Political Borders and Bank Lending

in Post-Crisis America

Matthieu Chavaz and Andrew K. Rose[1]

Updated: July 22, 2018

Abstract

We study political influences on private banks receiving government funds. Using spatial discontinuities associated with congressional district borders, we show that recipient banks of the 2008 TARP program increased mortgage and small business lending by 23-60% more in census tracts located just inside their home-representative's district than just outside; the effect also shows up in higher loan acceptance rates, and mortgages more likely to be impaired or in default. The effect is stronger when the representative voted for TARP, is politically powerful, connected to the financial industry, and when the bank is important in the district. These findings suggest that obtaining public funds subjects firms to political influences, which affects the quantity and quality of corporate investment because of political considerations.

Keywords: empirical; data; panel; fixed; effect; county; district; congress; policy; mortgage.

JEL Classification: F36, G28

Chavaz: Bank of England, Threadneedle Street, London UK EC2R 8AH; +44 (77) 8741-8688; matthieu.chavaz@bankofengland.co.uk; http://matthieuchavaz.wix.com/home

Rose: Haas School of Business, Berkeley CA USA 94720-1900; +1 (510) 642-6609; arose@haas.berkeley.edu; http://faculty.haas.berkeley.edu/arose

---

Private firms can receive substantial public funds in the form of procurements, subsidies or outright bailouts. The role of political connections in the allocation of these funds has attracted substantial attention.[2] This paper explores a related but different question: do political influences affect the behavior of private firms that benefit from public funds? Government funding can be critical for firms and the wider economy, particularly financial bailouts during crises. Because they mobilize substantial taxpayer money, these programs generate substantial political and media controversy. Yet, there is little evidence on how the political forces that create the programs consequently affect the economic behavior of recipients of the same programs. The objective of this paper is to show how a substantive public intervention in an important market changes the investment decisions of beneficiary firms across political districts, and, in particular, how this response likely reflects political influences.

Public funding programs provide scope to politicians to influence the availability and terms of funds to firms they are connected to. Our main hypothesis is that beneficiary firms, in return, increase investment in these politicians' constituencies. We exploit a large American government intervention, the injection of $209 billion capital into 709 banks under the 2008 Troubled Asset Relief Program (TARP). This program was exceptionally large and played a prominent role in the financial crisis. It also illustrates the impact of one particular type of political connection, based on geography. Specifically, Congressional representatives helped applicant banks headquartered in their constituencies gain access to the program. This leads to our question: did beneficiaries, in return, increase lending in politicians' constituencies? Since politicians tend to help firms located in their constituency, firms have an incentive to be responsive to local politicians, particularly in a context of regulatory overhaul.[3,4] We find a

---

[2] Among others, Cohen et al. (2011) and Schoenherr (2017) shows evidence of political influences on the allocation of public procurements, while Duchin and Sosyura (2012) document political influences on government bailouts. Anecdotal evidence of political interference in TARP allocation is available for instance from www.wsj.com/articles/SB123258284337504295.

[3] For instance, Faccio and Parsley (2009) and Kostovertsky (2015). Politicians might help firms located inside their constituency because of common interests in local economic activity, and personal or financial ties (e.g., Amore and Bennedsen 2013).

1

strong positive answer: TARP recipients' mortgage and small business lending growth increased by 23% to 60% more inside the district of their "home" Congress representative. Lending in areas immediately outside of the home district fell, whereas lending inside the home districts remained flat or increased.

The American political and banking system provides an ideal empirical laboratory to test whether banks respond to local political influences. The borders of the (435) American congressional districts create spatial discontinuities between small contiguous areas which are subject to the same political and regulatory circumstances, but part of different political constituencies. US regulators provide comprehensive mortgage and (to a lesser extent) small business data broken down by firm, time, and space, including detailed borrower characteristics. Since most banks are active in multiple districts, we can compare bank lending in small geographical units belonging to different congressional districts, while controlling for other (non-political) determinants of lending, like credit demand or neighborhood affluence. Finally, the relative transparency of the American legislative process provides data for politicians' votes on crucial issues like the TARP, and political contributions from the financial industry.

