Trading Credit (Subsidies) for Votes: The Effect of Local Politics on Small Business Lending*

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Abstract

Small businesses are championed by politicians looking to win votes, but face significant credit constraints that limit their growth. We study how the competitiveness of congressional elections affects government subsidies to small business lending. To identify the causal impact of electoral competitiveness, we examine politically-motivated congressional redistricting ("gerrymandering") and exploit the discontinuity in post-redistricting electoral competitiveness between districts where redistricting party incumbents narrowly won and narrowly lost the pre-redistricting election. We find that districts with electorally vulnerable congressional representatives receive larger Small Business Administration (SBA) loan guarantees than districts with more entrenched representatives, and this leads to higher growth in local employment and wages in the short run. The gains in employment and wages disappear in the long run, and is accompanied by a decline in the number of business establishments. Overall, our results suggest that politically-motivated credit subsidies to small businesses can provide short-term economic benefits, but may distort local credit markets in the long run.

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1 Introduction

The United States Congress wields significant power over the distribution of taxpayer-financed financial resources.[1] Individual members of Congress, motivated by the desire to win elections, have the incentive to deliver a greater share of those federal dollars to their constituents. If politician effort is determined by electoral accountability (Besley and Case, 1995), then a vulnerable incumbent congressional representative facing a strong electoral challenge should work harder to bring federal financial resources to her constituents than an entrenched incumbent insulated from electoral pressures.

One way that politicians cater to their constituents is through the provision of government-subsidized credit.[2] In particular, credit programs targeted at *small businesses* tend to be popular with voters. Small business owners are celebrated as paragons of positive values such as risk-taking, hard work, and perseverance in American culture, and their resonance with voters across the political spectrum is well understood by politicians. According to the Sunlight Foundation Capitol Words database, the phrase "small businesses" appeared more than 10,000 times in the Congressional Record in a two-year period.[3]

Loan subsidy programs targeted at small businesses in the United States are administered by the Small Business Administration (SBA). We study how the electoral vulnerability of congressional members of the House of Representatives affects the allocation of SBA lending to small businesses. SBA lending presents a particularly attractive target for politicians as it involves a significant amount of financial capital being channeled to a politically popular set of recipients, in which the involvement of private sector banks speeds up the distribution of funds and obfuscates the ultimate financial burden on taxpayers. We hypothesize that electorally vulnerable incumbents acquire a greater volume of SBA loans for their constituents relative to electorally secure incumbents.

As elections are decided by many factors, including local economic conditions, identifying the causal impact of electoral vulnerability on SBA loan allocation is challenging. We address this challenge by exploiting an institutional feature of the U.S. Congress: the periodic redrawing of congressional district boundaries. Our empirical design relies on the fact that, in the majority of U.S. states, the party with majority control of the state legislature also controls the redistricting process. The "redistricting party", with the objective of max-

[1] According to the CBO, in 2015, the US government spent $293 billion on non-defense investment and $180 billion on defense-related investment, where investment is defined as payment for goods and services "that are expected to be useful some years in the future" (`https://www.cbo.gov/publication/52463`).

[2] For instance, Akey, Dobridge, Heimer, and Lewellen (2018) find that entrenched politicians' constituents have more difficulty accessing consumer credit.

[3] See www.npr.org/sections/itsallpolitics/2012/04/18/150822919/small-businesses-get-big-political-hype-whats-the-reality.

imizing the number of congressional districts under party control, has a strong incentive to allocate "friendly" votes toward districts that are closely contested.

Given the incentive to target closely contested districts, we hypothesize that partisan gerrymandering should render a narrow-victory incumbent from the non-redistricting party significantly more vulnerable than a narrow-victory incumbent from the redistricting party. This naturally leads us to implement a novel regression discontinuity design (RDD), in which the "forcing" variable is the redistricting party's electoral margin of victory (defeat) in the election immediately prior to redistricting.

Under our hypothesis, we expect a discontinuous positive jump at the zero vote margin cutoff: below the cutoff the redistricting party narrowly loses and gerrymanders *against* the incumbent and above the cutoff the redistricting party narrowly wins and gerrymanders *in favor of* the incumbent. We find evidence in support of this hypothesis: a narrow win by the redistricting party results in a 20% higher vote margin for the incumbent in the post-redistricting election relative to a narrow loss by the redistricting party. This translates to a 29% higher probability of the incumbent winning the post-redistricting election across the cutoff.

Using this sharp discontinuity in electoral vulnerability, we show that more vulnerable incumbents (i.e., incumbents from districts in which the redistricting party narrowly lost) bring in larger amounts of SBA-backed lending to their districts. On average, annual SBA loan guarantees increase by over $10 million in districts where the redistricting party narrowly loses relative to districts where the redistricting party narrowly wins. This increase is mostly explained by increases in average loan size, rather than by increases in the number of loans. The results are also more pronounced for loans distributed through banks that are more sensitive to local politics, such as those headquartered in the same geographic region as their borrowers.

For our benchmark tests on SBA outcomes, we focus on the two-year window before the post-redistricting election. In the period *following* the post-redistricting election, the effect of incumbent vulnerability becomes conflated with that of incumbent tenure as more vulnerable incumbents are replaced by inexperienced newcomers at a higher rate. Prior research has shown that congressional seniority is positively associated with the ability to direct federal resources to one's constituents (Levitt and Poterba, 1999; Cohen, Coval, and Malloy, 2011; Akey, Heimer, and Lewellen, 2017). When examining long-run outcomes, we find some evidence that longer congressional tenures are associated with increases in the number of SBA loans that a district receives.

We also investigate the ultimate real effects of electoral vulnerability on district-level employment, wages, and establishment counts. Small businesses are widely seen as engines

of economic growth (Birch, 1987), but have limited access to finance due to opacity and transaction costs (Berger and Udell, 1998). Thus, the competitiveness of regional elections can affect the geographic distribution of economic growth by easing credit constraints facing small businesses. We find that districts with electorally vulnerable incumbents experience increases in employment and wages. These effects appear to be temporary, however, as we find the increases in employment and wages fade away in the long run. This is consistent with findings by Davis, Haltiwanger, and Schuh (1996) that, due to their high rates of failure, small businesses both create and destroy jobs at a high rate, and therefore do not generate *net* employment growth over the long run.

Lastly, we look for evidence that electorally vulnerable congressional representatives direct resources to their constituents through channels other than SBA lending. We find no evidence of an overall effect on small business credit, which suggests that SBA lending is more politically sensitive than other forms of small business lending. We also find no evidence of an effect on other forms of federal financial assistance, including government contracts, grants, and direct transfers. This is consistent with the distinctly widespread popularity of small businesses among the voting public, as well as the expedient manner in which SBA loans can be disbursed through private sector intermediaries. Nevertheless, we are open to the possibility that political pressure from vulnerable politicians affects other forms of financial resource allocation.

2 Literature Review

Our paper contributes to the broader literature on the effects of politics on the geographic allocation of economic resources. Much of the literature focuses on the role of political *power*, showing that federal funding tends to be directed towards districts held by long-tenured incumbents (Boyle and Matheson, 2009) and incumbents who hold senior positions in agenda-setting committees (Alvarez and Saving, 1997; Engstrom and Vanberg, 2010; Cohen et al., 2011). We ask a fundamentally different question by looking at how the incentives of vulnerable politicians, rather than the influence of powerful politicians, affect economic outcomes.

Prior research provides limited evidence that electoral competitiveness is positively related to targeted federal spending (Engstrom and Vanberg, 2010) and to local economic growth (Levitt and Poterba, 1999), but does little to address potential confounding economic factors related to electoral competitiveness. Prior research also has done little to distinguish between political power and electoral vulnerability, with good reason. Electoral vulnerabil-

ity is often inversely related to political power[4] and disentangling vulnerability from power has presented a difficult challenge that has largely eluded empiricists. We present a novel empirical strategy that aims to identify the effect of electoral vulnerability, distinct from other economic and political factors.

Our research also relates to an emerging literature on the economic effects of congressional redistricting. For example, Denes, Schulz, and Vig (2017) find that firms that change congressional representation due to redistricting experience higher uncertainty, lower abnormal equity returns, and decrease their capital and R&D expenditures. Most relevant to our paper, Akey, Dobridge, Heimer, and Lewellen (2018) find that the irregularity of congressional district boundaries is positively associated with declines in consumer credit. While they associate gerrymandering-induced boundary irregularity with incumbency advantage, we show that gerrymandering can either entrench or destabilize a congressional incumbent based on the alignment of the incumbent with the redistricting party. We rely on narrow election outcomes rather than district boundary shapes for identification. Moreover, we study small business lending and local economic growth rather than consumer credit.

Lastly, our research relates to the literature on small business financing and SBA loans. While the importance of small businesses in generating job creation is debated among economists,[5] researchers are generally in agreement that small businesses face significant constraints in accessing external finance (Berger and Udell, 1998; Beck, Demirgüç-Kunt, and Maksimovic, 2005; Schmalz, Sraer, and Thesmar, 2017). Recent evidence suggest that SBA loans have a positive effect on employment and income growth (Craig, Jackson III, and Thomson, 2007; Brown and Earle, 2017). Given the importance of SBA loans in driving economic growth, we explore the political incentives that drive the geographic allocation of SBA loans in the first place.

3 Federal Small Business Loans

3.1 SBA Lending

The U.S. Small Business Administration (SBA) administers federal loan guarantee programs aimed at helping "Americans start, build, and grow businesses".[6] The SBA operates two major capital access programs targeted at small businesses: the 7(a) loan guarantee program and the 504 loan guarantee program. The names of the two programs come from Section

[4] In Congress, the most influential elected representatives tend to be the longest-tenured members who have successfully won many elections and thereby are electorally secure.

[5] See Birch (1987), Davis et al. (1996), and Carree and Klomp (1996).

[6] See `https://www.sba.gov/about-sba/organization`.

7(a) of the Small Business Act of 1953 and Section 504 of the Small Business Investment Act of 1958, respectively.

The SBA 7(a) loan program is the SBA's flagship small business lending vehicle, with 60,353 loans totalling approximately $25.4 billion approved in 2018 (Dilger, 2019). The 7(a) program constitutes a *loan guarantee* program, through which the government provides guarantees on loans made by private sector intermediaries. This is in contrast to direct loan programs, in which the federal government itself acts as the lender. There are over 3,000 private sector SBA 7(a) lenders across the country, including large national banks such as Wells Fargo and JP Morgan Chase. Interest rates are negotiated between the borrower and lender, but are subject to SBA maximums pegged to a benchmark such as the LIBOR rate or the prime rate.

Under standard *non-delegated* SBA 7(a) loan processing, the originating lender submits an application package to the SBA requesting an SBA guarantee. The SBA conducts its analysis, and typically confirms the lender's credit decision within seven to 10 business days.[7] Some lenders have special authority to process 7(a) loans without submitting to loan-level oversight from the SBA. The most prominent delegated loan program is the Preferred Lenders Program (PLP), which grants the most experienced SBA lenders the authority to process, close, service, and liquidate SBA-guaranteed loans without prior SBA review. The other large category of delegated 7(a) loans is the SBA Express program, which grants lenders the authority to process and approve small "express" loans in a short amount of time.

The maximum loan amount for a regular non-delegated SBA 7(a) loan is $5 million, for which the SBA can guarantee up to 75% of the loan. For loans under $150,000, the SBA can guarantee up to 85% of the loan. The same limits apply for PLP loans, while SBA Express loans are capped at $350,000 with a maximum government guarantee of 50%. While there are several additional categories of SBA loans, regular 7(a) loans, PLP loans, and SBA Express loans constitute over 88% of all SBA 7(a) loans, with no other sub-category making up more than 4%.[8]

To qualify for a 7(a) loan, a business must be domiciled in the U.S., operate on a for-profit basis, have its owner's equity invested in the business, and satisfy size limit requirements that vary by industry.[9] The maximum size limit may be in terms of total annual sales, number of employees, or total assets. For example, a commercial bank must have less than $550 million in assets to qualify, while an automobile manufacturer must have fewer than 1,500 employees.

[7] See `www.occ.gov/publications-and-resources/publications/community-affairs/community-developments-insights/ca-insights-dec-2014.html`.

[8] These other sub-categories largely consists of special purpose programs targeted at promoting export and trade, at economically-undeserved communities, and at business-owning military veterans.

[9] Industry-specific size limits are available at `www.sba.gov/document/support--table-size-standards`.

Loan proceeds from 7(a) loans may be used for a wide range of purposes, including working capital investment, capital expenditures, business acquisitions, and refinancing of existing debt. The program is also specifically targeted at financially-constrained businesses, as an applicant must show that it has fully exhausted non-SBA loan options as a source of financing in order to be eligible.

The SBA 504 loan guaranty program constitute the other major federal loan guarantee program targeted at small businesses. With 6,099 loans approved in 2019 totaling nearly \$5 billion, the 504 loan program is significantly smaller than the 7(a) program.[10] The program provides long-term fixed-rate loans and, unlike the broader 7(a) program, is targeted specifically at the financing of fixed assets such land, buildings, machinery, and equipment, but explicitly not working capital. To qualify for eligibility, small businesses cannot have more than \$15 million in tangible net worth or an average net income of more than \$5 million for the two fiscal years prior to applying. There is no limit, however, on the size of the project being financed.

Another major difference between 504 and 7(a) loans is that 504 loans involve participation from Certified Development Companies (CDCs), which are private nonprofit corporations established to contribute to the economic development of their local communities. A SBA 504-funded capital project is partially financed by a CDC and partially by a third-party lender, typically a commercial bank. The bank can provide up to 50% of the financing, secured by a senior lien, and the CDC can provide up to 40% of the financing, secured by a junior lien. The small business applicant must contribute at least 10% of the financing using their own equity. The federal government subsidizes only the portion financed by the CDC, a 100% guarantee of CDC-issued debentures by the SBA.

The combined involvement of the SBA, a CDC, a third-party lender means that the application process for 504 banks are lengthy and laborious.[11] Just as the 7(a) program provides special delegated authority status to partner banks, the 504 program provides special delegated authority to CDCs. These consist of the Accredited Lenders Program (ALP) and the Premier Certified Lenders Program (PCLP), both of which grant designated experienced CDCs the increased authority to process, close, and service 504 loans.

We focus on SBA 7(a) and 504 loan programs in this paper due to their large size and wide scope. The SBA also administers a disaster loan program, which are available to individuals and businesses in federally declared disaster areas. The SBA disbursed \$3.59 billion in disaster loans in 2018, but approximately 80% of disaster loans are issued to individuals

[10] According to Congressional Research Service report RL33243, available online at `fas.org/sgp/crs/misc/RL33243.pdf`.

[11] According to Fundera, an online marketplace that connects small businesses with lenders, it often takes several months and many hours of paperwork (see `www.fundera.com/blog/sba-504-loan`).

and households rather than businesses. Lastly, the SBA administers a microloan program to nonprofit intermediary microloan lenders that, in turn, provide small loans of up to $50,000 to small businesses and nonprofit child care centers. The microloan program is relatively small in magnitude, with 5,459 loans totaling %76.8 million in 2018, and the SBA currently does not provide data on its microloans.

3.2 Hypothesis Development

We hypothesize that a vulnerable incumbent congressional representative facing a strong reelection challenge will bring in more financial resources specifically targeted at small businesses in her district, which implies a higher volume of SBA lending, relative to an entrenched representative who is insulated from electoral pressures. Given prior research showing Congresspeople who bring more federal dollars to their districts perform better in elections (Levitt and Snyder Jr, 1997), and the widespread popularity of small businesses in the eyes of the voting population, providing financial support for small businesses should be especially attractive to politicians.

How can an individual congressional representative influence the flow of SBA loans to their district? One way is to use their prominent position to apply pressure on agency bureaucrats at the SBA. This is consistent with empirical evidence from Scholz, Twombly, and Headrick (1991) that local partisan activities of legislators result in "street-level" influence of federal bureaucracies. A common practice is for congressional representatives to write letters to the SBA advocating for their constituents who applied for SBA loans.[12]

Congressional offices can also help their constituents by offering resources to overcome obstacles that they face in accessing SBA loans. For example, the website of Maryland Representative Ami Bera offers support to local small businesses applying for SBA loans by helping to "cut through government red tape".[13] As the complexity of the SBA program has grown over the years, congressional offices can also reduce frictions by helping constituents navigate the various sub-categories of SBA loans, such as programs targeted at exporters, veteran-owned businesses, low-income communities, etc.

While the U.S. government provides guarantees for SBA 7(a) and 504 loans, financial intermediaries such as banks and CDCs are largely responsible for processing and approving applications, especially in the case of delegated authority programs such as the PLP and SBA Express. Given the highly-regulated nature of the financial sector and active role of banks in politics, banks may respond to pressure from congressional representatives to increase the

[12] Examples of return correspondence from the SBA can be found on its website, `www.sba.gov/document/report--congressional-correspondence`.

[13] See `https://bera.house.gov/helpforbusinesses`.

flow of SBA lending their district, with the expectation of future political favors in return.[14]

Even in the absence of explicit political pressure from the local congressional office, banks may find it in their best interest to help the incumbent representative from their district stay in power. For example, banks may want to avoid the increased political uncertainty that accompanies a change in congressional representation. Recent work by Denes et al. (2017) show that changes in congressional representation result in higher levels of firm-level uncertainty and negative abnormal equity returns. Furthermore, the defeat of a sitting incumbent means the election of a freshman legislator with limited influence in Congress and hence limited means to benefit one's constituents.[15]

If more competitive elections induce incumbent representatives to obtain more SBA loans for their constituents, then what are the ultimate implications for the growth of the local economy? Here, we are faced with two competing hypothesis.

First, the *credit-easing hypothesis* predicts that higher amounts of politically-motivated SBA lending directed towards a congressional district should lead to stronger local economic growth. This prediction arises naturally from the widely-held view that small businesses are the primary drivers of job creation in the United States (Birch, 1987). Small businesses also tend to be financially constrained (Beck and Demirguc-Kunt (2006)), which implies the subsidization of credit targeted to alleviate the financial constraints on small businesses should have a large effect on economic growth. Prior research has provided empirical evidence of the positive effects of SBA lending on employment (Brown and Earle, 2017; Craig et al., 2007).

In contrast, the *misallocation hypothesis* predicts that higher amounts of politically-motivated SBA lending should lead to weaker local economic growth. This counter-intuitive prediction is based on the idea that government interference by self-interested politicians distorts the coordinating role of the market and leads to economically inefficient outcomes. Small businesses may be popular politically, but their perceived role as vital engines of job creation may be overstated, as prior research has documented that they both create and destroy jobs at a high rate (Davis et al., 1996). Thus, directing public funds toward financing small businesses for political purposes may generate short term job gains, but those jobs are unlikely to permanent.

We note that our empirical strategy is designed to identify the causal effect of electoral competition rather than the causal effect of SBA lending. Any effect that electoral competi-

[14] See Mian, Sufi, and Trebbi (2013), Akey et al. (2017), and Chavaz and Rose (2018) for evidence of banks benefiting from political connections.

[15] Upon his retirement, long-serving Maine Senator George J. Mitchell remarked that the most agonizing part of his decision was that the freshman Senator replacing him would lack the political clout in Congress to help his home state.

tion has on local economic outcomes may work through channels other than SBA loans. For example, more competitive elections potentially result in greater policy uncertainty, which has a dampening effect on economic activity.[16] Therefore, we cannot distinguish between a misallocation channel and a policy uncertainty channel if we were to find a *negative* connection between electoral competition and local economic growth. Similarly, any *positive* connection established between electoral competition and local economic growth cannot solely be attributed to the easing of credit for small businesses, as there may be other ways for vulnerable incumbents to bring economic benefits to their districts. In our empirical analysis, we explore potential alternative channels for politically-influenced resource allocation.

4 Empirical Design

We aim to identify the causal effect of congressional electoral competitiveness on district-level government financing of small businesses and economic growth. A simple OLS regression approach is subject to severe omitted variable concerns, as election outcomes are affected by many different factors, including local economic conditions. Considering that reelection rates for incumbent U.S. congressional representatives are extremely high,[17] a *vulnerable* incumbent Representative is especially likely to be facing an abnormal set of circumstances. This raises two major endogeniety concerns.

The first concern is that the electoral vulnerability of an incumbent Representative is likely to be correlated with local economic conditions in the district. A vulnerable incumbent may receive a greater share of federal assistance not because of their electoral vulnerability, but because their district was experiencing poor economic growth and thus in greater need of government support. This concern would lead one to obtain upward-biased estimates on the causal impact of electoral vulnerability.

The second major concern is that electoral vulnerability is correlated with electoral tenure–i.e. a secure incumbent is more likely to win reelection and therefore serve more terms in Congress than a vulnerable incumbent. Given prior research showing that congressional seniority is associated with higher levels of federal assistance (Levitt and Poterba (1999), Cohen et al. (2011)), incumbents who are reelected should wield greater influence over federal spending than newly-elected incumbents.[18] This concern would lead one to

[16] See, for example, Baker, Bloom, and Davis (2016), Gulen and Ion (2015), Julio and Yook (2012), and (Denes et al., 2017), among others.

[17] House incumbents have consistently experienced reelection rates of over 90% since the 1960s according to `www.opensecrets.org/overview/reelect.php`. This can largely be explained by advantages in name recognition and campaign financing.

[18] In 1994, departing Maine Senator George Mitchell remarked that the hardest part of his decision to retire was "recognizing that his position enabled him to help his home state in ways that a freshman taking his

obtain downward-biased estimates on the causal impact of electoral vulnerability.

Our solution to the endogeneity problem is to exploit an institutional feature of the U.S. political system that requires district boundaries for the House of Representatives to be periodically redrawn. Before describing the details of our identification strategy, we first provide some institutional details about the redistricting process.

4.1 Congressional Redistricting

Representatives for the U.S. House of Representatives serve two-year terms and are considered for reelection every even-numbered year. The redrawing of House district boundaries, a process known as redistricting, occurs immediately following the decennial census that takes place in years ending in a '0'. Following each census, the newly-drawn map first takes effect in the subsequent election, in the year ending in a '2'. The primary purpose of redistricting is to ensure that every district in the country has nearly equal populations. However, those in charge of redrawing district maps often have political motives and may be looking to manipulate the redrawing of the map to advantage or disadvantage specific political parties or population groups.

The history of politically-motivated manipulation of congressional maps stretches back to the early history of the United States. In a famous example from 1812, Massachusetts Governor Elbridge Gerry redrew the electoral boundaries in his state in a way that obviously benefited his Democratic-Republican Party. In a cartoon satirizing one particularly oddly-shaped district in Governor Gerry's Massachusetts congressional map, the Boston Gazette coined the term "gerrymander", a portmanteau of the Governor's name and "Salamander", the shape of which the offending district mimicked. The term has stuck around ever since.

The practice of gerrymandering has been heavily criticized for its harmful effects on democracy, but persists to this day. In fact, the use of advanced computation techniques has increased the sophistication of gerrymandering techniques in recent years.[19] An infamous example of modern day gerrymandering can be seen in the evolution of Pennsylvania's 7th district as shown in Figure I. In some cases, newly-drawn congressional maps are challenged in court by the disadvantaged party. Such cases can make it all the way up to the Supreme Court, as seen in recent examples from Wisconsin, Maryland, and North Carolina.[20] The Supreme Court has been hesitant to impose strict limits to the practice, however, due to the

place could not" (see `www.nytimes.com/1994/03/06/us/leader-on-senate-not-a-lifetime-job.html`).

[19] For example, recent works using statistical simulations find the redistricting that took place in Wisconsin following the 2010 Census resulted in an electoral map heavily skewed towards Republican candidates (Chen (2017), Herschlag, Ravier, and Mattingly (2017)).

[20] See `www.nytimes.com/2019/01/04/us/politics/gerrymandering-supreme-court.html` for more details.

difficulty in defining a consistent workable standard for evaluating the constitutionality of redrawn maps.

