Charles A. Dice Center for Research in Financial Economics

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Sumit Agarwal,
Georgetown University

Gene Amromin,
Federal Reserve Bank of Chicago

Itzhak Ben-David,
The Ohio State University and NBER

Serdar Dinc
Rutgers University

Dice Center WP 2017-21
Fisher College of Business WP 2017-03-21

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SUMIT AGARWAL, GENE AMROMIN, ITZHAK BEN-DAVID, and SERDAR DINC*

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Abstract

U.S. House of Representatives Financial Services Committee considered many important banking reforms in 2009-2010 including the Dodd-Frank Act. We show that during this period, the foreclosure starts on delinquent mortgages were delayed in the districts of committee members even though there was no difference in delinquency rates between committee and non-committee districts. In these areas, banks delayed the start of the foreclosure process by 0.5 months (relative to the 12-month average). The total estimated cost of delay to lenders is an order of magnitude greater than the campaign contributions by the political action committees of the largest mortgage servicing banks to the committee members in that period and is comparable to these banks' lobbying expenditures.

JEL Codes: D72, G21, G01

Keywords: Political Economy, Real Estate Lending, Household Finance, Financial Crisis, Lobbying.

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The financial crisis of 2008 led to a sharp rise in foreclosures. As a highly visible symbol of the crisis, foreclosures attracted considerable attention from the politicians, regulators, and the public. In particular, large banks—many of which also service securitized mortgages—and their foreclosure practices faced great public scrutiny. For example, during a congressional hearing on February 10, 2009, Barney Frank, then-chairman of the Financial Services Committee in the U.S. House of Representatives, called for a moratorium on new foreclosure starts.[1] At least one very large bank agreed within days to delay any new foreclosures.[2] Foreclosures remained a focal point in the political discussion in the following months.

Our paper focuses on whether the financial institutions' decisions to start the foreclosure process were systematically related to their political concerns in addition to economic concerns. Congress took many major legislative actions in 2009 to 2010 regarding the financial sector including the Dodd-Frank Act, as well as the issues of capital infusions into failing banks, derivatives trading, and executive compensation. The financial industry thus had many reasons to pay keen attention to legislative developments and to adjust some of its practices in anticipation.

The literature on campaign contributions and lobbying suggests many reasons why banks might attempt to influence the political process through their foreclosure actions.[3] First,

[1] Barney Frank said: "…while we wait for President Obama's plan, I call on institutions that hold or service mortgages to delay and stop any foreclosure proceedings…[I]n this situation where the Obama Administration will have a specific plan shortly, a moratorium is clearly called for."
http://democrats.financialservices.house.gov/press/PRArticle.aspx?NewsID=456

[2] See the letter from Jamie Dimon, the CEO of JPMorgan Chase, to Barney Frank, dated February 12, 2009. http://www.house.gov/apps/list/press/financialsvcs_dem/press021309.shtml

[3] See Grossman and Helpman (2001), Stratmann (2005) and Leech (2010) for surveys of campaign contributions and lobbying. Cooper et al. (2010) shows the relationship between political contributions and stock returns.

foreclosure delays might help decrease pressure on politicians from their voters and allow banks to obtain more favorable legislative outcomes.[4] Second, a lenient approach to foreclosure in the districts of powerful politicians may help banks gain access to them.[5] Third, these delays may help politicians who have a reputation of being sympathetic to the banks' perspective to get reelected.[6] Finally, the politicians themselves might pressure the banks for leniency on delinquent borrowers in their district as the quote above suggests. Naturally, these reasons are not mutually exclusive and possibly not exhaustive.

The foreclosure process on delinquent loans starts only when lender –or its agent, the loan servicer—takes explicit action; therefore, the *start* of the foreclosure process is largely discretionary and can be delayed. To identify political motivations in the banks' foreclosure decision, we follow the political economy literature that emphasizes the importance of the Financial Services Committee in the U.S. House of Representative for the laws related to the financial sector.[7] More specifically, we study whether banks delay foreclosure initiations on

[4] For the "quid pro quo" relationship between interest groups and politicians, see, for example, Austen-Smith (1987), Baron (1989), Baron (1994), Snyder (1990), Snyder (1991), and Grossman and Helpman (2001).

[5] See, for example, Austen-Smith (1995), Ansolabehere, Snyder, and Tripathi (2002), Bertrand, Bombardini, and Trebbi (2014) for the importance of access to politicians in influencing policy.

[6] See Kroszner and Stratmann (2005) for the role of politicians' reputation in the relationship between interest groups and politicians.

[7] See, for example, Romer and Weingast (1991) who study the legislation around the saving and loans crisis, and Nunez and Rosenthal (2004) who study the political economy issues surrounding the passage of the personal bankruptcy reform bill of 2005. Kroszner and Stratmann (1998) provide a theory congressional committees and interest groups and test their theory by analizing House Banking Committee, predecessor of the Financial Services Committee we study.

delinquent mortgages in the districts of House Financial Services Committee members during 2009-2010 when important financial sector legislation was being considered. We use institutional details of U.S. Congress in our test design. For example, given the importance of seniority in Congressional committees, incumbents tend to stay on the same committee for multiple terms. Hence, most members made the decision to be on the Financial Services Committee long before the financial crisis.

We find that mortgage-servicing banks indeed delayed the start of foreclosures on delinquent loans if those loans were located in the electoral districts of House Financial Services Committee members. Importantly, there was no difference in delinquency rates in committee districts so this differential delay cannot be attributed to servicers' capacity constraints under a large volume of delinquent loans. These results are robust to many loan- and location-specific controls, some of which are time-varying (e.g., zip code-level house price changes), as well as any state-specific time effects.

The average time to foreclosure starts in non-committee districts is about 12 months, taking into account the right-censoring at the sample end. In committee districts, however, this average is about half a month longer. Based on the foreclosure cost estimates from the literature and using only the aggregate value of delinquent loans in our sample, we conservatively estimate the direct cost of delay to lenders to be about $30M. Although this cost may be small in the context of the mortgage market, it should be judged relative to other political actions by the large banks. After all, the importance of banks' political activities is not captured by the direct impact of their cost on bank earnings but rather through their potential to influence the political process. For example, the top ten mortgage servicers, which include some of the largest financial institutions like Bank of America, Citigroup, JPMorgan, and Wells Fargo, collectively spent

about $44M during this period for lobbying all of the legislative and executive branches, not just committee members. The combined campaign contributions to the committee members by their Political Action Committees were about $1M. In other words, the cost of foreclosure delays in committee districts is comparable to the lobbying costs for the ten largest servicers and an order of magnitude greater than their campaign contributions to committee members.

Delaying foreclosure starts is a novel channel for banks to influence the political process. Despite the similarities of its cost to the lobbying expenses by these servicers, there are also differences with lobbying and campaign contributions. By law, lobbying expenditures cannot be channeled to politicians' campaigns. These delays, on the other hand, directly benefit the constituents of committee members. Campaign contributions can, of course, be used in the politicians' campaigns but the foreclosure delays may be able to target the politicians' voters even more precisely than some of the campaigning paid by contributions. For example, television advertisements are used heavily in political campaigns but their coverage may necessarily extend beyond the district borders especially in urban areas. By contrast, foreclosure delays can target voters in a district accurately. Unlike campaign contributions, these delays are not subject to campaign contribution limits and do not have to be disclosed.

We verify that our results are not spurious through a variety of tests. As placebo tests, we check the membership in the Transportation & Infrastructure and Defense committees and find no link between membership in either of those two committees and the timing of foreclosures. We also do not find any effect of the Financial Services committee membership in the earlier years when foreclosures did not attract as much attention. In addition, if the effect we detect were due to spurious correlation, one might expect that the Financial Services committee membership in 2009 to 2010 would influence foreclosure decisions before 2009. We find no such effect.

To address the concerns that legislators may self-select into the Financial Services committee based on the delinquency rates of their constituents, we check the robustness of our results by restricting our sample to legislators who were elected to the House and to the Financial Services committee before 2005, well before the onset of the crisis. We find similar, even stronger, effect of committee membership for this subsample as well. We also find similar results when the non-committee districts are restricted to those near the committee districts. Interestingly, we also check for the differences in delinquency rates but do not find any between committee and non-committee districts. Given the consistency of our robustness results, we interpret the delay in mortgage foreclosures as being due to loan servicing banks' attempt to influence the political process.

Our paper is related to three strands of the literature. First, there is a growing body of work that explores various aspects of mortgage market practices. In contrast to a large set of literature that evaluates pre-crisis market developments—see, for instance, Keys et al. (2010, 2012), Mian and Sufi (2009), Adelino, Schoar, and Severino (2016), Mayer, Pence, and Sherlund (2009), Jiang, Nelson, and Vytlacil (2014a, 2014b), and Agarwal, Chang, and Yavas (2012)—we focus on the aftermath of the crisis. As such, our paper is also closely related to studies that explore lenders' or their agents' approaches to loss mitigation of delinquent mortgages whose numbers surged during the crisis period. These studies include work by Piskorski, Seru, and Vig (2010), Agarwal et al. (2011), and Zhang (2013), which compare the start of foreclosures for portfolio loans with that for securitized loans, focusing on potential agency problems. Agarwal et al. (2017) evaluate the effects of the Home Affordable Modification Program that offered mortgage modifications to millions of borrowers. Our focus is the political motivations in banks' approach to delinquent mortgages.

Second, our paper contributes to the literature on political influences by corporations that largely focuses on campaign contributions and lobbying.[8] However, by focusing on a non-traditional political activity, it is similar to Bertrand et al. (2007) who find that politically connected firms distort their labor practices to help favored politicians in elections in France.[9]

Finally, our paper is related to the literature on the political economy of finance, in particular to the studies that examine the role of politics in financial crisis and in its aftermath.[10] Mian, Sufi, and Trebbi (2010) demonstrate that representatives of districts with high default rates were more likely to vote for the Foreclosure Prevention Act in the U.S. House of Representatives. Igan, Mishra, and Tressel (2009) document that lenders that lobbied more for relaxation of rules pertaining to securitization and consumer protection subsequently increased their mortgage lending more and originated loans with higher loan-to-income ratios. Duchin and Sosyura (2010) argue that banks with headquarters located in the district of a member of the House Financial Services committee were more likely to receive TARP funds. Adelino and Dinc (2014) find that financially distressed firms lobbied more for the Stimulus Act and the firms that had lobbied, and not necessarily the distressed firms per se, received larger stimulus funds.[11] Our paper instead studies the actions taken by banks, not by politicians.

[8] See Grossman and Helpman (2001), Stratmann (2005), and Leech (2010) for surveys.

[9] See also Purnanandam and Weagley (2016) who show how financial markets can discipline the government.

[10] See Chinn and Frieden (2011) and Rajan (2012) for general studies of the interaction between politics and economics that led to the financial crisis in 2008.

[11] For earlier studies, see Romer and Weingast (1991) on how the environment in which the Savings and Loans crisis developed during the 1980s was heavily influenced by lobbying; Nunez and Rosenthal (2004) for the correlation between campaign contributions and congressional voting patterns on the Bankruptcy Reform bills in

The rest of the paper is organized as follows. Section I describes the institutional background while Section II describes the data. Section III presents our empirical analysis and Section IV provides robustness analysis. Section V investigates whether delays in foreclosure actions were associated with improvements in other aspects of borrower welfare. Section VI concludes.

I. Institutional Background

A. Political Background

We focus on foreclosures initiated during the 111th Congress, January 2009 through December 2010. Following the literature on the importance of committees in Congress in general, and that of the Financial Services Committee for financial institutions in particular,[12] we study the role of membership in the House Financial Services Committee.

The House Financial Services Committee was very busy in this period. The issues discussed included Troubled Asset Relief Program (TARP) and similar crisis-related programs, deposit insurance limits, restrictions on executive compensation, credit cards, derivatives clearing houses, and consumer financial protection. The Committee also played a crucial role in shaping the broadest financial sector reform since the Glass-Steagall Act of 1933 that culminated in the Dodd-Frank Wall Street Reform and Consumer Protection Act.[13] Moreover, the Committee

2001; and Brown and Dinc (2005) for the role of the electoral cycle in the government's decision to intervene in failing banks in emerging markets.

[12] See, for example, Shepsle and Weingast (1987), Kroszner and Stratmann (1998) for the former, and Stratmann (2002) for the latter. Evans (2011) provides a recent survey of congressional committees.

[13] See Appendix for a timeline of the legislative events that led to the passage of Dodd-Frank Act.

was actively involved in investigating various aspects of mortgage servicing markets, including possible irregularities in the foreclosure process, commonly-referred to as "robo-signing".[14]

Financial institutions were active in this democratic process. The Center for Responsive Politics reports that the financial sector as a whole spent about $459M on lobbying during 2009-2010. Padovani and Gibson (2011) examine the lobbying efforts of banks in regards to the Dodd-Frank bill and find that banks with less traditional businesses, such as securitization, spent more money on lobbying, and increased their lobbying efforts after the financial reform proposal was announced.

B. Foreclosure Laws and the Role of Servicers

Servicers of mortgage loans play multiple roles. First, they monitor and receive scheduled periodic payments from a borrower pursuant to the terms of the mortgage contract. Second, they transfer these payments to the owner of the loan. Finally, if the loan is in default, the servicer has the right, subject to contractual limitations with the lender, to engage in a number of loss mitigation activities. These activities may include loan modifications, pre-foreclosure dispositions (e.g., short sales or deed-in-lieu arrangements), as well as foreclosures.

The foreclosure process works as follows. Typically, upon 90-day delinquency, the lender or servicer issues a 'Demand Letter' or 'Notice to Accelerate'. The borrower has typically 30 days to pay the full amount due. If the borrower fails to pay the full amount, the lender or servicer may initiate the foreclosure process (either through a judicial, i.e., court-supervised,

[14] See http://archives.financialservices.house.gov/legis111.shtml for the legislative decisions of the House Financial Services Committee during 111th Congress.

process or a non-judicial process).[15] Lenders are not likely to settle with borrowers for less than the full amount due.[16]

It should be emphasized that the start of foreclosure is not triggered automatically for delinquent loans; rather, it requires action by the loan servicer. Although the borrowers may have some power to delay the *resolution* of the foreclosure process, especially in judicial foreclosure states, they do not have much power in delaying the start other than making the missed payments.[17,18]

Mortgage foreclosure laws are set at the state level, and there exists wide heterogeneity in such laws across states (Pence (2006), Mian, Sufi, and Trebbi (2015)). Some states require a judicial foreclosure process, in which the repossession of the collateral property is overseen by the court. Before the start of foreclosure, the servicer may try to negotiate forbearance options with the borrower. However, if the servicer decides to initiate the foreclosure, the courts in the judicial foreclosure states start the hearings process to determine the payoff structure for the various lien holders on the mortgage subsequent to the auction of the property. Other states allow

[15] See http://www.nmacenter.org/foreclosureprocess.asp.