Our main test uses a 2006-2010 annual bank-county-level mortgage lending growth panel collected from Home Mortgage Disclosure Act (HMDA) data in a difference-in-difference-in-difference set-up. Given our hypothesis, we ask whether the mortgage growth of TARP recipients (compared to other banks) in a county and year is higher after the TARP (as opposed to before) if the area lies in the district of the recipient's home representative (not elsewhere).

---

[4] The case of Huntington Bank helps to illustrate our hypothesis. The bank is based in Columbus Ohio, but operated in several American states. In 2008, it received $1.4 billion in TARP funds, along with other Ohio lenders. The *Wall Street Journal* reported that this followed the intervention of an Ohio "congressional delegation". Six months later, Huntington announced that it would lend $1 billion to small businesses *in Ohio* through a partnership with the state government. *Free Enterprise* noted that "Many small businesses have waited for such initiatives from banks like Huntington that received TARP money". blogs.findlaw.com/free_enterprise/2009/05/public-private-partnership-in-ohio-to-offer-1-billion-in-small-business-loans.html

The main challenge is that participation in TARP could be correlated with other relevant characteristics of participants' home districts, besides those associated with political considerations. For instance, participants might receive more credit demand or may be more reluctant to cut lending in areas close to their headquarters ("home bias"). Our baseline approach mitigates these concerns in two complementary ways. First, we saturate the model with county-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.[5]

We pursue several strategies to rule out non-political explanations for this "home-district effect". Our effect is insensitive to dropping heavily gerrymandered districts. Our key result is also robust to controlling for a "home-bias" of TARP participants towards areas geographically closer to their headquarters, in which they may possess superior information. Controlling explicitly for the possibility that TARP recipients might receive more credit demand *in their home district* through county-year-TARP fixed effects does not change our results either.

We then strengthen our interpretation by investigating the variation of our result across time, policy beneficiaries, and politicians; we also examine its aggregate impact for district lending conditions and electoral outcomes. First, we show that the *timing* of the home-district effect coincides with periods during which politicians have the greatest latitude to interfere in the allocation process, namely before the Treasury stepped in to reduce lobbying. When a participant exited the TARP or its home representative was no longer in office, the home-district reversed: TARP recipients decreased lending inside their home district, and increased it elsewhere.

Second, the home-district effect is higher for *banks* where political interference is more likely (banks eligible during the first round of TARP disbursement) or more valuable (riskier

---

[5] In online appendices, we also explore instrumental variable estimators based on banks' pre-crisis regulatory or political connections, as well as propensity score matching estimators based on banks' pre-crisis probability to enter TARP.

banks less likely to be accepted by regulators). Third, the home-district effect is concentrated among *politicians* who might be more willing or able to help banks, either in the specific context of TARP applications or in the broader context of regulatory overhaul. The effect only holds if the representative supported the (tightly contested) TARP bill in Congress, and is stronger if he/she was a member of a House committee used for TARP-related legislation. The effect also increases with the amount of pre-crisis campaign contributions the politician received from the financial industry, as well as with the importance of the bank in the representative's district. Together, our results indicate that banks are particularly responsive to political influences (or the threat thereof) when the connection with the home representative has reciprocal benefits (whether potential or realized). This leads us to conclude that the home-district effect is political.

Aggregating mortgage lending data by districts and banks, we then show that districts with a larger presence of TARP participants headquartered locally see an increase in both *total* mortgage originations and application acceptance rates after TARP. Also, improved lending conditions are empirically associated with a better performance by incumbent candidates in the 2010 midterm House of Representatives election. While aggregation reduces the precision of our identification, these results suggest that the home-district effect matters for total credit supply, and that credit supply matter for electoral outcomes.

Finally, we explore whether the home-district effect on mortgage quantities is also associated with a reduction in the quality of mortgages. First, using disaggregated HMDA data, we find that an individual mortgage application was more likely to be accepted if it was submitted to a TARP bank and the borrower was located inside the bank's home-representative district. In other words, TARP lenders seemed to adopt looser underwriting standards than non-TARP competitors in their home district. Second, using mortgage-performance data from Freddie Mac, we find that districts and quarterly cohorts for which home-district TARP lenders

had a larger mortgage-market share also experienced higher incidence of non-performing or defaulted mortgages. Succinctly, TARP recipients issued a higher quantity of lower quality mortgages in a politically relevant way.