The prevalence of partisan gerrymandering is largely due to the fact that, in the majority of U.S. states, the redistricting process is overseen by the state legislature. This means that a state legislature under a single political party's control is able to redraw the congressional map in that state in a manner that favors that party. This practice is known as *partisan* gerrymandering. The alternative is to appoint an independent commission or bipartisan commission to oversee redistricting, which a minority of states have implemented in order to avoid partisan bias. A few low-population states contain only one district and therefore are not subject to concerns about redistricting bias.

The partisan gerrymanderer can use many techniques to draw district boundaries in a way that favors their own party. For example, in states where both parties are competitive, the redistricting party can "pack" the opposing party's voters into a small number of districts in order to gain a disproportionate number of seats. In states in which the redistricting party has a sizable electoral advantage, they can "crack" the opposing party's voters by spreading them out over many districts in a way to give themselves an even more dominant majority.

Many states have adopted districting principles and criteria that effectively constrains the "redistrictor" from having carte blanche mapping powers.[21] Several criteria, especially one requiring the cores of prior districts to be preserved, imply a degree of continuity in the geographic position of a given district over time. One should not see, for example, an incumbent congressperson suddenly finding their constituents shifting from the northwest corner of a state to the southeast corner of a state following a single redistricting cycle. Even in egregious cases of gerrymandering, as seen in the evolution of Pennsylvania's 7th district in Figure I, most redistricting cycles resulted in marginal changes around the core of the district shape from the previous cycle.

Given that the redrawing of district boundaries takes place largely around the margins of existing districts, we expect gerrymandering parties to try to increase their vote margins in *closely-contested* districts. The rationale is that a redistrictor looking to give his party an advantage in congressional seats in the state will look to target congressional races that are close to the tipping point. They can do this by allocating territories with friendly voters *towards* the target district or by allocating territory with unfriendly voters *away* from the target district.

Figure II provides a stylized illustrative example of this strategy. Votes for two opposing parties are represented by red and blue cells in a grid. We focus on one particular district delineated by the region of bold cells, with the faded cells representing surrounding districts.

[21] A brief description of these criteria can be found in Appendix A.

Figure II(a) represents the last congressional election before redistricting (i.e. in a year ending in '0'), in which the red party achieves victory by a single cell. We consider two cases, in which the blue party controls redistricting, and the red party controls redistricting.

In Figure II(b), we see that under red party control of redistricting, the gerrymanderer pulls in some red cells from surrounding districts in order to solidify their hold onto seat. In Figure II(c), we see that under blue party control of redistricting, the gerrymanderer pulls in some blue cells from the surrounding districts in order to flip the seat to be under blue control. The implication is that the gerrymandering party would aim to *entrench* the incumbent in a district that their party narrowly holds, and aim to *destabilize* the incumbent in a district that their opponent narrowly holds. This is the insight that we exploit in designing our identification strategy.

4.2 Identification Strategy

Our identification strategy exploits the partisan gerrymandering of closely-contested districts as described in the previous section. We predict that an incumbent congressperson who narrowly won the last *pre-redistricting* election (in a year ending in '0') should have stronger reelection prospects in the first *post-redistricting* election (in a year ending in '2') if she were a member of the redistricting party than if she were a member of the party in opposition to the redistricting party.[22] Identification relies on the assumption that this predicted difference in reelection prospects is purely driven by partisan gerrymandering and uncorrelated with local economic factors.

In the "first stage" of our analysis, we empirically verify that a narrow victory by the redistricting party in the pre-redistricting election predicts a larger incumbency advantage in the post-redistricting election relative to a narrow victory by the non-redistricting party in the pre-redistricting election. A visualization of this prediction is presented in Figure III, in which the horizontal axis represents the margin of votes for the redistricting party in the pre-redistricting election and the vertical axis represents the margin of votes for the incumbent candidate in the post-redistricting election.

The blue line in Figure III represents predicted vote margins *in the absence* of gerrymandering. The right half of the plot indicates that a narrow victory by the redistricting party predicts a close race in the next election, and a lopsided victory by the redistricting party predicts an easy victory for the incumbent party in the next election. This is mirrored in the left hand side of the plot, which shows a narrow victory by the non-redistricting party predicts a close race in the next election, and a lopsided victory by the non-redistricting

[22] The U.S. political landscape is dominated by two parties: the Democrats and Republicans. Therefore, it is reasonable to assume an incumbent is in one of the two parties.

party predicts an easy victory by the incumbent party in the next election. Without gerrymandering, the persistence of incumbency advantage should be symmetrical on both sides of the plot.

The red line in Figure III represents predicted vote margins *in the presence* of gerrymandering. Here, the symmetry between the left side and right side of the plot is broken. Notably, a close victory by the redistricting party should result in gerrymandering that entrenches the incumbent while a close victory by the non-redistricting party should result in gerrymandering that destabilizes the incumbent. This is represented by the red line falling below the blue line just to the left of the vertical axis and the red line coming above the blue line just to the right of the vertical axis.

We exploit the predicted discontinuity at the point where the redistricting party barely wins versus barely loses the pre-redistricting election by applying a regression discontinuity design (RDD) approach in which the pre-redistricting vote margin acts as the "forcing" variable. We implement this approach according to the following specification:

$$IncumbMargin_{i,c+2} = \alpha + f(RedistMargin_{i,c}) + \beta \times RedistrictorLoss_{i,c} + \epsilon_{i,c}, \qquad (1)$$

where i indexes districts and c indexes redistricting cycle years that end in a '0' (i.e., $c \in (2000, 2010)$), $IncumbMargin$ denotes the difference in percentage of votes received by the incumbent party candidate and the best-performing challenger candidate, $RedistMargin$ denotes the difference in percentage of votes received by the redistricting party and the non-redistricting party, $f(\cdot)$ represents a polynomial spline (we use independent cubic functions on the two sides of the zero $RedistMargin$ threshold in our baseline tests), $RedistrictorLoss$ denotes a dummy variable indicating whether $RedistMargin < 0$ (i.e. whether the redistricting party loses), and ϵ represents the residual error term. The redistricting party is defined as the party that controls the state legislature.

We define our variable of interest $RedistrictorLoss$ as an indicator of the redistricting party *losing* rather than winning in order to interpret the coefficient as the estimated effect of incumbent vulnerability rather than entrenchment. We expect the coefficient β to be positive, indicating a narrow redistrictor loss to produce a more vulnerable incumbent than a narrow redistrictor win. Given the inherent uncertainty of elections, the inability of politicians to precisely control which side of the discontinuity that they fall on implies that variation in "treatment" near the discontinuity threshold is randomized as though from a randomized experiment (Lee and Lemieux, 2010).

To check the validity of our RDD estimation framework, we verify that there are no sig-

nificant differences in district-level demographic and socioeconomic characteristics between "treatment" and "control" districts across the discontinuity threshold (Lee, 2008), and that the marginal distribution of $RedistMargin$ is smooth across the discontinuity threshold (McCrary, 2008).

We focus on states in which the state legislature oversees the redistricting process, as these are the states subject to gerrymandering motivated by partisan considerations. However. we also examine states that assign a bipartisan or independent commission to oversee the redistricting process as well as states containing only one district. If the redistricting process in these states are not subject to motivations driven by partisan politics, then we can use such "non-partisan states" as a placebo test, in which we define the party in control of the state legislature as the "psuedo-redistrictor". We expect that the discontinuity found at the cutoff point for the partisan states will be absent in the sample of non-partisan states.

In the "second stage" of our analysis, we study how electoral competitiveness as identified through the first stage of our analysis affects district-level SBA loan outcomes. To this end, we estimate the following regression:

$$\Delta Y_{i,c} = \alpha + f(RedistMargin_{i,c}) + \beta \times RedistrictorLoss_{i,c} + \epsilon_{i,c}, \tag{2}$$

where $\Delta Y_{i,c}$ represents the *change* in SBA loan outcomes from the pre-redistricting period to the post-redistricting election in district i. By comparing SBA outcomes before and after redistricting in narrow-redistrictor-win districts versus narrow-redistrictor-loss districts, we effectively apply Difference-in-Differences (DiD) estimation on top of our RDD setup. This further sharpens our identification by differencing out any time-invariant differences in economic conditions between narrow-win and narrow-loss districts.

Given our previously-stated concern that longer-tenured incumbents tend to have a greater ability to acquire federal resources for their constituents, we are careful not to conflate the effect of *anticipated* electoral competition with the effect of *realized* electoral tenure. To this end, we define the *early cycle* post-redistricting period as the years between the last pre-redistricting election and the first post-redistricting election, as illustrated in Figure IV. This is the period in which the effects of the gerrymandering are anticipated, as it is known which vulnerable incumbents are aligned with the redistricting party and which are opposed, but the impact has not yet been translated into differences in realized tenures. Therefore, we define the early cycle change in SBA loan outcomes as:

$$\Delta Y_{i,c}^{Early} \equiv \bar{E}_i(Y_{t\in(c+1,c+2)}) - \bar{E}_i(Y_{t\in(c-1,c)}) \tag{3}$$

where $\bar{E}_i(\cdot)$ denotes the time series average within district i, the pre-redistricting period

$t \in (c-1, c)$ spans the two years immediately preceding the pre-redistricting election, and the early cycle post-redistricting period $t \in (c+1, c+2)$ spans the two years immediately following the pre-redistricting election. Because $\Delta Y_{i,c}^{Early}$ captures changes in SBA lending due to *anticipated* electoral outcomes, we should be able to disentangle the effect of electoral competitiveness from the effect of realized electoral tenure.

We also investigate the period that comes after the first post-redistricting election, which we denote as the *late cycle* period in Figure IV. We define the late cycle change in SBA lending as

$$\Delta Y_{i,c}^{Late} \equiv \bar{E}(Y_{c+3 \leq t \leq c+8}) - \bar{E}(Y_{t \in (c-1,c)}) \tag{4}$$

which focuses on the time period during which incumbent Representatives to the left side of the discontinuity threshold are replaced by newcomers at a higher rate than to the right side of the discontinuity threshold. If longer tenures result in a greater ability to obtain federal resources, then we should expect a positive difference at the discontinuity cutoff for late cycle changes.

In the "third stage" of our analysis, we study the effects of incumbent vulnerability on local private-sector economic growth. We perform the same tests as we perform on SBA lending, except with $\Delta Y_{i,c}$ now representing changes in district-level employment, wages, and business establishments counts. We expect increased SBA lending to result in stronger local economic growth under the credit-easing hypothesis, and weaker local economic growth under the misallocation hypothesis.

Lastly, we check whether vulnerable incumbents look to alternative channels, apart from SBA lending, to stimulate the local economy. Specifically, we study the effects on overall lending to small businesses by constructing $\Delta Y_{i,c}$ based on *overall* small business lending (i.e. not just SBA lending), and based on alternative forms of federal financial assistance including government contracts, grants, and direct payments. Examining these alternative channels allows us to gain a fuller understanding of the connection between the incumbent vulnerability of congressional representatives and local economic growth.

5 Data

We employ several data sources to execute our empirical analysis. In this section, we first provide a brief explanation for each non-standard dataset that we use. After that, we describe our data in detail to help contextualize our findings.

5.1 Data Sources

We employ United States House of Representatives election outcomes for two purposes: to build our "first stage" dependent variables ($IncumbMargin$ and $IncumbWin$) and our key explanatory variables ($RedistMargin$ and $RedistWin$). We get US House election outcomes for 1976-2016 from MIT Election Lab (MIT Election Data and Science Lab, 2017). These data provide vote tallies for candidates in primary, general, and special elections for U.S. House of Representatives seats from 1976 to 2016. We clean and process the data so that we end up with one observation for each political party involved in the general election in each congressional district for all elections years.

To construct $RedistMargin$, our key explanatory variable, we also require information on the state legislatures and executive branches in power at the turn of the decade in each state. As described in Section 4.1, the state legislature oversees the redistricting process in the majority of states. Many states also grant the Governor powers to veto any newly-drawn congressional map. Therefore, we define the redistricting party as the party that maintains control of both chambers of the state legislature for a particular state in 2000 and 2010, and if that state is subject to gubernatorial veto, control of the Governorship as well. We then define $RedistMargin$ as the win (or loss) margin for each district of the party in charge of redistricting at the start of each decade.

We obtain data on party control of state legislatures and Governorships for *partisan redistricting states* (i.e. states where the state legislature controls redistricting) from the website of Professor Justin Levitt at the Loyola Law School.[23] For the set of *non-partisan redistricting states* (i.e. states with independent or bipartisan redistricting commissions and states with only one district) we obtain data on state legislature and Governorship control from the Book of the States published by The Council of State Governments (2001, 2002, 2012) in order to construct "pseudo-redistricting" parties for our placebo tests.

A list of non-partisan and partisan redistricting states is provided in Table I, which also provides further details on party control of state legislatures and Governorships. Note that states do not enter our sample if the state legislature is not under unified control by one party or has a Governor with veto powers who is a member of the opposing party. Under these circumstances, one cannot define a redistricting party (or pseudo-redistricting party) to construct the forcing variable $RedistMargin$ in our RDD framework. There are 435 congressional districts in the United States House of Representatives. Our main sample consists of 237 districts in the 2000 redistricting cycle and 269 districts in the 2010 redistricting cycle and our placebo sample consists of 26 districts in the 2000 redistricting cycle and 98 districts

[23] See `http://redistricting.lls.edu/who.php`.

in the 2010 redistricting cycle, for a total of 506 district-cycle main sample observations and 124 placebo observations.

We obtain data on SBA lending from the "Frequently requested records" section of the SBA's Freedom of Information Act webpage.[24] Loan-level data for both 7(a) and 504 loans include the name and address of borrowers and lenders, the amount guaranteed by the SBA, the maturity of the loan in months, whether loan approval was delegated to the lender, and whether loan was fully repaid or charged off as uncollectible by the SBA. The SBA also reports the number of "jobs supported" for each small business loan it helps to guarantee, although these figures are self-reported by borrowers.[25]

While data on the amount guaranteed by the SBA is available for both SBA 7(a) and 504 loans, data on total loan amounts, including the non-guaranteed portion, is unavailable for a significant portion of the sample for 504 loans. Specifically, only the CDC portion of 504 loans is guaranteed, and data on the non-guaranteed portion provided by third party lenders is unavailable before 2009. Therefore, we focus our main analysis on *guarantee* amounts, although we also examine total loan amounts when studying 7(a) loans only in later analysis.

To study local economic outcomes in our analysis, we use the Quarterly Census of Employment and Wages (QCEW) conducted by the Bureau of Labor Statistics (BLS). We subset the data to private sector establishments only[26] and use annual number of establishments, number of employees, and wages paid at the county-level provided by the QCEW. We then create a mapping from counties to congressional districts using the Missouri Census Data Center (MCDC) geographic correspondence engine in order to convert county-level measures of establishment counts, employees, and wages into district-level measures.

Lastly, we obtain district-level measures of demographic and economic characteristics from the Decennial Census provided by the U.S. Census bureau. Specifically, we obtain snapshots of district-level measures of age, race, house values, urbanization, per-capita income, and unemployment rate in 2000 and 2001 from Census Summary File 1 and Summary File 3. These measures provide covariates that we look to balance across narrow-redistrictor-win and narrow-redistrictor loss districts in our RDD validity tests.

[24] See `www.sba.gov/about-sba/open-government/foia`.

[25] The number of jobs supported is reported as zero for a significant portion of the loans in our sample. We find evidence that, at the loan level, the number of jobs supported correlates well with loan size, except at the point where jobs supported equals zero, which suggests that self-reported zeros largely constitute noise in the data. Therefore, we code zeros as missing before calculating district-level averages for jobs supported.

[26] Note that "private establishments" refers to any non-governmental, for-profit organizations, including publicly listed firms.

5.2 Descriptive Statistics

In this subsection, we present a summary of our data in order to contextualize our findings. First, we discuss the time-span limitations of our sample imposed by data availability and institutional factors. Next, we provide descriptive statistics for election outcomes that we study as part of our "first stage" tests. Following that, we do the same for SBA lending outcome variables as well as QCEW-based local economic performance variables.

We limit our analysis on the 2000 and 2010 redistricting cycles for two major reasons. First, we require observations from years prior to redistricting to construct our differenced outcome variables, and data on SBA lending is available only starting in 1991. Second, gerrymandering techniques were limited in sophistication prior to 2000, as up until the 1990s, most legislators lacked access to computerized mapping software and drew districts using colored pens on acetate sheets overlaid on top of big maps on the floor.[27]

The forcing variable in our RDD is $RedistMargin$, the redistricting party's electoral margin of victory (defeat) in the pre-redistricting election. In the first two rows Table II, we show that the redistricting party achieves an average margin of victory (defeat) of 13.81 percentage points, and wins the pre-redistricting election 64 percent of the time. Figure V visually illustrates the geographic distribution of $RedistMargin$ across districts, with subfigures (a) and (b) representing the 2000 and 2010 elections, respectively. Yellow districts represent decisive victories by the redistricting party, red districts represent decisive victories by the non-redistricting party, and orange districts represent closely-contested districts.[28] We note that closely-contested elections are geographically distributed across the map and not clustered in one region.

We provide summary statistics for $IncumbMargin$ and $IncumbWin$ in the next two rows in Table II. The figures reveal that incumbent representatives typically achieve a margin of victory (defeat) of 32.31 percent, and win the post-redistricting election 89 percent of the time. While the average incumbency advantage is substantial, the data also shows significant variation across elections, with a standard deviation of 29.71% for $IncumbWin$. With our RDD approach, we seek to identify a component of this variation that is exogenous to local economic trends.

The summary statistics for SBA lending, aggregated at the district-year level, are presented in the next panel of Table II. We see that a congressional district receives on average \$29.1 worth of SBA loan guarantees across 127.8 loans per year, which translates to an av-

[27] See `www.economist.com/united-states/2002/04/25/how-to-rig-an-election`. In unreported tests, we find no evidence of gerrymandering targeting closely-contested districts in the manner described in this paper in earlier decades.

[28] Districts from states with non-partisan gerrymandering or is not controlled by a unified state legislature are left blank

erage guarantee of approximately $300,000 per loan. On average, each loan supports 12.7 jobs, is charged off at a rate of 6.7 percent, is fully repaid at a rate of 34.4 percent, and has a maturity of 177.1 months.[29] We note that SBA lending exhibits significant variation, as the standard deviations for *GtdAmt* and *NumLoans* are similar in magnitude to their respective means.

In the final panel of Table II, we report summary statistics for local private sector economic performance measures, aggregated at the district-year level. On average, there are 14,400 private sector establishments in each district, with approximately 1 establishment for every 46 people in the district. There are 247,000 private sector jobs in each district, which amounts to 0.37 jobs per person. Lastly, the private sector pays out $10.3 billion dollars in wages annually in each district, or approximately $15.3 thousand per person. To put these figures in context, the average district in our sample contains approximately 750,000 people.

6 Results

In this section, we present and discuss the results of our analyses on the effects of electoral competition on 1) election outcomes, 2) SBA lending, and 3) local economic growth. Before presenting our results, however, we first establish the validity of our regression discontinuity design.

6.1 Regression Discontinuity Design Validity

We follow standard tests to assess the validity of RD designs as described in Lee and Lemieux (2010). First, we check whether baseline covariates are balanced across the discontinuity threshold. We choose covariates that reflect district-level socioeconomic characteristics from the U.S. Census, measured immediately prior to redistricting (i.e. in years ending in a zero). These include the median age of the population (*MedAge*), the percent of the population that is non-Hispanic white (*PctWhite*), the percent of the population that is male (*PctMale*), the percent of the population residing in a rural region (*PctRural*), the median house value (*HouseVal*), and the unemployment rate (*UnempRate*).

As we show in Table III and Figure VI, none of the covariates differ significantly between districts where the redistricting party narrowly loses and districts where the party narrowly wins the pre-redistricting election. The absence of a discontinuous change across various

[29] The sum of the proportions of loans charged off and fully repaid because a substantial portion of loans do not report their repayment outcomes, either because they are active ongoing loans, or are exempted from reporting.

economic and social covariates helps to alleviate concerns of systematic differences between the closely contested districts.

Following Lee and Lemieux (2010), we also check whether the density of $RedistMargin$, the forcing variable in our RDD, is continuous at the discontinuity threshold. Figure VII reveals no evidence of bunching near the RDD cutoff. We perform a density smoothness test based on McCrary (2008) and find a t-statistic of 0.646 for rejecting the null hypothesis of no sorting in $RedistMargin$ at the zero cutoff, as shown in Figure VII(a). We also test for sorting based on a more recent manipulation test from Cattaneo, Jansson, and Ma (2016) and find a p-value of 0.593 for rejecting the null hypothesis, as shown in Figure VII(b). Both tests imply that it is highly unlikely that election outcomes were manipulated sufficiently to alter the distribution of $RedistMargin$ around the zero cutoff.

6.2 Incumbent Vulnerability

We test whether partisan redistricting in closely-contest regions affects incumbent vulnerability in the manner described in Section 4. We analyze incumbent win margins ($IncumbMargin$) in the first post-redistricting election ($c+2$) as well as the likelihood of incumbent reelection ($IncumbWin$) in that election, with and without controls. The results of regression analyses based on the empirical specification presented in Equation 1 for both outcomes are presented in Table IV and the first two subfigures of Figure VIII. From this point on, all tables report results from regressions that include district-level control variables, and all figures represent results from regressions estimated *without* control variables.

The exhibits clearly show that, in districts where the redistricting party loses the pre-redistricting election (c) by a narrow margin, the incumbent's electoral share in the post-redistricting election ($c+2$) is 20-23 percentage points *lower* than an incumbent in a district where the redistricting party narrowly wins the pre-redistricting election. Similarly, the incumbent's probability of being reelected is approximately 30 percentage points lower if the redistricting party narrowly lost the pre-redistricting election. Both these findings are statistically significant and confirm our hypothesis that congressional incumbents become more vulnerable in "narrow-redistrictor-loss" districts relative to "narrow-redistrictor-win" districts.

The economic significance of the difference in election outcomes can be clearly seen in Figures VIII(a) and VIII(b). An incumbent from a narrow-redistrictor-loss district faces a very tight race for reelection, with an average margin of victory of close to zero percent and an average reelection rate of close to 55%, while an incumbent from a narrow-redistrictor-win district is very secure, with an average vote margin of almost 25% and an average

reelection rate close to 90%. These magnitudes imply that we are comparing truly *vulnerable* incumbents to *entrenched* incumbents.

Next, we check whether the effects on incumbent vulnerability persist past the first post-redistricting election. The first two columns from Panel A of Table V provide estimates for our baseline RDD regerssion using $IncumbMargin$ and $IncumbWin$ from years $c+2$ to $c+8$, and reveal that the impact on $IncumbMargin$ and $IncumbWin$ disappears after the first post-redistricting election. We propose two potential reasons for this lack of persistence. First, regional partisan preferences may change over time as political preferences fluctuate. Second, and more importantly, the incumbent's alignment with the redistricting party may *flip* following the first post-redistricting election. For instance, if an incumbent from the non-redistricting party loses the post-redistricting election, that district reverts to the redistricting party's control and in the following election, the original gerrymandering is in favor of the *new* incumbent.

One of our major empirical challenges is to disentangle the effects of electoral vulnerability from that of tenure. To check whether we are successful in this regard, we estimate our Eq. 1 with incumbent tenure ($IncumbTenure$) as the outcome variable. The estimates reported in the third column from Panel A of Table V show that incumbents from narrow-redistrictor-win districts have slightly *shorter* tenures than incumbents from narrow-redistrictor-loss districts entering into the first post-redistricting election. This difference in incumbent tenure does not persist over to subsequent elections. Nevertheless, this implies a simple comparison of narrow-redistrictor-win versus narrow-redistrictor-loss districts does not allow us to completely disentangle incumbent vulnerability from tenure.

