Politicizing Consumer Credit*

Pat Akey[†]
University of Toronto

Rawley Z. Heimer[‡]
Boston College

Stefan Lewellen[§]
Carnegie Mellon University

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Abstract

Using proprietary credit bureau data, we find that consumers' access to credit decreases by 4.5% - 8% when the borrower's home-state U.S. Senator becomes the chair of a powerful Senate committee. The reduction in credit access is concentrated among historically credit-constrained consumers (low income, non-white, and borrowers with poor credit scores), and is stronger in areas with less politically-engaged constituents and more politically-connected lenders. Our evidence is consistent with a "political protection" hypothesis in which politically-connected banks reduce their compliance with regulatory fair-lending guidelines after their political connections become more powerful. Our results highlight the distinction between political power and legislative outcomes and contrast with recent findings that governments expand credit access to firms and consumers.

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[†]University of Toronto. Phone: +1 (647) 545-7800, Email: pat.akey@rotman.utoronto.ca

[‡]Boston College. Phone: +1 (216) 774-2623, Email: heimer@bc.edu

[§]Carnegie Mellon University. Phone: +1 (412) 268-5033. Email: lewellen@cmu.edu

1 Introduction

Many U.S. consumers have historically faced difficulties accessing conventional credit markets.[1] In response, the U.S. government has passed numerous pieces of legislation designed to expand access to credit – legislation which recent literature finds to be largely successful.[2] However, legislative outcomes reflect the collective views of the U.S. Congress (and hence, the American people) more so than the incentives and beliefs of any one individual politician.[3] As such, it is an open question whether politicians use their *individual* powers to expand credit access for their constituents.

This paper uses shocks to the political influence of U.S. Senators and proprietary data on Americans' credit histories to examine the relationship between political power and consumers' access to credit. We find that increased political power is associated with *reductions* in consumer credit access in a politician's home state. The reduction in credit access primarily affects "disadvantaged" borrowers, such as those with poor credit scores, low-income households, and racial minorities. These results sharply contrast with the existing literature, which collectively posits that government intervention expands consumers' access to credit (e.g., Carvalho (2014), Antoniades and Calomiris (2016), Chavaz and Rose (2016)).

To identify the effect of political power on consumer credit, we use U.S. Senators' ascension to the chair of powerful Senate committees, an empirical strategy introduced by Cohen, Coval, and Malloy (2011). Senate committee chairs are powerful legislative figures; they have an outsized

[1] For example, Federal housing policies enacted in the 1930s encouraged discrimination against non-white borrowers, a practice known as "redlining" (Appel and Nickerson (2016)).

[2] See, e.g., Agarwal, Chomsisengphet, Mahoney, and Stroebel (2015), Agarwal, Chomsisengphet, Mahoney, and Stroebel (2016c), Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, and Seru (2016a), Mian, Sufi, and Trebbi (2010, 2013), among others.

[3] For example, when introducing a piece of legislation in December 2015, Senator Mark Kirk (R – Illinois) lamented the "1.4 million men and women in Illinois [who] are unable to build a credit score, making it very difficult to get a loan, [a] mortgage, or credit cards." However, the "bipartisan, bicameral" bill (S. 2355/H.R. 4172) introduced by Senator Kirk and Senator Joe Manchin (D – West Virginia) was not approved by either the House Financial Services Committee or the Senate Committee on Banking, Housing, and Urban Affairs, and hence, was not sent to either chamber of Congress for a vote.

role in determining what legislation sees the Senate floor and they have significant influence over the allocation of government resources.[4]

Crucial to identifying political power's effect on consumer credit, a Senator's ascension to committee chair is primarily determined by committee seniority, and therefore plausibly unrelated to current, statewide economic conditions that affect consumer credit markets. To illustrate, Daniel Inouye (D – Hawaii) replaced Robert Byrd (D – West Virginia) as chair of the Senate Appropriations Committee in 2009. Byrd stepped down because of health problems. Inouye replaced Byrd because Senate rules dictate that chairmanship passes to the next-longest-serving member of the committee from the same political party. Hence, Inouye (joined committee in 1971) took over as chair over Barbara Mikulski (D – Maryland, joined committee in 1987) due to events that happened decades before Inouye's ascension. Therefore, the fact that Hawaii and not Maryland experienced an increase in political power in 2009 is unlikely to be related to current economic conditions in either state.

To measure consumer credit access, we use the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (FRBNY CCP/Equifax), a proprietary panel of consumer credit histories covering a 5% random sample of the entire U.S. population since 1999. The data is well-suited to studying consumer credit provision because it contains the precise geographic location of borrowers (Census tracts), and it tracks individuals' credit applications, credit usage, credit scores, and delinquencies over time. We use this data to create a measure of credit access, *supply ratio*, equal to the ratio of new credit accounts to new applications on the consumer's credit report. Our tests focus on credit access to historically "disadvantaged" borrowers, because these consumers are more likely to be affected by changes in the supply of credit – their credit applications are more likely to be on the margin of being approved.

We find that increased political power decreases consumer credit access by 2 to 4 percentage points on average in the politician's home state, relative to credit access in unaffected states. These

[4]For example, in speaking about the age of current Senate Appropriations Committee chair Thad Cochran (R – Mississippi), former Senate Majority Leader Trent Lott (R – Mississippi) recently argued that "[the people of Mississippi] should keep that chairmanship until the bloody last day that we can!" Source: "How the oldest Senate ever is taking a toll on the business of Washington", *Washington Post*, December 16, 2017."

estimates are economically large: the sample average of *supply ratio* is 0.45, suggesting that overall consumer credit access falls by 4.5 to 8 percent in states that gain political power.[5] Moreover, historically credit-constrained borrowers – borrowers with credit scores below 640, low-income consumers, and consumers residing in Census tracts that have at least 50% minority residents – experience the largest reduction in credit access. The estimates are robust to using consumer-level fixed effects, which account for unobservable differences in borrower quality across constituencies. The estimates are stable over the sample period, not caused by outlier states, and are not the result of preexisting trends in credit access.

The most likely explanation for the reduction in credit access is that powerful home-state Senators provide banks with "political protection" against regulatory obligations. Fair-lending regulations require U.S. commercial banks to extend credit to historically disadvantaged borrowers. For example, the Community Reinvestment Act (CRA) and Equal Credit Opportunity Act (ECOA) were enacted to reduce credit-related discrimination and are considered "*quid pro quo* for privileges such as the protection afforded by federal deposit agencies and access to the Federal Reserve's discount window" (Bernanke, 2007).[6] These regulations constrain banks from denying credit to certain borrowers, and have been shown to increase risky lending (Agarwal, Benmelech, Berman, and Seru (2016b)). Failure to comply with the CRA can also impact a bank's ability to open new branches or engage in mergers and acquisitions.[7] However, if banks have access to powerful legislators that soften the expected negative consequences of regulatory non-compliance, then banks can tighten screening standards on disadvantaged borrowers and reallocate credit towards higher-quality borrowers.

Several empirical results are consistent with this explanation. First, the reduction in credit access is strongest in neighborhoods where the CRA is more likely to impact bank lending decisions. We exploit within-Metropolitan Statistical Area (MSA) variation in CRA eligibility that differs across MSAs (Census tracts below 80% of the MSA's median income) and find that in-

[5]This effect is equal to about 75% of the size of the drop in credit access observed during the financial crisis.

[6]Bernanke also noted that "it appears that, at least in some instances, the CRA has served as a catalyst, inducing banks to enter underserved markets that they might otherwise have ignored."

[7]In principle, CRA non-compliance can threaten a bank's charter as well.

creased political power reduces consumer credit access most strongly in Census tracts where the CRA binds, with a discontinuous reduction occurring at the CRA eligibility threshold. Indeed, affected banks' regulatory CRA exam scores decline following these changes in political power. Second, within a given state, the largest reductions in access occur in counties served by politically-active banks that are directly connected to the new committee chair. Third, after the state's Senator becomes committee chair, higher-quality borrowers receive an increased share of new loans, and consumers receiving these loans have lower delinquency rates in subsequent years. Finally, if banks are forced to forego more profitable investment opportunities in order to comply with fair-lending standards, then bank performance should increase following the politically-motivated relaxation of these regulatory guidelines. Indeed, we find evidence of increased bank profitability. Overall, the preponderance of evidence suggests banks obtain "political protection" when a home-state Senator becomes more powerful, which allows banks to curtail less-profitable lending to disadvantaged borrowers.

The primary alternative explanation for our results is that there are contemporaneous correlations with macroeconomic factors may independently be affecting the supply or demand for credit in ways that lead to equilibrium reductions in lending to certain borrower groups. For example, Cohen, Coval, and Malloy (2011) find that after Senators ascend to become chairs of important Senate committees, government spending increases and private-sector investment decreases in affected states. Hence, banks in affected states might optimally reduce the supply of consumer credit due to changes in their investment opportunity sets. In addition, negative shocks to private-sector employment could cause reductions in borrower creditworthiness, leading banks to reallocate capital away from disadvantaged consumers.[8] Finally, even if consumer credit supply remains fixed, changing macroeconomic conditions may also reduce the demand for consumer credit products, forcing banks to reallocate capital to other uses.

[8]Note that these stories rely on the existence of either pricing frictions or bank financial constraints. If banks are unconstrained, changes in investment opportunity sets need not produce reductions in any given lending bucket. Similarly, if banks can correctly price all loans based on expected default rates, then changes in borrower risk profiles need not affect the expected profits of the bank.

However, we find little evidence that macroeconomic factors are influencing our results. First, we find no meaningful differences in macro variables such as GDP and employment between states with a newly ascendant Senate chair and states with no recent chair ascensions. Second, our main results are not affected by the inclusion of state $\times$ quarter (or county $\times$ quarter) fixed effects that should account for common shocks to demand, supply, or profitability within a given state or county at a given point in time. Third, we specifically examine consumers' demand for new credit and find no significant changes in consumer credit demand around the times of Senator ascensions. Fourth, we find few meaningful differences in the composition of banks' loan portfolios before and after a state politician ascends to become a Senate committee chair, suggesting that any changes in investment opportunity sets are likely to be small. Finally, our results are robust to limiting the control group to other states represented by Senators from the ranking party on the same committee as the newly-ascendant chair.[9] Collectively, these tests suggest that mechanisms involving macroeconomic factors in affected states are unlikely to explain our results.

Our findings also contrast with the conventional view in the literature that politicians encourage expanded credit access for consumers. However, there are several reasons why the politicians we study are unlikely to be acting against their own self-interest when consumer credit access falls. First, these politicians are relatively entrenched: U.S. Senators serve long, six-year terms and incumbents win re-election about 90% of the time. Second, catering to financial institutions could be more politically beneficial than catering to constituents. Indeed, the largest reductions in credit access occur in neighborhoods where constituents are politically unengaged (as measured by household political contributions), and the effects are strongest in areas with banks that make political donations directly to the Senator in question. In addition, after the Senator becomes committee chair, there is an increase in credit availability within high-income neighborhoods, where constituents are significantly more likely to vote. Finally, we find no evidence that politicians are catering to traditional voting blocs – Democratic and Republican committee chairs are both associated with similar reductions in consumer credit access.

[9]That is, in the Daniel Inouye example, rather than comparing outcomes in Hawaii to outcomes in all other U.S. states, we compare Hawaii with other states represented by Democrats on the Appropriations Committee at that time.

Our paper contributes to the literature on politics and credit markets in three distinct ways. First, the existing empirical evidence supports the view that political influence is associated with increases in consumer credit supply (Mian, Sufi, and Trebbi (2010, 2013), Antoniades and Calomiris (2016), Chavaz and Rose (2016)), and more generally, a number of papers have shown that specific pieces of legislation have had (mostly) positive effects on consumer credit access (Agarwal, Chomsisengphet, Mahoney, and Stroebel (2015), Agarwal, Chomsisengphet, Mahoney, and Stroebel (2016c), Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, and Seru (2016a)). In contrast with these papers, we look at how changes in *political power* affect consumer credit supply, and we find that increased political power leads to a *decrease* in the supply of credit to disadvantaged borrowers, particularly in areas with a high penetration of politically connected banks.

Our paper also relates to the growing literature on the financial inclusion of economically disadvantaged households. This literature has evaluated the role of local financial development (Celerier and Matray (2015), Brown, Cookson, and Heimer (2017)) and access to traditional and unconventional financial products on consumer outcomes (Melzer (2011), McDevitt and Sojourner (2016), Begley and Purnanandam (2017)); the effects of financial literacy on consumer credit outcomes (Lusardi and Mitchell (2011), Brown, Grigsby, van der Klaauw, Wen, and Zafar (2016)); and the effects of cognitive biases on outcomes (e.g., Stango and Zinman (2011)). To the best of our knowledge, however, no papers study the effects of political power on financial inclusion in conventional debt markets.

Finally, our paper contributes to the literature on political connections and the financial system. This literature has linked political connections with government bailouts (Brown and Dinç (2005), Faccio, Masulis, and McConnell (2006), Duchin and Sosyura (2012), and Liu and Ngo (2014)) and financial deregulation (Kroszner and Stratmann (1998), Stratmann (2002)), while another strand of the literature links firm-specific political connections to bank lending or municipal funding around elections (Claessens, Feijen, and Laeven (2008), Carvalho (2014), Perignon and Vallée (2016)). In contrast to these studies, our paper links political power to both financial regulations and banks' lending decisions.

2 Political Power in the U.S. Senate

U.S. Senators that chair one of 16 standing Senate committees have considerable legislative power in the United States Congress, and are among the most influential members of the Federal Government more broadly. Senate committees are responsible for specific areas of policy, and for a bill to become law it must first be approved by the relevant committee(s) before moving to the floor of the Senate for a vote.[10] Importantly, this gives the members of a committee – and *particularly* the committee's chair – substantial influence over the final content of legislation. Committee chairs are also responsible for setting the committee's agenda, which allows them to dictate the legislative schedule, call hearings, and control the other actions taken by the committee. Furthermore, committee chairs have significant power within the Senate, even on legislation outside of the committee's policy area, because of the vote-trading ("log-rolling") that is crucial to the functioning of the Senate (Cohen and Malloy (2014)).