The key contribution of these findings is to document evidence of political forces that affect investment decisions by private firms that are beneficiaries of government funds. This provides a "flip side" to the evidence that firms derive important benefits from political connections, like support for relevant legislation, procurements, and bailouts.[6] We also show that politics influence bank lending even absent the explicit links between politicians and banks observed in emerging markets or some European countries. This complements recent British and American evidence (Agarwal et al., 2016, 2018; Akey et al., 2016; Rose and Wieladek, 2014). Our findings also shed new light on the mixed evidence the effect of TARP on lending (Black and Hazelwood, 2013; Duchin and Sosyura, 2014; Li, 2013). While many studies discuss the possibility of political influences on TARP participants (e.g. Veronesi and Zingales, 2010), ours is the first to explicitly investigate whether and how these influences affect credit supply.

1. The TARP and Political Influences

1.1. 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

---

[6] See among other Mian et al. (2012) for evidence on legislative outcomes; Schoenherr (2017) for evidence on procurements; and Duchin and Sosyura, (2012) for evidence on bailouts.

equity shares in banks. Since this Capital Purchase Program (CPP) mobilized the largest share of funds initially earmarked for the TARP, 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 banks or bank-holding-companies. This started with a forced injection of $125 bn into nine major banks on 14th October. Participation was then opened to all domestic regulated banks on a voluntary basis, subject to a three-step application process. Applications were successively reviewed by: a) the applicant's local regulator (e.g. a state branch of the FDIC), b) the national regulator (e.g., the FDIC's Washington headquarters), and c) the Treasury. 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.

1.2.   Sources of Political Influences

From its inception through at least the 2010 mid-term elections, the TARP generated contentious discussions around the program's perceived failure to boost lending to "Main Street".[7] Recipients and congressional supporters were vilified at both Tea Party and Occupy Wall Street rallies. The public authorities had no formal way to appease such outrage because 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.

This said, Congress retained a key source of leverage via a provision to modify ongoing CPP contracts unilaterally *even once the funds had been distributed*. Congress discussed imposing conditions on lending on that basis, prompting some industry observers to "fear that the TARP will become a vehicle by which Congress will impose credit allocation policies on

---

[7] www.nytimes.com/2010/07/11/us/politics/11tarp.html.

TARP investees."[8] While Congress renounced imposing formal lending mandates, it retained informal ways to encourage lending. Politicians could single out TARP recipients "guilty" of insufficient lending. TARP architect Henry Paulson 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… And so I think what happened was then some banks were reticent to take the capital."[9] Anecdotal evidence suggests that recipients were responsive to political circumstances, and used evidence of lending in key areas to counter criticisms.

This anecdotal evidence suggests that TARP recipients were 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. Still, the point of this research is to provide rigorous statistical evidence of this effect; we now turn to that task.

2. 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 to delineate local banking markets, following much of the literature.[10] 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

---

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

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

[10] See for instance Gilje et al. (2016). The contours of local banking markets do not generally coincide with congressional district borders.

home-district lending from other lending, we split any multi-district county into districts. Figure 1 illustrates this strategy for the state of Oklahoma (OK). The thick (black) lines delineate the five OK congressional districts in the 110th 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) lies entirely within the 3rd district. In contrast, the urban Oklahoma County (around Oklahoma City, population 718,633) spans the 4th and 5th districts. In the latter case, we split a bank's annual lending into loans made in the a) 4th and b) 5th districts. In what follows, we refer to these as "county-years" for convenience.

2.1. *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:

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

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 for 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,
- δ and ζ are vectors of nuisance coefficients,
- X is a vector of bank controls similar to Duchin and Sosyura (2014), which includes one-year lags of: size (log total assets, hereafter "TA"); tier-1 capital (%TA); cash (%TA); repossessed

real estate (% TA); deposits (%TA); charge-offs (% total loans); non-performing loans (% total loans); (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); and median family income in borrower's census tract,

- $\{\eta_{c,t}\}$ 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, β_{TH}, captures the differential effect of the TARP for mortgage growth in counties inside the bank's home district. β_T measures the effect of TARP on mortgage growth in non-home district counties. We interpret robust indications of a positive significant β_{TH} to be evidence of a "home-district effect" associated with political influences.