What drives the difference in tenures in the first post-redistricting election? We find that this pattern arises primarily due to the large number of Republican candidates who defeated Democratic incumbents during the 2010 midterm elections.[30] As the majority of state legislatures were controlled by Republicans, this resulted in a large number of close victories by the redistricting (Republican) party in which the incumbent (Democratic) candidate lost their seat in 2010.[31] In the first post-redistricting election in 2012, many narrow-redistrictor-win districts were represented by freshly-elected incumbents who had swept into power as part of the 2010 Republican wave. The disproportionate number of freshman Representatives in narrow-redistrictor-win districts mechanically implies a lower average tenure.

[30] The worst congressional defeat for the Democratic party in 70 years was largely attributed to backlash to President Obama's landmark healthcare reforms. In that election, Republicans gained 65 seats in the House whereas the Democrats gained only 2 seats, leading to shift towards the Republican party of 63 representatives that year.

[31] In 2010, 27 state legislatures were controlled by Republicans whereas only 16 were controlled by Democrats. As the upper and lower state houses may be controlled by different parties, not all state legislatures are controlled by one party.

To preserve our identification strategy in light of this historical coincidence, we limit our sample to "non-wave" districts that did *not* have its incumbent representative defeated in the pre-redistricting election. This allows us to exclude all cases in which the incumbent is a first term representative entering the post-redistricting election. As incumbent defeats are relatively rare, this sample restriction reduces our sample only marginally: the number of unique districts in our sample drops from 333 to 313 and the number of district-cycle-level observations drop from 484 to 427.

By limiting the sample to "non-wave" districts, we also ensure that the incumbent's party affiliation remains the same before and after the pre-redistricting election. Therefore, when calculating the early cycle change in SBA loan outcomes, as described in Section 4, we difference out any time-invariant relationship between party affiliation and SBA loan outcomes. This alleviates concerns, also raised by the Republican wave in 2010, that our results are driven by party switching.

We present the results from re-estimating our RDD specification on the limited sample in Panel B of Table V. The first two columns show results similar to those from the unrestricted sample: incumbents are significantly more vulnerable in narrow-redistrictor-loss districts relative to narrow-redistrictor-win districts, but only for the first election after redistricting. In fact, the magnitude of the difference is even larger than before. This is also visually illustrated in Figures VIII(c) and VIII(d).

Importantly, the third column from Panel B of Table V shows that the differences in incumbent tenure that we previously detected in the first post-redistricting election disappears in the "non-wave" sample. In subsequent elections, however, a significant positive difference in incumbent tenure between narrow-redistrictor-win and narrow-redistrictor-loss districts appears. As previously discussed, this is mechanically driven by the higher reelection rates of incumbents in narrow-redistrictor-win elections in the first post-redistricting election. We observe that this difference in incumbent tenure gradually dissipates and is undetectable after three election cycles.

The results from the restricted "non-wave" sample allow us to re-assert our ability to disentangle electoral vulnerability from electoral tenure, albeit only for the first election cycle following redistricting. Therefore, for the remainder of this paper, we base all our analysis on the "non-wave sample", unless otherwise indicated.[32]

[32] Our results are not sensitive to using non-wave samples only, and we produce qualitatively similar results using the full sample of districts.

6.3 Changes in SBA Lending Volume

We use the discontinuity in electoral vulnerability from our RDD to study the effects on government-subsidized SBA lending. We first estimate Eq. 2 using the full sample of all SBA loan guarantees, pooling 7(a) and 504 loans together, and report the results in Table VI and illustrate them visually in the top row of figures in Figure IX. Panel A of the table shows results using *early cycle* changes in SBA outcomes as the dependent variable, and Panel B shows results using *late cycle* changes in SBA outcomes as the dependent variable.

The first column of Panel (A) shows that *GtdAmt*, the total dollar amount of SBA guarantees, increases in districts with more vulnerable incumbents relative to districts with more entrenched incumbents. The estimate reported in the table indicates a difference of $10.23 million, which is economically significant compared to the sample mean of $29.1 million. This implies that a 28.8 percentage point decrease in the probability of reelection, our estimated difference between a narrow-redistrictor-loss and narrow-redistrictor-win district, results in a 35 percent increase in total SBA loan guarantees for the average district in our sample. Figure IX(a) illustrates the sharp discontinuity visually.

Next, we check whether the relative growth in total SBA loan guarantees is driven by changes in the number of loans (the extensive margin) or by changes in average amount guaranteed per loan (the intensive margin). Column (2) from Panel A shows no evidence that vulnerable incumbents increase the number of loans their district receives relative to entrenched incumbents. In contrast, column (3) shows that the amount guaranteed per loan increases for narrow-redistrictor-loss districts relative to narrow-redistrictor-win districts. These findings are visually illustrated in sub-figures (b) and (c) from Figure IX.

The lack of evidence on the extensive margin suggests that political influence from a vulnerable incumbent representative is ineffective in increasing the rate at which SBA-guaranteed loans are approved in their district. Eligibility rules for SBA loans are strictly defined, as we discussed in Section 3.1, but the criteria for how much to award per loan is generally more discretionary. Pressure to increase SBA lending volume would thus result in larger loans (i.e., on the intensive margin).

A congressional representative can attempt to increase the size of already-approved loans. As described in Section 3.2, it is common practice for congressional representatives to write letters on behalf of their constituents applying for loans to the SBA, and these letters often include requests to increase the size of the loan. An illustrative example of this can be found on the SBA website,[33] in which the SBA approves a request by Representative Robert Aderlholt in August 2012 to increase the amount for a loan granted to one of his constituents.

[33] See `https://www.sba.gov/document/report--congressional-correspondence`.

Political influence may also increase the rates at which larger loans are approved relative to smaller loans. For a vulnerable incumbent, the primary value of obtaining larger SBA loans for one's constituents comes from the immediate creation of jobs. We check whether loans directed towards districts with more vulnerable incumbent representatives support more jobs. Column (4) from Panel A shows that loans directed towards districts with more vulnerable incumbents indeed support more jobs on average, and this is visually illustrated in Figure IX(d). The reported coefficient estimate of 4.287 for *JobsPerLoan* is large relative to the sample average of 12.7. For the average district that awards 127.8 loans per year, this translates to an increase of approximately 548 total jobs supported through politically-motivated lending.

We next examine the impact of electoral competition on *late-cycle* changes in SBA-guaranteed lending growth. As discussed in Section 4.2, more competitive elections lead to shorter tenures, which result in inexperienced incumbents who lack political capital in Congress to obtain federal resources. Recall from Table V that the relative electoral advantage that incumbents from narrow-redistrictor-win districts disappear in the late cycle in the "non-wave" sample, but incumbents from these districts are longer-tenured in the late cycle as a result of being reelected at a higher rate in the first post-redistricting election.

We report the late-cycle results for SBA loans in Panel B of Table VI and illustrate them in the bottom row of sub-figures in Figure IX. The negative, albeit statistically insignificant, estimate in column (1) shows that the early cycle increase in total SBA lending in districts with more vulnerable incumbents disappears in the late cycle. The next two columns, however, show that narrow-redistrictor-loss districts experience a relative decrease in the number of SBA guaranteed loans and a relative increase in the amount awarded per loan in the late cycle. The increase in loan size is significantly larger in the late cycle relative to the early cycle, in both economic magnitude and statistical significance. We also see from column (4) and Figure IX(h) that the increase in *JobsPerLoan* in the early cycle persists into the late cycle.

The idea that longer-tenured incumbents wield greater influence to direct federal resources to their constituents is consistent with the observed decrease in number of loans directed towards narrow-redistrictor-loss districts in the late cycle, but not with the observed increase in guaranteed amounts and jobs supported per loan. The latter finding may potentially be explained by the initial push to accept larger loans that support more jobs becoming institutionalized through bank lending standards. We explore this idea in subsequent analysis by examining changes in interest rates as well as by exploring the role of banks.

6.4 Placebo Tests using Non-Partisan States

To confirm that we are identifying the effects of electoral vulnerability, we perform a set of placebo tests using the sample of states that do not have partisan redistricting, for which we define the party in control of the state legislature as the "psuedo-redistrictor". Given that the pseudo-redistrictor does not actually control redistricting in these states, we should not find any discontinuity in post-redistricting elections between districts where pseudo-redistrictor narrowly won versus narrowly lost the pre-redistricting election.

The insignificant coefficient estimates presented in the first two columns of Table VII show that there is no discontinuity in election outcomes between narrow-redistrictor-win and narrow-redistrictor-loss districts where the pseudo-redistrictor narrowly won versus narrowly lost. These results are visually illustrated by subfigures (a) and (e) in Figure X.

Next, we run a similar set of placebo tests on outcomes related to SBA lending. In particular, we check whether the same discontinuity we observed in *GtdAmt*, *GtdAmtPerLoan*, and *JobsPerLoan* for the sample of partisan redistricting states is also observed in the sample of non-partisan redistrict states. The results presented in columns (3) through (8) of Table VII and illustrated in Figure X show little support for a discontinuity in both the early cycle and in the late cycle. The estimate for *GtdAmt* is marginally statistical significant, but it is in the opposite direction from our main findings. Furthermore, one estimate out of eight significant at the 10% level can easily arise due to pure chance. Overall, the non-results in our placebo tests alleviate concerns that factors other than the electoral vulnerability of incumbent representatives are driving our benchmark results.

6.5 Delinquency and Repayment Rates

We study whether loans directed towards districts with more vulnerable incumbents are riskier. One expects to observe a drop in credit quality for larger loans directed towards vulnerable incumbents for the following reasons. First, politically-motivated interference may distort lending decisions normally based on borrower creditworthiness. Second, if profit-maximizing banks were able to make larger loans without facing a drop in credit quality, then we should expect them to already be making those loans even in the absence of political influence, given that larger loans are more cost-effective to administer on a per-dollar basis.

We investigate changes in credit quality using data on loan-level performance outcomes reported for both the 7(a) and 504 loan programs. Specifically, delinquent loans are reported as "charged off" once they are recognized as uncollectible by the SBA,[34] and as "paid in

[34] A list of cases in which the lender can justify a charge off can be found at `www.sba.gov/sites/default/files/bank_wrapup_report.pdf`.

full" once they are fully repaid by the borrower. These loan outcomes are retroactively coded into the data after the outcome is known, and the status of recently-awarded loans with yet unknown outcomes is coded as "exempt". A significant proportion of loans are listed as exempt in the data,[35] as these include not only ongoing loans, but also loans that qualify for exemptions from public disclosure requirements for a variety of reasons, including the potential for competitive harm.

We estimate Eq. 2 with the proportion of loans charged off (*ChgOff*), paid in full (*PIF*), and exempt (*Exempt*), respectively, as the outcome variable, and present the results in Table VIII. Panel A provides the immediate effects in the early cycle, and Panel B provides the long-term effects in the late cycle. The results are also visually illustrated in Figure XI. The outcome variable is defined as the annual number of SBA loans in a given performance category divided by the total number of SBA loans in a district in the first three columns of each panel.

In Panel A, the negative, albeit not statistically significant, estimate for *ChgOff* in column (1) indicates that the larger SBA loans awarded to districts with more vulnerable incumbents do not default at a higher rate in the early cycle. The significant negative estimate for *PIF* in column (2), however, suggests a decrease in repayment rates for SBA loans in these districts. The positive coefficient, albeit marginally statistically significant, for *Exempt* in column (3) suggests that the drop in repayment rate is at least partially accounted for by an increase in the proportion of loans exempted from reporting repayment outcomes.

We consider two potential explanations for a shift from paid-in-full loans to exempt loans in districts with vulnerable incumbents. The first possibility is a larger proportion of loans in these districts have not yet reached maturity and therefore are exempted from reporting repayment status. When we estimate Eq. 2 with average loan maturity *Maturity* as the outcome variable, however, we see from the insignificant negative estimate in column (4) of Panel A that there is no evidence of this.

The second possibility is that the increase in exempt loans reflects attempts to hide deteriorating performance, driven by the alignment of incentives across lenders, the SBA, and vulnerable incumbent representatives. Specifically, lenders have the incentive to hide losses in order to maintain their status as preferred SBA lenders, the SBA wants to avoid congressional scrutiny that would result from greater defaults, and the local incumbent representative is more concerned with generating short-term stimulus for the economy than with the long-term sustainability of the SBA loan programs.[36] Our empirical results are

[35] Exempt loans make up 21.25 percent of all loans in the data. This proportion is much higher in recent years as recent loans are more likely to be ongoing.

[36] An illustration of this is provided by the $76 million fraud case against SBA lender Business Loan Express in 2007, in which the SBA was accused of attempting to suppress an investigative report over its losses in

consistent with attempts to conceal deteriorating loan performance, though we lack direct evidence for this.

We examine the late cycle changes in SBA loan performance and present the results in Panel B. The estimates for *ChgOff* and *PIF* from columns (1) and (2) show that SBA lending is both charged off and fully repaid at a lower rate in narrow-redistrictor-loss districts relative to narrow-redistrictor-win districts. Column (3) shows a corresponding increase in the rate of loans exempted from reporting. In this case, the shift from both charged-off loans and paid-in-full loans to exempt loans can be explained by longer loan maturities, as seen in the positive and significant estimate for *Maturity* presented in column (4). As a significant portion of the late cycle for the 2010s is temporally placed near the end of our sample, it is not surprising that long-maturing loans in this period are still unresolved.

6.6 Comparison of 7(a) and 504 Loan Programs

Thus far, we have pooled together SBA 7(a) and 504 loans in our analysis. We now examine 7(a) loans and 504 loans separately, and check whether they are differentially affected by local political shocks. We expect 7(a) loans to be more sensitive to political influence than 504 loans for several reasons. First, as the SBA's flagship credit subsidy program, the 7(a) program is significantly larger in total volume than the 504 loan program. Second, as discussed in Section 3.1, the approval process for the 504 program is generally slower, taking up to several months. For congressional incumbents facing two year election cycles, the 7(a) loan program represents a more expedient target. Lastly, it is more difficult to coordinate approvals for 504 loans, since they involve both banks and CDCs, whereas 7(a) loans involve banks only.

We present the analysis of 7(a) loans in Table IX. Panel A shows the early cycle results, which are also visually illustrated in Figure XII, and Panel B shows the late cycle results, which are also visually illustrated Figure XIII. We observe similar results to the pooled analysis with respect to our main lending outcomes: narrow-redistrictor-loss districts receive more loans in terms of total dollars, dollar amount per loan, and jobs supported per loan in both the early cycle and the late cycle, while narrow-redistrictor-win districts receive a greater number of loans in the late cycle. The statistical significance is higher than that for the pooled results, while the economic magnitudes are similar.

We construct additional variables using information available only for 7(a) loans. These include the the total loan amount (*TotAmt*) and the average interest rate across 7(a) loans in each district (*IntRate*).[37] Column (2) and column (5) from Panel A and Panel B both

its 7(a) program. See `archive.nytimes.com/www.nytimes.com/allbusiness/07girard.html`.

[37] We calculate district-level interest rates as the average interest rate across loans, weighted by loan volume.

show that the total loan amount, both in terms of district aggregates and per loan averages, increases for narrow-redistrictor-loss districts. This indicates that political influence by vulnerable incumbents actually affects loan size, rather than merely affecting the proportional size of loan guarantees.

We study interest rates to check whether shocks to credit demand, rather than to credit supply, are driving the increased lending we observe in districts with vulnerable incumbents. Assuming standard supply and demand curves for credit, a positive demand shock should result in higher interest rates while a positive supply shock should result in lower interest rates. The estimates presented in column (7) of Panels A show that interest rates indeed decline in districts with vulnerable incumbents in the early cycle, suggesting an increase in credit supply. The estimate is statistically significant at the 1 percent level, with a relative decrease of 0.393 percentage points economically significant relative to the sample mean of 6.7. Column (7) from Panel B also shows a negative effect on interest rates, but the magnitude is much smaller and the estimate is not statistically significant.

Next, we examine the effects on SBA 504 lending outcomes and present the results in Table X, with Panel A showing early cycle results and Panel B showing late cycle results. The results are also visually illustrated in Figure XIV. We see that, unlike in the pooled sample and the 7(a) sample, there is no detectable effect on lending guarantees, both in the number of loans and in the average size of loans. We do, however, observe positive and significant coefficients in columns (4) and (5) in both Panel A and Panel B, which suggests a persistent shift toward loans that support more jobs in districts with vulnerable incumbents in the early cycle.

Overall, our analysis on 7(a) and 504 loans show that the effects we document in the pooled sample are mainly driven by 7(a) loans. This finding is consistent with the greater degree of political sensitivity of the 7(a) program due to its relative prominence, speed, and flexibility. We find some evidence of a shift in the 504 program towards loans with greater job support in districts with more vulnerable incumbents.

6.7 The Role of Banks

SBA loans are distributed by private sector intermediaries, with 504 loans distributed jointly by CDCs and banks and 7(a) loans distributed solely through banks. Although the objectives of CDCs align better with those of vulnerable incumbents looking to boost the local economy, banks may also be subject to political influences, especially considering that they operate in a heavily-regulated environment and are subject to policy risk. As discussed in Section 3.2, banks have a vested interest in keeping a sitting incumbent in Congress in order to avoid the

political uncertainty that accompanies changes in representation, and, for local banks within the incumbent's district, to avoid being represented by a freshman legislator with limited political capital in Congress.

Some banks possess special authority to process and award SBA loans without submitting to SBA oversight for each loan. To determine the presence of direct political influence through banks, we examine whether our benchmark results hold for loans distributed by banks with delegated authority. To this end, we define "delegated banks" as those that participate under the Preferred Lenders Program (PLP) or the SBA Express program. As described in Section 3, SBA Express status allows lenders to independently process 7(a) loans under \$350,000 while PLP status allows experienced lenders to independently process larger 7(a) loans. We focus on 7(a) loans, since the SBA only guarantees the CDC portion of loans under 504 program, and data on the bank portion of 504 loans is unavailable prior to 2009.

We estimate Eq. 2 using 7(a) lending volumes by delegated and non-delegated banks, respectively, and report the results in Table XI. Visualizations of the RDD plots for delegated banks and non-delegated banks are provided in Figure XV and Figure XVI, respectively. Panel A in the table shows results from the early cycle, and reveals that the relative increase in lending volume in districts with more vulnerable incumbents is more pronounced for lending by delegated banks. The estimates for lending by delegated banks are larger in magnitude and exhibit greater statistical significance. This suggests a direct channel of political influence from vulnerable incumbents to banks.

The fact that our benchmark results are more pronounced for loans from delegated banks can be explained by the greater political sensitivity of delegated banks stemming from their government-designated status. While delegated banks are not subject to SBA oversight on a loan by loan basis, they are periodically assessed by the SBA on the risk and performance of their loan portfolios in order to maintain their special status. Facing the possibility of having their delegated authority revoked, delegated banks have an incentive to placate congressional representatives who possess ultimate oversight over the SBA.

To further explore how congressional electoral politics affect bank incentives, we separately analyze SBA loans made by local banks and non-local banks. We hypothesize that banks have the incentive to increase lending in the region surrounding their own headquarters in order to keep their local congressional representative in office. We define loans to be from a local bank based on whether the borrower is located in the same state as the headquarters of the lending institution.[38]

[38] We choose state as the local geographic region because a) many congressional districts do not contain any headquarters, b) to account for geographic spillovers across district lines, and c) congressional representatives likely have influence throughout their state.

We present the results of estimating Eq. 2 separately for the sample of 7(a) loans made by local and non-local banks in Table XII. Visualizations of the RDD plots for local banks and non-local banks are provided in Figure XVII and Figure XVIII, respectively. Column (1) from Panel A shows that, in the early cycle, districts with vulnerable incumbents receive greater total loan amounts (*TotAmt*) from local banks. The aggregate effect does not seem to come exclusively from larger average loan size, as the next two columns show that the estimates for *NumLoans* and *TotAmtPerLoan* are both positive but under the threshold of statistical significance. A visualization of these results can be seen in the RDD plots for local banks in Figure XVII and for non-local banks in Figure XVIII.

Column (4) from Panel A reveals that non-local banks do not increase total lending to districts with vulnerable incumbents, although column (6) shows that they do increase the average size of loans. This may be a result of the price effect that we document in Section 6.6, in that lower prevailing interest rates induced borrowers to demand larger SBA loans from *all* lenders, including non-local banks. The negative, albeit statistically insignificant, estimate on *NumLoans* in column (6) implies that non-local banks did not loosen their lending standards in districts with more vulnerable incumbents.

The results presented in Panel B show that the increase in lending to narrow-redistrictor-loss districts by local banks persists into the late cycle, with the intensive margin effect on *TotAmtPerLoan* presented in column (3) driving the increase in aggregate *TotAmt* presented in column (1). We also see from column (6) that the intensive margin effect on *TotAmtPerLoan* persists into the late cycle for non-local banks, but column (5) shows that narrow-redistrictor-loss districts experience a relative decrease in the number of loans made by non-local banks in the late cycle.

A comparison of column (2) and column (5) suggests that lending behavior by non-local banks is behind the relative decline in the number of loans directed towards narrow-redistrictor-loss districts in the late cycle that we observe in the aggregate sample of 7(a) loans. One potential explanation for this is that non-local banks, who operate across state lines by definition, are more sensitive to federal legislation and therefore more willing to curry favor with long-tenured incumbent representatives who carry more legislative weight in Congress. As we document that narrow-redistrictor-*win* districts are represented by longer-tenured incumbents in the late cycle, it makes sense for non-local banks to approve loans to these regions at a higher rate.

A second potential explanation for the extensive margin effects we observe for non-local banks in the late cycle is that these banks have internal risk controls that aim to balance their risk exposure across geographic regions. Thus they respond to lower prevailing local interest rates and larger loan sizes that persist from the early cycle by raising lending standards in

narrow-redistrictor-loss districts. The net result is fewer loans and a lower rate of business creation, an implication that we explore in the following section.

6.8 Implications for Local Economic Growth

We investigate whether politically-motivated SBA lending leads to stronger economic growth, following the credit-easing hypothesis, or weaker economic growth, following the misallocation hypothesis. To this end, we examine changes in the number of establishments (*Estabs*), employment (*Emp*), and wages (*Wage*) using data from the QCEW, and present the results in Table XIII. As in our analysis of SBA lending, we study both the early cycle and the late cycle, with a visualization for each cycle provided in Figure XIX and Figure XX, respectively.

The estimates from Panel A show that employment and wages increase in districts with more vulnerable incumbents during the early cycle. Specifically, *Emp* increases by 8,650 jobs and *Wage* increases by \$500 million in narrow-redistrictor-loss districts relative to narrow-redistrictor-win districts. In per capita terms, *EmpPerCap* increases by 0.0122 jobs per person and *WagePerCap* increase by \$648 per person. There does not appear to be a detectable effect on the number of establishments, both in the aggregate and on a per capita basis.

Overall, our findings for the early cycle are consistent with the credit-easing hypothesis described in Section 3.2. The significant results on employment and wages but not for the number of establishments suggest that the economic benefits associated with the vulnerable incumbent representatives appear to be on the intensive margin, rather than the extensive margin. This is consistent with our findings for SBA lending, in that we find vulnerable incumbents direct larger loans but not a higher number of loans to their districts.

We check whether the economic growth in districts with vulnerable incumbents persist into the late cycle. The estimates from Table XIII(B) show that the gains in employment and wages do not persist. Column (1) and (4) of Panel B reveal that the number of establishment actually *declines* for narrow-redistrictor-loss districts in the late cycle. This is also consistent with extensive margin results from examining SBA lending, in that the number of loans declines in narrow-redistrictor-loss districts in the late cycle.

