is that, conditional on a rich set of borrower, loan, and geographic controls and fixed effects, whether a borrower's balance at origination falls below the conforming loan limit affects voter turnout only through its effect on refinancing incentives during QE1. In particular, borrowers on either side of the cutoff are otherwise similar but faced different exposure to the refinancing opportunities created by the policy.

4.2.2 Results using QE1-Induced Variation

To conduct the analysis, we first build a relevant sample of borrowers. We start with the sample of purchase mortgages from the main sample described in Table 1 and then restrict attention to those with no second lien, whose mortgage is conventional, and whose loan-to-value ratio at origination is below 80.5%. The instrumental variable is a dummy equal to one if the borrower's mortgage balance at origination was under the conforming loan limit, i.e., less than or equal to $417,000. Because mortgage size is a strong predictor of refinancing incentives, all specifications control for log mortgage amount. As a result, this research design compares borrowers across two segments of the mortgage market, conforming and non-conforming loans, rather than exploiting a sharp regression discontinuity at the conforming loan limit.

TABLE 7 HERE

Column (1) of Table 7 presents the baseline OLS relationship between refinancing in between 2009 and 2012 and voter turnout in the 2012 general election among this sample. Column (2) reports the first-stage relationship between QE1 exposure and refinancing. Borrowers with mortgage balances below the conforming loan limit at origination were substantially more likely to refinance during the QE1 period, with conforming-loan eligibility increasing the probability of refinancing by 23 percentage points. The corresponding first-stage F-statistic indicates that the instrument is highly relevant. Column (3) reports the two-stage least squares estimate, which uses conforming-loan eligibility as an instrument for refinancing during the QE1 period.

The estimated effect of refinancing on voter turnout is 3.76 percentage points and is statistically significant. Note that a concern with this research design is self selection into the conforming limit. However, by using the loan balance at origination, we use the pre-determined variation in whether a loan is conforming, and thus, "exposed" to QE 1. We also show that these results are robust to using different "jumbo" bands to further exlcude the possibility that the very liquid homeowners self-select into the conforming limit, and are also more likely to vote. In any event, given the stability of the treatment effect, this self-selection concerns may be small.

Column (4) reports the reduced-form relationship between conforming-loan eligibility and voter turnout. Borrowers who were eligible for QE1-induced refinancing opportunities exhibit higher turnout in the 2012 election, even without conditioning on realized refinancing behavior. This result indicates that predetermined exposure to QE1-driven refinancing incentives translated directly into higher political participation.

Taken together, the QE1 results provide independent quasi-experimental evidence that refinancing increases voter turnout. While conforming-loan eligibility captures a broader contrast across segments of the mortgage market, the similarity in sign and magnitude to our previous estimates suggests that the relationship is not driven by instrument-specific features. We also acknowledge that the endogenous variable indicates whether a borrower refinanced at any point between 2009 and 2012, which aggregates refinancing responses over the post-crisis period rather than isolating refinancing that occured precisely during the QE1 window. We therefore interpret the QE1 estimates as capturing the cumulative effect of policy-induced refinancing opportunities during this window.

That said, while the variation in the timing of a household's home purchase–the rate gap instrument–and loan size–QE 1 conforming instrument–are both pre-determined we cannot completely exclude the possibility that some unobserved factor might drive both the timing of these choices and subsequent refinancing and voting behavior. For example, even when potential savings are large, refinancing requires navigating institutional frictions, meeting underwriting requirements, and actively engaging with financial intermediaries. As a result, variation in refinancing incentives may still differentially affect borrowers with greater financial sophistication or access to credit, leaving open the possibility that the IV estimates partly reflect heterogeneity in borrowers' ability to act on available opportunities rather than the causal effect of refinancing itself. In the next section, we develop a fundamentally different identification strategy using a borrower's Home Affordable Refinance Program (HARP) eligibility.

4.3 Quasi-Random Refinancing Access: Evidence from HARP

This sub-section uses the quasi-random access to refinancing generated by eligibility for the Home Affordable Refinance Program (HARP). Unlike the previous strategies, which operate through changes in incentives, HARP directly relaxed institutional constraints on refinancing for a targeted group of borrowers who were otherwise unable to refinance despite substantial potential gains. This feature makes HARP particularly well suited for isolating the causal effect of refinancing and for understanding how policy design shapes household action and downstream political behavior.

4.3.1 The Home Affordable Refinance Program

The Home Affordable Refinance Program (HARP) was introduced by the Obama Administration in March 2009 with the explicit goal of enabling highly leveraged homeowners to refinance into lower-rate, fixed-term mortgages (Abel and Fuster (2021), Ehrlich and Perry (2015), and Karamon et al. (2017)). By allowing borrowers with loan-to-value ratios above conventional underwriting limits to refinance without new equity or mortgage insurance, HARP substantially reduced refinancing frictions for a population that had been effectively locked out of the mortgage market following the housing bust.

A central institutional feature of HARP was that eligibility was restricted to loans that had been securitized by Fannie Mae or Freddie Mac on or before May 31, 2009. This cutoff plays a crucial role in our identification strategy. While borrowers choose when to originate their mortgages, the timing of when a loan is sold to a government-sponsored enterprise (GSE) is determined by lenders and securitization pipelines and is largely outside the borrower's control. For loans originated in the months preceding the cutoff, assignment to HARP eligibility was therefore plausibly quasi-random from the borrower's perspective.

We exploit this feature by using HARP eligibility as an instrument for refinancing between 2009 and 2012. Under this strategy, identification comes from comparing otherwise similar borrowers who differed in their access to refinancing solely due to the timing of GSE securitization. Because HARP eligibility directly altered refinancing access rather than refinancing incentives, this approach allows us to isolate the causal effect of refinancing without relying on variation in borrowers' responsiveness to financial incentives.

4.3.2 Empirical Strategy and Sample Construction

Our empirical strategy closely follows the approach in Abel and Fuster (2021). We focus specifically on fixed-rate purchase mortgages originated during the first six months of 2009. Given the importance of accurate measurement of borrowers' primary loan's loan-to-value ratios (LTV) for our identification strategy, we restrict our analysis to purchase mortgages, for which the LTV on the first-lien loan at origination is precisely defined. This ensures clarity about borrowers' initial equity positions and thus their likely eligibility for refinancing through HARP. Specifically, we restrict our sample to borrowers whose first-lien loans with initial loan-to-value (LTV) ratios of at least 75%.[16] This sample restriction leaves us with those borrowers most likely targeted by HARP: Homeowners with significant leverage at origination who subsequently faced challenges refinancing through conventional means. To isolate economically meaningful rate refinancing activity, we further exclude households

[16]We also drop borrowers whose primary loans had LTVs over 105% which are, as best we can tell, mostly data entry errors.

whose refinanced loan amounts deviated substantially (more than \$5,000 above or \$100,000 below) from their original mortgage. We then match these mortgages to voter registration records (L2 voter data), restricting attention to borrowers who were registered and eligible to vote in both the 2008 and 2012 elections and who remained in their homes until at least the 2012 election.[17]

A crucial aspect of our identification strategy exploits the quasi-random assignment of HARP eligibility based on the timing of loan securitization. To operationalize this, we merge anonymized CoreLogic loan-level data with investor-type information from ICE/McDash, identifying whether each mortgage was held by a government-sponsored enterprise (GSE) at two critical dates: May 31, 2009 – the HARP eligibility cutoff – and December 31, 2009. We restrict our final analytic sample to mortgages confirmed as GSE-held as of Dec 31, 2009, ensuring that HARP eligibility is determined solely by timing rather than unobservable loan or borrower characteristics. And our instrumental variable (IV) for refinancing is a binary indicator equal to one for loans already held by a GSE on or before May 31, 2009. Our final dataset comprises 56,197 borrowers who meet these criteria. This sample includes the same borrower and mortgage information as reported in our main analyses (Table 1), but now supplemented by an additional variable crucial for our HARP analysis: an indicator of HARP eligibility.[18]

The validity of our empirical strategy rests on the assumption that the timing of loan sales to government-sponsored enterprises (GSEs), and thus HARP eligibility, was effectively random with respect to borrower characteristics and voter participation. As discussed previously, this assumption is supported by prior literature, which shows no systematic bunching of loan originations or securitizations around the May 31, 2009 eligibility cutoff, nor evidence of strategic behavior by borrowers anticipating the program. Nonetheless, we explicitly test this assumption by examining balance on observable borrower characteristics.