2.2. *Estimation*

We estimate (1) with OLS, clustering the standard errors by bank. The main econometric challenge is that participation in the TARP could be correlated with post-TARP 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 both to apply to the TARP, and to be accepted by the regulator. Alternatively, TARP banks could be financially weaker, and may choose to cut lending first in distant areas while maintaining lending in areas close to its headquarters where it has a comparative advantage in identifying profitable

investments ("home bias").[11] We address this challenge in two complementary ways: a) fixed effects and b) a sample selection highlighting spatial discontinuities associated with borders that are purely political. (We pursue further strategies in an online appendix.)

Most straightforward are the two sets of fixed effects.[12] County-year fixed effects $\{\eta_{c,t}\}$ control for credit demand and economic activity in a given county-year. The bank-home fixed effects $\{\theta_{i,c}\}$ control for time-invariant heterogeneity across banks and the way they behave across counties, for instance because of local knowledge.

2.3. Spatial Discontinuity

We further attenuate unobservable heterogeneity between home and non-home lenders and counties by measuring $\Delta\text{Loan}_{i,c,t}$ using only loans inside a county (or district) that are originated in areas *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.

Of 77 Oklahoma counties, only 33 contain census tracts adjacent to an intrastate district border (these counties are shaded in Figure 1); we 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, while 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) lines separate census tracts. Out of the 227 tracts in Oklahoma County, we only keep loans from the 33 census tracts adjacent to district borders (shaded in Figure 2).

---

[11] 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).

[12] We use mortgage growth rather than its level to avoid adding a third set of fixed effects to the analysis.

The goal of zooming onto "frontier" tracts is to make $Home_{i,c}$ irrelevant for non-political reasons, like home bias. A bank headquartered in downtown Oklahoma City (the 5th OK district) might have superior information on lending 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 compared with tracts immediately on the other side of the same border. This restriction – combined with fixed effects – reduces the danger of our results being tainted by bank home-bias, and makes us comfortable assuming that unobserved home-district characteristics (such as expected local demand or knowledge) cannot explain either selection into TARP, or post-TARP lending growth. Since we exclude areas contiguous to any district border which coincides with a state border, we also attenuate differences due to bank regulation and supervision or the broader institutional framework. The drawback of the strategy is that it removes much of the data. In the online appendix, we show that our approach does not seem to create selection bias. We also show below that we obtain similar results using all census tracts; our discontinuity approach thus adds safety to our identification but is not strictly necessary for our results.

2.4. Data and Sample

We focus on the mortgage market, for two reasons.[13] 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 are required to 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. Crucially, the dataset provides detailed borrower location information, a key requirement for our strategy.

---

[13] We explore small business lending in an online appendix.

We focus on data for the 2006-2010 period. From the raw dataset, we discard applications reported by non-banks (credit unions, subprime specialists, etc.) or in overseas territories, applications with incomplete location or income information, 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. We used Census Bureau maps for the 110th congress to map census tracts into districts, and a relationship file from Brown University to identify contiguous tracts. 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.

Since the majority of TARP recipients were bank-holding companies (BHCs) rather than banks, we aggregate lending data to the BHC level, and refer to "banks" for convenience in what follows. 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. 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. The data indicates the size and timing of capital injections, as well as the dates of the initiation and completion of repayment, if applicable. 672 distinct firms (mostly banks) participated in TARP; 204 entered the program in 2008, and

another 468 in 2009. We observe 444 of the 672 participants in our full bank-county-year mortgage lending. 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. We use the end-2007 bank headquarters location, to rule out strategic relocation after the crisis, and again map headquarter location into districts using Census Bureau maps. 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).[14]

2.5. *Summary Statistics and Parallel Trends Assumption*

Table 1 provides descriptive statistics. (Log) mortgage lending grows by 1.5% for the average bank-county-year. 22% of all observations cover banks in their home district; 21% capture banks in the TARP program, around 10% of which are home-district lending. In the online appendix we show that average home and non-home lending trends follow roughly comparable trends for participants and other banks before TARP. After TARP, participants cut non-home lending whereas home lending remains comparable for the two groups. This effect is reversed in 2010, when most participants have exited the program. This informal result proves to be consistent with our more rigorous statistical work; we now turn to the latter.