[16] See http://portal.hud.gov/hudportal/HUD?src=/topics/avoiding_foreclosure/fctimeline.

[17] See http://portal.hud.gov/hudportal/HUD?src=/topics/avoiding_foreclosure/fctimeline.

[18] Delinquent borrowers may also try to persuade the servicer informally to delay the start of foreclosure. However, to the extent that they also would try to persuade the servicer to accept a 'short sale', in which the servicer accepts the sale of the collateral at a price lower than the outstanding balance without taking the ownership of the collateral, the delinquent borrowers have not been very successful in these informal negotiations before the start of foreclosure. Only two loans that meet our screening criteria had short sales before the start of foreclosure and our current sample of nearly 370 thousand delinquent loans excludes them. The remaining short sales took place *after* the foreclosure process started and, hence, after a loan exited from our analysis.

lenders wide discretion in foreclosure actions although lender actions may still be subject to judicial review to ensure their legality. In these states, mortgages include a 'power of sale' clause, which allows lenders or servicers to repossess and sell the property in the case of default. Typically, the property is put up for auction and the proceeds go toward paying off the remaining debt.

II. Data

Our main dataset is comprised of loan-level information provided by residential mortgage servicers and collected by McDash Analytics. The dataset provides extensive information about the loan, property and borrower characteristics at the time of origination as well as dynamically updated loan information subsequent to origination. The servicer or the lender of a loan is not identified, however. We focus on the loans that became 90-day delinquent for the first time between January 2009 and December 2010, our sample period. Following the literature, we restrict our sample to owner-occupied, single family, first lien loans that were originated in 2005 or after. We include all loans that were owned by one of the three major types of investors: private securitization trusts, securitization trusts by government sponsored enterprises (GSEs) such as Fannie Mae and Freddie Mac, and the banks ('portfolio loans'). We drop the loans whose investor type switches after delinquency; such loans may include the loans that are sold back to the originator due to fraud or early delinquency as discussed in Piskorski, Seru, and Vig (2010). We eliminate loans that are at the top or bottom 1% in appraisal amount, Loan-to-Value ratio, and loan amount, all as of loan origination, to mitigate the effect of outliers.

In addition to loan-level servicer data, we make use of a unique dataset that is constructed through a high-quality match between loan records and borrowers' full credit reports. This dataset is known as Equifax's Credit Risk Insight™ Servicing McDash (CRISM). It is structured

as a borrower-level panel that matches every loan in the McDash servicing dataset with the borrower's Equifax credit file. The matching algorithm relies on a wide array of data available only to the credit bureau such as detailed payment histories, resulting in a high-quality combined dataset. The credit bureau data span the period during which a mortgage loan exists in the servicer dataset. This period is further augmented by the 6 months prior to loan origination and the 6 months following the loan termination (i.e., foreclosure sale or refinancing). For each borrower associated with a given loan, the dataset contains time series that capture that borrower's credit score, total debt amount outstanding in various credit categories, the breakdown of these aggregates into performing and non-performing components, and category-specific required monthly payment. In particular, the dataset captures home equity lines of credit, auto loans (both from banks and captive auto finance firms), and credit cards. Consequently, we are able to observe performance of existing non-mortgage credit and originations of new credit for all of the delinquent mortgage borrowers in our main sample.

The finest level of geographic data for loans available to us is their zip codes. We match all loans to their congressional districts by zip code using the MABLE/Geocorr2K dataset from the Missouri Census Data Center. We drop loans from the zip codes that match to multiple congressional districts. We obtain party affiliation and committee assignments of the representatives from the dataset by Stewart and Woon.[19] Out of a total of 435 members of the House of Representatives in the 111th Congress, 71 served on the Financial Services committee (the Finance committee, for brevity)—42 Democrats and 29 Republicans. Figure 1 provides a map of committee member districts in the contiguous 48 states; Alaska and Hawaii had no

[19] Charles Stewart III and Jonathan Woon. Congressional Committee Assignments, 103rd to 111th Congresses, 1993-2009, available at http://web.mit.edu/17.251/www/data_page.html#2.

representation in the Finance committee. The map leads to several observations. First, the committee members represent a broad geographic cross-section of the US. Second, states with high proportion of financial institutions such as New York, Massachusetts, and North Carolina are well represented. Finally, some of the states with high appreciation of house prices before the crisis such as Nevada and Arizona are not represented in the committee.

<Insert Figure 1 about here>

Loan-to-value (LTV) of a loan, both at origination but also especially during the loan's life is likely to be an important factor in servicer's decision to foreclose when the loan becomes delinquent. Unfortunately, only LTV at origination is provided in our data set. To estimate LTV during loan's life, we use zip code-level Home Price Index provided by CoreLogic. This index is available for at least one zip code for each congressional district except for 19 congressional districts, one of which is represented in the Financial Services committee. Consequently, the loans from those districts are omitted from the analysis. Table I, Panel A gives the number of loans and the number of districts by party affiliation and committee membership.[20] One key takeaway from this table is that the fraction of loans from Finance committee districts (17.2%) is very similar to the fraction of the Finance committee member districts (16.9%).

<Insert Table I about here>

Appendix A provides the description and source of our variables and Table I, Panels B and C, present summary statistics for zip code-level demographic variables and for borrower-level variables, respectively, with the latter provided by CRISM. The statistics are reported for

[20] One district that switched from Republican to Democratic midterm after special elections due to vacancy is omitted from the analysis. One independent member of the U.S. House of Representatives caucuses with Democrats so he is included as 'Democrat'.

the overall sample as well as by the districts of committee and non-committee members. The differences in subsample averages are also provided. The standard errors for the differences in means are corrected for clustering at the congressional district level.

Table I, Panel B, reports sample statistics for demographic variables such as median income, the fraction of African Americans and Hispanic households, both from American Community Survey, unemployment rate from Bureau of Labor Statistics, and the fraction of urban population from the 2000 Census. If the data are provided at the county level, we used the county-level data. The average for demographic variables is not statistically significantly different from each other based on the Finance committee membership with the exception of the median household income and the urbanization, both of which are significantly higher in committee member districts at the 10% and 5% level respectively. The table also provides sample statistics on the decrease in the home price index from the loan origination date to the delinquency date using the zip code-level single-home residential price index from Corelogic. This variable is updated monthly with the contemporaneous index in the regressions. Loans in both non-committee and committee member districts experienced similar decline in house prices at 33.5% and 31.6%, respectively, as of the 90-day delinquency date; the difference is not statistically significant.

Table I, Panel C, provides sample statistics at the borrower level obtained from CRISM. We use data on the borrower's FICO score and total non-mortgage borrowing 6 months before the onset of 90-day mortgage delinquency. We observe no statistically significant difference in sample means between committee and non-committee districts.

Table I, Panel D, provides sample statistics on loan-level variables. As we only have data on the first-lien Loan-To-Value (LTV) ratio as of loan origination, we estimate the

contemporaneous LTV by updating the house value using a zip code-level price index. Although Panel D reports this variable as of the month of delinquency, this variable is updated in regression analysis with contemporaneous index values to capture the continuing changes in house prices during the sample period. Not surprisingly for a sample restricted to delinquent loans, the estimated first-lien LTV ratios are very high, averaging 109%.

Importantly, there is no statistical difference in terms of subsample averages based on committee membership for any of our variables as indicated in the Difference column. Notice that the standard errors reported for those differences are robust to clustering at the congressional district level. Specifically, looking at loan amounts, FICO scores at origination, estimated LTV, interest rate, and various measures of mortgage types (jumbo, interest only, and subprime) there is no difference across non-committee and committee member districts.

Table I, Panel E presents the mean time from the onset of 90-day delinquency to the start of foreclosure. These are 'restricted' means in the sense that they ignore the fact that for many loans the foreclosure process does not commence by the end of the sample period. In other words, these simple calculations ignore the resulting right censoring and underestimate the mean. Nevertheless, the average time to the start of foreclosures is longer in the committee districts and the 95% confidence intervals do not overlap.[21] Perhaps more informatively, we also test for the equality of the distributions of time to foreclosure starts in committee and non-committee districts. The last column in Table I.E provides the chi-square test and its p-value. The test indicates that the distributions of time to the start of foreclosure in committee and non-committee districts are different and the difference is highly statistically significant.

[21] These statistics are estimated using the 'stci, rmean' command of Stata v14.2; no cluster robust standard errors are available for this command.

III. Regression Analysis

A. Empirical Approach

Our identification strategy relies on studying the impact of a political factor that is unlikely to proxy for any economic factor that might affect the servicers' foreclosure decision. The Financial Services Committee membership of the congressperson in whose district the mortgage is located is likely to be such a political, but not economic, factor for several reasons. First, unlike party affiliation, which might proxy for many economic and social factors due to gerrymandering of congressional districts, committee memberships are not tied to districts. If an incumbent loses an election, the newly elected politician in that district does not inherit the incumbent's committee seat. Second, each member's likelihood of becoming the committee chair or ranking member depends on the member's seniority *within* that particular committee, not on their seniority in the House of Representatives. This rule discourages many members to self-select themselves into the Finance committee from their previous committees once the crisis started and the foreclosures increased in their districts. Third, because of the aforementioned seniority rules, most members in the Finance committee became members for the first time well before the onset of the crisis. We perform further checks on this point in the robustness section. Finally, the role of committee membership, especially the role of Finance committee membership on the financial sector has already been well studied though not in this context. Hence, any impact of Finance committee membership on foreclosure starts is very likely to be due to political, not economic, reasons.

Focusing on the Finance committee membership also has disadvantages. First, our identification of politically motivated (in)action by the servicers is relative to their actions in non-committee districts. This does not rule out any political action by the servicers in those non-

committee districts. We only assume that any political motives by the servicers are likely to be stronger in Finance committee districts than in non-committee districts because the former has more power over the banks as shown in the literature in other contexts. To the extent that some of the servicer behavior is also politically motivated in non-committee districts, our methodology underestimates the politically-motivated foreclosure delays in the country. Second, some states and their senators may have greater political power, which might also affect the servicer behavior. As typical in political economy literature, we choose to use state fixed effects—or, rather, state*time interaction fixed effects—to control for all state-level differences. In that respect, our approach will again underestimate the political effect on foreclosures. Finally, the majority party has more power in the House of Representatives. Unfortunately, congressional districts are not drawn randomly and the party affiliation of the district representative may proxy for unobserved demographic factors. Therefore, we control for the politician's party affiliation in the analysis but we choose not to interpret its coefficient in political terms. In other words, to the extent that servicer behavior is different in the districts of majority party members, our analysis will capture it but not interpret it as political delay.

Our null hypothesis is that the servicer decision to initiate the foreclosure process on a delinquent loan does not vary according to whether the loan is in the district served by a member of the Finance Committee. To test whether our null hypothesis can be rejected, we focus on the rate of foreclosure starts of 90-day delinquent mortgages conditional on the foreclosure not being initiated yet, namely, the hazard rate of foreclosure starts. Our main empirical specification is an exponential hazard model of the servicers' foreclosure decision for a loan that becomes 90-day delinquent for the first time in month t during the period spanning January 2009 through December 2010:

$$h(t) = \exp\left(\beta \mathbf{x}_{it} + \gamma * FinanceCommittee_i + \theta_{st}\right), \; t = t_{i0} \ldots t_{iT}, \tag{1}$$

where $FinanceCommittee_i$ is a binary variable that is set to one if the loan is located in a district whose Representative is a member of the House Financial Services Committee; $\mathbf{x}_{it}$ is the vector of explanatory variables for loan i in month t. It includes both loan level and zip code level variables that are as of origination or as of month t as well as the party affiliation of the district's representative; θ_{st} are state-specific month fixed effects.[22]

The loan i enters the study in month t_{i0}, which is the first occurrence of the 90-day delinquency status for that loan. The same loan exits the study in month t_{iT}, which is the earliest occurrence of the start of foreclosure or one of the exit events such as becoming current or no longer being reported. Finally, since the servicer decision in a given congressional district may not be independent from another decision within the same district, the error terms in the regression above may be correlated within a district, which would lead to underestimated standard errors for the coefficients. Hence, following Bertrand, Duflo, and Mullainathan (2004), all the errors reported in this study are corrected for clustering at the congressional district level.

Loan-level controls are motivated by the literature. They include indicators for FICO score above 680 or between 620 and 680 (omitted category: FICO<620), debt-to-income ratio at origination, indicators for fixed rate and interest only loans (omitted category: Adjustable Rate Mortgages), indicators for full documentation and unknown documentation (omitted category: no/low documentation), and indicators for jumbo, low grade, refinance loans and securitization

[22] The Cox hazard model, which allows for arbitrary duration dependence, takes a prohibitively long time to converge with state and month fixed effects for us to adopt it as the main model throughout the paper. We still repeat the main regression using the Cox model and find our results to be very robust, as reported in the Robustness section.

status (GSE or private label, omitted category: portfolio loans). Following the literature, we also include an indicator variable for LTV at origination equal to 80% as a proxy for the existence of a second lien on the property. Continuous loan-level variables include (log of) loan amount, first interest rate observed, elapsed time from origination to the first classification as 90-day delinquent, and the decrease in the residential home price index for that zip code since the loan origination.

Since our main hypothesis variable is based on politics and geography, our regressions also include zip code-level demographic variables in addition to the party affiliation of the congressperson representing that zip code. These variables include (log of) median household income, shares of African Americans and Hispanic households, unemployment, urbanization and the decrease in the home price index since the loan origination.

B. Main Result

Our main result, showing that foreclosure starts are delayed in the districts of Financial Services committee members, is presented in Table II. Column (1) serves as the benchmark; it includes all our control variables, including party affiliation, but not the Financial Services Committee membership.

<Insert Table II about here>

The results from the baseline specification are largely intuitive and fall within the broader context of the literature evaluating servicer decisions to modify or foreclose delinquent mortgage loans. The discussion in this literature focuses on servicer ability to identify loans that are either likely to self-cure in the absence of any actions or would redefault even in the event of modification (e.g., Piskorski, Seru, and Vig (2010), Agarwal et al. (2011), Adelino, Gerardi, and Willen (2013), Zhang (2013)). These studies show also that foreclosure decisions are influenced

by a number of agency frictions that create conflicting incentives between owners of the loans (GSEs or private investors) and entities making modification/foreclosure decisions (servicers). Consistent with these studies, we find that securitized loans are foreclosed more quickly than portfolio loans. The same is true for low-grade loans, loans underwritten without documentation, and non-fixed rate and non-amortizing loans, all of which suggest ex ante higher risk of redefault. These findings are consistent with Agarwal et al. (2011), who use a more detailed dataset and find that these borrowers are more likely to experience foreclosure and less likely to be offered loan modifications.

Column (2) of Table II includes only party affiliation and our main hypothesis variable, *Finance Committee*, which is set to one if the congressperson representing that zip code is a member of the House Financial Services Committee. The latter has a negative coefficient that is statistically significant at the 1% level. This suggests that the loan servicers delay the start of foreclosure for a delinquent loan if the loan is located in the district of a Financial Services committee member. Note that the analysis also controls non-parametrically for all the time-specific state-level factors through state*month fixed effects so this result is unlikely to be capturing any state-level factors. Also, the errors are robust to clustering at the congressional district level so the results are unlikely to be due to the large number of observations.

