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# POLITICAL BORDERS AND BANK LENDING IN POST-CRISIS AMERICA

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**ABSTRACT**

We use spatial discontinuities associated with congressional district borders to identify the effect of political influences on American banks' lending. We show that recipients of the 2008 public capital injection program (TARP) increased mortgage and small business lending by 23% to 60% more in areas located inside their home-representative's district than elsewhere. The impact is stronger if the representative supported the TARP in Congress, was subsequently re-elected, and received more political contributions from the financial industry. Together, these results suggest that political considerations influence credit allocation in a politically mature system like the United States without the formal possibility of political interference in lending decisions, and that this influence is larger if the flows between banks and politicians are reciprocal.

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## 1. Introduction

Do political considerations influence corporate investment? The political economy literature suggests that politicians seek to encourage investments that increase either their constituents' welfare, or their own (Shleifer and Vishny, 1994). In turn, firms may benefit from responding to political influences, if, for instance, this increases their access to the public safety net (Faccio et al., 2006) or their influence on key votes (Mian et al., 2010).

The banking industry provides compelling evidence of political influence on bank lending in developing countries (Khwaja and Mian, 2005; Dinc, 2005; Cole, 2009; Carvalho, 2014) and in advanced economies like Italy (Sapienza, 2004). However, a limitation of this literature is that it draws on government-controlled banks in environments with weak institutions. Thus, it is unclear whether banks also respond to political influences in highly developed political and regulatory systems such as the United States, which provide few explicit possibilities of political interference in lending decisions.

The absence of American evidence to date is significant, given America's exceptional role in the global financial system and the 2008-09 crisis. Influential descriptive accounts of the run-up to the crisis attribute misallocations in US mortgage supply to political distortions (Rajan, 2011; Calomiris & Haber, 2014). Mian et al. (2010) provide evidence that politicians catered to banks' interests when voting on key issues like the 2008 Troubled Assets Relief Program (hereafter "TARP"); this suggests that the crisis might have further increased the relevance of connections between politicians and banks.[1]

This paper proposes a novel strategy to identify political influences on the lending decisions of American banks. Our strategy exploits three key features unique to the United

States. First, the borders of the (435) American congressional districts provide spatial discontinuities between areas that are similar from economic, regulatory and cultural standpoints, yet belong to different political constituencies. Since most banks are active in multiple districts, these borders allow us to explore how the same bank behaves in different constituencies.

Second, American data provides comprehensive coverage of mortgage and (to a lesser extent) small business lending, broken down by banks, years and geography. This rich information set allows us to compare lending behaviors in small geographical units belonging to different congressional districts, while controlling for other (non-political) determinants of lending, like credit demand. Further, the (relative) transparency of the American legislative process provides data for politicians' votes on issues crucial for banks such as the TARP.

Finally, the TARP provides a shock to the political connections between banks and politicians. This unprecedented injection of public capital into US banks did not formally allow Congress to affect participants' lending decisions. However, anecdotal evidence suggests that Congress could exert informal pressures via media appearances, congressional hearings or threats to impose "lending mandates" on recipients. Importantly, evidence also suggests that (some) members of Congress helped banks headquartered within their district to get through the TARP application process (Duchin and Sosyura, 2012). This suggests that informal pressures, or the threat thereof, may be particularly prevalent in markets inside the congressional district of the representative for the area in which the bank is headquartered (the bank's "home district").

We use a 2006-2010 annual panel of bank-county-level mortgage lending growth to examine this idea empirically. Our baseline model compares the mortgage growth of TARP recipients (compared to other banks) after the TARP (as opposed before) in areas located inside the district of their home representative (not elsewhere). We control for other possible determinants of TARP participation, such as credit demand, in three complementary ways. First, we saturate the model with county-year (or county-TARP-year) and bank-home fixed effects. Second, we focus on mortgages originated in neighborhoods (census tracts) located immediately next to an intrastate congressional district border. Third, we use Instrumental Variable (IV) estimators based on banks' pre-crisis regulatory or political connections (Li, 2013; Duchin & Sosyura, 2014).

We find that TARP recipients increased mortgage lending by 23% to 60% more inside the district of their "home" Congress representative; lending in areas outside of the home district stayed flat or fell, whereas lending inside the home districts increased substantially. This "home-district effect" is insensitive and is not explained by public loan guarantees or loan purchases by Government Sponsored Enterprises (GSEs, such as Fannie Mae or Freddie Mac), gerrymandering, or a post-crisis home-district bias. In addition to OLS and IV estimation, we also find similar results using a matched sample of banks based on their pre-crisis propensities to participate in the TARP. The home-district effect also holds in a wholly different dataset of small business lending. In contrast, the effect disappears when we falsify the timing of the TARP or the precise geographical location of congressional district borders.

We then explore how the home-district effect varies with the intensity of the connection between the recipient and its home-district representative. We find that the home-district effect only holds if the representative supported the TARP bill in Congress. The home-district effect is also stronger if the TARP supporter was re-elected in the 2008 elections and thus able to influence recipients. Finally, the effect is stronger if the representative received substantial campaign contributions from the financial industry, a proxy for the representative's sensitivity to banks' special interest in congressional votes (Mian et al. 2010). Together, these results are consistent with political considerations affecting the lending choices of beneficiaries of TARP, in particular if the 'flows' between a bank and a politician are reciprocal.

## 2. Context in the Literature

To our knowledge, our findings provide among the first formal evidence suggestive of political influences on bank lending decisions for the modern American banking sector. Our paper complements Agarwal et al. (2016), who find that banks adopted more lenient foreclosure procedures inside the districts of members of key congressional committees in 2009-2010. Our work explores new mortgage lending rather than bank loan renegotiations. However, the two findings are consistent, as both suggest that political considerations influence bank behavior in a context of heightened political tension around credit supply.[2]

Our results also contribute to several strands of the broader literature on political connections and corporate behavior. One key contribution is to investigate how political influences change with politicians vote on legislation of special interest to firms. A large political economy literature studies whether connections between firms and politicians (e.g., via political contributions) influence voting behaviors (Snyder, 1990; Mian et al. 2010). Less is known about how voting affects connected firms' subsequent behavior. Our findings suggest a form of reciprocity; banks which have benefited from government support increase lending in their home-representative's district, though only if he/she supported the support program in Congress.

Our findings also speak to a literature on political connections and corporate bailouts. Access to the government safety net is a key reason why political connections boost firm value (Faccio et al., 2006). Previous research has investigated how political considerations influence bank bailouts in developing countries (Brown and Dinc, 2005) and industrial countries like Germany where politicians have formal links with banks (Dam and Koetter, 2012; Behn et al. 2014).[3] But less is known about the behavior of corporates after bailout, and thus about the benefits that politicians derive from bailouts. Our findings shed indirect light on this question by showing that publicly supported banks increase lending to the constituents of their home-district representative.

Our findings also help reconcile a related tension between two parts of the TARP literature. Black and Hazelwood (2013) and Duchin and Sosyura (2014) find that TARP recipients increased the riskiness rather than the volume of new lending; Li (2013) find that they used only one third of their capital injection to support new lending. This mixed outcome suggests that the TARP fell short of the intentions of its supporters in Congress. This raises questions around the motives behind congressional activism in facilitating the participation of banks in their district (Duchin and Sosyura, 2012). Our study sheds light on this puzzle by showing that participants do indeed lend more … in their home-district representative's district. While most studies of the TARP discuss the possibility of political influences on participants (e.g. Veronesi and Zingales, 2010), ours is the first to explicitly investigate whether and how these influences affect the allocation of credit.

Our study also adds to the debate concerning the causes of the credit fragmentation that followed the global financial crisis (e.g., Giannetti and Laeven, 2012). We do not explicitly search for evidence of aggregate post-crisis financial fragmentation in the United States.

Nevertheless, our results are consistent with the hypothesis that financial fragmentation can result from "financial protectionism," that is, a distortion of credit flows towards the local economy after large government intervention in the financial sector (Rose and Wieladek, 2014).

Finally, our results also speak to a broader literature in international trade and finance on home bias and the border effect.[4] One argument is that borders affect corporate investment because borders tend to delineate regulatory, fiscal and monetary areas. The problem is that international political borders – and indeed, many intra-national borders – coincide with economic, regulatory or cultural borders. We exploit a set-up where areas which belong to different *political* constituencies still have similar *economic* characteristics within a comparable social and cultural environment.

## 3. The TARP and Political Influences

### The TARP

The Troubled Assets Relief Program (TARP), a plan to purchase illiquid mortgage-backed ("toxic") securities from banks, was submitted to Congress on 20th September 2008 as part of the Emergency Economic Stabilization Act (EESA). The bill initially failed to pass through the House of Representatives on September 29th; after a stock market collapse that day, it was reconsidered and obtained a bipartisan majority less than a week later, on October 3rd. Shortly thereafter, the Treasury announced its intention to use TARP funds primarily to purchase equity shares in banks. Since this Capital Purchase Program (CPP) mobilized the largest share of funds initially earmarked for the TARP, this paper studies the behaviour of CPP recipients.

Following the TARP moniker which prevails in the media and political discussion, we refer to CPP and TARP interchangeably.

As of March 2016, \$209.1 bn of CPP funds had been used to buy preferred equity in 709 different banks or bank-holding-companies. This started with a forced injection of \$125 bn into nine major American banks on 14th October. Participation was then opened to all domestic regulated deposit-takers on a voluntary basis, subject to a three-step application process. First, applications were submitted to and reviewed by the applicant's local regulator (e.g., a state branch of the FDIC). Second, this initial review was screened by the applicant's national regulator (e.g., the FDIC's Washington headquarters). Finally, the Treasury made one last review. Criteria included measures of applicants' financial health such as capitalization, liquidity, and local concentration. Once in the program, recipients were subject to a mandatory 5% annual dividend payable to the Treasury.

<u>The Political Context</u>

The beginnings of the TARP were marked by contentious and widely publicized discussions around the program's perceived failure to boost lending to "Main Street." TARP recipients and congressional supporters were vilified at both Tea Party and Occupy Wall Street rallies.[5] The TARP "stigma" had concrete repercussions; Ng et al. (2011) find that the media coverage of TARP recipients was mostly negative, which depressed their stock returns.