Specifically, we conduct a series of balance tests by regressing key borrower and loan attributes on a dummy variable for HARP eligibility, using the following specification:

$$Y_i = \Psi \times \text{HARP Eligible}_i + \text{Controls}_i \times \Phi + \text{Party-by-County}_{p,c} \tag{2}$$

[17] Since voters often keep the same registration number if they move locally, it is possible to observe participation decisions at prior and future addresses. Our HARP sample, which consists of 2009 purchasers with non-missing 2008 participation is therefore a sample of local movers.

[18] As before, we visualize the creation of this dataset in Appendix A.

where Y_i is an attribute or characteristic of interest. Table B5 reports these balance results. Most importantly, column (1) shows that the likelihood of participation in the 2008 election is completely unrelated to HARP eligibility. Given the strong persistence of voting behavior, this absence of pre-treatment differences in voting behavior is particularly reassuring. We also find no meaningful differences across eligible and ineligible borrowers in demographics, income, loan size, or property characteristics. Taken together, these results support the interpretation of HARP eligibility as plausibly exogenous with respect to political engagement.

4.3.3 Results using the HARP Eligibility Instrument

Table 8 presents the results exploiting quasi-random variation in refinancing access generated by HARP eligibility.

TABLE 8 HERE

In this smaller subsample, the OLS results (column (1)) continue to suggest that refinancing is positively associated with voting in the 2012 election. And consistent with the existing literature on HARP eligibility and refinancing, the first-stage estimates in column (2) reveal a strong positive effect of HARP eligibility on refinancing. HARP eligibility increases refinancing probability by roughly 13 percentage points, and the first-stage relationship is highly statistically significant (F-statistic = 203.42), indicating a robust instrument. The IV estimate, presented in column (3), directly shows that refinancing through HARP eligibility had an economically and statistically large effect on political participation. The IV estimate is roughly four times larger than the corresponding OLS estimate in column (1), and suggests that HARP refinancing increased participation by 4.4 percentage points among those that complied with this offer of refinancing.

The larger IV estimate in column (3) relative to the OLS result in column (1) is economically intuitive. OLS measures the average correlation across all borrowers who refinanced, whereas the IV estimate in column 3 identifies the local average treatment effect specifically among "compliers" – borrowers in our sample whose refinancing decisions depended critically on HARP eligibility. These borrowers were the most liquidity constrained, and thus the substantial positive IV estimate is consistent with the resource theory of political participation, suggesting that the causal impact of refinancing on voting is strongest among borrowers for whom refinancing most meaningfully relaxed financial constraints.

The reduced form, or intent-to-treat, estimate in column (4) further corroborates our approach. Specifically, we find that voters whose mortgages were HARP eligible were about 0.6 percentage points more likely to vote than otherwise similar voters whose mortgages just missed the HARP eligibility cutoff. Because HARP eligibility affects voting only through its impact on refinancing access, this intent-to-treat effect provides particularly compelling evidence of a causal relationship.

The magnitude of the HARP estimates highlights an important role for policy design. HARP targeted borrowers who stood to benefit substantially from refinancing but were unable to act due to institutional constraints. By relaxing these constraints, the program generated larger political responses than broader, less targeted interventions such as QE1. This pattern is consistent with prior work showing that highly salient, individualized, and clearly attributable policies generate stronger political feedback than more diffuse interventions (Baicker and Finkelstein, 2019; Campbell, 2011; Margalit, 2019; Mettler, 2011). Furthermore, lower mortgage costs are both material and individualized, and HARP was a widely publicized government initiative. This may explain why our results point to HARP having elicited stronger turnout responses than more diffuse interventions like quantitative easing.

5 Heterogeneity in the Response by Party Affiliation

We next examine heterogeneity by baseline political engagement, proxied by party affiliation. Voters registered with a major political party are substantially more likely to vote consistently across elections, while unaffiliated voters are more likely to be infrequent or "sometimes" voters whose participation decisions are less certain. As a result, mortgage policy may interact differently with participation decisions across these groups, even when the underlying financial shock is similar.

TABLE 9 HERE

Columns (1) and (2) of Table 9 estimate our baseline specification separately for voters affiliated with either major political party and for voters unaffiliated with either party. Refinancing between 2009 and 2012 is associated with a statistically significant increase in turnout among affiliated voters, but the estimated effect is substantially smaller than that observed among unaffiliated voters. This pattern holds across all three instrumental-variable strategies, indicating that the differential response by party affiliation is not driven by a particular research design.

From a finance perspective, this heterogeneity helps clarify how mortgage policy feeds into political participation. The response to refinancing is concentrated among households with lower baseline political engagement, rather than among those with strong partisan attachments. This suggests that the political effects of mortgage interventions depend not only on their impact on household balance sheets, but also on whether households are near the participation margin and responsive to changes in economic conditions.

6 Discussion

Using four empirical strategies that rest on distinct identifying assumptions—regressions with rich controls and granular fixed effects, a rate-gap instrument, QE1, and the HARP policy experiment—we find consistent evidence that households who refinance are significantly more likely to vote. These results contribute to a long-standing debate on whether and how household financial conditions shape political participation. Rather than isolate a single mechanism, we finish the paper by providing some context for how mortgage policy translates into political behavior.

Turnout reflects a combination of resources, habits, mobilization, psychological factors, and institutional context (Smets and Van Ham, 2013). Refinancing plausibly interacts with several of these channels simultaneously. By lowering monthly mortgage payments, freeing up disposable income, and reducing financial strain, refinancing can ease economic and cognitive constraints on participation. At the same time, refinancing may increase residential stability, strengthen local ties, and affect households' perceptions of government and institutions. Because refinancing is economically salient and affects multiple dimensions of household life, we do not attempt to isolate a single mechanism. Instead, we interpret our results as evidence that mortgage policy affects political participation through a bundled set of financial, social, and psychological channels.

Several features of the results are consistent with this interpretation. The positive relationship between refinancing and turnout, and its increase with the size of realized savings, aligns with perspectives emphasizing that improved financial conditions can lower barriers to participation by freeing time, attention, and emotional resources (Brady et al., 1995; Schlozman et al., 2012; Verba et al., 1995). Relief from financial stress may also enhance political efficacy or social connection, factors linked to civic engagement (Hirvonen et al., 2024; Schaub, 2021). In addition, salient financial gains that are at least partially attributable to government action may foster institutional trust or a sense of political inclusion (Baicker and Finkelstein, 2019; Bruch et al., 2010; Campbell, 2011; Mettler, 2011). Our data do not allow these channels to be separated cleanly, and we do not attempt to do so.