Overall, the evidence offers some support for the *misallocation hypothesis* in the long run. Under this interpretation, political pressure from vulnerable short-sighted politicians induces lenders to give out larger loans, resulting in short term gains for workers. In the long run, such actions prove to be distortionary and result in non-local banks cutting their supply of SBA lending at the extensive margin, which reduces an important source of financing for new businesses.

Before accepting this interpretation, however, we must recall that, in the late-cycle period, the effects of greater early-cycle electoral competition become conflated with the effects of lower late-cycle realized tenure, as discussed in the previous subsection. Therefore an alternative explanation for our long-run results is that entrenched incumbents gain more influence to acquire resources for their constituents which leads to stronger growth. Unfortunately, our setting does not allow us to clearly disentangle these interpretations.

Lastly, we note that the magnitudes of the short-run effects on local economic growth in the early-cycle are large relative to the effects on small business loans. For instance, a comparison of the relative change in job growth (8,650) and change in total SBA guarantees ($10.23 million) between narrow-redistrictor-loss districts and narrow-redistrictor-win districts suggests that a new job can be "generated" with only $845.55 worth of SBA guarantees.

The magnitudes of our estimates suggest that SBA lending cannot solely account for the short-term increase we observe in employment and wages. As noted in Section 3.2, our identification strategy cannot rule out the possibility that electoral competition affects local economic outcomes through channels other than SBA lending. In the following section, we investigate alternative channels through which politicians can boost short term economic performance.

6.9 Alternative Channels for Political Influence

We explore alternative ways for an incumbent representative with short-term reelection concerns to give the local economy a boost. Since we find evidence of banks responding to the reelection incentives of vulnerable incumbent representatives in our SBA analysis, we check whether overall small business lending (i.e. not just SBA-guaranteed lending) responds to shocks to incumbent vulnerability. To this end, we obtain small business lending data provided by the Federal Financial Institutions Examination Council (FFIEC) through the Community Reinvestment Act (CRA), and construct annual measures of district-level small business lending.[39]

We estimate Eq. 2 using CRA-reported small business lending outcomes and present the results in Panel A of Table XIV. The first three columns show no evidence of incumbent vulnerability affecting CRA-reported small-business lending in the early cycle. The last three columns show some evidence that the number of small business loans declines in narrow-

[39] Under the CRA, all institutions regulated under the Office of the Comptroller of the Currency, Federal Reserve System, Federal Deposit Insurance Corporation, and the Office of Thrift Supervision that meet the asset size threshold are subject to data collection and reporting requirements. The CRA reports lending at the county level, which we map to districts using the Missouri Census Data Center geographic correspondence engine.

redistrictor-loss districts in the late cycle, which matches our findings on SBA lending. As discussed in Section 6.7, this may be due to banks lowering lending standards to curry favor with longer-tenured representatives in narrow-redistrictor-win districts, or due to politically-motivated SBA lending in the early cycle distorting overall credit supply in the long run.

Overall, the evidence suggests that the influence of vulnerable incumbent representatives is focused on SBA lending rather than small business lending more broadly.[40] The greater political sensitivity of SBA lending may be explained by the fact that SBA lending is directly subsidized by the federal government, and hence exhibits a tighter link between bankers and politicians. Indeed, the SBA has faced criticism in the past that it serves as a form of corporate welfare for the banking industry (de Rugy and DeHaven, 2011).

Next, we investigate whether vulnerable incumbent representatives turn to forms of federal financial assistance *other* than SBA loan guarantees to boost their reelection prospects. We examine four broad categories of federal financial assistance as outlined by USASpending.gov: a) contracts, which involve the U.S. federal government acquiring goods or services from a non-federal entity, b) grants, which go toward funding ideas and projects that benefit the general public and stimulate the economy, c) loans, which involve the U.S. government providing financial capital to the private sector, and d) direct payments, a form of non-reimbursable transfer of cash from the federal government to an individual, private firm, or private institution. Note that SBA loan guarantees fall under the category of loans under this classification.

We obtain data on federal financial assistance from USASpending.gov, a government website that reports financial assistance awards of more than $25,000 starting in 2008.[41] For grants, the reported amounts represent obligations, which are binding agreements by the federal government that result in future outlays. For loans, the reported amounts represent subsidy costs, the present value of future cash flows on credit extended in the current budget year. Since data is available starting only in 2008, we examine only the 2010 redistricting cycle.

We report the analysis on federal financial assistance in Panel B of Table XIV. The results provide no evidence for districts with more vulnerable incumbents receiving a greater amount of contracts, grants, or direct payments. There is suggestive evidence for an increase in *Loans*, a category which includes SBA guarantees, in both the early cycle, as seen in column (3), and late cycle, as seen in column (7). While this is consistent with the intensive

[40] We note that one cannot make a perfect comparison between SBA loans and CRA-reported loans, as banks only have to report loans under $1 million while SBA loans are up to $5 million.

[41] Specifically, we use the "spending_by_geography" endpoint on USAspending's application programming interface (API). For more information on the USAspending API, please refer to their website, `https://api.usaspending.gov`.

margin results we find in our SBA analysis, the estimate is not significant in column (3) and only marginally significant in column (7). A potential explanation for the weakness of this result is that forms of government credit subsidy other than SBA loan guarantees are less politically expedient, as in the case of direct loans that require the government to provide the upfront financing, or have more limited reach, as in the case of agricultural subsidies that affect only certain industries and geographic areas.

Ultimately, we fail to find evidence that vulnerable incumbent representatives look to alternative forms of federal financial assistance to win financial resources for their constituents in a bid for reelection. The large magnitudes of our measured effects on local employment and wages, however, suggest that SBA lending is unlikely to be the sole channel through which vulnerable incumbents stimulate the local economy. Nevertheless, given the political popularity of small businesses and the political expediency of SBA lending, we have shown SBA lending to be an important channel subject to political influence.

7 Conclusions

In this paper, we study how electoral competition incentivizes congressional incumbents to bring subsidized small business financing to their districts. Small business lending is a particularly attractive target for politicians due to the popularity of small businesses with voters, the expedient manner in which loans can be distributed through private-sector intermediaries, and the potential for immediately visible job creation. We focus on the flagship lending programs administered by the U.S. Small Business Administration.

We use a novel empirical approach to solve the difficult challenge of identifying electoral vulnerability by exploiting the politically-motivated redistricting of U.S. House of Representatives district boundaries. We conjecture and empirically verify that the party in charge of redistricting allocates votes toward closely-contested districts, which creates a sharp discontinuity in incumbency advantage in districts where the redistricting party narrowly wins versus narrowly loses the pre-redistricting election.

Using our RDD setup, we find that SBA lending volume increases in more competitive districts relative to less competitive districts, mainly driven by changes in average loan size. Our findings also indicate an increase in credit risk accompanying the increase in loan size, which suggests a degree of political interference in the credit evaluation process. Lastly, we find evidence that banks react to the local congressional politics, as our results are stronger for loans from banks authorized to independently approve SBA loans, and for loans from local banks headquartered in the same state as its borrowers.

Our main findings are limited to short-run outcomes within a single election cycle, in

which we are able to empirically disentangle expected electoral competitiveness from realized tenure. In the long run, we find that more vulnerable incumbents are replaced by inexperienced newcomers at a higher rate, while the short run effects of gerrymandering on incumbency advantage fades. Indeed, our findings on long-run outcomes are mixed, as the evidence suggests that the short-run increases in loan size persists into later election cycles, but that the number of SBA loans increases in districts with longer-tenured incumbents.

In examining the ultimate implications for local economies, we find districts with more vulnerable incumbents experience relative increases in employment and wages, consistent with the idea that job creation is limited by credit constraints facing small businesses. In the long run, however, we find that the short-run effects on employment and wages fade, which suggests that subsidizing small business finance is not effective in generating lasting *net* job growth, possibly due to low rates of business survival. In fact, we find evidence of long-term decreases in number of establishments, which is consistent with political forces distorting credit allocation in the long run.

We investigate alternative channels through which vulnerable incumbent representatives can direct economic resources to their constituents. We fail to find evidence that vulnerable incumbents increase the flow of *overall* small business credit and broad categories of federal financial assistance to their district. We remain open, however, to the existence of additional channels, especially considering that the magnitudes of the increases we detect for employment and wage increases are large relative to the magnitudes of the increases we detect for SBA lending. We leave the identification of these channels to future research.

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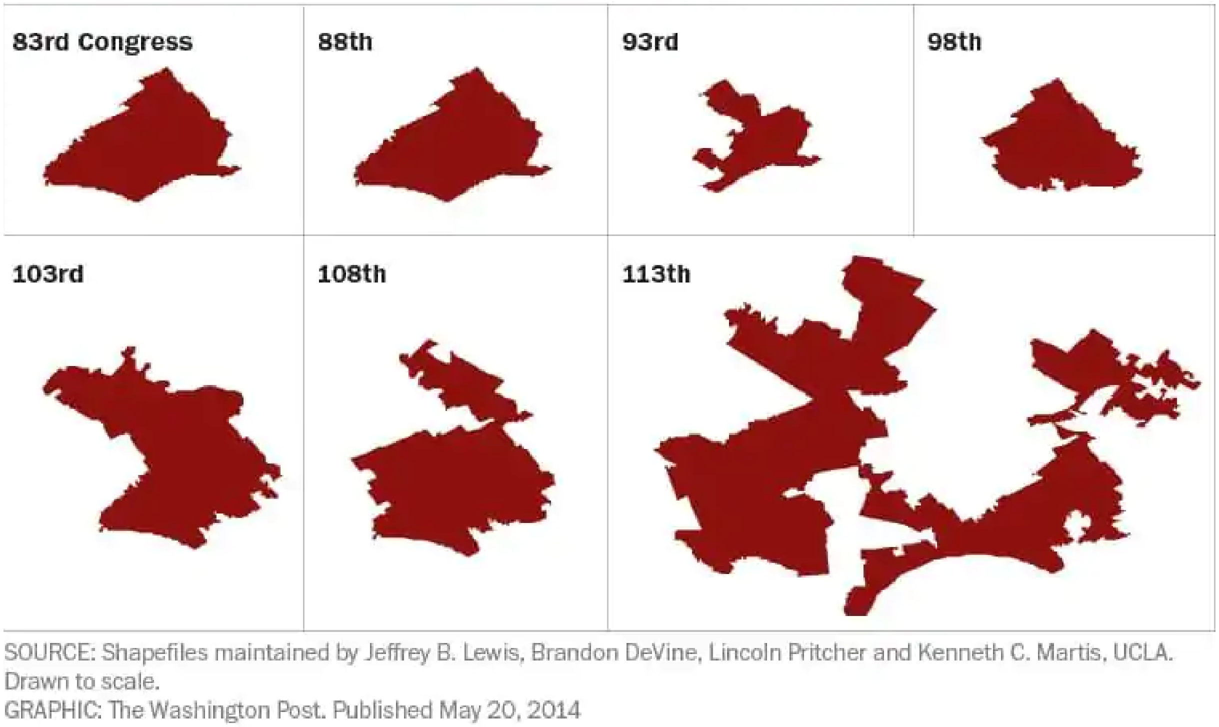
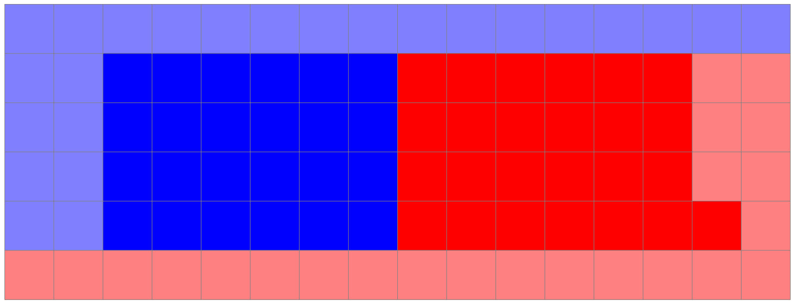
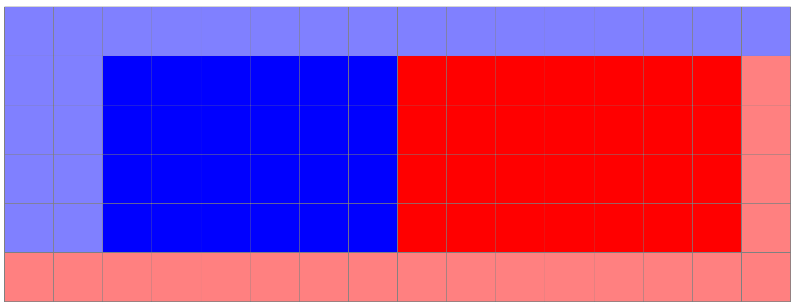


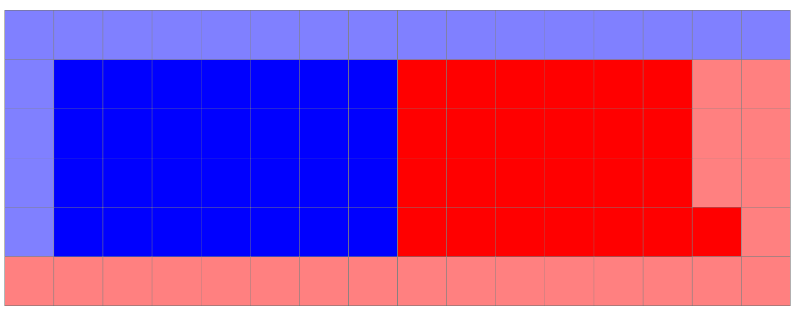
Figure I: Evolution of Pennsylvania's 7th District. This figure shows the evolution of Pennsylvania's 7th congressional district through the 83rd Congress (1953 - 1954), 88th Congress (1963 - 1964), 93rd Congress (1973 - 1974), 98th Congress (1983 - 1984), 103rd Congress (1993 - 1994), 108th Congress (2003 - 2004), and 113th Congress (2013 - 2014). The image was obtained from a 2014 Washington Post article (available online at `https://www.washingtonpost.com/news/wonk/wp/2014/05/21/what-60-years-of-political-gerrymandering-looks-like/`.)



(a) Pre-Redistricting Map



(b) Post-Redistricting Map under Red Gerrymander



(c) Post-Redistricting Map under Blue Gerrymander

Figure II: Illustration of Gerrymandering Closely-Contested Districts. These figures represent an abstract illustration of a gerrymandering strategy targeted at a closely-contested district, where red and blue represent votes for opposing political parties, bold cells delineate boundaries for a single district, and faded cells delineate surrounding districts. Subfigure (a) represents the pre-redistricting district map, subfigure (b) represents the post-redistricting district map under gerrymandering by the red party, and subfigure (c) represents the post-redistricting district map under gerrymandering by the blue party.

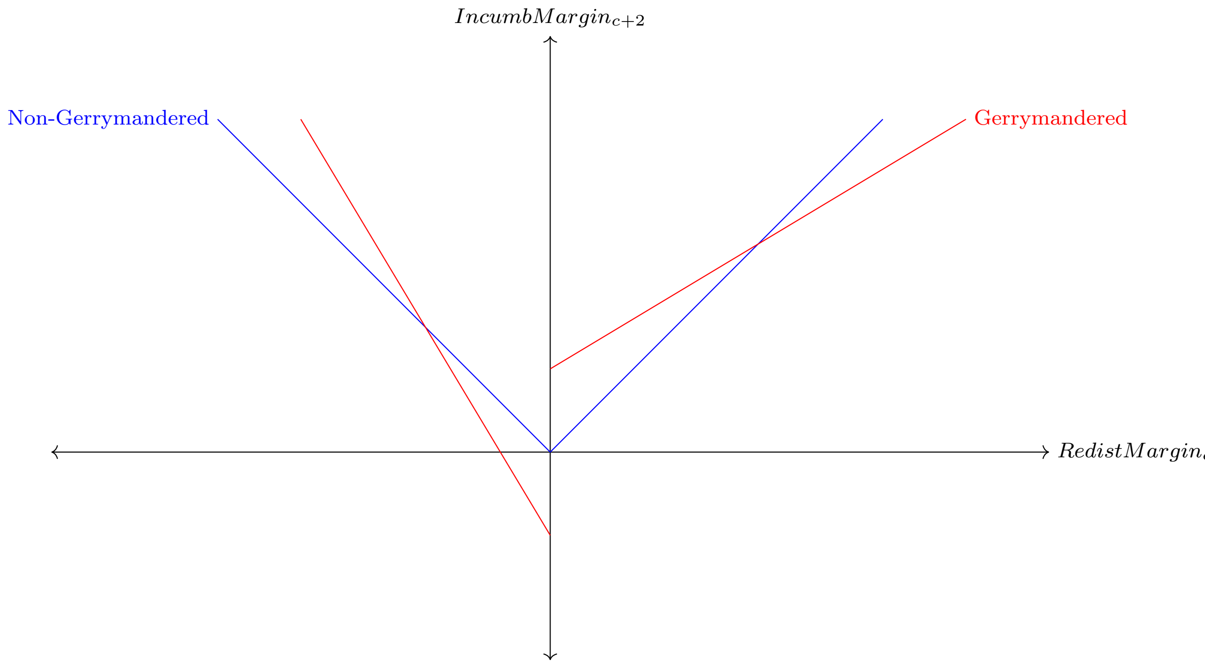


Figure III: Predicted Incumbent Vote Margins vs. Redistricter Votes. This figure represents the predicted relationship between the margin of victory for the redistricting party in the pre-redistricting election, represented on the horizontal axis, and the margin of victory for the incumbent candidate in the post-redistricting election, represented on the vertical axis.

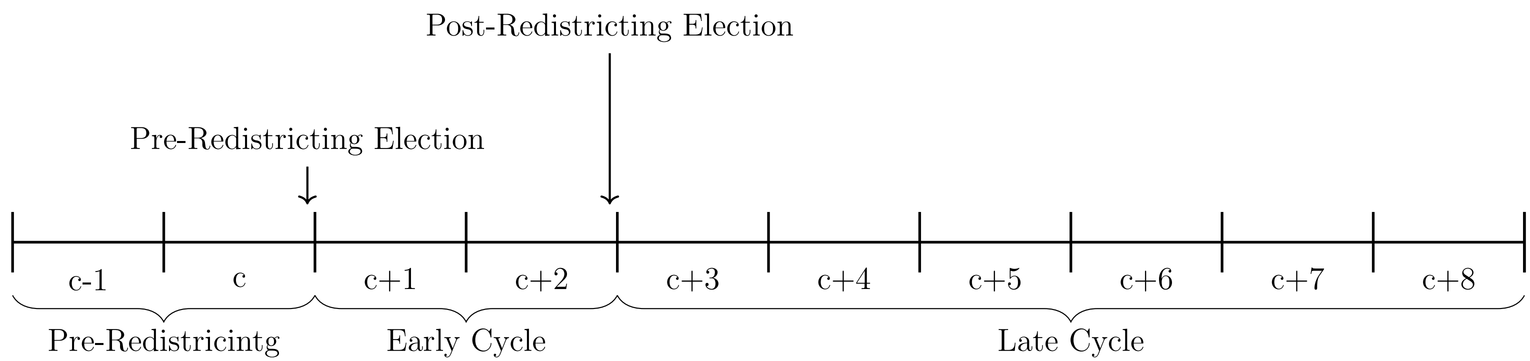
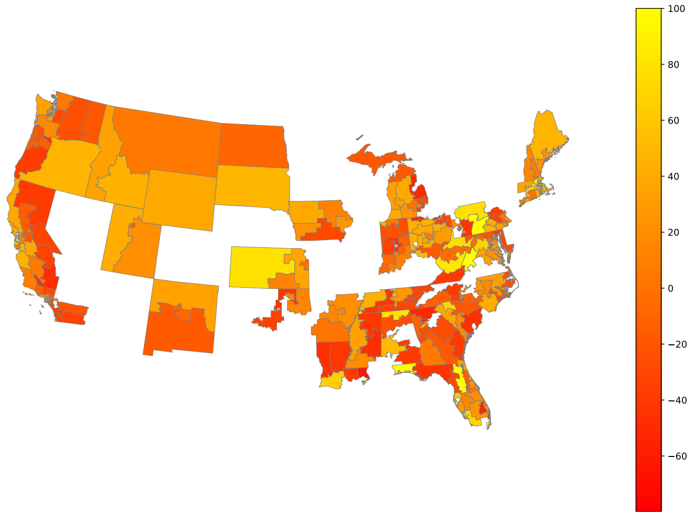
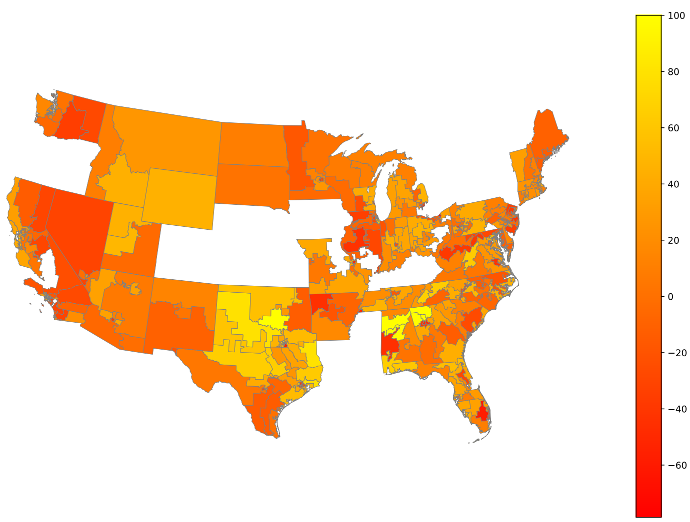


Figure IV: Gerrymandering Timeline. This figure represents the timeline of redistricting, in which the pre-redistricting election occurs at the end of year c (a year ending in '0') and the post-redistricting election occurs at the end of $c + 2$ (a year ending in '2'). Elections occur in November on every even-numbered year.

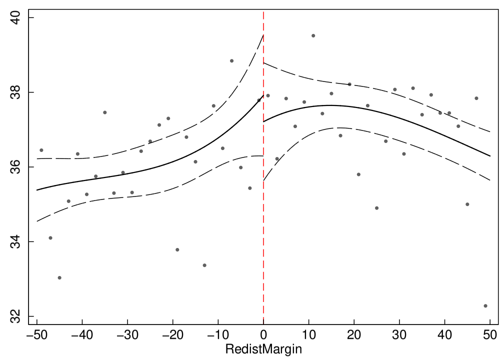


(a) 2000 elections

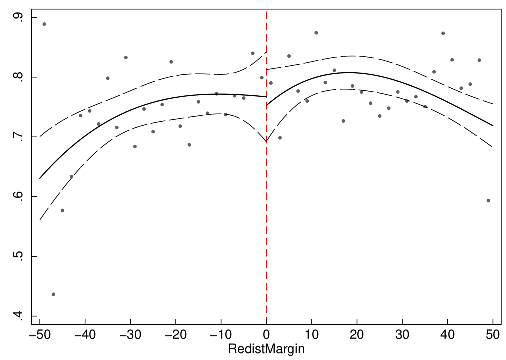


(b) 2010 elections

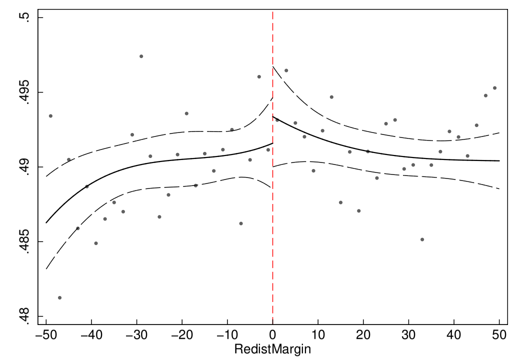
Figure V: Redistrictor Electoral Margins 2000 and 2010 US House of Representatives Elections. These figures present the electoral margins for the redistricting party in the 2000 and 2010 US House of Representatives elections. The more yellow the district, the larger the margin of victory for the redistricting party. The more red the district, the larger the margin of victory for the non-redistricting party. In the middle between yellow and red, orange represents closely-contested elections. Districts from states with non-partisan gerrymandering or states not controlled by a unified state legislature are left blank.