Per Senate rules, the role of committee chair is filled by the Senator from the majority party who is the longest tenured member of that committee, provided they do not already chair another committee. This practice has been in place in the Senate for over 100 years and, only in a few cases, have deviations from these rules affected succession (Collie and Roberts (1992)). Hence, the committee's line-of-succession is a deterministic function of member seniority, and seniority is, at any point in time, a historical artifact of political circumstances across all U.S. States. Furthermore, chairmanship turnover can only be caused by the previous chair's resignation, reelection defeat, or a change in party control of the Senate. These reasons have little relation to events in the new chair's home state.

As a result of these Senate rules, Senate committee chair turnover is unlikely to be due to current economic or credit market conditions in the new chair's home state. The available evidence supports this proposition. Appendix Table A.1 examines the effect of state-level macroeconomic variables – such as GDP, employment, income, house prices, and bankruptcies – on the propensity

[10] For example, the Gramm-Leach-Bliley Act initially passed both the Senate Banking and Judiciary committees prior to coming to a vote before the full Senate, because the bill related to both banking and anti-trust policy.

for the state's Senator to become a committee chair. These macroeconomic variables do not statistically or economically predict a Senators' ascent. Therefore, factors causing changes in political power do not seem to be correlated with the traditional economic characteristics that would most likely affect credit allocation to consumers.

3 Data

3.1 Political Data: Senate Committee Chairs

We obtain data on Senate Committee membership from the website of Charles Stewart III (see Edwards and Stewart III (2006) for more details on this data). To construct our measure of a State Senator's power, *Powerful Politician*, we identify each Senate committee leadership change during our sample period (1999-2012). We discard all committee leadership changes where a ranking member (the most senior member of the minority party) takes over as chair because the control of Congress changed. We do so because promotions based only on voting outcomes could be caused by contemporaneous economic conditions that influence voter behavior. Though this refinement reduces the number of Senator ascensions in our sample, it also provides us with the cleanest measure of political power shocks. We then assign a value of one to a given state in a given quarter if one of the state's Senators took over as chair of a Senate committee within the previous two calendar years (or one congressional term).[11]

Table 2 presents summary statistics by Congressional cycle for each of our definitions of political power shocks. Shocks occur in all election cycles for most definitions of our shock variable. Figure 1 displays a map of the United States, showing "shocked" and "non-shocked" states. Panel A contains all of the shocks in our data set, while Panel B contains shocks to "important"

[11] Cohen, Coval, and Malloy (2011) use the six years (corresponding to one Senate term) following the ascension of a new chair to measure the effects of a Senator's chairmanship on their home state. However, Snyder and Welch (2015) argue that the six year duration is too long, because Senators can (for example) lose their position as Chair due to changes in control of the Senate. Using a shorter post-event window (two years) alleviates concerns raised by Snyder and Welch (2015), but potentially makes the empirical tests understate the effects of political power.

committees and Panel C splits all shocks by political party.[12] We observe a wide geographical variation in shocks, particularly for "important" shocks.[13] The shocks also affect a roughly equal number of Democrat and Republican Senators, indicating that "shocked" politicians are unlikely to disproportionately favor one type of ideology over another.

3.2 Consumer Credit Data: The FRBNY Consumer Credit Panel

Our main source of data on consumer credit is the FRBNY Consumer Credit Panel (FRBNY CCP/Equifax), which is a longitudinal data set tracking household liabilities and repayment using a five percent randomized sample of individuals with a social security number and a credit report on file at Equifax.[14] The data start in 1999Q1 and are collected quarterly thereafter (our sample ends in 2012Q4). The sample design of the FRBNY CCP/Equifax alleviates concern over attrition: the panel re-samples at every quarter to incorporate new credit report holders, and thus, is representative at any quarter. Brown, Grigsby, van der Klaauw, Wen, and Zafar (2016) show that the FRBNY CCP/Equifax offers a more comprehensive coverage of U.S. household liabilities than other nationally-representative surveys such as the the Flow of Funds Accounts and the Survey of Consumer Finances.

There are several reasons why the FRBNY CCP/Equifax is particularly well-suited to studying the relation between changes in political power and household finances. First, the data set tracks individual consumers over time for up to sixteen years, allowing us to account for unobservable differences in borrower quality via the use of individual-level fixed effects. Second, though some individuals (close to ten percent of the U.S. population) do not have a credit report, the FRBNY CCP/Equifax offers coverage of individuals that have had difficulty obtaining credit. For

[12] Some of our tests examine leadership changes among a subset of "important" Senate committees. Following the definitions in Cohen, Coval, and Malloy (2011), we define *Important Committee* shocks as those involving the Senate Finance Committee (responsible primarily for tax policy), the Senate Appropriations Committee (responsible primarily for spending policy), the Senate Armed Service Committee, the Senate Veterans committee, and the Senate Rules Committee. Given that much of our paper revolves around the actions of banks, we also add the Senate Banking, Housing, and Urban Development Committee to this list, for a total of six "important" committees.

[13] Several states are shocked more than once (although not more than twice) for our primary shock variable.

[14] Technically, the sample is randomized by using five pairs of arbitrarily selected digits at the end of an individual's social security number.

example, a consumer who was denied a loan in 1999 will remain in the data set going forward regardless of whether they ever again seek formal credit. Third, the FRBNY CCP/Equifax data offers an unbiased view of precise geographies, including rural areas. This allows our analysis to consider within-state changes in the composition of consumer credit.[15]

The primary shortcoming of the FRBNY CCP/Equifax relative to other household surveys is that – due to federal fair-lending and privacy laws – no demographic information is linked to the credit records aside from consumer age. To alleviate this gap in the data, we merge the FRBNY CCP/Equifax with Census tract demographics from the 2000 U.S. Census. Because Census tracts include a small population (the target size of a Census tract is 4,000 people), this merge allows us to precisely and reliably estimate demographic and socioeconomic factors of consumers in the data.

Our analysis focuses on several key variables from the FRBNY CCP/Equifax (summary statistics are in Table 1). Our measure of credit availability, *supply ratio*, equals the number of new credit lines divided by the number of hard credit inquiries on the consumer's credit report. *Supply ratio* is best paired with high credit risk borrowers (borrowers with a credit score below 640), because these applicants are less likely to be automatically approved by lenders' algorithms. Both Bhutta and Keys (2014) and Brown, Cookson, and Heimer (2017) validate *supply ratio* by showing that it varies significantly over time and geographically in a manner that reflects the tightening and loosening of credit supply.[16]

Many of our tests measure the supply of new loans by the number of new credit lines per individual, which we sometimes decompose into secured and unsecured lending. We test overall consumer financial health using the Equifax Riskscore. Riskscore is a nationally standardized measure that summarizes an individual's history of borrowing and repayment activity. Lenders

[15]These features of the data offer considerable advantages over the most likely alternative data set used by the literature on credit provision to households: Home Mortgage Disclosure Act Loan/Applicant Register (HMDA) data. For our purposes, HMDA data has a number of shortcomings. Because it only contains data on mortgage credit, it is missing the sizable population of disadvantaged individuals who are excluded from mortgage markets or prefer to rent housing. Also, HMDA does not track individuals over time, and the data is sometimes too thin to use at very precise geographic levels (such as a Census tract).

[16]The measure's main limitation is that the FRBNY CCP/Equifax does not specify the purpose of the loan for which the hard credit inquiry was obtained. In addition, consumers who receive a loan offer, but decide to not open a new credit account, will show up as having been denied credit.

use metrics like the Equifax Riskscore in the decision to extend credit, as well as to determine an appropriate interest rate to charge. Thus, a higher Riskscore can lead to a higher propensity to obtain credit and/or significant interest cost savings. To measure financial performance, we calculate the fraction of credit accounts that are at least 90 days past due. Our delinquency variable equals the number of credit accounts 90 days past due, 120 days past due, in collections, or in "severe derogatory" status divided by the total number of credit accounts for the consumer in a given quarter. We also examine consumers' credit utilization, which equals the consumer's total outstanding revolving balance divided by the limit on the consumer's credit cards.

3.3 Political Data: Campaign Contributions

Some of our tests also employ data on contributions made by individuals and banks to political action committees (PACs) that are affiliated with a given political candidate or political party. We obtain PAC contribution data from the U.S. Federal Election Commission (FEC) for all federal elections from 1998-2010.[17] For each election cycle, we obtain individual (personal) political donations to their Senators at the ZIP code level and donations by bank PACs at the level of the headquarter bank. Because corporations are prohibited from donating money directly to political causes, we examine donations made by bank PACs to the election campaigns of the shocked senators. For our primary tests, we consider a bank to be politically active if it made political contributions from its PAC to the Senator in question before the Senator became committee chair.

3.4 Banking Data

We also create geographic measures of the intensity of politically connected banks. To do so, we start with annual branch-level data on bank deposits from the FDICs Summary of Deposits (SOD) report. For each bank branch in the SOD data, we assign a flag equal to one if the branch's parent holding company has made campaign contributions to a "shocked" Senator prior to the

[17] FEC data is transaction-level data organized by election cycle. Political contribution data is available from the FEC, the Center for Responsive Politics, or the Sunlight Foundation. The latter two organizations are non-partisan, non-profit organizations who assemble and release government datasets to further the public interest.

Senator's ascension to the chair of a Senate committee. We then aggregate this measure over geographic areas (ZIP codes, counties) to construct two measures of political connectedness. Our first measure, $Branch\,Frac$, is the ratio of "politically-connected" bank branches within a ZIP code/county relative to the total number of bank branches in that ZIP code or county. Our second measure, $Deposit\,Frac$, is the ratio of deposits held by "politically-connected" banks in a ZIP code or county relative to the total deposits held by bank branches in that ZIP code or county.

Finally, we examine whether increased political power affects bank profitability. We use banks' quarterly FFIEC 031/041 filings (the "Call Reports") to obtain data on bank balance sheet composition and profitability.

3.5 Other Data

In some tests we use economic data that come from a variety of sources including the Bureau of Labor Statistics, the Bureau of Economic Analysis, the Census Bureau, the Federal Housing Finance Agency, the Department of Labor, and the Administrative Office of U.S. Courts.

4 Powerful Politicians and Consumer Credit: Empirical Evidence

To estimate the effect of political power on consumer credit outcomes, we use the following difference-in-difference regressions:

$$Credit\,Outcome_{i,g,t} = \beta \times Powerful\,Politician_{g,t} + \Gamma' Controls_{i,g,t} + \alpha_t + \alpha_g + \varepsilon_{i,g,t},$$

where *i* indexes consumers, g indexes a geographical unit (generally a Census tract or a state), and *t* indexes year-quarter (e.g., the second quarter of 2003). The independent variable of interest, $Powerful\,Politician_{g,t}$, equals one if a Senator from a given state has ascended to become a committee chair within the past two years (which corresponds to one full congressional cycle), and zero otherwise. The regression model also includes a time fixed effect (α_t) and a geographic fixed effect (α_g) to control for unobserved heterogeneity across time and location. Some of our

specifications also include consumer fixed effects (α_i) to account for unobservable differences in borrower quality.

Identification in this model comes from comparing consumer credit outcomes in states with new committee chairs against credit outcomes in all other states, controlling for observable differences in consumer and geographic characteristics. For example, consider two consumers, one in Hawaii and one in Maryland, who have identical demographic profiles and identical credit profiles at the time when Daniel Inouye (Hawaii) ascends to chair the Senate Appropriations Committee. The variable $Powerful\ Politician$ will take the value of zero for both consumers in 2008, but will take the value of one for the Hawaii consumer and zero for the Maryland consumer in 2010. Hence, any post-event differences in credit outcomes between the Hawaii and Maryland consumers above and beyond differences observed in 2008 will be captured by the coefficient estimate on $Powerful\ Politician$. As such, this variable should capture the effects of political power on consumer credit outcomes.

Historically disadvantaged borrowers are most likely to be affected by changes to credit supply. So, our tests also interact *Powerful Politician* with various consumer demographic characteristics. These tests take the form:

$$\begin{aligned} Credit\,Outcome_{i,g,t} = {} & \beta_1 \times Powerful\,Politician_{g,t} \\ & + \beta_2 \times Powerful\,Politician_{g,t} \times Demographic\,Characteristic_g \\ & + \Gamma' Controls_{i,g,t} + \alpha_t + \alpha_g + \varepsilon_{i,g,t}, \end{aligned}$$

where $Demographic\,Characteristic_g$ equals one if Census tract g is heavily populated by minorities, low-income households, or people with low credit scores.

4.1 Main Results on Credit Provision

4.1.1 Credit Supply to Disadvantaged Borrowers

We begin our analysis by examining how shocks to Senators' power impact the supply of credit to consumers in their home states. We focus much of our analysis on historically disadvantaged borrowers such as racial minorities. For example, powerful politicians may seek to increase (efficiently or inefficiently) access to credit for these groups of borrowers either to cater to a particular voting bloc or because of ideological considerations. On the other hand, economic improvements or increased government spending may also shift the investment opportunity set of lenders, leading to a reduction in consumer credit supply that particularly affects disadvantaged borrowers. In addition, several authors argue that lending practices are more standardized for borrowers with good established credit history (a high credit score) and that lenders have more discretion over lending policies to consumers who may be accessing credit for the first time or consumers for whom information asymmetry is higher (see, e.g., Bhutta and Keys (2014)). Hence, we also focus much of our main analysis within the sample of borrowers who have a Riskscore less than or equal to 640 (which is the typical cutoff between "prime" and "subprime" credit scores).

Table 3 tests how political power affects the supply of credit to borrowers and how the effects differ to borrowers who are located in majority-minority Census tracts. The indicator variable $Powerful\ Politician$ captures the main effect of a home-state senator ascending to Chair of a committee. We interact this variable with an indicator variable $Majority\ Minority$ to see how the effects differ for borrowers in majority-minority areas.

Senate committee ascension decreases consumer credit access in the Senator's home state. The coefficient estimates on *Powerful Politician* range from -0.014 to -0.019. This corresponds to 3% to 4% of the sample mean of *supply ratio*. In addition, the credit contraction is stronger for borrowers in minority Census tracts. For example, column (2) suggests that the incremental reduction in credit supply in majority-minority areas is -0.021, which is fifty percent larger than the effect on non-minority borrowers of similar quality. These estimates are similar whether or not we control for the borrower's Riskscore and Census tract incomes. The estimates of the interaction effect are

robust to controlling for state $\times$ time and county $\times$ time fixed effects ($Powerful\ Politician$ is collinear with these fixed effects), suggesting that changing demand conditions or investment opportunities within affected states or counties, such as local income shocks or macroeconomic trends cannot explain the results in column (2). In addition, estimates with borrower-level fixed-effects (columns 5 and 6) suggest that changes in unobservable borrower composition across locations cannot explain our results. The results are also robust to using Cohen, Coval, and Malloy (2011)'s definition of increased political power, which is restricted to ascents involving chairs of one of six "important" Senate committees (Appendix Table A.2).