At the same time, our findings help rule out several alternative interpretations. A longstanding debate asks whether household finances meaningfully affect political participation at all (Rosenstone, 1982). Some theories predict that improved financial conditions could reduce participation by dulling political urgency or lowering perceived stakes (Brunner et al., 2011), while others emphasize ideology, habit, or civic duty in ways that leave little role for material conditions (Gerber et al., 2003; Plutzer, 2002; Wolfinger and Rosenstone, 1980). Empirical evidence has been mixed, with studies finding positive, negative, or null effects of economic shocks on turnout (Margalit, 2019; Smets and Van Ham, 2013). Our results show that mortgage refinancing neither disengages households nor leaves participation unchanged. Instead, refinancing increases turnout, particularly among households least bound by partisan habit and most likely to be marginal participants.

In this sense, the paper's central contribution is not to isolate a single mechanism, but to establish that easing refinancing constraints causally increases political participation, and to show where in the electorate these effects arise. Refinancing is best viewed as a bundled intervention that affects households along multiple dimensions—financial, psychological, and social (Agarwal et al., 2023; Andersen et al., 2020; Campbell, 2006; Di Maggio et al., 2017; Ganong and Noel, 2020; Keys et al., 2016). By expanding the set of households willing or able to participate, mortgage policy can shape not only aggregate turnout, but also the composition of the electorate. The larger effects among unaffiliated voters are consistent with refinancing primarily mobilizing individuals near the participation margin, whose decisions are most sensitive to economic and psychological constraints (Arceneaux and Nickerson, 2009; Enos et al., 2014).

7 Conclusion

This paper studies whether mortgage refinancing during the Great Recession affected political participation. Using matched mortgage and voter records and four complementary empirical strategies including rich controls, variation in refinancing incentives from interest rate movements, QE1 exposure, and quasi-random access to refinancing through HARP, we find consistent evidence that households who refinanced between 2009 and 2012 were more likely to vote in the 2012 general election. The estimated effects are economically meaningful and robust across specifications. They are larger when refinancing generated greater payment reductions and are particularly pronounced among unaffiliated voters, who are more likely to be marginal participants. These patterns suggest that refinancing influences participation primarily among households whose voting decisions are most responsive to changes in economic conditions.

Taken together, the results indicate that policies affecting household balance sheets can have downstream political effects. Mortgage relief programs and monetary interventions aimed at lowering borrowing costs did not only shape household finances, but also altered political participation by increasing turnout and, on the extensive margin, voter registration. While the analysis does not isolate a single mechanism, the findings are consistent with refinancing operating through a combination of reduced financial strain, increased disposable resources, and changes in households' engagement with institutions. More broadly, the results highlight a channel through which housing and credit policy can influence democratic participation, underscoring the close connections between household finance, public policy, and the composition of the electorate.

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Figure 1: Treatment Effects vs Prevailing Rates

To create this figure, we estimate our main model as in Table 2 except instead of comparing those who refinanced to those who did not, we compare those who did not refinance to those who refinanced in the first month of 2009, to those who refinanced in the second month of 2009, and so on up to those who refinanced in the tenth month of 2012. The omitted group is those who did not refinance. We plot the effect of refinancing each month relative to not refinancing with a solid line. We also plot the prevailing rate each month with a dashed line. Data Sources: L2, CoreLogic, Freddie Mac, and FHFA.

