Politicizing Consumer Credit*

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Abstract

Using proprietary data on individual Americans' credit histories, we find that access to consumer credit decreases by 4.5% - 8% when the borrower's home-state U.S. Senator gains political power by becoming the chair of a Senate committee. This contraction in credit availability is concentrated among historically credit-constrained borrower groups (poor borrowers, non-white borrowers, and borrowers with low credit scores), and is stronger in areas with fewer politically-active consumers and more politically-connected lenders. We present evidence consistent with a "political protection" hypothesis in which banks view regulatory fair-lending guidelines as less binding when they are connected to powerful politicians. Our results highlight the distinction between political power and legislative outcomes, and provide a counterpoint to recent findings that government interventions improve consumers' access to credit.

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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 standing of U.S. Senators and a proprietary database of Americans' credit histories to examine the relationship between political power and consumer access to credit in the United States. Unlike the existing literature, we find that increases in political power are associated with *reductions* in consumer credit access in a politician's home state relative to other, unaffected states. These reductions are concentrated among "disadvantaged" borrower groups, such as racial minorities and borrowers with low incomes or poor credit histories. These reductions in credit supply are also more pronounced in areas with few politically-active citizens and many politically-connected banks. In contrast, we find no significant relationships between political power and household credit demand. Collectively, these results challenge the conventional wisdom that political influence is used to expand consumer credit supply.

Our primary data source is the Federal Reserve Bank of New York Consumer Credit Panel (FRBNY - CCP), a proprietary panel of consumer credit histories covering a 5% random sample of the entire U.S. population over a sixteen year period. This database is particularly well-suited to

[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.

studying consumer credit provision, as it allows us to track individuals' credit applications, credit usage, credit scores, and delinquencies across time, for every type of borrowing group (old versus young, rich versus poor, prime versus subprime, minority versus non-minority, etc.), within very precise geographic locations (Census tracts).

Our primary tests examine three aspects of consumer credit: access to credit (as measured by the ratio of new accounts to new applications, which we call the "supply ratio"), the demand for credit (as measured by new credit applications), and total credit provision (the number of new credit accounts opened by a consumer).[4] Most of our tests also focus on "disadvantaged" borrowers, since these borrowers are more likely to be affected by any material change in consumer credit markets, whether politically motivated or otherwise.

However, in order to identify the effects of political power on consumer credit, we must first isolate changes in political standing that are unrelated to current economic conditions and other variables affecting consumer credit provision. We navigate this challenge by studying shocks to the chair or ranking member of powerful U.S. Senate committees using a modified version of the identification strategy introduced by Cohen, Coval, and Malloy (2011). In particular, we exploit the fact that committee leadership roles are a deterministic function of committee seniority and are hence plausibly uncorrelated with other factors that might affect consumer credit. For example, Robert Byrd (D – West Virginia) stepped down as Chair of the powerful Senate Appropriations Committee in 2009 due to recurrent health problems. Because Byrd was a Democrat, Senate rules dictate that the committee chairmanship passes to the next-longest-serving Democrat on the committee (Daniel Inouye, D – Hawaii, a committee member since 1971).[5] Importantly, Inouye's ascension was a function of *both* Robert Byrd stepping down from his role *and* the fact that Inouye was the longest-tenured Democrat on the committee (which is itself a function of historic events

[4] Some of our tests also examine consumers' credit utilization, which is defined as a consumer's current outstanding credit balance divided by their credit limit.

[5] We focus on the ascensions of committee chairs such as Inouye because chairs have an outsized role in determining which pieces of legislation are brought to the Senate floor, and committee chairs also have significant influence over the amount of government spending directed to their home states (Cohen, Coval, and Malloy (2011)).

dating back to 1970). Hence, we argue that Inouye's ascension was unlikely to be related to the *current* supply and demand for consumer credit in Hawaii relative to other U.S. states.

We find that increases in political power decrease the availability of consumer credit in the politician's home state by an average of 2 to 4 percentage points relative to credit access in unaffected states. These estimates are economically important: the sample average of our "supply ratio" varaible is 0.45 (or 45 percentage points), suggesting that overall consumer credit access is reduced by 4.5 to 8 percent in states affected by a shock to political power.[6] Moreover, these effects are primarily concentrated within segments of borrowers that tend to be credit constrained: consumers with credit scores below 640 (the typical cutoff between "prime" and "subprime" borrowers), low-income consumers, and consumers residing in Census tracts that have at least 50% minority residents.

The most likely explanation for our finding of reduced credit availability is that banks obtain "political protection" from the ascension of home-state Senators in a manner that allows them to alter their consumer lending patterns away from disadvantaged borrower groups. U.S. commercial banks must comply with a number of fair-lending regulations requiring them 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).[7] Regulations such as those imposed by the CRA can be thought of as constraints that prohibit banks from denying credit to certain borrowers. If banks believe that the regulatory consequences of noncompliance are less severe when they have powerful legislative advocates, then holding investment opportunities constant, we would expect banks to tighten screening standards in a manner that reallocates consumer credit away from disadvantaged borrowers and towards other, higher-quality borrower groups.

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

[7] 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."

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-MSA variation in CRA eligibility (Census tracts below 80% of the MSA's median income) and find that shocks to political power reduce consumer credit supply in Census tracts where the CRA binds, with a discontinuous reduction occurring right around the CRA eligibility threshold.[8] Second, within a given state, the reductions in consumer credit supply are most pronounced in areas with politically-active banks that possess a direct connection to the shocked politician. Third, following a political power shock, higher-quality borrowers receive an increased share of new loans, and consumers receiving these loans have lower delinquency rates in the years following the shock. Finally, if certain loans are extended in order to comply with fair lending standards, and if these loans are less profitable than alternative lending opportunities, then bank profitability should increase following the politically-motivated relaxation of these regulatory guidelines. Consistent with this hypothesis, we find evidence of increased bank profitability following political power shocks. Overall, the preponderance of evidence is consistent with banks obtaining "political protection" when a home-state Senator becomes more powerful, which in turn allows "protected" banks to curtail less-profitable lending activities to disadvantaged borrower groups.

The primary alternative explanation for our results is that the decrease in consumer credit in "shocked" states is simply a byproduct of the increase in government spending and decrease in private-sector investment documented by Cohen, Coval, and Malloy (2011). For example, macroeconomic changes induced by the newly-powerful politician's actions could affect firms' demand for credit, which could in turn affect banks' credit allocation to disadvantaged consumers.[9] Furthermore, even if demand is held constant, macroeconomic changes could also affect the investment opportunity set of banks. In particular, changes in borrowers' incomes (or economic conditions in the region as a whole) could affect the expected profitability of different lending opportuni-

[8] "MSA" stands for Metropolitan Statistical Area, which is the U.S. Census Bureau's definition of a city.