The public authorities had no formal way to appease public outrage by encouraging participants to lend. The Treasury bought non-voting shares (warrants) from banks, and CPP contracts initially did not contain any covenants on lending, nor on the disclosure of usage of funds. Oversight bodies, both public (e.g., the US Government Accounting Office) and private tried to fill this gap by monitoring recipients' behavior. For instance, a 2010 report by the Woodstock Institute pointed to a decline in mortgage lending to underserved communities in major American cities by the largest TARP recipients.[6]

However, Congress retained an important source of leverage on participants via a provision to modify ongoing CPP contracts unilaterally. Congress made use of this provision in 2009 to force participants to obtain the Treasury's approval for executive compensation and CPP repayment plans, motivating the largest American banks to exit the TARP quickly (Bayazitova and Shivdasani, 2011; Wilson and Wu, 2012). Congress also discussed using the clause to impose conditions on lending, prompting some industry observers to "fear that the TARP will become a vehicle by which Congress will impose credit allocation policies on TARP investees."[7]

While Congress renounced imposing formal lending mandates, it retained informal ways to encourage lending. Politicians could single out TARP recipients "guilty" of insufficient lending.[8] For instance, the chairman of the March 2009 hearing "Is TARP working for Main Street?" invited a deli-owner from his district to testify about the refusal of a TARP recipient to roll over his loan. In his opening statement, the chairman acknowledged that such hearings could lead "critics of Congress" to "argue that we are setting out to force banks to lend or encouraging banks to make bad loans."[9] TARP architect Henry Paulson himself acknowledged that "banks rushed to repay because of the associated restrictions on pay levels and the political atmosphere," pointing in particular to "calls for mandatory lending" from Congress in 2008; "as soon as we announced it (…) people were saying, 'Make them lend. Why aren't they lending more?' … And so I think what happened was then some banks were reticent to take the capital."[10]

Anecdotal evidence suggests that TARP recipients were responsive to local political circumstances, and could use evidence of lending in key areas to counter criticisms. Confronted by an Ohio congresswoman about foreclosures in her district, TARP recipient J.P. Morgan communicated that "the company lent more than $16 billion to more than 3.5 million Ohio consumers last year and provided $3.8 billion in loans to more than 70,000 companies in the state."[11] A couple days ahead of his testimony before Congress, the CEO of (TARP recipient) MidSouth Bank announced to local TV channel KPLC that his bank "is extending its popular Town Hall-style meeting series. Two meetings, one on Old Jeanerette Road in New Iberia and another at the bank's headquarters on Versailles Boulevard in Lafayette, have been added to the original 14-meeting series throughout Louisiana and Texas ... 'The series was a big success in that it helped us get the message out that we are looking for qualified borrowers …', Cloutier said."[12] Financial Marketing Solutions, a Tennessee-based consultancy offered advice on "how to communicate acceptance of TARP money", recommending that banks "promote … TARP-driven opportunities to lend more to the community, sparking new growth and economic activity", rather than "defending the media accusation that banks are hoarding the TARP money to cover losses."[13]

Thus anecdotal evidence suggests that TARP recipients were both exposed and potentially responsive to political forces in their lending decisions, especially from TARP supporters in Congress, and that this exposure could differ across constituencies. The "flows" between politicians and recipients were even explicit in some cases, with states like Ohio establishing public-private lending partnerships with some local TARP recipients.[14]

The TARP remained a contentious issue for both representatives and recipients through at least the 2010 mid-term elections.[15] Official efforts to boost the impact of the TARP on lending resulted in a number of follow-up programs designed to increase mortgage refinancing (HAMP), small business credit (SBLF), lending to underserved communities (CDFCI), and other programs.

## 4. Methodology and Data

We are interested in whether political considerations matter for credit decisions of banks which received TARP capital injections. In particular, we seek to determine if these banks lent more *inside* the congressional district of the political representative where the bank is headquartered – the "home district" – than outside.

We choose counties as the baseline unit of a local banking market following much of the literature (e.g., Gilje et al., 2016).[16] Accordingly, our dependent variable of interest is the lending growth for a given bank in a particular county for a single year. One complication is that counties in urban areas often span multiple districts (e.g., in Los Angeles County). Since we are interested in separating home-district lending from other lending, we further split any multi-district county by district.

Figure 1 illustrates this strategy for the state of Oklahoma (OK). The thick (black) lines delineate the five OK districts from the 110$^{th}$ Congress, identified by their number (inside red circles). Thinner (gray) lines delineate the 77 OK counties. The rural Caddo County (southwest of Oklahoma City; population 29,600) belongs entirely to the 3$^{rd}$ district. In contrast, the urban Oklahoma County (around Oklahoma City; population 718,633) spans the 4$^{th}$ and 5$^{th}$ districts. In the latter case, we split a bank's annual lending into loans made in the a) 4$^{th}$ and b) 5$^{th}$ districts.[17] In what follows, we refer to these as "county-years" for convenience.

Empirical model

We employ a difference-in-difference-in-difference strategy; we examine credit growth of TARP recipients (as opposed to non-recipients), after the TARP (as opposed to before), in counties inside a bank's home district (as opposed to outside). Our empirical model is:

$$\Delta Loan_{i,c,t} = \beta_T TARP_{i,t} + \beta_{TH} TARP_{i,t} \cdot HOME_{i,c} + \delta X_{i,t} + \zeta Z_{i,c,t} + \{\eta_{c,t}\} + \{\theta_{i,c}\} + \varepsilon_{i,c,t} \quad (1)$$

where:

- $\Delta Loan_{i,c,t}$ is the first difference in the natural logarithm of aggregate mortgage lending originated by bank i in county c (or county-district c in multiple-district counties), in year t,
- $TARP_{i,t}$ is a dummy variable which is one if i had received CPP capital by time t, and zero otherwise,
- $Home_{i,c}$ is a dummy variable which is one if county c is part of the congressional district in which bank i is headquartered (using districts from the 110th congress), and zero otherwise,[18]
- $\delta$ and $\zeta$ are nuisance coefficients,
- X is a vector of bank controls similar to Duchin & Sosyura (2014), which includes one-year lags of: size (log total assets, hereafter "TA"); tier-1 capital (%TA); cash (%TA); charge-offs (% total loans); non-performing loans (% total loans); repossessed real estate (% TA); deposits (%TA); (log) bank age; return on equity; and exposure to local shocks (average change in Philadelphia Fed yearly state-level economic activity index, weighted by bank's branch presence in a state),

- Z is a vector of borrower controls, which includes weighted average characteristic in a county-year (using loan size as weight) of: loan-to-income ratio; log income; log loan size; dummy for ethnic minority (non-Caucasian); dummy for gender (non-male); median family income in borrower's census tract; and share of ethnic minority people in borrower's census,
- $\{\eta_{c,t}\}$ and $\{\theta_{i,c}\}$ are comprehensive sets of county-year and bank-home (district) fixed effects, respectively, and
- $\varepsilon_{i,c,t}$ is a (hopefully) well-behaved residual, to represent all other determinants of loan growth.

The main coefficient of interest is $\beta_{TH.}$ The parameter captures the differential effect of the TARP for mortgage growth in counties inside the bank's home district. $\beta_T$ measures the effect of TARP on mortgage growth in non-home district counties. We interpret robust indications of a positive significant $\beta_{TH}$ to be evidence of a "home-district effect" associated with political influences.

Estimation

We estimate (1) with four different techniques, clustering the standard errors by bank. The main econometric challenge is that participation in the TARP could be correlated with unobserved characteristics of the participants' home district, besides those linked to political effects. For instance, a bank anticipating high credit demand in its home district could be more prone to apply and to be accepted by the regulator. Alternatively, a bank could apply to the TARP if it anticipates that it would have a comparative advantage in identifying profitable investments in areas close to its headquarters.[19] We address this challenge in three complementary ways: a) fixed effects, b) a sample selection highlighting spatial discontinuities associated with political borders and c) instrumental variables.

We remove considerable unobserved heterogeneity via the two sets of fixed effects in (1). The county-year fixed effects $\{\eta_{c,t}\}$ control for credit demand and economic activity in a given county-year.[20,21] The bank-home fixed effects $\{\theta_{i,c}\}$ control for time-invariant heterogeneity across banks and the way they behave in home and non-home-district counties, for instance because of superior local knowledge.

<u>Spatial Discontinuity</u>

We further attenuate unobservable heterogeneity between home and non-home lenders and counties by measuring $\Delta Loan_{i,c,t}$ using only loans inside a given county c (or district in a multi-district county) that are originated in census tracts immediately adjacent to an intrastate congressional district border. We can do so since our data reports the location of a borrower at the level of the census tract, a small unit designed to contain a socio-economically homogeneous population of about 7,000 individuals.

Out of the 77 Oklahoma counties, 33 contain census tracts adjacent to an intrastate district border (these counties are colored in Figure 1); we thus drop the other 44 counties. Within the remaining 33 counties, we then focus on census tracts next to district borders. The mean/median OK county has 12.9/5 census tracts; rural counties have only few tracts, but urban counties have many. Keeping only "frontier" census tracts allows us to increase the sharpness of discontinuities (particularly in urban areas). This strategy is illustrated in the close-up map of the Oklahoma County in Figure 2. The thick (black) lines again delineate districts, while thin (gray or red) lines separate census tracts. Out of the 227 tracts in Oklahoma County, we only keep loans from the 33 tracts adjacent to district borders (colored in Figure 2).

The goal of zooming onto "frontier" census tracts is to make $HOME_{i,c}$ irrelevant for non-political reasons. A bank headquartered in downtown Oklahoma City (in the 5th OK district) might have superior ability to anticipate credit demand or identify investment opportunities in the average Oklahoma County tract, compared with "outsider" banks or counties. But it is less plausible that this advantage also characterizes Oklahoma County tracts immediately next to the 5th district border, especially by way of comparison with tracts immediately on the other side of the same border. This restriction – combined with fixed effects and instrumental variables (see below) – makes us comfortable with assuming that unobserved home-district characteristics (such as expected local demand or knowledge) cannot explain selection into TARP, and post-TARP lending growth. Since we exclude areas contiguous to any district border which coincides with a state border, we can also rule out differences due to bank regulation and supervision or the broader institutional framework.

Instrumental Variable Strategy

A third way to reduce concerns that participation in the TARP is correlated with post-TARP district-level heterogeneity is to instrument for a bank's TARP participation using pre-crisis bank characteristics. We follow Li (2013) and Duchin and Sosyura (2014) by predicting TARP participation using pre-crisis connections with regulators and influential Congress members. We first estimate a cross-sectional probit model of a bank's i participation in TARP:

$$TARP_i = \lambda Connections_i + \psi X_i + \xi_i \qquad (2)$$

where

- $Connections_i$ is a measure of connections for bank i. We use two measures of connections:

1. *Fed director* is one if an executive director of bank i served on the Federal Reserve Bank's Board of Governors or the board of a regional Fed,
2. *Subcommittee member* is one for bank i if its home-district representative served on the Financial Institutions Sub-Committee of the 111th Congress,

- X are control variables, taken from equation (1), using 2008q3 values,
- $\lambda$ and $\psi$ are coefficients to be estimated for instrument generation, and
- $\xi$ is a well-behaved residual.