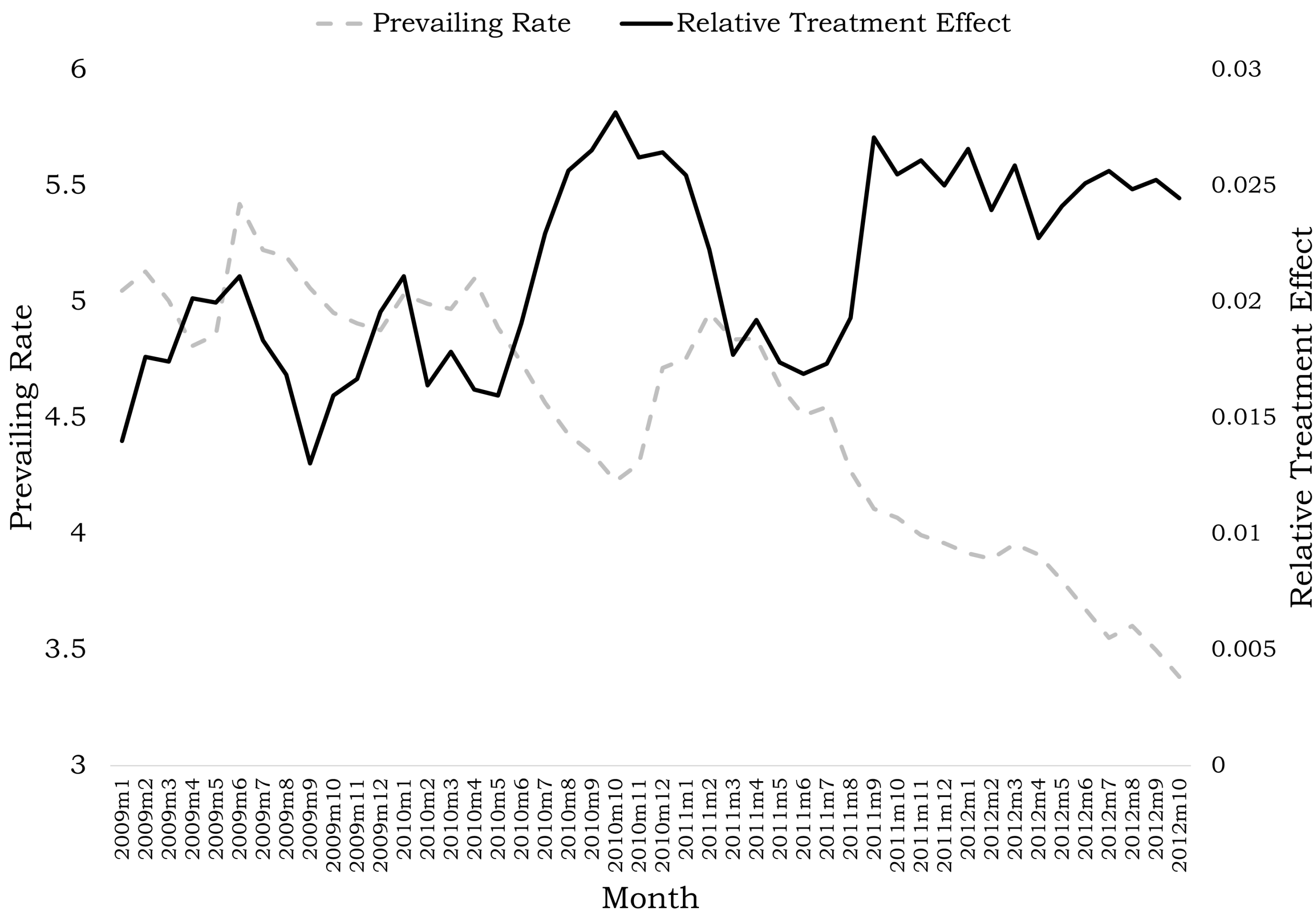


Table 1: Summary Statistics

	Mean	25th Pctile	75th Pctile	Count
Participation Rates				
2012 General Election (=1)	0.88	1.00	1.00	10,829,823
2008 General Election (=1)	0.89	1.00	1.00	10,829,823
Refinancing Activity				
Refinanced in 2009-2012 (=1)	0.45	0.00	1.00	10,829,823
Rate Drop (Ppts)	1.61	1.17	2.02	4,792,979
Voter Demographics				
Male (=1)	0.47	0.00	1.00	10,829,823
Birth Year	1961	1952	1970	10,829,823
Registration Year	1997	1992	2004	10,829,823
Democrat (=1)	0.36	0.00	1.00	10,829,823
Republican (=1)	0.37	0.00	1.00	10,829,823
Unaffiliated (=1)	0.27	0.00	1.00	10,829,823
Income at Origination ($s)	$94,387	$54,000	$111,000	6,484,015
Property Characteristics				
Year Built	1975	1959	1997	10,829,823
Square Feet	1,985	1,375	2,387	10,829,823
2006 to 2010 Change in Local House Prices	-16.50%	-27.26%	-3.02%	10,057,458
Outstanding Mortgage Characteristics				
Refi (=1)	0.64	0.00	1.00	10,829,823
Mortgage Amount ($s)	$208,293	$114,500	$265,500	10,829,823
Combined LTV Ratio	0.84	0.75	0.99	3,780,606
Conventional (=1)	0.89	1.00	1.00	10,829,823
Under Conforming Loan Limit (=1)	0.95	1.00	1.00	10,829,823
Originated in 2005 (=1)	0.24	0.00	0.00	10,829,823
Originated in 2006 (=1)	0.22	0.00	0.00	10,829,823
Originated in 2007 (=1)	0.26	0.00	1.00	10,829,823
Originated in 2008 (=1)	0.28	0.00	1.00	10,829,823

Notes: This table describes our main sample which includes all individuals registered to vote in the 2012 election who (i) were also registered to vote in 2008, (ii) originated mortgages between 2005 and 2008, and (iii) had not refinanced those mortgages at any time before January 1, 2009. The sample and its construction is described in detail in Section 2.2. Participation data come from L2. Refinancing activity comes from CoreLogic. Rate drop is measured using data from Freddie Mac Data and is only defined for borrowers who refinanced. Sex, birth year, registration year, and party affiliation come from L2. Income at origination comes from HMDA. Property characteristics, loan purpose, loan type, loan amount, and year of origination are from CoreLogic. Combined LTV Ratio divides the sum of all loan amounts, up to a maximum of three loans, used to purchase a property by its purchase price. Author's calculations using data from FHFA defines loans under their county's conforming loan limit. Data Sources: L2, CoreLogic, Freddie Mac, HMDA, and FHFA.

Table 2: The Effect of Refinancing on Voting

Dependent Variable:	Voted in the 2012 General Election (=1)			
	(1)	(2)	(3)	(4)
Refinanced in the Post Period	0.0385***	0.0284***	0.0320***	0.0236***
	(0.000518)	(0.000369)	(0.000423)	(0.000322)
Prior Participation				
Voted in the 2008 General Election		0.373***		0.366***
		(0.00310)		(0.00306)
Controls			X	X
Fixed Effects				
Party × County × Origination Qtr	X	X	X	X
Counts				
N	10,829,823	10,829,823	10,829,823	10,829,823
Sample Means				
Voted in the 2012 General Election	0.88	0.88	0.88	0.88
Refinanced in the Post Period	0.45	0.45	0.45	0.45

Notes: This table estimates the effects of refinancing between 2009 and 2012 on voting in the 2012 general election. The sample is described in detail in Table 1. Controls include a dummy for if the voter is male, four birth year bins, four registration year bins, four year built bins, four bins for building square footage, a dummy for whether the outstanding mortgage is a purchase or refinance, log mortgage origination amount, a dummy for if the outstanding mortgage is a conventional loan, and a dummy for if the outstanding mortgage was under the county's conforming loan limit at the time of origination. Data Sources: L2, CoreLogic, and FHFA. Standard errors, adjusted for clustering at the county-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 3: Heterogeneity with Respect to Voter Savings

Dependent Variable:	Voted in the 2012 General Election (=1)	
Sample:	Refinanced 0 or 1 times Between 2009 and 2012	
Subsample:	Full	Non-Missing Income
	(1)	(2)
Rate Drop		
Did Not Refinance (omitted)		
Rate Drop: 99 bpts or less	0.0175***	0.0175***
	(0.000547)	(0.000681)
Rate Drop: 100 bpts - 199 bpts	0.0214***	0.0209***
	(0.000347)	(0.000424)
Rate Drop: 200 bpts or more	0.0251***	0.0245***
	(0.000418)	(0.000489)
Income		
Income: \$0 - \$49,999 (omitted)		
Income: \$50,000 - \$74,999		0.0135***
		(0.000453)
Income: \$75,000 - \$124,999		0.0226***
		(0.000498)
Income: \$125,000 - \$2,000,000		0.0274***
		(0.000674)
Controls	X	X
Fixed Effects		
Party × County × Origination Qtr	X	X
Counts		
N	9,870,324	5,877,618

Notes: This table estimates the effects of refinancing between 2009 and 2012 on voting in the 2012 general election. Different from our other analyses, refinance is not a dummy variable, but rather a categorical variable with four categories: did not refinance, refinanced and saved 99 basis points or less, refinanced and saved between 100 and 199 basis points, or refinanced and saved between 200 basis points or more. The sample is described in detail in Table 1 with the added restriction that borrowers who refinanced two times or more and omitted. Controls include a dummy for participation in 2008, a dummy for if the voter is male, four birth year bins, four registration year bins, four year built bins, four bins for building square footage, a dummy for whether the outstanding mortgage is a purchase or refinance, log mortgage origination amount, a dummy for if the outstanding mortgage is a conventional loan, and a dummy for if the outstanding mortgage was under the county's conforming loan limit at the time of origination. Data Sources: L2, CoreLogic, Freddie Mac, HMDA, and FHFA. Standard errors, adjusted for clustering at the county-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 4: Using Purchase Loans to Control for Combined Loan-to-Value Ratios

Dependent Variable:	Voted in the 2012 General Election (=1)			
	(1)	(2)	(3)	(4)
Refinanced in the Post Period	0.0252***	0.0249***	0.0251***	0.0236***
	(0.000454)	(0.000455)	(0.000464)	(0.000595)
Combined Loan-to-Value Ratio				
0% < CLTV ≤ 79.5% (omitted)				
79.5% < CLTV ≤ 80.5%		-0.00426***		
		(0.000643)		
80.5% < CLTV ≤ 94.5%		-0.00161**		
		(0.000744)		
94.5% < CLTV ≤ 120.0%		-0.00471***		
		(0.000741)		
Controls	X	X	X	X
Fixed Effects				
Party × County × Origination Qtr	X	X		
Party × County × Origination Qtr × CLTV Bin			X	
Party × ZIP Code × Origination Qtr × CLTV Bin				X
Counts				
N	3,780,606	3,780,606	3,780,606	3,495,797

Notes: This table estimates the effects of refinancing between 2009 and 2012 on voting in the 2012 general election by party while controlling for borrowers' LTVs. The sample is the purchase-loan subset of that described in Table 1. Controls include a dummy for participation in 2008, a dummy for if the voter is male, four birth year bins, four registration year bins, four year built bins, four bins for building square footage, a dummy for whether the outstanding mortgage is a purchase or refinance, log mortgage origination amount, a dummy for if the outstanding mortgage is a conventional loan, and a dummy for if the outstanding mortgage was under the county's conforming loan limit at the time of origination. Data Sources: L2, CoreLogic, and FHFA. Standard errors, adjusted for clustering at the county-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 5: The Effect of Refinancing on Registering