[9] This explanation relies on the assumption that banks face supply constraints. Under this assumption, a demand shock could lead to an optimal reallocation of bank credit away from disadvantaged consumers in favor of another borrowing group.

ties in a way that makes it optimal for banks to shift away from lending money to disadvantaged consumers.[10]

However, we do not find much evidence to support these macro-driven channels. First, political shocks do not affect aggregate consumer credit demand.[11] Second, political shocks do not affect state-level macroeconomic variables such as economic growth, house prices, or personal income. Third, banks with operations in "shocked" states do not reallocate their lending portfolios to provide more credit to other borrowing groups (such as businesses) at the expense of consumers. Fourth, while we cannot rule out the possibility that banks' investment opportunities are changing, there is little evidence that banks' loan portfolio concentrations change significantly around the time of the political power shocks. Finally, it is unlikely that state-level macroeconomic changes would be able to explain our CRA lending results, where we document a reduction in credit supply (but not demand) that disproportionately affects an extremely narrow group of borrowers. Collectively, the evidence presented in this paper suggests that macro-driven shifts in consumer credit markets are unlikely to fully explain our results.

We also find that shocks to Democratic and Republican Senators have similar effects on consumer credit access, suggesting that differences in traditional voting blocs cannot explain our results. In addition, the largest contractions in credit to disadvantaged borrowers occur in areas that are politically unengaged (as measured by locations with below-median political contributions to U.S. Senators), and these effects are amplified in regions with a large proportion of politically-connected banks. Finally, 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, which is consistent with lenders tightening screening standards following these shocks.

[10] For example, business loans that were negative-NPV before the macroeconomic changes could now be positive-NPV, even though the demand for all loans remains unchanged.

[11] There is a small increase in the number of credit inquiries and the number of new accounts opened by high-income borrowers following political power shocks. However, these effects are economically modest. Moreover, for this result to be explained by a macroeconomic change in consumer credit markets, we would expect high-quality borrowers to increase their demand for credit at *all* banks and not just politically-active banks, where we find the strongest effects.

Our results are robust to controlling for consumers' credit scores at the time of their applications — a variable that should be the principal determinant of lenders' decision to extend credit. Our results are also robust to increasingly stringent geographic fixed effects as well as *individual*-level fixed effects that account for unobserved heterogeneity in borrower quality across political constituencies. Finally, our results are also consistent with the idea that disadvantaged borrowers appear to be facing an *unexpected* shock to credit supply when their home-state Senator becomes the chair of a powerful committee.[12]

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 (2016a)) and access to traditional and unconventional financial products on consumer outcomes (Melzer (2011), McDevitt and Sojourner (2016)); the effects of financial literacy on consumer credit outcomes (Lusardi and Mitchell (2011), Brown, Grigsby, van der Klaauw, Wen, and Zafar (2016b)); and the effects of cognitive biases on outcomes (e.g. Stango and Zinman (2011)). To the best of our knowledge, however, no papers

[12] Since borrowers should optimally weigh the expected costs and benefits of applying for credit, we should expect to see a fairly similar fraction of applications be denied across different borrower groups, regardless of a Senator's political standing. However, we find large differences in the fraction of denied applications across borrower groups, particularly following shocks to political power.

study the effects of political power on the financial inclusion of disadvantaged borrowers 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 bailout likelihood (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 has linked 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 the bank regulatory environment and banks' lending decisions.

2 Shocks to Political Power

We are interested in identifying the effects of political power on consumer credit supply and demand. To credibly estimate a causal relationship, changes in political power need to be unrelated to other variables (such as economic conditions) that could be correlated with consumer credit allocation in a Senator's home state.

Our identification strategy is a modified version of the identification strategy introduced by Cohen, Coval, and Malloy (2011). In particular, we identify "shocks" to a politician's power by exploiting changes in the leadership of powerful U.S. Senate committees. The United States Senate has 16 standing committees, each of which is responsible for specific areas of policy. A necessary step for bills to become law is for each bill to be approved by the relevant committee(s) before moving to the floor of the House or Senate for a vote.[13] Importantly, this gives the members of a committee – and *particularly* the committee's chair – significant influence over which pieces of legislation make it to the floor of the Senate for a vote. Committee chairs are also responsible for setting the agenda for their respective committees, which gives them significant power to dictate

[13] 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, since the bill related to both banking and anti-trust policy.

the legislative schedule, call hearings, and control the other actions taken by the committee. As such, a Senator ascending to become the chair of a committee obtains a large, discrete increase in his or her political power.

Per Senate rules, the role of committee chair is traditionally filled by the Senator from the majority party who possesses the longest seniority *on that committee*, provided they do not already chair another committee. For example, Robert Byrd (D – West Virginia) stepped down as chair of the powerful Senate Appropriations Committee in 2009 due to recurrent health problems. Because Byrd was a Democrat and the Democrats controlled the Senate at the time of Byrd's departure, Senate rules dictated that the committee chairmanship pass to the next-longest-serving Democrat on the committee (Daniel Inouye, D – Hawaii, a committee member since 1971).

Our identification strategy exploits the fact that committee seniority is essentially a deterministic function of the amount time spent on the committee, and hence, is plausibly exogenous with respect to current economic conditions in the new chair's home state.[14] For example, Daniel Inouye's ascension as chair of the Appropriations Committee was a function of *both* Robert Byrd stepping down from his role (which was unexpected) *and* the fact that Inouye was the longest-tenured Democrat on the committee (which was itself a function of historic events dating back to 1970). The next longest-serving Democrat on the Appropriations Committee was Barbara Mikulski (D – Maryland), who had served on the committee since 1987. Importantly, due to the "seniority rule" described above, the fact that Inouye (Hawaii) rather than Mikulski (Maryland) takes over as committee chair is based on a series of elections that happened more than 20 years prior to the event date. Hence, Inouye's ascension was unlikely to be related to *current* economic conditions in Hawaii relative to other U.S. states (like Maryland). However, after Inouye became the committee chair, he had a much greater ability to control the flow of government funds to Hawaii (relative to Maryland), and his general political power also increased, allowing him to take other actions that may have disproportionately affect the citizens of his home state. Hence, we argue that

[14] In addition, a politician's *committee* seniority can be quite different from his or her *overall* seniority in the Senate. For example, in the current congress Roy Blunt (R–Missouri) is the current chair of the Senate Rules Committee despite only being 26th of 54 in overall Republican seniority and can be compared to Lindsay Graham (R–South Carolina) who is currently 14th in seniority and has never chaired a committee.