The decrease in *supply ratio* is caused by a reduction in new credit lines, rather than an increase in credit applications. According to Table A.4, *Powerful Politician* does not statistically significantly affect the number of credit inquiries for borrowers with poor credit scores in either white or majority minority Census tracts, and the economic magnitudes of these effects are small. This result is consistent with the effects documented in Table 3 being caused by tightened credit supply, as opposed to changes in demand.

4.1.2 Robustness

We now present further evidence in support of a causal interpretation of our difference-in-difference regressions. Figure 2 presents a graphical representation of how credit supply changes before and after a shock to a home-state Senator's power. For both minority and non-minority borrowers, the estimated average treatment effect on *supply ratio* is stable for the two years prior to the political power shock. Immediately following the Senator's ascension, *supply ratio* sharply declines.[18] The effect of a Senator's ascension on credit access persists for at least four years after the Senator takes power. Because this graph shows no time trends in the dependent variable prior to the actual Senator ascension, it is unlikely that pre-trends lead to false positive coefficients on *Powerful Politician*.

[18] Because committee allocations along with Chairmanships are officially announced in late December, immediately prior to the seating of Congress in January of odd-numbered years, it is reasonable for the effect to start in shock-year equal to zero.

One concern with our identification strategy is that the states that are represented on particular committees is endogenous. While our identification exploits the fact that many years separates the choice of when a politician joins a committee and when he/she can potentially become the chair of the committee, we verify that unobserved differences in states that did not have committee representation versus those that did at the time of each Chairmanship ascension event are not driving our results. Specifically, we rerun the analysis presented in Table 3 but include only control states that had a Senator from the majority party on the committee, (i.e., those states that had as Senator that *could have been* promoted to chair with a sufficient amount of seniority). Table A.3 of the internet appendix shows that our results are quantitatively and statistically comparable, despite having a sample size that is roughly 40% of the size of our baseline analysis.

We also provide additional evidence that our analysis is likely to satisfy the assumption of parallel trends necessary for well-identified difference-in-difference tests. We use a random set of shocks to construct a placebo distribution of t-statistics (the top panel of Appendix Figure A.1). These placebo t-statistics are normally distributed around zero (as demonstrated by the P-P plot in the bottom panel of Appendix Figure A.1), and few are above 1.96 in absolute value, suggesting that our setting plausibly satisfies the parallel trends assumption.

The relation between political power and reduced credit availability is also robust across different states and over our sample period. Appendix Figure A.2 shows that the results are not particuarly sensitive to the exclusion of any single state, which indicates that outlier states are unlikely to affect the coefficient estimates. The estimates are also not much changed when we exclude any given year from the analysis. This result is encouraging, because our sample period includes the broad contraction in consumer credit during the Great Recession.

4.1.3 Political Incentives and Consumer Credit

A plausible concern with our consumer credit supply results is that they are caused by random unobserved factors that are unrelated to our political shocks. Hence, the argument for a causal

interpretation can be strengthened by showing that politicians are not harming their reelection chances by reducing credit access to their voters.

To consider the role of political incentives, we test whether the reductions in credit supply occur in areas where constituents are unengaged with the political process – precisely the segment of the population that is less likely to punish politicians for reduced credit supply. We follow Ovtchinnikov and Pantaleoni (2012) in using consumers' personal political contributions to measure the political engagement of individuals. We then examine whether the contraction in credit is stronger or weaker in areas that are politically engaged (i.e. areas with large amounts of personal political contributions) relative to areas that are less politically engaged. While we do not claim to test a specific theory, Becker (1983) proposes a notion of political influence as a zero-sum game (with political decisions advantaging some interest groups while simultaneously disadvantaging others) that is similar in spirit to our analysis.

The reduction in credit access is strongest in areas that have politically unengaged consumers. Table 4 sorts the data into areas that are politically engaged (ZIP codes with individual political contributions above the state median) or politically unengaged (ZIP codes below the state median) and reruns our *supply ratio* in the two subsamples. The coefficients on $Powerful\,Politician$ and $Powerful\,Politician \times Majority\,Minority$ are substantially larger in areas with below-median political contributions. *Powerful Politician* is associated with a 0.04 reduction in credit access to minority borrowers in politically unengaged ZIP codes, which is approximately 12% larger in absolute value than the estimate from the full sample (columns 3a and 4a of Table 3). In politically engaged ZIP codes, the coefficient estimates are also negative, but they are smaller and statistically insignificant (columns 1a and 2a).

5 Why Does Political Power Reduce Consumer Credit Supply?

5.1 Political Protection

The results in the previous section suggest that increased political power reduces the availability of credit to historically disadvantaged consumer groups. This section proposes and tests an explanation for these results. Our preferred explanation hinges on the "political protection" that may be afforded to banks once a Senator becomes a powerful committee chair. In particular, financial institutions are subject to a number of federal guidelines that encourage them to apply lower screening standards to various types of disadvantaged borrower groups (for example, low-income borrowers and racial minorities). However, an institution "protected" by a powerful politician may expect to face less severe consequences for noncompliance with fair lending standards. For example, a powerful legislator can help an institution become eligible for government programs (such as TARP), open new branches, or complete an M&A deal even if the institution violates federal lending standards. A politician could also take actions (such as speaking directly with regulators) that would reduce the expected costs of enforcement for banks that have political "protection."

This mechanism suggests that "politically protected" banks should no longer feel the need to apply lower screening standards to disadvantaged borrowers. As such, we would expect screening standards to tighten in "shocked" states following political ascension. Once screening standards tighten, disadvantaged consumers that were previously able to obtain access to credit may be denied additional credit by existing lenders. In addition, since many federally-mandated loans to disadvantaged borrowers may not be positive-NPV, other banks in the same neighborhood may refrain from stepping in to fill the newly-created credit void.[19] Hence, even if only *some* banks within a given area tighten their lending standards following the ascension of a committee chair, this would still be likely to result in a reduction in the area's overall supply of credit to disadvantaged borrower groups.

[19] This is true as long as federally-mandated loans are strictly dominated by other investment opportunities, even if such loans are technically positive-NPV.

Our political protection hypothesis yields five empirical predictions. First, the declines in consumer credit we observe should be strongest in areas served by banks that have contributed to an ascending committee chair's PAC. Intuitively, banks that already have a political connection to the "shocked" Senator should gain the most from the Senator's "protection." Second, the reduction in consumer credit supply should be strongest in Census tracts that are most likely to receive additional credit due to federal lending guidelines (such as the CRA and ECOA). Relatedly, if banks expect politicians to intervene during certain states of the world, we might expect regulatory CRA exam results to be more negative for affected banks, even if these banks expect to receive a benefit (say) if they are expecting to open new branches or borrow from the Fed's discount window. Third, since banks are able to tighten screening standards, we would expect to find an increased share of consumer lending going to higher-quality borrowers in a given area following the Senator's ascension. Finally, if banks are able to shift some of their consumer lending from federally-mandated (and potentially low-profitability) borrowers to higher-quality borrowers, this may allow banks to become more profitable. As such, we would expect banks in areas with a large number of disadvantaged borrowers to have better performance after the ascension of a powerful committee chair from that state.

5.1.1 Politically Connected Banks

We first examine the extent to which reductions in credit supply are concentrated within the sample of banks that have a direct political connection to the shocked Senator. Intuitively, we might expect to see larger reductions in credit supply in areas populated by banks that have a direct connection to the newly-powerful Senator.

To examine this hypothesis, we define a bank as being "politically active" if it operates a Political Action Committee (PAC) that contributes to a shocked Senator prior to that Senator's ascension to the role of committee chair.[20] We next compute the fraction of bank branches in each

[20] We focus on contributions made *prior* to the Senator's ascension so as to avoid picking up contributions that were caused by the shock itself. However, our results are also robust to using more general definitions of political activity such as whether the bank ever operated a PAC.

county that belong to a politically-connected bank.[21] We compute both equal-weighted averages and deposit-weighted averages to ensure that our measure of politically-connected banks is not simply picking up small, economically unimportant bank branches. For both measures, we split the sample at the median into two buckets representing areas with more-connected and less-connected banks. We then test to see whether our measures of banks' political connectedness are correlated with our previous findings on consumer credit supply.

The reduction in credit availability predominantly occurs in areas with a high concentration of politically-active banks. Panel A of Table 5 sorts the data by the deposit-weighted fraction of politically active banks in the county. Panel B sorts by the equal-weighted fraction. In both panels, the total effect of political power on credit access for minorities is an order of magnitude larger in counties with above-median fractions of politically active banks than in below-median counties. These results suggest that connections between banks and powerful politicians play an important role in determining consumer credit supply.

5.1.2 Regulatory Constraints on Consumer Lending

Our second prediction is that there are larger reductions in credit supply in areas that are more likely to receive loans coming from legislation designed to improve access to credit for disadvantaged borrowers. In particular, federal laws such as the CRA and ECOA impose tough anti-discrimination lending standards on banks that have branches within areas containing disadvantaged borrowers. For example, the CRA forces banks that operate within a given MSA to extend a certain amount of credit to borrowers residing in low-income Census tracts within the MSA. These types of regulatory mandates likely have the effect of forcing banks to approve loan applications submitted by consumers that they would otherwise deny.[22] However, if powerful politicians provide "political protection" to banks, this may in turn lead banks to believe that they can reduce

[21] The results are similar when we aggregate by ZIP code.

[22] Thakor (2017) develops a model of political influence on bank lending and capital structure. Agarwal et al. (2016b) find that CRA enforcement leads banks to change their lending choices, suggesting that these types of policies act as a binding constraint on bank behavior.

the amount of lending to disadvantaged borrowers that is required by laws such as the CRA and ECOA.

To test this hypothesis, we follow Bhutta (2011) and define a Census tract within a given MSA as being subject to CRA regulation if its median income is less than 80% of the MSA's median income, which is the legal threshold at which banks become subject to the consumer lending provisions of the CRA. If banks view the CRA's lending guidelines as being less binding when they are connected to a powerful Senator, we would expect consumer lending to discontinously decline at the 80% income threshold relative to other areas of the same MSA where the CRA restrictions are not binding.

Table 6 shows that the reduction in credit access is concentrated in areas with income below the CRA eligibility threshold. Columns (1) and (2) examine the change in *supply ratio* for disadvantaged borrowers in all Census tracts following a Senate chair ascension.[23] We interact this political power shock with a variable that equals one for Census tracts where the ratio of median Census tract income to average MSA income is less than 0.8. The interaction of $Powerful\ Politician$ and $CRA\ Eligible$ is -0.016, statistically significant and comparable in magnitude to previous estimates. This result is consistent with banks reducing their CRA-mandated lending when their home-state Senator becomes a powerful committee chair. In addition, since the within-MSA income threshold varies across MSAs, even within a given state, this evidence provides more proof that the results we obtain are not a function of specific changes in bank lending to low-income borrowers. In other words, banks seem to be specifically refraining from lending to CRA-eligible borrowers, even if some of these borrowers reside in relatively high-income MSA areas.[24]

However, these results may capture a broad reduction in lending to low-income populations that by chance coincides with CRA eligibility. We address this possibility by re-estimating our CRA tests after restricting the sample to Census tracts that have a ratio of Census tract/MSA

[23]The sample size of these tests falls by about one-fifth because rural Census tracts often do not belong to any MSA.

[24]We compliment this analysis in our online appendix by examining whether our baseline contraction in credit supply for minorities occurs in the same states where high-income borrowers have the highest demand for credit. Table A.6 reports analysis where we repeat our baseline supply ratio tests after sorting states by the intensity of the increase in new accounts in high income Census tracts. The results suggest that there is a *reallocation* of credit from low-income, minority borrowers to high-income borrowers.

median incomes within a narrow range around the 0.8 CRA threshold: 0.6 to 1. According to the estimates in columns (3) and (4), the interaction of $Powerful\ Politician$ and $CRA\ Eligible$ is similar to the full sample (-0.0184 and -0.0166), suggesting that even when we restrict the sample to a narrow window around the CRA eligibility cutoff, there is a significant decline in credit access only within the areas that are subject to the CRA lending mandates.

We complement this result with two placebo tests where we falsely set the CRA eligibility threshold at income ratios of 0.6 and 1.0 (i.e. above and below the actual threshold of 0.8). Columns (5) and (6) of Table 6 set the eligibility threshold at 1.0 and examine a narrow window around this threshold of 0.8 – 1.2 (with the variable *CRA Placebo* taking the value of one for Census tracts with income ratios between 0.8 and 1.0 and the value of zero for Census tracts with income ratios between 1.0 and 1.2). Columns (7) and (8) set the placebo eligibility threshold at 0.6 and restrict the sample to include Census tracts with income ratios of between 0.4 – 0.8. In all of our placebo tests, we find that the interactions between $Powerful\ Politician$ and the CRA eligibility indicators are economically and statistically small. Moreover, in columns (7) – (8) (which restrict the sample to areas where the CRA requirements are binding), we find a large negative effect on $Powerful\ Politician$ equal to approximately -0.03, a magnitude that is comparable to the total effect for CRA eligibility in columns (1) – (4). In other words, we find that the decline in credit supply is large across *all* areas with income ratios below 0.8 (where the CRA binds), whereas there is effectively no decline in credit supply in areas with income ratios above 0.8.

To provide further evidence supporting our mechanism, we also examine the regulatory response to CRA-related lending declines by affected banks in Table 7. Banks are examined periodically by regulators to confirm that they are complying with CRA lending guidelines. The regulatory rating scale has four levels ranging from "outstanding" to "substantial non-compliance." Following Senate chair ascensions, Table 7 shows that the CRA scores of banks with branches in affected states drop by approximately 0.2 points, which is a statistically and economically large change. This effect is also slightly larger for small banks that would presumably gain the most

from political protection.[25] This result suggests (speculatively) that banks *expect* to receive protection from powerful politicians if a left-tail shock occurs in the future (or if the bank wishes to open new branches or engage in M&A activity), even if politicians do not directly intervene with regulators immediately upon becoming a Senate committee chair.