After estimating (2), we retrieve the fitted values and interact them with a simple dummy variable (unity post 2007, zero before) to create the time-varying instrumental variable needed for our panel estimation of $\beta_T$. That is, we form $(\hat{\lambda}\ \text{Connections}_i + \hat{\psi}X_i)\cdot I_t$ where $I_t$ is one after 2007 and zero otherwise. We call this instrument $\widehat{\text{TARP}}$. To estimate $\beta_{TH}$ we follow the same procedure, but interact the IV additionally with our home-district dummy variable, which we call $\widehat{\text{TARP}} \cdot \text{Home}$.[22]

Coefficient estimates from the first stages are presented in Appendix Table A1; it is immediately obvious that ours are not weak instruments. A second condition is that $\widehat{\text{TARP}} \cdot \text{Home}$ affect $\Delta\text{Loan}_{i,c,t}$ only via its impact on $\text{TARP}_{i,t}\cdot\text{Home}_{i,c}$ rather than via factors excluded from model (1). In other words, $\widehat{\text{TARP}} \cdot \text{Home}$ should be uncorrelated with $\varepsilon_{i,c,t}$. Duchin and Sosyura (2014) show that committee memberships are largely exogenous to bank lending behavior since they are allocated on the basis of federal political outcomes and rotate regularly. It would take an even stronger assumption for the exclusion restriction to be violated in our case: congressional sub-committee membership or Fed directorship would have to be correlated with (post-TARP) credit demand faced by *TARP recipients only* (since we control for county-year fixed effects) and *only in their home-district*, an implausible assumption in our view.

Propensity Score Matching

A final identification challenge is that participation in TARP could proxy for other bank-level variables. For instance, banks may choose to apply based on their financial strength, and regulators may screen applications based on such factors. Following again Duchin & Sosyura (2014), as an alternative estimator we use a matched sample consisting of a) all TARP recipients and b) their closest non-recipient counterpart. To match recipients and non-recipients, we estimate a probit model of TARP participation similar to (2), but without the political variables (we also add a dummy for whether a bank is part of a Bank-Holding Company or not). We then match each recipient to a non-recipient using predicted participation probabilities.

Data and Sample

We focus on the mortgage market, for two reasons. First, its intrinsic importance, especially for the 2007-09 financial crisis which probably originated in the American housing market. Second, the relevant dataset is of high quality and covers the majority of the American market. All financial institutions must report their mortgage origination activity to the FFIEC under the 1975 Home Mortgage Disclosure Act (HMDA) on a mandatory annual basis, minimizing the selection bias present for instance in small business lending data.[23] Crucially, the dataset provides detailed borrower location information, a key requirement for our identification strategy.

We focus on data for the 2006-2010 period.[24] From the raw dataset, we discard applications received by non-commercial bank lenders (credit unions, thrifts, subprime specialists, etc.), applications with incomplete location or income information, applications in overseas territories, and applications for unusual products (multi-family dwellings and loans guaranteed by the Veterans Administration or the Farm Service Agency). This leaves us with 44.8 million mortgage applications.

For each entry, HMDA reports whether the application was accepted, the identity of the bank, loan-, borrower- and borrower-census-tract characteristics, including loan size, income, race, and sex. The borrower location is reported at the level of the census tract. We use this information to discard loans made in tracts not contiguous to an intrastate congressional district border. This amounts to dropping 53,003 (82.2%) of the 65,310 US census tracts. We used Census Bureau maps for the 110$^{th}$ congress to map census tracts into districts, and a relationship file from Brown University to identify contiguous tracts.[25] Finally, we aggregate the data by bank-county-year (or bank-county-district year in multiple-district counties). The final dataset spans 8,708 county-year and 5,272 bank-home combinations. While zooming onto "frontier" tracts allows us to identify the home-district effect cleanly, it discards a substantial share of the data. Accordingly, we check that our main finding survives when including all US census tracts.

Since the majority of TARP recipients were Bank Holding Companies (BHC) rather than banks, we aggregate lending data to the BHC level, and refer to "banks" for convenience in what follows.[26] We typically do not include data for the nineteen biggest US banks, those which participated in the Fed's 2009 SCAP stress test; since they were forced to participate in the TARP, there is no way to separate the effect of TARP from the effect of being a systemically important bank.[27] We also drop foreign-owned banks ineligible for the TARP, and banks for which we cannot find the end-2007 headquarter in Call Reports.

We add data on TARP recipients taken from the US Treasury's website and merge it with HMDA data using the recipient's name.[28,29] The data indicates the size and timing of a capital injection into a BHC (or bank), as well as the dates of the initiation and completion of repayment, if applicable. We observed 672 TARP recipients in our dataset.[30] 204 banks entered the program in 2008, and another 468 participants in 2009. The FDIC's Call Reports database provides us with bank-year controls and the unique regulatory identifier of a bank and its parent BHC. We aggregate all these controls to the BHC-year level. The BHC-level Call Reports provide us with the BHC's headquarters location.[31] We use the end-2007 bank headquarters location, to rule out strategic relocation after the crisis. We again map the headquarter location into districts using Census Bureau maps.[32] We merge HMDA and Call Reports data using the regulatory identifier provided by HMDA.

Finally, we find data from the House of Congress website on congressional representatives, membership in key committees, and voting behavior for TARP-related roll calls. Data from service on Federal Reserve Bank boards are from Li (2013).[33]

## 5. Main Results

### Benchmark Estimates

Our benchmark results are presented in Table 1, which tabulates estimates from OLS (column 1), both of our instrumental variables (IV, in columns 2-3), and propensity score matching (PSM, in column 4). We tabulate the $\{\beta\}$ coefficients of interest: the effect of TARP on mortgage-loan growth outside the home-district below, and whether this effect differs significantly between areas inside and outside the home-district (other estimates are available online).

The effect of the TARP on mortgage lending outside the home-district, tabulated in the second row, is mixed. In particular, the coefficient tabulated on the bottom row, $\beta_T$, is statistically insignificant and small for OLS and PSM estimators, negative and significant for both IV estimators. In other words, banks that received TARP funds either maintained or cut lending for areas outside their home-district. Still, our main interest is in the top row, which tabulates estimates of the home-district effect, $\beta_{TH}$. In contrast with the small or negative effect of the TARP *outside* the home-district, the parameter for TARP x Home indicates that the effect of TARP *inside* the home district is highly statistically and economically significant. Our OLS estimates indicate that mortgage lending grows (exp(.22)-1≈) 25% more in home districts; the PSM estimate is comparable. IV estimates suggest an even larger effect, indicating that the TARP leads to around (exp(.47)-1≈) 60% more lending in the home district. The fact that IV estimates are larger than the OLS/PSM estimates is consistent with IV measuring the local treatment effect of being a TARP recipient as the result of connections with a particularly powerful local politician or regulator. In contrast, OLS and PSM estimators estimate the average treatment effect of the TARP, including banks which do not "owe" their participation to a powerful politician or former regulator.

### Sensitivity Analysis

Table 2 reports sensitivity analysis for the key coefficient, $\beta_{TH}$, the estimate of the TARP home-district effect. We consider twenty perturbations to our benchmark methodology, and tabulate OLS, IV, and PSM estimates; our default estimates from Table 1 are recorded on top to facilitate comparison.

First, we consider slightly different definitions of our regressand to check the importance of loans prone to federal political distortions. We successively and separately eliminate: loans purchased by Government-Sponsored Enterprises, loans guaranteed by the FHA, and refinance loans. The home-district effect remains positive and statistically significant throughout these perturbations. Next, we drop all loans granted in the 40 most gerrymandered districts, as identified by Mackenzie (2009) based on their geographically abnormal shapes.[34] The results change little; gerrymandered districts are not the source of our finding.[35] Counties in rural areas typically belong to one district, while urban counties can span multiple districts. Since political circumstances may differ across rural and urban areas, we run the baseline regression separately for a) single-district counties and b) multiple-district counties. The home-district effect is significant in both samples, with only minor differences in statistical and economic significance.

Next, we account for banks that exit the TARP during the (2006-10) estimation window. Specifically, we add a separate dummy Exit (along with its interaction with Home), which is unity once the bank has finished repaying TARP money. This perturbation does not change our essential results.

Our baseline set-up assumes that BHCs (or banks in the case of independent banks) are connected to the representative for the district in which the BHC is headquartered. We make this assumption to conform to the facts that a) TARP funds were granted at the BHC level, and b) connections between a BHC and its home-district representative predict participation in the TARP (Duchin & Sosyura, 2012). A (non-mutually exclusive) possibility is that political connections exist between BHCs and representatives of the districts in which *subsidiaries* are headquartered. We thus explore an alternative definition of Home, which is unity not only in a

BHC's home district, but also in any of its subsidiaries' home district. The resulting home-district effect is essentially unchanged.

The next row adds a control Crisis x Home, where Crisis is unity after 2008, and zero otherwise. This allows us to capture any general retrenchment towards the home district, for instance if banks seek to strengthen their connection with the home-district representative in a context of impending regulatory overhaul, irrespective of participation in TARP. The OLS/PSM estimates are unaffected, and the IV estimates are higher. This suggests that the home-district effect operates over and above any crisis-induced extra home bias effect.[36]

To control for changes in the observable quality of applicants across different banks and counties, we then replace bank-county-year *borrower* controls by bank-county-year *applicant* controls. This does not materially change results. Dropping all the bank controls also has little effect on our results.

Another issue to consider is the importance of outliers; we pursue this in two different ways. First, we winsorize all variables at the 1% level. Alternatively, we drop outliers, defined as observations with residuals lying more than two standard deviations from the mean. Our results remain statistically solid through these checks, but the economic magnitude falls by around a fourth in the second perturbation. This suggests that our headline economic estimates are somewhat inflated by observations with extreme lending growth.

Yet another set of checks changes the underlying sample of data. We try both shorter (2006-2009) and longer (2005-2012) spans. This changes little; we obtain similar results, though they are economically less sizable for the larger window.

Our next checks involve exploring alternative sets of banks and geographies. First, we put aside our spatial discontinuity approach and include *all* loans, instead of only those made in tracts contiguous to district borders; this marginally reduces the economic magnitude of our estimates. Then we add back into our sample the largest (nineteen) American banks (those who participated in the 2009 stress test). Our results are economically and statistically somewhat smaller, but they are still significantly different from zero at the 5% level.

We then experiment with standard errors, using state-level clustering. We obtain smaller standard errors, confirming that bank-clustering is more conservative. Finally, we experiment with our fixed effects. First, we replace bank-home fixed effects with bank-county fixed effects; this is of little consequence.[37] Second, we replace county-year with TARP-county-year fixed effects, again finding comparable results. This suggests that the home-district effect cannot be explained by changes in average credit demand faced by TARP recipients in their home-district (the application-level evidence below demonstrates this more directly).

To summarize, we have found that TARP recipients do *not* provide higher mortgage growth on average, but they *do* make substantially more loans to borrowers inside their home-representative's congressional district. Our twenty robustness checks ensure that the home-district effect does not depend delicately on minor features of our methodology.

Placebo Tests

We run two placebo tests. In the first, we falsify the timing of the shock. Specifically, we assume that the TARP recipients receive an equity injection three years before the actual injection date; we then re-estimate the baseline regression for the 2003-2007 period. The home-district coefficient is not statistically significant in this specification for any estimator, suggesting that our main results are not driven by different pre-shock trends across recipients and non-recipients.

