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ABSTRACT

Voters punish incumbent Presidential candidates for contractions in the local (county-level) supply of mortgage credit during market-wide contractions of credit, but they do not reward them for expansions in mortgage credit supply in boom times. Our primary focus is the Presidential election of 2008, which followed an unprecedented swing from very generous mortgage underwriting standards to a severe contraction of mortgage credit. Voters responded to the credit crunch by shifting their support away from the Republican Presidential candidate in 2008. That shift was particularly pronounced in states that typically vote Republican, and in swing states. The magnitude of the effect is large. If the supply of mortgage credit had not contracted from 2004 to 2008, McCain would have received half the votes needed in nine crucial swing states to reverse the outcome of the election. The effect on voting in these swing states from local contractions in mortgage credit supply was five times as important as the increase in the unemployment rate; if unemployment had not increased from 2004 to 2008, that improvement in local labor markets would only have given McCain only 9% of the votes needed to win the nine crucial swing states. We extend our analysis to the Presidential elections from 1996 to 2012 and find that voters' reactions are similar for Democratic and Republican incumbent parties, but different during booms and busts of mortgage credit. These asymmetric results indicate that voters react strongly and negatively to credit supply contraction; however, organized political bargaining (the "smoke-filled room channel") rather than voting was the primary vehicle for rewarding politicians for supporting government subsidies for mortgage risk during booms.

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I. Introduction

Empirical work on the economic voter hypothesis consistently shows that the state of the economy affects voting behavior.¹ But the same work fails to pin down at the micro level the economic variables (e.g. inflation, unemployment) that matter the most for voters. Here we offer another economic variable for consideration, which is the change in the supply of credit.

There are good reasons to suspect voting and credit subsidies are related: government policies subsidizing homeownership have been a hallmark of American politics for nearly a century and have also figured prominently in various electoral campaigns across the world. That said, we also recognize that there are mechanisms other than voting that may reward politicians for supporting credit subsidies. For example, apart from voting, politicians may be rewarded by the financial or political support of well-defined and organized vested interests, which may include banks and urban activist organizations, which played a crucial role in promoting mortgage credit subsidies, especially from 1992 to 2007 (Calomiris and Haber 2014). We will refer to these two alternatives as “the smoke-filled room channel” and “voting channel,” respectively.²

While there is ample evidence of the strong relation between the state of the macro-economy and elections’ outcomes (Fair 1978, 1996, 1998, 2002; Lewis-Beck and Stegmaier 2000) there is no micro evidence of the relation between changes in credit supply and voting behavior. In this paper, we provide the first such micro evidence and find that voters do, in fact, punish incumbent Presidential candidates for experienced contractions in the supply of mortgage credit. We build on Fair’s work by connecting votes for President at the county level to county-level conditions in the mortgage market, as well as other economic variables, including unemployment. Using the Home Mortgage Disclosure

¹ For a review of the literature see Lewis-Beck and Stegmaier (2000) and references therein.

² We avoid referring to political support for mortgage credit subsidies as reflecting “populist politics” because populism has multiple meanings. Our results are more consistent with Riker’s (1982) broad definition of populism than with the narrower definition that refers to claiming to represent the interests of common people.

Act (HMDA) data on banks' provision of mortgage credit, we identify supply shifts in mortgage credit at the county level and examine how shifts in the supply of mortgage credit affected voting in the Presidential election of 2008. We focus primarily on the period 2004 to 2008, and the election results of 2008 because this four year period saw an unprecedented swing from the most generous underwriting standards for mortgages in U.S. history in 2004-2006 to a severe contraction of mortgage credit during the subprime crisis of 2007-2009. It also saw a dramatic swing in electoral results, with the Republican Presidential candidate winning many key swing states in 2004, but losing those same states in 2008.

We find that, after controlling for other relevant factors, voters responded to the contraction in credit by shifting their support away from the Republican Presidential candidate in the 2008 Presidential election (John McCain). The shift toward the Democratic Presidential candidate (Barack Obama) was particularly pronounced in swing states (those that have the least predictable support for either party). The magnitude of the effect of mortgage credit supply shifts on voting is large in 2008. Our estimates indicate that in the absence of the mortgage credit supply contraction, some important swing states – most obviously, North Carolina – would have cast their electoral votes for McCain. In other swing states, the absence of mortgage credit supply contraction by itself would not have reversed the electoral result, but nevertheless, would have substantially narrowed the gap between votes received by McCain and Obama in 2008. Overall, taking into account the effects of mortgage credit in the crucial swing states that voted for Obama, we find that if mortgage credit supply had not shifted adversely from 2004 to 2008, McCain would have received half the votes needed to capture all nine of the swing states that Bush had won in 2004 but that McCain lost in 2008, which would have reversed the outcome of the election. In that sense, the contraction in mortgage credit supply from 2004 to 2008 was five times as important as the increase in the unemployment rate; if

unemployment had not increased from 2004 to 2008, that improvement in local labor markets would only have given McCain 9% of the votes he needed in those crucial swing states.

We extend our analysis to other Presidential elections from 1996 to 2012. Consistent with the findings for 2008, we find that contractions in credit supply from 2008 to 2012 penalized the incumbent party and benefited the candidacy of Mitt Romney. In the mortgage credit boom phase, however, which was relevant for the 2000 and 2004 elections, there is no evidence that counties with relatively high credit expansion voted in favor of either the Democratic Presidential candidate in 2000 or the Republican Presidential candidate in 2004. In other words, voters did not reward Presidential candidates of the incumbent party in response to experiencing a greater than average local boom in mortgage credit supply. These results suggest that the way voters react to mortgage credit changes do not vary substantially according to the political party of the incumbent, but do vary according to whether a boom or a contraction in credit is occurring. Voters don't reward Presidential candidates for booms of credit, but they do punish them for contractions.

Our findings have important implications for research on the politics of mortgage credit. Most importantly, our findings do not lend support to the view that Presidential candidates gained direct votes from supporting the relaxation of underwriting standards for mortgage lending from 1996 to 2004. If political rewards attended that support, those rewards would have had to come from other sources (the smoke-filled room channel). The contraction of credit supply, however, had large and tangible consequences for Presidential candidates. That finding suggests at least part of the explanation for recent policies by the Obama Administration to relax underwriting standards may be a concern for electoral consequences. Indeed, our findings may help explain the recent actions by Mel Watt, the recently appointed Director of the Federal Housing Finance Agency (FHFA) to lower downpayment requirements on Fannie Mae and Freddie Mac mortgages, and to limit the insurance

premium charged by the Federal Housing Administration (FHA).³ GSE mortgage-backed securities were also exempted from the Volcker Rule’s prohibition on proprietary trading. Finally, although the Dodd-Frank Act of 2010 called for strict new standards for “qualifying mortgages,” rather than create two standards (strict and less strict), regulators opted to only create a single, less strict standard, after considerable lobbying pressure and pressure from members of Congress pushed the regulatory agencies to weaken previously proposed standards (Gordon and Rosenthal 2016).

Section II provides an overview of the related literature. Section III describes our data and empirical methodology. Section IV reports our findings. Section V concludes.

II. Literature Review

II.1 Mortgage Credit Subsidies, Banking Crises, and Politics

Government policies subsidizing homeownership have been a hallmark of American politics for nearly a century. Those policies have taken many forms, most of which operate through the subsidization of mortgage credit risk (making the amount of credit and the price of credit risk paid by the borrower lower than it would be without government subsidies). Such mortgage cost subsidization can take the form of Federal Housing Administration or Veterans Administration guarantees, mandates for subsidized mortgage purchases or guarantees from the Housing GSEs (Fannie Mae and Freddie Mac), regulatory pressures on lenders to provide subsidized lending to favored groups (through enforcement of the Community Reinvestment Act), low and risk-insensitive minimum capital ratio requirements associated with mortgage lending, and forbearance from closing down insolvent mortgage lenders (as during the U.S. thrift crisis). Calomiris and Haber (2014, Chapters 6-8) review those policies over the past century and show that all of them have been used extensively

³ See <http://www.marketwatch.com/story/freddie-mac-ceo-wants-more-low-down-payment-mortgages-2015-10-20> and <http://www.fhfa.gov/Media/PublicAffairs/Pages/Prepared-Remarks-Director-Watt-at-MBA-Annual-Convention-and-Expo-2015.aspx>

to channel subsidies to mortgage borrowers. Leading up to the subprime mortgage debacle, those subsidies resulted in a massive debasement of underwriting standards in mortgage lending and substantial undercapitalization on the part of mortgage lenders.⁴

The United States is not the only country in which the prevalence of housing subsidization, generally via mortgage subsidies, has figured prominently in electoral politics. Margaret Thatcher's popularity owed no small part to her championing of the privatization of council flats. In the U.K. today, the credit risk subsidies from the "help-to-buy" program were the major exception from the government's austerity policies, and Prime Minister Cameron has made increased housing opportunities a hallmark of his current electoral campaign. In Brazil's 2014 election, President Dilma Roussef squeaked to a narrow electoral victory, which some observers attributed to her "Minha Casa Minha Vida" home-buying program. Neither is the United States the only country that has experienced a severe banking crisis associated with subsidized housing credit. Jorda, Schularick and Taylor (2014) show that the share of mortgages on banks' balance sheets doubled during the 20th century for the 17 advanced economies that they track since 1870. Jorda, Schularick and Taylor (2015) show the credit-financed housing bubbles have become the single most important contributor to banking crises for these 17 countries. Laeven and Valencia (2012) document the unprecedented pandemic of costly banking crises during the period 1970-2010, which has seen over a hundred major banking crises throughout the world, with the negative net worth of failed banks averaging about 16 percent of GDP. Real estate collapses figure prominently in these crises, too. In the United States, as elsewhere, prospective homeowners are generally regarded as a powerful political constituency, and mortgage credit subsidies have been used as a primary means of subsidizing the acquisition of a home.