In summary, the results in Tables 6 and 7 suggest that following the ascension of a newly-powerful Senator, consumer credit access in the Senator's home state declines mostly in areas that are subject to CRA lending mandates, with the discontinuity occurring exactly around the Census tract/MSA income ratio that makes an area subject to CRA lending mandates.

5.1.3 The Creditworthiness of New Borrowers

If banks reduce the supply of credit to disadvantaged borrowers, we would also expect the pool of borrowers that are able to obtain credit following a committee chair shock to be of higher average quality than the pool of borrowers who obtain credit prior to the shock. Figure 3 plots the characteristics of borrowers that receive new credit lines before and after Senate committee chair shocks. The average borrower that receives new credit after the increase in political power is older, has a higher credit score, and has a lower credit utilization rate – all measures that are typically associated with higher credit quality. The results are similar for borrowers in both non-minority and majority-minority Census tracts (Panels A and B, respectively). Following the political shock, new majority-minority borrowers also have longer credit histories (as indicated by the age of their oldest credit account).[26]

Furthermore, tightening credit standards following committee chair shocks affects the default rates of consumers receiving new loans (Figure 4). Borrowers who receive new loans in the affected

[25] CRA examinations are required to take place at least once every two years for large banks and every five years for small banks. Hence, given our assumption that political power shocks last two years, we are only including roughly 40% of affected small banks in our sample, which significantly reduces our statistical power. Nonetheless, the strongest effects we find, both economically and statistically, are for small banks.

[26] A potential concern with these results is that, though the FRBNY – CCP sample design makes the data representative in any given quarter, some of these characteristics — e.g. the average age of a borrower — increase over time mechanically regardless of the political shock. To verify that our figures are not contaminated by time trends, we calculate t-statistics on identical figures generated using a set of 100 shocks that are structured to occur at random points in time. These "placebo" results are available upon request.

states in years before and after the political shock have similar delinquency rates (approximately 5% of accounts). In the years following the Senator's ascension, borrowers that receive loans before the shock have ten percentage points higher subsequent delinquency rates, a difference that persists for several years. This result is consistent with banks extending credit to riskier borrowers prior to the Senator's ascension, and that these new loans have worse performance ex-post.

The increased delinquency rate for these borrowers could also be attributed to tightened credit supply, which makes it more difficult for distressed borrowers to roll over debts. Indeed, credit utilization rates increase and credit scores decline for borrowers who receive credit in the two years prior to the shock (Figure A.3). In contrast, the utilization rate and credit scores of borrowers receiving credit after the political shock remain stable in the following years. These results are potentially consistent with banks beginning to deny credit to consumers that borrow to service existing debt, which causes such borrowers to subsequently default.

5.1.4 Bank Performance

As additional evidence, we use Call Report data to examine whether banks become more profitable after they tighten screening requirements for disadvantaged borrowers. In particular, we use branch-level Summary of Deposits data from the FDIC to identify all of the states in which a specific bank has branches. We then define all banks with branches in a newly-powerful Senator's home state as being "shocked," and all banks with branches in other states as being "non-shocked." We use ROA to measure bank profitability.[27] Table 8 presents the results of our tests, which also include a size control along with time, state, and bank fixed effects. Specifications (1) – (3) include the entire sample of banks, while specifications (4) – (6) restrict the sample to only include banks with branches in one state. This restriction ensures that the same bank is not in both the treatment group and the control group, which should potentially allow us to obtain more precise estimates of the effects of increased Senator political power on bank profitability.

[27] Consistent with the banking literature, using ROE instead of ROA yields results that are even stronger in both economic magnitude and statistical significance.

Across all specifications, we find that increased political power is associated with increases in bank ROA. Average annualized ROA increases by approximately six basis points (or 3.7%) at banks in "shocked" states, and this relationship grows statistically and economically stronger when we restrict the sample to banks that operate in a single state. Hence, consistent with the political protection hypothesis, banks that are operating in "shocked" states seem to earn higher income after politicians become more powerful and these banks are able to reallocate lending from less-profitable ("required") CRA loans to other, more profitable purposes.

5.2 Alternative Explanations

5.2.1 Catering to Political Voting Blocs

One alternative possibility for our results is that the reduction in credit access for disadvantaged borrowers is caused by newly-powerful politicians' attempts to cater to specific voting blocs (either to expand politicians' electoral base or to provide favors for politicians' previous supporters). For example, a recent report (Pew (2005)) shows that since 1992, 80–82% of black Americans, 50–56% of Hispanic Americans, and 40–44% of white Americans lean towards the Democratic party (with the balance largely affiliating with the Republican party). Given that most racial minorities lean towards supporting Democrats, we might expect shocks to Republican Senators to have a larger impact on credit supply in majority-minority areas than shocks to Democratic Senators.

Table 9 contains the results of these tests. In columns (1) – (4), the sample is restricted to only include shocks to Senate Republicans, while the sample is restricted in specifications (5)–(8) to only include shocks to Senate Democrats. In most specifications, political power shocks involving Senators from *both* parties lead to contractions in consumer credit in majority-minority areas, suggesting that a simple story about catering to an expected voting bloc is unlikely to explain our results. In other words, simple ideological differences across politicians are unlikely to explain our results.

5.2.2 Lending Substitution

Another plausible explanation for the reduction in consumer credit access is that an increase to a home-state Senator's political power changes the relative profitability of different types of lending. In particular, banks may rationally respond to a Senator's ascension by reallocating capital away from consumer lending and towards other types of loans. This explanation suggests that banks would be more likely to cut lending to higher-risk borrowers for whom they need to exert more effort screening loans in favor of other types of lending. This would cause reduced consumer lending overall or as a fraction of a banks total lending.

Table 10 tests for substitution across lending categories. Panels A and B examines whether (log) levels of loans are changing for different categories of bank lending following a Senator's ascension. Panel A presents analysis for the entire sample of banks, while Panel B presents analysis for the subset of banks that only operate in one state. Overall, we observe a modest increase in corporate lending of about 3.9% following a Senator's ascension that is statistically significant in banks that only operate in the shocked state. However, we do not observe economically or statistically significant effects on any other types of loans (including real estate and consumer loans). These results are also similar across politically connected and unconnected banks. Panels C and D test to see whether the composition of banks' loan portfolios change following committee chair shocks. Panel C presents analysis for the entire sample of banks, while Panel D presents analysis for the subset of banks that only operate in one state. We again find no evidence that banks are changing the composition of their lending portfolios in response to these shocks.

5.2.3 Powerful Politicians and Aggregate Consumer Credit Conditions

Another plausible explanation for our results is that shocks to a home-state Senator's power are correlated with broader changes in the consumer credit market in that state. For example, Cohen, Coval, and Malloy (2011) find that government spending increases significantly while corporate investment and employment decrease significantly in states associated with a newly-powerful Senate committee chair. These types of effects could plausibly change the demand or supply of consumer

credit in the affected state for a variety of reasons that are unrelated to our hypotheses regarding political protection.

To examine this possibility, we first assess whether the demand for consumer credit changes materially around the ascension of a new committee chair. Table A.4 shows that the number of new credit inquiries by disadvantaged borrowers remains unchanged following the ascension of a new committee chair. In Table 11, we extend these tests by examining the effects of Senate chair ascendance on a wider array of credit outcome variables and by expanding the sample to include all consumers (and not just disadvantaged borrowers). Table 11 reports estimates of $Powerful\ Politician$ when the dependent variables are the number of credit inquiries, the number of accounts, the credit utilization rate, and the supply ratio, along with consumer Riskscores and delinquency rates. Across our lending variables (columns (1) – (3)), we find that powerful politicians do not significantly affect consumer credit utilization rates, the number of new credit inquires, or the number of accounts (either statistically or economically). Column (4) replicates our main effect for comparison. We also examine whether these political power shocks led consumers' overall creditworthiness to decline, which could then drive the observed reduction in credit supply. However, column (5) suggests that consumers' average credit scores are not affected by these shocks. Moreover, column (6) suggests there is no overall change in delinquency rates following a shock to a home-state Senator's political power. Hence, it appears that the only significant change in consumer credit markets following these political power shocks is a decline in the supply of consumer credit.

Of course, a decline in consumer credit supply could still be caused by macroeconomic forces, even if there is no effect on consumer credit demand. For example, investment opportunities could change in a manner that makes it optimal for banks to reallocate credit to certain groups of borrowers. However, Table 10 shows that banks did not significantly shift the size or composition of their loan books. Moreover, Table 6 shows that the decline in credit supply occurs disproportionately in areas that are subject to CRA lending mandates. Furthermore, these CRA lending results suggest that banks reduce consumer lending in very small geographic areas within each MSA, even though the CRA eligibility threshold (which is based on within-MSA variation in

incomes) differs significantly across areas within the same state. In other words, it is not the case that lending falls on an absolute basis for borrowers with low incomes; rather lending seems to fall primarily within the low-income areas of MSAs, even if on an absolute basis these incomes are not particularly low. Finally, we find that the differential reduction in consumer lending to majority-minority neighborhoods persists even when county$\times$year fixed effects are included (Specifications (3) and (4) of Table 3), suggesting that even more localized shocks regional shocks cannot explain our results. It is very difficult to come up with a macroeconomic explanation for why consumer credit supply, but not demand, would drop disproportionately in these very narrowly defined portions of an MSA, particularly given the findings from our other tests. For example, even if a politician's programs or spending changes were targeted at very specific groups of consumers, these changes should most naturally affect credit demand, not credit supply. Collectively, our tests suggest that aggregate changes in credit market conditions are unlikely to fully explain our main results.

6 Conclusion

This paper examines the effects of political power on U.S. consumers' access to credit. Our analysis uses a long history of systematic shocks to the political standing of U.S. Senators and a proprietary panel data set of consumer credit histories covering a 5% random sample of nearly the entire U.S. population since 1999. We test whether the ascension of a home-state Senator to chair a powerful Senate committee affects consumer credit supply in the Senator's home state relative to other, unaffected states. Our focus on the role of political power contrasts with the existing literature, which has focused on assessing the effects of government legislation on consumer credit outcomes.

We find that increases in political power decrease consumer credit access in the Senator's state by an average of 4.5 to 8 percent relative to unaffected states. Moreover, the reduction in credit access mostly affects borrowers that have historically been credit constrained: consumers with low incomes and poor credit scores, and racial minorities. These results are robust to increasingly

stringent geographic fixed effects as well as individual fixed effects, which account for unobserved heterogeneity in borrower quality across political constituencies.

Our results are consistent with a "political protection" hypothesis whereby banks tighten screening standards on disadvantaged borrowers once they are "protected" by a powerful home-state Senator. Consistent with this explanation, we find that the largest reductions in credit access occur in Census tracts where regulatory guidelines, such as the Community Reinvestment Act, are most likely to cause additional lending. We also find that the largest contractions in credit to disadvantaged borrowers occur in areas that are politically unengaged, while the effects are amplified in regions with a large proportion of politically-connected banks. Additionally, we find that the applicants who receive credit following political power shocks tend to be of higher observable credit quality than the applicants who receive credit prior to political power shocks. Finally, we find that banks become more profitable following these shocks. Collectively, these results suggest that increased political power causes lenders to tighten screening standards in a manner that reduces credit provision to disadvantaged borrowers.

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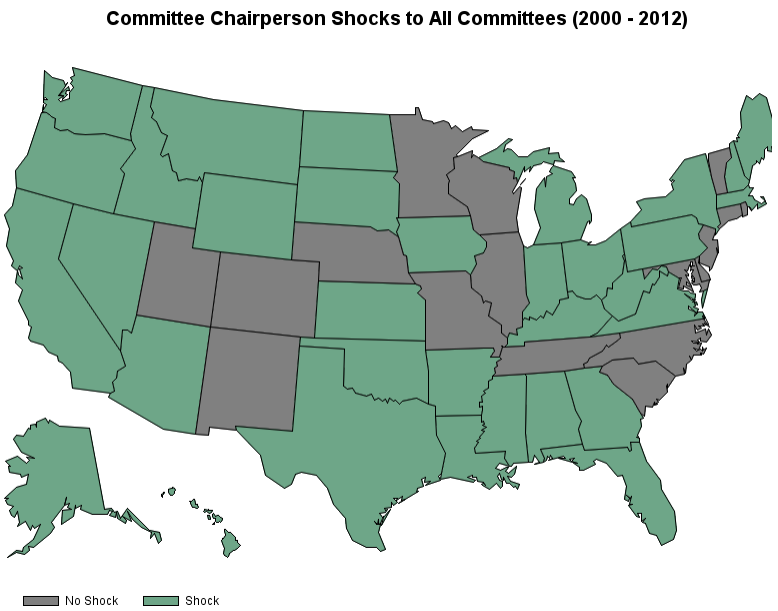
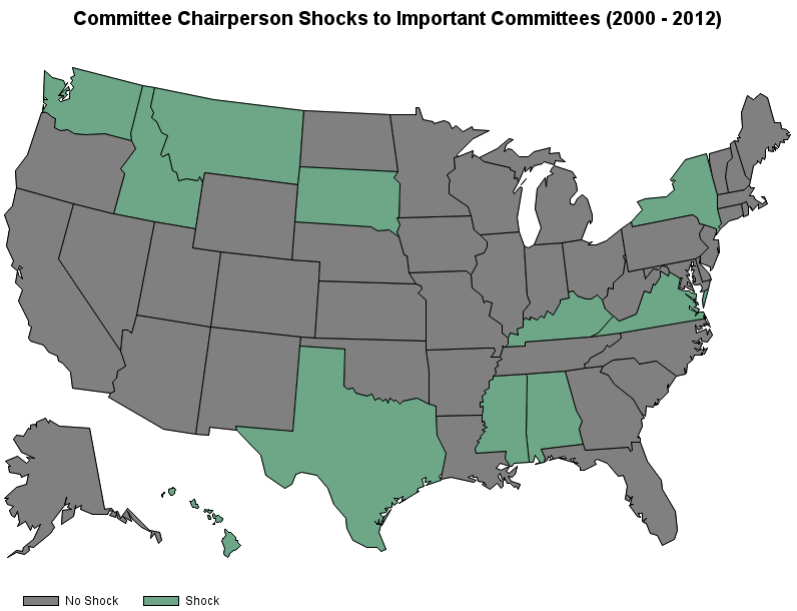


(a)



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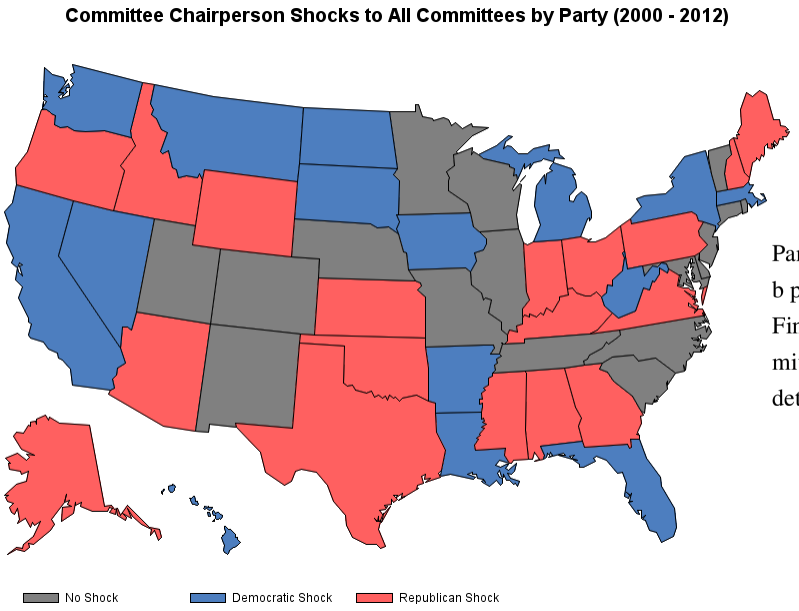


(c)

Panel a presents the state distribution of our primary shock measure. Panel b presents the distribution of shocks to "important" committees (the Senate Finance, Appropriations, Veteran's, Armed Services, Rules, and Banking Committees). Panel c presents the shock distribution by political party. See text for details about the shock construction.