The regression in column (3) adds all the loan-level and demographic variables. The *Finance Committee* variable again has a negative coefficient that is statistically significant at the 1% level, which confirms the foreclosure delays in committee districts. The magnitude of the delay in committee members' districts relative to non-committee members' districts is about 3.7% (=1 – exp(-0.038)). Finally, the regression in column (4) repeats that in column (3) with only state and month fixed effects rather than state*month interaction fixed effects as some of

the estimation presented later did not converge with the latter set of fixed effects. The results are again both quantitatively and qualitatively very similar.

Notice that the magnitude of the coefficient for the *Finance Committee* variable changes little from columns (2) to (3) or (4) even though the latter specifications include many demographic and loan-level variables suggested in the literature. This suggests that committee membership of the politician is indeed orthogonal to many loan and borrower features as well as zip code demographics. On the other hand, the coefficient of the majority party indicator, which may proxy for demographic and economic factors unlike the committee membership indicator, changes substantially and loses significance. In other words, both for the institutional reasons that determine the committee membership and for the presence of many financial and demographic control variables, the effect of committee membership we find is very unlikely to be due to any economic reason. Instead, the delay we find in these districts is likely to be due to political reasons.

C. Time to Foreclosure and Economic Significance

The hazard model presented above is the most common approach to survival time analysis but its interpretation may not be very transparent. In this subsection, we focus directly on the time it takes from the onset of 90-day delinquency to the start of foreclosure. More precisely, we estimate the following regression:

$$\log(T_i) = \beta \mathbf{x}_i + \gamma * FinanceCommittee_i + \theta_s + \lambda_t + \varepsilon_i, \qquad (2)$$

where T_i is the time from the onset of 90-day delinquency to the start of foreclosure for loan i. $\mathbf{x}_i$ includes all the control variables in the previous hazard estimation. All of the time-varying variables are measured as of the onset of delinquency. The model also includes fixed effects for state and the first month of delinquency.

The OLS estimation of (2) cannot distinguish the exit reason of a loan after *T*. In particular, unless the foreclosure process commences for all delinquent loans before the end of our sample period, OLS will underestimate the time-to-the-start-of-foreclosure. However, a loan may exit the analysis after *T* not only due to the start of foreclosure but also because a loan may become current (self-cure) or because we do not follow the loans beyond the end of our sample period in December 2010 (right censoring). Hence, the proper estimation needs to take the exit reason into account. Consequently, regression (2) is estimated by Maximum Likelihood with the likelihood function that incorporates the exit reason for a loan after *T*. Naturally, the likelihood function depends on the assumed distribution for the error term ε_i. We estimate (2) by using four different distributional assumptions from the literature, namely, normal, logistic, extreme value, and extreme value with a scale parameter.[23,24] Note that the latter two distributions imply exponential and Weibull hazard functions, which further relate this analysis to the one in the previous section (Table II). The regressions include state and month of delinquency fixed effects[25] and standard errors are again clustered at the congressional district level.

Table III presents the results of this analysis. The estimated coefficient for the *Finance Committee* variable is positive and statistically significant at the 1% level, which indicates longer time to foreclosure for delinquent loans in the districts of finance committee members. The

[23] See Klein and Moescherber (2013, pp. 45-49) and, for a more applied approach, Cleves, Gould, and Gutiérrez (2004, pp. 222-243).

[24] The estimation was performed using Stata's 'streg' command. Option 'time' was specified to indicate the use of accelerated failure time metric for the latter two distributions.

[25] Margin calculations to estimate the mean and median survival time in committee districts below did not converge with state and delinquency month interaction fixed effects.

magnitude of the coefficient estimates range from 0.037 to 0.044. Given the logarithmic form of our dependent variable, these estimates suggest about 3.7-4.4% increase in the time to foreclosure in committee districts.

<Insert Table III about here>

The estimated mean survival time to the start of foreclosure for the full sample ranges from 12.1 to 33.8 months depending on the assumed distribution for the error term. The estimated marginal increase in the mean survival time for the committee districts ranges from 0.5 to 1.5 months.[26] Since the estimates of the mean may be sensitive to the distributional assumptions for the right tail of the error term, we also report the predicted median time, which is a more robust indicator of centrality. The estimated median survival time for the full sample ranges from 7.7 to 8.5 months, while the estimated marginal increase for the median survival time in the committee districts ranges 0.3 to 0.4 months—again a non-negligible marginal effect.

We also use these results to estimate the dollar amount for the cost of delay to the financial sector and compare it to the campaign contributions to the Financial Services committee members by the largest banks (servicers) that determine the delay. This estimate will naturally be imprecise because we lack information on when the banks expected to complete the foreclosure process and what amount they expected to recover from foreclosures. Their expectations, unobservable to the econometrician, are related to their expectations about future house price dynamics, which are also unobservable.

[26] These are the sample mean and median of marginal effects at the loan level, not the 'margin at the mean/median'. They are estimated using Stata's 'margins' command taking into account that *Finance Committee* is a discrete variable.

Any cost of delay to the lenders is also likely to be underestimated because our test design is geared towards identifying the political delays in the foreclosure starts, not towards quantifying the total effect of political motivations in the foreclosure starts. In particular, our methodology measures delay in committee districts relative to non-committee districts and thus it is silent about the possibility of political delays in the non-committee districts as well.

Despite these problems about precision and underestimation, a proverbial "ballpark figure" may still be informative and we proceed by using the existing work on this topic. Cordell et al. (2015) focus on direct foreclosure costs that vary with the time the loan remains in delinquency and foreclosure. These costs include hazard insurance, property taxes, maintenance and repair, and increased depreciation in house value (Melzer (2017)). Their calculation controls for fixed foreclosure costs and excludes any negative externalities to the neighboring properties. These delay costs are ultimately borne by the lenders but the servicers may have to make these expenditures until liquidation and recover them only at liquidation. They estimate the total foreclosure delay costs to be, on average, about 18% of the unpaid loan balance and the average time from the start of foreclosure to liquidation to be 32.1 months during the period from November 2008 to August 2010. These figures rise to 20% of the loan balance and 33.6 months during the September 2010 to January 2012 period.[27] We use these costs and length of time as the basis for our calculation. For the average delay in committee districts, we use 0.5 months, the lowest estimate in Table III. Assuming that the total costs of foreclosure delays in Cordell et al. (2015) increases linearly by time, we arrive at the monthly cost estimate of $280-$300 per

[27] See Tables 3 and 6 in Cordell et al. (2015). The length of time spent *in* foreclosure is derived by constructing a weighted average of Cordell et al. estimates for judicial and non-judicial states' timelines using our sample composition.

$100,000 remaining loan balance on average.[28] Even with an aggressive assumption of 20% annual cost of capital for the lenders, the present value of monthly delay costs as of the onset of delinquency and evaluated at the average time from delinquency to foreclosure and to liquidation is about $170 to $180 per $100K of delinquent loan balances.[29]

Although this may not be a large amount per loan, with $17.1B in outstanding delinquent balances in districts of the Financial Services committee members in our sample, it implies a total non-discounted cost of about $48M to $51M at the time of delinquency (or about $30M using the 20% cost of capital assumption). As one benchmark for comparison, the largest ten residential mortgage servicers,[30] which service the large majority of the loans in our sample, made campaign contributions of $980,000 to the Financial Services committee during our sample period.[31] Their *total* lobbying expenditures for the Legislative (both chambers in

[28] (0.5*100000*0.18/32.1) and (0.5*100000*0.20/33.6), respectively. The lower figure is based on liquidation in the earlier period while the higher figure is based on the liquidation in the latter period as discussed above.

[29] We do not know the ultimate lenders except that about 10% of loans are portfolio loans owned by the servicers themselves. We adopted a high discount rate to emphasize the fact that the qualitative comparison of foreclosure delay costs to political expenses by these banks, as described below, is not very sensitive to cost of capital estimates within the likely range.

[30] As the mortgage servicing industry is fairly concentrated, the top ten servicers account for the vast majority of loans. This is reflected in all loan-level servicer datasets. As reported by Mortgage Servicing News (2010), the ten largest servicers are Bank of America, Wells Fargo, JPMorgan Chase, Citigroup, Residential Capital (formerly GMAC), U.S. Bank, Sun Trust, PNC, PHH, and OneWest.

[31] Based on the official disclosure reports as provided by the Center for Responsive Politics through its website opensecrets.org. The size of campaign contributions of large banks may seem small relative to the size of the mortgage market but they are in line with total corporate campaign contributions relative to the size of the U.S. economy, see Ansolabehere, de Figueiredo, and Snyder (2003).

Congress) and the Executive branch of the government was about $44M in 2009 and 2010.[32] In other words, the total in-sample cost of delay is an order of magnitude larger than the campaign contributions of the large servicing banks to the Financial Services committee members during our sample period; in fact, it is more comparable to the lobbying expenses by these firms.

The fact that these costs are substantially greater than the campaign contributions has important political economy implications. Corporate lobbying expenses faced little restrictions but corporations were subject to limits in their campaign contributions during this period. However, lobbying expenses could not legally be channeled to politicians. The delays in foreclosure starts we document could, on the other hand, be targeted directly to the constituents of particular politicians. In fact, in some respects, they could be even better targeted to their constituents than some of the mass media political advertisements for which campaign contributions are used to pay, as discussed in the Introduction.

The calculations above are necessarily imprecise. However, they also suggest that, if the campaign contributions by the largest banks to the Financial Services committee members or

[32] The lobbying disclosure filings are much less detailed than those for the campaign finance; the former do not even disaggregate between the expenses for lobbying the legislative branch from those for the executive branch let alone a specific politician. During the 2009 to 2010 two-yearly cycle, these ten servicers spent about $44M for lobbying using the data from the Center for Responsive Politics through its website opensecrets.org. Just to obtain an estimate for expenses the banks incurred lobbying the committee members, one may assume that none of their lobbying was for the executive branch (probably a very strong assumption) and that the expenses incurred for lobbying the House Finance committee members was 34.3%, the same fraction of their campaign contribution to the committee members relative to their total contribution to all the House and Senate members. With these assumptions, the expenses incurred by these servicers for lobbying the House Finance committee members can be estimated to be about $15.1M or about half of the estimated cost of delaying the foreclosure starts.

their lobbying are politically important as shown in the political economy literature reviewed in the Introduction, the delay in foreclosure starts in the district of committee members is also politically important.[33]

IV. Robustness

We conduct several robustness tests for our results. First, we run a series of placebo tests where we explore foreclosure patterns for members of other committees and for the Financial Services committee members in past periods. Second, we test whether we see a similar impact for the onset of delinquency. Third, we discuss whether committee members may be self-selecting into financial services committee. Fourth, we check the robustness of our results to alternative econometric procedures. Finally, we test whether the results hold in subsamples.

A. Placebo Tests: Other Congressional Committees

We conduct several falsification tests. In the first falsification test we explore whether differences in the rate of foreclosure initiations are affected by the membership on Congressional committees that have no ostensible link to housing markets. The three largest committees in the U.S. House of Representatives in the 111th Congress were Transportation & Infrastructure, Financial Services, and Armed Services, in that order. It is unlikely that banks would choose to adjust their servicing practices in the districts of the Armed Services or the Transportation Committee members. The membership in those committees can serve as a good placebo to our

[33] Although our sample includes all mortgages in McDash that became delinquent in 2009-10 and passed the screens described in the Data section, the data coverage is not universal. The approach of comparing the cost of delay *in* our sample to the *total* political expenditures by these servicers avoids extrapolation of results out of sample but it is conservative.

analysis because they have very few common members with the Financial Services committee.[34] Using the membership on each of these committees in turn, we re-estimate our main specification. The results are presented in Table IV, columns (1) and (2). We detect no statistically significant effect of membership in the defense or the transportation committee on servicers' foreclosure initiation decisions.

<Insert Table IV about here>

B. Placebo Tests: Past Periods

Next, in columns (3) and (4) of Table IV, we also use foreclosure choices for loans that became delinquent in previous Congresses as placebos. Our focus is on the 109th (2005 to 2006) and 110th (2007 to 2008) Congresses, as delinquency rates during those years were not as high as in 2009 and 2010 and had not yet become as political an issue as during our main study period. Hence, any bank that was active in mortgage servicing was much less likely to adjust its foreclosure actions to mitigate any legislative concerns it had. Moreover, in previous sessions of Congress, the Financial Services committee did not consider any legislation that was nearly as important as that taken up by the 111th Congress.

We repeat our main regression specification from Table II first for 2007 and 2008 years focusing on loans that become 90-day delinquent for the first time during this period. We then repeat the analysis for the 2005 to 2006 period. In both cases, we reconstruct our political economy variables to reflect contemporaneous party affiliation and committee membership. The results are reported in Table IV, columns (3) and (4). As expected, the foreclosure decisions on

[34] After eliminating the few members who did not stay in the House or in the Committee for the full term, Financial Services committee has only 4 members who are also in the Transportation committee and only one in the Armed Services committee.

delinquent loans are not measurably different in districts of the Financial Services committee members. The results show that coefficients are statistically insignificant and, for the 2005-2006 period—have the opposite sign.

C. Self-Selection into Committee Membership? Analysis of Delinquency Rates

One possible concern is whether legislators self-select themselves into the Financial Services committee based on foreclosure practices in their districts. The reverse causality is not very plausible; legislators are unlikely to choose to be a member of the Financial Services just because banks delay foreclosing on their delinquent voters. However, an unobservable factor driving both the committee membership decision of legislators and the foreclosure decisions of banks would present a bigger concern. For example, if delinquency rates are expected to be high in a district, the legislator representing the district may want to be in the Financial Services Committee. An increase in delinquencies in committee districts in itself is not sufficient to observe the pattern we document because our analysis is conditional on the onset of delinquency. However, if the foreclosures are delayed later due to the large volume of delinquencies in that district, we can then see the effect we observe above.

To rule out this possibility, we extract all the loans that are current and have never experienced delinquency as of December 2008 and follow them through December 2010. We study their rate of reaching 30-day and 90-day delinquency in a hazard model.[35] We use the same

[35] Note that our main sample of first-time 90-day delinquent loans is *not* a subset of this sample of current loans with a three-month lag. Many of the delinquent loans in our main sample were 30-day delinquent for the first time more than two months before their first 90-day delinquency; they became current again by making payments in arrears before reaching 90-day delinquency that leads to the loan's inclusion in our main sample. The loans with such delinquencies in their past are excluded from the analysis in this subsection.

control variables as in our main analysis. The results are reported in Table V. We find no statistical difference in the rate of delinquencies in districts of Financial Services committee members. In other words, our results are unlikely to be driven mechanically by differential rates of delinquencies in committee members' districts or by the self-selection of legislators based on expected delinquencies.