---

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

3. Main Results

3.1. *Benchmark Estimates*

Our benchmark OLS result is presented in the first column of Table 2. We tabulate the $\{\beta\}$ coefficients of interest: the effect of TARP on mortgage-loan growth outside the home-district, and whether this effect differs significantly between areas just 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, β_T, is statistically insignificant and small. In other words, banks that received TARP funds maintained lending for areas outside their home-district. Still, our main interest is in the top row, which tabulates estimates of the home-district effect, $\beta_{TH,}$ on loan origination. 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 significant, both statistically and economically. Our estimate indicates that mortgage lending grows $(\exp(.22)\text{-}1\approx)$ 25% more in home districts.

The average home-district lending growth of TARP participants can be gauged by adding the parameter estimates for TARP and TARP x Home. This sum is 0.18; the t-test at the bottom of Table 1 indicate that this sum is significantly different from zero at any reasonable confidence level. This suggests that TARP participants increased net lending inside their home district.

3.2. *Sensitivity Analysis and Alternative Explanations*

Columns 2 to 6 in Table 2 report selected robustness checks for our key finding (more checks are reported in the online appendix). First, we include *all* loans, instead of only those made in tracts contiguous to district borders; this marginally reduces the economic magnitude of our estimates. We then add back the largest (nineteen) American banks. Our results are

economically and statistically somewhat smaller, but still significantly different from zero at the 5% level; our results are not driven by "mega-banks".

Next, we address three alternative explanations for the home-district effect: gerrymandering, credit demand, and home bias. We first drop all loans granted in the 40 most gerrymandered districts based on their geographically abnormal shapes (Mackenzie, 2009). The results change little. 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 presented below demonstrates this even more directly).

TARP banks might be weaker and thus more prone to cut lending. In turn, weak banks might want to cut lending first in those markets in which they do not have superior information, like distant counties (Landier et al., 2007; Giannetti and Laeven, 2012) or quantitatively less important counties. This TARP-specific home bias could explain our results if these "core" counties also lie within banks' home district. We explore this issue in two ways. First, we add a control "Close to Headquarter" – one if the distance between a given county and the county where the bank is headquartered is smaller or equal to the 5th percentile for a given bank-year, and zero otherwise – and its interaction with TARP. Our key conclusion is unchanged. Second, we add a dummy "Home County" – unity in the county where a bank is headquartered, and zero elsewhere. Unlike congressional districts, counties do not delineate areas with obvious differential exposure to federal politics. Thus, TARP recipients should not face any political incentive to increase lending in their home county, other than because of its overlap with the home district. But if our results are driven by distance rather than by political factors, adding this control should reduce the size of the home-district effect. The results suggest otherwise.

Finally, we add a placebo test, in which we falsify the timing of the shock. We assume that the TARP recipients receive an equity injection three years before the actual date; we then estimate the baseline regression for the 2003-2007 period. The home-district coefficient is economically and statistically insignificant for all estimators; this suggests that our main result is not driven by different pre-shock trends across recipients and non-recipients.

4. Variation across time, banks, and politicians, and aggregate effects

We now provide additional evidence to strengthen our interpretation that the home-district effect reflects political influences on mortgage lending decisions by TARP recipients.

The political economy literature and anecdotal evidence surveyed above suggest two possible reasons for our findings. First, recipients might want to (or be compelled to) *reciprocate* a "favor" provided by their home representative. This can include direct help in entering (or exiting) the program, or other congressional actions beneficial to a participant. Second, recipients might seek to *preempt* political interference. Access to public funds attracts political and media scrutiny on banks. The evidence above suggests that a) politicians could use platforms like Congressional hearings to pressure participants, and b) participants could use evidence of lending in key areas to counter (or pre-empt) criticisms on their lending behavior. These mechanisms are neither observable, nor mutually exclusive; we thus do not attempt to disentangle them. Instead, we explore predictions consistent with either channel.