Figure 1: **Chairperson Shock Distribution**

Figure 2: **Subprime Lending Before and After Senate Chairperson Shocks**

This figure presents fitted estimates of *supply ratio*, the number of new credit lines divided by the number of hard credit inquiries on the consumer's credit report, regressed on leads and lags of *Powerful Politician*, a variable equal to one if the consumer's home-state Senator ascends to chair of a powerful Senate committee. Majority white (minority) Census tracts are at least (less than) fifty percent white/caucasian according to the 2000 U.S. Census. The panel regression also includes fixed effects for the current quarter and the consumer's Census tract. The sample includes consumers with Equifax riskscores less than 640. 95% prediction intervals are calculated using standard errors clustered by state.

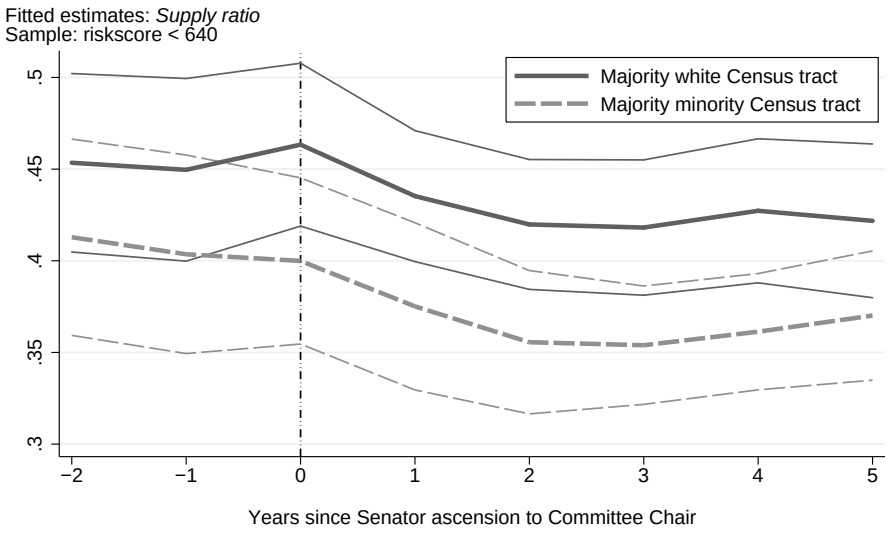
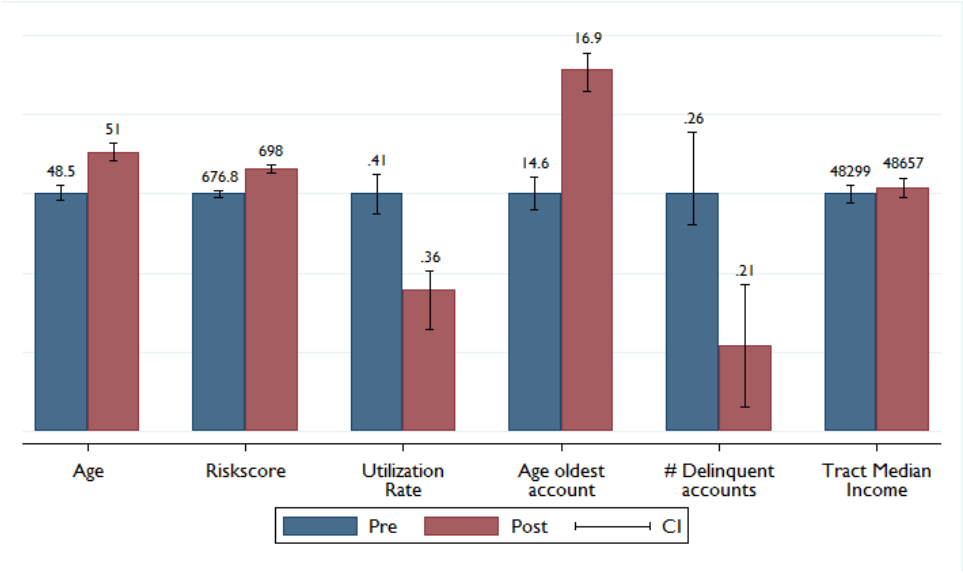


Figure 3: **Borrower Characteristics for New Credit Lines**

This figure presents the characteristics of borrowers who receive at least one new credit line in the two years before (after) the ascension of a home-state Senator to a powerful committee chair. **Panel (a)** shows borrowers in majority white Census tracts and **Panel (b)** shows borrowers in majority minority Census tracts.

(a) **New Loan Recipients in Majority White Census Tracts**



(b) **New Loan Recipients in Majority Minority Census Tracts**

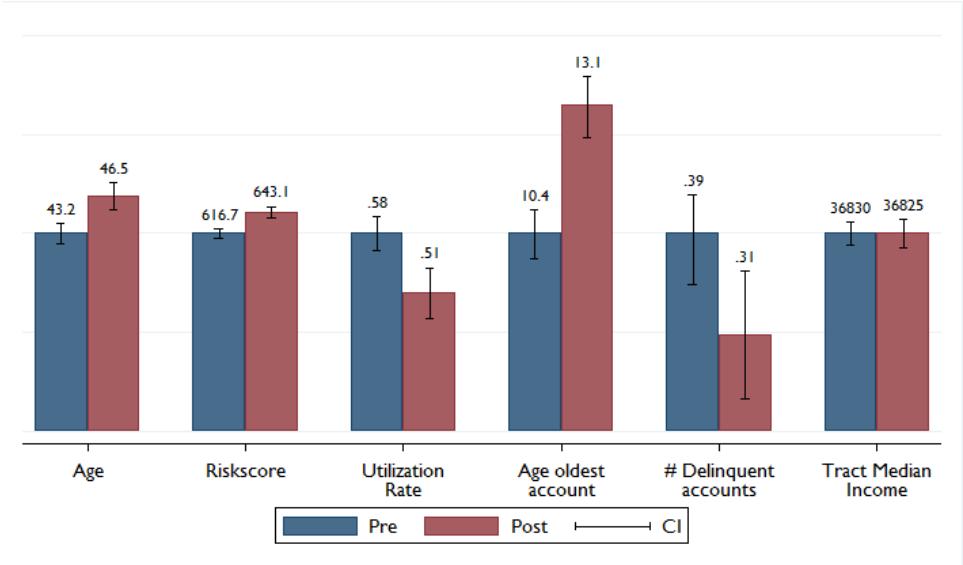


Figure 4: **New Account Delinquencies**

This figure shows the fraction of credit accounts that are delinquent for borrowers who receive at least one new line of credit in the two years before (after) the ascension of a home-state Senator to a powerful committee chair. The figure presents fitted estimates (and 95% prediction intervals calculated using standard errors clustered by state) of an OLS panel regression that includes fixed effects for the consumer's Census tracts. The dependent variable *Frac. delinquent accounts* equals the number of accounts at least 90 days past due over the total number of credit lines on the consumer's credit report.

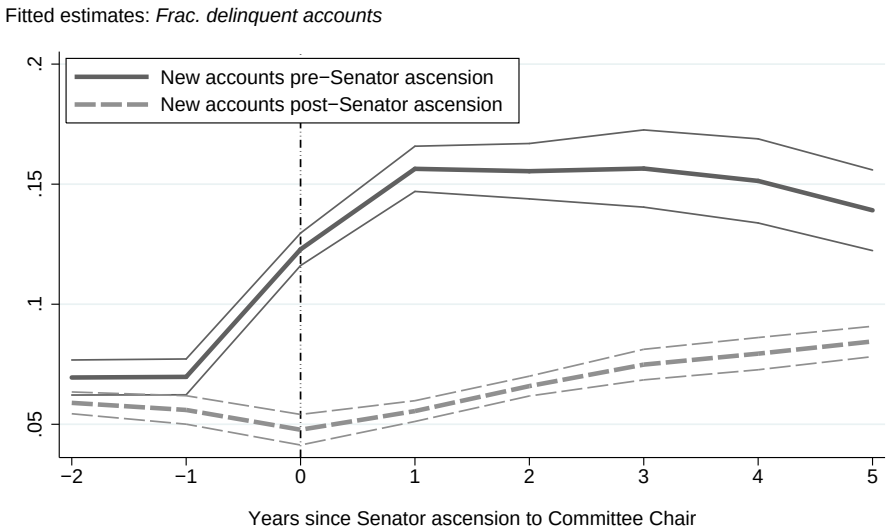


Table 1: **Summary Statistics**

The following table presents summary statistics for our main variables of interest. Panel A presents results for the entire sample, while Panel B restricts the sample to those consumers with a Riskscore <640.

Panel A	*Sample:* All consumers			
	Mean	Median	Std. Dev.	N (Consumer-Qtr)
$Supply\ Ratio$	0.718	0.5	0.892	2,777,537
$Riskscore$	689.9	710	106.1	5,145,204
$Utilization\ Rate$	0.372	0.205	0.377	3,626,566
$Fraction\ Delinquent$	0.0855	0	0.243	4,588,929
$\#\ New\ Accounts$	0.987	0	1.465	5,138,615
$\#\ Inquiries$	2.17	1	2.556	3,544,134
$Powerful\ Politician$	0.0993	0	0.299	5,145,204
$Majority\ Minority$	0.135	0	0.342	4,893,271
$Median\ Income$	47,954.5	43,709	20,591.4	4,892,375
$Senate\ Contributions$	180,133	44,750	738,924.9	5,101,753
$Connected\ Branches$	0.294	0.316	0.139	5,084,186
$Connected\ Deposits$	0.22	0.226	0.115	5,084,186

Panel B	*Sample:* Consumer Riskscore $<$ 640			
	Mean	Median	Std. Dev.	N (Consumer-Qtr)
$Supply\ Ratio$	0.454	0.182	0.745	1,142,132
$Riskscore$	557.4	570	61.72	1,605,280
$Utilization\ Rate$	0.81	0.948	0.273	866,878
$Fraction\ Delinquent$	0.271	0	0.373	1,336,726
$\#\ New\ Accounts$	1.005	0	1.651	1,598,917
$\#\ Inquiries$	2.993	2	3.274	1,362,094
$Powerful\ Politician$	0.102	0	0.303	1,605,280
$Majority\ Minority$	0.218	0	0.413	1,514,597
$Median\ Income$	41,502.5	38,434	16,679.6	1,514,250
$Senate\ Contributions$	117,062.2	34,300	435,210.2	1,589,661
$Connected\ Branches$	0.298	0.318	0.141	1,580,929
$Connected\ Deposits$	0.224	0.232	0.117	1,580,929

Table 2: **Chairperson Shock Summary Statistics**

The following table present the distribution by election cycle for our shocks to politicians for our main shock definition. Panel A presents statistics on the frequency of shocks per election cycle. Column (1) presets the number of shocks for all politicians while Column (2) shows the shocks to "important" committees, which are the Senate Finance, Armed Services, Appropriations, Rules, Veteran's Affairs, and Banking Committees. Panel B provides details about the events that led to the shocks.