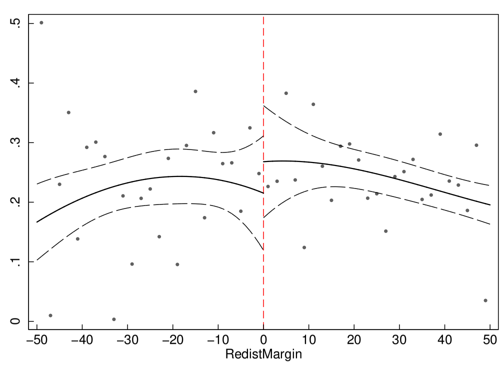
(a) MedAge



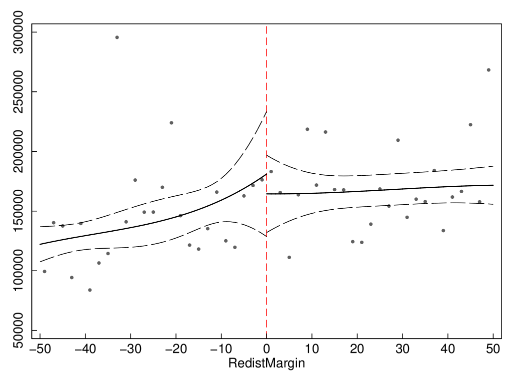
(b) PctWhite



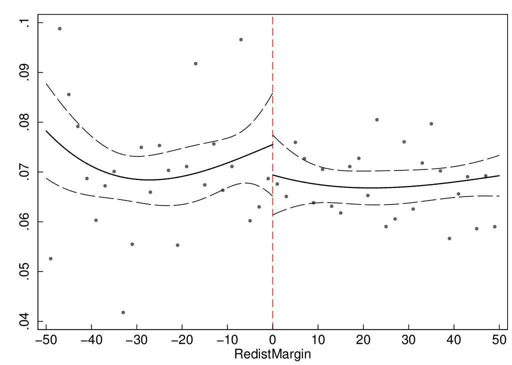
(c) PctMale



(d) PctRural

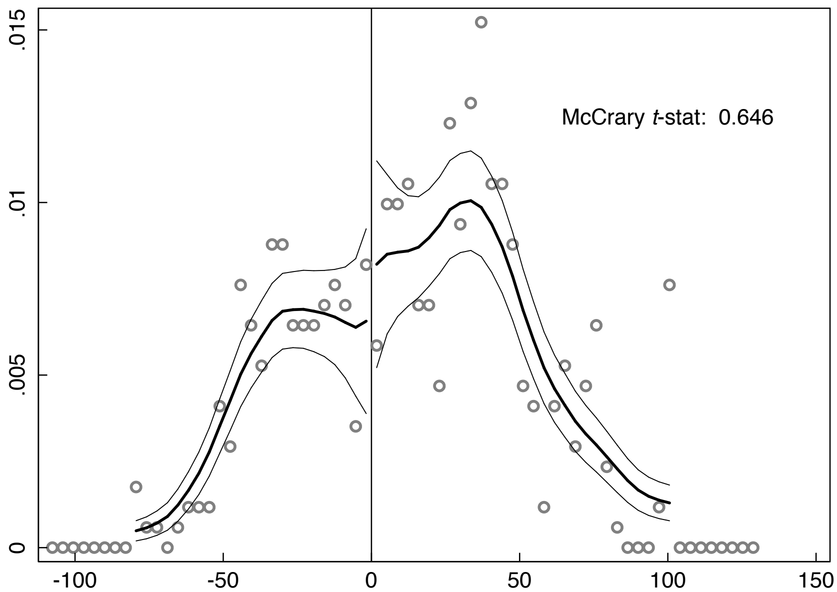


(e) HouseVal

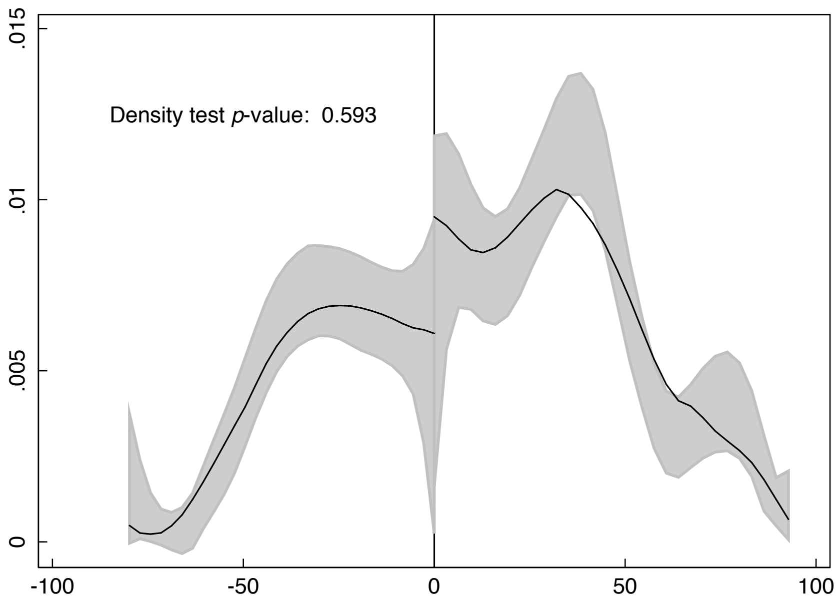


(f) UnempRate

Figure VI: Narrow Redistrictor Losses and Census Covariates. These figures illustrate the results of estimating Equation 1 on covariates measured in the pre-redistricting election year. The solid lines represent the mean estimated outcome for every level of redistrictor win margin in the pre-redistricting election. The dashed lines represent the 95% confidence intervals for the estimated outcome. The covariates are: median age, percent of population that is non-Hispanic white, percent of population that is male, percent of population that is rural, median house value, and unemployment rate. These estimates are based on regressions with standard error clustering at the district level.

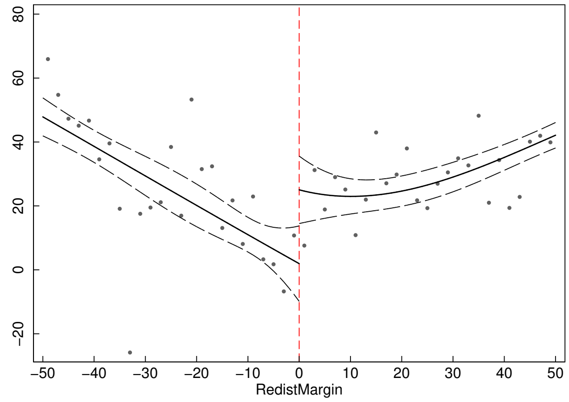


(a) Distribution Density Smoothness Test (McCrary, 2008)

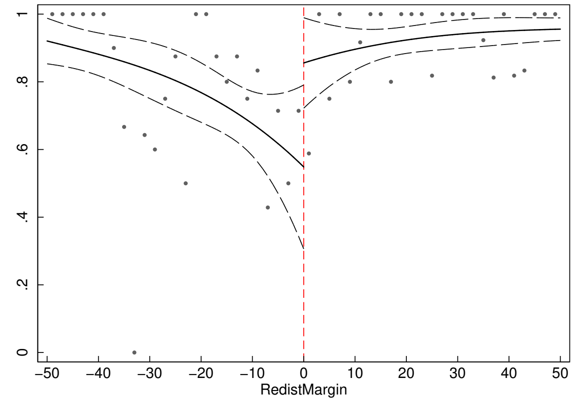


(b) Distribution Density Smoothness Test (Cattaneo et al., 2016)

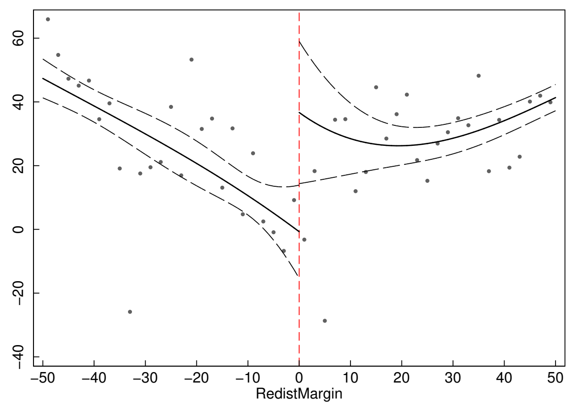
Figure VII: Distribution Density of Pre-Redistricting Election Outcomes. These figures illustrate two tests of density smoothness for the redistricting party win margins in all partisan redistricting states in 2000 and 2010. The tests report smoothness around the zero cutoff point employed in the RDD analyses. Figure (a) visually presents the density plot and test statistic for smoothness based on McCrary (2008). Figure (b) presents the density plot and p-value of a test for smoothness based on Cattaneo et al. (2016).



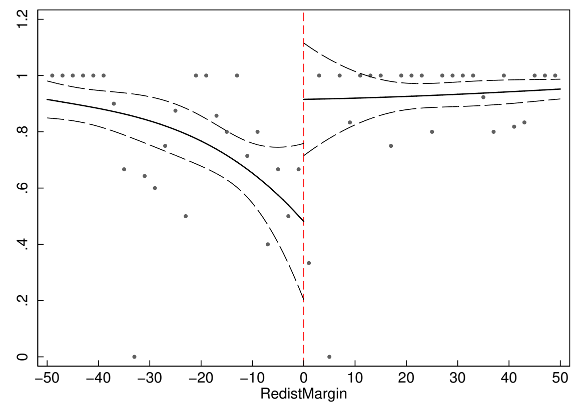
(a) $RedistMargin$ vs. $IncumbMargin$ (all districts)



(b) $RedistMargin$ vs. $IncumbWin$ (all districts)

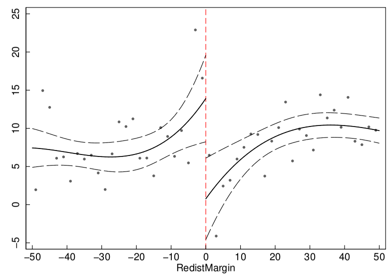


(c) $RedistMargin$ vs. $IncumbMargin$ (non-wave districts)

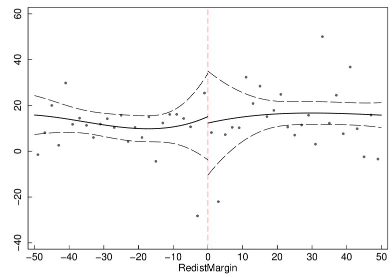


(d) $RedistMargin$ vs. $IncumbWin$ (non-wave districts)

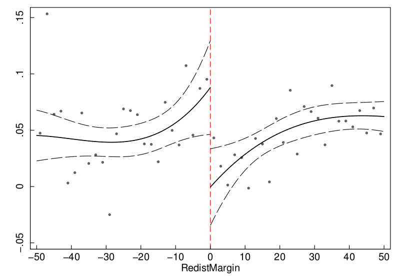
Figure VIII: Redistrictor Electoral Margin and Post-Redistricting Election Outcomes. These figures illustrate the results from estimating Equation 1 on incumbent win margins (figures (a) and (c)) and incumbent win dummy variables (figures (b) and (d)). The first row of figures (figures (a) and (b)) presents results for all districts in states with partisan redistricting whereas the second row (figures (c) and (d)) presents results for only the districts in partisan redistricting states where the incumbent party won the pre-redistricting election. The solid lines represent the mean estimated change in election outcomes for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. These estimates are based on regressions with standard error clustering at the district level.



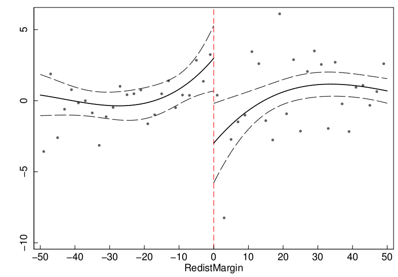
(a) GtdAmt (early cycle)



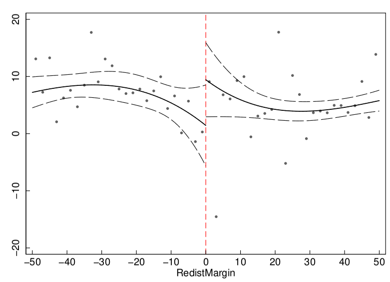
(b) NumLoans (early cycle)



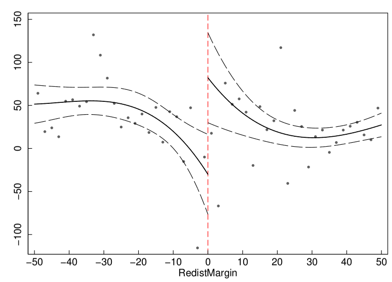
(c) GtdAmtPerLoan (early cycle)



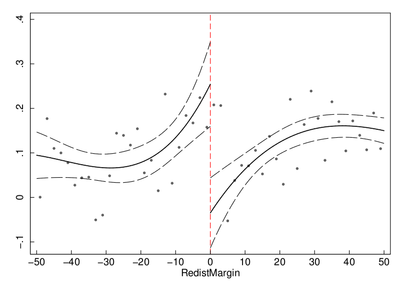
(d) JobsPerLoan (early cycle)



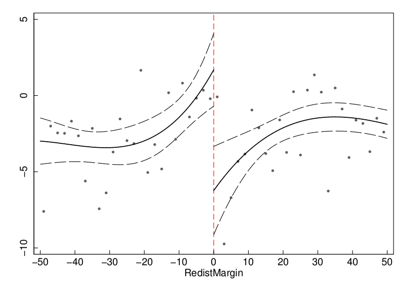
(e) GtdAmt (late cycle)



(f) NumLoans (late cycle)

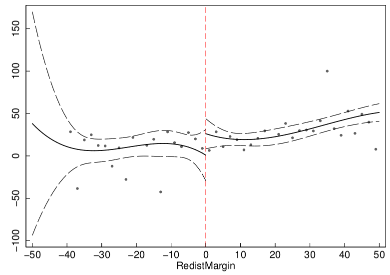


(g) GtdAmtPerLoan (late cycle)

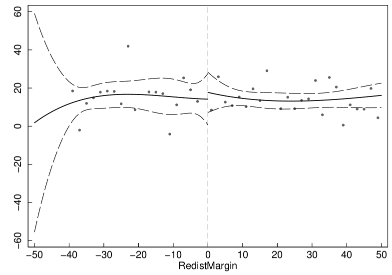


(h) JobsPerLoan (late cycle)

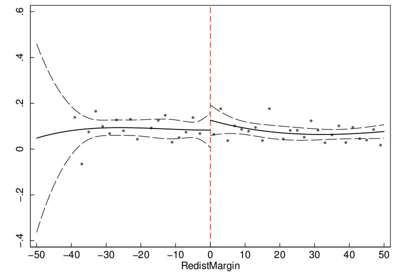
Figure IX: Redistrictor Electoral Margin and Changes in SBA-Guaranteed Lending. These figures illustrate the findings reported in Table VI. The solid lines in each subfigure present the mean estimated change in each outcome variable for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: SBA guarantee amounts (in millions of USD), number of loans issued, guarantee amount per loan (in millions of USD), and jobs supported per loan. The first row of figures ((a) through (d)) present changes in these variables measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). The second row of figures ((e) through (h)) present changes in these variables measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). All figures illustrate findings for districts in partisan redistricting states where the redistricting party won the pre-districting election.



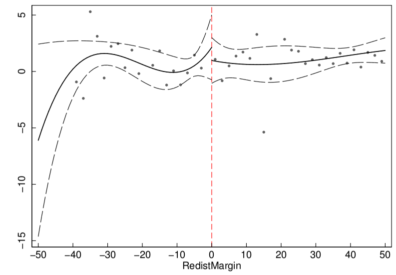
(a) IncumbMargin $(c+2)$



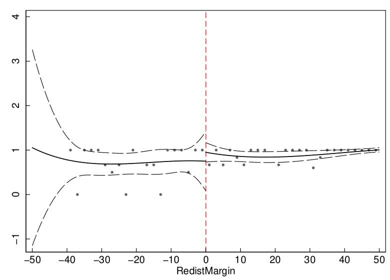
(b) GtdAmt (early cycle)



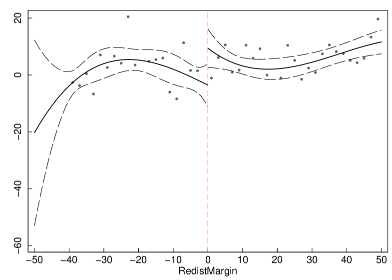
(c) GtdAmtPerLoan (early cycle)



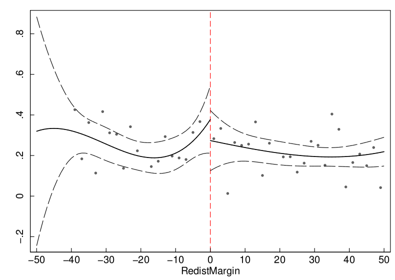
(d) JobsPerLoan (early cycle)



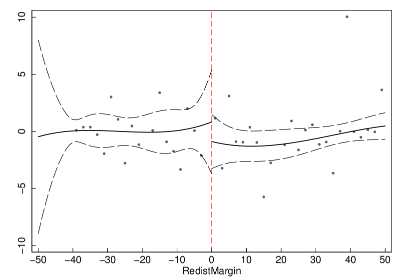
(e) IncumbWin $(c+2)$



(f) GtdAmt (late cycle)

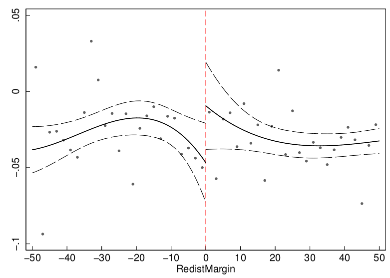


(g) GtdAmtPerLoan (late cycle)

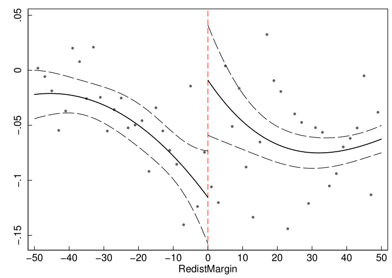


(h) JobsPerLoan (late cycle)

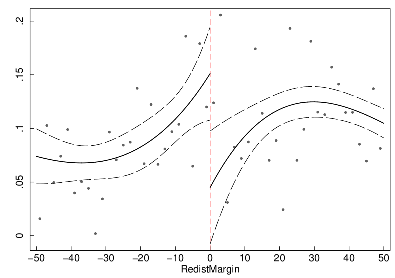
Figure X: Placebo Test on Sample of Non-Partisan Redistricting States. These figures illustrate the findings reported in Table VII. The solid lines in each subfigure present the mean estimated change in each outcome variable for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. There are two sets of outcome variables presented. For elections, the outcomes are: vote margins for incumbent wins in the first post-redistricting election $(c+2)$ (figure (a)) and likelihood of incumbent win in the first post-redistricting election (figure (e). For SBA lending, the outcomes are change in: SBA guarantee amounts (in millions of USD) (figures (b) and (f)), guaranteed amount per loan (in millions of USD) (figures (c) and (g)), and jobs supported per loan (figures (d) and (h)). The first row of SBA lending figures ((b) through (d)) present changes in these variables measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). The second row of SBA lending figures ((f) through (h)) present changes in these variables measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). All figures illustrate findings for districts in non-partisan redistricting states.



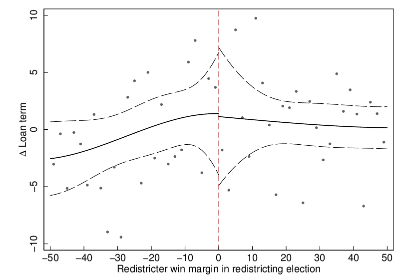
(a) ChgOff (early cycle)



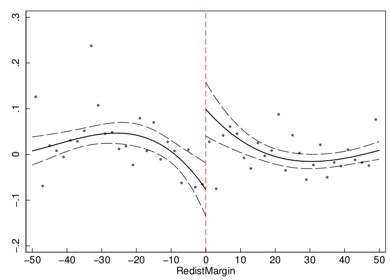
(b) PIF (early cycle)



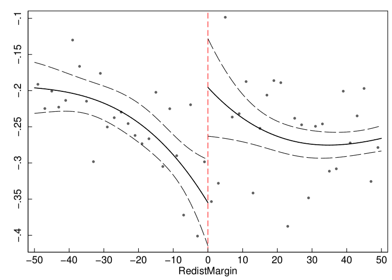
(c) Exempt (early cycle)



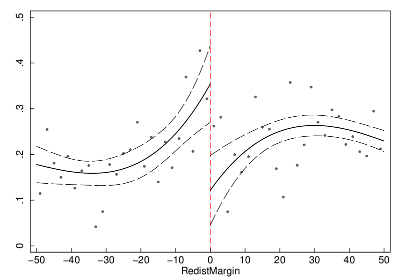
(d) Maturity (early cycle)



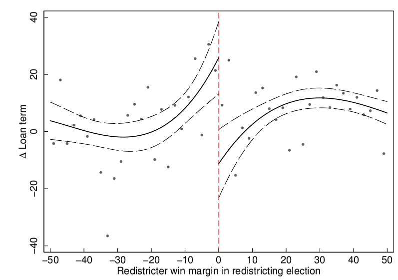
(e) ChgOff (early cycle)



(f) PIF (late cycle)

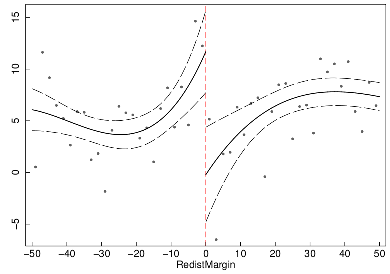


(g) Exempt (late cycle)

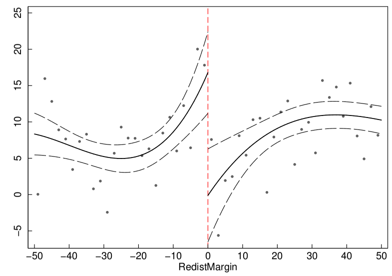


(h) Maturity (late cycle)

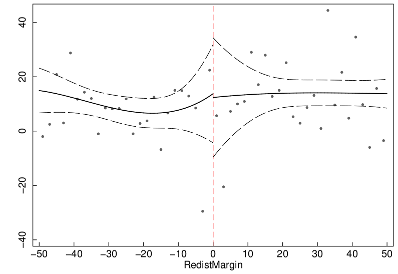
Figure XI: Redistrictor Electoral Margin and SBA Loan Performance. These figures illustrate the findings reported in Table VIII. The solid lines in each subfigure present the mean estimated change in each outcome variable for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: percent of total principal and loans charged-off (figures (a) and (e)), percent of total principal and loans paid in full (figures (b) and (f)), percent of total principal and loans exempt from reporting (figures (c) and (g)), and average maturity of loans at issuance (figures (d) and (h)). The first row of figures ((a) through (d)) present changes in these variables measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). The second row of figures ((e) through (h)) present changes in these variables measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). All figures present findings for districts in partisan redistricting states where the redistricting party won the pre-districting election.