Dependent Variable:	Registered for the 2012 General Election (=1)				
Sample:	2005-2008 Borrowers Not Registered for the 2008 General Election				
Subsample:	All		Purchase Mortgage Borrowers		
	(1)	(2)	(3)	(4)	(5)
Refinanced in the Post Period	0.0219***	0.0166***	0.0300***	0.0218***	0.0229***
	(0.000447)	(0.000338)	(0.000531)	(0.000411)	(0.000414)
Controls		X		X	X
Fixed Effects					
County × Origination Qtr	X	X	X	X	
County × Origination Qtr × CLTV Bin					X
Counts					
N	14,819,569	14,819,569	6,147,161	6,147,161	6,147,161
Sample Means					
Registered for the 2012 Election	0.05	0.05	0.06	0.06	0.06
Refinanced in the Post Period	0.29	0.29	0.31	0.31	0.31

Notes: This table estimates the effects of refinancing between 2009 and 2012 on registering for the 2012 general election. The sample includes those borrowers who originated fixed-rate mortgages between 2005 and 2008 and who were not registered to vote for the 2008 general election. Controls include four year built bins, four bins for building square footage, a dummy for whether the outstanding mortgage is a purchase or refinance, log mortgage origination amount, a dummy for if the outstanding mortgage is a conventional loan, and a dummy for if the outstanding mortgage was under the county's conforming loan limit at the time of origination. Data Sources: L2, CoreLogic, and FHFA. Standard errors, adjusted for clustering at the county-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 6: Rate Gap Instrument

Dependent Variable:	Voted in 2012	Refi'd in Post Period	Voted in 2012	Voted in 2012
	OLS	First Stage	2SLS	Reduced-form
	(1)	(2)	(3)	(4)
Refinanced in the Post Period	0.0251***		0.0383***	
	(0.000464)		(0.0128)	
Rate at Origination		0.0941***		0.00361***
		(0.00325)		(0.00121)
Controls	X	X	X	X
Fixed Effects				
Party × County × Origination Qtr × CLTV Bin	X	X	X	X
First Stage F-Stat			841.40	
Counts				
N	3,780,606	3,780,606	3,780,606	3,780,606

Notes: This table estimates the effects of refinancing between 2009 and 2012 on voting in the 2012 general election using prevailing mortgage rates at the time the borrower originated their 2005-2008 mortgage, our proxy for the borrowers' "rate gap", as an instrumental variable. The sample is the purchase-loan subset of that described in Table 1. Column (1) presents the estimates from an OLS regression that estimates our main model, Equation 1, on this sample. Column (2) presents the results of the first stage regression of refinancing between 2009 and 2012 on the mortgage rate that was prevailing at the time of origination. Column (3) presents the IV regression estimate of voting in the 2012 general election on refinancing between 2009 and 2012 where prevailing rate at origination is the IV. Column (4) presents the estimates from the reduced form regression of voting in the 2012 general election on prevailing rate at origination. Controls include a dummy for participation in 2008, a dummy for if the voter is male, four birth year bins, four registration year bins, four year built bins, four bins for building square footage, log mortgage origination amount, and log sale amount. Data sources: CoreLogic, L2, and FHFA. Standard errors, adjusted for clustering at the county-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 7: QE1 Instrument

Dependent Variable:	Voted in 2012	Refi'd in 2009Q1 - 2010Q1	Voted in 2012	Voted in 2012
	OLS	First Stage	2SLS	Reduced-form
	(1)	(2)	(3)	(4)
Refinanced in the Post Period	0.0262***		0.0376***	
	(0.000654)		(0.00611)	
Conforming Loan Amount		0.232***		0.00873***
		(0.00602)		(0.00145)
Controls	X	X	X	X
Fixed Effects				
Party × County × Origination Qtr × CLTV Bin	X	X	X	X
First Stage F-Stat			1488.97	
Counts				
N	1,601,250	1,601,250	1,601,250	1,601,250
Sample Means				
Dependent Variable	0.89	0.57	0.89	0.89

Notes: This table reports estimates of the relationship between refinancing during the QE1 period (2009Q1–2010Q1) and voting in the 2012 general election. The sample consists of purchase mortgages from the main sample described in Table 1, plus the following restrictions. We use just borrowers (i) with no second lien and whose mortgage (ii) is conventional and (iii) has a loan-to-value ratios at origination below 80.5%. The instrumental variable is a dummy equal to one if the borrower's mortgage balance at origination was under the conforming loan limit, i.e., less than or equal to $417,000. Column (1) reports OLS estimates of voting in the 2012 general election on an indicator for refinancing during 2009Q1–2010Q1. Column (2) reports the first-stage regression of refinancing during 2009Q1–2010Q1 on the conforming-loan indicator. Column (3) reports the two-stage least squares estimate of voting in the 2012 general election on refinancing during 2009Q1–2010Q1, instrumented by the conforming-loan indicator. Column (4) reports the reduced-form regression of voting in the 2012 general election on the conforming-loan indicator. Controls include a dummy for participation in 2008, a dummy for if the voter is male, four birth year bins, four registration year bins, four year built bins, four bins for building square footage, and log mortgage origination amount. Data sources: CoreLogic and L2. Standard errors, adjusted for clustering at the county-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 8: HARP Results

Dependent Variable:	Voted in 2012	Refi'd in Post Period	Voted in 2012	Voted in 2012
	OLS	First Stage	2SLS	Reduced-form
	(1)	(2)	(3)	(4)
Refinanced in the Post Period	0.009***		0.044**	
	(0.003)		(0.020)	
HARP Eligible		0.130***		0.006**
		(0.010)		(0.003)
Controls	X	X	X	X
Fixed Effects				
Party × County × Origination Qtr	X	X	X	X
First Stage F-Stat			170.57	
Counts				
N	56,197	56,197	56,197	56,197
Sample Means				
Dependent Variable	0.90	0.32	0.90	0.90

Notes: This table estimates the effects of refinancing between 2009 and 2012 on voting in the 2012 general election using HARP eligibility as an instrumental variable. The sample consists of loans originated between January 2009 and June 2009 that were held by a GSE by the end of 2009 and is described in detail in Section 4.3.2. HARP eligibility equals 1 if a loan was held by a GSE by the end of May 2009, and 0 otherwise. Column (1) presents the estimates from an OLS regression that estimates our main model, Equation 1, on this sample. Column (2) presents the results of the first stage regression of refinancing between 2009 and 2012 on HARP eligibility. Column (3) presents the IV regression estimate of voting in the 2012 general election on refinancing between 2009 and 2012 where HARP eligibility is the IV. Column (4) presents the estimates from the reduced form regression of voting in the 2012 general election on HARP eligibility. Controls include a dummy for participation in 2008, a dummy for if the voter is male, four birth year bins, four registration year bins, four year built bins, four bins for building square footage, four bins for income, log mortgage origination amount, and log sale amount. Data sources: CoreLogic, L2, HMDA, and ICE, McDash. Standard errors, adjusted for clustering at the state-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 9: Heterogeneity with Respect to Political Affiliation, IV Approaches

Dependent Variable:	Voted in the 2012 General Election (=1)							
Instrumental Variable:	Benchmark OLS		Rate Gap Instrument		QE1 Instrument		HARP Instrument	
Party Affiliation:	Affiliated	Unaffiliated	Affiliated	Unaffiliated	Affiliated	Unaffiliated	Affiliated	Unaffiliated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Refinanced in the Post Period	0.0211***	0.0320***	0.0226	0.0708***	0.0304***	0.0609***	0.015	0.102**
	(0.000346)	(0.000637)	(0.0138)	(0.0245)	(0.00646)	(0.0121)	(0.024)	(0.044)
Controls	X	X	X	X	X	X	X	X
Fixed Effects								
County × Origination Qtr	X	X					X	X
County × Origination Qtr × CLTV Bin			X	X	X	X		
First Stage F-Stat			712.77	547.60	1338.56	1055.65	169.30	85.96
N	7,899,984	2,930,621	2,591,139	1,189,467	1,124,675	476,575	38,711	17,486

Notes: This table presents results analogous to those in column (4) of Table 2, column (3) of Table 6, column (3) of Table 7, and column (3) of Table 8. Columns (1), (3), (5), and (7) estimate the effect of refinancing on voting among the sample of voters affiliated with either the Democratic or Republican party; and columns (2), (4), (6), and (8) among the sample of voters unaffiliated with either party. Data sources: CoreLogic, L2, and FHFA. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Household Finance and Voter Turnout: Evidence from Mortgage Refinancing

Haoyang Liu, W. Ben McCartney, Rodney Ramcharan, Calvin Zhang, and Xiaohan Zhang

Online Appendix

A Visualizing the Sample Creation Process

In this data appendix we illustrate and describe in greater detail how our two main samples are created. Complete data cleaning and sample creation code as allowed by the contracts with our data vendors will also be supplied to interest readers.