In the second placebo test, we falsify the definition of home districts. Specifically, we use borders of 1% “super” Public Use Microdata Areas (“PUMA”s) instead of congressional district borders. Super PUMAs were created by the Census Bureau for the purpose of statistical representation of 2000 US Census data. Super PUMAs, illustrated in Figure 3, are comparable to districts in several respects. They are designed to contain about 400,000 individuals, against 700,000 for districts. Like congressional districts, PUMAs do not cross state lines; also, there can be multiple PUMAs within a county in densely populated urban areas like Oklahoma City. Importantly, however, PUMA borders rarely coincide with district borders (except by accident, e.g., between Caddo and Grady counties southwest of Oklahoma City). Therefore, we can investigate how TARP recipients lend in areas close to PUMA borders as a placebo, since these do not separate different political constituencies, only statistical areas.

To execute the test, we keep only loans made in census tracts located outside of the home district and immediately next to an intrastate PUMA border, unless this border coincides with a congressional district border. We then aggregate mortgages, by bank-county-year, (or bank-county-PUMA-year for multi-PUMA counties). Finally, we run a regression similar to our default, but defining Home as 1 if bank i's 2008 headquarter is located in the PUMA in which the bank headquarters lies, and 0 otherwise.

The results, tabulated in the bottom row of Table 2, show that TARP x Home is not statistically significant in this placebo exercise. This again suggests that our baseline results are driven by borders associated with political constituencies. This non-result also suggests that our baseline result is not driven by a distance effect; that is, TARP recipients do not simply extend loans close to their headquarters. Just like home-district counties, home-PUMA counties are also closer to the bank's headquarter than non-home PUMA counties. So, if distance matters, we would expect to find a significant home-PUMA effect. We do not.

Application-level Evidence

As a final check on our spatial identification strategy, we exploit our mortgage data at the most disaggregated level possible, that of the mortgage application. We estimate the model:

$$\text{Accepted}_{i,a,c,t} = \beta_T \text{TARP}_{i,t} + \beta_{TH} \text{TARP}_{i,t} \cdot \text{HOME}_{i,c} + \delta X_{i,t} + \zeta Z_{i,a,c,t} + \{\eta_{c,t}\} + \{\theta_{i,c}\} + \varepsilon_{i,a,c,t} \quad (3)$$

where:

- $\text{Accepted}_{i,a,c,t}$ is 1 if bank i accepts application a in census tract c and year t, and 0 otherwise,
- Bank controls, borrower controls and the bank-home fixed effect $\theta_{i,c}$ are similar to the baseline model,[38] and
- The location-time fixed effects are discussed below.

The application-level analysis gives a distorted picture of the average home-district effect, since it a) overweighs large banks and areas and b) only captures changes in acceptance rates, not lending volume. If a TARP recipient aggressively solicited applications (in line with anecdotal evidence in section 2) or used its funding advantage to outbid its competitors without increasing risk-taking, this model would not capture it.

However, this approach has two benefits. First, we can estimate the differential effect of the TARP a) within a given neighborhood and year (via census tract-year fixed effects) and b) across a pair of neighborhoods located on either side of an intrastate district border (via census tract pair-year fixed effects). Second, since it models a mortgage supply decision *conditional on* *a given mortgage demand*, this approach removes unobservable individual demand-side effects. Together, these can help us gauge the robustness of our identification strategy.

We saturate the model with fixed effects in two alternative ways. First, we replace the county-year fixed effects (used in the baseline model) with census tract-year fixed effects (henceforth, "within-tract model"). This controls for credit demand and unobservable average borrower quality within a given neighborhood and period. Combined with the alternative dependent variable, this approach is the most conservative way to control for demand factors feasible using HMDA data.

Second, we keep the tract-year fixed effects, but replace the bank-home fixed effects (of the baseline set-up) with bank-census pair fixed effects (henceforth, "across census pairs model"). This allows us to control for unobserved heterogeneity in the way a given bank behaves on average within a pair of two census tracts located on either side of an intrastate district border.[39]

Given the extensive potential number of observations and fixed effects, we drop loans which play no role in our baseline results according to our robustness checks (see Table 2), namely loan purchased by GSEs, loans guaranteed by the FHA and refinance loans.[40] We do not report IV estimates since they often fail to converge, given the high-dimensionality of the fixed effects.[41]

The results in Table 3 indicate a positive and strongly significant coefficient for TARP x Home. This is true for both OLS and PSM estimates, and for both models (within census and across census pairs). For instance, the OLS estimate of the within-tract model (column 1) indicates that, controlling for his/her characteristics, an applicant's chance to be accepted is 4% higher if he/she applies with a TARP recipient, and his/her house is located in the bank's home district. We find comparable estimates in the other three columns. In contrast, the coefficient for TARP is always negative but statistically insignificant, indicating that borrowers are treated insignificantly different outside a TARP bank's home district.

We conclude that at least a portion of the home-district effect can be ascribed to TARP recipients' willingness to accept applications from their home district disproportionately.

External Validity: Small Business Lending

Our focus on the mortgage market is partly motivated by data considerations. The HMDA is the only comprehensive database containing precise information on the geographic location of borrowers. In principle however, there is no reason why the home-district effect should not hold for other markets, such as those for commercial and industrial lending. Banks are the single most important source of credit for small firms, and recipients' small business lending was an important focal point of TARP surveillance bodies such as the Congressional Oversight Panel, and of media discussions around credit supply after the crisis and TARP.[42] This concern may have opened the door to political influences similar to those at work in the case of mortgage lending.

Accordingly, we investigate the home-district effect in the small business lending market. We use a panel of small business loans for the 2006-2010 period collected under the auspices of the Community Reinvestment Act (CRA). The CRA dataset has two main disadvantages compared with the HMDA database. First, although a majority of American banks file CRA reports, participation is voluntary. Second, the publicly-accessible version of CRA aggregates lending to the bank-county-year level, where HMDA reports application-level data. This has two implications for our methodology. First, one cannot explicitly control for borrower characteristics (the vector Z), at least beyond those captured by a county-year fixed effect.

Second, we cannot zoom onto loans within a given county granted in *census tracts* contiguous to district borders. Instead, we focus on *counties* which are contiguous to congressional district borders, within the same state. Note that since urban counties often have multiple districts, this implies that the sample in this regression is biased towards rural areas. With those exceptions, our empirical model is the same as that of our baseline model (1).

Table 4 reports OLS, IV, and PSM results for small business lending. We find qualitatively similar results to the mortgage regression. The coefficient for the home-district effect ranges from 0.21 (PSM) to 0.48 (IV). As expected from the poorer data quality and identification, the precision of the effect is lower than with mortgages. The OLS estimate is significantly different from zero at the 10% level only, though the others are significant at the 5% level. The TARP coefficient is never significantly different from zero in any of the equations. In other words, the TARP has no significantly discernable effect on small business lending except inside the recipients' home-representative congressional district.

These results provide an independent check for our mortgage results; the fact that we find evidence that is similar both qualitatively and quantitatively with a different dataset supports the home-district hypothesis, showing that it is robust to idiosyncrasies associated with the mortgage market and/or HMDA data.

## 6. Linking the Home-District Effect to the Home Representative

We have established the existence of a "home-district" effect; recipients of public capital injections have higher lending growth inside their home-representative's congressional district than outside. That is, the nature of mortgage and small business lending by TARP recipients appears to have been influenced by political considerations. In this section, we provide additional evidence to supplement and strengthen our interpretation.

Congressional Votes and 2008 Elections

One key motive for connected corporates is to influence politicians' votes on key legislation (Snyder, 1990). We thus begin by investigating whether the home-district effect is stronger if the home representative voted in Congress for the Emergency Economic Stabilization Act (EESA), the legislation that created the TARP. The EESA vote divided both Democrats and Republicans; the EESA was voted down 205-228 on Sept 29, 2008 before being voted through 263-171 on Oct 3, 2008. We focus on the latter vote. Our prior is that any link between TARP recipients and their home representative will be stronger if the representative supported the EESA in Congress. A 'yes' vote may have signaled a greater ability or willingness to intervene with the Treasury or bank regulators to help a local bank with access to TARP assistance, or to exert informal pressure on recipients.

Accordingly, we re-estimate baseline model (1) in Table 5 for two sub-samples: those with banks whose home representative a) supported and b) opposed the EESA in Congress. Our main coefficient of interest remains the interaction term TARP x Home, which captures the home-district effect. The first column (at the extreme left) shows that the home-district effect is present for banks whose representatives voted for the TARP. Specifically, the coefficient for TARP x Home is statistically significant and somewhat larger in size than in the baseline regression (0.28 against 0.22). By way of contrast, the home-district effect in the next column to the right is small, negative and insignificantly different from zero for banks whose home representatives opposed the EESA.[43] In other words, political considerations seem to matter only if the representative's vote conformed to the special interests of banks.

Politicians catering to constituent interests may be more prone to influence bank lending, for instance to distressed communities. Thus, we then test whether the home-district effect is also stronger if the representative had voted for the 2008 American Housing Rescue and Foreclosure Prevention Act (AHRFPA), a federal program supporting mortgage renegotiations and GSEs. Unlike the EESA, the AHRFPA targeted constituent interests (those of underwater mortgagors), rather than those of the financial industry (Mian et al. 2010). The third and fourth columns in Table 5 show that TARP x Home is statistically significant and economically comparable in samples containing only banks whose representative voted a) for and b) against AHRFPA. In other words, the home-district effect does not seem to vary with votes on objects directly targeting constituent interests.[44,45]

Together, these results suggest that banks are responsive to political influences when the 'flow' with politicians is reciprocal, as proxied by a vote in favor of a key legislation for banks' special interests. To further support the hypothesis that special interest votes increase political connections, we test whether the home-district effect is stronger if the home representative was not only a TARP supporter, but also if she/he remained in office after the 2008 elections. The 111$^{th}$ Congress elections took place on 4$^{th}$ November, only a month after the passage of the EESA. Re-elected TARP supporters should remain able to influence recipients, and recipients should remain responsive to this pressure (or the threat thereof).

To check, we further split the sample of banks whose home representative supported TARP into two sub-groups, depending on whether the representative was subsequently re-elected or not. Consistently with our intuition, the home-district effect, tabulated in the middle column of Table 5, remains strongly statistically significant for banks whose home representatives were re-elected, and increases in size to 0.31. By way of contrast, the home-district effect is small in both statistical and economic terms for banks whose representative was not re-elected.[46]

Political Contributions and Ideology

The EESA vote correlates with political contributions from the financial industry received by a representative and his/her ideology – the higher contributions and the less conservative, the more likely a 'yes' (Mian et al. 2010).[47]

Accordingly, we test whether the home-district effect grows with political contributions. Politicians receiving more contributions are more prone to cater to banks' special interests in Congress, and may thus represent a potentially more valuable connection for banks in a context of impeding regulatory overhaul. We use Mian et al. (2010)'s database to construct a variable Contributions, which corresponds to the (log) amount received by a 110$^{th}$ Congress representative (up to November 2008) from a Political Action Committee affiliated to the financial industry, as measured by the Center for Responsive Politics. Our main interest is the interaction of Contributions with TARP x Home; this captures whether the home-district effect increases with the home-representative's connection to the financial industry. We re-estimate the augmented baseline model for the sub-sample of representatives which were re-elected in 2008, and thus still able to exert influence. Our result is tabulated in column 5 of Table 5. Consistent with our intuition, we find that the triple interaction term of interest is positive and significant (at the 5% confidence level).[48]

We then test whether the home-district effect is higher for more liberal representatives. These representatives are more prone to support government intervention in private markets (Mian et al., 2010), and thus potentially to exert influence on recipients' lending. We use a representative's DW-Nominates index from Mian et al. (2010)'s database, which increases with conservative ideology. Column 6 in Table 5 shows that the triple interaction between TARP x Home and ideology is not statistically significant.