⁴ See also Mayer, Pence and Sherlund (2009), Rajan (2010), Rajan, Seru and Vig (2010), Acharya et al. (2011), Agarwal, Benmelech and Seru (2012), Fishback et al. (2014), and McCarty, Poole and Rosenthal (2013), pp. 17, 19, 44, 126-133.

There is a long theoretical and empirical literature in political economy that seeks to explain these facts, and study their variation over the business cycle and electoral cycle.⁵

With respect to the question of how subsidies are delivered to politically favored recipients, it is widely recognized that the granting of access to cheap credit can be politicians' preferred means of subsidizing favored groups, either because other more direct means of taxes and transfers are blocked by political obstacles that do not apply to banking regulations (Rajan 2010, Calomiris and Haber 2014), or because those bearing the costs of providing credit subsidies may not be able to detect those costs easily. With respect to the latter point, Coate and Morris (1995) show that, for that reason, in the presence of imperfect information by voters, inefficient methods of redistribution (like credit subsidies) may be preferred.⁶

Where government owns and directly controls major lenders, there is substantial evidence that hidden credit subsidies are used to favor particular borrowers, including both firms and individuals. With respect to favored credit to firms, Sapienza (2004) studies the behavior of state-owned banks in Italy. She finds that state-owned banks provide cheap credit to large firms and to firms residing in depressed areas, and that the size of the subsidy provided reflects the extent of the dominance of the local political party. Dinc (2005) finds that state-owned banks in emerging market countries substantially increase their lending in election years relative to private banks. Khwaja and Mian (2005) study Pakistani state-owned banks, and find that they favor politically connected firms with cheap credit, and that the size of the subsidies received reflect the degree of the political power of the recipient. Claessens, Feijen and Laeven (2008) study the political quid pro quo for firms in Brazil.

⁵ For contributions and reviews of the political business cycle literature, see Nordhaus (1975), Alesina, Roubini and Cohen (1997), Drazen (2000), and Person and Tabellini (2002).

⁶ Employment of constituents is another example of an inefficient tax and transfer method that is chosen because the implied subsidy is hard for voters to identify (Alesina, Baqir and Easterly 2000). Hidden patronage via employment can also occur within politically influential firms that expect to be rewarded for that behavior (Bertrand et al. 2007).

They find that firms that contribute to politicians experience higher stock returns than other firms if those politicians are elected. They trace that superior performance to credit subsidies that those firms receive from banks. Carvalho (2014) finds that Brazilian politicians use credit subsidies from state-controlled banks to pressure firms in politically important areas to increase their employment near elections, and presumably the politicians do so to improve their electoral outcomes. Firms that increase their employment as a quid pro quo for receiving government credit subsidies act as intermediaries who ensure that politicians are rewarded for their credit subsidies by the votes of their workers (by firing featherbedding workers if their subsidies end as the result of their patrons losing the election).

The recent U.S. experiment with government investments into banks reveals similar evidence of favoritism that took the form of financing subsidies to politically influential firms. Duchin and Sosyura (2012) find that politically connected banks were more likely to be funded by the Troubled Asset Relief Program (TARP) than other banks, *ceteris paribus*. Blau et al. (2013) find that politically connected banks also received a greater amount of TARP support, and received it faster, than other banks.

Channeling subsidized credit by politicians to powerful firms in exchange for contributions or other political favors is one thing, but credit subsidies to the masses must operate through a different mechanism, namely voting or other forms of mass support (e.g., demonstrations or rallies in support of the politicians). In the case of demonstrations, unions or other activist groups can act as intermediaries in the deal between the politicians and the masses (Calomiris and Haber 2014, Chapter 7). But anonymous programs for subsidizing mortgage credit, such as Fannie and Freddie credit subsidies, FHA subsidies, low minimum capital ratios for mortgage lending, and forbearance policies toward mortgage lenders lack any intermediaries able to credibly commit to generate votes in exchange for credit. In countries with a secret ballot, like the United States, politicians expecting to

gain votes for delivering cheap mortgage credit must rely on the loyalty of individual voters to reward them for having done so.

There is plenty of evidence that politicians in the United States and other democracies behave as if they believe that voters will reward them for delivering cheap credit. In a study of state-owned bank agricultural lending in India, Cole (2009) finds that agricultural credit increases by 5-10 percentage points in election years (resulting in a spike in post-election defaults), and that election-year increases in state bank lending are larger in districts for which the election is closely contested. In the United States, Liu and Ngo (2014) find that in states where governors are up for reelection, bank failure is 45% less likely in the year leading up to the election. This effect is twice as strong in states where the governor has control of both the upper and lower houses of the state legislature heading into the gubernatorial election. Romer and Weingast (1991) find similar evidence about political pressures in the U.S. Congress to delay thrift closures in the 1980s. They argue that Congress was the main source of delay in closing insolvent thrifts in the mid-1980s. Romer and Weingast study Congressional voting on a key 1987 piece of legislation that would have limited forbearance for insolvent thrifts. They find that contributions from thrifts to Congressional campaigns were influential on voting. They also find that Representatives from Congressional districts that were heavily populated by under-capitalized thrifts were more likely to support forbearance. They describe politicians' behavior as "fairly routine politics," reflecting politicians' concerns both about campaign contributors and about the supply of mortgage credit in their districts.

The same combination of lobbying by mortgage lenders, and concerns about voters' responses to tightening mortgage credit policies underlay Congressional behavior during the mortgage credit boom and bust of 2000 to 2009. During 2000-2007, Igan, Mishra and Tressel (2011) find that the riskiest mortgage lenders were the most active lobbyists of Congress. Mian, Sufi and Trebbi (2013) find that as the political stakes rose in the early 2000s mortgage industry firms increased their

campaign contributions to Congress sharply, and that campaign contributions had an increasingly powerful influence on Representatives' voting behavior on housing-finance legislation. Campaign contributions had a significant effect on roughly 20 percent of the mortgage-finance-related votes in 2003-2004; in contrast, only 3 percent of mortgage-finance-related votes seem to have been affected by campaign contributions in 1995-96. Mian, Sufi and Trebbi (2013) also find that the presence of subprime borrowers influenced Representatives' voting behavior, not just campaign contributions. As in the case of the thrift crisis, Representatives acted not only in response to money, but also to expand and preserve mortgage credit in response to their constituents. When the mortgage default crisis began, Mian Sufi and Trebbi (2010) find that the same combination of campaign contributions and constituents' circumstances predict the voting patterns of Representatives. Representatives whose constituents experienced a sharp increase in mortgage defaults – especially in more competitive districts, and especially if the constituents belonged to same political party as the Representative – were more likely to support the Foreclosure Prevention Act of 2008.

Presidential politics seems to have also been influenced by housing finance policy, and here, as in Congress, the support was bipartisan. As Calomiris and Haber (2014, Chapter 7) show, Presidents George H.W. Bush, Bill Clinton, and George W. Bush all were vocal and active supporters of expanding mortgage credit subsidies. George H.W. Bush signed the GSE Act of 1992 establishing mortgage purchase mandates for low-income and urban housing for Fannie Mae and Freddie Mac. President Clinton substantially expanded those mandates and weakened FHA lending standards. President George W. Bush further expanded the GSE mandates as part of his “blueprint for the American dream.” Barack Obama has also supported expanded mortgage credit. He not only enacted a mortgage relief program, but also appointed former Congressman Mel Watt in 2014 to oversee the renewed expansion of GSE credit. Calomiris and Haber (2014) argue that the bipartisan presidential support for housing credit subsidies reflected, in part, the growing importance of cities. No one

running for President can win without capturing many important swing states, such as Ohio and Florida, which are highly urban, and therefore, have been especially dependent on GSE and other mortgage credit subsidies.

The empirical literature on the political economy of mortgage credit subsidies and the actions of Presidents George H.W. Bush, Bill Clinton, George W. Bush, and Barack Obama clearly show that politicians act as if they believe that they will gain at the polls from expanding the supply of mortgage credit to their constituents. Nevertheless, to our knowledge, there is no direct evidence that constituents' voting behavior actually rewards politicians who deliver cheap mortgage credit.

II.2 Voting Behavior

It is far from obvious that voters will reliably reward politicians when they see improvements in the supply of mortgage credit. The smoke-filled room channel may be more important. Politicians may be more swayed by special interest groups, including bankers, GSEs, and urban activist groups that reward politicians' actions with contributions, influence on other matters, or public demonstrations of support. Although unions and activist groups may be able to organize their supporters to help politicians in observable ways, the secret ballot prevents credible (i.e., verifiable) contracting with politicians or their intermediaries regarding votes. Furthermore, as an individual act, voting cannot be analyzed by any simple theory purporting to explain the private gains to the voter from the anticipated outcome of the voter: there is zero probability that a voter can affect the outcome. People, therefore, must be voting to fulfill some psychic or sociological need – to feel patriotic, to feel avenged, or to gain the respect of those that observe them taking the time to fulfill their civic duty. Economists since Hotelling (1929) have assumed that voters vote for politicians with whom

they are more aligned philosophically (the one whose policy proposal is closest to their “bliss” point).⁷ Whatever the explanation for voter behavior, the goals of the voter cannot hope to achieve any objective outcome as the result of voting one way or the other.

Nonetheless, there is a vast literature in economics and political science establishing the empirical grounds for believing that voters respond to economic circumstances (e.g., Key 1966, Kramer 1971, Hibbs 1987a, 1987b, Lewis-Beck 1988, Alesina et al. 1993, Campbell and Garand 2000, Persson and Tabellini 2000), including Fair’s (1978, 1996, 1998, 2002) influential time series analysis of the roles of inflation, economic growth, and economic events on Presidential elections. There is an indisputable, strong relationship between the state of the economy and voters’ reactions to incumbents; when inflation is low, output growth is high, and no adverse economic shocks are apparent, voters tend to support incumbents, and when those indicators are opposite, voters tend to oppose incumbents.