A.1 Full Sample

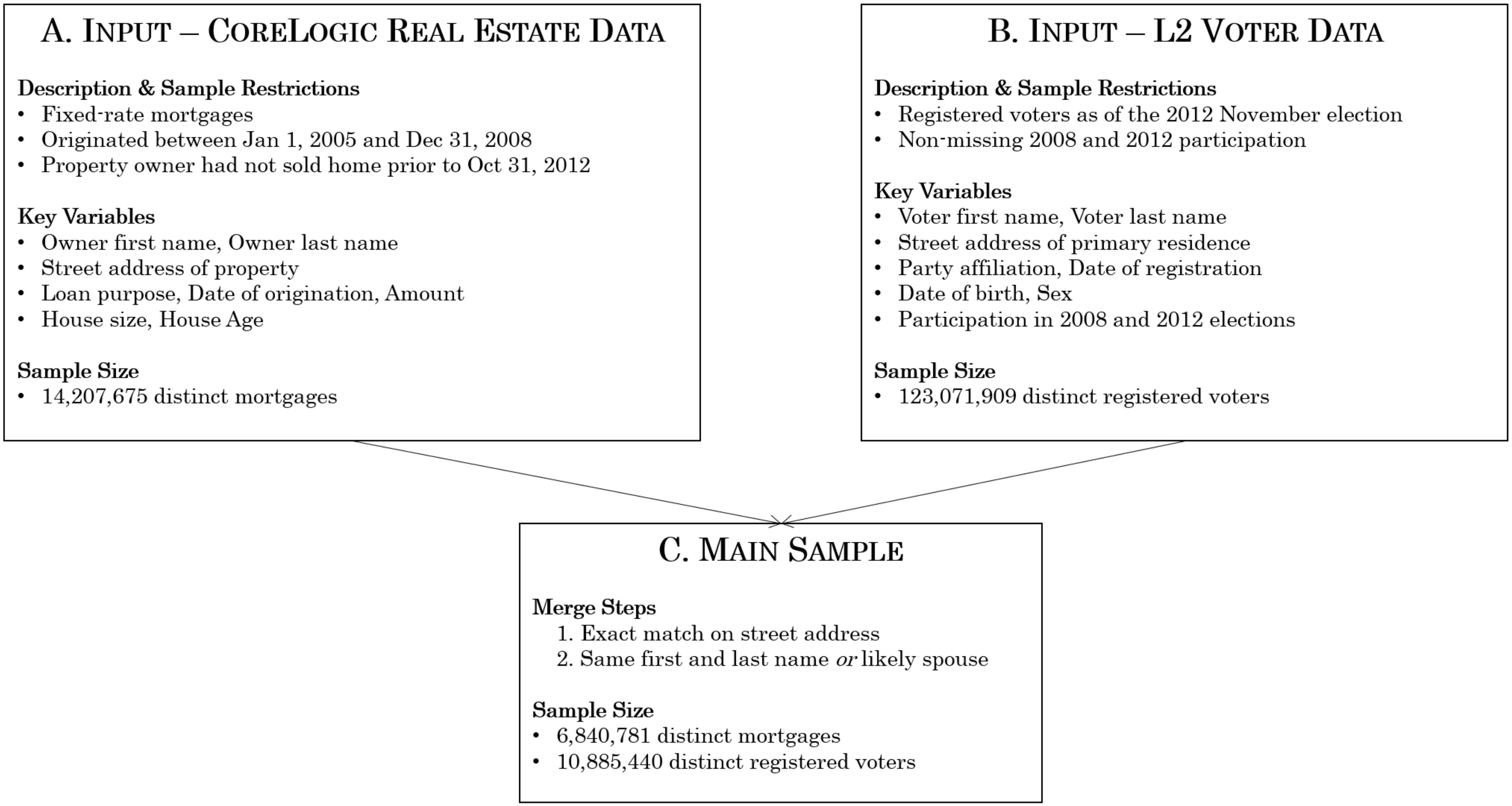
In Figure A1, we visually represent how the full sample is constructed. We use two main data sources. The first, shown in Panel A, is the deeds and mortgage data from CoreLogic. Our goal with this data is to create a sample of all mortgages outstanding as of January 1, 2009 that were "at risk" of being refinanced. To that end, we start with the sample of all fixed-rate mortgages originated between 2005 and 2008. Among that set, we then keep each household's most recently originated one. That is, if a household took out a purchase mortgage in 2005 and then refinanced that mortgage in 2007, we keep only the refinance in our sample. We then restrict this sample to just those mortgages tied to properties that were not sold before the 2012 election. This yields a sample of more than 14 million mortgages. Our data set thus focuses on "never-movers," at least over the time series of interest – 2005-2008 mortgage origination to 2012 general election. Focusing on this group allows for the cleanest measure of treatment status and estimate of said treatment.

Panel B describes our voter data. We use the snapshot of registered voters as of early 2014. This ensures that 2012 participation, which sometimes takes states a long time to record in the data, is fully documented in the voter files. Since voter files remain fairly stale except between major elections when voters go to update their registrations, using early 2014 snapshots does a good job describing the population that was registered to vote in the 2012 election. Our key sample restriction here is to drop voters whose 2008 participation decision we cannot observe. Since we eventually merge this data with the sample of homeowners who have lived in their homes since at least 2008, this effectively drops just those voters who did not register until after the 2008 election. The final sample of voters includes more than 123 million voters.

Panel C presents the match rates. We begin with the sample of voters and then ask if somebody with their same name and same address exists in our sample of mortgages. We allow some flexibility here to match potential spouses. The final data set that we use for our full sample analysis includes just under 11 million voters attached to just under 7 million unique mortgages.

Figure A1: Full Sample Creation Schema

This schema describes overviews the sample creation procedure used to build the full sample, discussed in the main text in Section 2.2.



A.2 HARP Sample

In Figure A2, we visually represent how the HARP sample is constructed. Panels A, B, and C are very similar to Panels A, B, and C in Figure A1. The exceptions come in the sample restrictions for the mortgages. We closely follow Abel and Fuster (2021) in our construction of mortgages eigible for the HARP experiment. Specifically, we limit to mortgages originated in the first half of 2009. Since the HARP policy was designed to help highly levered and even underwater borrowers refinance, we limit our sample to borrowers with high LTVs. We choose to use the LTV at the time of purchase since measuring current LTVs is notoriously difficult, especially during this period of rapidly falling house prices. In some sense, whether a borrower had a "true" current LTV of 93% or 98% or 107% is not knowable; three different appraisers could well come up with three different current home values. We therefore opt to include everybody who took out an economically meaningful loan, which we define as having an LTV at purchase, a time when both the loan and the house has a very well-defined value, of at least 75%.