Inouye's ascension (like the other ascensions in our sample) represented a discrete increase in his political power that was plausibly exogenous with respect to other factors that might be affecting the provision of consumer credit in Hawaii relative to other states (such as Maryland) in 2009.[15]

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 differences in credit outcomes across consumers in *states with new committee chairs* against consumers in *all other states*, controlling for observable differences in consumer and geographic characteristics, as well as current macroeconomic conditions. For example, consider two consumers, one in Hawaii and one in Maryland, who have identical demographic profiles and identical credit profiles. 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

[15] We do not examine whether credit allocation changes in the state associated with the *previous* committee Chair because the two most common factors causing incumbent Chairs to depart – an election defeat for the Chair, or a change in the political party that has control of the Senate – are likely to be correlated with economic outcomes in Senators' home states, and hence, not exogenous from the standpoint of contemporaneous consumer credit allocation.

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.

We also interact the *Powerful Politician* variable with various consumer demographic characteristics, because we expect disadvantaged borrowers to be most affected by regional changes to consumer lending. 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 $Demographic\,Characteristic_g$ is a binary variable capturing (for example) whether a given Census tract is heavily populated by minorities, poor people, people with short credit histories, or people with low credit scores.

Our identification strategy rests on the assumption that our political shocks are uncorrelated with current economic conditions in Senators' home states. In Table A.1, we examine the correlations between our political shock variable and state-level macroeconomic variables such as GDP, employment, income, house prices and bankruptcies in a variety of specifications. We find no statistically or economically significant differences between "shock" periods and other periods, both in the same state and across other states or that any of these macroeconomic variables are consistent predictors of senate ascensions. Hence, state-level macroeconomic conditions do not seem to be changing significantly either before or after the ascension of a new Senate committee chair.

3 Data

3.1 Consumer Credit Data: The FRBNY Consumer Credit Panel

Our main data source is the FRBNY Consumer Credit Panel (FRBNY - CCP), 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.[16] The data start in 1999Q1 and are collected quarterly thereafter (our sample ends in 2012Q4). The sample design of the Consumer Credit Panel 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 (2016b) show that the FRBNY - CCP 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 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 offers coverage of individuals that have had difficulty obtaining credit. For 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 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.[17]

The primary shortcoming of the FRBNY - CCP relative to other household surveys is that no demographic information is linked to the credit records aside from consumer age due to federal fair-lending and privacy laws. To overcome this problem, we merge the FRBNY - CCP data with Census tract demographics from the 2000 U.S. Census. Since a Census tract typically covers a very small geographic area (often much smaller than a neighborhood), we are able to add demographic and socioeconomic factors to our analysis at a fairly precise geographic level.

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

[17] 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 is unable to track individuals over time, and the data is sometimes too thin to use at very precise geographic levels (such as a Census tract).

Our analysis focuses on several key variables from the FRBNY - CCP (summary statistics are in Table 1). Our measure of credit availability is called the *supply ratio*, which 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 so-called "subprime" 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 (2016a) validate the supply ratio measure by showing that it varies significantly over time and geographically in a manner that appears to reflect the tightening and loosening of credit provision. The measure's main limitation is that the FRBNY - CCP data does not specify the purpose of the loan for which the hard credit inquiry was obtained.[18]

Many of our tests measure the supply of new loans by the number of new credit lines per individual, which we sometimes breakdown 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 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.

3.2 Political Data: Senate Committee Chairs and Campaign Contributions

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 *Powerful Politi-*

[18] In addition, consumers who receive credit but choose not to open a new credit account will show up as having been denied credit.

cian variable, we first identify each Senate committee leadership change during our sample period (1999-2012). We then 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 on only 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).[19]

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" committees and Panel C splits all shocks by political party.[20] We observe a wide geographical variation in shocks, particularly for "important" shocks.[21] 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.

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

[19] 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 the Senator's ascension on their home state. However, Snyder and Welch (2015) have criticized the duration of the six-year shocks employed by Cohen, Coval, and Malloy (2011) as potentially too long, since Senators may (for example) lose their position as Chair due to changes in control of the Senate over this six-year period. Our shorter post-event window (two years) should alleviate the concerns expressed by Snyder and Welch (2015), at the cost of potentially understating the effects that we document.

[20] 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.

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

elections from 1998-2010.[22] For each election cycle, we obtain individual political donations at the zip code level and bank donations at the level of the bank. Because corporations are prohibited from donating money directly to political causes, we examine donations made by bank PACs to the PACs of elected officials and political parties. In our primary analysis, we consider a bank to be politically active if (i) it operated a PAC during our sample and (ii) it contributed to a "shocked" politician at some point during our sample period prior to the date that the politician ascends to become a committee chair.

3.3 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 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 political power shocks affect bank profitability. We use banks' quarterly FFIEC 031/041 filings (the "Call Reports") to obtain data on bank balance sheet composition and profitability.

[22] 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.

3.4 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 US Courts.

4 Powerful Politicians and Consumer Credit: Empirical Evidence

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 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 some of our initial 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. Panel A of the table includes the entire

sample of borrowers, while Panel B restricts the analysis to the sample of borrowers with subprime credit scores. Specifications (1) – (4) of both panels present this analysis with a variety of fixed-effects and control variables.

Senate committee ascensions lead to a *decrease* in 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, particularly for the sample of disadvantaged borrowers in Panel B. For example, column (2) of Panel B suggests that the incremental reduction in credit supply in majority-minority areas is -0.0208, which is 1.5 times larger than the main effect. These estimates are similar with and without controlling for a borrower's Riskscore, as well as Census tract incomes, consistent with the political power shocks exploiting variation that is not a function of larger macroeconomic conditions in these states. The estimates are robust to controlling for borrower fixed-effects, suggesting that changes in unobservable borrower composition across locations cannot explain the results. The results are also robust to using Cohen, Coval, and Malloy (2011)'s definition of increased political power, ascent to chair of one of six "important" committees (Appendix Table A.2).

The decrease in *supply ratio* is caused by a reduction in new credit lines, and not an increase in credit applications. According to Panel C of Table 3, *Powerful Politician* does not statistically significantly affect the number of credit inquiries for sub-prime borrowers in either white or majority minority Census tracts. This result is consistent with the effects documented in Panels A and B being caused by tightened credit supply, as opposed to changes in demand.