The literature on “political psychology,” which uses experimental and other data to sort out the connection between the economic environment and voting, provides a more nuanced view of the connection between the economy and voting. First, there is some evidence that voters’ responses to economic outcomes reflect their identification with a group interest, rather than a selfish attempt to “vote their pocketbook.” The context also seems to be important. Sears (1987) reviews the early literature on the relationship between voting and economic outcomes and argues that “[s]trong self interest effects do seem to occur when the stakes are high and clear or when the threat is high and ambiguous and the political remedy is clear and certain but these prove to be rather rare circumstances in the political world of the ordinary citizen.” In other words, one would expect to find strong voting

⁷ See Kamada and Kojima (2014) for a discussion of the theoretical literature in economics on voting, and a review of some of the most important studies of voters’ utility functions. Most of the interesting theoretical questions about voters that come from these perspectives pertain to multi-dimensional voting behavior, where concavity vs. convexity of utility can have important consequences. See also Boleslavsky and Cotton (2015).

responses when the economic outcome is especially important and visible to the voter. From this perspective, mortgage credit supply change appears to be an obvious candidate for a strong voter response.

Lewis-Beck and Stegmaier's (2000) review of the voting literature concludes that "good times keep parties in office, bad times cast them out. This proposition is robust, as the voluminous body of research reviewed here demonstrates." The economic voter, according to Lewis-Beck and Stegmaier's review of the literature, symmetrically "holds the government responsible for economic performance, rewarding or punishing it at the ballot box." The authors report that voters tend to be retrospective rather than prospective in their reactions to economic matters and they report more mixed findings on the question of whether voter behavior reflects the voter's own "pocketbook," as opposed to their "sociotropic" sense of the public good.⁸ Alesina et al. (1993) espouse a somewhat opposing view, in which voters are not only prospective, but consider the overall balance of power within the government between opposing forces, and the consequences of their vote for that balance, when casting their votes.

Additional contributions gauge the circumstances under which economic issues receive great weight by the electorate compared to other issues. For example, Kayser and Wlezien (2011) find that in Western Europe the declining of partisan identification of voters has increased the relative importance of economic issues in electoral outcomes over time. In the United States, the partisan divide has intensified over recent decades, which raises the question of whether they may have been a declining importance of economic issues in U.S. elections. Lewis-Beck, Nadeau and Elias (2008)

⁸ For early contributions to that debate, see Kinder and Kiewiet (1981), and Kramer (1983).

disentangle the respective roles of partisanship and economic influences in the American context and find that economic influences on voting tend to dominate partisan ones.

An interesting finding in some of the voting literature is that the state of the economy may matter asymmetrically for affect voting behavior. In particular, there is evidence that voters punish incumbents for bad economic outcomes, but fail to reward them for good ones (see Jordan 1960; Campbell et al. 1965; Bloom and Price 1975; Pacek and Radcliff 1995; Nannestad and Paldam 1997; Quinn and Woolley 2001; Alcaniz and Hellwig 2011; and Jensen et al. 2015). As Bloom and Price (1975) state: “we believe that economy conditions play a small role in determining of voting behavior in times of prosperity. During these times, their impact is dominated by such forces as the underlying balance of party identification and other, more salient political issues.” Therefore, we may expect the effect of credit expansions on voting to be muted, whereas that of credit contractions to be more pronounced. Indeed, as we show later, we are able to confirm this in our study. Asymmetries in voting response may reflect attribution bias. When a voter gets a job or secures a mortgage, he or she may conclude that this is a consequence of his or her achievements; when a voter loses a job or is rejected for a mortgage, he or she may find it easier (in the sense of avoiding cognitive dissonance) to blame others.

The teasing out of economic influences on voting can be particularly challenging in the context of time series analysis, where endogeneity problems can produce spurious inferences, especially if voting itself affects expected economic outcomes through partisan biases in expectations. Indeed, Wlezien, Franklin and Twiggs (1997) and Ladner and Wlezien (2007) argue that, for that reason, time series studies have led to a general overstatement of the extent to which voters respond to economic outcomes. To the extent that this is true, analyses that make use of cross-sectional and panel evidence on voting are better suited to avoid spurious identification problems associated with endogeneity. Indeed, Gerber and Huber (2009) find that when using county-level data to analyze

voting patterns, partisan expectations – reflected in consumption behavior – are an important influence on economic expectations.

III. Data and Methodology

We obtain loan level data from the Home Mortgage Disclosure Act (HMDA). HMDA requires the collection of detailed data regarding applications for mortgage loans, which include information about whether the mortgage application was approved or denied, as well as information about some of the borrower's personal characteristics and the location of the property.⁹

We use the application outcomes in the millions of mortgage applications included in HMDA, to isolate a bank-specific measure of mortgage credit supply growth. More specifically, we condition on an application having been filed – to control for the demand for credit –, and trace the supply of credit by estimating an OLS model of the application decision (reject-not reject).¹⁰ We extract the bank-specific supply of credit from the (negative of the) coefficients of bank-year dummies in the model, where, to rid the estimates from the confounding effects of borrower/market risk, we also control for variation in individual borrower-specific attributes and for differences in the economic environment of counties (using county fixed effects) in which would-be mortgage borrowers are located. Data from the 2004 and 2008 HMDA files are pooled together, and all variables and fixed effects are interacted with time. We refer to this as the *first-stage regression model*.

We then use the bank-specific measures of credit supply to compute county-specific changes in mortgage-credit-supply. We do that in two steps. First, for each county and year (2004, 2008) we

⁹ Very small banks are not required to participate in the HMDA survey. Presumably, the effect of these omissions on changes in credit supply are much smaller, given that only changes in unreported small bank lending are missing from our measure of mortgage credit supply change.

¹⁰ When a large number of fixed effects needs to be estimated, OLS produces consistent estimators for the coefficients of these fixed effects, while a logisitic specification produces inconsistent estimates. For more on this issue – known as the incidental parameters problem – see Wooldridge (2002, p484) and references therein.

take a weighted average of the estimated bank-specific fixed effects over all banks that operated in a county that year. For weights, we use the number of loan applications received within each county and year to account for bank importance. Then, we compute the 2004-to-2008 difference of the two separately computed county-level measures of mortgage-credit-supply aggregates.

Because the first-stage model of mortgage rejection includes a full range of the relevant controls capturing borrower, location, and contract characteristics, it removes all demand-side variation that is associated with the changing composition of borrowers. It also likely removes some supply-side influences that are location-specific. For example, banks may target credit differentially to some locations in response to changes in location-specific risk (e.g., expected house price appreciation differences). Such supply-side influences would be captured within the time-interacted county fixed effects in our first-stage regressions. Therefore, we interpret our measure of credit-supply change (based solely on changes in bank fixed effects) as a partial, but clean, measure of mortgage-credit-supply change, recognizing that additional location-specific information (in county fixed effects) captures a mixture of supply and demand influences. In our robustness analysis of the second-stage regression conclusions, we investigate the potential importance of excluding location-specific exogenous factors from our measure of mortgage credit supply growth. As we discuss further below, we find that this does not affect our conclusions about the importance of mortgage-credit-supply change (measured using bank fixed effect changes) on electoral outcomes.¹¹ In other words, while we recognize that there may be additional location-specific factors influencing mortgage credit supply that our model does not capture, omitting those does not affect the estimated influence of the components of mortgage credit supply on election outcomes that we capture in our model. In that

¹¹ Specifically, we find that when we include the change in county-level fixed effects (estimated from our first-stage mortgage-rejection regressions) in the second stage regression, the inclusion of these fixed effects does not alter the coefficient of credit-supply change captured by the weighted change in bank fixed effects. The change in county fixed effects (estimated separately for 2004 and 2008) does not enter significantly in the second-stage regression.

sense, our conclusions should be considered conservative; others influences on mortgage-credit supply that cannot be identified cleanly may have exerted additional effects on electoral outcomes that we do not capture.¹²

We then connect the identified change in county-specific mortgage credit supply to changes in voting behavior within each county by running a *second-stage OLS* regression, where the dependent variable is defined as the change in the percentage of votes within the county in support of the Democratic Presidential candidate, the challenger, over the period 2004 to 2008. The second-stage regression includes county attributes to control for other differences in the local environment that may affect voting behavior, such as changes in personal income, the unemployment rate, and various demographic characteristics that are subject to change over time within each county.

Finally, we use the estimation results of this second-stage regression analysis to gauge the importance of mortgage-credit supply changes for the 2008 election by asking the counterfactual question of how much the shifts in mortgage-credit supply contraction mattered for election results in important swing states.

Summary statistics of data we employ from the HMDA database are presented in Table 1. We have a sample of 8.6 million applications in 2004 and 4.8 million in 2008.¹³ Despite the decline in the number of applications, mortgage rejection rates are 37% in 2008, compared to 25% in 2004. In our analysis, we control for all of these attributes of the applicant and the mortgage, as well as additional county-level attributes collected from the Bureau of Economic Analysis (BEA), Bureau of

¹² Additionally, the fact that our results are robust to including county-level variation in mortgage rejection rates as a control in the second-stage regression mitigates concerns about possible selectivity bias related to individuals' decisions about whether to apply for a mortgage. To the extent that local conditions are correlated with cross-sectional differences in that selectivity bias, including county-level controls for mortgage-credit supply reduces that potential source of selectivity bias.

¹³ From the HMDA data we drop loans purchased from other institutions or loans withdrawn (22% of all observations), non-conventional loan applications (10%), and loan applications with missing demographics (9%). We also drop the smallest banks (defined as those that process less than 1% of all loan applications processed within a state-year) because of computing constraints in estimating all the county- and bank-specific fixed effects (27%) in the first stage.