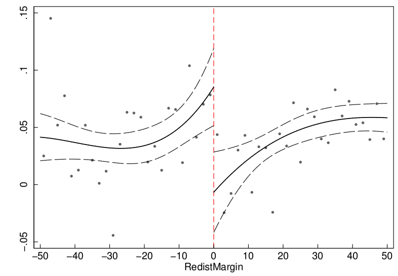
(a) GtdAmt



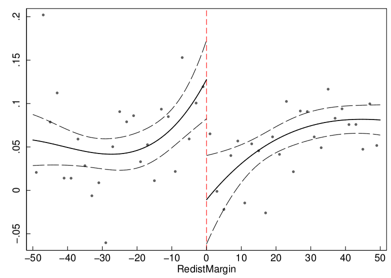
(b) TotAmt



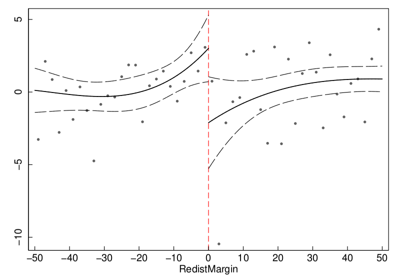
(c) NumLoans



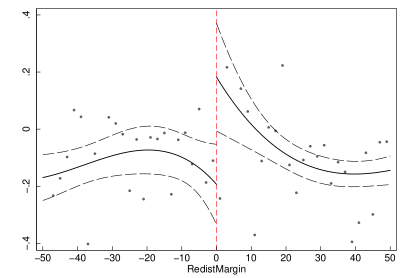
(d) GtdAmtPerLoan



(e) TotAmtPerLoan

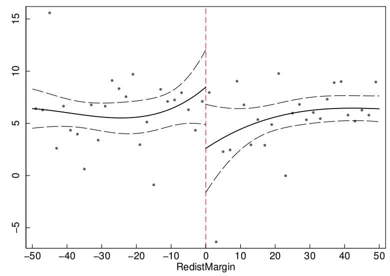


(f) JobsPerLoan

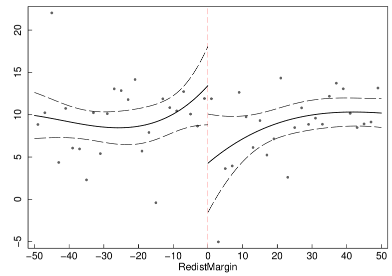


(g) IntRate

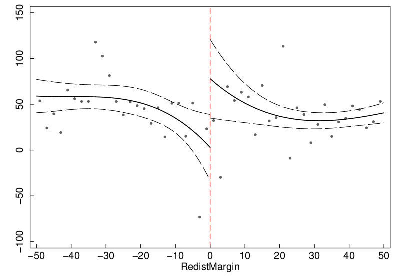
Figure XII: Redistrictor Electoral Margin and SBA 7(a) Lending (Early Cycle). These figures illustrate the findings reported in Panel A of Table IX. The solid lines in each subfigure present the mean estimated change in each outcome variable for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: total SBA-guaranteed loan amounts (in millions of USD), SBA guarantee amounts (in millions of USD), number of loans issued, guaranteed amount per loan (in millions of USD), principal amount per loan (in millions of USD), jobs supported per loan, and dollar-weighted average interest rates. Changes in these variables are measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle) for districts in partisan redistricting states where the redistricting party won the pre-districting election.



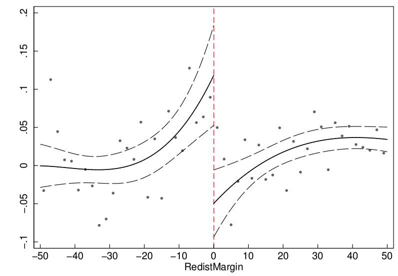
(a) GtdAmt



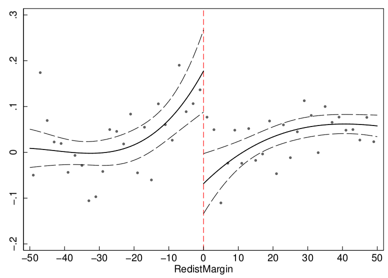
(b) TotAmt



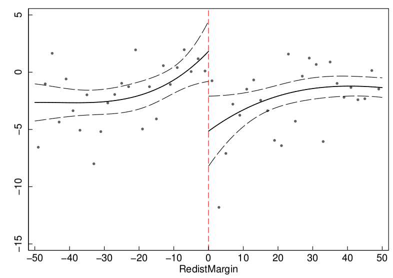
(c) NumLoans



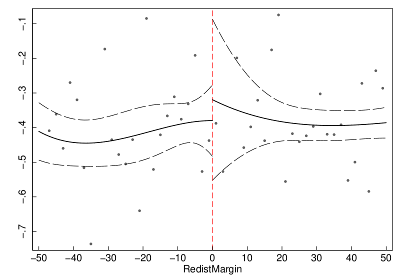
(d) GtdAmtPerLoan



(e) TotAmtPerLoan

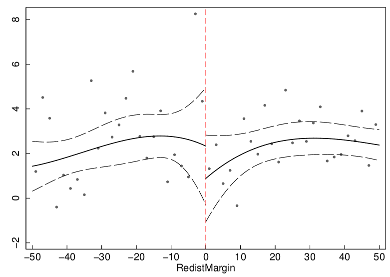


(f) JobsPerLoan

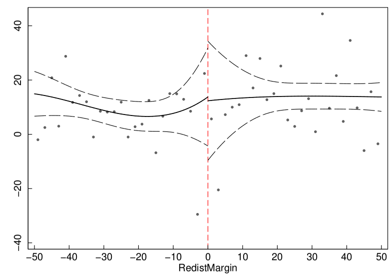


(g) IntRate

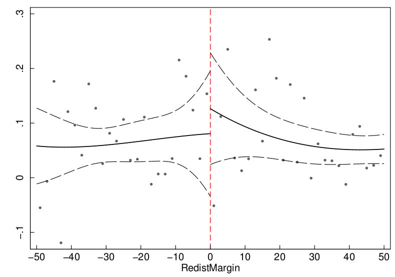
Figure XIII: Redistrictor Electoral Margin and SBA 7(a) Lending (Late Cycle). These figures illustrate the findings reported in Panel B of Table IX. The solid lines in each subfigure present the mean estimated change in each outcome variable for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: total SBA-guaranteed loan amounts (in millions of USD), SBA guarantee amounts (in millions of USD), number of loans issued, guaranteed amount per loan (in millions of USD), principal amount per loan (in millions of USD), jobs supported per loan, and dollar-weighted average interest rates. Changes in these variables are measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle) for districts in partisan redistricting states where the redistricting party won the pre-districting election.



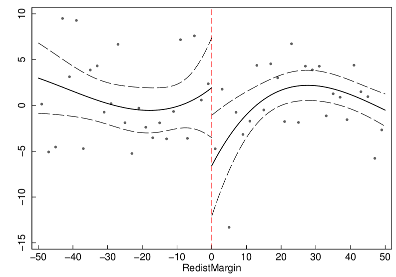
(a) GtdAmt (early cycle)



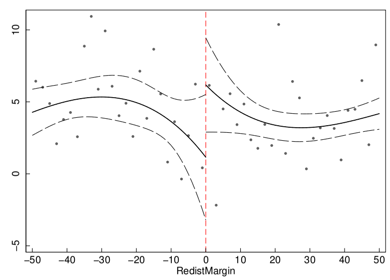
(b) NumLoans (early cycle)



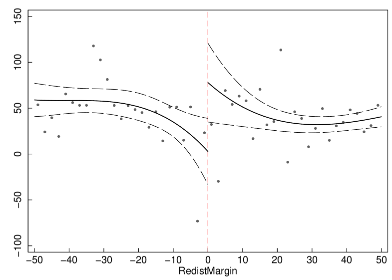
(c) GtdAmtPerLoan (early cycle)



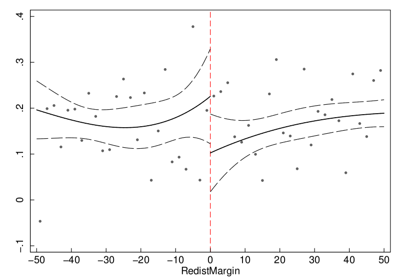
(d) JobsPerLoan (early cycle)



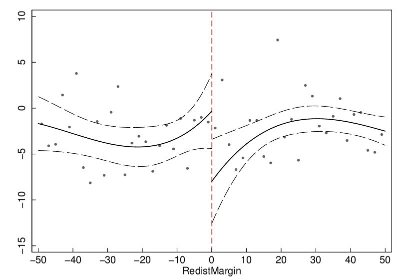
(e) GtdAmt (late cycle)



(f) NumLoans (late cycle)

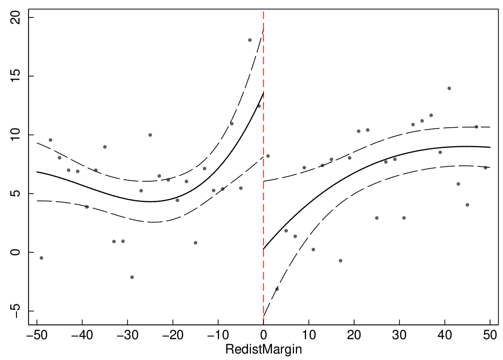


(g) GtdAmtPerLoan (late cycle)

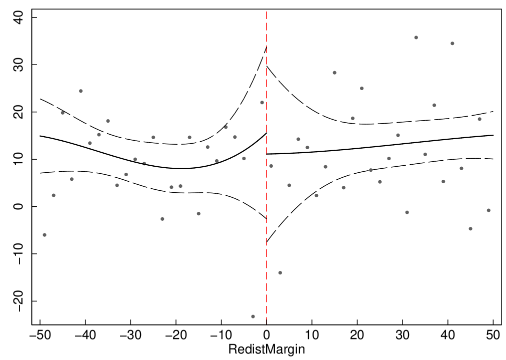


(h) JobsPerLoan (late cycle)

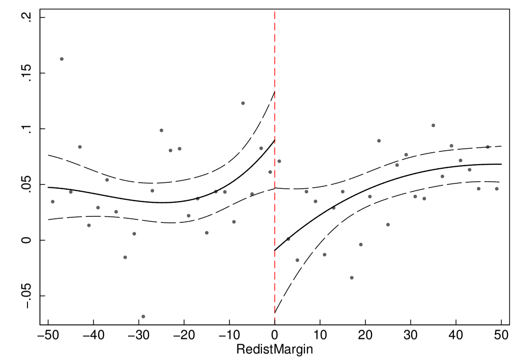
Figure XIV: Redistrictor Electoral Margin and SBA 504 Lending. These figures illustrate the findings reported in Table X. The solid lines in each subfigure present the mean estimated change in each outcome variable for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: SBA guarantee amounts (in millions of USD), number of loans issued, guaranteed amount per loan (in millions of USD), and jobs supported per loan. The first row of figures ((a) through (d)) present changes in these variables measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). The second row of figures ((e) through (h)) present changes in these variables measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). All figures present findings for districts in partisan redistricting states where the redistricting party won the pre-districting election.



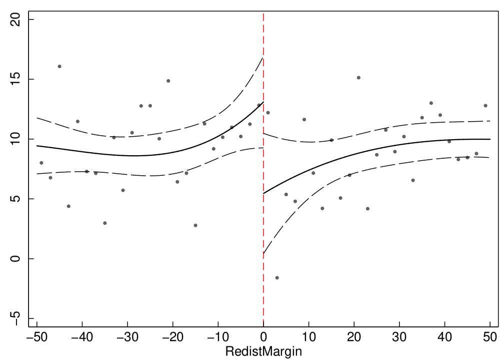
(a) TotAmt (early cycle)



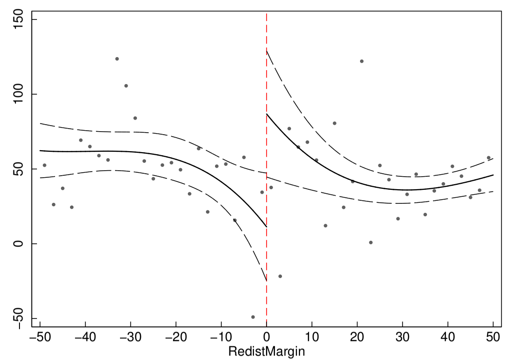
(b) NumLoans (early cycle)



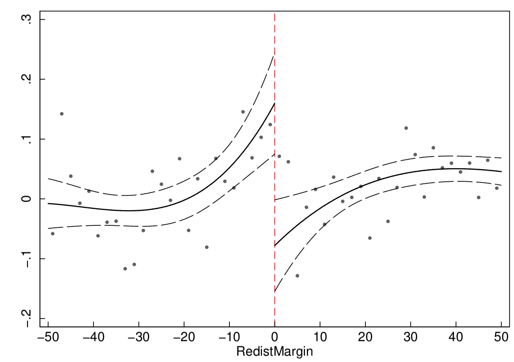
(c) TotAmtPerLoan (early cycle)



(d) TotAmt (late cycle)

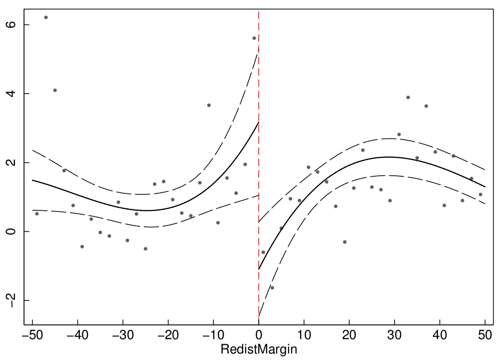


(e) NumLoans (late cycle)

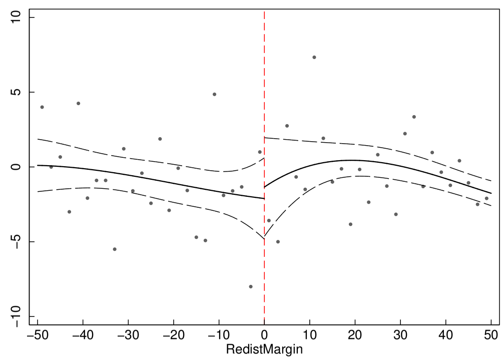


(f) TotAmtPerLoan (late cycle)

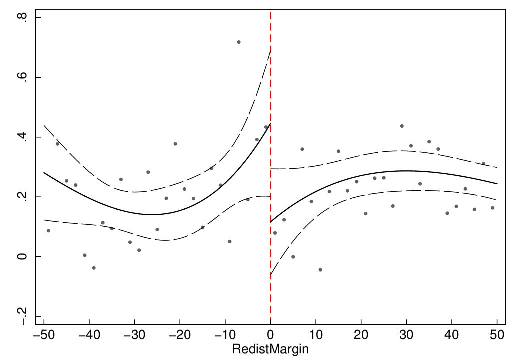
Figure XV: Redistrictor Electoral Margin and SBA 7(a) Lending by Delegated Banks. These figures illustrate the findings for delegated banks reported in Table XI. The solid lines in each subfigure present the mean estimated change in each outcome variable for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: total SBA-guaranteed loan amounts (in millions of USD), number of loans issued, and principal amount per loan (in millions of USD). The first row of figures ((a) through (c)) present changes in these variables measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). The second row of figures ((d) through (f)) present changes in these variables measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). All figures present findings for districts in partisan redistricting states where the redistricting party won the pre-districting election.



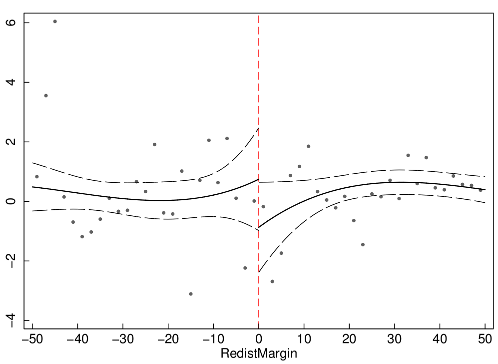
(a) TotAmt (early cycle)



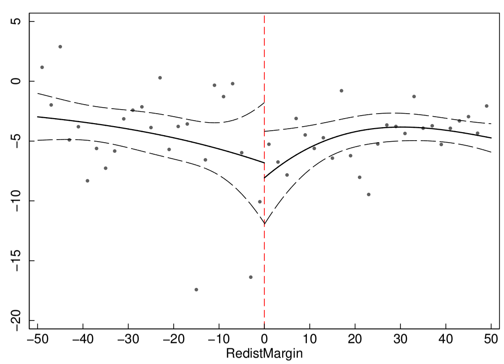
(b) NumLoans (early cycle)



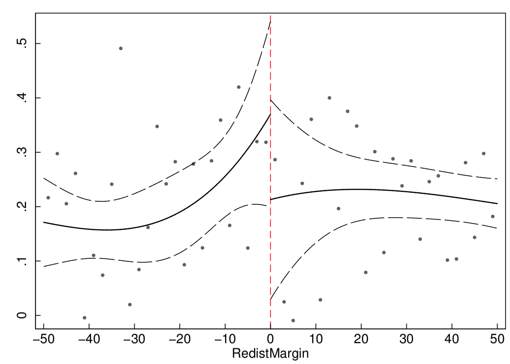
(c) TotAmtPerLoan (early cycle)



(d) TotAmt (late cycle)

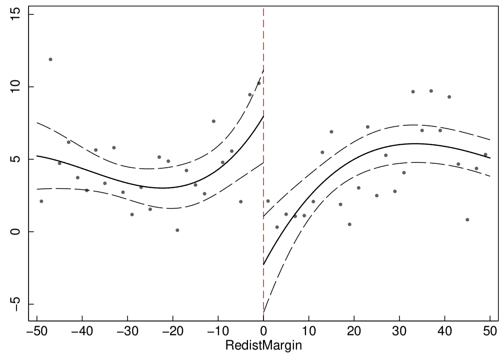


(e) NumLoans (late cycle)

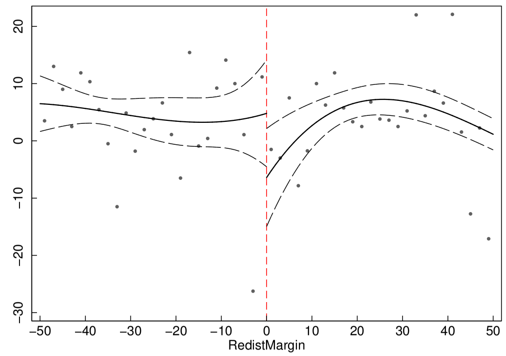


(f) TotAmtPerLoan (late cycle)

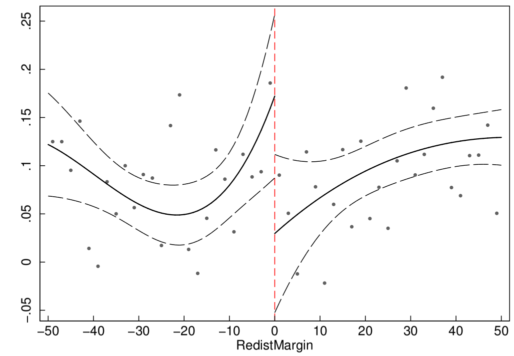
Figure XVI: Redistrictor Electoral Margin and SBA 7(a) Lending by Non-delegated Banks. These figures illustrate the findings for non-delegated banks reported in Table XI. The solid lines in each subfigure present the mean estimated change in each outcome variable for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: total SBA-guaranteed loan amounts (in millions of USD), number of loans issued, and principal amount per loan (in millions of USD). The first row of figures ((a) through (c)) present changes in these variables measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). The second row of figures ((d) through (f)) present changes in these variables measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). All figures present findings for districts in partisan redistricting states where the redistricting party won the pre-districting election.



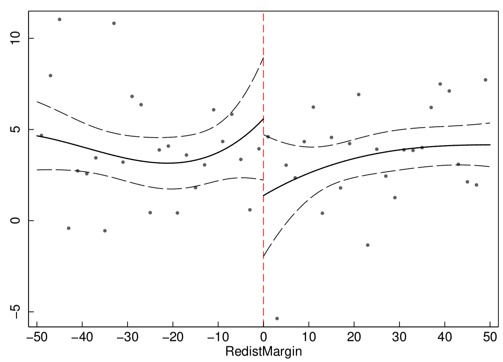
(a) TotAmt (early cycle)



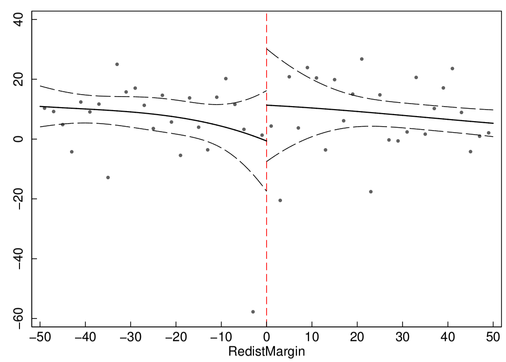
(b) NumLoans (early cycle)



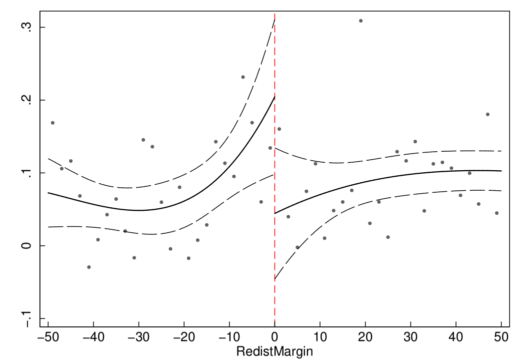
(c) TotAmtPerLoan (early cycle)



(d) TotAmt (late cycle)

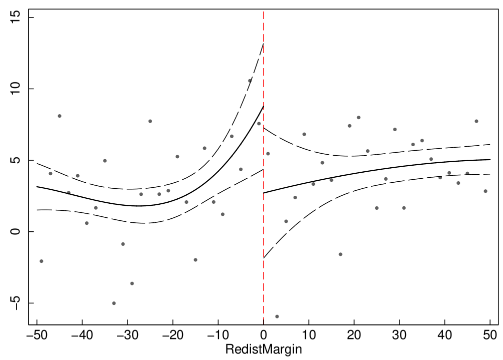


(e) NumLoans (late cycle)

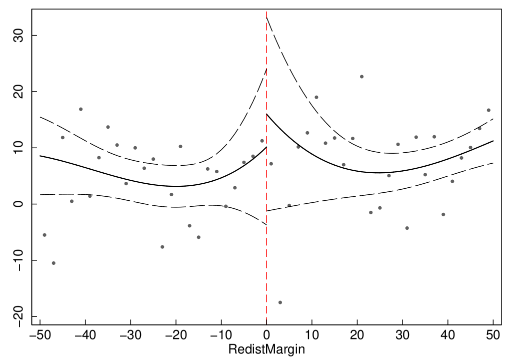


(f) TotAmtPerLoan (late cycle)

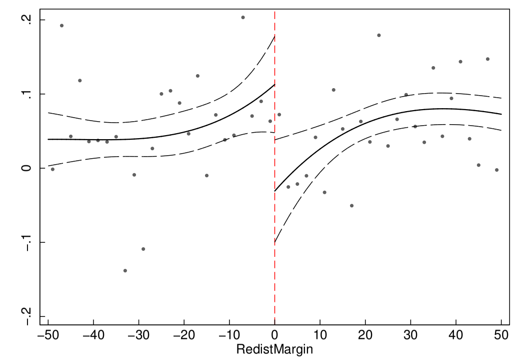
Figure XVII: Redistrictor Electoral Margin and SBA Lending by Local Banks. These figures illustrate the findings for local banks reported in Table XII. The solid lines in each subfigure present the mean estimated change in each outcome variable for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: total SBA-guaranteed loan amounts (in millions of USD), number of loans issued, and principal amount per loan (in millions of USD). The first row of figures ((a) through (c)) present changes in these variables measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). The second row of figures ((d) through (f)) present changes in these variables measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). All figures present findings for districts in partisan redistricting states where the redistricting party won the pre-districting election.