<Insert Table V about here>

D. Self-Selection into Committee Membership? Analysis Using Senior Committee Members

The institutional features of congressional committees provide us with another test for the possibility of self-selection by the legislators into the Financial Services committee. Senior committee members tend to be more powerful and legislators receive seniority based on their service tenure in a given committee. When legislators leave or switch their committee, they lose their seniority in the committee even if they remain in the Congress. In fact, all 32 members of the Financial Services committee in the 108th Congress (2003-2004) that were still in the U.S. House in the 111th Congress remained on the Committee. In other words, many members of the 2009-2010 Financial Services committee made the decision to be in the committee well before the foreclosure crisis. We use this feature to address the concerns of self-selection into the Financial Services committee. Whatever the economic determinants of the banks' foreclosure decision might be in 2009 and 2010, they are likely to be orthogonal to the committee membership decision made by the politicians years before.

The regression in column (1) in Table VI repeats our main specification by excluding all the Financial Services committee members who joined the committee in 2005 or later. The second column repeats our main specification by excluding all the representatives who first took office in the House in 2005 or after. Our main results remain robust to these exclusions; the

foreclosure of delinquent loans is delayed in the districts of Financial Services committee members. In fact, the magnitude of this political effect becomes larger, which suggests that the political effect we detect may be stronger in the districts of senior members.[36]

<Insert Table VI about here>

E. Alternative Time Periods

Our sample lasts until the end of 2010 but in the latter part of that year the so-called "robo-signing" practice came to widespread attention. This practice refers to cases where the large servicing banks seemed to process large numbers of foreclosure documents in a short time without legally required individual attention. Although it would be interesting in itself if the servicing banks reacted differently in committee districts to the attention by the media and politicians on this issue, it is also desirable to check the robustness of our results to the exclusion of that period. We repeated our main regression by stopping the analysis period first at the end of December 2009 and then at the end of March 2010, both of which were well before the robo-signing attracted attention. We present the results in Table VII, columns (1) and (2). Our earlier results remain robust both quantitatively and qualitatively to excluding the latter part of our sample period. It is also worth noting that all of our specifications include state-month interaction fixed effects, which would pick up state-level delays caused by robo-signing.

[36] Notice that, in the second column, which focuses only on senior politicians, the coefficient of the majority party is also negative and very significant. This indicates that foreclosure starts were delayed in Democratic districts and this delay may also be, at least partially, due to political reasons because more senior members of the majority party, even outside the Finance committee, are likely to have more power over legislation of interest to the banks. However, we choose not to interpret the statistical significance of the party affiliation coefficient as evidence of political delay. Unlike the committee membership, party affiliation may proxy for demographic and economic factors that may also affect bank decisions.

<Insert Table VII about here>

F. Alternative Econometric Methods

We also check the robustness of our results to different econometric methods. First, we estimate our main regression in Table II, column (3) using the Cox proportional hazard model. The Cox hazard model has the advantage of allowing arbitrary duration dependence (baseline hazard). We report the results in Table VII, column (3). The coefficient of the finance committee indicator is very similar to that reported in Table II both in magnitude and in statistical significance. Next, we repeat that regression using Weibull distribution and report the results in column (4). We again obtain very similar results both quantitatively and qualitatively.

We also estimate a discrete hazard model based on the linear probability model given below:

$$y_{it} = \beta \mathbf{x}_{it} + \gamma * FinanceCommittee_i + \theta_{st}, \;\; t = t_{i0} .. t_{iT}, \qquad (3)$$

where y_{it} is a binary variable that is one if the foreclosure starts for loan i in month t. This model may not properly take right censoring into account and face the usual problems of estimating probabilities using OLS but it also provides a robustness test for the distributional assumptions in the previous analysis. The results, presented in column (5) of Table VII, show that our previous results are robust to this alternative econometric specification.

G. Nearby Zip Codes Only

A possible concern for our results is that we may be comparing the foreclosure rates in non-committee member districts that may be rural to those in committee-member districts that may be urban within the same state despite including the urbanization rate of the congressional district in the control variables. Although we control for the proportion of urban population in the regressions, since the housing crisis affected urban areas disproportionately, it is still important

to check the robustness of our results by limiting the comparison only to the zip codes that are near one another. Hence, in Table VII, columns (6), (7), and (8) we estimate our main regression in Table II, column (3), but we now require all the non-committee zip codes to be within 10, 25, and 50 miles of a committee zip code in the same state and vice versa. This significantly reduces the sample size. For example, our 10-mile sample in column (6) is less than a quarter of the original sample we have for our main regression in Table II, column (3). However, in all three specifications our results are both economically and statistically significant.

H. Subsamples by Loan Characteristics

We also examine subsamples by loan characteristics. In Table A.I in Online Appendix, we purge subsets of the sample. These columns show that the results hold in all the subsamples. In column (1) we examine only mortgages that are classified as 'grade A' (or non-subprime) by the servicers. In column (2) we restrict the sample to loans with maturity of 15, 20, and 30 years at origination. These are the most common maturities and the loans with different maturities may be non-standard on other dimensions as well. In column (3) we drop from the analysis October 2010 and afterwards. That period includes both the 'lame duck' months of November and December 2010 as well as October 2010 when the news about the so-called "robo-signing" in the foreclosure process broke out. Our results remain robust in all the subsamples; foreclosures of delinquent loans are delayed in the districts of the Financial Services committee members and the magnitudes are comparable across these specifications.

Our identification strategy is already based on a factor that has a very political nature but little, if any, economic nature. These robustness checks allow us further to rule out economic explanations for the delay in foreclosures. Hence, we interpret the delay in foreclosure initiations as due to loan servicers' political concerns.

V. Welfare and Potential Economic Channels

It is conceivable that loan servicers granted foreclosure delays in the Financial Services Committee member districts in order to allow the delinquent borrowers residing in such districts to regain their financial footing and cure their delinquencies. If this were the case, we would expect to find superior performance on existing non-mortgage credit obligations among delinquent mortgage borrowers in such districts. Similarly, we would expect to find that these borrowers were also more successful in obtaining new non-mortgage credit. The CRISM dataset that uniquely links mortgage loans with the rest of the borrower's credit records allows testing of this hypothesis. In particular, we analyze performance and origination of credit in several categories: auto loans, credit cards, as well as student loans and retail (store card) credit for the borrowers whose mortgages are 90-day delinquent during 2009 to 2010 and thus form the sample for our main analysis above.

A. Summary Statistics

We begin our analysis by documenting the relative importance of various categories of non-mortgage borrowing in Panel A of Table AVII in Online Appendix. Credit card balances account for nearly half of outstanding non-mortgage debt among the delinquent borrowers in our sample while auto loans make up about 29% of non-mortgage balances. More importantly, there are no statistically significant differences between shares of various non-mortgage obligations across member and non-member districts.

Panel B reports delinquency rates on non-mortgage credit for borrowers in our sample, all of whom are delinquent on their mortgages at the time they enter the sample. The panel thus documents incidence rates of non-mortgage delinquency during the two-year period following mortgage delinquency. Starting with auto loans, we find an incidence rate of 2.54% for auto loan

delinquency where the incidence rate is the number of loans that fall into delinquency divided by the total number of months loans remain in our sample until delinquency. The incidence rate is very similar across committee member and non-member districts. The incidence rate of credit card delinquencies is somewhat higher at 3.85% for the entire sample. However, the incidence rates are virtually identical between the member and non-member subgroups. We find similar results for all other non-mortgage loans, which span student debt, retail, and consumer finance, as well as for the aggregate encompassing all non-mortgage loan obligations.

Panel C repeats the analysis of incidence rates but focuses on origination of new credit, as opposed to performance of existing obligations. The data suggest that mortgage delinquency does not necessarily exclude a borrower from participation in credit markets. The incidence rates for new auto loans and credit card accounts stand at 11.4% and 15.0% of borrower-month observations, respectively. The incidence rates of new instances of other non-mortgage debt are higher still at 21.6%. This finding is not surprising, since this category includes student debt, most of which is not underwritten and provided independent of borrower credit standing. The differences in rates between member and non-member districts are mixed: member districts display higher incidence of new auto loans, but lower incidence of new bank credit cards, and almost identical incidence of other non-mortgage debt. However, none of these differences surpasses conventional levels of statistical significance.

The summary statistics suggest that delinquent borrowers in Committee member districts, which were shown to experience foreclosure delays in the earlier analysis, did not experience favorable credit outcomes in non-mortgage categories. We next turn to a more rigorous regression-based hazard analysis to evaluate the validity of these results.

B. Hazard Regressions: Non-mortgage Loan Delinquencies

Table A.VIII presents the results for non-mortgage performance utilizing the hazard regression framework developed in section III.A. In each of the specifications, we estimate the hazard rate of being 90-day delinquent on a given type of existing non-mortgage obligations. Each specification employs a set of loan, geography, and time controls that are identical to those described in section III.A. For all the loan categories except auto loans, credit cards the main hypothesis variable, *Finance Committee member*, is a statistically insignificant factor in the hazard analysis. Moreover, the point estimates of this variable change signs from one category to the next. For auto loans, the coefficient is positive and statistically significant at the 10% level, which indicates that these loans are more likely to become delinquent in committee districts; this is the opposite of what might be expected if banks were delaying foreclosures to increase the welfare. In other words, there is no evidence that the foreclosure delays in committee district allowed delinquent mortgage borrowers in those districts to stay current in non-mortgage loans.

<Insert Table VIII about here>

It is possible that any possible benefits from foreclosure delays may have been concentrated among the most vulnerable borrowers so we repeat our analysis in that subgroup and report our results in Table A.IX, Panel A, only considers borrowers characterized by low (below median) FICO score at the time of sample entry, while Panel B only looks at borrowers who failed to fully document their income when they obtained their mortgage.[37] Both of these groups can be thought of as potentially most vulnerable, and might therefore have a different response to foreclosure delays in committee member districts.

[37] We also estimate a specification on a subsample of borrowers defined by below-median FICO scores at the time of mortgage origination. The results are both qualitatively and quantitatively similar.

In either of these subsamples, we fail to find a beneficial (negative) effect of being in the Committee member district. The only statistically significant results for low-documentation borrowers—that for auto loan delinquency and for all other loans—point in the opposite direction. In other words, there is no evidence that foreclosure delays in committee districts allowed delinquent mortgage borrowers to stay current in non-mortgage loans either in the total sample or in the subsample of most credit-constrained borrowers.

C. Hazard Regressions: Obtaining New Non-mortgage Loans

Turning our attention to the origination of new credit, we present the results of hazard analysis in Table A.X. Once again, we fail to detect a statistically significant beneficial, this time positive, effect of being in a Committee member district. We repeat this analysis for borrower subsamples in the same fashion as was done for non-mortgage delinquencies. The results in Table A.XI also indicate the absence of any measurable effect of being in the Committee member district.

To summarize, the results of the analysis in this section fail to provide support for the hypothesis that foreclosure delays in Committee member districts led to welfare gains for the affected borrowers. In particular, there is no evidence for improved performance on non-mortgage credit or for obtaining new non-mortgage credit. Instead, the results confirm that servicers were motivated by political, not economic, reasons in delaying the start of foreclosures in the districts of Finance Committee members.

D. Potential Economic Channels

Having documented the delay in foreclosure initiations, we attempt to disentangle potential economic mechanisms by exploiting several sources of cross-sectional variation.[38] In

[38] We thank a referee for suggesting these tests.

particular, we consider the role of banking market competitiveness, consumer protection laws, degree of political competitiveness, and costs of foreclosure delays. Heterogeneity in these measures across states may be reflected in potential costs and benefits of foreclosure delays. The results of this investigation are summarized in the Online Appendix Table A.XII.

We start with a hypothesis that more competitive banking markets may motivate servicers to delay foreclosures as they fight to enhance their relative competitive position. To test this, we define the degree of banking market competitiveness by computing MSA-level Herfindahl-Hirschman index values using FDIC Summary of Deposits data for 2008. We then split the sample along the median HHI value and estimate our baseline specification (equation (1) as implemented in regression (3) in Table II) for each of the resulting subsamples. We find that foreclosure delays are somewhat higher and more precisely estimated in less-competitive markets, inconsistent with the null hypothesis.

To further exploit cross-sectional variation in costs and benefits from delaying, we partition the sample between states with judicial foreclosure review processes and states with non-judicial foreclosures. The former group has experienced much lengthier foreclosure timelines, and so in relative terms, a politically-motivated delay is less costly, However, we find statistically significant evidence of politically-motivated delays in both types of states (Table A.XII, columns (3) and (4)). The strength of this effect is somewhat larger in the non-judicial states, although the difference in point estimates is not statistically significant.

We next test the hypothesis that lenders might benefit more from targeted delays in states where existing consumer-level protections are stronger. We identify the presence of anti-predatory consumer protection laws (APL) at the state level using the classification in Ding et al. (2012). The subsample analysis shown in columns (5) and (6) of the Table A.XII suggests

somewhat weaker effects in states with active APL statutes, which is not quite consistent with the notion that servicer actions are more pervasive in states where they are already under tighter scrutiny. Part of the explanation for the apparent lack of cross-sectional variation in foreclosure delays with APL laws may be that all of the servicers in our data have a large national footprint and that the legislation under consideration was national in scope as well.

As a final cross-sectional check, we contrast Congressional districts with varying levels of political competitiveness, as measured by the margin of victory. Using the results of the November 2008 election, only 2% of loans in our sample come from districts in which the margin of victory was less than 2 percentage points. Raising the win threshold to 5% only increases our measure of loans in "competitive districts" to 7% of the original sample. The last two columns of Table A.XII show the results using the 10% victory margin. In this case, 14% of the loans are in less-than-10% districts. However, there is not enough statistical power to identify the political foreclosure delay in a relatively small sample of quasi-competitive districts. We conclude that the extreme skewness of electoral victory margins makes it very difficult to evaluate the hypothesis of greater political benefits for foreclosure delays in more competitive districts.