Together, these results reinforce the interpretation that banks are responsive to political influences (or the threat thereof) when the 'flow' with politicians is reciprocal. Banks connected to representatives who support key special interest legislations because of contributions from the financial industry seem to be responsive. But this is not necessarily the case for banks connected with representatives who support the same object for ideological reasons.

The evidence presented in this section is supplemental, not definitive. Still, it is consistent with the notion of a reciprocal political channel that steers mortgage growth after the TARP towards areas within the district borders of the bank's congressional representative.[49]

## 7. Conclusion

This paper has documented the existence of a "home-district effect"; banks that received capital from the Troubled Asset Relief Program (TARP) lent 23%-60% more in their home-representative's congressional district than elsewhere. To our knowledge, this is among the first evidence that political considerations influence banks' mortgage and small business lending decisions in the United States, despite the maturity of the American political and financial systems and the absence of formal possibilities for politicians to influence bank lending decisions. Of course, we do not know whether our result is general, or an idiosyncratic result of an exceptional financial intervention during a financial crisis. Changes in key regulations may constitute insightful laboratories for further research.

From a policy perspective, our results bear on discussions around newly created cross-border bank funding and resolution arrangements such as the European Stability Mechanism (ESM). Our results suggest that conflicting local interests may steer the impact of bailout programs, even in areas as politically and financially integrated as the United States. This suggests that international mechanisms may find it even more difficult to mute conflicting national interests over bailouts and the associated impact on credit supply.

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**TABLE 1: Baseline Estimates of Home-district Effect: Effect of TARP participation on home-district mortgage lending**

| | (1) OLS | (2) IV (1) | (3) IV (2) | (4) Propensity Score Matching |
|---|---|---|---|---|
| *Stage 0 instrument:* | | Fed director | Subcommittee member | |
| TARP x Home | 0.22** | 0.47** | 0.46** | 0.21** |
| | (0.07) | (0.14) | (0.13) | (0.06) |
| TARP | -0.05 | -0.38** | -0.39** | -0.13 |
| | (0.09) | (0.15) | (0.16) | (0.07) |
| Observations | 93,671 | 93,671 | 93,671 | 44,553 |
| Adjusted $R^2$ | 0.40 | 0.39 | 0.39 | 0.44 |
| Kleibergen-Paap statistic | | 42.07 | 37.23 | |

Coefficients, with standard errors (clustered by bank-holding company) in parentheses; one (two) asterisk(s) indicates significantly different from zero at .05 (.01) level. Regressand is first difference in log mortgage lending for bank-county-year. Columns correspond to different estimators. Annual American data 2006-2010, for all loans given to census tracts adjacent to a within-state congressional district border. All HMDA-reporting commercial banks active as of 2007q4 are included except the 2009 stress test participants. *TARP* is one if bank participates in TARP, zero otherwise; *Home* is one if county is inside congressional district for bank headquarters. Bank-year controls included but not recorded: log total assets; tier-1 capital (%Total Assets); cash (%TA); charge-offs(%TA); non-performing loans(%TA); repossessed real estate(%TA); deposits(%TA); (log) bank age; return on equity; and exposure to local shocks. Bank-county-borrower controls included but not recorded: log income; loan-to-income; log loan size; non-white dummy; non-male dummy; tract percentage of minority population; tract median income. Bank-home and county-year fixed effects included but not recorded.

**TABLE 2: Sensitivity analysis for TARP x Home** ($\beta_{TH}$)

| | OLS | IV (1) | IV (2) | PSM | Obs. |
|---|---|---|---|---|---|
| *Stage 0 instrument:* | | Fed director | Subcom. member | | |
| Default | 0.22** (0.07) | 0.47** (0.14) | 0.46** (0.13) | 0.21** (0.06) | 93,671 |
| Drop GSE-purchased Loans | 0.19** (0.07) | 0.39** (0.13) | 0.37** (0.13) | 0.19** (0.06) | 84,406 |
| Drop FHA-guaranteed Loans | 0.23** (0.08) | 0.49** (0.14) | 0.47** (0.14) | 0.22** (0.07) | 90,931 |
| Drop refinance Loans | 0.25** (0.09) | 0.30* (0.12) | 0.27* (0.12) | 0.20** (0.07) | 65,793 |
| Drop gerrymandered districts | 0.22** (0.08) | 0.49** (0.14) | 0.48** (0.14) | 0.22** (0.07) | 88,388 |
| Single-district counties only | 0.19* (0.08) | 0.51** (0.15) | 0.49** (0.15) | 0.19* (0.08) | 53,959 |
| Multiple-district counties only | 0.25** (0.09) | 0.39** (0.15) | 0.38* (0.15) | 0.23** (0.08) | 39,712 |
| Control for TARP Exit | 0.24** (0.08) | 0.51** (0.14) | 0.49** (0.14) | 0.23** (0.07) | 93,671 |
| Home also subsidiary headquarter | 0.24** (0.08) | 0.53** (0.15) | 0.51** (0.14) | 0.19** (0.06) | 93,671 |
| Control for Post-2008 x Home | 0.24** (0.08) | 0.72** (0.20) | 0.70** (0.20) | 0.23** (0.09) | 93,671 |
| Applicant Controls | 0.21** (0.08) | 0.46** (0.14) | 0.44** (0.13) | 0.21** (0.07) | 93,671 |
| No Bank Controls | 0.28* (0.12) | 0.43** (0.14) | 0.42** (0.13) | 0.30** (0.11) | 93,671 |
| 1% Winsorizing | 0.21** (0.07) | 0.45** (0.13) | 0.43** (0.12) | 0.21** (0.06) | 93,671 |
| Drop >2 Std Err Residuals | 0.16** (0.06) | 0.32** (0.10) | 0.31** (0.10) | 0.15** (0.05) | 89,286 |
| 2006-2009 | 0.27** (0.08) | 0.62** (0.16) | 0.60** (0.15) | 0.24** (0.07) | 73,354 |
| 2005-2012 | 0.15* (0.06) | 0.40** (0.11) | 0.40** (0.11) | 0.14** (0.04) | 130,033 |
| Keep loans in all census tracts | 0.21** (0.07) | 0.52** (0.15) | 0.49** (0.15) | 0.17** (0.06) | 220,192 |

**Table 2 (continued)**

| | | | | | |
|---|---|---|---|---|---|
| Keep all banks | 0.16* (0.08) | 0.26* (0.10) | 0.26* (0.10) | 0.21** (0.06) | 133,629 |
| State clustering | 0.22** (0.04) | 0.47** (0.08) | 0.46** (0.08) | 0.21** (0.04) | 93,671 |
| Bank-County fixed effect | 0.24* (0.10) | 0.47** (0.14) | 0.46** (0.14) | 0.23** (0.08) | 93,671 |
| County-Year-TARP fixed effects | 0.17** (0.06) | 0.75** (0.28) | 0.74** (0.28) | 0.20** (0.07) | 93,671 |
| Placebo (wrong timing) | 0.21 (0.15) | 0.07 (0.10) | 0.03 (0.09) | 0.07 (.12) | 108,767 |
| Placebo (wrong borders) | 0.06 (.11) | 0.41 (.25) | 0.38 (.25) | 0.15 (0.09) | 39,645 |

Coefficients, with standard errors (clustered by bank-holding company) in parentheses; one (two) asterisk(s) indicates significantly different from zero at .05 (.01) level. Regressand is first difference in log mortgage lending for bank-county-year. Columns correspond to estimators; rows correspond to perturbations of benchmark methodology. "Obs." is the number of observations in the OLS regression. American data 2006-2010 unless marked, for all loans given to census tracts adjacent to a within-state congressional district border. All HMDA-reporting commercial banks active as of 2007q4 are included except the 2009 stress test participants. *TARP* is one if bank participates in TARP, zero otherwise; *Home* is one if county is inside congressional district for bank headquarters; *TARP* dummy included but not recorded. Bank-year controls included but not recorded: log total assets; tier-1 capital (%Total Assets); cash (%TA); charge-offs(%TA); non-performing loans (%TA); repossessed real estate(%TA); deposits(%TA); (log) bank age; return on equity; and exposure to local shocks. Bank-county-borrower controls included but not recorded: log income; log loan size; loan-to-income non-white dummy; non-male dummy; tract percentage of minority population; tract median income. Bank-home and county-year fixed effects included but not recorded.

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**TABLE 3: Application-level evidence for the home-district effect of TARP participation on mortgage lending**

| | (1) | (2) | (1) | (2) |
|---|---|---|---|---|
| | OLS | Propensity Score Matching | OLS | Propensity Score Matching |
| Model: | **Within census tracts** | | **Across census pairs** | |
| **TARP x Home** | 0.04** | 0.04** | 0.03** | 0.03** |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| TARP | -0.03 | -0.01 | -0.02 | -0.01 |
| | (0.02) | (0.02) | (0.01) | (0.02) |
| | | | | |
| *Fixed effects*: | | | | |
| Tract-Year | Yes | Yes | Yes | Yes |
| Bank-Home | Yes | Yes | | |
| Bank-Pair | | | Yes | Yes |
| Observations | 767,397 | 439,774 | 1,632,856 | 927,653 |
| Adjusted $R^2$ | 0.23 | 0.26 | 0.33 | 0.33 |

Coefficients, with standard errors (clustered by bank-holding company) in parentheses; one (two) asterisk(s) indicates significantly different from zero at .05 (.01) level. Regressand is one if mortgage application accepted, zero otherwise. Annual American data 2006-2010, for all loan applications received in counties adjacent to a within-state congressional district border, except loans sold to GSEs, loans guaranteed by the FHA and refinancing loans. All HMDA-reporting commercial banks active as of 2007q4 are included except the 2009 stress test participants. *TARP* is one if bank participates in TARP, zero otherwise; *Home* is one if county is inside congressional district for bank headquarters. Bank-year controls included but not recorded: log total assets; tier-1 capital (%Total Assets); cash (%TA); charge-offs(%TA); non-performing loans(%TA); repossessed real estate(%TA); deposits(%TA); (log) bank age; return on equity; and exposure to local shocks. Applicant controls included but not recorded: log income; loan-to-income; log loan size; latino dummy; black dummy; non-male dummy.