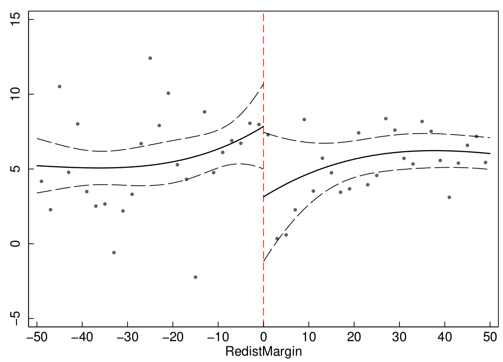
(a) TotAmt (early cycle)



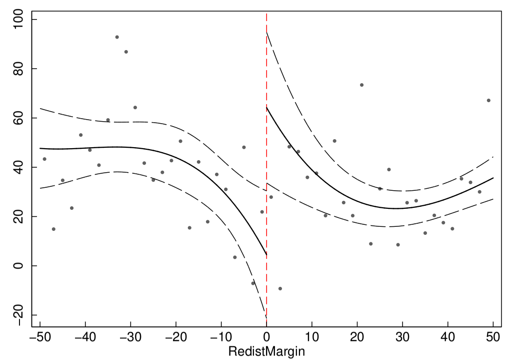
(b) NumLoans (early cycle)



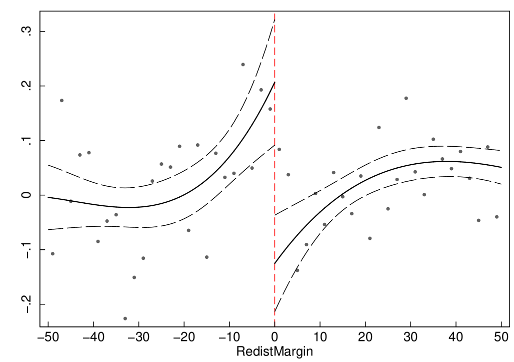
(c) TotAmtPerLoan (early cycle)



(d) TotAmt (late cycle)

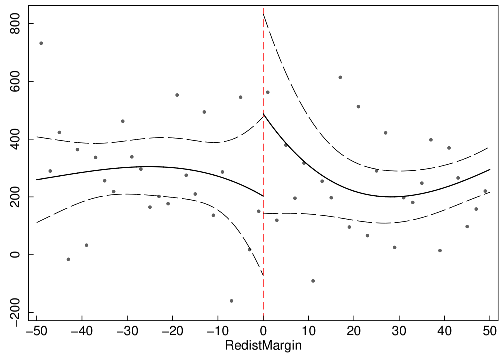


(e) NumLoans (late cycle)

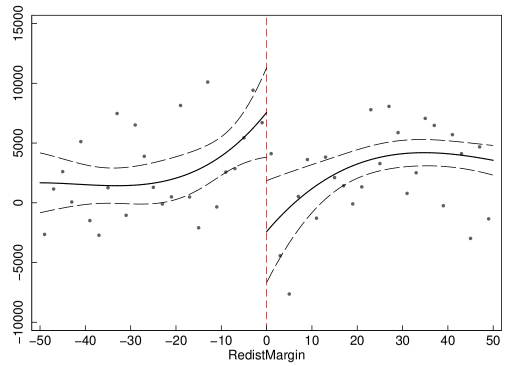


(f) TotAmtPerLoan (late cycle)

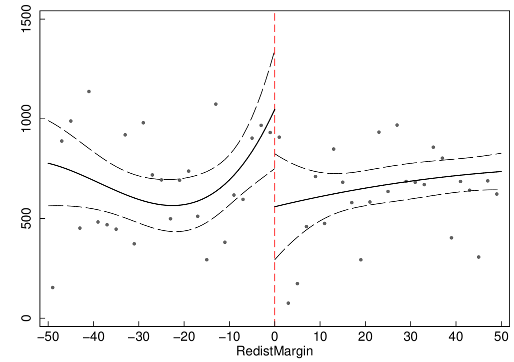
Figure XVIII: Redistrictor Electoral Margin and SBA 7(a) Lending by Non-Local Banks. These figures illustrate the findings for non-local banks reported in Table XII. The solid lines in each subfigure present the mean estimated change in each outcome variable for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: total SBA-guaranteed loan amounts (in millions of USD), number of loans issued, and principal amount per loan (in millions of USD). The first row of figures ((a) through (c)) present changes in these variables measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). The second row of figures ((d) through (f)) present changes in these variables measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). All figures present findings for districts in partisan redistricting states where the redistricting party won the pre-districting election.



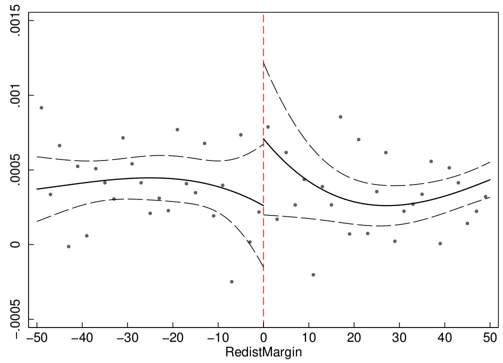
(a) Estabs



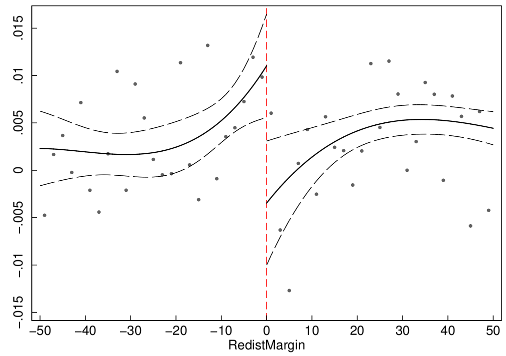
(b) Emp



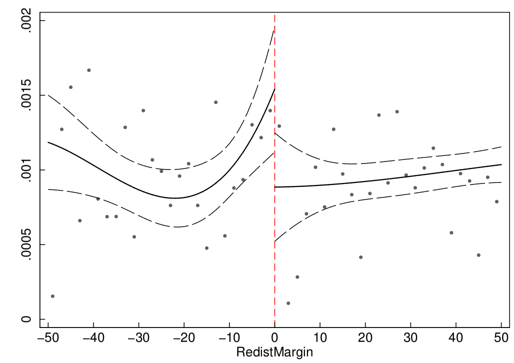
(c) Wages



(d) EstabsPerCap

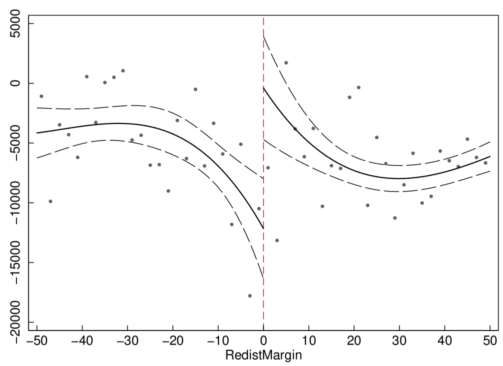


(e) EmpPerCap

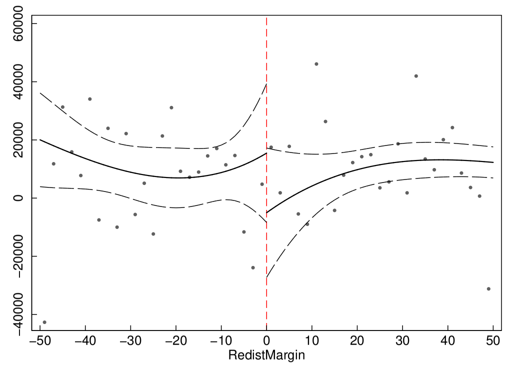


(f) TotAmtPerLoanWagesPerCap

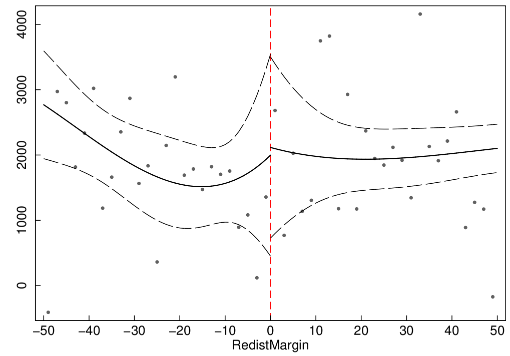
Figure XIX: Redistrictor Electoral Margin and Local Economic Growth (Early Cycle). These figures illustrate the findings reported in Panel A of Table XIII. The solid lines represent the mean estimated change in outcomes for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: number of establishments, employment, and wages, on aggregate and per-capita bases. Aggregate wage is in millions of USD. Changes are measured from the two years before redistricting (e.g. 1999 and 2000) to the two years before the first post-redistricting election (e.g. 2001 and 2002) for districts in partisan redistricting states where the redistricting party won the pre-districting election.



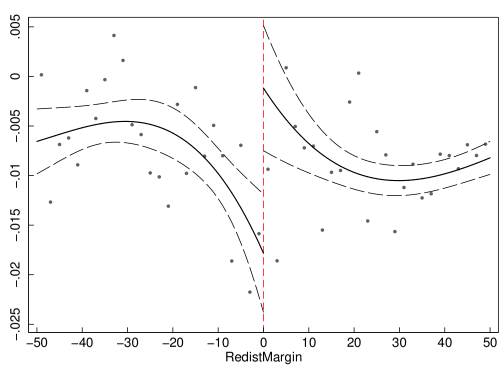
(a) Estabs



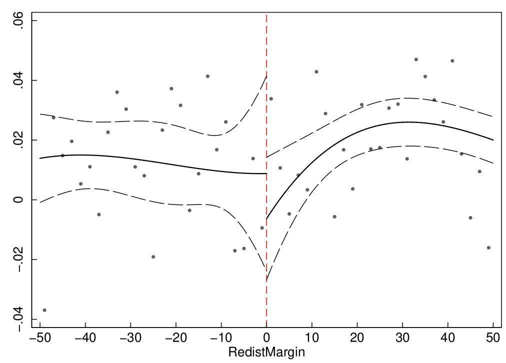
(b) Emp



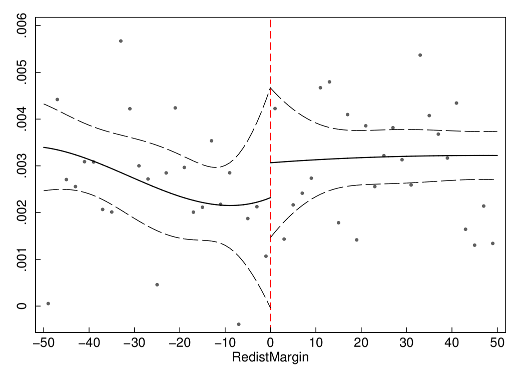
(c) Wages



(d) EstabsPerCap

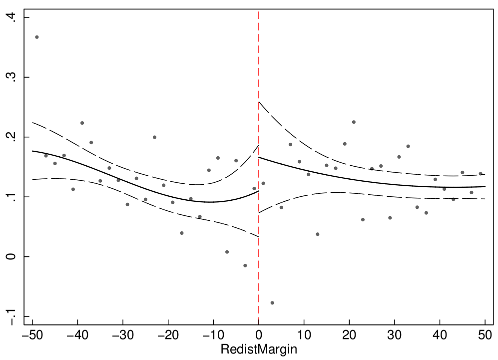


(e) EmpPerCap

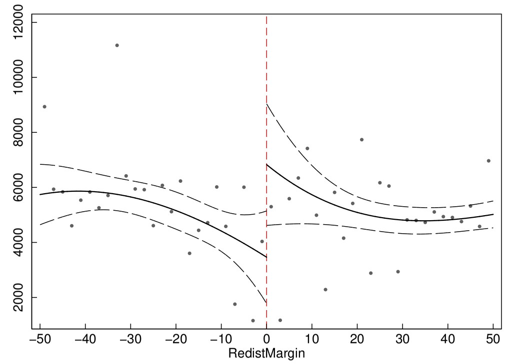


(f) WagesPerCap

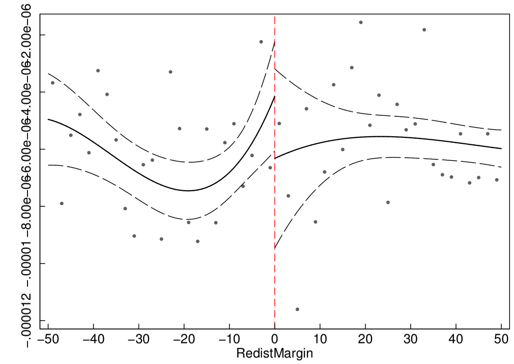
Figure XX: Redistrictor Electoral Margin and Local Economic Growth (Late Cycle). These figures illustrate the findings reported in Panel B of Table XIII. The solid lines represent the mean estimated change in outcomes for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: number of establishments, employment, and wages, on aggregate and per-capita bases. Aggregate wage is in millions of USD. Changes are measured from the two years before redistricting (e.g. 1999 and 2000) to the six years after the first post-redistricting election (e.g. 2003 through 2008) for districts in partisan redistricting states where the redistricting party won the pre-districting election.



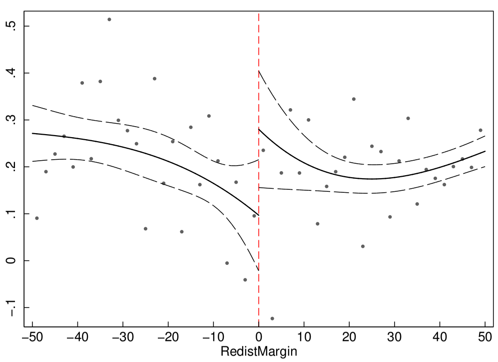
(a) TotAmt (early cycle)



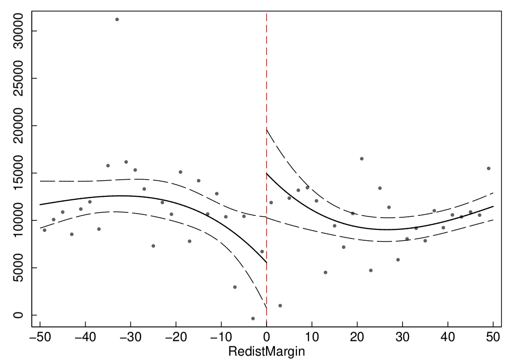
(b) NumLoans (early cycle)



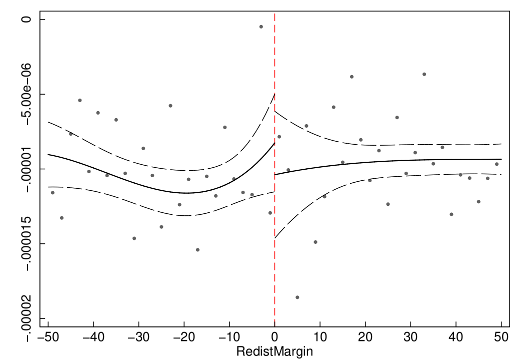
(c) TotAmtPerLoan (early cycle)



(d) TotAmt (late cycle)

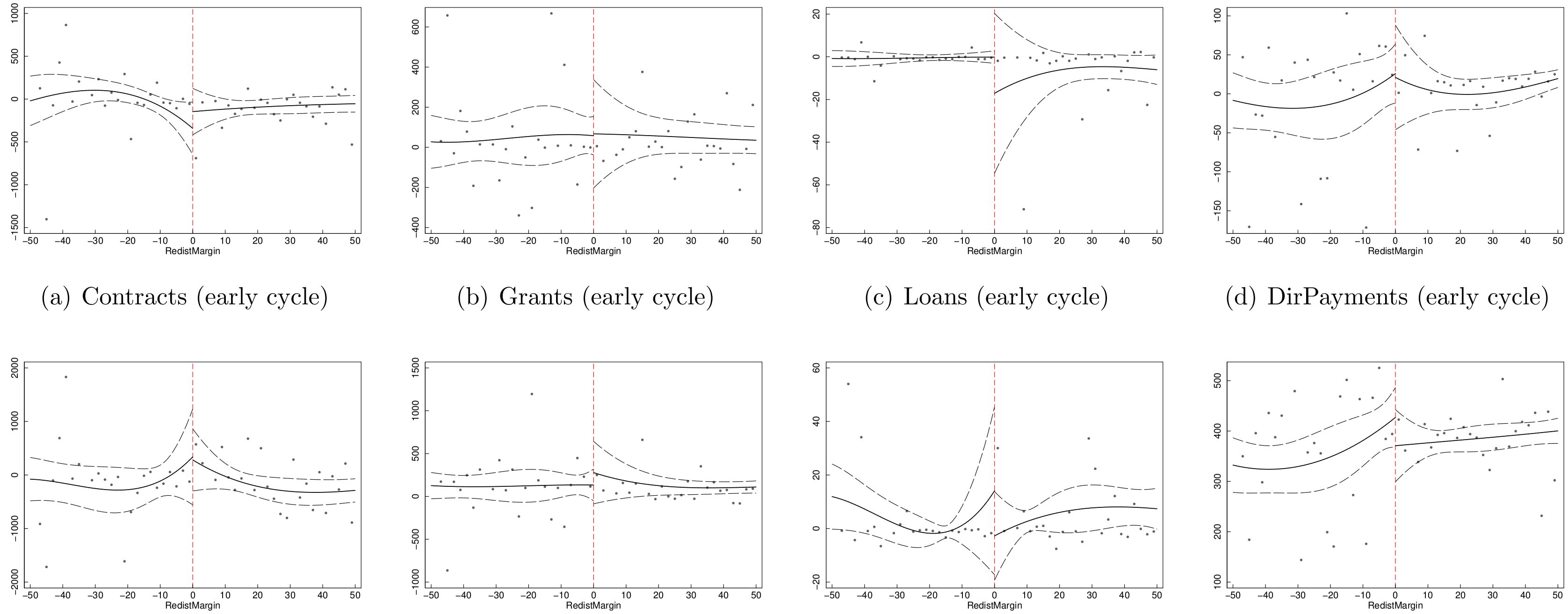


(e) NumLoans (late cycle)



(f) TotAmtPerLoan (late cycle)

Figure XXI: Redistrictor Electoral Margin and CRA-Reported Small Business Lending. These figures illustrate the findings reported in Panel A of Table XIV. The solid lines represent the mean estimated change in outcomes for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: annual small business lending (in millions of USD), number of loans per year, and annual principal per loan (in millions of USD). The first row of figures ((a) through (c)) present changes in these variables measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). The second row of figures ((d) through (f)) present changes in these variables measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). All figures present findings for districts in partisan redistricting states where the redistricting party won the pre-districting election.



(a) Contracts (early cycle) (b) Grants (early cycle) (c) Loans (early cycle) (d) DirPayments (early cycle)

(e) Contracts (late cycle) (f) Grants (late cycle) (g) Loans (late cycle) (h) DirPayments (late cycle)

Figure XXII: Redistrictor Electoral Margin and Federal Financial Assistance. These figures illustrate the findings reported in Panel B of Table XIV. The solid lines represent the mean estimated change in outcomes for every level of redistrictor win margin in the pre-redistricting election from -50 to 50. The dashed lines represent the 95% confidence intervals for the estimated change. The outcome variables are change in: total amount in contracts (in millions of USD), total amount in grants (in millions of USD), total amount in loans (in millions of USD), and total amount in direct payments (in millions of USD). The first row of figures ((a) through (d)) present changes in these variables measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). The second row of figures ((e) through (h)) present changes in these variables measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). All figures present findings for districts in partisan redistricting states where the redistricting party won the pre-districting election.

Table I: Redistricting Control. This table presents data on how each US state handles decennial redistricting. The first four columns present the mechanism of congressional redistricting: whether the state legislature is in control, there is only one district in the state, an independent commission is in control, or a bipartisan commission is in control. Aside from California, there is no change from the 2000 cycle to the 2010 cycle in redistricting mechanism. The last four columns depict whether, for states with legislature-controlled redistricting, one party is in charge of redistricting in 2000 and 2010 and whether the legislature's control is invulnerable to an opposing party governor's veto in those cycles.

	State legislature	One district	Independent commission	Bipartisan commission	Partisan control		Veto-proof	
					2000	2010	2000	2010
Alabama	×				×	×	×	×
Alaska		×						
Arizona			×					
Arkansas	×				×	×	×	×
California	00		10		×		×	
Colorado	×							
Connecticut	×							
Delaware		×						
Florida	×				×	×	×	×
Georgia	×				×	×	×	×
Hawaii				×				
Idaho			×					
Illinois	×					×		×
Indiana	×				×	×	×	×
Iowa	×				×			
Kansas	×				×	×	×	×
Kentucky	×							
Louisiana	×				×	×		×
Maine	×							
Maryland	×				×	×	×	×
Massachusetts	×				×	×		×
Michigan	×				×	×	×	×
Minnesota	×					×		
Mississippi	×				×	×	×	
Missouri	×					×		×
Montana			×					
Nebraska	×							
Nevada	×					×		
New Hampshire	×				×	×		×
New Jersey				×				
New Mexico	×				×	×		
New York	×							
North Carolina	×				×	×	×	×
North Dakota		×						
Ohio	×				×	×	×	×
Oklahoma	×				×	×		×
Oregon	×				×			
Pennsylvania	×				×	×	×	×
Rhode Island	×				×	×	×	×
South Carolina	×				×	×		×
South Dakota		×						
Tennessee	×				×	×		×
Texas	×					×		×
Utah	×				×	×	×	×
Vermont		×						
Virginia	×				×	×	×	×
Washington			×					
West Virginia	×				×	×	×	×
Wisconsin	×					×		×
Wyoming		×						

Table II: Summary Statistics for Elections, SBA Lending, and Local Economic Performance. This table presents summary statistics for three sets of data used in our analyses: U.S. House of Representatives elections, SBA lending in its 7(a) and 504 loan programs, and local economic performance. We present mean, standard deviation, median, 25th percentile, 75th percentile, and total number of observations for each variable. For the elections data, we provide summary statistics for incumbents' win margin for all elections in a redistricting cycle and for redistricting parties' win margin for the election immediately before redistricting occurs. We include all districts in states with partisan redistricting in the 2000 and 2010 redistricting cycles. For SBA lending data, we provide summary statistics at the district-year level aggregated for all 7(a) and 504 lending between 1999 and 2016 in states with partisan redistricting. For local economic performance, we provide summary statistics for data sourced from QCEW at the district-year level between 1999 and 2016 in states with partisan redistricting.

	Mean	SD	Median	25th %ile	75th %ile	N
Elections						
RedistMargin (c)	0.138	0.387	0.152	-0.174	0.401	506
RedistWin (c)	0.636	0.482	1.000	0.000	1.000	506
IncumbMargin ($c+2$)	0.323	0.297	0.313	0.153	0.480	484
IncumbWin ($c+2$)	0.886	0.318	1.000	1.000	1.000	484
SBA lending						
GtdAmt, $1M	29.1	20.1	24.3	15.1	38.5	4,952
NumLoans	127.8	101.8	104.0	60.0	174.5	4,952
GtdAmtPerLoan, $1M	0.3	0.3	0.2	0.2	0.4	4,952
JobsPerLoan	12.7	6.1	11.8	9.4	14.8	4,948
ChgOff	6.7	9.6	2.2	0.0	10.2	4,952
PIF	34.4	26.5	32.0	8.5	57.4	4,952
Maturity	179.1	26.8	172.8	163.9	186.0	4,952
Local economic performance						
Estabs, 1,000s	14.40	7.62	16.00	12.75	19.11	4,502
Jobs, 1,000s	247.00	56.61	237.44	205.10	278.26	4,502
Wages, $1M	10341.97	4,739.04	9,228.04	7,004.14	12294.96	4,502
EstabsPerCap	0.02	0.01	0.02	0.02	0.03	4,502
JobsPerCap	0.37	0.08	0.36	0.31	0.41	4,502
WagesPerCap, $1M	0.02	0.01	0.01	0.01	0.02	4,502

Table III: Narrow Redistrictor Losses and Census Covariates. This table presents the results of RDD regressions as presented in Equation 1 on Census covariates measured in the pre-redistricting election year. The covariates are district-level median age, percent of population that is non-Hispanic white, percent of population that is male, percent of population that is rural, median house value, and percent of labor force that is unemployed. Each RDD regression uses two independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. All regressions are performed at the district-decade level for all redistricting states districts for two redistricting cycles: 2000s and 2010s. For each cycle, the outcome variable is for the year of the pre-redistricting election (e.g., 2000 and 2010). Standard errors are clustered at the district level.