In sum, the data limitations of our paper make it difficult to take a stand on the mechanisms through which banks link their foreclosure actions and their political concerns. For example, politicians may pressure the banks for delays as reported in the Introduction. Alternatively, banks may agree on the delay voluntarily, as again reported in the Introduction. Our test design and the available data do not allow us to distinguish among the various mechanisms. More importantly, multiple mechanisms may, in fact, result in actions taken simultaneously by different agents in an equilibrium. For example, Kroszner and Stratmann

(1998) provide a theory of congressional committees and interest groups in which interest groups provide campaign contributions for favorable legislation. Crucially, they also show that this equilibrium exists only if the interest groups and the politicians interact repeatedly, which is facilitated by the committee structure in the Congress. The authors also provide empirical evidence from the House Financial Services Committee (then called Banking Committee). In our case, foreclosure delays have a role akin to campaign contributions. As a repeated game equilibrium is likely to break down if the chance of reelection is low for the politicians, we can expect to see *both* of the mechanisms mentioned above. In other words, potential mechanisms may not be competing explanations of how the delay we demonstrate is originated but may, in fact, be complementary and simultaneous aspects of a single equilibrium phenomenon.[39]

VI. Conclusion

This paper is the first to document an effect of political motivations on foreclosure decisions by banks in the aftermath of the financial crisis of 2008. Our results show that mortgage servicing banks delayed the start of foreclosures for delinquent mortgages in the districts of U.S. House Financial Services Committee members. There was no difference in the

[39] Although several mechanisms may be expected to operate simultaneously in equilibrium, data also suggest that some possible explanations are unlikely. For example, the delays in foreclosures are unlikely to be the result of delinquent borrowers having more bargaining power in the committee district. First, the institutional features of foreclosures discussed above make this improbable. Second, if this were the case, we would expect to see many short sales, which are advantageous to delinquent borrowers, *before* the start of the foreclosure process. In our data set of more than 360 thousand loans, there are only two loans that resulted in short sales before any foreclosure action (both of those loans are excluded in our analysis) with the rest of (still infrequent) short sales taking place *after* the foreclosure process is initiated.

onset of delinquencies based on the committee membership, however. This result is robust to controlling for many loan- and zip code-level factors, and time-specific state-level fixed effects. We do not find a similar effect in our placebo tests using the membership in Transportation and Armed Services committees or in previous periods where foreclosures were not a major political issue. Our results are also unlikely to be driven by concerns about sharing the stimulus funds because the House Finance committee did not have jurisdiction on how those funds were allocated.

Our calculations suggest that the cost of delay to financial institutions is an order of magnitude larger than the campaign contributions to the Financial Services committee by the ten largest loan servicers during our sample period. Our calculations further imply that the cost of delay is at par with their lobbying expenses. These estimates are based on the in-sample figures and are thus fairly conservative.

In addition to contributing to the finance literature on mortgages and foreclosures, our work has implications for the political economy literature as well. Most of the political economy literature on political influences focuses on campaign contributions and lobbying, which depend on explicit direct expenditures and which are subject to disclosure requirements. The political delays we document indicate instead an 'in-kind' political activity by the firms through their day-to-day operations. Yet, these delays may be more effective than campaign contributions in some respects because the delays precisely target a politician's constituents. However, some campaign activities like election-time television advertisements, one of the biggest expenditures that campaign contributions finance, are typically broadcast over a geographic area that also includes neighboring districts and, thus, are less precisely targeted. Lobbying expenditures, on the other

hand, are not directly captured by the politicians and cannot be legally directed to election campaigns.

One question might be why the politicians prefer a delay in the foreclosures as indicated by the quotes in the Introduction. After all, they may also have non-delinquent constituents in their district. Although non-delinquent homeowners may have different incentives in general, they may also prefer avoiding large number of foreclosures taking place in their neighborhood due to the local negative externalities of foreclosures (Campbell, Giglio, and Pathak (2011)). In addition, even if non-delinquent borrowers prefer rapid foreclosures, the benefit they obtain from an expeditious process is likely to be diffuse across the non-delinquent owners. However, the benefit of delay in foreclosures will be concentrated on delinquent borrowers and the classic interest group arguments of Olson (1965) suggest that the politicians are likely to favor the delinquent borrowers.

We only focus on the equilibrium outcome instead of equilibrium strategies due to the lack of detailed data. However, the basic premise of the paper only requires that elected officials be aware of foreclosure practices in their districts and that lenders or loan servicers be aware of such interest. Both of these assumptions are quite plausible during that period of financial crisis. Politicians are likely to learn about foreclosures in their districts directly from their constituents, or from a number of non-profit organizations that focus on housing-related issues.[40] For their part, large financial institutions are likely to be aware of publicity surrounding foreclosure activities.

[40] For example, Woodstock Institute publishes its quarterly foreclosure analysis at the *neighborhood* level for the Chicago area, see, for example, Woodstock Institute (2010).

Our paper does not focus and does not provide any evidence on what the banks received or whether they received anything in return for delaying foreclosures. One potential future research area might be to study the returns to banks from their political activities.

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Table I. Sample Statistics

The table gives the sample statistics for our variables. The sample is all the portfolio, GSE, or privately securitized loans that become 90-day delinquent for the first time between January 2009 and December 2010; see text for the details of the sample constructions. *Finance Committee* are the loans that are located in the districts of members of the U.S. House of Representatives Financial Services Committee during the 111th Congress (2009-2010); congressional districts with no zip-code level residential price index data are omitted. See Appendix A for variable descriptions. The difference column gives the difference of means for each variable based on committee membership. Time to the Start of Foreclosures ignores the fact that some loans were still delinquent but not under foreclosure at the end of our sample period (right censoring); hence, the mean time to foreclosure reported in this table is underestimated. Equality of Survivor Functions is a chi-square test for the equality of distributions for the length of time from 90-day delinquency to the start of foreclosures for committee and non-committee districts. Standard errors in parentheses are robust to clustering at the congressional district level except for Panel E for which no option for cluster robust standard errors exists.

Panel A. Number of Loans and Districts

		Finance committee member	Non committee member	Total
Democrat	**Loans:**	38,104	157,736	195,840
		10.31%	42.68%	53.00%
	Districts:	41	203	244
Republican	**Loans:**	25,257	148,444	173,701
		6.83%	40.17%	47.00%
	Districts:	29	142	171
Total	**Loans:**	63,361	306,180	369,541
		17.15%	82.85%	100.00%
	Balance:	$17.14B	$83.74B	$100.88B
		17.00%	83.00%	
	Districts:	70	345	415

Panel B. Zip code-Level Demographic Variables

		Full sample	Finance committee members	Non committee members	Difference
% African American	Mean	11.36	12.85	11.05	1.80
	SD (se)	(10.42)	(9.12)	(10.65)	(1.25)
% Hispanic	Mean	20.95	19.72	21.21	-1.48
	SD (se)	(16.55)	(15.49)	(16.75)	(2.92)
Median household income, thousands	Mean	55.31	59.26	54.49	4.77*
	SD (se)	(13.43)	(15.58)	(12.79)	(2.64)
% Urban Population	Mean	89.17	91.72	88.65	3.08**
	SD (se)	(13.65)	(11.78)	(13.95)	(1.36)
Unemployment Rate	Mean	10.48	10.12	10.55	-4.29
	SD (se)	(2.85)	(2.33)	(2.94)	(4.04)
Decrease in home price index (%)	Mean	33.51	31.61	33.91	-2.29
	SD (se)	(23.30)	(20.80)	(23.77)	(3.83)
Number of Loans	Mean	369,541	63,361	306,180	

Panel C. Borrower-Level Variables

		Full sample	Finance committee members	Non committee members	Difference
FICO score 6 months before delinquency	Mean	626.29	623.03	626.97	-3.94
	SD (se)	(97.47)	(97.12)	(97.53)	(3.21)
Non first mortgage loan amount 6 months before delinquency	Mean	65302.05	63653.58	65643.19	-1989.60
	SD (se)	(85920.07)	(84892.49)	(86127.38)	(3049.19)

Panel D. Loan-level Variables

		Full sample	Finance committee members	Non committee members	Difference
FICO score at origination	Mean	687.1	684.3	687.7	-3.388
	SD (se)	(55.7)	(56.1)	(55.6)	(2.400)
Loan-to-Value (LTV) ratio at origination (%)	Mean	77.927	78.158	77.879	0.28
	SD (se)	(11.413)	(11.648)	(11.362)	(.669)
Estimated Contemporaneous (LTV) ratio (%)	Mean	109.2	106.7	109.8	-3.06
	SD (se)	(43.7)	(26.8)	(46.4)	(3.821)
Loan amount, thousands	Mean	272.9	270.5	273.5	-3.006
	SD (se)	(147.2)	(152.2)	(146.1)	(17.524)
First interest rate reported (%)	Mean	6.481	6.524	6.473	0.051
	SD (se)	(1.251)	(1.243)	(1.252)	(0.043)
Fixed rate flag	Mean	0.688	0.71	0.683	0.027
	SD (se)	(0.463)	(0.454)	(0.465)	(0.022)
Interest only flag	Mean	0.236	0.207	0.242	-0.035
	SD (se)	(0.424)	(0.405)	(0.428)	(0.021)
Jumbo flag	Mean	0.144	0.148	0.143	0.005
	SD (se)	(0.351)	(0.355)	(0.350)	(0.036)
Subprime flag	Mean	0.061	0.065	0.06	0.005
	SD (se)	(0.239)	(0.246)	(0.237)	(0.004)
Refi flag	Mean	0.615	0.614	0.614	0.002
	SD (se)	(0.487)	(0.487)	(0.487)	(0.015)
Full documentation flag	Mean	0.379	0.372	0.381	-0.009
	SD (se)	(0.485)	(0.483)	(0.486)	(0.011)
Documentation unknown flag	Mean	0.372	0.375	0.372	0.003
	SD (se)	(0.483)	(0.484)	(0.483)	(0.005)
Debt-to-Income ratio	Mean	28.017	27.818	28.058	-0.240
	SD (se)	(19.867)	(20.020)	(19.840)	(0.483)
Missing DTI flag	Mean	0.262	0.266	0.261	0.005
	SD (se)	(0.440)	(0.442)	(0.439)	(0.009)
Elapsed Time at First Delinquency	Mean	36.420	36.497	36.405	0.092
	SD (se)	(12.443)	(12.468)	(12.438)	(0.309)
Public Securitized Flag	Mean	0.599	0.603	0.598	0.005
	SD (se)	(0.490)	(0.448)	(0.490)	(0.024)
Private Securitized Flag	Mean	0.284	0.277	0.285	-0.008

	SD (se)	(0.451)	(0.489)	(0.452)	(0.020)
Portfolio Flag	Mean	0.117	0.119	0.117	0.003
	SD (se)	(0.321)	(0.324)	(0.321)	(0.007)

Panel E. Time to the Start of Foreclosures (right censoring ignored)

	Whole sample	**Non-committee**	**Committee**	**Equality of Survival Functions (Chi-sq)**
Mean	10.718	10.690	10.853	16.51
Std. Err./p-value	(0.018)	(0.020)	(0.044)	[<0.0001]
95% Confidence Interval	(10.683-10.754)	(10.651-10.729)	(10.767-10.939)	--
Number of borrowers	369,541	306,180	63,361	--
Number of borrower-months	2,063,579	1,707,690	355,889	--

Table II. U.S. House of Representatives Finance Committee Membership and the Foreclosure of Delinquent Mortgages

The table presents exponential hazard analysis for the start of foreclosure process for 90-day delinquent loans. The sample period covers the 111th Congress (January 2009 through December 2010); the sample includes all the mortgages that become 90-day delinquent for the first time during that period. *Finance Committee Member* is a binary variable that is one if the loan is for a house located in a district whose U.S. House representative is a member of the Financial Services Committee. All the regressions include state*month fixed effects except for the fourth regression, which includes state and month fixed effects without their interaction. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Finance committee member		-0.040***	-0.038***	-0.039***
		(0.015)	(0.013)	(0.013)
Majority Party	-0.013	-0.022*	-0.009	-0.010
	(0.012)	(0.012)	(0.011)	(0.011)
Zip code level variables				
Unemployment rate, by zip	0.010***		0.010***	0.001
	(0.003)		(0.003)	(0.002)
Urban population (%), by zip	-0.068**		-0.068**	-0.103***
	(0.033)		(0.032)	(0.032)
Log(median household income), by zip	-0.054*		-0.040	-0.042
	(0.032)		(0.031)	(0.032)
% black/African American, by zip	-0.001		-0.001	-0.000

	(0.001)		(0.001)	(0.001)
% Hispanic or Latino, by zip	-0.000		-0.000	0.000
	(0.000)		(0.000)	(0.000)
Decrease in home price index since origination	-0.125***		-0.122***	-0.062***
	(0.046)		(0.045)	(0.044)
Borrower-level variables				
FICO 620-680 6 months pre delinquency	0.058***		0.058***	0.059***
	(0.007)		(0.007)	(0.007)
FICO>=680 6 months pre delinquency	0.080***		0.080***	0.082***
	(0.007)		(0.007)	(0.007)
Log(non first mortgage loan amounts 6 months pre delinquency)	0.006***		0.006***	0.007***
	(0.002)		(0.002)	(0.002)
Loan-level controls				
LTV at origination = 80% flag	0.002		0.001	0.004
	(0.006)		(0.006)	(0.006)
LTV Ratio	0.025***		0.025***	0.024***
	(0.001)		(0.001)	(0.001)
LTV Ratio Squared	-0.000***		-0.000***	-0.000***
	(0.000)		(0.000)	(0.000)
LTV Ratio Cubed	0.000***		0.000***	0.000***
	(0.000)		(0.000)	(0.000)
Missing debt to income flag	0.237***		0.237***	0.240***
	(0.018)		(0.018)	(0.018)
Debt to income ratio	-0.000		-0.000	-0.000
	(0.000)		(0.000)	(0.000)
Full documentation flag	-0.102***		-0.102***	-0.102***
	(0.010)		(0.010)	(0.010)

Unknown documentation flag	-0.258*** (0.015)		-0.258*** (0.015)	-0.256*** (0.015)
FICO 620-680 at origination	0.193*** (0.010)		0.193*** (0.010)	0.198*** (0.010)
FICO>= 680 at origination	0.352*** (0.012)		0.352*** (0.012)	0.356*** (0.012)
Log(original loan amount)	-0.131*** (0.012)		-0.132*** (0.012)	-0.139*** (0.012)
Elapsed term at first delinquency	0.007*** (0.000)		0.007*** (0.000)	0.007*** (0.000)
First interest rate reported	-0.062*** (0.003)		-0.062*** (0.003)	-0.063*** (0.004)
Fixed rate flag	-0.156*** (0.010)		-0.156*** (0.010)	-0.153*** (0.010)
Interest only flag	0.127*** (0.008)		0.127*** (0.008)	0.129*** (0.008)
Jumbo flag	0.030** (0.014)		0.031** (0.014)	0.034*** (0.013)
Low grade flag	0.366*** (0.018)		0.366*** (0.018)	0.365*** (0.018)
Refi flag	-0.040*** (0.006)		-0.040*** (0.006)	-0.040*** (0.006)
Public securitized flag	0.296*** (0.012)		0.296*** (0.012)	0.295*** (0.012)
Private securitized flag	0.309*** (0.010)		0.309*** (0.010)	0.306*** (0.010)

Fixed effects Clustering	State*month Congressional District	State*month Congressional District	State*month Congressional District	State, month Congressional District
# of Loan-months	2,063,579	2,063,579	2,063,579	2,063,579
# of Loans	369,541	369,541	369,541	369,541