**TABLE 4: Home-district effect of TARP participation on small business lending**

| | (1) OLS | (2) IV (1) | (3) IV (2) | (4) Propensity Score Matching |
|---|---|---|---|---|
| *Stage 0 instrument:* | | Fed director | Subcommittee member | |
| TARP x Home | 0.26 | 0.48* | 0.50* | 0.21* |
| | (0.14) | (0.21) | (0.22) | (0.10) |
| TARP | 0.05 | -0.34 | -0.27 | 0.03 |
| | (0.16) | (0.21) | (0.22) | (0.09) |
| Observations | 43,019 | 42,896 | 42,896 | 25,868 |
| Adjusted $R^2$ | 0.25 | 0.03 | 0.03 | 0.28 |

Coefficients, with standard errors (clustered by bank-holding company) in parentheses; one (two) asterisk(s) indicates significantly different from zero at .05 (.01) level. Regressand is small business lending (log) growth. Annual American data 2006-2010, includes all loans given out in counties adjacent to a within-state congressional district border. All HMDA-reporting commercial banks active as of 2007q4 are included except the 2009 stress test participants. *TARP* is one if bank participates in TARP, zero otherwise; *Home* is one if county is inside congressional district for bank headquarters. Bank-year controls included but not recorded: log total assets; tier-1 capital (%Total Assets); cash (%TA); charge-offs(%TA); non-performing loans(%TA); repossessed real estate(%TA); deposits(%TA); (log) bank age; return on equity; and exposure to local shocks. Bank-home and county-year fixed effects included but not recorded.

**TABLE 5: Effect of political variables on the "Home-district" effect**

| | (1) | (2) | (3) | (4) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|---|---|
| | **EESA vote** | | **AHRFPA vote** | | **EESA vote and re-election** | | **Political contributions** | **Ideology** |
| *Included banks:* | | | | | | | | |
| *Representative vote?* | Yes | No | Yes | No | Yes | Yes | | |
| *Rep. re-elected?* | | | | | Yes | No | Yes | Yes |
| TARP x Home | 0.28** | -0.04 | 0.23* | 0.14* | 0.31** | 0.02 | -0.63* | 0.22** |
| | (0.09) | (.08) | (0.11) | (0.07) | (0.13) | (.18) | (0.32) | (0.06) |
| TARP | -0.15 | .20* | -0.01 | -0.02 | -0.18* | 0.04 | 1.06 | -0.02 |
| | (0.08) | (.08) | (0.11) | (0.06) | (0.09) | (.11) | (0.33) | (0.08) |
| TARP x Home x Contributions | | | | | | | 0.07** | |
| | | | | | | | (0.03) | |
| TARP x Contributions | | | | | | | -0.09** | |
| | | | | | | | (0.03) | |
| TARP x Home x Ideology | | | | | | | | -0.22 |
| | | | | | | | | (0.16) |
| TARP x Ideology | | | | | | | | 0.27 |
| | | | | | | | | (0.12) |
| Observations | 59,383 | 34,173 | 61,659 | 28,432 | 52,555 | 6,828 | 82,774 | 82,774 |
| Adjusted $R^2$ | 0.44 | 0.52 | 0.42 | 0.55 | 0.44 | 0.69 | 0.40 | 0.40 |

Coefficients, with standard errors (clustered by bank-holding company) in parentheses; one (two) asterisk(s) indicates significantly different from zero at .05 (.01) level. Regressand is first difference in log mortgage lending for bank-county-year; each column represents a different regression. Annual American data 2006-2010, for all loans given to census tracts adjacent to a within-state congressional district border. All HMDA-reporting commercial banks active as of 2007q4 are included except the 2009 stress test participants. *TARP* is one if bank participates in TARP, zero otherwise; *Home* is one if county is inside congressional district for bank headquarters. *EESA/AHRFPA vote* is 'yes' if a bank's home representative voted in favor of EESA/AHRFPA in Congress (October 2nd/26th July 2008 roll call), and 0 otherwise. *Representative re-elected* is 'yes' if the bank's home representative was re-elected in the 2008 House elections, and 0 otherwise. *Contributions* is log contributions made by financial industry to 110th representative (up to November 2008). Ideology is the representative's DW-Nominate score. Bank-year controls included but not recorded: log total assets; tier-1 capital (%Total Assets); cash (%TA); charge-offs(%TA); non-performing loans(%TA); repossessed real estate(%TA); deposits(%TA); (log) bank age; return on equity; and exposure to local shocks. Bank-county-borrower controls included but not recorded: log income; loan-to-income; log loan size; non-white dummy; non-male dummy; tract percentage of minority population; tract median income. Bank-home and county-year fixed effects included but not recorded.

**FIGURE 1: Oklahoma State county and congressional district borders.** Thick black lines delineate 110th Congress district borders; thin gray lines delineate county borders. Colored counties contain census tracts contiguous to an intrastate congressional district border. Each color corresponds to a different congressional district; circled numbers indicate district identifiers. Oklahoma County is highlighted in red (see Figure 2 below for a detailed view). Authors' illustration based on an original map from the Census Bureau.

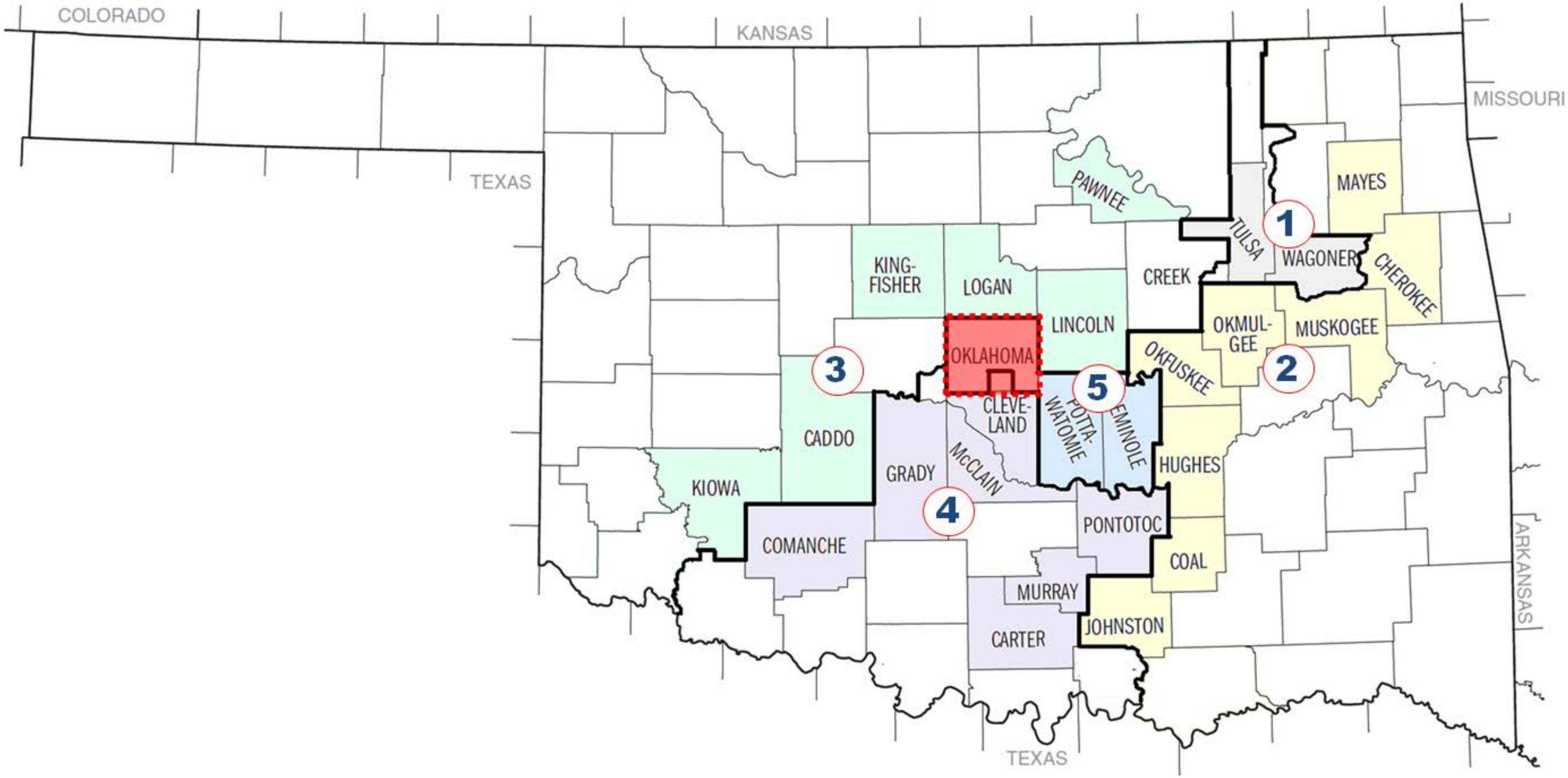

**FIGURE 2: Oklahoma County census tracts.** The top panel shows all the 2000 census tracts of the Oklahoma County. Thin red or gray lines delineate census tract borders. Thick black lines delineate 110$^{th}$ Congress district borders. Colored census tracts are contiguous to an intrastate congressional district border. Each color corresponds to a different district; circled numbers indicate district identifiers. The bottom panel shows the location of Oklahoma County (in red) in the State of Oklahoma. Thin gray lines indicate county borders; thick black lines indicate 110$^{th}$ Congressional district borders. Authors' illustration based on original maps from the Census Bureau.