Panel A — Number of Shocks by Year		
	(1)	(2)
Sample	All Committees	Important Committees
1999–2000	5	3
2001–2002	4	1
2003–2004	7	2
2005–2006	9	2
2007–2008	1	2
2009–2010	8	0
2011–2012	4	2
Total Shocks	38	12

Panel B — Reasons for Shocks	
Reason for Change in Chairmanship	Number
Previous Chair Changed Committees	14
Change in Control of Congress	11
Previous Chair Left Office	7
Previous Chair had Health Problems	4
Other	2
Total Shocks	38

Table 3: **Powerful Politicians and Access to Credit**

This table uses OLS regressions to test the effect of a Senator's ascension to a powerful committee chair on the supply of consumer credit and the number of credit inquiries. It uses data from the FRBNY-CCP, a representative panel of individual credit records from Equifax. The sample period is years 1999 – 2012. $Supply\,ratio$ equals the number of new credit lines divided by the number of credit inquiries in the consumer's credit report. $Powerful\,Politician$ equals one in the two years following ascension, zero otherwise. $Majority\,Minority$ equals one if the consumer lives in a Census tract that is majority non-white, zero otherwise. Other variables are described in the text. The sample includes borrowers with an Equifax Riskscore less than or equal to 640. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Dependent variable:	Supply ratio					
Sample:	Consumer Riskscore < 640					
	(1)	(2)	(3)	(4)	(5)	(6)
$Powerful\,Politician$	-0.0147*	-0.0140*			-0.0191***	-0.0190***
	(0.0074)	(0.0072)			(0.0071)	(0.0071)
$Powerful\,Politician \times Majority\,Minority$	-0.0225**	-0.0208**	-0.0172**	-0.0209**	-0.0130*	-0.0130*
	(0.0085)	(0.0086)	(0.0083)	(0.0095)	(0.0075)	(0.0075)
$Majority\,Minority$					-0.00422	-0.00234
					(0.0066)	(0.0069)
$Consumer\,Riskscore/100$		0.0930***	0.0934***	0.0931***	0.0665***	0.0665***
		(0.0038)	(0.0038)	(0.0036)	(0.0042)	(0.0042)
$Census\,Tract\,Median\,Income\,(Z)$						0.00278
						(0.0032)
Date - quarter FE	x	x			x	x
Census tract FE	x	x	x	x		
State$\times$ time FE			x			
County$\times$ time FE				x		
Consumer FE					x	x
N	1,077,773	1,077,773	1,036,546	1,036,546	1,074,941	1,074,678
Adj. R^2	0.19	0.19	0.2	0.2	0.26	0.26

Table 4: **Household Campaign Contributions and Credit Provision**

This table uses OLS regressions to test the marginal effect of campaign contributions on the relation between a Senator's ascension to a powerful committee chair and the supply of consumer credit (**Panel A**). The data, the sample, and the variables are described in **Table 3**. In columns (1) and (2), the sample includes ZIP codes in which personal campaign contributions to Senators are above the median (within-state). The sample in columns (3) and (4) includes ZIP codes that are below the median. Campaign contributions data comes from the Federal Elections Commission. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Panel A				
Dependent variable:	Supply ratio			
Sample:	Consumer Riskscore < 640			
Campaign contributions in ZIP code:	Above median		Below median	
	(1a)	(2a)	(3a)	(4a)
$Powerful\,Politician$	-0.0106	-0.0102	-0.0189**	-0.0180**
	(0.0096)	(0.0093)	(0.0073)	(0.0071)
$Powerful\,Politician \times Majority\,Minority$	-0.0159	-0.0156	-0.0251***	-0.0229***
	(0.014)	(0.014)	(0.0078)	(0.0081)
$Consumer\,Riskscore/100$		0.0980***		0.0869***
		(0.0040)		(0.0041)
Year - quarter FE	x	x	x	x
Census tract FE	x	x	x	x
N	491,986	491,986	584,987	584,987
Adj. R^2	0.20	0.20	0.19	0.20

Table 5: **Politically Connected Banks and Credit Provision**

This table uses OLS regressions to test the marginal effect of political connections to banks on the relation between a Senator's ascension to a powerful committee chair and the supply of consumer credit. In this table, we sort counties by the fraction of bank branches that have connections to Senators that ascend to a chair of a powerful Senate Committee. We call a bank connected when it has made a contribution to the Senator's election campaign. In **Panel A**, we measure the fraction of banks in a county that are connected to a politician, weighting banks by the size of their deposits. Banks are equally-weighted in **Panel B**. In columns (1) and (2), the sample includes counties that are above the median fraction in the state. The sample in columns (3) and (4) is below the median. Campaign contributions made by financial institutions comes from the Federal Election Commission. Bank branch data comes from the FDIC Summary of Deposits. The other data, the sample, and the variables are described in **Table 3**. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Panel A				
Dependent variable:	Supply ratio			
Sample:	Consumer Riskscore < 640			
Deposit-weighted fraction of politically connected branches:	County is above median		County is below median	
	(1a)	(2a)	(3a)	(4a)
$Powerful\ Politician$	-0.0176	-0.0161	-0.0128	-0.0131
	(0.011)	(0.0097)	(0.011)	(0.011)
$Powerful\ Politician \times Majority\ Minority$	-0.0239**	-0.0231**	-0.00188	-0.000351
	(0.0091)	(0.0087)	(0.019)	(0.019)
$Consumer\ Riskscore/100$		0.0901***		0.0963***
		(0.0041)		(0.0046)
Year - quarter FE	x	x	x	x
Census tract FE	x	x	x	x
N	566,813	566,813	510,960	510,960
Adj. R^2	0.17	0.18	0.20	0.21
Panel B				
Dependent variable:	Supply ratio			
Sample:	Consumer Riskscore < 640			
Equally-weighted fraction of politically connected branches:	County is above median		County is below median	
	(1b)	(2b)	(3b)	(4b)
$Powerful\ Politician$	-0.0133	-0.0124	-0.0179*	-0.0175*
	(0.011)	(0.010)	(0.0094)	(0.0098)
$Powerful\ Politician \times Majority\ Minority$	-0.0272***	-0.0259***	0.00181	0.00295
	(0.0094)	(0.0092)	(0.016)	(0.016)
$Consumer\ Riskscore/100$		0.0891***		0.0976***
		(0.0037)		(0.0050)
Year - quarter FE	x	x	x	x
Census tract FE	x	x	x	x
N	568,823	568,823	508,950	508,950
Adj. R^2	0.17	0.18	0.20	0.20

Table 6: **Credit Provision Under the Community Reinvestment Act and Powerful Politicians**

This table uses OLS regressions to test the effect of a Senator's ascension to a powerful committee chair on the supply of consumer lending that is likely to be affected by regulatory guidelines from the Community Reinvestment Act (CRA). The CRA encourages banks to relax screening standards on loan applications from households living in Census tracts with average incomes less than 80% of the median income in the MSA. This table sorts the data by the ratio of Census tract average income to MSA median income. Columns (1) and (2) uses all Census tracts. Census tracts with a ratio of tract to MSA income between 60% and 100% are in columns (3) and (4), 80% to 120% are in Columns (5) and (6), and 40% to 80% are in columns (7) and (8). $CRA\,Eligible\,(I < 80\%)$ equals one if the consumer resides in a Census tract with ratio tract income to MSA income less than 80%. $CRA\,Placebo\,(I < 100\%)$ and $CRA\,Placebo\,(I < 60\%)$ are placebo thresholds for CRA eligibility that equal one if the consumer resides in a Census tract with ratio tract income to MSA income less than 100% and 60%, respectively. The data, the sample, and other variables are described in **Table 3**. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Dependent variable:	Supply ratio							
Sample:	Consumer Riskscore < 640							
Census tract average income / MSA median income:	Full Sample		60 – 100%		80 – 120%		40 – 80%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Powerful\,Politician$	-0.0164*	-0.0155*	-0.0155	-0.0150	-0.0257	-0.0239	-0.0349***	-0.0327***
	(0.0084)	(0.0080)	(0.011)	(0.011)	(0.016)	(0.015)	(0.010)	(0.0095)
$Powerful\,Politician \times CRA\,Eligible\,(I < 80\%)$	-0.0166**	-0.0159**	-0.0184**	-0.0166*				
	(0.0076)	(0.0078)	(0.0087)	(0.0091)				
$Powerful\,Politician \times CRA\,Placebo\,(I < 100\%)$					0.0105	0.00952		
					(0.014)	(0.014)		
$Powerful\,Politician \times CRA\,Placebo\,(I < 60\%)$							0.00175	-0.000826
							(0.017)	(0.017)
$Consumer\,Riskscore/100$		0.0943***		0.0850***		0.0973***		0.0729***
		(0.0037)		(0.0056)		(0.0043)		(0.0066)
Year - quarter FE	x	x	x	x	x	x	x	x
Census tract FE	x	x	x	x	x	x	x	x
N	875,566	875,566	376,315	376,315	396,031	396,031	237,729	237,729
Adj. R^2	0.18	0.18	0.17	0.18	0.17	0.18	0.18	0.18

Table 7: **Powerful Politicians and CRA Regulatory Exams**

This table uses ordered logit regressions to test the relation between a Senator's ascension to a powerful committee chair and subsequent CRA regulatory exam performance for banks headquartered in the politican's home state. The data on CRA exams comes from the Federal Financial Institutions Examination Council. The dependent variable, *CRA exam rating* equals 4 for "outstanding", 3 for "satisfactory", 2 for "needs to improve", and 1 for "substantial noncompliance". Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Ordered logit dep. var.:	CRA exam rating			
Sample:	All bank exams	All bank exams	Large bank exams	Small bank exams
	(1)	(2)	(3)	(4)
Powerful Politician	-0.165*	-0.169*	-0.178	-0.219**
	(0.095)	(0.094)	(0.13)	(0.10)
Year FE	x	x	x	x
State FE	x	x	x	x
Exam method FE		x		
N	33,412	33,412	6,093	22,287
pseudo R^2	0.040	0.053	0.051	0.049

Table 8: **Bank Profitability and Powerful Politicians**

This table documents the effect of a Senator's ascension shock on bank return on assets using OLS regression. $Powerful\ Politician$ is a binary variable that takes the value of one in the two years following an ascension shock and zero otherwise. Details of the variable construction are provided in the text. $Connected\ Bank$ is a binary variable that takes the value of one if the bank made political contributions in a given time period and zero otherwise. Columns (1) – (3) present the analysis on the full sample of banks while columns (4) – (6) present the analysis for banks that operate in a single state only. Quarterly bank ROA data comes from Call Reports data, campaign contributions data comes from the Federal Election commission and bank location data comes from the Summary of Deposits data. Quarterly ROA values are annualized and are multiplied by 100. Other variables are described in the text. Standard errors are clustered at the state level are presented in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels respectively.

Dependent Variable:	Bank ROA (Annualized)					
Sample:	All Banks			Single-State Banks		
	(1)	(2)	(3)	(4)	(5)	(6)
$Powerful\ Politician$	0.0606**	0.0544*	0.0522*	0.0640**	0.0583**	0.0571*
	(0.0283)	(0.0275)	(0.0281)	(0.0302)	(0.0291)	(0.0294)
$Connected\ Bank$			-0.4258***			-0.4770***
			(0.0655)			(0.1178)
$Powerful\ Politician \times Connected$			0.0636			0.0837
			(0.0434)			(0.0881)
$LnAssets$		0.6834***	0.6841***		0.8213***	0.8235***
		(0.0573)	(0.0573)		(0.0684)	(0.0682)
Year FE	x	x	x	x	x	x
State FE	x	x	x	x	x	x
Bank FE	x	x	x	x	x	x
N	502,237	502,237	502,237	442,744	442,744	442,744
Adj. R^2	0.535	0.555	0.555	0.536	0.563	0.564

Table 9: **Credit Constraints and Political Parties**

This table uses OLS regressions to test the effect of a Senator's political affiliation and the relation between the Senator's ascension to a powerful committee chair and the supply of consumer lending. The data, the sample, and other variables are described in **Table 3**. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Dependent variable:	Supply ratio							
Sample:	Consumer Riskscore < 640							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Powerful\ Republican$	-0.00354	-0.00432	-0.00871	-0.00864				
	(0.012)	(0.012)	(0.011)	(0.011)				
$Powerful\ Republican \times Majority\ Minority$	-0.0269*	-0.0252*	-0.0372***	-0.0371***				
	(0.014)	(0.014)	(0.012)	(0.012)				
$Powerful\ Democrat$					-0.0170*	-0.0152	-0.0180	-0.0179
					(0.0092)	(0.0091)	(0.011)	(0.011)
$Powerful\ Democrat \times Majority\ Minority$					-0.0264***	-0.0249***	-0.00558	-0.00572
					(0.0074)	(0.0074)	(0.0075)	(0.0075)
$Majority\ Minority$			-0.00400	-0.00215			-0.00520	-0.00332
			(0.0061)	(0.0064)			(0.0067)	(0.0070)
$Consumer\ Riskscore/100$		0.0931***	0.0666***	0.0666***		0.0930***	0.0665***	0.0665***
		(0.0038)	(0.0042)	(0.0042)		(0.0038)	(0.0042)	(0.0042)
$Census\ Tract\ Median\ Income\ (Z)$				0.00275				0.00277
				(0.0031)				(0.0032)
Year - quarter FE	x	x	x	x	x	x	x	x
Census tract FE	x	x			x	x		
Consumer FE			x	x			x	x
N	1,077,773	1,077,773	1,074,941	1,074,678	1,077,773	1,077,773	1,074,941	1,074,678
Adj. R^2	0.19	0.19	0.26	0.26	0.19	0.19	0.26	0.26

Table 10: **Powerful Politicians and Aggregate Lending Portfolios**

This table documents the effect of a Senator's ascension shock on lending using OLS regression. $Powerful\ Politician$ is a binary variable that takes the value of one in the two years following an ascension shock and zero otherwise, details of the variable construction are provided in the text. $Connected\ Bank$ is a binary variable that takes the value of one if the bank made political contributions in a given time period and zero otherwise. Panels A and B present the analysis on log-levels of different categories of bank lending, while Panels C and D present analysis of the fraction of total lending across different categories of bank lending. Panels A and C present the analysis on the full sample of banks while Panels B and D present the analysis for banks that operate in a single state only. Bank lending data comes from Call Reports data, campaign contributions data comes from the Federal Election commission and bank location data comes from the Summary of Deposits data. Other variables are described in the text. Standard errors are clustered at the state level are presented in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels respectively.