Labor Statistics (BLS), and American Consumer Survey (ACS), all of which are defined in Table 2. For example, we include personal income in our model, as reported by the BEA, which is defined as the sum of net earnings by place of residence, property income, and personal current transfer receipts. We also include measures of religious affiliations, to capture potential ideological or partisan differences. Here our source is the 2000 and 2010 *U.S. Religion Census: Religious Congregations & Membership Study* (RCMS). These are all measures that are shown in previous work to affect voting, and that are available annually at the county-level.

After analyzing the effects on voting of the growth in mortgage credit supply from 2004 to 2008 in Sections IV.1-IV.3, we repeat this exercise for three other Presidential election cycles in Section IV.4, using comparable data for 1996, 2000, and 2012, to construct mortgage-credit-supply growth measures at the county level for the periods 1996 to 2000, 2000 to 2004, and 2008 to 2012.

IV. Empirical Findings

IV.1 First-Stage Results: Election of 2008

Our first-stage OLS regressions for each year take the form:

$$(1) \quad LAR_{ijkt} = \alpha + \beta_{kt} + \gamma_{it} + \delta X_{jt} + \zeta X_{jt} \bullet \text{AFTER}_t + \eta \bullet Y_{jt} + \lambda \bullet Y_{jt} \bullet \text{AFTER}_t + e_{ijkt},$$

where *LAR* denotes *Loan Application Rejection*, which takes the value of 1 if the application is rejected, and 0 if it is accepted; *j* indexes the particular loan application; *i* indexes the particular lender considering the application; *k* indexes the county in which the applicant resides; *t* indexes the year the application is made (t=2004, 2008); β_{kt} denote county fixed effects; γ_{it} denote bank fixed effects;

vectors X and Y capture various applicant and loan type controls, respectively; $AFTER$ takes the value of 1 if the year is 2008, 0 otherwise; and e is the error term. Applicant and loan characteristics are interacted with time in order to capture potential shifts in aggregate risk aversion among loan officers. Bank and county fixed effects are also allowed to vary across time.¹⁴

Table 3 reports our first-stage regression results for predicting mortgage application denial in 2004 and 2008, with standard errors clustered by bank. As expected, we find that application rejection depends on a variety of personal attributes, as well as county and bank fixed effects (not reported here). Table 4 reports descriptive statistics for our county-specific measure of the change in mortgage credit supply ($\Delta Mortgage Credit Supply$) from 2004 to 2008, which is the aggregation of the (negative of the) weighted bank fixed effects for the banks rejecting mortgage applicants from that county.

Of interest is to examine how changes in the computed supply of credit *at the bank level* between 2004 and 2008 vary across lenders. Our sample of lenders (referred to as “banks” for convenience) includes all types of mortgage lenders, not just depository institutions. In results not reported here, we grouped lenders by type according to their primary regulator (the Office of the Comptroller of the Currency, the Federal Reserve System, the Federal Deposit Insurance Corporation, the Office of Thrift Supervision, the National Credit Union Administration, and the Department of Housing and Urban Development) and compared their $\Delta Mortgage Credit Supply$ from 2004 to 2008. Average or median values were quite similar across the seven groups. The most negative group average was negative 19% for Federal Reserve System-regulated institutions, and the most positive group average was negative 16% for those regulated by the Department of Housing and Urban Development). We also divided the lenders into two groups based on lender size. Smaller and larger lenders exhibited similar $\Delta Mortgage Credit Supply$. For lenders regulated by the Federal Reserve

¹⁴ To allow meaningful comparisons, we ensure that the omitted county and bank is always the same across the two years (04 and 08) in the specification.

System, smaller institutions had slightly more negative average values of Δ *Mortgage Credit Supply*, but for lenders regulated by others, smaller lenders exhibited average declines in mortgage credit supply that were equal to, or slightly less than, those of larger banks. None of these average differences across size categories within a regulatory group was large (the maximum difference was 0.05, and four of the seven group size differences either were zero or 0.01). We conclude from our investigation of lenders that the changes in credit supply are quite similar across regulated types and bank size types.¹⁵

To check the plausibility of our measure of Δ *Mortgage Credit Supply*, we regressed the estimated change in each bank's fixed effect on the change in bank return on assets (*ROA*) from 2004 to 2008, where *ROA* is defined as the ratio of net earnings to total assets.¹⁶ Data on earnings and total assets are obtained from the Reports of Condition and Income, widely known as Call Reports. The estimated coefficient on the change in *ROA* is 3.09 and is statistically significant (with a t statistic of 2.8) and an R-Squared of 0.07. Obviously, *ROA* change does not, and should not, capture most of the variation in credit supply change; nevertheless, it is reassuring that it is positively and significantly correlated with our measure of credit supply change.

Figure 1 maps the geographical variation across counties in our measure of the change in mortgage credit supply from 2004 to 2008, and Figure 2 graphs the density function of the county-specific change in mortgage credit supply between 2004 and 2008. As both figures reveal, credit supply contracted in almost all counties. This change may understate the change that voters perceived, given that the peak of mortgage credit supply was in late 2006, after which supply retreated. The

¹⁵ As part of our general due diligence investigating potential patterns of cross-sectional differences in Δ *Mortgage Credit Supply*, we also checked to see whether the relative generosity in the level of credit supply in 2004 was associated with a greater decline in 2008. We found no such patterns of association..

¹⁶ Our approach for attributing changes in the supply of credit to bank-specific variables follows Cornett et al. (2011) Puri et al. (2011), Jimenez et al. (2012), and Antoniadis (2014).

comparison between 2004 and 2008 reported here, therefore, may understate the extent of the experienced decline just prior to the election.

IV.2 Second-Stage Results: Election of 2008

Our second-stage regression regresses the change in the percentage of county-level votes for the Democratic party candidate ($\Delta\%DemCan$), over the period 2004 to 2008, on the estimated mortgage credit supply change ($\Delta Mortgage Credit Supply$), which is measured by the change in the county-weighted aggregation of the bank fixed effects, estimated in the first-stage regressions). The regression also includes various county-level control variables that potentially affect voting. Specifically, we estimate:

$$(2) \quad \Delta\%DemCan_{kt} = \alpha + \beta(\Delta Mortgage Credit Supply_{kt}) + \gamma Z_{kt} + \delta S_{zt} + q_{kt},$$

where k indexes the county in which the voter resides; z indexes the state in which the county is located; t indexes the election year (2008 initially); Z captures various county-level characteristics; S denotes state fixed effects; and q is the error term.

Table 5 presents second-stage regression results, where the dependent variable is the change in the proportion of votes going to Barack Obama in 2008, less the percentage for John Kerry in 2004 within each county. Standard errors are clustered by Metropolitan Statistical Area (*MSA*). The number of counties for which all data fields are populated is 1,545. We find that a number of control variables capturing changes in counties' economic and demographic characteristics affect the change in each county's support for the Democratic Presidential candidate (which is a vote against the incumbent party in 2008). As predicted by Fair's (and others') analyses, a rise in the unemployment rate or a

drop in personal income improves Democratic voting margins. Younger people, college graduates, and minorities were associated with gains for Democratic shares of votes in 2008, while the presence of Evangelicals (who are typically more likely to vote for Republican candidates) had a negative influence. Perhaps surprisingly, a higher proportion of males tended to favor a rise in the Democratic share, too. In other words, although women tend to vote more for Democrats, they apparently preferred Kerry to Bush even more than they preferred Obama to McCain, *ceteris paribus*.¹⁷

Following Powell and Whitten (1993) we add a *swing vote* variable that controls from short-term shifts in voter support during the previous election cycle, which is meant to capture temporary shifts in voting preferences. Consistent with the literature, we find that these temporary shifts in voting behavior during the 2004 election are reversed in 2008. That is, we find a positive relation between democratic gains/ republican losses in 2008, and the republican gains/democratic losses in 2004.

Column (1) of Table 5 includes only control variables. Column (2) includes the change from 2004 to 2008 in the county's raw mortgage application rejection rate rather than Δ *Mortgage Credit Supply*. Column (3) includes Δ *Mortgage Credit Supply* and the controls. The coefficient on the raw mortgage rejection rate is negative (the "wrong" sign), indicating that greater decreases in mortgage credit supply are associated, *ceteris paribus*, with a decline in the share of Democratic votes, but this coefficient is not statistically significant. Column (3) presents our main finding: decreases in mortgage credit supply from 2004 to 2008 (after controlling for other factors) have a large and highly statistically significant positive effect on increased support for the Democratic candidate. The comparison of columns (2) and (3) shows the importance of identifying mortgage credit *supply*.

¹⁷ We considered alternative specifications of the control variables, which defined them alternatively in levels or growth rates. As Table A1 shows, neither of these alternative specifications reduces the coefficient on Δ *Mortgage Credit Supply*; in fact, we conservatively chose to report the main specification in Table 5 partly because it implies the lowest coefficient value of the three.

Voters reacted to supply shifts, not to changes in raw rejection rates, per se, which reflect a combination of supply-side and demand-side influences.

IV.3 Other Specifications

Here we investigate the robustness of our results to a variety of alternative specifications, which are presented in detail in the Appendix. First, as discussed above, we consider how adding county fixed effects for mortgage credit supply, estimated in equation (1), affect our results. We regard those fixed effects as reflecting a mixture of exogenous demand and supply influences not otherwise captured by the variables included in equation (1). When we add the estimated change from 2004 to 2008 in the county fixed effects estimated by separate cross-sectional regressions in 2004 and 2008 from equation (1), and include the change in those county fixed effects as a county characteristic in equation (2), the estimated coefficient (-0.002) is small and insignificant statistically (its standard error is 0.03). The inclusion of this variable slightly raises the magnitude of the estimated coefficient on Δ *Mortgage Credit Supply*.