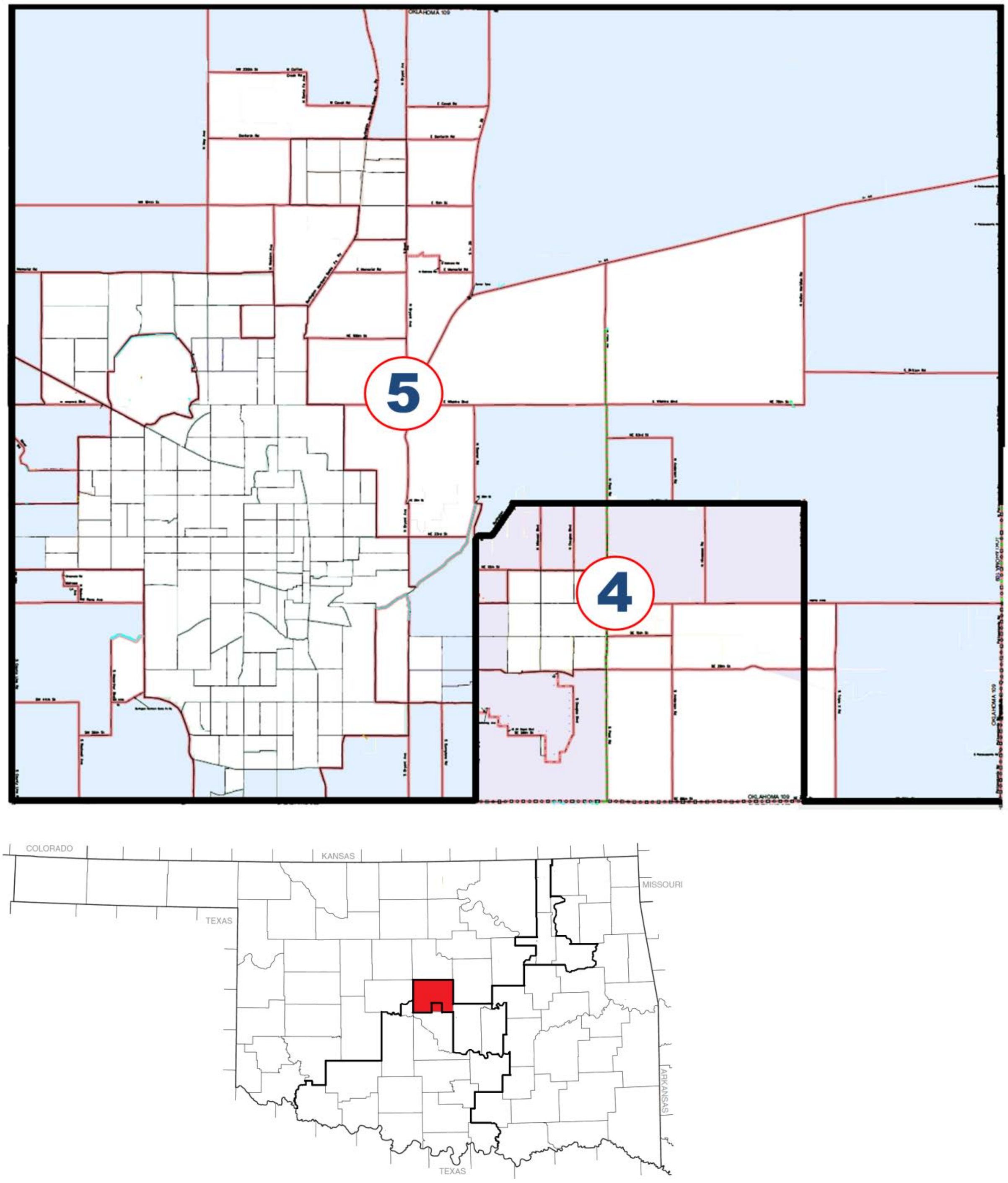

**FIGURE 3: Oklahoma State county, congressional district, and super PUMA borders.** Thin gray lines delineate county borders. Thick black lines delineate 110th Congress district borders. Thick blue lines delineate 1% "super" Public Use Microdata Areas (PUMA) borders as defined by the Census Bureau for the 2000 US Census. Colored counties contain census tracts contiguous to an intrastate super PUMA border. Each color corresponds to a different super PUMA; Census PUMA identifiers are shown inside red circles. Authors' illustration based on an original map from the Census Bureau.

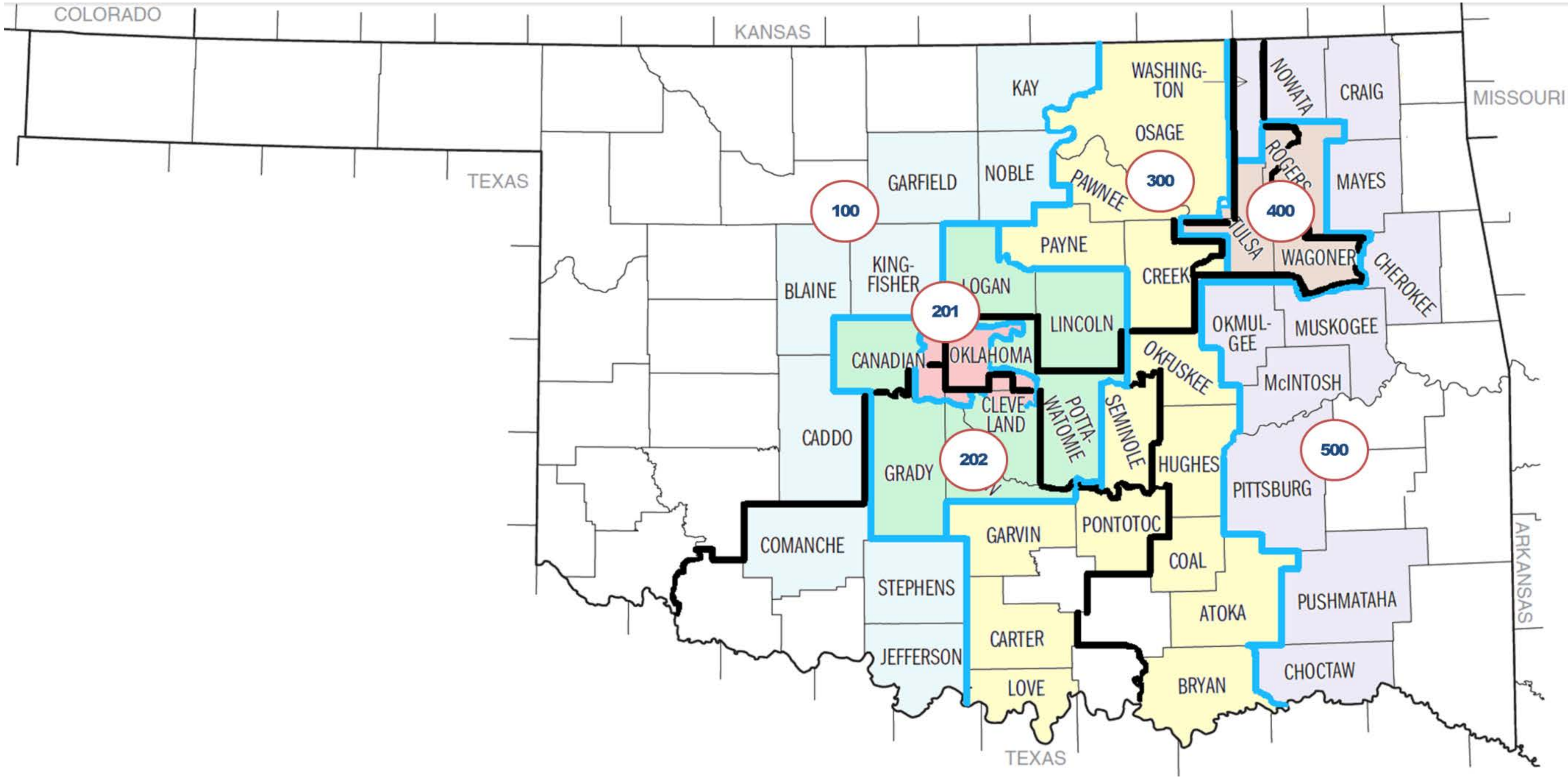

**Table A1: Instrumental Variable estimation**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | **IV (1)** | | | **IV (2)** | |
| *IV Stage:* | Stage 0 | Stage 1 | | Stage 0 | Stage 1 | |
| *Dependent variable:* | I(TARP) | TARP | TARP x Home | I(TARP) | TARP | TARP x Home |
| **Stage 0 instrument**: | | | | | | |
| Fed director | 0.72** | | | | | |
| | (0.18) | | | | | |
| Subcommittee member | | | | 0.30** | | |
| | | | | (0.07) | | |
| **Stage 1 instruments:** | | | | | | |
| $\widehat{TARP}$ (1) | | 0.76** | -0.013 | | | |
| | | (0.17) | (0.08) | | | |
| Home x $\widehat{TARP}$ (1) | | -0.066 | 1.08** | | | |
| | | (0.03) | (0.07) | | | |
| $\widehat{TARP}$ (2) | | | | | 0.74** | -0.01 |
| | | | | | (0.19) | (-1.84) |
| Home x $\widehat{TARP}$ (2) | | | | | -0.09** | 1.08** |
| | | | | | (0.03) | (0.08) |
| Observations | 6251 | 92,790 | 92,790 | 6251 | 92,790 | 92,790 |
| Adjusted R2 | | 0.81 | 0.44 | | 0.799 | 0.432 |
| F-test: $\widehat{TARP}$ + $\widehat{TARP}$ x Home=0 (p-value) | | 0.0 | 0.0 | | 0.0 | 0.0 |

This table reports results of the stage-0 (columns 1 and 4) and stage-1 (columns (2-3 and 5-6) of the three-stage least square model whose stage 2 is presented in Table 1 (columns 2-3). **Stage-0** is a cross-sectional bank-level probit regression of participation in TARP conditional on an exogenous instrument and bank controls (2008 values). *I(TARP)* is 1 if bank *b* is a TARP recipient. *Fed director* is 1 if a member of *b*'s board is or was a member of a Federal Reserve Bank, and 0 otherwise. *Subcommittee member* is 1 if *b*'s home district representative is member of the House sub-committee for financial affairs in 2008, and 0 otherwise. Robust, non-clustered standard errors in parentheses (* p<0.05, ** p<.01). **Stage 1** is the usual first stage of a 2SLS model. It regresses the bank-county-year-level endogenous variables *TARP* and *TARP x Home* against instruments obtained from Stage 0. Specifically, $\widehat{TARP}$ is the fitted value of TARP participation obtained from Stage 0 after 2008, and 0 otherwise. *Home x* $\widehat{TARP}$ is the fitted value of TARP participation obtained from Stage 0 after 2008 and if county *c* is inside b's home congressional district, and 0 otherwise. The sample covers the 2006-2010 period, and includes all loans given out in census tracts adjacent to a within-state congressional district border. All US commercial banks active as of 2008q3 are included. Regressions also include bank-year controls as of 2008q3 (log total assets, tier-1 capital, cash, charge-offs, non-performing loans, repossessed real estate, deposits, (log) bank age, return on equity and exposure to local shocks) and bank-county-borrower controls (log income, non-white dummy, non-male dummy, tract percentage of minority population, tract median income), as well as bank-district and county-year fixed effects. Standard errors clustered by Bank-Holding Company in parentheses (* p<0.05, ** p<.01).

**TABLE A2: Effect of political variables on the "Home-district" effect – Additional evidence**

| | (1)<br>EESA vote | (2)<br>AHRFPA vote | (3)<br>Financial contributions (only re-elected EESA supporters) |
|---|---|---|---|
| TARP x Home | -0.06 | 0.09 | -0.35 |
| | (0.09) | (0.09) | (0.30) |
| TARP | 0.24* | 0.10 | 0.31 |
| | (0.10) | (0.11) | (0.42) |
| TARP x Home x Vote Yes | 0.37* | 0.17 | |
| | (0.16) | (0.17) | |
| TARP x Vote Yes | -0.39 | -0.20 | |
| | (0.14) | (0.14) | |
| TARP x Home x Contributions | | | 0.06* |
| | | | (0.03) |
| TARP x Contributions | | | -0.04 |
| | | | (0.03) |
| Observations | 93,556 | 90,001 | 52,918 |
| Adjusted $R^2$ | 0.40 | 0.40 | 0.45 |

Coefficients, with standard errors (clustered by bank-holding company) in parentheses; one (two) asterisk(s) indicates significantly different from zero at .05 (.01) level. Regressand is first difference in log mortgage lending for bank-county-year; each column represents a different regression. Annual American data 2006-2010, for all loans given to census tracts adjacent to a within-state congressional district border. All HMDA-reporting commercial banks active as of 2007q4 are included except the 2009 stress test participants. *TARP* is one if bank participates in TARP, zero otherwise; *Home* is one if county is inside congressional district for bank headquarters. *EESA/AHRFPA Vote Yes* is 1 if a bank's home representative voted in favor of EESA/AHRFPA in Congress (October 2$^{nd}$/26$^{th}$ July 2008 roll call), and 0 otherwise. *Contributions* is log contributions made by financial industry to 110$^{th}$ representative (up to November 2008). Bank-year controls included but not recorded: log total assets; tier-1 capital (%Total Assets); cash (%TA); charge-offs(%TA); non-performing loans(%TA); repossessed real estate(%TA); deposits(%TA); (log) bank age; return on equity; and exposure to local shocks. Bank-county-borrower controls included but not recorded: log income; loan-to-income; log loan size; non-white dummy; non-male dummy; tract percentage of minority population; tract median income. Bank-home and county-year fixed effects included but not recorded.