Our chief interest is to test whether the home-district effect is stronger in a) periods, b) banks and c) politicians where the scope or motive for political intervention and/or the incentive of the bank to be responsive to political influences (or the threat thereof) is higher. Panel C in Table 1 reports summary statistics for the proxies used to test these predictions.

4.1.   *Timing*

We start by investigating whether the timing of the home-district effect coincides with periods during which politicians have the greatest scope to intervene, and banks are most liable or vulnerable to interference. First, political influences should be stronger around the capital injection time. We thus create three dummies $TARP_t$, $TARP_{t+1}$ and $TARP_{t+2}$ – unity during the year the bank enters the TARP, one year after, and two years after, and zero otherwise – and interact them with Home. We find that $TARP_t$ x Home and $TARP_{t+1}$ x Home are significant and of comparable economic size. (The sum of the two coefficients is reported in column 1 of Table 3). But $TARP_{t+2}$ x Home is insignificant. Consistent with intuition, the home-district effect is thus concentrated around the date of the capital injection.

Second, political influences should be stronger as long as the bank holds TARP funds, and its home representative at the start of the program remains in office. We thus create a dummy "Exit" (and its interaction with Home) – unity if a bank has repaid TARP funds, or its 2008 home representative is no longer in office after the November 2008 elections, and zero otherwise. The results tabulated in column 2 of Table 3 show that the home-district effect is reversed once the recipient's relationship ceases (either because the bank leaves the TARP or there is a different home representative for the bank). TARP x Home x Exit is negative and significant; this suggests that TARP recipients *decrease* home-district credit growth when this relationship ends.

4.2.   *Bank characteristics*

We now explore whether the effect is also stronger for those banks with more scope for such influence, or a bigger incentive to respond to it. Anecdotal evidence suggests that applicant banks helped by politicians tended to access TARP during the first round of the distribution of CCP funds. The *Wall Street Journal* first reports about political interference on

22 January 2009; five days later, Treasury Secretary Geithner announced rules to prevent lobbying on behalf of applicants. This should have restricted the scope for political interference during the next application rounds. To explore this timing while mitigating endogeneity, we exploit two aspects of bank organizational structure. First, the initial application round was opened only to public banks. One reason is that many privately held banks are organized as "Corporation S", a type of firms which can only have one class of shareholders. The Treasury was thus initially unable to purchase preferred stock in these banks as foreseen by the terms of the TARP. We thus construct a dummy "Eligible for 1st round" – unity if a bank is a) publicly traded and b) not a Corporation S, and zero otherwise. The results tabulated in column 3 of Table 3 show that the interaction of Eligible and TARP x Home is positive and significant. In other words, the home-district effect is higher for potential first-round recipients, in line with our intuition.

Second, the TARP was not intended as a bailout of unhealthy banks. Riskier applicants were less likely to be accepted (Bayazitova and Shivdasani (2012), Duchin and Sosyura (2012)). The finding common to both studies is that banks with greater funding risk were less likely to be accepted. We thus create a proxy "Deposit-to-asset ratio" measured as the share of bank total assets in the form of deposits in 2008q3. The results in column 4 show that the interaction of Deposit-to-asset ratio and TARP x Home is negative and significant. That is, participants less likely to be accepted were more subject or responsive to political influences or the threat thereof.

4.3.   Politician characteristics

Next, we explore whether the effect also changes with proxies for politicians *willingness* and *ability* to help banks as part of TARP, or more generally. We begin by investigating the role of TARP votes in Congress. Most representatives featured in the anecdotal evidence above were TARP supporters. We assume that TARP supporters found it easier both to intervene for

applicants and to pressure participants to lend. The TARP vote might also indicate a broader inclination to help banks, as it was largely determined by a representative's proximity to the financial industry (Mian et al., 2010). Finally, the TARP vote was tight; individual politicians seeking to help firms could make a difference (Cohen and Malloy, 2014).