With respect to our analysis of the effects of the unemployment rate, we considered whether voters may have been reacting in anticipation of unemployment rate changes rather than in reaction to experienced unemployment (a possibility discussed in several articles reviewed in Section II.2). To investigate this possibility, in our analysis of the 2008 election, we added to our existing model the county-level change in the unemployment rate over the period 2008 to 2009. This variable was insignificant economically and statistically and had no effect on our 2008 voting results. We also experimented with including the homeownership rate in the county and found that it had no effect on our results.

We considered whether voter reactions to mortgage credit supply contraction in 2008 perhaps reflected reactions to other variables that might have been correlated with mortgage credit supply

change. Obvious candidates include foreclosures, home vacancy rates, or changes in housing prices. The home vacancy rate did enter significantly and negatively in the voting regression (indicating that high home vacancy rates in the county led voters to penalize the Republican party in 2008), but the coefficient on the change in the supply of mortgage credit was essentially identical across all these various specifications, including in the presence of the home vacancy rate.

IV.4 The Importance of Mortgage-Credit Supply for the 2008 Election

We approach the question of the importance of the change in mortgage-credit supply contraction for the election by asking a counterfactual question: how much would the electoral result have changed if mortgage-credit supply had been unchanged from 2004 to 2008? We compute the counterfactual voting difference that would have resulted for each county, and aggregate those voting differences to the state level. We measure the importance of the effect by comparing this implied vote difference for each state (if $\Delta Mortgage\ Credit\ Supply$ were zero for all counties in that state in 2008) to the number of votes by which Obama won that state. Obviously, importance is going to be highest in swing states, where Obama's margin of victory was relatively small – in very “blue” or “red” states, the effects of changing economic circumstances are unlikely to swing the state to one or the other candidate.

For nine swing states (defined as the states where Bush won in 2004 but Obama won in 2008), Figure 3 displays the relative magnitudes of the Obama victory margin (shown in blue) and the counterfactual change in voting for Obama from zeroing out the $\Delta Mortgage\ Credit\ Supply$ effect (shown in red). We also show brackets surrounding the counterfactual change indicating the range of values associated with plus or minus one standard error around the estimated effect.

In North Carolina, the implied improvement in the Republican vote share is much larger than Obama's margin of victory, implying that, absent the $\Delta Mortgage\ Credit\ Supply$ effect, North

Carolina's electoral votes would have gone to McCain. In each of the states of Indiana, Florida, and Ohio, zeroing out the Δ *Mortgage Credit Supply* is almost enough to shift these states' electoral votes from Obama to McCain. Indeed, assuming the upper range of the estimated coefficient value (between one and two standard errors above the estimate), all three of these states would have shifted to McCain, implying a shift of 79 electoral votes relative to the needed 93 electoral votes to change the outcome of the election.

Another way to measure the importance of the Δ *Mortgage Credit Supply* effect is to aggregate across the swing states and compare the number of votes in swing states that would have remained Republican under the counterfactual to the total amount of votes needed to win all of the swing states. If Δ *Mortgage Credit Supply* had been zero, fully 51% of the votes needed to win all the swing states would have shifted to McCain (82% if one adds a standard error to the coefficient estimate). In comparison, if unemployment had not risen from 2004 to 2008, the McCain would only have received 9% of the votes necessary to win all the swing states.

IV.5 Extending the Analysis to Other Presidential Elections

The adverse mortgage-credit-supply shift from 2004 to 2008 seems likely to have been particularly dramatic in the minds of voters. How did voters react to mortgage credit supply changes in other periods? We extend our analysis to the other elections over the period 1996 to 2012. As before, we estimate first-stage regressions for mortgage application denials, and use the weighted bank fixed effects from those regressions to estimate cross-sectional differences in county-specific mortgage-credit supply, which are then differenced across election years to produce measures of Δ *Mortgage Credit Supply* at the county level. Figure 4 is analogous to Figure 2, and displays the density functions for the implied cross-sectional mortgage credit supply changes for 1996 to 2000,

2000 to 2004, 2004 to 2008, and 2008 to 2012. Table 6 reports summary statistics analogous to Table 1 for each presidential election year from 1996 to 2012.

Table 7 reports the second-stage regressions comparable to those in column (3) of Table 5, where the dependent variable, as before, is the change in the share of Democratic votes – from 1996 to 2000, from 2000 to 2004, from 2004 to 2008 (which were the focus of our discussion above), and from 2008 to 2012. Due to some data limitations, the specifications reported in Table 7 are not identical to one another across years, but they are very similar.¹⁸

Consistent with the findings for 2008, we find that contractions in credit supply from 2008 to 2012 penalized the incumbent (Democratic) party and benefited Mitt Romney’s candidacy. In the halcyon days of mortgage credit expansion, encompassing the 2000 and 2004 election years, however, there is no evidence that counties with relatively high credit expansion voted in favor of the incumbent party’s Presidential candidate (the coefficients on the change in mortgage credit supply are statistically zero). That is true both when the incumbent was a Democrat (in 2000) and when the incumbent was a Republican (in 2004). In other words, neither incumbent party was rewarded in counties experiencing unusually high expansion of mortgage credit supply during those elections.

These findings indicate an important fact about voters’ reactions to local credit conditions. Apparently, the cross-sectional differences in relative credit supply shifts across counties do not affect voting behavior much in an environment of booming average credit supply. Put somewhat differently, voters don’t reward politicians for experiencing a boom in local credit supply, but are quick to punish politicians for a contraction. This result resonates with the findings reported in Mian, Sufi and Trebbi (2014) in their cross-country analysis of political reactions to financial crises. They find important

¹⁸ The absence of the GINI coefficient in the 1996 and 2000 regressions reflects the fact that county-level data on inequality only became available in the 2008 version of the American Consumer Survey and onward.

and unusual shifts in voting patterns in response to financial crises. The results also resonate with studies we cited above in Section II.2 that find muted responses of voting to economic conditions when the economy is performing well.

Our results regarding asymmetric voter responses across years led us to consider whether those same asymmetries in voter responses are visible across counties *within* years. Specifically, we wanted to investigate whether in 2008, counties with positive credit supply growth had a zero coefficient on Δ *Mortgage Credit Supply*, and similarly, whether in 2004, counties with negative credit supply growth punished incumbent party candidates. Unfortunately, as Figure 4 shows, all of our election years are extreme cases of either nationwide positive, or nationwide negative, mortgage credit supply change. There are not enough positive-value counties in 2008 and 2012, or negative-value counties in 2000 and 2004, to permit testing for within-year asymmetries.

V. Conclusion

Mortgage-credit supply has been an especially active arena for public policy interventions toward the financial system, in the United States and around the world. There is substantial empirical evidence from the behavior of U.S. Presidential candidates, as well as Congressional Representatives, Senators and Governors, which suggests that politicians expect to be rewarded for policies that subsidize mortgage risk and thereby expand the supply of mortgage credit. It is less clear whether those rewards flow through what we have labeled the smoke-filled room channel or the voting channel. To our knowledge, however, prior to this study, there had been no existing empirical study investigating whether voters actually reward politicians for expansions in the supply of mortgage credit, or punish them for contractions.

We identify mortgage-credit supply contractions by modeling the mortgage rejection behavior of banks, using HMDA and other data to control for other influences on mortgage rejections. We find that changes in the supply of mortgage credit at the county level do, in fact, affect voting patterns in Presidential elections.

In the 2008 election, in the wake of the most dramatic reversal in the supply of mortgage credit in U.S. history, the incumbent Republican party suffered huge vote losses (relative to 2004) as the result of contractions in mortgage credit supply. The effects on voting in swing states were particularly pronounced. In the absence of this influence, even if other geopolitical and macroeconomic problems such as the Iraqi War and higher unemployment had remained in place, the election would have been much closer. Half of the votes needed to reverse the election's outcome in nine crucial swing states would have gone to McCain in 2008 if not for the contraction in mortgage credit from 2004 to 2008.

The election of 2012 saw a qualitatively similar effect on vote losses for Barack Obama. However, voting in the elections of 2000 and 2004 – which occurred in the middle of a mortgage credit supply boom – was not affected by changes in local mortgage credit supply. Apparently, relative supply changes do not matter for voting during a boom, but do matter in a bust.

Our results have important implications for the study of the relationship between economic phenomena and electoral outcomes. One econometric lesson is that identification matters: if we had employed county-level measures of mortgage application denial rates, rather than identifying the extent to which those denials reflected supply-side contraction, we would have concluded that there was a muted effect on voting from changes in mortgage credit supply.

The asymmetrically high response of voters to relative supply contraction during a bust indicates that voters' respond very differently to expansions of credit than to contractions. It remains

to be seen if other responses of voters to other economic influences exhibit the same asymmetry. In Table 7, we do find statistically significant responses to unemployment only in the 2012 election, which is suggestive of a similar potential asymmetry in voting reactions to it.

Our finding that voters do not reward the incumbent party for expansions of credit suggests that the “smoke-filled-room” channel is the more important means through which the political process rewards politicians for expanding the supply of mortgage credit. This has two important implications for the potential reform of mortgage credit subsidies. First, reformers seeking to reduce mortgage credit subsidies should focus their efforts during the expansion phase of the credit cycle, which avoids electoral consequences from reforms that limit the expansion of credit supply. Second, that reform bargain should focus on placating special interests rather than voters, given the likely importance of the smoke-filled-room channel during expansion phases of the mortgage credit cycle.

It is interesting that voting responses were especially high in swing states. That finding makes it unlikely that our results can be ascribed to partisan biases that are only coincidentally related to economic differences (a theme of some of the criticisms of the literature claiming to connect economic circumstances with voting outcomes, summarized in Section II.2).