Table III. Finance Committee Membership and the Time to Foreclosures

The table presents the results of Maximum Likelihood Estimation where the dependent variable is the logarithm of the time length from the onset of 90-day delinquency to the start of foreclosure. Each regression is estimated under a different assumption for the error term distribution and takes right censoring into account. The sample period covers the 111th Congress (January 2009 through December 2010); the sample includes all the mortgages that become 90-day delinquent for the first time during that period. The estimation uses single-observation survival-time data, where each regressor is set to its value in the month of 90-day delinquency. *Finance Committee* is a binary variable that is one if the loan is for a house located in a district whose U.S. House representative is a member of the Financial Services Committee. Predicted Mean (Median) is the average (median) time to the foreclosure starts predicted by the model. Finance Committee Marginal Effect for Mean (Median) is the marginal effect of being in a finance committee district on the predicted mean (median) for the time to the start of foreclosures. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Error Term Distribution	Normal	Logistic	Extreme Value	Extreme Value with a scale parameter
Finance committee	0.037***	0.043***	0.043***	0.044***
	(0.014)	(0.015)	(0.014)	(0.014)
Majority Party	0.010	0.011	0.013	0.013
	(0.010)	(0.011)	(0.010)	(0.011)
Borrower-level controls	Yes	Yes	Yes	Yes

Loan level controls Zip level controls Fixed Effects	Yes Yes State, Month	Yes Yes State, Month	Yes Yes State, Month	Yes Yes State, Month
Predicted mean (months)	18.714*** (0.606)	33.762*** (1.424)	12.122*** (0.247)	12.620*** (0.259)
Finance Committee Marginal Effect on Mean (months)	0.698*** (0.256)	1.482*** (0.518)	0.522*** (0.168)	0.559*** (0.167)
Predicted Median (months)	7.687*** (0.186)	7.744*** (0.187)	8.402*** (0.171)	8.464*** (0.174)
Finance Committee Marginal Effect on Median (months)	0.287*** (0.105)	0.340*** (0.119)	0.362*** (0.117)	0.375*** (0.112)
Clustering level	Congressional district	Congressional district	Congressional district	Congressional district
# of Loans	369,540	369,540	369,540	369,540

Table IV. Finance Committee Membership and Foreclosures: Placebo Tests

The table presents exponential hazard analysis for the start of foreclosure process for the loans that become 90-day delinquent for the first time during the analysis period. *Finance / Defense / Transportation Committee Member* are binary variables that are one if the loan is for a house located in a district whose U.S. House representative is a member of the House Finance / Transportation/ Defense Committee, respectively. All the regressions include state*month fixed effects except for the fourth regression, which includes state and month fixed effects without their interaction. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Defense committee member, 2009-2010	-0.009			
	(0.015)			
Transportation committee member, 2009 - 2010		0.010		
		(0.013)		
Finance committee member, 2007-2008			-0.023	
			(0.017)	
Finance committee member, 2005-2006				0.037
				(0.037)
Majority Party	-0.014	-0.013	0.021	-0.019
	(0.012)	(0.012)	(0.013)	(0.034)
Borrower-level controls	Yes	Yes	Yes	Yes
Loan level controls	Yes	Yes	Yes	Yes
Zip level controls	Yes	Yes	Yes	Yes
State*month FE	Yes	Yes	Yes	No

Sample	90-day delinquent in (Jan-2009, Dec-2010)	90-day delinquent in (Jan-2009, Dec-2010)	90-day delinquent in (Jan-2007, Dec-2008)	90-day delinquent in (Jan-2005, Dec-2006)
Clustering Level	Congressional district	Congressional district	Congressional district	Congressional district
# of Loan-months	2,063,579	2,063,579	499,183	25,089
# of Loans	369,541	369,541	180,004	11,939

Table V. Finance Committee Membership and Mortgage Delinquencies

The table presents exponential hazard analysis for the delinquency of loans that are current at the start of the sample period. The delinquency is defined as 30-day delinquency in regression (1) and 90-day delinquency in regression (2). The sample period covers the 111th Congress (January 2009 through December 2010). *Finance Committee Member* is a binary variable that is one if the loan is for a house located in a district whose U.S. House representative is a member of the House Financial Services Committee in the 111th Congress. All the regressions include state*month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	30-day delinquency	90-day delinquency
Finance committee member	0.003	0.009
	(0.024)	(0.032)
Majority Party	0.022	0.023
	(0.019)	(0.025)
Borrower-level controls	Yes	Yes
Loan level controls	Yes	Yes
Zip level controls	Yes	Yes
State*month FE	Yes	Yes
Sample	Current in Dec-2008	Current in Dec-2008
Clustering level	Congressional district	Congressional district
# of Loan-months	39,597,857	42,231,510
# of Loans	2,319,364	2,322,181

Table VI. Finance Committee Membership and Foreclosures:

Senior Committee Members Only

The table presents exponential hazard analysis for the start of foreclosure process for 90-day delinquent loans. The sample period covers the 111th Congress (January 2009 through December 2010); the sample includes all the mortgages that become 90-day delinquent for the first time during that period. *Finance Committee Member* is a binary variable that is one if the loan is for a house located in a district whose U.S. House representative is a member of the Financial Services Committee. The first regression excludes committee members that were appointed to the committee in 2005 or after. The second regression excludes the politicians who took office in the U.S. House of Representatives for the first time in 2005 or after. All the regressions include state*month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
Finance committee member	-0.044***	-0.054***
	(0.016)	(0.016)
Majority Party	-0.013	-0.035**
	(0.012)	(0.014)
Borrower-level controls	Yes	Yes
Loan level controls	Yes	Yes
Zip level controls	Yes	Yes
State*month FE	Yes	Yes
Sample	Exclude first-time committee members as of 2005 or after	Exclude first-time House members as of 2005 or after
Clustering level	Congressional	Congressional

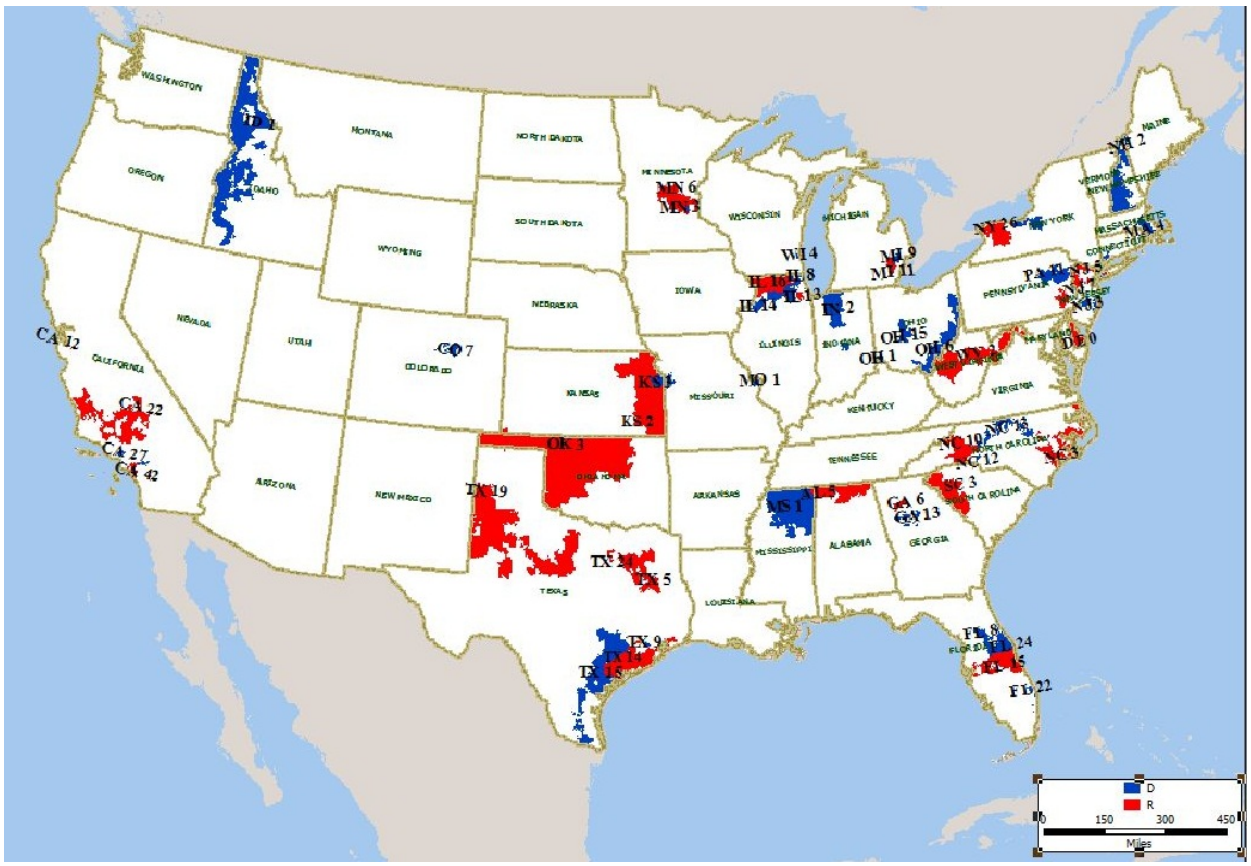
	district	district
# of Loan-months	1,878,804	1,373,386
# of Loans	334,298	241,368

Table VII. Finance Committee Membership and Foreclosures: Alternative Econometric Models

Columns (1) and (2) present exponential hazard analysis for the start of foreclosure process for 90-day delinquent loans using sample periods from January 2009 to December 2009 and to March 2010, respectively. The third and fourth columns provide hazard analysis for the start of foreclosure process for 90-day delinquent loans using the Cox proportional hazard and Weibull models. The fifth column presents the discrete hazard analysis with the linear probability model for the start of foreclosures of 90-day delinquent loans; the dependent variable is one if the loan is foreclosed that month conditional on not being in foreclosure before. Columns (6), (7), (8) represent the committee and non-committee zip codes that are within 10, 25, and 50 miles of one another. The sample period covers the 111th Congress (January 2009 through December 2010), except for columns (1) and (2). *Finance Committee Member* is a binary variable that is one if the loan is for a house located in a district whose U.S. House representative is a member of the House Financial Services Committee in the 111th Congress. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Exponential Hazard	Exponential Hazard	Cox Proportional Hazard	Weibull Hazard	Discrete Hazard, Linear Probability Model	Exponential Hazard	Exponential Hazard	Exponential Hazard
Finance committee member	-0.035**	-0.042**	-0.037***	-0.038***	-0.003***	-0.046**	-0.033**	-0.032**
	(0.014)	(0.013)	(0.012)	(0.013)	(0.001)	(0.019)	(0.016)	(0.016)
Majority Party	-0.005	-0.004	-0.009	-0.009	-0.001	-0.020	0.004	-0.011
	(0.012)	(0.012)	(0.010)	(0.011)	(0.001)	(0.025)	(0.020)	(0.018)
Borrower-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*month FE	State*month	State*month	State*month	State*month	State*month	State*month	State*month	State*month
Sample	Jan. 2009 to Dec. 2009	Jan. 2009 to Mar. 2010	Full	Full	Full	Committee and non-committee zip codes within 10 miles	Committee and non-committee zip codes within 25 miles	Committee and non-committee zip codes within 50 miles
Clustering Level	Congressional district	Congressional district	Congressional district	Congressional district	Congressional district	Congressional district	Congressional district	Congressional district
# of Loan-months	745,441	1,118,307	2,063,579	2,063,579	2,063,579	462,298	799,549	968,799
# of Loans	213,795	268,184	369,541	369,541	369,541	79,118	137,011	166,214

Figure 1. Districts of the Members of the Financial Services Committee in U.S. House of Representatives during 111th Congress (2009 to 2010)



Appendix A. Variable Description and Source

Variable	Description	Source
Political Variables		
Majority Party	Binary variable equal to one if a member of the majority party represents the district in which the mortgaged home is located during the 111th Congress	Stewart and Woon
Finance committee member	Binary variable equal to one if a member of the Financial Services Committee represents the district in which the mortgaged home is located during the 111th Congress	Stewart and Woon
Demographic Variables		
Unemployment rate	ZCTA-level monthly unemployment rate	BLS
% Urban population	% of population living in urban areas based on 2000 census. Original data is at the county level and aggregated to the zip code level using Mable/Geocorr county-to-zip crosswalk population-based allocation factors.	Census 2000 SF 1
Log median household income	Natural log of annual median household income on annual basis at the congressional district level	ACS
% black/African American	% of population reported black or African American on an annual basis. Original data is at the county level and aggregated to the zip code level using Mable/Geocorr county-to-zip crosswalk population-based allocation factors.	ACS
% Hispanic or latino	% of population reported Hispanic or Latino. Original data is at the county level and aggregated to the zip code level using Mable/Geocorr county-to-zip crosswalk population-based allocation factors.	ACS
Decrease in home price index since origination	Negative change in log zip code-level home price index between month of origination and current month	Corelogic Home Price Index

Loan-Level Variables		
FICO 620-680 at origination	Binary variable equal to one if borrower FICO score at origination greater than or equal to 620 and less than 680	Mortgage servicer dataset
FICO >= 680 at origination	Binary variable equal to one if borrower FICO score at origination greater than or equal to 680	Mortgage servicer dataset
LTV at origination = 80% flag	Binary variable equal to one if the loan-to-value ratio at origination is 80%	Mortgage servicer dataset
Missing dti flag	Binary variable equal to one if the debt-to-income at origination is not reported	Mortgage servicer dataset
DTI ratio	Debt-to-income ratio at origination	Mortgage servicer dataset
Estimated current LTV	Ratio of the principal remaining to the estimated current home value, derived from the appraisal value at origination and the ratio of the current home price index to the home price index at origination	Mortgage servicer dataset
Log loan amount	Natural log of the original loan amount in dollars	Mortgage servicer dataset
First interest rate reported	Earliest current interest rate reported in LPS dynamic data	Mortgage servicer dataset
Fixed rate flag	Binary variable equal to one if principal and interest are constant at loan origination	Mortgage servicer dataset
Interest only flag	Binary variable equal to one if loan is interest only at origination	Mortgage servicer dataset
Jumbo flag	Binary variable equal to one if loan is jumbo at origination	Mortgage servicer dataset
Low grade flag	Binary variable equal to one if loan is grade "B" or "C" at origination	Mortgage servicer dataset
Refi flag	Binary variable equal to one if loan purpose is refinance at origination	Mortgage servicer dataset

Full documentation flag	Binary variable equal to one if full documentation was presented at loan origination	Mortgage servicer dataset
Unknown documentation flag	Binary variable equal to one if documentation type at loan origination classified as unknown or other	Mortgage servicer dataset
Elapsed term at first delinquency	Months between loan origination and first 90-day delinquency	Mortgage servicer dataset
Public securitized flag	Binary variable equal to one if loan is securitized by Fannie Mae or Freddie Mac in the month of first 90-day delinquency	Mortgage servicer dataset
Private securitized flag	Binary variable equal to one if loan is securitized by a private investor in the month of first 90-day delinquency	Mortgage servicer dataset

Appendix B. Key Dates in the Passage of the Financial Reform Act

June 17, 2009: Obama Administration releases its financial reform proposal

July–December 2009: Discussions and hearings in the Financial Services Committee on the proposal.