4.1.2 Evidence of Proper Identification and Robustness

We present further evidence consistent with 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 politi-

cal power shock. Immediately following the shock, *supply ratio* sharply declines.[23] Though our regressions set the shock to last for two years, the impact of the shock persists for at least two additional years. 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*.

We also provide 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 following the Great Recession.

4.2 Demand for New Credit

Our tests suggest that shocks to Senators' political power are followed by a reduction in credit supply to disadvantaged borrowers. A natural follow-up question is whether other groups respond to these shocks by increasing their demand for credit or whether they are better able to access credit, potentially at the expense of disadvantaged borrowers. In order to examine this question, we next look at new credit accounts that are created following our political power shocks (Table 4). In contrast to the analysis in Tables 3, we look at how these shocks differ by high and low-income

[23] 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.

borrowers. We do so in order to later test whether any possible increase or decrease in new credit accounts overlaps with the reduction in the supply ratio that we have previously documented.

Increased political power tends to decrease the number of new accounts in low-income areas, and to an *increase* in the number of new accounts in high-income areas (Panel A of Table 4). Indeed, once we include Census tract fixed effects, borrowers in high-income neighborhoods experience a net increase in the number of new accounts that they open (e.g. a total effect of 0.01147 in specification (4), controlling for a borrower's Riskscore).

Panel B of Table 4 breaks this analysis down based on whether the new accounts are for "installment" credit (i.e. loans) versus "revolving" credit (i.e. credit cards). Specifications (1) – (3) examine installment accounts, which includes all types of non-revolving credit such as mortgages, loans for consumer durable goods, and educational loans. Specifications (4) – (6) examine revolving credit, which includes credit card debt and lines of credit. Our analysis suggests that the number of new accounts only declines (increases) for low- (high-)income borrowers when the accounts in question are for installment loans.

We find evidence that increased political power of home-state Senators result in a *reallocation* of consumer credit away from disadvantaged borrowers and towards high-income borrowers. We combine the demographic analysis in Tables 3 and 4 to examine whether the contraction in credit supply for minorities occurs in the same states where the number of new accounts increases for high-income borrowers. Table 5 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 the reduction in credit supply for disadvantaged borrowers occurs predominately (and potentially only) in areas that themselves are higher-income.

4.2.1 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 supply is strongest in areas that have politically unengaged consumers. Table 6 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). Panel A tests the effect on *supply ratio*, while Panel B tests the effect on new credit accounts. The coefficients on $Powerful\,Politician$ and $Powerful\,Politician \times Majority\,Minority$ are substantially larger in magnitude and statistically significant in areas with below-median political contributions. Futher, *Powerful Politician* has about a 0.04 reduction in credit availability to minority borrowers in politically unengaged ZIP codes, which is a one eighth increase over the estimate from the full sample. In politically engaged ZIP codes, the coefficient estimates are also negative, but they are smaller and statistically insignificant.

Similarly, the number of new credit accounts falls in politically unengaged areas and rises in politically engaged areas. For example, the coefficient on $Powerful\,Politcian$ in column (4) is -0.0180 and statistically significant (with no differential effect for high-income borrowers). We do not observe a statistically significant effect in the politically engaged areas, but do see evidence of an *increase* in credit for high income borrowers in politically engaged areas (the coefficient on $Powerful\,Politcian \times High\,Income$ is significantly positive, as is the total effect for high

income borrowers). Collectively, these results suggest that borrowers in politically unengaged areas are those that experience a reduction in their access to credit markets while higher-income borrowers in politically engaged areas experience moderately increased credit market access.

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) 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. 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.

Our political protection hypothesis yields four 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). Third, 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 defined 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.[24] We next compute the fraction of bank branches in each county that belong to a politically-connected bank.[25] We compute both equal-weighted averages and deposit-weighted averages to ensure that our measure of politically-connected banks does not

[24] 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.

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

simply proxy for bank size. 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 7 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.[26] However, if powerful politicians provide "political protection" to banks, this may in turn lead banks to believe that they can reduce the amount of lending to disadvantaged borrowers that is required by laws such as the CRA and ECOA.

[26]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.

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 MSAs 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 discretely decline at the 80% income threshold relative to other areas of the same MSA where the CRA restrictions are not binding.

Table 8 shows that the contraction in credit supply is indeed 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.[27] 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 ascends to become a powerful committee chair.

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 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 8 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

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

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.

In summary, the results in Table 8 suggest that following the ascension of a newly-powerful Senator, consumer credit supply in the Senator's home state contracts primarily 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 cut back on 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).[28]

[28] 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

Further, 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 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 consumer lending. In particular, we use branch 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, although our results are similar using ROE. Table 9 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

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.

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.

Across all specifications, we find that increased political power is associated with increases in bank ROA. Average ROA increases by approximately 3% 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. We also examine whether politically-connected banks in shocked states obtain even greater increases in profitability relative to less-connected banks. However, we do not find significant differences across these two groups of banks. Hence, it appears that even non-connected banks improve their operating performance following a positive shock to the power of their home-state Senator.

5.2 Alternative Explanations

5.2.1 Political Catering to Voting Blocs

One alternative possibility is that the declines we observe in credit access for disadvantaged borrowers could be driven by politicians' attempts to cater to their primary voting blocs. 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 these patterns, 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 10 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. While the magnitudes differ somewhat across specifications, the results show that shocks involving *both* parties lead to contractions in consumer credit in majority-minority areas, suggesting that a simple story about catering to an expected voting bloc cannot explain our results.

5.2.2 Corporate Lending Substitution

Another plausible explanation for the reduction in consumer credit supply is that a shock 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 11 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. Panel C of Table 3 shows that the number of new credit inquiries by disadvantaged borrowers remains unchanged following the ascension of a new committee chair. In Table 12, 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 U.S consumers (and not just disadvantaged borrowers). Table 12 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 11 shows that banks did not significantly shift the size or composition of their loan books. Moreover, Table 8 shows that the decline in credit supply occurs disproportionately in areas that are "just" subject to CRA lending mandates. 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 areas, particularly given the find-

ings from our other tests.[29] Collectively, our tests suggest that aggregate changes in credit market conditions are unlikely to fully explain our main results.

6 Conclusion

This paper provides novel evidence on 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 over a sixteen year period. Our main tests measure 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 the supply of consumer credit in the politician's home state by an average of 4.5 to 8 percent relative to credit supply in unaffected states. Moreover, these effects are primarily concentrated within segments of borrowers that tend to be 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. These results also hold after controlling for consumers' credit scores at the time of their applications.

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 supply occur in Census tracts that are most likely to having lending caused by regulatory guidelines in the Community Reinvestment Act. We also find that the largest contractions in credit to disadvantaged

[29] 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.