Our findings don’t provide conclusive evidence about the debate over whether voters “vote their pocketbooks” rather than that voting for “sociotropic” reasons, but they do indicate that if “sociotropic” reasons are relevant to voters, they include local, not just national factors. Voters react to *local* mortgage-supply conditions, after taking into account other influences (fixed effects) that apply more broadly to their states or to the nation as a whole. That could mean either that voters are “voting their pocketbooks,” or it could mean that they view *local* mortgage credit supply change as an important sociotropic criterion, over and above news related to state-level or national-level economic concerns.

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Table 1 – Summary statistics of the Home Mortgage Disclosure Act (HMDA) data.

	2004	2008
States	51	51
Counties	3,180	3,168
Census Tracts	19,011	18,971
Financial Institutions (Banks)	317	369
Loan Applications	8,557,111	4,811,881
Applicant Income (average, in thousand US dollars)	87.04	100.84
Loan Amount (average, in thousand US dollars)	165.19	192.63
Loan to Income Ratio	2.25	2.37
Type of Loan: Home Purchase (% of total)	0.35	0.28
Type of Loan: Home Improvement (% of total)	0.08	0.12
Type of Loan: Home Refinance (% of total)	0.56	0.60
Female Applicants (% of total)	0.32	0.33
Hispanic Applicants (% of total)	0.12	0.10
Minority Applicants (% of total)	0.18	0.17
Applications with Co-Applicant (% of total)	0.46	0.48
Loan Rejection Rate	0.25	0.37

Table 2 – Data description

Variable Name	Source	Description
Female Applicant	HMDA	Indicator function -- 1 if loan applicant's gender is female, 0 otherwise.
Ethnicity: Hispanic	HMDA	Indicator function -- 1 if loan applicant's ethnicity is hispanic, 0 otherwise.
Race: Minority	HMDA	Indicator function -- 1 if loan applicant's race is minority (non-white), 0 otherwise.
Loan to Income	HMDA	Requested loan amount over applicants' income (total income for application with co-applicant).
Log(Income)	HMDA	log(Applicants' Income).
Log(Loan Amount)	HMDA	Log(Loan Amount).
Loan Purpose: Home Purchase	HMDA	Indicator function -- 1 if loan purpose is home purchase, 0 otherwise.
Loan Purpose: Home Improvement	HMDA	Indicator function -- 1 if loan purpose is home improvement, 0 otherwise
Loan Purpose: Home Refinance	HMDA	Indicator function -- 1 if loan purpose is home refinance, 0 otherwise
Co Applicant	HMDA	Indicator function -- 1 if application has a co-applicant, 0 otherwise.
Δ (Personal Income)	BEA	Change in per capita personal income between two election years.
Δ (Unemployment Rate)	BLS	Change in unemployment rate between two election years.
Median Age	ACS, Census 2000	Median age of household members.
Median Income	ACS, Census 2000	Median income of household members.
Black	ACS, Census 2000	Black or African American -- Share of total population.
Evangelical	RCMS 2000, 2010	Evangelical Protestant -- Rates of adherence per 1,000 population.
BA Graduate	ACS, Census 2000	Total population 25 and over -- Percent bachelor's degree or higher.
Sex Ratio	ACS, Census 2000	Males per 100 females.
Age Dependency Ratio	ACS, Census 2000	(Population below 18 + Population above 64)/population(18 to 64)
Gini Coefficient	ACS	Gini coefficient at the county level.
Δ (Raw mortgage rejection rate)	HMDA	Loan Applications rejected - Percentage of total applications filed at each county.
Voting	CQ Voting and Elections Collection	US Presidential Elections voting data by county, 1996-2012.
Votes (t-1)	Authors' calculations	Share of votes received by the challenges in the previous election.
Swing votes	Authors' calculations	Change in challenger's voting share during the previous election cycle.

Notes: ACS = American Consumer Survey. BEA = Bureau of Economic Analysis. BLS = Bureau of Labor Statistics, HMDA = Home Mortgage Disclosure Act. RCMS = Religious Congregations & Membership Study. The ACS 3-year surveys of 2008 and 2012 are used because of high coverage (about 1,550 counties sampled each time versus 700 in the ACS 1-year survey). ACS 3-year surveys are not available prior to 2007. For 2000, US Census 2000 are used. 2004 data are the county average of Census 2000 and ACS 2008 data. Evanrate for 2008 and 2012 are based on Census 2010, and are identical for each county across the two periods. Gini coefficient is only available for 2008 and 2012. Data on Hispanic applicants are not available in the HMDA dataset for the years 1996 and 2000.

Table 3 – First stage OLS regression results. Dependent variable: Loan Application Rejection

Female Applicant	-0.00317	(0.00310)
Female Applicant * AFTER	0.00896**	(0.00444)
Ethnicity: Hispanic	0.0486***	(0.0105)
Ethnicity: Hispanic * AFTER	0.0547***	(0.0156)
Race: Minority	0.0653***	(0.00560)
Race: Minority * AFTER	0.0170	(0.0122)
Log(Income)	0.0243	(0.0162)
Log(Income) * AFTER	0.0223	(0.0259)
Log(loan amount)	-0.0631***	(0.0173)
Log(loan amount) * AFTER	-0.00626	(0.0274)
Loan to income	0.0387***	(0.00636)
Loan to income * AFTER	0.0128	(0.0119)
Loan Purpose: Home Purchase	-0.0497***	(0.00753)
Loan Purpose: Home Purchase * AFTER	-0.0542**	(0.0214)
Loan Purpose: Home Improvement	0.0468	(0.0344)
Loan Purpose: Home Improvement * AFTER	-0.0119	(0.0413)
Co Applicant	-0.0291***	(0.00506)
Co Applicant * AFTER	-0.00282	(0.00863)
County Fixed Effects	YES	
Bank Fixed Effects	YES	
Constant	0.328***	(0.0265)
Observations	13,090,171	
R-squared	0.285	

Notes: Standard errors in parenthesis, clustered by bank, *** p<0.01, ** p<0.05, * p<0.1.

Table 4 - Descriptive statistics, change in county-level mortgage credit supply, 2004 to 2008

	(1) Unweighted	(2) Weighted
Counties	3,069	3,069
Min	-0.51	-0.51
Max	0.17	0.17
Mean	-0.18	-0.20
Standard Deviation	0.07	0.03
p10	-0.26	-0.24
p25	-0.22	-0.22
p50	-0.19	-0.20
p75	-0.15	-0.18
p90	-0.10	-0.15

Notes: In the *Weighted* specification, changes in mortgage credit supply are (weighted) averaged across US counties based on total votes casted in 2004 and 2008 in each county.

Table 5 - Second stage regression results. Dependent variable: Change in Democratic votes (% share), 2008

	(1)	(2)	(3)
Δ (Personal Income)	-0.0735*** (0.0154)	-0.0693*** (0.0155)	-0.0724*** (0.0168)
Δ (Unemployment Rate)	0.00192 (0.00187)	0.00166 (0.00131)	0.00178 (0.00157)
Median Age	-0.00319*** (0.000463)	-0.00315*** (0.000510)	-0.00317*** (0.000554)
Black	0.100*** (0.0217)	0.101*** (0.0225)	0.101*** (0.0211)
Evangelical	-7.11e-05*** (1.36e-05)	-6.78e-05*** (1.37e-05)	-6.78e-05*** (1.52e-05)
BA Graduate	0.000909*** (0.000145)	0.000918*** (0.000139)	0.000910*** (0.000138)
Sex Ratio	0.000137 (0.000129)	0.000150 (0.000105)	0.000162 (0.000114)
Age Dependency Ratio	0.00129*** (0.000227)	0.00129*** (0.000208)	0.00130*** (0.000280)
Swing Vote	0.298*** (0.0463)	0.309*** (0.0506)	0.304*** (0.0433)
Votes(t-1)	-0.0724*** (0.0200)	-0.0753*** (0.0192)	-0.0723*** (0.0152)
Δ (Raw mortgage rejection rate)		0.0218 -0.0172	
Δ (Mortgage credit supply)			-0.0634*** (0.0215)
State Fixed Effects	YES	YES	YES
Constant	0.102*** (0.0229)	0.0949*** (0.0201)	0.0846*** (0.0214)
Observations	1,544	1,544	1,544
R-squared	0.726	0.728	0.729

Notes: Bootstrapped standard errors in parentheses. Clustered by MSA. *** p<0.01, ** p<0.05, * p<0.1

Table 6 - Summary statistics of the Home Mortgage Disclosure Act (HMDA) data, 1996 - 2012.