December 11, 2009: U.S. House of Representatives passes the final version.

January–May 2010: Senate considers the financial reform proposal.

May 20, 2010: Senate passes its own version and the bill moves to the conference committee.

June 29, 2010: The unified bill leaves the conference committee.

June 30, 2010: House passes the bill.

July 15, 2010: Senate passes the bill.

July 21, 2010: President signs the bill and it becomes law.

ONLINE APPENDIX

Table AI. Finance Committee Membership and Foreclosures: Robustness in Subsamples

The table presents exponential hazard analysis for the start of foreclosure proceedings for 90-day delinquent loans. The sample period covers the 111th Congress (January 2009 through December 2010); the sample includes all the mortgages that become 90-day delinquent for the first time during that period. *Finance Committee Member* is a binary variable that is one if the loan is for a house located in a district whose U.S. House representative is a member of the Financial Services Committee. All the regressions include state*month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
Finance committee member	-0.037***	-0.037***	-0.036***
	(0.013)	(0.013)	(0.013)
Majority Party	-0.008	-0.010	-0.009
	(0.011)	(0.011)	(0.012)
Borrower-level controls	Yes	Yes	Yes
Loan level controls	Yes	Yes	Yes
Zip level controls	Yes	Yes	Yes
State*month FE	Yes	Yes	Yes
Sample	Exclude subprime	Loan term 15,20,30 years	Excludes Oct-Dec 2010
Clustering level	Congressional District	Congressional District	Congressional District
# of Loan-months	1,964,577	2,045,095	1,798,311
# of Loans	347,129	366,707	338,579

Table AII. U.S. House of Representatives Finance Committee Membership and the Foreclosure of Delinquent Mortgages (Counterpart of Table II with Time to Foreclosure)

The table presents the results of Maximum Likelihood Estimation where the dependent variable is the logarithm of the time length from the onset of 90-day delinquency to the start of foreclosure. The assumed distribution for the error term is extreme value distribution, which generates exponential hazard. The sample period covers the 111th Congress (January 2009 through December 2010); the sample includes all the mortgages that become 90-day delinquent for the first time during that period. The table uses single-observation survival-time data, where each regressor is set to its value in the month of 90-day delinquency. *Finance Committee Member* is a binary variable that is one if the loan is for a house located in a district whose U.S. House representative is a member of the Financial Services Committee. All the regressions include state*delinquency month fixed effects except for the fourth regression, which includes state and delinquency month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Finance committee member		0.044***	0.044***	0.043***
		(0.015)	(0.014)	(0.014)
Majority Party	0.016	0.020	0.012	0.013
	(0.011)	(0.013)	(0.011)	(0.010)
Borrower-level controls	Yes	No	Yes	Yes
Loan level controls	Yes	No	Yes	Yes
Zip level controls	Yes	No	Yes	Yes
Fixed Effects	State*month	State*month	State*month	State, month
Sample	90-day delinquent in (Jan-2009, Dec-2010)	90-day delinquent in (Jan-2009, Dec-2010)	90-day delinquent in (Jan-2009, Dec-2010)	90-day delinquent in (Jan-2009, Dec-2010)
Clustering level	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	369,541	369,541	369,541	369,541
# of Loans	369,541	369,541	369,541	369,541

Table AIII. Finance Committee Membership and Foreclosures: Placebo Tests

(Counterpart of Table IV with Time to Foreclosure)

The table presents the results of Maximum Likelihood Estimation where the dependent variable is the logarithm of the time length from the onset of 90-day delinquency to the start of foreclosure. The assumed distribution for the error term is extreme value distribution, which generates exponential hazard. The table uses single-observation survival-time data, where each regressor is set to its value in the month of 90-day delinquency. *Finance / Defense / Transportation Committee Member* are binary variables that are one if the loan is located in a district of a member of the House Finance / Transportation/ Defense Committee, respectively. All the regressions include state*delinquency month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Defense committee member, 2009-2010	0.010			
	(0.015)			
Transportation committee member, 2009 - 2010		-0.016		
		(0.013)		
Finance committee member, 2007-2008			0.027	
			(0.019)	
Finance committee member, 2005-2006				-0.020
				(0.032)
Majority Party	0.018	0.017	-0.019	0.006
	(0.011)	(0.011)	(0.014)	(0.032)
Borrower-level controls	Yes	Yes	Yes	Yes
Loan level controls	Yes	Yes	Yes	Yes
Zip level controls	Yes	Yes	Yes	Yes
State*month FE	Yes	Yes	Yes	Yes
Sample	90-day delinquent in (Jan-2009, Dec-2010)	90-day delinquent in (Jan-2009, Dec-2010)	90-day delinquent in (Jan-2007, Dec-2008)	90-day delinquent in (Jan-2005, Dec-2006)
Clustering Level	Congressional district	Congressional district	Congressional district	Congressional district
# of Loan-months	369,541	369,541	180,004	11,929
# of Loans	369,541	369,541	180,004	11,929

Table AIV. Finance Committee Membership and Mortgage Delinquencies (Counterpart of Table V with Time to Delinquency)

The table presents the results of Maximum Likelihood Estimation where the dependent variable is the logarithm of the time length from December 2008 to the onset of delinquency, for all loans that are current at the start of the sample period. The assumed distribution for the error term is extreme value distribution, which generates exponential hazard. The delinquency is defined as 30-day delinquency in regression (1) and 90-day delinquency in regression (2). The sample period covers the 111th Congress (January 2009 through December 2010). The table uses single-observation survival-time data, where each regressor is set to its value in December 2008. *Finance Committee Member* is a binary variable that is one if the loan is for a house located in a district whose U.S. House representative is a member of the House Financial Services Committee in the 111th Congress. All the regressions include state*origination month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	30-day delinquency	90-day delinquency
Finance committee member	-0.010	-0.066
	(0.026)	(0.0235)
Majority Party	-0.025	-0.136
	(0.021)	(0.180)
Borrower-level controls	Yes	Yes
Loan level controls	Yes	Yes
Zip level controls	Yes	Yes
State*month FE	Yes	Yes
Sample	Current in Dec-2008	Current in Dec-2008
Clustering level	Congressional district	Congressional district
# of Loan-months	2,108,212	2,108,212
# of Loans	2,108,212	2,108,212

Table AV. Finance Committee Membership and Foreclosures: Senior Committee Members Only (Counterpart of Table VI with Time to Foreclosure)

The table presents the results of Maximum Likelihood Estimation where the dependent variable is the logarithm of the time length from the onset of 90-day delinquency to the start of foreclosure. The assumed distribution for the error term is extreme value distribution, which generates exponential hazard. The sample period covers the 111th Congress (January 2009 through December 2010); the sample includes all the mortgages that become 90-day delinquent for the first time during that period. The table uses single-observation survival-time data, where each regressor is set to its value in the month of 90-day delinquency. *Finance Committee Member* is a binary variable that is one if the loan is for a house located in a district whose U.S. House representative is a member of the Financial Services Committee. The first regression excludes committee members that were appointed to the committee in 2005 or after. The second regression excludes the politicians who took office in the U.S. House of Representatives for the first time in 2005 or after. All the regressions include state and delinquency month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
Finance committee member	0.042**	0.055***
	(0.017)	(0.016)
Majority Party	0.018	0.041***
	(0.011)	(0.013)
Borrower-level controls	Yes	Yes
Loan level controls	Yes	Yes
Zip level controls	Yes	Yes
State*month FE	No	No

Sample	Exclude first-time committee members as of 2005 or after	Exclude first-time House members as of 2005 or after
Clustering level	Congressional district	Congressional district
# of Loan-months	334,298	241,368
# of Loans	334,298	241,368

Table AVI. Finance Committee Membership and Foreclosures: Alternative Econometric Models (Counterpart of Table VII with Time to Foreclosure)

Columns (1) and (2) present the results of Maximum Likelihood Estimation where the dependent variable is the logarithm of the time length from the onset of 90-day delinquency to the start of foreclosure, using sample periods from January 2009 to December 2009 and to March 2010, respectively. Columns (5), (6), (7) represent the committee and non-committee zip codes that are within 10, 25, and 50 miles of one another. The sample period covers the 111th Congress (January 2009 through December 2010), except for columns (1) and (2). The table uses single-observation survival-time data, where each regressor is set to its value in the month of 90-day delinquency. *Finance Committee Member* is a binary variable that is one if the loan is for a house located in a district whose U.S. House representative is affiliated with the majority party in the U.S. House. All the regressions include state*delinquency month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Error Term Distribution	Extreme Value	Extreme Value	Extreme Value with Scale Parameter	Extreme Value	Extreme Value	Extreme Value
Implied Hazard	Exponential	Exponential	Weibull	Exponential	Exponential	Exponential
Finance committee member	0.037***	0.046***	0.045***	0.058***	0.041**	0.037**
	(0.014)	(0.014)	(0.014)	(0.018)	(0.016)	(0.016)
Majority Party	0.008	0.009	0.012	0.006	-0.008	0.013
	(0.012)	(0.012)	(0.011)	(0.025)	(0.019)	(0.017)
Borrower-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan level controls	Yes	Yes	Yes	Yes	Yes	Yes
Zip level controls	Yes	Yes	Yes	Yes	Yes	Yes
State*month FE	State*month	State*month	State*month	State*month	State*month	State*month
Sample	Jan. 2009 to Dec. 2009	Jan. 2009 to Mar. 2010	Full	Committee and non-committee zip codes within 10 miles	Committee and non-committee zip codes within 25 miles	Committee and non-committee zip codes within 50 miles
Clustering Level	Congressional district	Congressional district	Congressional district	Congressional district	Congressional district	Congressional district
# of Loan-months	213,795	268,184	369,541	79,118	137,011	166,214
# of Loans	213,795	268,184	369,541	79,118	137,011	166,214

Table AVII. Selected Statistics for Non-mortgage loans

The table provides the sample statistics and incidence rates of non-mortgage borrowings of the borrowers whose mortgage became 90-day delinquent between January 2009 and December 2010. Panel A. provides the share of total non-mortgage borrowing that a given subcomponent of borrowing represents as of the delinquency date. Panel B. and C. provide the incidence rates of 'failure' for the exponential hazard analysis of non-mortgage borrowings. 'Failure' is defined as the delinquency of the nonmortgage borrowing in Panel B. and as the appearance of a new non-mortgage loan in Panel C. The data on household non-mortgage liabilities are from the Equifax CRISM dataset.

Panel A. Share of Non-mortgage Borrowing by Type of Loan

		Full Sample	Finance committee members	Non-committee members	Difference
Auto loans share of non-mortgage balance	**Mean**	28.57%	28.05%	28.68%	-0.63%
	SD (se)	(32.81)	(32.63)	(32.85)	(0.65)
	n	358,827	61,514	297,313	
Bank card share of non-mortgage balance	**Mean**	45.96%	45.64%	46.02%	-0.38%
	SD (se)	(35.65)	(35.58)	(35.66)	(0.89)
	n	358,827	61,514	297,313	
All others (incl. student loans, retail loans, consumer finance loans)	**Mean**	25.47%	26.31%	25.30%	1.01%
	SD (se)	(30.58)	(30.90)	(30.51)	(0.62)
	n	358,827	61,514	297,313	
Ratio of non-mortgage debt to total mortgage balance outstanding	**Mean**	16.48%	16.73%	16.42%	0.31%
	SD (se)	(32.01)	(21.90)	(33.73)	(0.92)
	n	369,541	63,361	306,180	

Panel B. Incidence Rates for Delinquency of a Non-mortgage Loan

Auto loans incidence rate	Whole sample	Non-committee	Committee	=Committee - nonCommittee
Incidence Rate	2.54%	2.52%	2.64%	0.12%
Incidence rate Std.Err. (%)	(0.05)	(0.06)	(0.10)	(0.11)
Number of 'failures'	32,392	26,698	5,694	
Number of borrowers	177,858	147,614	30,244	
Number of borrower-months	1,274,449	1,058,721	215,728	

Bankcard loans incidence rate	Whole sample	Non-committee	Committee	=Committee - nonCommittee
Incidence Rate	3.85%	3.86%	3.85%	-0.01%
Incidence rate Std.Err. (%)	(0.05)	(0.05)	(0.11)	(0.12)
Number of 'failures'	57,217	47,514	9,703	
Number of borrowers	218,454	181,571	36,883	
Number of borrower-months	1,484,436	1,232,292	252,144	

All other loans (student, retail, consumer finance, other)	Whole sample	Non-committee	Committee	=Committee - nonCommittee
Incidence Rate	3.45%	3.45%	3.47%	0.02%
Incidence rate Std.Err. (%)	(0.04)	(0.05)	(0.10)	(0.11)
Number of 'failures'	53,438	44,137	9,301	
Number of borrowers	234,914	194,482	40,432	
Number of borrower-months	1,548,859	1,280,608	268,251	

Non-mortgage loans	Whole sample	Non-committee	Committee	=Committee - nonCommittee
Incidence Rate	5.63%	5.63%	5.68%	0.05%
Incidence rate Std.Err. (%)	(0.07)	(0.08)	(0.16)	(0.18)
Number of 'failures'	116,884	96,669	20,215	
Number of borrowers	324,456	268,955	55,501	
Number of borrower-months	2,074,414	1,718,485	355,929	

Panel C. Incidence Rates for the Appearance of a New Non-mortgage Loan

Auto loans incidence rate	Whole sample	Non-committee	Committee	=Committee - nonCommittee
Incidence Rate	11.39%	11.35%	11.60%	0.25%
Incidence rate Std.Err. (%)	(0.09)	(0.09)	(0.24)	(0.26)
Number of 'failures'	70,312	58,229	12,083	
Number of borrowers	129,936	107,836	22,100	
Number of borrower-months	617,200	513,005	104,195	

Bankcard loans incidence rate	Whole sample	Non-committee	Committee	=Committee - nonCommittee
Incidence Rate	15.04%	15.08%	14.86%	-0.21%
Incidence rate Std.Err. (%)	(0.09)	(0.10)	(0.26)	(0.28)
Number of 'failures'	102,623	85,188	17,435	
Number of borrowers	172,836	143,245	29,591	
Number of borrower-months	682,401	565,085	117,316	

All other loans (student, retail, consumer finance, other)	Whole sample	Non-committee	Committee	=Committee - nonCommittee
Incidence Rate	21.60%	21.61%	21.54%	-0.07%
Incidence rate Std.Err. (%)	(0.08)	(0.09)	(0.21)	(0.23)
Number of 'failures'	125,422	103,548	21,874	
Number of borrowers	173,222	143,008	30,214	
Number of borrower-months	580,671	479,142	101,529	

Non-mortgage loans	Whole sample	Non-committee	Committee	=Committee - nonCommittee
Incidence Rate	24.22%	24.23%	24.17%	-0.06%
Incidence rate Std.Err. (%)	(0.11)	(0.12)	(0.28)	(0.31)
Number of 'failures'	214,338	177,564	36,774	
Number of borrowers	281,609	233,276	48,333	
Number of borrower-months	884,942	732,778	152,164	

Table AVIII. Financial Committee Membership and Non-mortgage Delinquencies

The table presents exponential hazard analysis for the 90-day delinquency of non-mortgage loans of the borrowers whose mortgages became delinquent between January 2009 and December 2010. *Finance Committee Member* is a binary variable that is one if the loan is associated with an individual with a house located in a district whose U.S. House representative is a member of the House Financial Services Committee. The control variables include those in Table II as well as a 6-month lag of the logged balance of the loan in question and the logged number of loans for the type in question. All the regressions include state*month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. The data on household non-mortgage liabilities are from the Equifax CRISM dataset.