The other major requirement to be in the HARP experiment is that the loan *could have* been purchased by a GSE. Since we cannot know this using just information available in the deeds data, we need to merge in information from ICE/McDash. In order to be in compliance with our contracts and protect privacy, the bridge between CoreLogic and McDash was made behind a firewall, independently of this project. We are then given a file with three variables: the CoreLogic loan ID, whether the loan was owned by a GSE as of December 31, 2009, and whether the loan was owned by a GSE as of May 31, 2009. Of course, we know that loans owned by a GSE as of Dec 31, 2009 *could have* been purchased by a GSE since we see that they were. When we then merge in these variables to the CoreLogic-L2 matched sample we are left with 163,000 distinct mmortgages that match to ICE/McDash.

We then make the conservative approach to limit our sample to just these loans. This likely omits some loans that could have been purchased by a GSE and for whatever reason were not, but it has the key advantage of ensuring that, within our final sample, the loans differ only in when they were purchased by a GSE. This final sample includes 43,500 mortgages that were owned by a GSE as of the end of 2009. On these mortgages are just over 60,000 distinct voters.

Our final sample is much smaller than the 219,407 in Abel and Fuster (2021). There are three main reasons for this difference. First, LTV is a crucial variable in this context, our decision to focus on purchase loans drop many mortgages from the sample. 27.6%, or approximately 60,500 of the loans in Abel and Fuster (2021), are purchase mortgages. Second, to be included in our sample requires, as before, that the loan in CoreLogic match to at least one voter in the L2 data. And finally, privacy considerations mean we cannot use Equifax CRISM data. Instead, we use a bridge between CoreLogic and ICE/McDash. The coarsness of the matching variables means that many loans in our main dataset fail to match to ICE, McDash (of the 294,168 mortgages that meet the first set of sample requirements, 162,964 match to McDash).

Figure A2: HARP Sample Creation Schema

This schema describes overviews the sample creation procedure used to build the HARP sample, discussed in the main text in Section 4.3.2.

A. Input – CoreLogic Real Estate Data

Description & Sample Restrictions
- Fixed-rate purchase mortgages
- Originated between Jan 1, 2009 and Jun 30, 2009
- Property owner had not sold home prior to Oct 31, 2012
- LTV at Purchase >= 75% and <= 105%
- Refi amount no more than $5,000 above or $100,000 below

Key Variables
- Owner first name, Owner last name
- Street address of property
- Date of purchase/origination
- Purchase Price, Loan Amount, LTV
- House size, House Age

Sample Size
- 708,142 distinct mortgages

B. Input – L2 Voter Data

Description & Sample Restrictions
- Registered voters as of the 2012 November election
- Non-missing 2008 and 2012 participation

Key Variables
- Voter first name, Voter last name
- Street address of primary residence
- Party affiliation, Date of registration
- Date of birth, Sex
- Participation in 2008 and 2012 elections

Sample Size
- 123,071,909 distinct registered voters

C. Merged CL Owners and L2 Voters

Merge Steps
1. Exact match on street address
2. Same first and last name *or* likely spouse

Sample Size
- 294,168 distinct mortgages
- 420,093 distinct registered voters

D. Input – CoreLogic Real Estate Data

Description
- Fixed-rate purchase mortgages

Key Variables
- CoreLogic ID
- Origination Date
- ZIP Code of Parcel
- Loan Amount, Loan Term

E. Input – ICE/McDash Data

Description
- Fixed-rate purchase mortgages

Key Variables
- Origination date
- ZIP Code of parcel
- Loan Amount, Loan Term
- Ownership as of May 31 and Dec 31, 2009

F. CL - McDash Bridge (Built Behind a Firewall)

Merge Steps
1. Unique matches on origination date, ZIP code, loan amount, loan term

Key Variables
- CoreLogic ID
- Dummy = 1 if GSE Owned as of May 31, 2009 (HARP Eligible)
- Dummy = 1 if GSE Owned as of Dec 31, 2009 (Sample Requirement)

G. HARP Sample

Merge Steps
1. Merge using CoreLogic ID
    - 162,964 distinct mortgages
    - 231,192 distinct registered voters

Key Sample Restriction
- GSE Owned as of Dec 31, 2009 = 1 results in
    - 43,461 distinct mortgages
    - 64,212 distinct registered voters

B Supplemental Figures and Tables

Table B1: The Effect of Refinancing on Voting

Dependent Variable:	Voted in the 2012 General Election (=1)				
	(1)	(2)	(3)	(4)	(5)
Refinanced in the Post Period	0.0385***	0.0284***	0.0320***	0.0236***	0.0221***
	(0.000518)	(0.000369)	(0.000423)	(0.000322)	(0.000317)
Prior Participation					
Voted in the 2008 General Election		0.373***		0.366***	0.362***
		(0.00310)		(0.00306)	(0.00316)
Demographics					
Male			-0.0149***	-0.00709***	-0.00681***
			(0.000290)	(0.000222)	(0.000224)
Birth Year: 1942 or Earlier (omitted)					
Birth Year: 1943 - 1958			0.00644***	0.00866***	0.0101***
			(0.00129)	(0.00122)	(0.00122)
Birth Year: 1959 - 1974			-0.0161***	-0.0101***	-0.00792***
			(0.00163)	(0.00151)	(0.00149)
Birth Year: 1974 - 1990			-0.0376***	-0.0248***	-0.0217***
			(0.00184)	(0.00165)	(0.00165)
Registration Year: 1983 or Earlier (omitted)					
Registration Year: 1984 - 1991			-0.0146***	-0.00917***	-0.00879***
			(0.000786)	(0.000664)	(0.000646)
Registration Year: 1992 - 1999			-0.0362***	-0.0228***	-0.0227***
			(0.000953)	(0.000710)	(0.000696)
Registration Year: 2000 - 2008			-0.0646***	-0.0420***	-0.0418***
			(0.00109)	(0.000783)	(0.000763)
Property Characteristics					
Year Built: 1959 or Earlier (omitted)					
Year Built: 1960 - 1979			-0.00137**	-0.000443	0.00173***
			(0.000625)	(0.000528)	(0.000447)
Year Built: 1980 - 1999			0.0000592	0.000300	0.00424***
			(0.000714)	(0.000609)	(0.000459)
Year Built: 2000 or Later			0.00103	0.00202***	0.00721***
			(0.000830)	(0.000701)	(0.000534)
Square Feet: 1249 or Less (omitted)					
Square Feet: 1250 - 1999			0.0220***	0.0147***	0.0134***
			(0.000551)	(0.000428)	(0.000452)
Square Feet: 2000 - 2999			0.0361***	0.0241***	0.0215***
			(0.000774)	(0.000573)	(0.000613)
Square Feet: 3000 or more			0.0384***	0.0266***	0.0237***
			(0.00107)	(0.000792)	(0.000820)

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	(1)	(2)	(3)	(4)	(5)
Outstanding Mortgage Characteristics					
Refi			-0.0224***	-0.0233***	-0.0220***
			(0.000685)	(0.000525)	(0.000497)
Log Mortgage Amount			0.0166***	0.0112***	0.00851***
			(0.000502)	(0.000350)	(0.000318)
Conventional			0.0197***	0.0134***	0.0114***
			(0.000760)	(0.000617)	(0.000571)
Under Conforming Loan Limit			0.00136	0.000263	0.00183***
			(0.000900)	(0.000676)	(0.000643)
Fixed Effects					
Party × County × Origination Qtr	X	X	X	X	
Party × ZIP Code × Origination Qtr					X
Counts					
N	10,829,823	10,829,823	10,829,823	10,829,823	10,775,930
Sample Means					
Voted in the 2012 General Election	0.88	0.88	0.88	0.88	0.88
Refinanced in the Post Period	0.45	0.45	0.45	0.45	0.45