We thus add a control for "TARP supporter" – one for banks whose home representative supported the TARP, and zero otherwise – and its interaction with TARP x Home. Column 5 in Table 3 shows that the home-district effect increases significantly with a 'yes' vote: the home-district effect is 0.34 (≈0.38-0.04) for 'yes'-vote banks, against -0.04 for 'no'-vote banks. Political considerations influenced a bank's lending only if the vote of its home representative aligned with the bank's interests.

Second, we exploit the fact that politicians receiving more contributions from the financial industry were more prone to cater to banks' special interests in Congress, and thus represented a potentially more valuable connection in a context of crisis and regulatory overhaul. We use Mian et al. (2010)'s database to construct a variable "Financial contributions" (log amount received by a bank's representative up to November 2008 from a Political Action Committee affiliated to the financial industry, as measured by the Center for Responsive Politics) and interact it with TARP x Home. Consistent with our intuition, column 6 of Table 3 shows that the triple interaction term is positive and significant at the 5% confidence level.

Third, a politician's willingness to help a bank should also increase with the importance of the bank for its district. Cohen et al. (2013) show that politicians are more prone to cater to a firm's interests if it can have a sizable impact on economic activity in his/her district. We thus create a variable "District market share" which is the bank's share of mortgage origination in its home district before TARP (2006-2007). The results in column 7 of Table 3 confirm our prior: the home-district effect increases significantly with the home-district market share. Specifically,

the estimate for the TARP x Home x District market share interaction (2.03) suggests that the home-district effect is 0.23 for a bank with an average market share (9%) and increases to 0.52 when the market share increases by one standard deviation (14%).

Finally, we explore the *ability* of politicians to help or pressure firms. To do so, we consider membership in key committees (Duchin and Sosyura, 2012; Agarwal et al., 2016; Akey et al., 2016). Powerful politicians have more sway in Congress; they were thus in a good position to help applicants at the start of the program, and to influence key legislation affecting the terms of TARP after it had been launched. An appendix table shows that one committee of the 110th Congress worked on bills related to TARP, and another three in the 111th Congress. We therefore create a dummy "Powerful politician" which is one for a bank whose home representative sat on one of these committees during the corresponding period. Column 8 in Table 3 shows that the interaction of this dummy with TARP x Home is positive and significant. Specifically, the home-district effect is 0.39 (≈0.31+0.08) for participants with a powerful home representative, against 0.09 for other participants. Political considerations were thus stronger for banks connected to a powerful politician.

Together, these results reinforce the interpretation that political influences (or the threat thereof) influence lending decisions in periods, banks and politicians where the scope or motive for political interference and the incentive on the part of the bank to respond to them or pre-empt them is higher. 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.

4.4. Aggregate Effects

Our tests thus far do not indicate either whether *aggregate* district lending increased, or that congressional representatives benefited from any higher lending. We seek to fill these two

gaps simultaneously using a two-stage least squares cross-sectional regression. The first stage

relates aggregate lending growth in a district after TARP to the district's total exposure to the

home-district effect. Formally, we estimate:

$$\Delta Mortgages_d = \gamma \cdot \%(TARP \cap Home)_d + Controls_d + \varepsilon_d,$$

where $\Delta Mortgages_d$ is the 2006-2007 to 2008-2009 change in total log mortgage origination

volume in a district (thus including non-frontier areas and all banks). The explanatory variable

of interest $\%(TARP \cap Home)_d$ is the share of mortgages originated by TARP participants

headquartered in the district, measured before the TARP (that is, in 2006-2007) to avoid reverse

causality. This variable captures the prior that districts with a larger presence of locally

headquartered TARP banks stand to gain more from the home-district effect than other districts.

Stage 2 relates district lending growth to the incumbent's 2010 electoral performance:

$$Incumbent\ performance_d = \beta \cdot \Delta Mortgages_d + Controls_d + \varepsilon_d,$$

where the regressand is Win – one if the incumbent won the 2010 House midterm election, and

zero otherwise, and $\%(TARP \cap Home)_d$ is used as an instrumental variable for $\Delta Mortgages_d$.