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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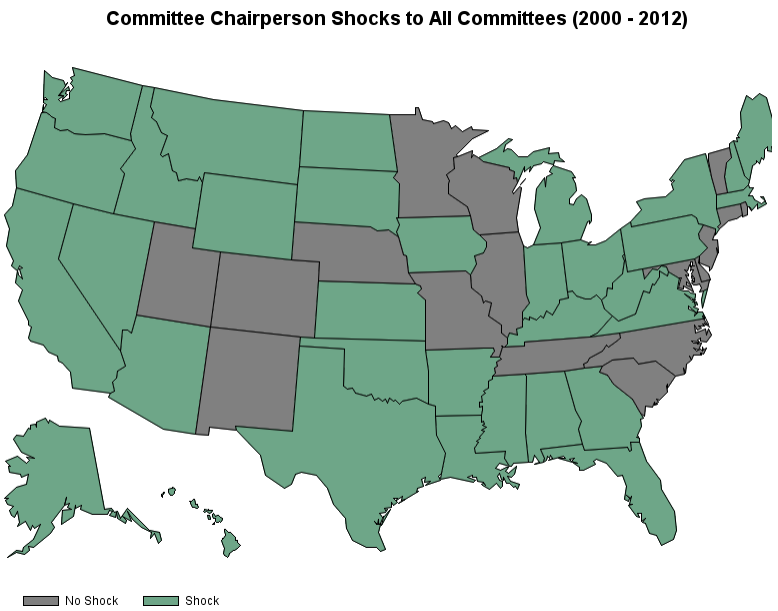
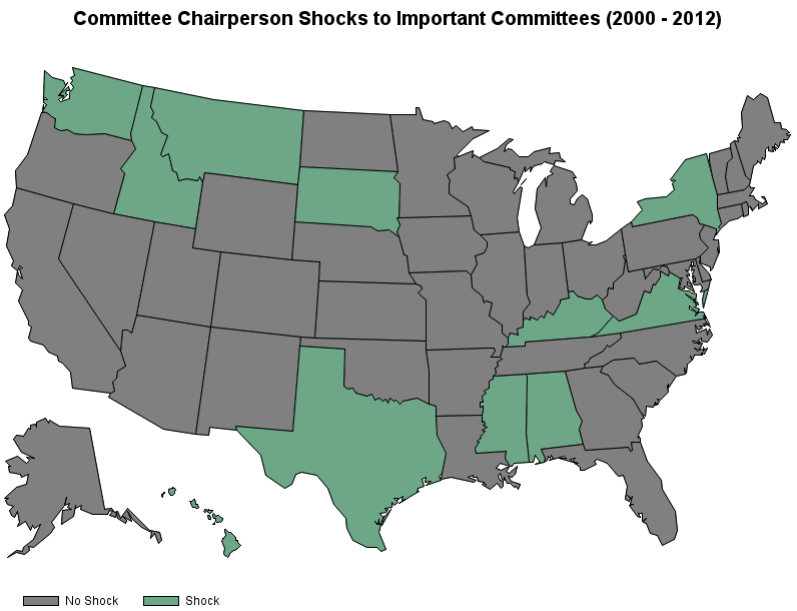


(a)



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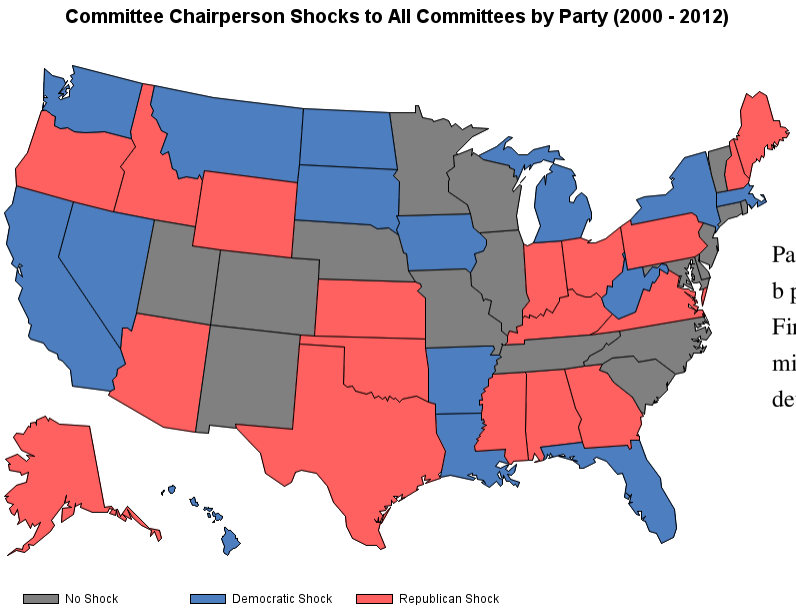


(c)

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.

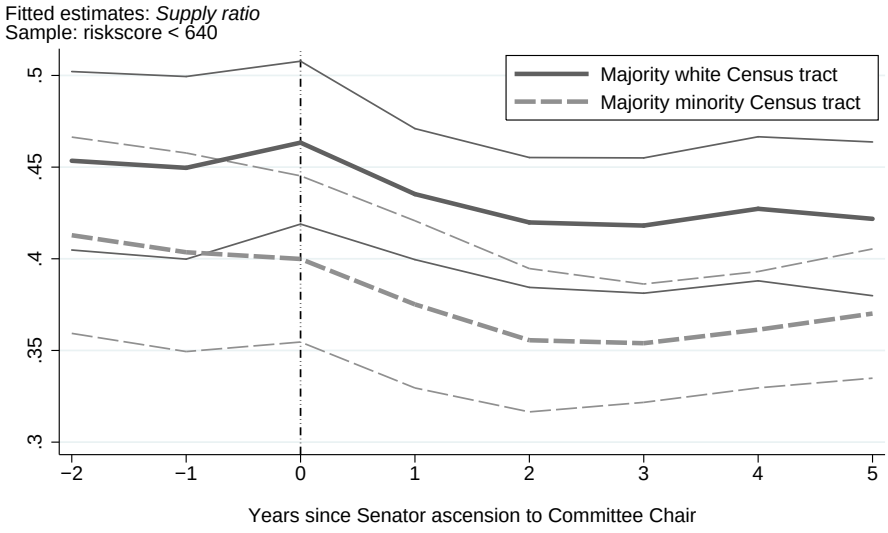
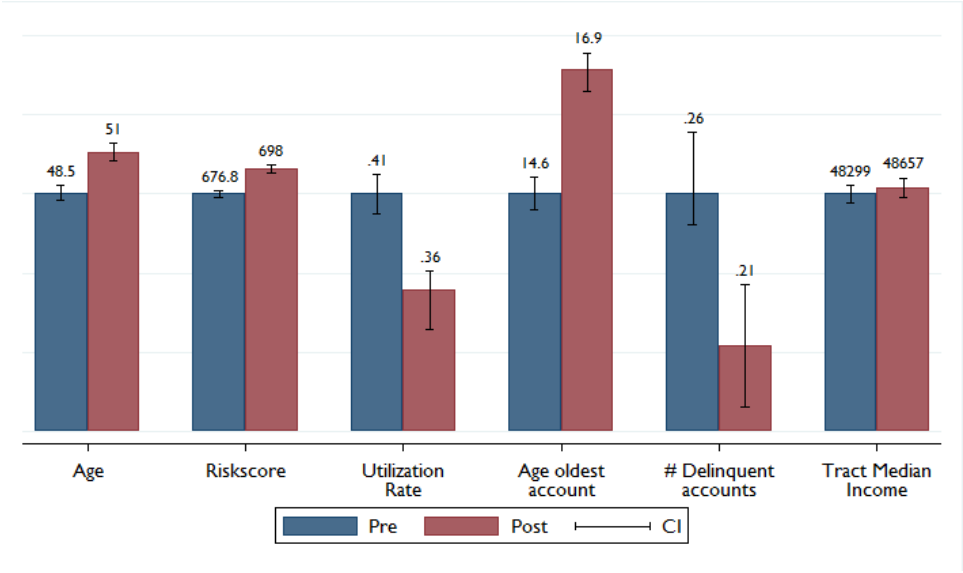