Notes: This table estimates the effects of refinancing between 2009 and 2012 on voting in the 2012 general election. The sample is described in detail in Table 1. Data Sources: L2, CoreLogic, and FHFA. Standard errors, adjusted for clustering at the state-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table B2: Heterogeneity with Respect to Voter Savings, Controlling for CLTV

Dependent Variable:	Voted in the 2012 General Election (=1)	
Sample:	Refinanced 0 or 1 times Between 2009 and 2012	
Subsample:	Full	Non-Missing Income
	(1)	(2)
Rate Drop		
Did Not Refinance (omitted)		
Rate Drop: 99 bpts or less	0.0196***	0.0190***
	(0.000955)	(0.00114)
Rate Drop: 100 bpts - 199 bpts	0.0227***	0.0217***
	(0.000532)	(0.000634)
Rate Drop: 200 bpts or more	0.0272***	0.0261***
	(0.000643)	(0.000738)
Income		
Income: \$0 - \$49,999 (omitted)		
Income: \$50,000 - \$74,999		0.0111***
		(0.000789)
Income: \$75,000 - \$124,999		0.0191***
		(0.000865)
Income: \$125,000 - \$2,000,000		0.0210***
		(0.00110)
Controls	X	X
Fixed Effects		
Party × County × Origination Qtr × CLTV Bin	X	X
Counts		
N	3,400,922	2,315,618

Notes: This table estimates the effects of refinancing between 2009 and 2012 on voting in the 2012 general election. Different from our other analyses, refinance is not a dummy variable, but rather a categorical variable with four categories: did not refinance, refinanced and saved 99 basis points or less, refinanced and saved between 100 and 199 basis points, or refinanced and saved between 200 basis points or more. The sample is described in detail in Table 1 with the added restriction that borrowers who refinanced two times or more and omitted. Controls include a dummy for participation in 2008, a dummy for if the voter is male, four birth year bins, four registration year bins, four year built bins, four bins for building square footage, a dummy for whether the outstanding mortgage is a purchase or refinance, log mortgage origination amount, a dummy for if the outstanding mortgage is a conventional loan, and a dummy for if the outstanding mortgage was under the county's conforming loan limit at the time of origination. Data Sources: L2, CoreLogic, Freddie Mac, HMDA, and FHFA. Standard errors, adjusted for clustering at the state-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table B3: Heterogeneity with Respect to Voter Income

Dependent Variable:	Voted in the 2012 General Election (=1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Main Effect of Refinancing						
Refinanced in the Post Period	0.0238***	0.0230***	0.0222***	0.0248***	0.0242***	0.0227***
	(0.000374)	(0.000371)	(0.000384)	(0.000541)	(0.000540)	(0.000767)
Main Effect of Income						
Income: $0 - $49,999 (omitted)						
Income: $50,000 - $74,999		0.0133***	0.0129***		0.0110***	0.00955***
		(0.000438)	(0.000456)		(0.000757)	(0.00102)
Income: $75,000 - $124,999		0.0225***	0.0213***		0.0191***	0.0165***
		(0.000487)	(0.000504)		(0.000832)	(0.00113)
Income: $125,000 - $2,000,000		0.0274***	0.0247***		0.0210***	0.0171***
		(0.000657)	(0.000669)		(0.00105)	(0.00141)
Controls	X	X	X	X	X	X
Fixed Effects						
Party × County × Origination Qtr	X	X				
Party × ZIP Code × Origination Qtr			X			
Party × County × Origination Qtr × CLTV Bin				X	X	
Party × ZIP Code × Origination Qtr × CLTV Bin						X
Counts						
N	6,481,245	6,481,245	6,424,240	2,594,130	2,594,130	2,340,267

Notes: This table estimates the effects of refinancing between 2009 and 2012 on voting in the 2012 general election. The sample is described in detail in Table 1. Controls include a dummy for participation in 2008, a dummy for if the voter is male, four birth year bins, four registration year bins, four year built bins, four bins for building square footage, a dummy for whether the outstanding mortgage is a purchase or refinance, log mortgage origination amount, a dummy for if the outstanding mortgage is a conventional loan, and a dummy for if the outstanding mortgage was under the county's conforming loan limit at the time of origination. Data Sources: L2, CoreLogic, HMDA, and FHFA. Standard errors, adjusted for clustering at the county-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table B4: Rate Gap Instrument, Including Refinancers

Dependent Variable:	Voted in 2012	Refi'd in Post Period	Voted in 2012	Voted in 2012
	OLS	First Stage	2SLS	Reduced-form
	(1)	(2)	(3)	(4)
Refinanced in the Post Period	0.0247***		0.0659***	
	(0.000331)		(0.0119)	
Rate at Origination		0.0585***		0.00385***
		(0.00222)		(0.000680)
Controls	X	X	X	X
Fixed Effects				
Party × County × Origination Qtr	X	X	X	X
First Stage F-Stat			696.29	
Counts				
N	10,829,823	10,829,823	10,829,823	10,829,823

Notes: This table estimates the effects of refinancing between 2009 and 2012 on voting in the 2012 general election using prevailing mortgage rates at the time the borrower originated their 2005-2008 mortgage, our proxy for the borrowers' "rate gap", as an instrumental variable. The sample is described in Table 1. Column (1) presents the estimates from an OLS regression that estimates our main model, Equation 1, on this sample. Column (2) presents the results of the first stage regression of refinancing between 2009 and 2012 on the mortgage rate that was prevailing at the time of origination. Column (3) presents the IV regression estimate of voting in the 2012 general election on refinancing between 2009 and 2012 where prevailing rate at origination is the IV. Column (4) presents the estimates from the reduced form regression of voting in the 2012 general election on prevailing rate at origination. Controls include a dummy for participation in 2008, a dummy for if the voter is male, four birth year bins, four registration year bins, four year built bins, four bins for building square footage, four bins for income, log mortgage origination amount, and log sale amount. Data sources: CoreLogic, L2, and FHFA. Standard errors, adjusted for clustering at the county-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table B5: HARP Balance Test

Dependent Variable:	Voted in 2008	Log Sale Amount	Log Loan Amount	Male	Age	Years Registered	Income	Square Footage	Year Built
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
HARP Eligible	-0.004	0.001	0.000	0.001	0.162	0.006	548.234	-0.311	-0.618**
	(0.004)	(0.001)	(0.001)	(0.003)	(0.146)	(0.099)	(1456.699)	(9.746)	(0.299)
Controls	X	X	X	X	X	X	X	X	X
Fixed Effects									
Party × County × Origination Qtr	X	X	X	X	X	X	X	X	X
Counts									
N	56,197	56,197	56,197	56,197	56,197	56,197	44,960	56,197	56,197
Sample Means									
Dependent Variable	0.85	12.49	12.30	0.50	40.35	8.58	$105,688	2201.68	1980

Notes: This table estimates the effects of being HARP eligible on various borrower and loan characteristics. The sample consists of loans originated between January 2009 and June 2009 that were held by a GSE by the end of 2009 and is described in detail in Section 4.3.2. HARP eligibility equals 1 if a loan was held by a GSE by the end of May 2009, and 0 otherwise. Controls include a dummy for participation in 2008, a dummy for if the voter is male, four birth year bins, four registration year bins, four year built bins, four bins for building square footage, four bins for income, log mortgage origination amount, and log sale amount. When the variable is used as the dependent variable, it is not included as a control and we use the raw amount, not a binned version of it. Data sources: CoreLogic, L2, HMDA, and ICE, McDash. Standard errors, adjusted for clustering at the state-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.