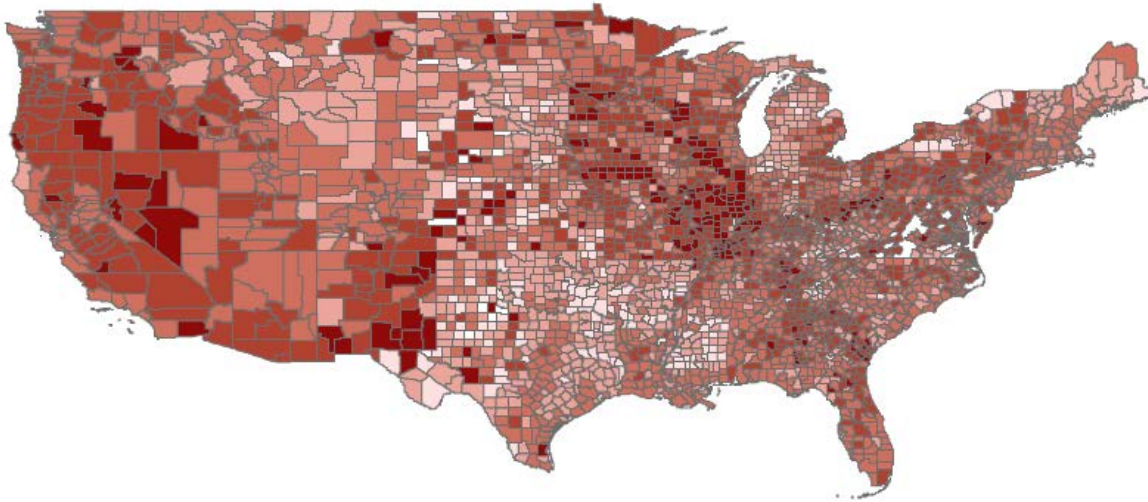
	1996	2000	2004	2008	2012
States	51	51	51	51	51
Counties	3,185	3,182	3,180	3,168	3,166
Census Tracts	15,381	15,437	19,011	18,971	23,607
Financial Institutions (Banks)	505	369	317	369	463
Loans	4,215,083	4,683,734	8,557,111	4,811,881	5,227,738
Applicant Income (avg, in thousand US dollars)	57.82	71.88	87.04	100.84	114.78
Loan Amount (avg, in thousand US dollars)	75.24	102.30	165.19	192.63	201.04
Loan to Income Ratio	1.41	1.62	2.25	2.37	2.25
Type of Loan: Home Purchase (% of total)	0.51	0.57	0.35	0.28	0.17
Type of Loan: Home Improvement (% of total)	0.18	0.12	0.08	0.12	0.06
Type of Loan: Home Refinance (% of total)	0.31	0.31	0.56	0.60	0.77
Female Applicants (% of total)	0.25	0.29	0.32	0.33	0.28
Hispanic Applicants (% of total)	.	.	0.12	0.10	0.77
Minority Applicants (% of total)	0.19	0.24	0.18	0.17	0.13
Applications with Co-Applicant (% of total)	0.61	0.52	0.46	0.48	0.55
Loan Rejection Rate	0.29	0.28	0.25	0.37	0.22

Table 7 - Second stage regression results. Dependent variable: Change in Democratic votes (% share), 2000 – 2012

	2000	2004	2008	2012
Challenger	REP	DEM	DEM	REP
Δ (Personal Income)	-0.0146 (0.00968)	-0.00708 (0.0146)	-0.0724*** (0.0137)	0.0333* (0.0190)
Δ (Unemployment Rate)	0.000454 (0.000446)	0.00179 (0.00115)	0.00178 (0.00141)	-0.00174** (0.000777)
Median Age	0.00159*** (0.000322)	0.000798*** (0.000288)	-0.00317*** (0.000524)	0.000984*** (0.000180)
Black	-0.0925*** (0.00712)	0.0676*** (0.0114)	0.101*** (0.0230)	-0.0476*** (0.00929)
Evangelical	7.20e-05*** (1.05e-05)	-3.71e-05*** (1.22e-05)	-6.78e-05*** (1.23e-05)	6.25e-06 (7.40e-06)
BA Graduate	-0.00270*** (0.000173)	0.00165*** (0.000191)	0.000910*** (0.000131)	7.54e-05 (8.81e-05)
Sex Ratio	0.000651*** (8.70e-05)	-0.000117 (0.000131)	0.000162 (0.000136)	-8.41e-05 (0.000108)
Age Dependency Ratio	2.18e-05 (0.000129)	-0.000372** (0.000162)	0.00130*** (0.000239)	-0.000274** (0.000127)
Gini Coefficient			-0.124*** (0.0372)	0.0408** (0.0196)
Swing	-0.0748** (0.0369)	0.0251 (0.0417)	0.304*** (0.0453)	-0.00434 (0.0340)
Votes(t-1)	-0.0758*** (0.0123)	-0.0422*** (0.0120)	-0.0723*** (0.0201)	0.0399*** (0.00817)
Δ(Mortgage credit supply)	0.0212 (0.0155)	0.00850 (0.0173)	-0.0634*** (0.0198)	-0.0370*** (0.0143)
State Fixed Effects	YES	YES	YES	YES
Constant	0.0149 (0.0174)	-0.0250 (0.0215)	0.0846*** (0.0242)	-0.0132 (0.0141)
Observations	2,966	1,504	1,544	1,485
R-squared	0.663	0.604	0.729	0.660

Notes: Bootstrapped standard errors in parenthesis, clustered by MSA, *** p<0.01, ** p<0.05, * p<0.1. 2004 data extrapolated from Census 2000 and ACS 2008 data.

Figure 1 – Geographic representation of county-level growth in mortgage credit supply, 2004-2008



Notes: We superimpose the 2004-to-2008 changes in mortgage credit supply at the county level to a map of the US. Each county is represented by a cell. The magnitude of the change is color-coded from white (no change) to dark red (large negative change). Because credit expansion occurred in only 4% of the US counties, it is not observable on the map.

Figure 2 - County-level growth in mortgage credit supply between 2004 and 2008.

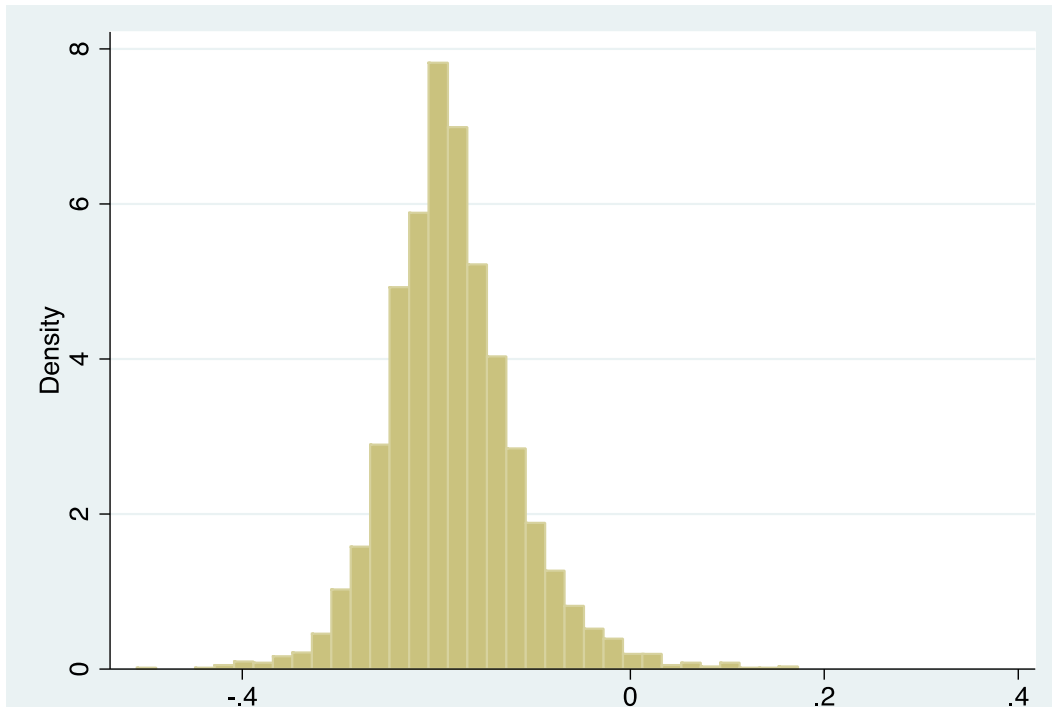
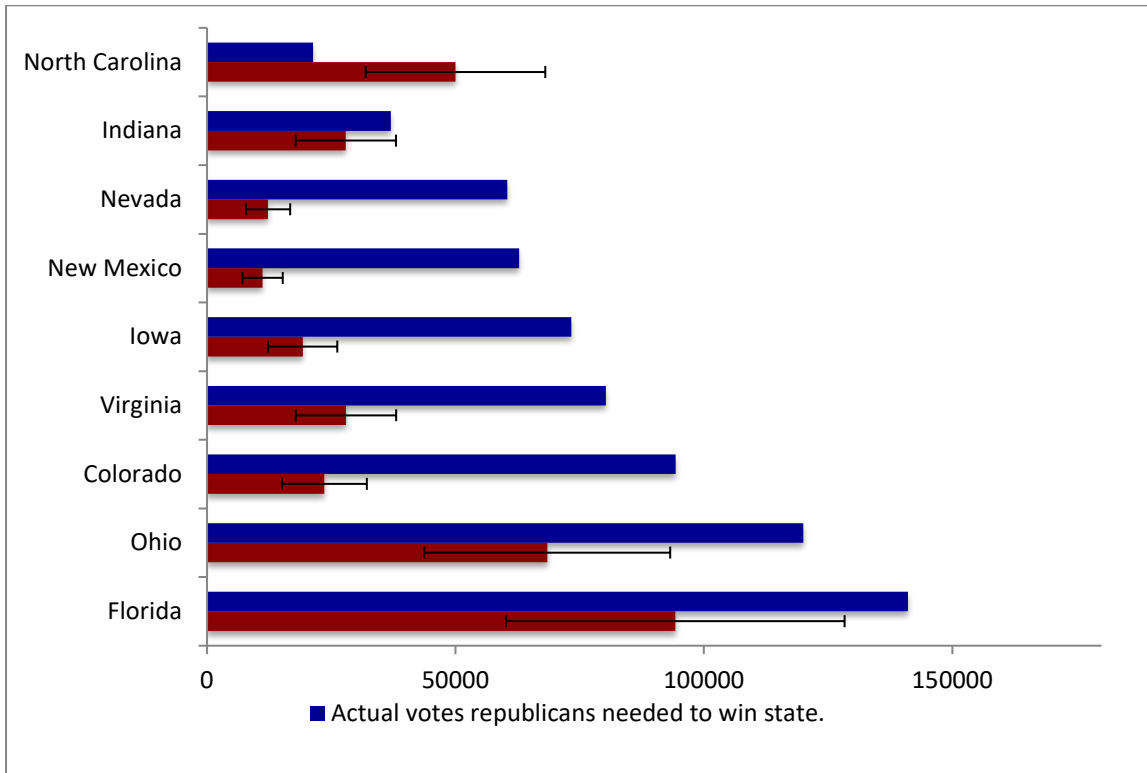
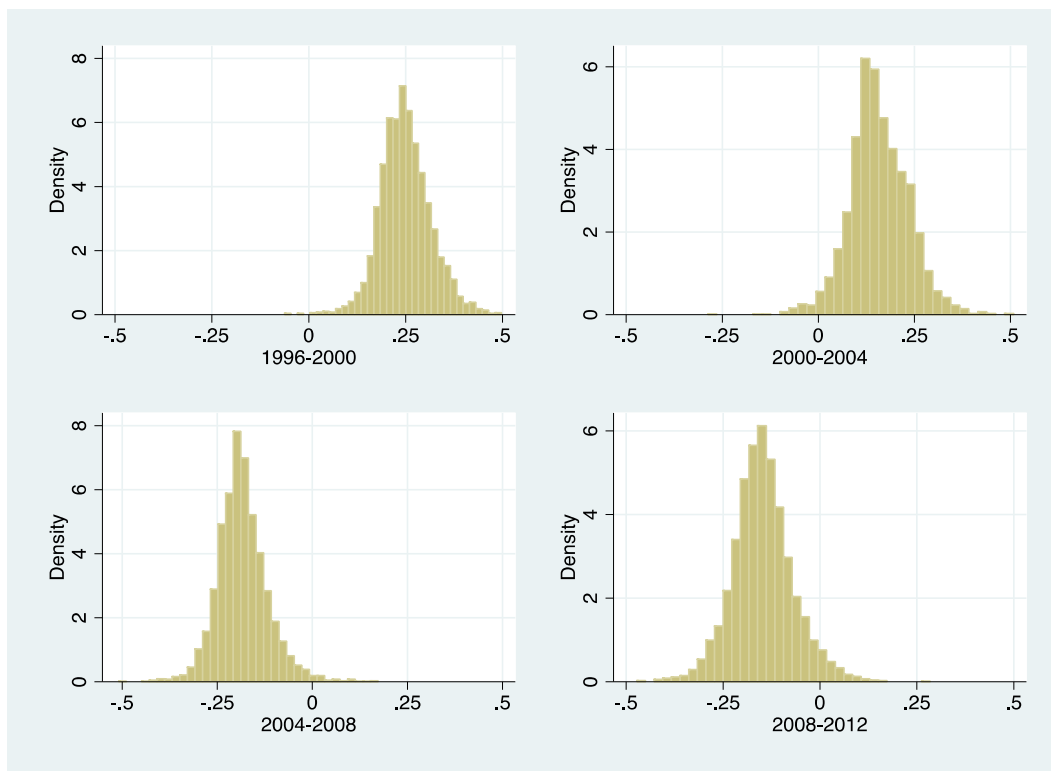