Figure 3: **Borrower Characteristics for New Credit Lines**

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**

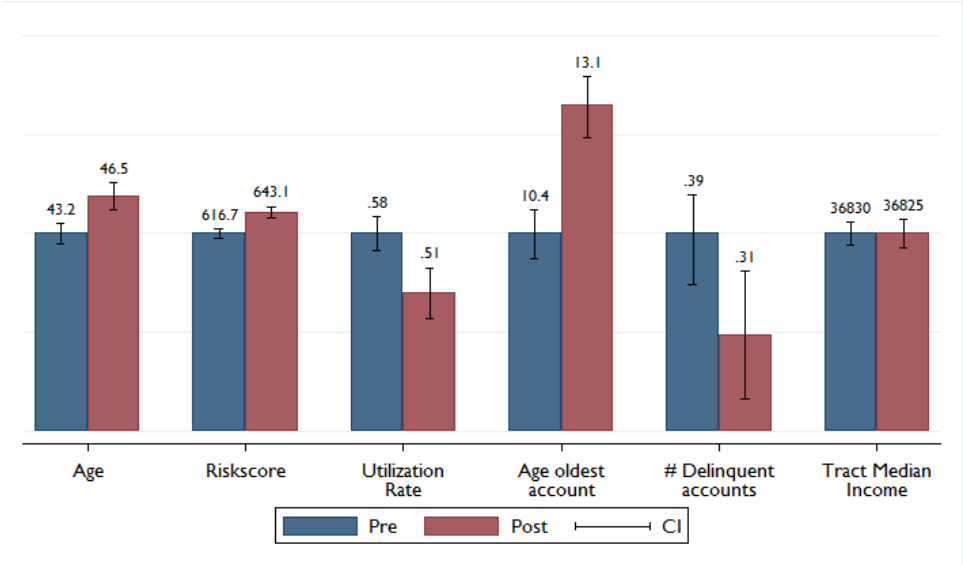


Figure 4: **New Account Delinquencies**

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.

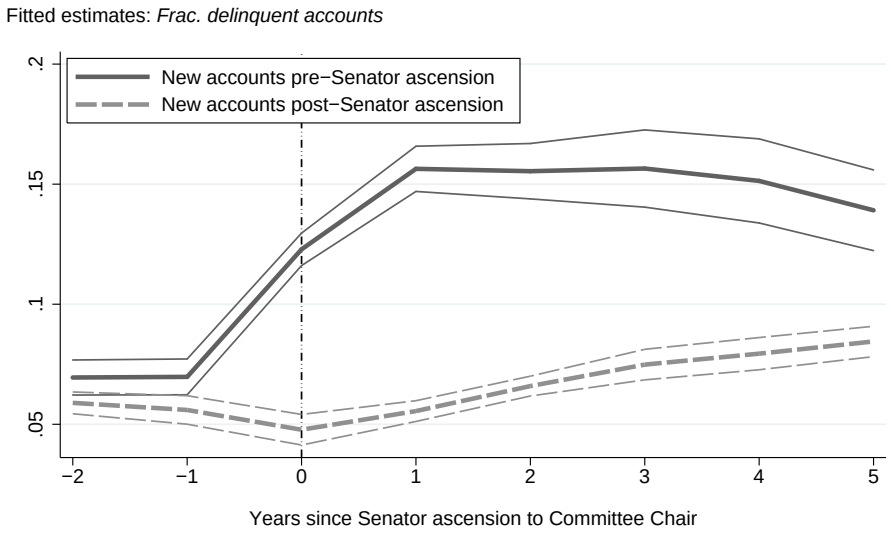


Table 1: **Summary Statistics**

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. Columns (1) and (2) include all changes in Chairpersons while columns (3) and (4) exclude cases where the Ranking Member was promoted to Chairperson (the shocks used for most of the analysis in this paper). Columns (2) and (4) show the shocks to "important" committees, which are the Senate Finance, Armed Services, Appropriations, Rules, Veteran's Affairs, and Banking Committees.

	(1)	(2)	(3)	(4)
Sample	All Committees	Important Committees	All Committees	Important Committees
Including Ranking Member Promotions	Yes	Yes	No	No
1999–2000	5	3	5	3
2001–2002	15	5	4	1
2003–2004	16	6	7	2
2005–2006	10	2	9	2
2007–2008	11	5	1	2
2009–2010	9	2	8	0
2011–2012	4	2	4	2
Total Shocks	70	25	38	12

Table 3: **Powerful Politicians 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 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. **Panel A** includes all borrowers in our FRBNY-CCP dataset. **Panels B** and **C** include 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.