Panel A — Loan Amounts, All Banks

	(1)	(2)	(3)	(4)	(5)	(6)
	$Ln\ Secured\ Real\ Estate$	$Ln\ Commercial\ and\ Industrial$	$Ln\ Consumer$	$Ln\ Secured\ Real\ Estate$	$Ln\ Commercial\ and\ Industrial$	$Ln\ Consumer$
$Powerful\ Politician$	-0.00790	0.0265	-0.00436	-0.00912	0.0288	-0.00339
	(0.00927)	(0.0166)	(0.0128)	(0.00939)	(0.0173)	(0.0135)
$Connected\ Bank$				-0.0474**	-0.0789***	-0.145***
				(0.0233)	(0.0246)	(0.0500)
$Powerful\ Politician \times Connected$				0.0289**	-0.0470	-0.0143
				(0.0144)	(0.0344)	(0.0473)
$Bank\ Size$	1.141***	0.996***	0.811***	1.141***	0.995***	0.811***
	(0.0132)	(0.0175)	(0.0260)	(0.0131)	(0.0175)	(0.0260)
Year FE	x	x	x	x	x	x
State FE	x	x	x	x	x	x
Bank FE	x	x	x	x	x	x
N	497,954	490,086	496,014	497,954	490,086	496,014
Adj. R^2	0.984	0.957	0.954	0.984	0.957	0.954

Panel B — Loan Amounts, Single-State Banks

	(1)	(2)	(3)	(4)	(5)	(6)
	$Ln\ Secured\ Real\ Estate$	$Ln\ Commercial\ and\ Industrial$	$Ln\ Consumer$	$Ln\ Secured\ Real\ Estate$	$Ln\ Commercial\ and\ Industrial$	$Ln\ Consumer$
$Powerful\ Politician$	-0.0124	0.0383**	-0.00548	-0.0137	0.0390**	-0.00393
	(0.0101)	(0.0174)	(0.0108)	(0.0102)	(0.0174)	(0.0109)
$Connected\ Bank$				0.0271	-0.181*	-0.230**
				(0.0543)	(0.0955)	(0.0949)
$Powerful\ Politician \times Connected$				0.0998	-0.0320	-0.104*
				(0.0780)	(0.110)	(0.0528)
$Bank\ Size$	1.196***	1.052***	0.878***	1.196***	1.053***	0.879***
	(0.0270)	(0.0226)	(0.0419)	(0.0271)	(0.0227)	(0.0421)
Year FE	x	x	x	x	x	x
State FE	x	x	x	x	x	x
Bank FE	x	x	x	x	x	x
N	263,986	258,349	262,914	263,986	258,349	262,914
Adj. R^2	0.958	0.899	0.908	0.958	0.899	0.908

Table 10: **Powerful Politicians and Aggregate Lending Portfolios (Continued)**

Panel C — Loan Fraction, All Banks						
	(1) $\frac{Real\ Estate}{Total\ Loans}$	(2) $\frac{Commercial}{Total\ Loans}$	(3) $\frac{Consumer}{Total\ Loans}$	(4) $\frac{Real\ Estate}{Total\ Loans}$	(5) $\frac{Commercial}{Total\ Loans}$	(6) $\frac{Consumer}{Total\ Loans}$
$Powerful\ Politician$	-0.00541	-0.00258	0.00325	-0.00582*	-0.00246	0.00340
	(0.00338)	(0.00453)	(0.00225)	(0.00345)	(0.00461)	(0.00226)
$Connected\ Bank$				-0.000928	-0.0145	-0.00551
				(0.00518)	(0.00882)	(0.00454)
$Powerful\ Politician \times Connected$				0.00881**	-0.00202	-0.00295
				(0.00439)	(0.00836)	(0.00430)
Year FE	x	x	x	x	x	x
State FE	x	x	x	x	x	x
Bank FE	x	x	x	x	x	x
N	501,585	501,585	500,787	501,585	501,585	500,787
Adj. R^2	0.888	0.751	0.868	0.888	0.751	0.868

Panel D — Loan Fraction, Single State Banks						
	(1) $\frac{Real\ Estate}{Total\ Loans}$	(2) $\frac{Commercial}{Total\ Loans}$	(3) $\frac{Consumer}{Total\ Loans}$	(4) $\frac{Real\ Estate}{Total\ Loans}$	(5) $\frac{Commercial}{Total\ Loans}$	(6) $\frac{Consumer}{Total\ Loans}$
$Powerful\ Politician$	-0.00641*	0.00190	0.00149	-0.00648*	0.00201	0.00173
	(0.00322)	(0.00423)	(0.00184)	(0.00325)	(0.00426)	(0.00178)
$Connected\ Bank$				-0.000928	-0.0145	-0.00551
				0.0115	-0.0106	-0.0285**
				(0.00899)	(0.0291)	(0.0138)
$Powerful\ Politician \times Connected$				0.00493	-0.00723	-0.0158
				(0.00896)	(0.0174)	(0.0122)
Year FE	x	x	x	x	x	x
State FE	x	x	x	x	x	x
Bank FE	x	x	x	x	x	x
N	267,193	267,193	266,395	267,193	267,193	266,395
Adj. R^2	0.905	0.773	0.894	0.905	0.773	0.894

Table 11: **Powerful Politicians and Average Consumer Credit Outcomes**

This table uses OLS regressions to test the effect of a Senator's ascension to a powerful committee chair on consumer credit outcomes in the Senator's home-state. The data, the sample, and variables are described in **Table 3** or in the text. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Sample:	All consumers					
Dependent variable:	Utilization rate	# Inquiries	# Accounts	Supply ratio	Riskscore	Fr. Delinquent
	(1)	(2)	(3)	(4)	(5)	(6)
$Powerful\ Politician$	0.00258	0.00255*	-0.00397	-0.0168***	-0.642	0.000143
	(0.0031)	(0.0015)	(0.0032)	(0.0061)	(0.64)	(0.0015)
Year - quarter FE	x	x	x	x	x	x
Census tract FE	x	x	x	x	x	x
N	3,625,669	3,693,247	5,138,800	2,776,905	5,145,204	4,588,736
Adj. R^2	0.32	0.041	0.087	0.15	0.37	0.22

Appendix to: **Politicizing Consumer Credit**

Intended for online publication only

Figure A.1: **Placebo test of Senator Power and Consumer Credit Constraints**

This figure presents placebo estimates of the effect of a home-state Senator's ascension to a powerful committee chair on the supply of consumer credit. The regression is described in **Table 3**. The dependent variable is *supply ratio*. The placebo test randomly assigns senators to committee chairs in different years (each one of the 100 iterations contains the same total number of ascensions that occur over our sample period). The top panel presents histograms of the *t*-stats (standard errors clustered by state) on the coefficient estimates of *Powerful Politician* and of *Powerful Politician* × *Majority Minority*. The bottom panel presents, for both placebo variables, a P-P plot, a graphical test of normality of the distribution.

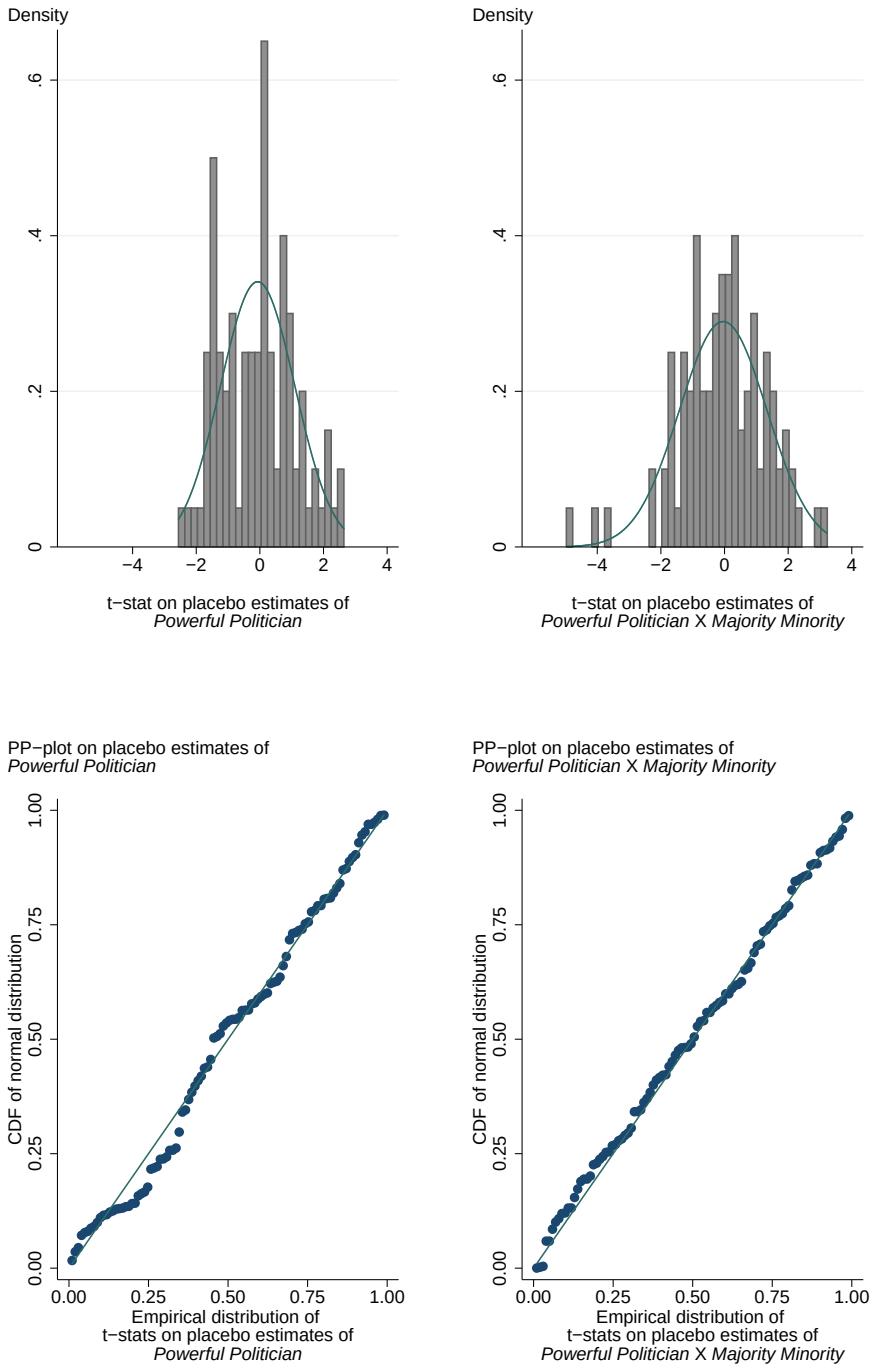
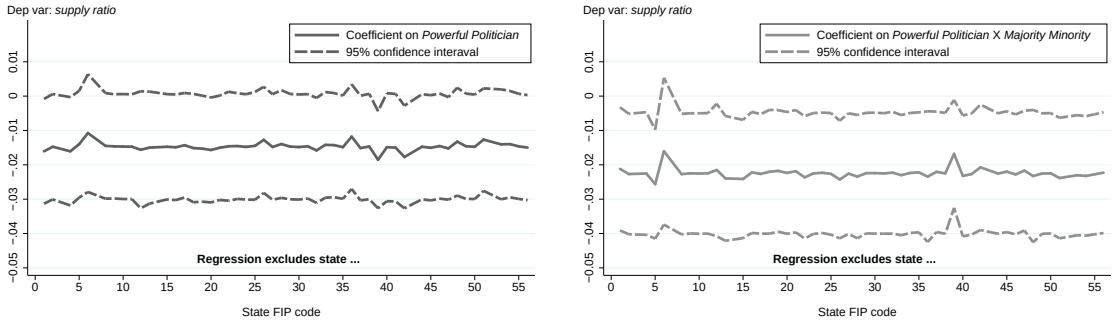


Figure A.2: **Sensitivity of Relation Between Senator Power and Consumer Credit Constraints**

This figure presents regression estimates of the effect of a home-state Senator's ascension to a powerful committee chair on the supply of consumer credit. The regression is described in **Table 3**. The dependent variable is *supply ratio*. **Panel A** excludes one state in each iteration of the regression. **Panel B** excludes one year in each iteration of the regression.

Panel A: Sensitivity across states



Panel B: Sensitivity across years

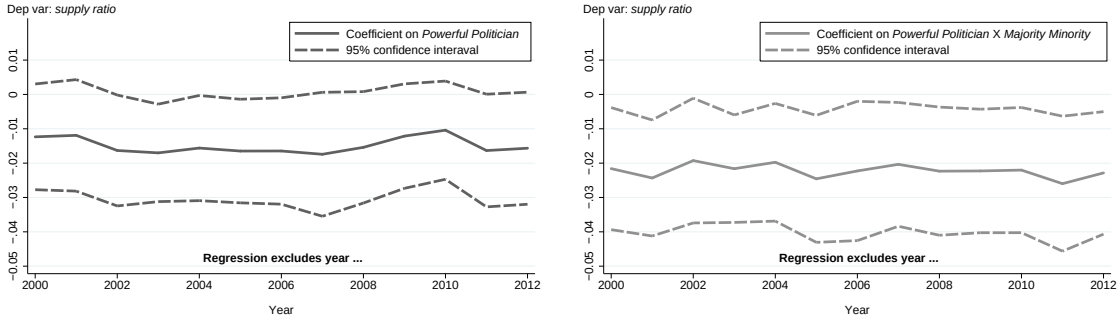
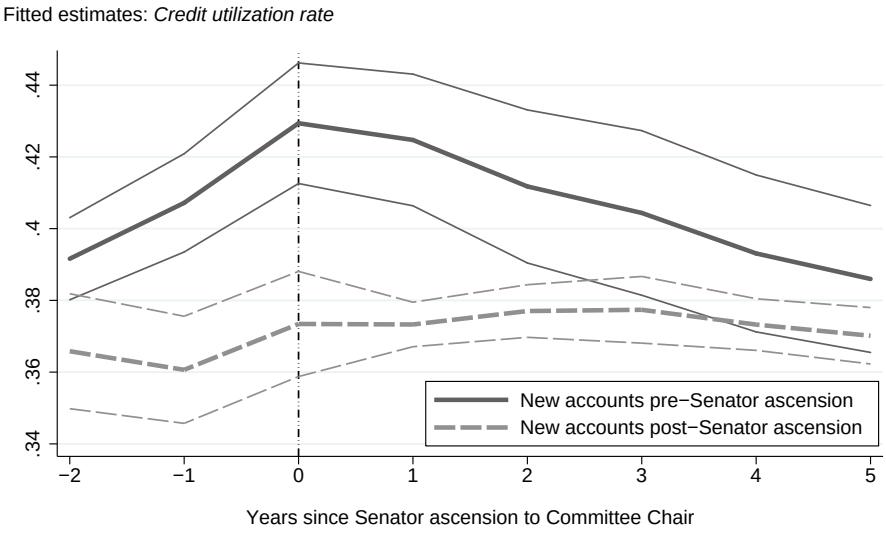


Figure A.3: **New Accounts and Borrower Dynamics**

This figure shows credit utilization rates (top figure) and riskscores (bottom figure) for borrowers who receive at least one new line of credit in the two years before (after) the ascension of a home-state Senator to a powerful committee chair. The figure presents fitted estimates (and 95% prediction intervals calculated using standard errors clustered by state) of an OLS panel regression that includes fixed effects for the consumer's Census tracts.