Figure 3 - Swing states electoral counterfactual, 2008.



Notes: Swing states are those where the Democrats won the popular vote in 2008, but not in 2004. In total, had the mortgage credit supply not changed between the 2004 and 2008 elections, the Republicans would have received 51% of the votes needed to win all swing states (82% if we add a standard deviation.). In contrast, had the unemployment rate not changed, they would have received only 9% of the votes.

Figure 4 – County-level growth in mortgage credit supply between presidential elections, 1996 to 2012.



Appendix

Here we present second-stage results from alternative specifications discussed in section *IV.3*.

First, we consider a specification where all control variables enter in levels and another one where they enter as changes between the years 2004 and 2008. The results from these specifications are shown in columns 2 and 3, respectively, of *Table A1* below. For comparison purposes, the original specification is also included in column 1. In all three specifications, the change in the supply of mortgage credit on voting behavior appears to be statistically significant and the magnitude is the same.

Second, we consider omitted variable bias by including additional controls that could potentially matter. Specifically, we add foreclosure rates to test whether the actual concern of voters is not the change in the supply of credit, but rather the expectation that foreclosure rates will rise (which is correlated with the drop in credit). We control for variation in the characteristics of the housing markets across the US by including loan rates, vacancy rates, home-ownership rates, and the peak-to-trough change in housing prices across counties. Finally, if consumers perceive a drop in the supply of credit as a signal that local unemployment will rise in the near future, then the voting effect we document here may be due to the voters' sensitivity to changes in future unemployment and not to changes in the supply of credit. To test this, we include the true unemployment rate between 2008 and 2009 as a proxy for the change in expected future unemployment.

The results from these alternative specifications are summarized in *Table A2*. We present results when each variable is added into the baseline specification, along with the baseline specification (column 1) and a specification with all these variables added together (column 8). We find that changes in the supply of credit affect voting robustly. This effect remains statistically significant and the magnitude does not change across the various specifications.

Table A1 - Second stage regression results: alternative specification of control variables

	(1) Baseline Specification	(2) In Levels (2008)	(3) In Differences, 2004 to 2008
Δ (Personal Income)	-0.0724*** (0.0164)	Personal Income -2.34e-07 (1.58e-07)	Δ (Personal Income) -0.0632*** (0.0194)
Δ (Unemployment Rate)	0.00178* (0.00104)	Unemployment Rate 0.000616 (0.000689)	Δ (Unemployment Rate) 0.00181 (0.00165)
Median Age	-0.00317*** (0.000479)	Median Age -0.00310*** (0.000493)	Δ (Median Age) -0.00175 (0.00136)
Black	0.101*** (0.0218)	Black 0.101*** (0.0214)	Δ (Black) 0.805*** (0.167)
Evangelical	-6.78e-05*** (1.42e-05)	Evangelical -6.51e-05*** (1.26e-05)	Δ (Evangelical) 0.000179*** (4.05e-05)
BA Graduate	0.000910*** (0.000130)	BA Graduate 0.00110*** (0.000171)	Δ (BA Graduate) 0.00145*** (0.000441)
Sex Ratio	0.000162 (0.000144)	Sex Ratio 0.000152 (0.000147)	Δ (Sex Ratio) -0.00165** (0.000803)
Age dependency ratio	0.00130*** (0.000236)	Age dependency ratio 0.00130*** (0.000233)	Δ (Age dependency ratio) 0.00148* (0.000762)
Swing Vote	0.304*** (0.0524)	Swing Vote 0.309*** (0.0536)	Swing Vote 0.311*** (0.0568)
Votes(t-1)	-0.0723*** (0.0188)	Vote(t-1) -0.0758*** (0.0184)	Vote(t-1) -0.00522 (0.0143)
Δ(Mortgage credit supply)	-0.0634*** (0.0204)	Δ(Mortgage credit supply) -0.0684*** (0.0189)	Δ(Mortgage credit supply) -0.0684** (0.0275)
State Fixed Effects	YES	State Fixed Effects YES	State Fixed Effects YES
Constant	0.125*** (0.0290)	Constant 0.117*** (0.0239)	Constant 0.0410*** (0.0104)
Observations	1,545	Observations 1,545	Observations 1,545
R-squared	0.733	R-squared 0.729	R-squared 0.653

Notes: We compare coefficient estimates from three alternative specifications: (1) baseline, (2) with all variables entering in levels, and (3) with all variables entering as 2004 to 2008 differences. The impact of credit supply in voting is significant across all specifications. Bootstrapped standard errors in parentheses. Clustered by MSA. *** p<0.01, ** p<0.05, * p<0.1

Table A2 – Second stage regression results: additional controls included

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Foreclosure rate		0.118 (0.0956)						0.0184 (0.234)
Vacancy rate			-0.119** (0.0500)					-0.127*** (0.0478)
Loan rate				0.0273 (0.0221)				0.0290 (0.0407)
Δ (un rate between 08 and 09)					0.000763 (0.000919)			0.000529 (0.00111)
OFHEO price change						-0.0243 (0.0298)		-0.0179 (0.0532)
Home ownership rate							-0.0252* (0.0147)	-0.0378** (0.0181)
Δ (Mortgage credit supply)	-0.0634*** (0.0215)	-0.0625*** (0.0224)	-0.0635*** (0.0232)	-0.0632*** (0.0225)	-0.0648** (0.0256)	-0.0626** (0.0257)	-0.0623*** (0.0227)	-0.0624*** (0.0229)
Observations	1,544	1,544	1,544	1,544	1,544	1,544	1544	1,544
R-squared	0.729	0.729	0.731	0.729	0.729	0.729	0.729	0.732

Notes: We provide further robustness checks by examining how changes in foreclosure rates (column 2), vacancy rates (column 3), loan rates (column 4), changes in expected unemployment rate (proxied by the actual change in the rate between 2008 and 2009, column 5) and OFHEO price changes (column 6) affect our results. The baseline specification (presented in column 1) and the specification with all regressors included (presented in column 7) are also reported. The data, with the exception of the unemployment rates for 2008 and 2009 and the home ownership rates come from the U.S. Department of Housing and Urban Development (HUD, http://www.huduser.gov/portal/datasets/nsp_foreclosure_data.html). The unemployment data come from the Bureau of Labor Statistics (www.bls.gov). According to HUD, *Foreclosure rate* estimates the number of foreclosures starts over 18 months through June 2008 divided by number of mortgages over the same period. The *Vacancy rate* is the share of addresses that remain vacant for 90 days or longer divided by total addresses as of June 2008 times 100. The *Loan rate* is the percent of loans made between 2004 and 2006 shown to be high cost according to HMDA data. The *OFHEO price change* is a measure of price decline in home values that uses data from the OFHEO Housing Price Index (HPI) to calculate price decline from peak value in the second quarter of any year between 2000 and 2008 and the second quarter home price of 2008. Home-ownership rates come from the American Consumer Survey. Bootstrapped standard errors in parenthesis, clustered by MSA, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In all specifications, changes in mortgage credit supply do affect voting behavior.