	(1) MedAge	(2) PctWhite	(3) PctMale	(4) PctRural	(5) HouseVal	(6) UnempRate
RedistrictorLoss	0.706	0.0145	16622.2	-0.0527	0.00610	-0.00178
	[0.62]	[0.31]	[0.53]	[0.78]	[0.91]	[0.77]
Controls	No	No	No	No	No	No
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3
R^2	0.068	0.164	0.045	0.041	0.086	0.109
Observations	506	506	506	506	506	506

Table IV: Narrow Redistrictor Losses and Post-Redistricting Election Outcomes. This table presents the results of RDD regressions as presented in Equation 1 on incumbent win margins and incumbent win dummy variables. Each RDD regression uses two independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. For each cycle, the outcome variable is defined for the first post-redistricting election (e.g., 2002 and 2012). For columns (3) and (4),we include the following demographic and socioeconomic covariates from the pre-redistricting election year as controls: median age in district, percent of district population that is white, median home value, percent of population that is rural, unemployment rate in the district, and percent of district population that is male. All regressions are performed at the district-decade level for two redistricting cycles: 2000s and 2010s. Standard errors are clustered at the district level.

	(1) IncumbMargin	(2) IncumbWin	(3) IncumbMargin	(4) IncumbWin
RedistrictorLoss	-23.10***	-0.307**	-20.32***	-0.288**
	[2.98]	[2.20]	[2.60]	[2.07]
Controls	No	No	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3
R^2	0.216	0.101	0.261	0.126
Observations	484	484	484	484

Table V: Narrow Redistrictor Losses and Later Election Outcomes. This table presents the coefficient estimates for RDD regressions as presented in Equation 2 on incumbent win margins, incumbent win likelihood, and number of terms an incumbent has been in office. Each RDD regression uses two independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. In Panel A, the regressions are performed on all districts in states with partisan redistricting. In Panel B, only districts in partisan redistricting states where the redistricting party won the election prior to the pre-redistricting election are included. The four rows in each panel vary the post-redistricting election year. For instance, in the second row, change in the incumbent win margin is defined as the difference between the first post-redistricting election (e.g., 2004 and 2014) and the last pre-redistricting election (e.g., 2000 and 2010). All regressions are performed at the district-decade level for two redistricting cycles: 2000s and 2010s. Standard errors are clustered at the district level.

Panel A: All Districts

Post-redistricting year	IncumbMargin	IncumbWin	IncumbTenure
$t+2$	20.3***	0.288**	-3.4*
	[7.8]	[0.1]	[1.8]
$t+4$	3.6	0.005	-0.1
	[7.5]	[0.1]	[1.4]
$t+6$	-3.2	0.033	-0.1
	[7.7]	[0.1]	[1.4]
$t+8$	1.3	-0.003	-0.3
	[11.8]	[0.2]	[2.3]

Panel B: Non-Wave Districts

Post-redistricting year	IncumbMargin	IncumbWin	IncumbTenure
$t+2$	34.5***	0.423**	1.3
	[13.1]	[0.2]	[2.5]
$t+4$	5.1	0.007	5.9**
	[11.9]	[0.1]	[2.4]
$t+6$	0.8	-0.078	4.6*
	[10.6]	[0.1]	[2.4]
$t+8$	-1.8	0.024	0.8
	[14.6]	[0.2]	[3.2]

Table VI: Narrow Redistrictor Losses and Changes in SBA-Guaranteed Lending. This table presents the results of RDD regressions on early- and late-cycle growth in SBA-guaranteed lending. Panel A present early-cycle growth results and Panel B presents late-cycle growth results. In Panel A, growth in the outcome variables is measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). In Panel B, growth in the outcome variables is measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). In both panels, the four columns present results for: (1) annual SBA guarantee amounts (in millions of USD), (2) number of loans issued per year, (3) annual SBA guarantee per loan (in millions of USD), and (4) jobs supported per loan. Each RDD regression uses independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. Each regression also has demographic and socioeconomic covariates from the pre-redistricting election year as covariates: median age in district, percent of district population that is white, median home value, percent of population that is rural, unemployment rate in the district, and percent of district population that is male. All regressions are performed at the district-decade level for all redistricting states districts for districts in partisan redistricting states where the redistricting party won the pre-districting election for the 2000 and 2010 redistricting cycles. Standard errors are clustered at the district level.

Panel A: Early Cycle Changes

	(1) GtdAmt	(2) NumLoans	(3) GtdAmtPerLoan	(4) JobsPerLoan
RedistrictorLoss	10.23**	-1.446	0.0635**	4.287**
	[2.55]	[0.10]	[2.42]	[2.34]
Controls	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3
R-sq	0.192	0.123	0.195	0.144
Observations	433	433	433	433

Panel B: Late Cycle Changes

	(1) GtdAmt	(2) NumLoans	(3) GtdAmtPerLoan	(4) JobsPerLoan
RedistrictorLoss	-4.128	-71.38**	0.212***	5.837***
	[0.89]	[2.28]	[3.47]	[3.29]
Controls	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3
R-sq	0.198	0.291	0.271	0.162
Observations	433	433	433	433

Table VII: Placebo Test on Sample of Non-Partisan Redistricting States. This table presents the results of RDD regressions on elections, early-cycle SBA-guaranteed lending growth, and late-cycle SBA-guaranteed lending growth in states where redistricting is not controlled by the party in power. For elections, in columns (1) and (2), it presents results for incumbent win margins and likelihood of an incumbent win. For early-cycle and late-cycle SBA-guaranteed lending growth, in columns (3) through (5) and (6) through (8), respectively, it presents results for annual SBA guarantee amounts (in millions of USD), annual SBA-guaranteed amount per loan (in millions of USD), and jobs supported per loan. Each RDD regression uses two independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. In columns (1) and (2), the outcome variable is defined for the first post-redistricting election (e.g., 2002 and 2012). In columns (3) through (5), growth in the outcome variables is measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). In columns (6) through (8), growth in the outcome variables is measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). We include the following demographic and socioeconomic covariates from the pre-redistricting election year as controls: median age in district, percent of district population that is white,median home value, percent of population that is rural, unemployment rate in the district, and percent of district population that is male. All regressions are performed at the district-decade level for all non-partisan redistricting states districts for two redistricting cycles: 2000s and 2010s. Standard errors are clustered at the district level.

	Elections		SBA Early-cycle			SBA Late-cycle		
	(1) IncumbMargin	(2) IncumbWin	(3) GtdAmt	(4) GtdAmtPerLoan	(5) JobsPerLoan	(6) GtdAmt	(7) GtdAmtPerLoan	(8) JobsPerLoan
RedistrictorLoss	-22.67	-0.145	0.164	-0.0388	1.753	-9.822*	0.155	1.924
	[1.30]	[0.43]	[0.02]	[0.82]	[0.85]	[1.97]	[1.59]	[0.67]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3	3	3
R^2	0.319	0.110	0.444	0.389	0.069	0.269	0.550	0.135
Observations	117	117	112	112	112	112	112	112

Table VIII: Narrow Redistrictor Losses and SBA Loan Performance. This table presents the results of RDD regressions on early- and late-cycle changes in SBA-guaranteed lending performance. Panel A present results for early-cycle performance changes and Panel B presents results for late-cycle performance changes. In Panel A, change in the outcome variables is measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). In Panel B, change in the outcome variables is measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). In both panels, the outcome variables are percent of total principal and loans charged-off (columns (1) and (4)), percent of total principal and loans paid in full (columns (2) and (5)), percent of total principal and loans exempt from reporting (columns (3) and (6)), and average maturity of loans at issuance. Each RDD regression uses independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. Each regression also has demographic and socioeconomic covariates from the pre-redistricting election year as covariates: median age in district, percent of district population that is white, median home value, percent of population that is rural, unemployment rate in the district, and percent of district population that is male. All regressions are performed at the district-decade level for all redistricting states districts for districts in partisan redistricting states where the redistricting party won the pre-districting election for the 2000 and 2010 redistricting cycles. Standard errors are clustered at the district level.

Panel A: Early Cycle Changes

	(1) ChgOff	(2) PIF	(3) Exempt	(4) Maturity
RedistrictorLoss	-0.0199	-0.0744**	0.0582*	-2.280
	[0.98]	[2.35]	[1.74]	[0.58]
Controls	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3
R-sq	0.161	0.215	0.316	0.098
Observations	433	433	433	433

Panel B: Late Cycle Changes

	(1) ChgOff	(2) PIF	(3) Exempt	(4) Maturity
RedistrictorLoss	-0.117***	-0.105**	0.147***	24.71***
	[2.86]	[2.54]	[2.89]	[2.92]
Controls	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3
R-sq	0.314	0.348	0.372	0.297
Observations	433	433	433	433

Table IX: Narrow Redistrictor Losses and SBA 7(a) Lending. This table presents the results of RDD regressions on early- and late-cycle growth in SBA-guaranteed lending through the 7(a) loan program. Panel A present early-cycle growth results and Panel B presents late-cycle growth results. In Panel A, growth in the outcome variables is measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). In Panel B, growth in the outcome variables is measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). In both panels, the seven columns present results for: (1) annual SBA guarantee amounts (in millions of USD), (2) annual SBA lending (in millions of USD), (3) number of loans issued per year, (4) annual SBA guarantee per loan (in millions of USD), (5) annual SBA principal per loan (in millions of USD), and (6) jobs supported per loan, and (7) dollar-weighted average interest rate. Each RDD regression uses independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. Each regression also has demographic and socioeconomic covariates from the pre-redistricting election year as covariates: median age in district, percent of district population that is white, median home value, percent of population that is rural, unemployment rate in the district, and percent of district population that is male. All regressions are performed at the district-decade level for all redistricting states districts for districts in partisan redistricting states where the redistricting party won the pre-redistricting election for the 2000 and 2010 redistricting cycles. Standard errors are clustered at the district level.

Panel A: Early Cycle Changes

	(1) GtdAmt	(2) TotAmt	(3) NumLoans	(4) GtdAmtPerLoan	(5) TotAmtPerLoan	(6) JobsPerLoan	(7) IntRate
RedistrictorLoss	9.866***	13.67***	-1.918	0.0682***	0.0999***	3.450*	-0.393***
	[3.15]	[3.13]	[0.14]	[2.89]	[3.07]	[1.68]	[3.92]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3	3
R-sq	0.142	0.163	0.109	0.198	0.232	0.130	0.191
Observations	433	433	433	433	433	433	217

Panel B: Late Cycle Changes

	(1) GtdAmt	(2) TotAmt	(3) NumLoans	(4) GtdAmtPerLoan	(5) TotAmtPerLoan	(6) JobsPerLoan	(7) IntRate
RedistrictorLoss	5.716**	8.659**	-47.17*	0.125***	0.179***	4.660**	-0.0756
	[2.03]	[2.28]	[1.88]	[3.18]	[3.23]	[2.43]	[0.58]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3	3
R-sq	0.088	0.100	0.263	0.255	0.269	0.197	0.065
Observations	433	433	433	433	433	433	217

Table X: Narrow Redistrictor Losses and SBA 504 Lending. This table presents the results of RDD regressions on early- and late-cycle growth in SBA-guaranteed lending through the 504 loan program. Panel A present early-cycle growth results and Panel B presents late-cycle growth results. In Panel A, growth in the outcome variables is measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). In Panel B, growth in the outcome variables is measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). In both panels, the four columns present results for: (1) annual SBA guarantee amounts (in millions of USD), (2) number of loans issued per year, (3) annual SBA guarantee per loan (in millions of USD), and (4) jobs supported per loan. Each RDD regression uses independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. Each regression also has demographic and socioeconomic covariates from the pre-redistricting election year as covariates: median age in district, percent of district population that is white, median home value, percent of population that is rural, unemployment rate in the district, and percent of district population that is male. All regressions are performed at the district-decade level for all redistricting states districts for districts in partisan redistricting states where the redistricting party won the pre-redistricting election for the 2000 and 2010 redistricting cycles. Standard errors are clustered at the district level.

Panel A: Early Cycle Changes

	(1) GtdAmt	(2) NumLoans	(3) GtdAmtPerLoan	(4) JobsPerLoan
RedistrictorLoss	0.625	1.921	-0.0719	7.004*
	[0.38]	[0.83]	[0.89]	[1.71]
Controls	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3
R-sq	0.159	0.150	0.046	0.063
Observations	430	430	430	430

Panel B: Late Cycle Changes

	(1) GtdAmt	(2) NumLoans	(3) GtdAmtPerLoan	(4) JobsPerLoan
RedistrictorLoss	-3.096	-5.940	0.119*	6.440*
	[1.17]	[1.36]	[1.68]	[1.87]
Controls	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3
R-sq	0.231	0.177	0.054	0.044
Observations	431	431	431	430

Table XI: Narrow Redistrictor Losses and SBA 7(a) Lending by Delegated and Non-delegated Banks. This table presents the results of RDD regressions on early- and late-cycle growth in SBA 7(a) program lending through banks with more and less discretion (Delegated banks versus Non-delegated banks). Banks that have issued loans through the PLP and/or Express programs in a year are treated as having greater discretion in lending. Panel A present early-cycle growth results and Panel B presents late-cycle growth results. In Panel A, growth in the outcome variables is measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). In Panel B, growth in the outcome variables is measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). In both panels, results are provided for delegated and non-delegated banks separately for three outcomes: annual SBA lending (in millions of USD) (columns (1) and (4)), number of loans issued per year (columns (2) and (5), and annual SBA principal per loan (columns (3) and (6)). Each RDD regression uses independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. Each regression also has demographic and socioeconomic covariates from the pre-redistricting election year as covariates: median age in district, percent of district population that is white, median home value, percent of population that is rural, unemployment rate in the district, and percent of district population that is male. All regressions are performed at the district-decade level for all redistricting states districts for districts in partisan redistricting states where the redistricting party won the pre-redistricting election for the 2000 and 2010 redistricting cycles. Standard errors are clustered at the district level.

Panel A: Early Cycle Changes

	Delegated banks			Non-delegated banks		
	(1) TotAmt	(2) NumLoans	(3) TotAmtPerLoan	(4) TotAmt	(5) NumLoans	(6) TotAmtPerLoan
RedistrictorLoss	10.56***	0.351	0.0679**	3.856***	0.0952	0.124
	[2.62]	[0.03]	[2.09]	[2.97]	[0.04]	[0.93]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3
R-sq	0.145	0.104	0.169	0.089	0.077	0.221
Observations	433	433	433	433	433	423

Panel B: Late Cycle Changes

	Delegated banks			Non-delegated banks		
	(1) TotAmt	(2) NumLoans	(3) TotAmtPerLoan	(4) TotAmt	(5) NumLoans	(6) TotAmtPerLoan
RedistrictorLoss	7.607**	-46.92*	0.167***	1.046	0.458	0.0300
	[2.39]	[1.93]	[2.96]	[0.94]	[0.15]	[0.25]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3
R-sq	0.111	0.278	0.285	0.091	0.110	0.167
Observations	433	433	433	433	433	427

Table XII: Narrow Redistrictor Losses and SBA 7(a) Lending by Local and Non-local Banks. This table presents the results of RDD regressions on early- and late-cycle growth in SBA 7(a) program lending through local and non-local banks. Banks that are headquartered in the same state as the borrower are treated as being local. Panel A present early-cycle growth results and Panel B presents late-cycle growth results. In Panel A, growth in the outcome variables is measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). In Panel B, growth in the outcome variables is measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). In both panels, results are provided for local and non-local banks separately for three outcomes: annual SBA lending (in millions of USD) (columns (1) and (4)), number of loans issued per year (columns (2) and (5), and annual SBA principal per loan (columns (3) and (6)). Each RDD regression uses independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. Each regression also has demographic and socioeconomic covariates from the pre-redistricting election year as covariates: median age in district, percent of district population that is white, median home value, percent of population that is rural, unemployment rate in the district, and percent of district population that is male. All regressions are performed at the district-decade level for all redistricting states districts for districts in partisan redistricting states where the redistricting party won the pre-redistricting election for the 2000 and 2010 redistricting cycles. Standard errors are clustered at the district level.

Panel A: Early Cycle Changes

	Local banks			Non-local banks		
	(1)	(2)	(3)	(4)	(5)	(6)
	TotAmt	NumLoans	TotAmtPerLoan	TotAmt	NumLoans	TotAmtPerLoan
RedistrictorLoss	8.854***	7.926	0.0859	4.219	-5.969	0.0964**
	[3.68]	[1.31]	[1.48]	[1.39]	[0.55]	[2.08]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3
R-sq	0.098	0.127	0.148	0.159	0.123	0.206
Observations	433	433	422	433	433	433

Panel B: Late Cycle Changes

	Local banks			Non-local banks		
	(1)	(2)	(3)	(4)	(5)	(6)
	TotAmt	NumLoans	TotAmtPerLoan	TotAmt	NumLoans	TotAmtPerLoan
RedistrictorLoss	4.765**	-6.423	0.126*	3.641	-37.88**	0.239***
	[2.09]	[0.56]	[1.84]	[1.38]	[2.03]	[3.08]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3
R-sq	0.072	0.100	0.125	0.087	0.273	0.271
Observations	433	433	422	433	433	433

Table XIII: Narrow Redistrictor Losses and Private Sector Economic Growth. This table presents the results of RDD regressions on early- and late-cycle local economic growth. Panel A present early-cycle growth results and Panel B presents late-cycle growth results. In Panel A, growth in the outcome variables is measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). In Panel B, growth in the outcome variables is measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). In both panels, the six columns present aggregate and per-capital results for three outcomes: number of establishments (columns (1) and (4)), employment (columns (2) and (5)), and (3) wages (in millions of USD) (columns (3) and (6)). Each RDD regression uses independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. Each regression also has demographic and socioeconomic covariates from the pre-redistricting election year as covariates: median age in district, percent of district population that is white, median home value, percent of population that is rural, unemployment rate in the district, and percent of district population that is male. All regressions are performed at the district-decade level for all redistricting states districts for districts in partisan redistricting states where the redistricting party won the pre-redistricting election for the 2000 and 2010 redistricting cycles. Standard errors are clustered at the district level.

Panel A: Early Cycle Changes

	(1) Estabs	(2) Emp	(3) Wages	(4) EstabsPerCap	(5) EmpPerCap	(6) WagesPerCap
RedistrictorLoss	-129.2	8650.4***	500.7***	-0.000238	0.0122***	0.000648***
	[0.63]	[3.09]	[2.81]	[0.77]	[2.93]	[2.60]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3
R-sq	0.214	0.226	0.238	0.206	0.239	0.259
Observations	440	440	440	440	440	440

Panel B: Late Cycle Changes

	(1) Estabs	(2) Emp	(3) Wages	(4) EstabsPerCap	(5) EmpPerCap	(6) WagesPerCap
RedistrictorLoss	-7621.8***	6469.9	-561.2	-0.0102***	0.00488	-0.000863
	[2.75]	[0.41]	[0.51]	[2.72]	[0.24]	[0.58]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3
R-sq	0.351	0.167	0.068	0.394	0.093	0.045
Observations	440	440	440	440	440	440

Table XIV: Narrow Redistrictor Losses and Alternative Channels of Economic Stimulus. This table presents the results of RDD regressions on early- and late-cycle small business lending and federal government financial assistance growth. Panel A present early-cycle growth results and Panel B presents late-cycle growth results. In Panel A, growth in the outcome variables is measured from the two years before redistricting (e.g., 1999 and 2000 for the 2000 redistricting cycle) to the two years post-redistricting before the first post-redistricting election (e.g., 2001 and 2002 for the 2000 cycle). In Panel B, growth in the outcome variables is measured from the two years before redistricting to the six years post-redistricting after the first post-redistricting election (e.g., 2003 through 2008 for the 2000 cycle). In both panels, the first three columns present results for overall small business lending growth based on CRA data and the last four columns present results for federal financial assistance growth based on USASpending data. The three small business lending outcomes are: (1) annual small business lending (in millions of USD), (2) number of loans per year, and (3) annual principal per loan (in millions of USD). The four federal financial assistance outcomes are: (4) total amount in contracts (in millions of USD), (5) total amount in grants (in millions of USD), (6) total amount in loans (in millions of USD), and (7) total amount in direct payments (in millions of USD). Each RDD regression uses independent cubic polynomial splines of the pre-redistricting vote margin as the "running" variables for districts where the redistricting party won and lost the pre-redistricting local election. The reported coefficient is the discontinuous difference in the estimated impact of the win margin between districts where the redistricting party narrowly lost the pre-redistricting election and districts where it narrowly won that election. Each regression also has demographic and socioeconomic covariates from the pre-redistricting election year as covariates: median age in district, percent of district population that is white, median home value, percent of population that is rural, unemployment rate in the district, and percent of district population that is male. All regressions are performed at the district-decade level for all redistricting states districts for districts in partisan redistricting states where the redistricting party won the pre-redistricting election for the 2000 and 2010 redistricting cycles. Standard errors are clustered at the district level.

Panel A: CRA-Reported Small Business Lending

	Early-cycle			Late-cycle		
	(1)	(2)	(3)	(4)	(5)	(6)
	TotAmt	NumLoans	TotAmtPerLoan	TotAmt	NumLoans	TotAmtPerLoan
RedistrictorLoss	0.00708	-1903.5	0.00000229	-0.101	-6217.2**	0.00000186
	[0.12]	[1.50]	[1.17]	[1.14]	[1.98]	[0.63]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3
R-sq	0.341	0.387	0.121	0.213	0.299	0.057
Observations	442	442	442	440	440	440

Panel B: Federal Financial Assistance

	Early-cycle				Late-cycle			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Contracts	Grants	Loans	DirPayments	Contracts	Grants	Loans	DirPayments
RedistrictorLoss	-164.0	57.68	16.51	10.10	-25.06	-140.1	27.33*	13.46
	[0.74]	[0.41]	[0.85]	[0.26]	[0.04]	[0.68]	[1.72]	[0.32]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE clustering	Dist	Dist	Dist	Dist	Dist	Dist	Dist	Dist
Deg. of polynomial	3	3	3	3	3	3	3	3
R-sq	0.049	0.051	0.057	0.045	0.058	0.032	0.165	0.270
Observations	217	217	217	217	217	217	217	217

Appendix A Redistricting Criteria

According to the National Council of State Legislatures's website (see `http://www.ncsl.org/research/redistricting/redistricting-criteria.aspx`), the following traditional redistricting principles (or criteria) have been adopted by many states:

- Compactness: Having the minimum distance between all the parts of a constituency (a circle, square or a hexagon is the most compact district).
- Contiguity: All parts of a district being connected at some point with the rest of the district.
- Preservation of counties and other political subdivisions: This refers to not crossing county, city, or town, boundaries when drawing districts.
- Preservation of communities of interest: Geographical areas, such as neighborhoods of a city or regions of a state, where the residents have common political interests that do not necessarily coincide with the boundaries of a political subdivision, such as a city or county.
- Preservation of cores of prior districts: This refers to maintaining districts as previously drawn, to the extent possible. This leads to continuity of representation.
- Avoiding pairing incumbents: This refers to avoiding districts that would create contests between incumbents.