Panel A				
Dependent variable:	Supply ratio			
Sample:	All consumers			
	(1a)	(2a)	(3a)	(4a)
$Powerful\,Politician$	-0.0142*	-0.0143**	-0.0176**	-0.0176**
	(0.0073)	(0.0071)	(0.0069)	(0.0069)
$Powerful\,Politician \times Majority\,Minority$	-0.0190*	-0.0145	-0.0127	-0.0127
	(0.0099)	(0.011)	(0.011)	(0.011)
$Majority\,Minority$	-	-	-0.0149***	-0.0129**
	-	-	(0.0055)	(0.0054)
$Consumer\,Riskscore/100$		0.171***	0.111***	0.111***
		(0.0042)	(0.0046)	(0.0046)
$Census\,Tract\,Median\,Income\,(Z)$				0.00285
				(0.0022)
Year - quarter FE	x	x	x	x
Census tract FE	x	x		
Consumer FE			x	x
N	2,644,102	2,644,102	2,642,201	2,641,651
Adj. R^2	0.15	0.17	0.26	0.26
Panel B				
Dependent variable:	supply ratio			
Sample:	Consumer Riskscore < 640			
	(1b)	(2b)	(3b)	(4b)
$Powerful\,Politician$	-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**	-0.0130*	-0.0130*
	(0.0085)	(0.0086)	(0.0075)	(0.0075)
$Majority\,Minority$	-	-	-0.00422	-0.00234
	-	-	(0.0066)	(0.0069)
$Consumer\,Riskscore/100$		0.0930***	0.0665***	0.0665***
		(0.0038)	(0.0042)	(0.0042)
$Census\,Tract\,Median\,Income\,(Z)$				0.00278
				(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 3: **Powerful Politicians and Credit Constraints (Continued)**

Panel C				
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 4: **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 5: **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

Table 6: **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**) or the number of credit inquiries (**Panel B**). 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
Panel B				
Dependent variable:	Number of new credit accounts			
Sample:	all consumers			
Campaign contributions in zip code:	Above median		Below median	
	(1b)	(2b)	(3b)	(4b)
$Powerful\ Politician$	-0.00316	-0.00336	-0.00856**	-0.00892**
	(0.0034)	(0.0035)	(0.0039)	(0.0039)
$Powerful\ Politician \times\ High\ Income$	0.0162**	0.0167**	0.0133	0.0131
	(0.0075)	(0.0074)	(0.016)	(0.015)
$Consumer\ Riskscore/100$		-0.0456***		-0.0332***
		(0.0032)		(0.0056)
Year - quarter FE	x	x	x	x
Census tract FE	x	x	x	x
N	2,623,980	2,623,920	2,513,845	2,513,716
Adj. R^2	0.086	0.088	0.10	0.10

Table 7: **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 8: **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 9: **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. 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. 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					
Sample:	All Banks			Single-State Banks		
	(1)	(2)	(3)	(4)	(5)	(6)
$Powerful\ Politician$	0.000151**	0.000136*	0.000130*	0.000191**	0.000160**	0.000161**
	(7.06e-05)	(6.87e-05)	(7.03e-05)	(9.10e-05)	(6.43e-05)	(6.55e-05)
$Connected\ Bank$			-0.00106***			-0.000827
			(0.000164)			(0.000510)
$Powerful\ Politician \times Connected$			0.000159			-2.63e-05
			(0.000108)			(0.000447)
$Bank\ Size$		0.00171***	0.00171***		0.00292***	0.00292***
		(0.000143)	(0.000143)		(0.000226)	(0.000226)
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	267,775	267,775	267,775
Adj. R^2	0.547	0.565	0.566	0.588	0.625	0.625

Table 10: **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 11: **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 11: **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 12: **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.

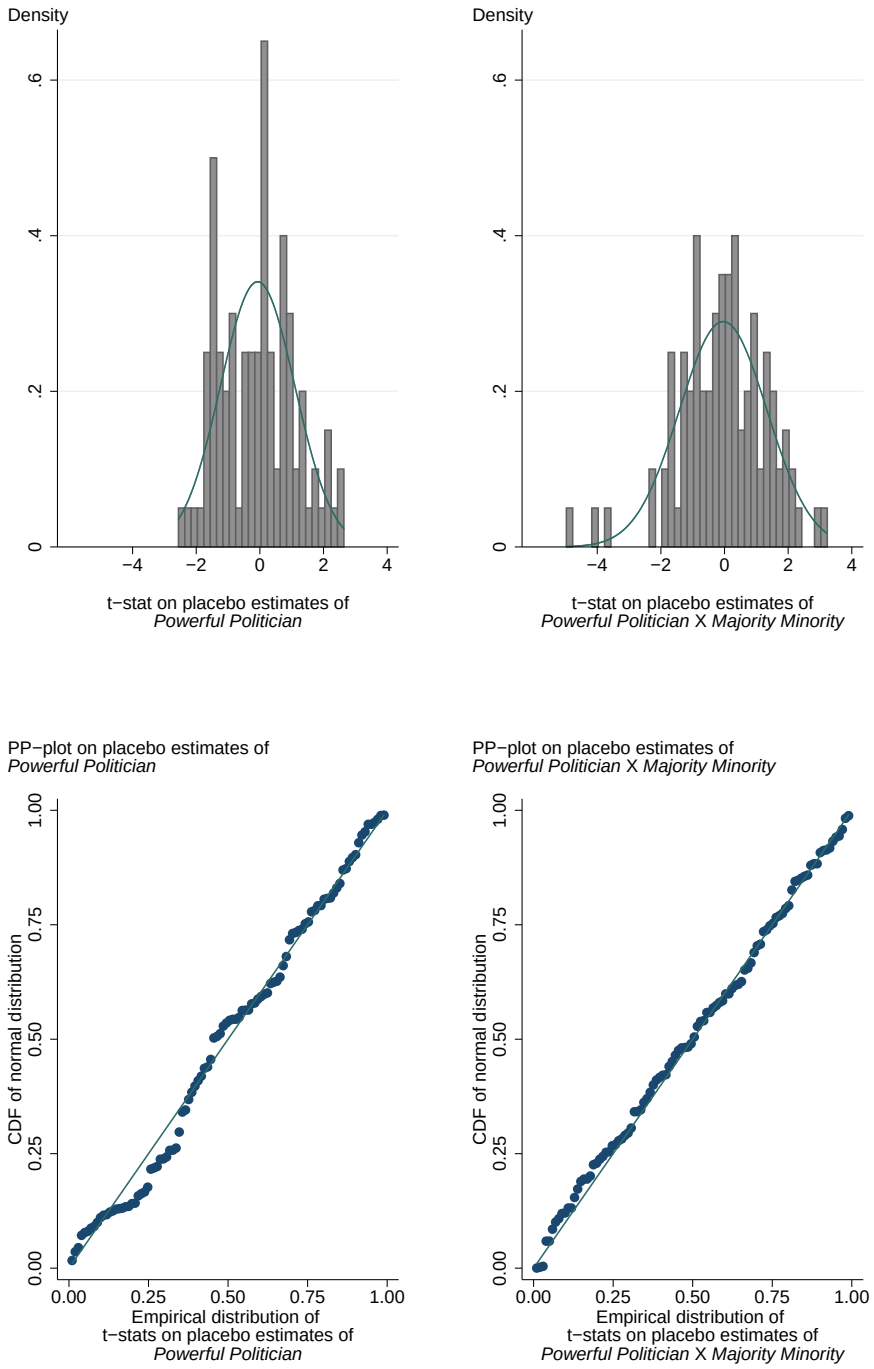
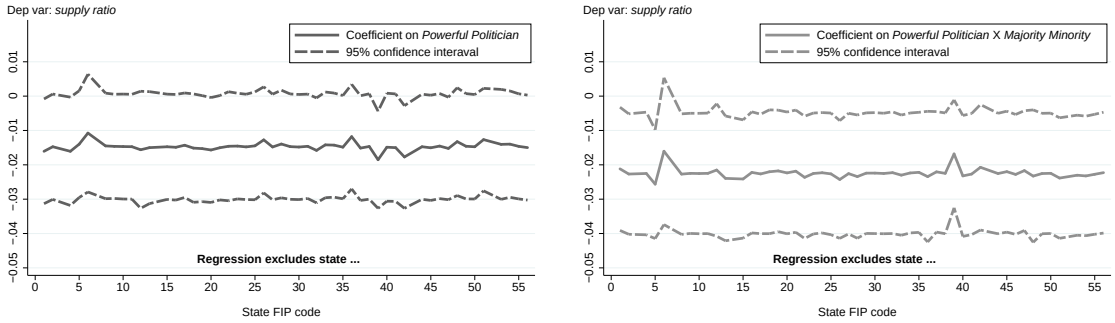