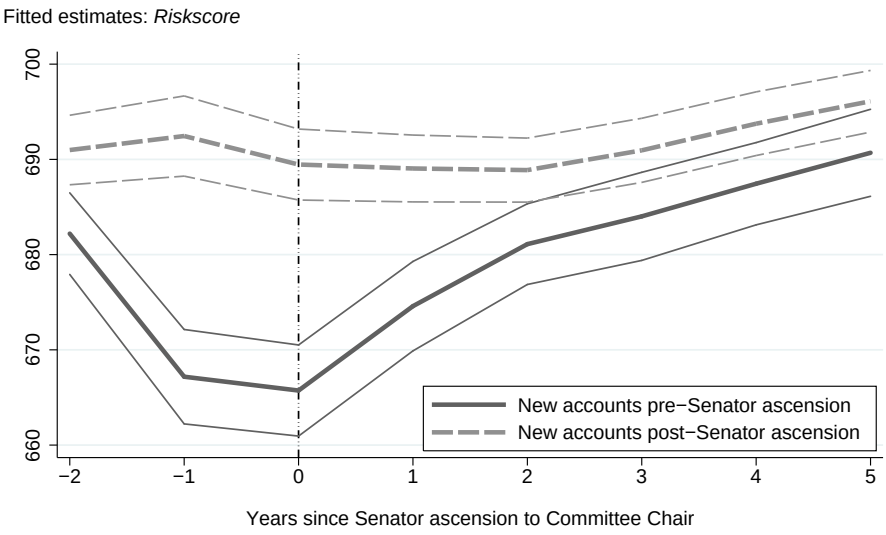


Table A.1: **Senator Shocks and State Macroeconomic Conditions**

This table uses OLS regressions to test the effect of a Senator's ascension to chair a Senate committee on macroeconomic indicators in the Senator's home state. $Powerful\ Politician$ is a binary variable that takes the value of one in the two years following an ascension shock and zero otherwise. The depdendent variables are state-level measures of macroeconomic conditions. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels respectively.

Panel A — Macroeconomic Variables and Political Shocks							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable:	Ln(GDP)	Ln(Personal Income)	Ln(Employ-ment)	Ln(Disposable Income)	Unemply-ment Rate	Ln(House Price Index)	Ln(Bank-ruptcies)
$Powerful\ Politician$	0.000404	-0.00262	-0.00450	-0.00392	-0.161	0.00582	-0.0157
	(0.00612)	(0.00523)	(0.00418)	(0.00496)	(0.130)	(0.0125)	(0.0463)
Year FE	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X
Observations	700	700	700	700	700	700	700
Within R-squared	5.44e-06	0.000443	0.00239	0.00103	0.00405	0.000454	0.000243

Panel B — Lagged Macroeconomic Variables and Political Shocks									
Dependent Variable: $Powerful\ Politician$									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Lag\ Log(GDP)$	-0.198							0.101	-0.129
	(0.231)							(0.474)	(0.506)
$Lag\ Log(Personal$		-0.385						-0.275	1.983
$Income)$		(0.316)						(0.835)	(3.195)
$Lag\ Log(Employment)$			-0.580					-0.420	-0.414
			(0.428)					(0.679)	(0.695)
$Lag\ Log(Disposable$				-0.479					-2.598
$Income)$				(0.324)					(3.252)
$Lag\ Log(Unemployment$					-0.0332				-0.0459**
$Rate$					(0.0201)				(0.0217)
$Lag\ Log(House$						0.0571			0.120
$Price\ Index$						(0.187)			(0.218)
$Lag\ Log(Bankruptcies)$							-0.0339		0.00111
							(0.0383)		(0.0427)
Year FE	X	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X	X
Observations	650	650	650	650	650	650	650	650	650
Within R-squared	0.00111	0.00219	0.00276	0.00326	0.00584	0.000246	0.00115	0.00299	0.0177

Table A.1: **Senator Shocks and State Macroeconomic Conditions Continued**

Panel C — Changes in Macroeconomic Variables and Political Shocks									
Dependent Variable: $Powerful\ Politician$									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\Delta Log(GDP)$	-1.51e-06							1.06e-06	1.39e-06
	(2.08e-06)							(2.79e-06)	(2.43e-06)
$\Delta\ Log(Personal$		-2.72e-06						-1.76e-06	-1.17e-06
$Income)$		(2.18e-06)						(2.45e-06)	(7.43e-06)
$\Delta\ Log(Employment)$			-0.000481					-0.000412	-0.000417
			(0.000343)					(0.000305)	(0.000289)
$\Delta\ Log(Disposable$				-3.90e-06					-6.33e-07
$Income)$				(4.47e-06)					(9.80e-06)
$\Delta Unemployment$					0.00129				-0.0254
$Rate$					(0.0311)				(0.0287)
$\Delta\ Log(House$						-0.00140			-0.00109
$Price\ Index$						(0.00178)			(0.00147)
$\Delta\ Log(Bankruptcies)$							5.77e-05		1.65e-05
							(6.47e-05)		(4.99e-05)
Year FE	X	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X	X
Observations	650	650	650	650	650	650	650	650	650
Within R-squared	0.00273	0.00573	0.00824	0.00463	4.30e-06	0.00489	0.00407	0.00888	0.0127

Table A.2: **"Important" Committees and Credit Constraints**

This table uses OLS regressions to test the effect of a Senator's ascension to an "important" committee (the Senate Finance, Appropriations, Veteran's, Armed Services, Rules, or Banking Committee) on the supply of consumer credit. $Important\ Politician$ is a binary variable that takes the value of one in the two years following an ascension shock from one of these committees and zero otherwise. Otherwise, the table is identical to **Table 3**, **Panel B**. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Dependent variable:	Supply ratio			
Sample:	Consumer Riskscore < 640			
	(1)	(2)	(3)	(4)
$Important\ Politician$	-0.00979	-0.0101	-0.00284	-0.00278
	(0.015)	(0.015)	(0.016)	(0.016)
$Important\ Politician \times Majority\ Minority$	-0.0241***	-0.0237***	-0.0302***	-0.0302***
	(0.0060)	(0.0062)	(0.010)	(0.010)
$Majority\ Minority$	-	-	-0.00406	-0.00221
	-	-	(0.0061)	(0.0063)
$Consumer\ Riskscore/100$		0.0931***	0.0666***	0.0666***
		(0.0038)	(0.0042)	(0.0042)
$Census\ Tract\ Median\ Income\ (Z)$				0.00275
				(0.0032)
Year - quarter FE	x	x	x	x
Census tract FE	x	x		
Consumer FE			x	x
N	1,077,773	1,077,773	1,074,941	1,074,678
Adj. R^2	0.19	0.19	0.26	0.26

Table A.3: **Powerful Politicians within Committee and Credit Constraints**

This table uses OLS regressions to test the effect of a Senator's ascension to a powerful committee chair on the supply of consumer credit. The sample is restricted to include observations only with consumers in states that had Senators from the majority party that were members of a committee where an ascension event occurred (i.e., only those consumers that could have been shocked). It uses data from the FRBNY-CCP, a representative panel of individual credit records from Equifax. The sample period is years 1999 – 2012. $Supply\,ratio$ equals the number of new credit lines divided by the number of credit inquiries in the consumer's credit report. $Powerful\,Politician$ equals one in the two years following ascension, zero otherwise. $Majority\,Minority$ equals one if the consumer lives in a Census tract that is majority non-white, zero otherwise. Other variables are described in the text. The sample includes borrowers with an Equifax Riskscore less than or equal to 640. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Dependent variable:	Supply ratio			
Sample:	Consumer Riskscore < 640			
	(1)	(2)	(3)	(4)
$Powerful\,Politician$	-0.0144*	-0.0145*	-0.0158**	-0.0159**
	(0.0082)	(0.0082)	(0.0071)	(0.0071)
$Powerful\,Politician \times Majority\,Minority$	-0.0333**	-0.0337**	-0.0217*	-0.0217*
	(0.015)	(0.015)	(0.012)	(0.012)
$Majority\,Minority$			0.00109	0.000259
			(0.0088)	(0.0090)
$Consumer\,Riskscore/100$		0.0874***	0.0551***	0.0551***
		(0.0034)	(0.0047)	(0.0047)
$Census\,Tract\,Median\,Income\,(Z)$				-0.000700
				(0.0038)
date - quarter FE	x	x	x	x
Census tract FE	x	x		
consumer FE			x	x
N	441,501	441,501	440,673	440,566
Adj. R^2	0.26	0.26	0.34	0.34

Table A.4: **Powerful Politicians and Credit Demand**

This table uses OLS regressions to test the effect of a Senator's ascension to a powerful committee chair on the supply of consumer credit and the number of credit inquiries. It uses data from the FRBNY-CCP, a representative panel of individual credit records from Equifax. The sample period is years 1999 – 2012. $Supply\,ratio$ equals the number of new credit lines divided by the number of credit inquiries in the consumer's credit report. $Powerful\,Politician$ equals one in the two years following ascension, zero otherwise. $Majority\,Minority$ equals one if the consumer lives in a Census tract that is majority non-white, zero otherwise. Other variables are described in the text. The sample includes borrowers with an Equifax Riskscore less than or equal to 640. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Dependent variable:	Number of credit inquiries over past 12 months			
Sample:	Consumer Riskscore < 640			
	(1c)	(2c)	(3c)	(4c)
$Powerful\,Politician$	-0.0136	-0.0185	0.0147	0.0152
	(0.054)	(0.051)	(0.053)	(0.053)
$Powerful\,Politician \times Majority\,Minority$	0.0339	0.0215	0.0542	0.0531
	(0.028)	(0.029)	(0.036)	(0.035)
$Majority\,Minority$	-	-	-0.172***	-0.139***
	-	-	(0.030)	(0.031)
$Consumer\,Riskscore/100$		-0.677***	-0.449***	-0.449***
		(0.059)	(0.037)	(0.037)
$Census\,Tract\,Median\,Income\,(Z)$				0.0542***
				(0.015)
Year - quarter FE	x	x	x	x
Census tract FE	x	x		
Consumer FE			x	x
N	1,077,773	1,077,773	1,072,621	1,072,364
Adj. R^2	0.24	0.25	0.35	0.35

Table A.5: **Powerful Politicians and New Credit Accounts**

This table uses OLS regressions to test the effect of a Senator's ascension to a powerful committee chair on new consumer credit accounts in the Senator's home-state. **Panel A** uses a dependent variable equal to total number of new credit accounts on the consumer's credit report. In **Panel B**, columns (1) – (3), the dependent variable is the number of new installment credit accounts. In columns (4) – (6), the dependent variable is the number of new revolving credit accounts. $High\ Income$ equals one if the consumer lives in a Census tract that has an income above the 75th percentile of the within-state income distribution, zero otherwise. The data, the sample, and other variables are described in **Table 3**. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Panel A						
Dependent variable:	Number of new credit accounts					
	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)
$Powerful\ Politician$	-0.00786**	-0.00826**	-0.00701**	-0.00733**	-0.00492	-0.00475
	(0.0034)	(0.0035)	(0.0032)	(0.0033)	(0.0041)	(0.0041)
$Powerful\ Politician \times High\ Income$	0.0178*	0.0184*	0.0184**	0.0188**	0.0138*	0.0137*
	(0.010)	(0.0098)	(0.0087)	(0.0085)	(0.0077)	(0.0078)
$High\ Income$	0.0373***	0.0560***	-	-	0.00986***	0.00937***
	(0.0055)	(0.0047)	-	-	(0.0032)	(0.0032)
$Consumer\ Riskscore/100$		-0.0421***		-0.0396***		0.0170***
		(0.0047)		(0.0041)		(0.0046)
Year - quarter FE	x	x	x	x	x	x
state FE	x	x				
Census tract FE			x	x		
Consumer FE					x	x
N	5,140,009	5,139,820	5,138,800	5,138,611	5,128,280	5,128,090
Adj. R^2	0.0081	0.010	0.087	0.089	0.19	0.19
Panel B						
Dependent variable:	Number of new installment accounts			Number of new revolving accounts		
	(1b)	(2b)	(3b)	(4b)	(5b)	(6b)
$Powerful\ Politician$	-0.00789*	-0.00485	-0.00332	-0.000327	-0.00245	-0.00139
	(0.0046)	(0.0036)	(0.0035)	(0.0041)	(0.0034)	(0.0038)
$Powerful\ Politician \times High\ Income$	0.0198***	0.0108**	0.00749	-0.00157	0.00786	0.00610
	(0.0060)	(0.0046)	(0.0049)	(0.0099)	(0.0056)	(0.0050)
$High\ Income$	0.00861**	-	0.00400*	0.0474***	-	0.00525**
	(0.0039)	-	(0.0022)	(0.0052)	-	(0.0024)
$Consumer\ Riskscore/100$	-0.0458***	-0.0405***	0.0169***	0.00392	0.00109	0.000214
	(0.0066)	(0.0059)	(0.0035)	(0.0024)	(0.0025)	(0.0024)
Year - quarter FE	x	x	x	x	x	x
State FE	x			x		
Census tract FE		x			x	
Consumer FE			x			x
N	5,139,820	5,138,611	5,128,090	5,139,820	5,138,611	5,128,090
Adj. R^2	0.016	0.092	0.18	0.010	0.073	0.16

Table A.6: **Credit Rationing**

This table uses OLS regressions to test the relation between a Senator's ascension to a powerful committee chair and the supply of consumer credit, and whether the effect is concentrated in states that experience an expansion of credit to high income borrowers. The data, the sample, and the variables are described in **Table 3**. In this table, we sort states by the intensity of the relation between $Powerful\ Politician$ and $\#\ new\ credit\ accounts$, in Census tracts that are above the 75th percentile of median income. The sample in columns (1) and (2) include states that are above the median in the strength of this relationship, while columns (3) and (4) includes states below the median. Standard errors clustered at the state level are in parentheses. ***, **, and * denote statistical significance at the one, five, and ten percent levels, respectively.

Dependent variable:	Supply ratio			
Sample:	Consumer Riskscore < 640			
Intensity of credit expansion to high-income households in state:	State is above median		State is below median	
	(1)	(2)	(3)	(4)
$Powerful\ Politician$	-0.0164*	-0.0159*	-0.0148	-0.0143
	(0.0087)	(0.0083)	(0.018)	(0.018)
$Powerful\ Politician \times Majority\ Minority$	-0.0250***	-0.0232**	-0.00198	-0.00174
	(0.0084)	(0.0086)	(0.019)	(0.019)
$Consumer\ Riskscore/100$		0.0900***		0.104***
		(0.0052)		(0.0072)
Year - quarter FE	x	x	x	x
Census tract FE	x	x	x	x
N	567,524	567,524	228,831	228,831
Adj. R^2	0.18	0.19	0.19	0.19