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	0.0338*	0.0011	0.0046	-0.0035
	(0.0201)	(0.0127)	(0.0110)	(0.0091)
Majority Party	-0.0533***	-0.0096	-0.0117	-0.0241**
	(0.0193)	(0.0118)	(0.0109)	(0.0097)
Borrower-level controls	Yes	Yes	Yes	Yes
Loan-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District

# of Loan-months	1,274,449	1,484,436	1,548,859	2,074,414
# of Loans	177,858	218,454	234,914	324,456

Table AIX. Financial Committee Membership and Non-mortgage Delinquencies in Subsamples

The table presents exponential hazard analysis for the 90-day delinquency of non-mortgage loans of the borrowers whose mortgages became delinquent between January 2009 and December 2010. The sample for panel (A) is loans associated with borrowers with a FICO score below the median at the time of first 90-day mortgage delinquency. The sample for panel (B) is loans associated with borrowers with low- or no documentation for their mortgage. *Finance Committee Member* is a binary variable that is one if the loan is associated with an individual with a house located in a district whose U.S. House representative is a member of the House Financial Services Committee. The control variables include those in Table II as well as a 6-month lag of the logged balance of the loan in question and the logged number of loans for the type in question. All the regressions include state*month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. The data on household non-mortgage liabilities are from the Equifax CRISM dataset.

Panel A. FICO score below median at the time of first 90-day mortgage delinquency

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	0.0313	-0.0012	0.0036	-0.0108
	(0.0219)	(0.0198)	(0.0152)	(0.0113)
Majority Party	-0.0356*	-0.0002	-0.0104	-0.0083
	(0.0197)	(0.0166)	(0.0137)	(0.0107)
Borrower-level controls	Yes	Yes	Yes	Yes

Loan-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	532,854	319,042	607,042	739,084
# of Loans	79,398	57,031	96,521	137,020

Panel B. Mortgages with Low Documentation or Documentation Information Missing

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	0.0459**	0.0073	0.0238*	0.0049
	(0.0221)	(0.0148)	(0.0142)	(0.0125)
Majority Party	-0.0410*	-0.0098	-0.0077	-0.0248**
	(0.0216)	(0.0134)	(0.0132)	(0.0116)
Borrower-level controls	Yes	Yes	Yes	Yes
Loan-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	793,324	940,988	966,040	1,307,336
# of Loans	109,823	136,798	145,515	201,311

Table AX. Financial Committee Membership and New Non-mortgage Loans

The table presents exponential hazard analysis of obtaining a new non-mortgage loan by the borrowers whose mortgage became delinquent between January 2009 and December 2010. *Finance Committee Member* is a binary variable that is one if the loan is associated with an individual with a house located in a district whose U.S. House representative is a member of the House Financial Services Committee. The control variables include those in Table II as well as a 6-month lag of the logged balance of the loan in question and the logged number of loans for the type in question. All the regressions include state*month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. The data on household non-mortgage liabilities are from the Equifax CRISM dataset.

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	-0.0025	-0.0143	0.0061	0.0014
	(0.0111)	(0.0096)	(0.0089)	(0.0088)
Majority Party	0.0182	-0.0015	-0.0078	0.0053
	(0.0121)	(0.0100)	(0.0068)	(0.0085)
Borrower-level controls	Yes	Yes	Yes	Yes
Loan-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	617,200	682,401	580,671	884,942
# of Loans	129,936	172,836	173,222	281,609

Table AXI. Financial Committee Membership and New Non-mortgage Loans in Subsamples

The table presents exponential hazard analysis of obtaining a new non-mortgage loan by the borrowers whose mortgage became delinquent between January 2009 and December 2010. The sample for panel (A) is loans associated with borrowers with a FICO score below the median at the time of first 90-day mortgage delinquency. The sample for panel (B) is loans associated with borrowers with low- or no documentation for their mortgage. *Finance Committee Member* is a binary variable that is one if the loan is associated with an individual with a house located in a district whose U.S. House representative is a member of the House Financial Services Committee. The control variables include those in Table II as well as a 6-month lag of the logged balance of the loan in question and the logged number of loans for the type in question. All the regressions include state*month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. The data on household non-mortgage liabilities are from the Equifax CRISM dataset.

Panel A. FICO score below median at the time of first 90-day mortgage delinquency

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	-0.0073	-0.0153	-0.0063	-0.0076
	(0.0162)	(0.0150)	(0.0109)	(0.0113)
Majority Party	0.0129	0.0138	-0.0091	0.0184*
	(0.0165)	(0.0142)	(0.0090)	(0.0111)

Borrower-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	310,139	252,590	302,344	399,638
# of Loans	64,471	71,496	87,603	128,739

Panel B. Mortgages with Low Documentation or Documentation Information Missing

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	-0.0033	-0.0122	-0.0011	-0.0049
	(0.0144)	(0.0113)	(0.0093)	(0.0091)
Majority Party	0.0060	-0.0073	-0.0086	-0.0034
	(0.0136)	(0.0114)	(0.0083)	(0.0093)
Borrower-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	383,625	432,669	357,463	556,593
# of Loans	80,425	108,118	106,424	174,829

Table AXII. Analysis of Economic Channels through Subsamples

The table presents exponential hazard analysis for the start of foreclosure process for 90-day delinquent loans carried out on subsamples of the data. The sample period and variable definitions are the same as in Table II. The Herfindahl-Hirschman index (HHI) values are computed at the MSA level using FDIC Summary of Deposits data for 2008. Judicial/non-judicial state designations are from Ghent and Kudlyak (2011). States with anti-predatory laws (APL) are identified as in Ding et al. (2012). Victory margin in a Congressional District is based on the November 2008 results. All the regressions include state*month fixed effects except for the fourth regression, which includes state and month fixed effects without their interaction. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels

	(1) Above Median HHI	(2) Below Median HHI	(3) Judicial States	(4) Non-Judicial States	(5) APL in effect	(6) APL not in effect	(7) Victory Margin Above 10%	(8) Victory Margin Below 10%
Finance committee member	-0.040**	-0.024	-0.034**	-0.050***	-0.032*	-0.050***	-0.032**	-0.067
	(0.020)	(0.023)	(0.016)	(0.019)	(0.017)	(0.019)	(0.015)	(0.065)
Majority Party	-0.005	-0.014	-0.004	-0.008	-0.010	-0.004	-0.014	-0.009
	(0.017)	(0.015)	(0.017)	(0.014)	(0.017)	(0.014)	(0.012)	(0.057)
Borrower-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Sample	Exclude first-time committee members as of 2005 or after	Exclude first-time House members as of 2005 or after	Exclude first-time House members as of 2005 or after	Exclude first-time House members as of 2005 or after	Exclude first-time House members as of 2005 or after	Exclude first-time House members as of 2005 or after	Exclude first-time House members as of 2005 or after	Exclude first-time House members as of 2005 or after
Clustering level	Congressional district	Congressional district	Congressional district	Congressional district	Congressional district	Congressional district	Congressional district	Congressional district
# of Loan-months	909,901	932,330	700,308	1,363,271	1,270,814	792,765	1,769,345	294,234
# of Loans	165,465	165,901	131,635	237,906	215,379	154,162	315,980	53,561

Table AXII. Financial Committee Membership and Non-mortgage Delinquencies

(Counterpart of Table AVIII with Time to Non-mortgage Delinquency)

The table presents the results of Maximum Likelihood Estimation where the dependent variable is the logarithm of the time length from the onset of 90-day mortgage delinquency to the start of 90-day delinquency of non-mortgage loans. The assumed distribution for the error term is extreme value distribution, which generates exponential hazard. The sample covers borrowers whose mortgages became delinquent between January 2009 and December 2010. The table uses single-observation survival-time data, where each regressor is set to its value in the month of 90-day delinquency. *Finance Committee Member* is a binary variable that is one if the loan is associated with an individual with a house located in a district whose U.S. House representative is a member of the House Financial Services Committee. The control variables include those in Table II as well as a 6-month lag of the logged balance of the loan in question and the logged number of loans for the type in question. All the regressions include state*delinquency month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	-0.0337	0.0040	0.0020	0.0039
	(0.0215)	(0.0135)	(0.0112)	(0.0095)
Majority Party	0.0588***	0.0132	0.0144	0.0286***
	(0.0195)	(0.0125)	(0.0112)	(0.0103)
Borrower-level controls	Yes	Yes	Yes	Yes
Loan-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	170,662	213,515	215,949	324,302
# of Loans	170,662	213,515	215,949	324,302

Table AXIII. Financial Committee Membership and New Non-mortgage Loans

(Counterpart of Table AIX with Time to New Non-mortgage Loan)

The table presents the results of Maximum Likelihood Estimation where the dependent variable is the logarithm of the time length from the onset of 90-day mortgage delinquency to the month when the borrower obtains a new non-mortgage loan. The assumed distribution for the error term is extreme value distribution, which generates exponential hazard. The sample covers borrowers whose mortgage became delinquent between January 2009 and December 2010. The table uses single-observation survival-time data, where each regressor is set to its value in the month of 90-day delinquency. *Finance Committee Member* is a binary variable that is one if the loan is associated with an individual with a house located in a district whose U.S. House representative is a member of the House Financial Services Committee. The control variables include those in Table II as well as a 6-month lag of the logged balance of the loan in question and the logged number of loans for the type in question. All the regressions include state*delinquency month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	0.0136	0.0126	-0.0009	0.0024
	(0.0122)	(0.0101)	(0.0088)	(0.0077)
Majority Party	-0.0147	0.0043	0.0042	-0.0046
	(0.0113)	(0.0099)	(0.0067)	(0.0077)
Borrower-level controls	Yes	Yes	Yes	Yes
Loan-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	127,684	170,816	167,995	276,054
# of Loans	127,684	170,816	167,995	276,054

Table AXIV. Financial Committee Membership and Non-Mortgage Delinquencies in Subsamples (Counterpart of Table AX with Time to Non-Mortgage Delinquency)

The table presents the results of Maximum Likelihood Estimation where the dependent variable is the logarithm of the time length from the onset of 90-day mortgage delinquency to the start of 90-day delinquency of non-mortgage loans. The assumed distribution for the error term is extreme value distribution, which generates exponential hazard. The sample covers borrowers whose mortgage became delinquent between January 2009 and December 2010. The sample for panel (A) is loans associated with borrowers with a FICO score below the median at the time of first 90-day mortgage delinquency. The sample for panel (B) is loans associated with borrowers with low- or no documentation for their mortgage. The table uses single-observation survival-time data, where each regressor is set to its value in the month of 90-day delinquency. *Finance Committee Member* is a binary variable that is one if the loan is associated with an individual with a house located in a district whose U.S. House representative is a member of the House Financial Services Committee. The control variables include those in Table II as well as a 6-month lag of the logged balance of the loan in question and the logged number of loans for the type in question. All the regressions include state*delinquency month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. FICO score below median at the time of first 90-day mortgage delinquency

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	-0.0256	0.0248	0.0084	0.0149
	(0.0230)	(0.0211)	(0.0156)	(0.0119)
Majority Party	0.0373*	-0.0033	0.0116	0.0120
	(0.0200)	(0.0175)	(0.0142)	(0.0114)
Borrower-level controls	Yes	Yes	Yes	Yes
Loan-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	77,232	56,371	92,690	136,930
# of Loans	77,232	56,371	92,690	136,930

Panel B. Mortgages with Low Documentation or Documentation Information Missing

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	-0.0397**	-0.0045	-0.0188	-0.0050
	(0.0234)	(0.0155)	(0.0144)	(0.0132)
Majority Party	0.0455**	0.0111	0.0045	0.0281**
	(0.0218)	(0.0141)	(0.0135)	(0.0123)
Borrower-level controls	Yes	Yes	Yes	Yes
Loan-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	105,500	133,594	133,105	201,221
of Loans	105,500	133,594	133,105	201,221

Table AXV. Financial Committee Membership and New Non-Mortgage Loans in Subsamples (Counterpart of Table AXI with Time to New Non-Mortgage Loan)

The table presents the results of Maximum Likelihood Estimation where the dependent variable is the logarithm of the time length from the onset of 90-day mortgage delinquency to the month when the borrower obtains a new non-mortgage loan. The assumed distribution for the error term is extreme value distribution, which generates exponential hazard. The sample covers borrowers whose mortgage became delinquent between January 2009 and December 2010. The sample for panel (A) is loans associated with borrowers with a FICO score below the median at the time of first 90-day mortgage delinquency. The sample for panel (B) is loans associated with borrowers with low- or no documentation for their mortgage. The table uses single-observation survival-time data, where each regressor is set to its value in the month of 90-day delinquency. *Finance Committee Member* is a binary variable that is one if the loan is associated with an individual with a house located in a district whose U.S. House representative is a member of the House Financial Services Committee. The control variables include those in Table II as well as a 6-month lag of the logged balance of the loan in question and the logged number of loans for the type in question. All the regressions include state*delinquency month fixed effects. Standard errors are robust to clustering at the congressional district level and are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. FICO score below median at the time of first 90-day mortgage delinquency

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	0.0168	0.0154	0.0123	0.0133
	(0.0188)	(0.0154)	(0.0105)	(0.0108)
Majority Party	-0.0033	-0.0173	0.0070	-0.0184*
	(0.0169)	(0.0141)	(0.0088)	(0.0110)
Borrower-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	63,569	71,060	86,138	128,597
# of Loans	63,569	71,060	86,138	128,597

Panel B. Mortgages with Low Documentation or Documentation Information Missing

	(1)	(2)	(3)	(4)
	Auto Loans	Bankcard Loans	All other loans (student, retail, consumer finance, other)	Non-mortgage Loans
Finance committee member	0.0120	0.0033	0.0046	0.0093
	(0.0155)	(0.0120)	(0.0100)	(0.0093)
Majority Party	-0.0001	0.0061	0.0037	0.0043
	(0.0133)	(0.0122)	(0.0091)	(0.0088)
Borrower-level controls	Yes	Yes	Yes	Yes
State*month fixed effects	Yes	Yes	Yes	Yes
Clustering	Congressional District	Congressional District	Congressional District	Congressional District
# of Loan-months	79,048	106,771	102,980	174,718
# of Loans	79,048	106,771	102,980	174,718