## Endnotes

[1] Collusion between politicians and banks in their constituencies are also known to have played a key role behind the historical evolution of the long regionally fragmented American banking system (Kroszner and Strahan, 1999). Rajan and Ramcharan (2011) show that local political circumstances affected access to bank credit in early 20th Century America.

[2] Tahoun and Vasvari (2016) show that members of key US Congress committees obtain bank loans at preferential terms. While this study is also suggestive of political influences on US bank lending decisions, it does not investigate whether these influences bind beyond individual loans to politicians.

[3] More surprisingly, Liu & Ngo (2014) find that US banks failures are less likely in a year before a gubernatorial election.

[4] See e.g. Cooper (2013) for a survey.

[5] The Tea Party 'backlash' was triggered by the launch of the HAMP, a follow-up program to TARP. https://en.wikipedia.org/wiki/Tea_Party_movement

[6] http://www.woodstockinst.org/sites/default/files/attachments/payingmore4_may2010_collaboration_0.pdf

[7] https://www.gpo.gov/fdsys/pkg/CHRG-111hhrg48862/html/CHRG-111hhrg48862.htm

[8] In December 2008, the Chicago firm Republic Windows & Doors shut its doors, laying off 300 employees, after failing to renegotiate an important loan with Bank of America. Speaking at a worker's sit-in, TARP supporter Illinois Senator Dick Durbin said: "We are going to sit down with my friends in the Senate and talk about ways to reach out to the bank, which is receiving funds from the $700 Troubled Assets Relief Program"; http://www.findingdulcinea.com/news/business/2008/December/Factory-Closure-Leads-to-Worker-Sit-in--Calls-for-Bank-of-America-Boycott-.html

[9] The Chairman contended that he was "not interested in encouraging banks to made bad loans" but that "even under [stricter lending] standards there are thousands of businesses across the country that can qualify for loans." https://www.gpo.gov/fdsys/pkg/CHRG-111hhrg48862/html/CHRG-111hhrg48862.htm

[10] https://www.ft.com/content/3379543e-5913-11df-90da-00144feab49a

[11] http://www.wsj.com/articles/SB10001424052748703416204575145743093039972

[12] http://www.kplctv.com/story/9973030/midsouth-bank-extends-town-hall-meeting-schedule

[13] http://www.fms4banks.com/blog/2009/01/23/how-to-communicate-the-acceptance-of-tarp-money/

[14] In May 2009, online media FinLaw reported that "the state of Ohio and [Ohio-based] Huntington Bancshares announced a joint program to lend $1 billion to small businesses over the next three years. Many small businesses have waited for such initiatives from banks like Huntington that received TARP money … As noted by the Columbus Dispatch, the initiative seeks to bolster small business lending, which has lagged despite the infusion of federal money from the Troubled Asset Relief Program into many banks. Huntington received $1.4 billion in TARP money"; http://blogs.findlaw.com/free_enterprise/2009/05/public-private-partnership-in-ohio-to-offer-1-billion-in-small-business-loans.html

[15] New York Times (2010). "Bank Bailout Is Potent Issue for Fall Elections", July 10.

[16] The contours of local banking markets do not generally coincide with congressional district borders. Mortgage lending data for 2005 (sources are discussed in the following section) indicates that the median/average American bank originate mortgages in 5/11.2 congressional districts, respectively. The 10th percentile bank lends in two districts, suggesting that even small banks operate in more than one congressional district. The disconnect between banking markets and districts seems obvious, since district maps are not primarily drawn based on economic or socio-economic homogeneity, but rather on intrinsically political criteria, starting with the need for each district to contain a similar number of people. Further, the allocation of congressional districts to states is changed every decade and district maps are regularly re-drawn; consequently, the shapes of some districts change in the absence of economic changes.

[17] In practice, we use only loans within a given county which are made in census tracts adjacent to an intrastate district border; more on this below.

[18] Note that $Home_{i,c}$ does not appear in level in the model since it is captured by the bank-home fixed effect.

[19] Banks behave differently in markets closer to their headquarters since geographical proximity attenuates informational asymmetries (Petersen and Rajan, 2002), particularly so after downturns (Giannetti and Laeven, 2014; Chavaz, 2016).

[20] For robustness, we check for the possibility that TARP recipients received more unsolicited applications via county-year-TARP fixed effects.

[21] Our home-district effect estimate could be biased upwards if TARP recipients receive more unsolicited applications in their home district. It is unclear whether or not unsolicited prospective borrowers might prefer to apply with a TARP recipient rather than with other lenders. Applicants might perceive TARP banks to have higher lending capacity due to lower funding costs, but applicants might also be wary of the stigma attached to TARP participation, perceiving it as a signal of underlying weakness (Berger and Roman, 2015). Either way, it seems implausible to us that this perception would prevail only a) inside the bank's home district and not elsewhere, and b) for borrowers with higher unobservable quality.

[22] One caveat: we lack information on bank applications to the TARP. Duchin & Sosyura (2012, 2014) have collected application information from banks' annual reports or other communications. However, this information is only available for public banks, which are a minority in our sample. Duchin & Sosyura (2012) find that 80.2% of eligible banks applied, and that pre-existing connections do not correlate with application decision.

[23] The only HMDA reporting exemption is for banks under a size threshold (e.g. $36 mn in 2007) and banks without a branch in a Metropolitan Statistical Area (MSA). This means that the data covers the vast majority of mortgage lending, excepted in a few rural areas. Data can be downloaded from the FFIEC website (https://www.ffiec.gov/hmda/hmdaproducts.htm). The coverage is less comprehensive in the small and medium enterprise (SME) lending dataset stemming from the Community Reinvestment Act (CRA). We use this dataset as a robustness check below.

[24] We use a relatively short window (2006-2010) for the baseline estimation to diminish the problem of banks exiting TARP. To check the importance of the window, we experiment with alternatives later on.

[25] See http://www.s4.brown.edu/us2010/Researcher/Pooling.htm. A limited number of tracts can be attributed to more than one district; we drop loans granted in these tracts.

[26] We map the bank identifier provided in HMDA into a BHC identifier using the Regulatory High Holder identifier provided in bank Call Reports. For banks unaffiliated to a BHC, we aggregate data at the bank level.

[27] Duchin & Sosyura (2014). We check for the importance of this exclusion below by including these banks in a sensitivity check.

[28] See treasury.gov/initiatives/financial-stability/reports/Pages/default.aspx.

[29] We thank Anja Kleymenova for kindly sharing her merging file.

[30] The remaining 47 participants do not show up for a variety of reasons, including inexistent mortgage lending (e.g. Goldman Sachs).

[31] Bank- and BHC-level Call Reports can be downloaded from the Federal Reserve Bank of Chicago website (https://www.chicagofed.org/banking/financial-institution-reports/commercial-bank-data and https://www.chicagofed.org/banking/financial-institution-reports/bhc-data, respectively).

[32] In single-district counties, we map bank headquarters into districts using the Census Bureau relationship file. For banks with headquarters located in counties with multiple districts, we either use the headquarters' zip code and combine it with a ZCTA-District and ZIP-ZCTA relationship file from the Census Bureau, or headquarters' geographical coordinates (from Summary of Deposits) mapped into districts using our own geo-coding routine. We drop the few banks whose headquarters can be attributed to multiple districts.

[33] We thank Lei Li for kindly sharing the data for his instrument.

[34] Gerrymandering could bias our results *upwards* if it increases the sharpness of the discontinuity between politically heterogeneous but economically similar areas. Alternatively, gerrymandering could bias our results *downwards* if abnormal district shapes make it unlikely that a bank only maintains relationships with the representative of its home district.

[35] We have also checked that the home-district effect is not statistically different for the most gerrymandered districts.

[36] If we substitute 2007 for 2008, the IV estimates remain essentially unchanged, but the OLS and PSM estimates fall to 0.16 (0.09) and 0.16 (0.07) respectively.

[37] The same is true of using bank-district fixed effects, as we did in earlier versions of the paper.

[38] A minor difference is that borrower controls appear at the borrower level rather than as a bank-county-year average as in the baseline regression. Specifically, Z includes: loan-to-income, log loan size, log income, and binary variables for borrowers that are black, Latino and non-male. We do not include the tract-level borrower controls used in the baseline regression since they are picked up by the tract-year fixed effects.

[39] Since they vary in size and shape, a majority of census tract can be paired with multiple census tracts on the other side of the border (2.3 on average). Thus, a given observation must be included several times in some cases for this model to be identified; specifically, applications must appear once for each possible pair they can be attributed to. For instance, an application from a tract that can be matched to three different tracts on the other side of the border must be included three times in the dataset.

[40] This also allows us to focus on applications for which banks have the greatest margin of discretion. In particular, GSE loans are typically underwritten automatically using the GSEs' own software and standardized data, leaving little discretion for alternative considerations (such as political ones) to be factored into screening decisions. Refinancing loans also leave less discretion to banks, since the ability to observe the applicant's payment history reduces contracting frictions (Gilje et al., 2016).

[41] Since non-linear models are biased in the presence of high-dimensional fixed effects, we estimate the PSM model via OLS (Puri et al. 2011; Duchin & Sosyura, 2014).

[42] See e.g. Congressional Oversight Panel (2010), "The Small Business Credit Crunch and the Impact of the TARP". Available for download from www.gpo.gov.

[43] Column 1 in appendix Table 5 establishes this result more formally using interaction terms instead of sub-samples.

[44] We establish this non-result more formally using interactions terms in the second column of appendix Table A2.

[45] A 'no' vote for the AHRFPA does not necessarily mean that the representative does not cater to constituent interests altogether. It may also reflect constraints linked to party discipline or ideology (Mian et al. 2010).

[46] Since less than five weeks separated the passage of TARP from the general election, it seems reasonable to rule out reverse causality, whereby more home-district lending by TARP recipients would facilitate a representative's re-election.

[47] In contrast, the EESA vote does not correlate with mortgage defaults in the representative's district.

[48] Column 3 in appendix Table 5 shows that this result also holds (at the 5% level) for a subsample of banks whose representative supported the EESA. In other words, political contributions also matter independently of their effect on the EESA vote.

[49] We can only speculate about the precise nature of this linkage. However, these results do not necessarily require or suggest *active* involvement from representatives. Political tensions around bank lending, coupled with a representative's enhanced potential leverage on banks may be enough to influence bank investment, whether intentionally or not.