Figure A.2: **Sensitivity of Relation Between Senator Power and Consumer Credit Constraints**

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

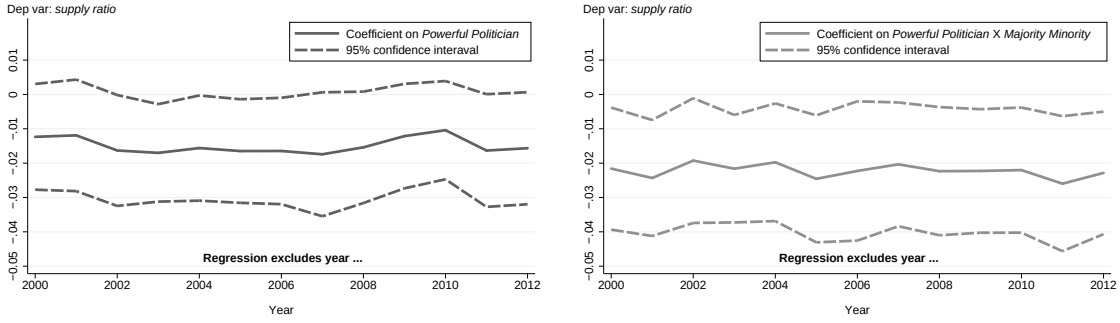
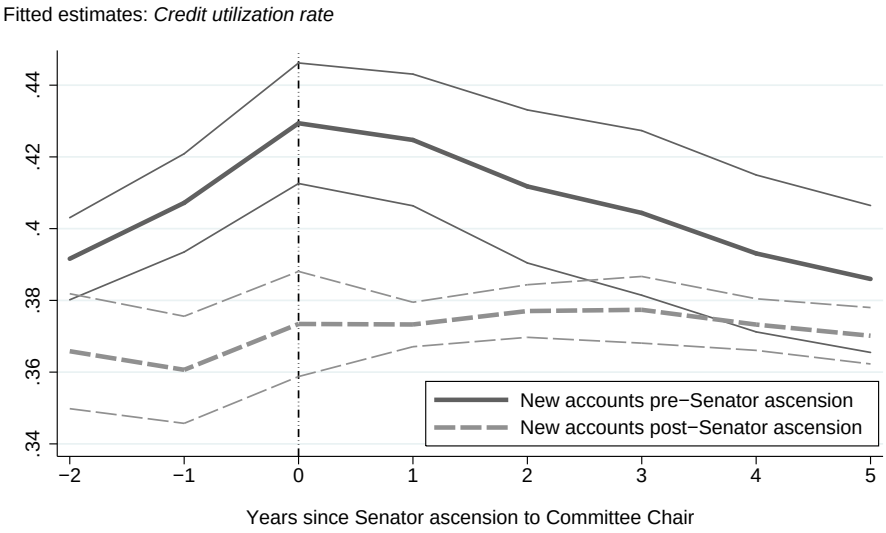


Figure A.3: **New Accounts and Borrower Dynamics**

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.



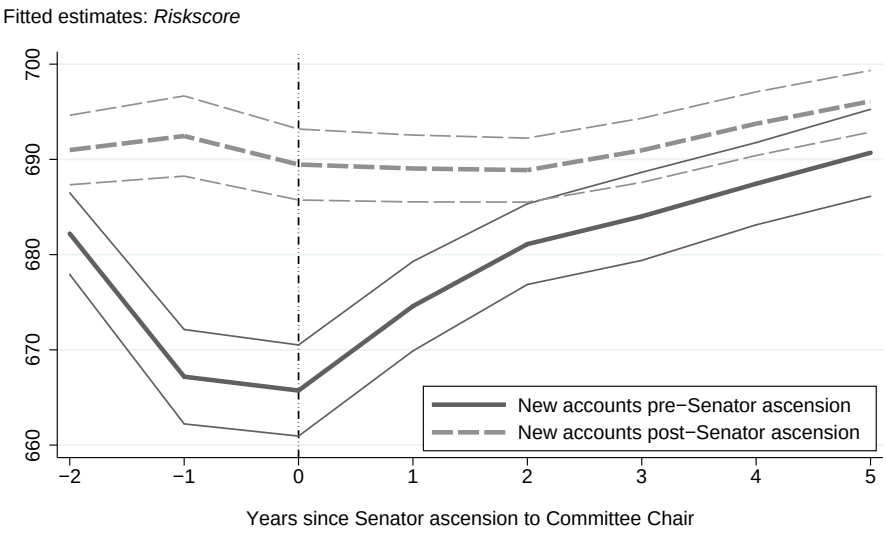


Table A.1: **Senator Shocks and State Macroeconomic Conditions**

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