$\%(TARP \cap Home)_d$ is pre-determined, but still might be correlated with determinants

of lending growth and electoral outcomes; failing to control for these could violate the exclusion

restriction. We thus control for characteristics of the districts' borrowers, incumbent candidate

and electoral competition. We also include state fixed effects to control for state-wide shifts in

economic conditions or political preferences. The two equations are estimated on the cross-

section of 392 incumbent congressional representatives who ran for re-election in 2010.

Table 4 reports the estimation results. Districts more exposed to the home-district effect

experience higher aggregate lending. The results in column 1 indicate that when $\%(TARP \cap$

Home)$_d$ increases by 10%, post-TARP lending is 4.2% higher. Reassuringly, the second-stage results show that higher post-TARP lending in the district is associated with a higher probability that the incumbent wins the 2010 midterm election, when using either the post-TARP change in total lending (column 3) or acceptance rate (column 4) as endogenous variable of interest. In contrast to our main regression results, these tests seek to maximise representativeness, which comes at the cost of lower precision and statistical power. While magnitudes should thus be interpreted with caution, the results qualitatively support the notion that the home-district effect matters for district lending conditions and political outcomes.

5. The Home-District Effect and Mortgage quality

Did the increase in loan quantities associated with the home-district effect come at the cost of reduced mortgage quality? We now investigate two dimensions of mortgage quality – underwriting standards at origination and *ex post* performance.

5.1. Underwriting Standards

Following Dell'Arriccia et al. (2011) and Agarwal et al. (2012), we measure underwriting standards by exploring acceptance rates using our data at its most disaggregated level, that of the individual 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, and
- Location-time fixed effects are discussed below.

Because it models a mortgage supply decision *conditional on a given mortgage demand*, this approach has the additional benefit of removing unobservable individual demand-side effects.

We use two alternative sets of fixed effects. 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 borrower quality in a given neighborhood and period. Second, we retain 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. 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 A4), namely loan purchased by GSEs, loans guaranteed by the FHA and refinance loans.

The results in Table 5 indicate a positive and strongly significant coefficient for TARP x Home for both models. The 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. 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 TARP participants adopt looser underwriting standards than their competitors in their home district census tracts, as opposed to elsewhere and the pre-crisis periods. In addition, this finding indicates 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.

5.2.   *Mortgage Performance*

Do looser underwriting standards also coincide with poorer mortgage performance *ex post*? HMDA data does not allow us to track the performance of these loans over time. But data recently released by Freddie Mac (hereafter "FM") allows us to follow the performance of the subset of mortgage sold to FM. The original FM data is split across multiple datasets; each dataset reports information on the characteristics and subsequent monthly performance of all American mortgages originated during a given quarter ("cohort") and sold to FM by the originator. We download the data for cohorts from 2007q1 to 2010q4 in order to cover the immediate pre-TARP and post-TARP periods. At the time of writing, the monthly performance information in these datasets covers the period between the origination month to September 2017.

The data has three key limitations. First, mortgages sold to FM by their originator account for a significant proportion of all originated mortgages, but might not necessarily be representative of the whole population of loans or the particular behaviour of TARP banks.

Second, FM provides poorer data on the originator identity than HMDA. We thus cannot precisely distinguish mortgages originated by TARP participants vs. other lenders. This said, FM provides information on the quarter during which a mortgage was originated and its geographical location. We can thus exploit variation in the market share of TARP and home-district banks across time and geographies (as measured from HMDA data) to proxy for the exposure of a mortgage to the home-district effect. Concretely, the assumption is that, the higher the market share of home-district TARP participants in a given district and cohort, the more likely it is that a mortgage has been originated as a result of the political home-district effect.

The main practical challenge and third limitation of FM data is to attribute FM mortgages to congressional districts. FM provides relatively imprecise geographical location information: FM reports the state, Metropolitan Statistical Area (MSA), and three-digit ZIP code (hereafter

"3zip") of a mortgage, where HMDA provides information at the census tract. Therefore, we can only use FM mortgages which can be attributed a unique congressional district based on their state-MSA-3zip. We manage to do this for 965 of 2,405 distinct state-MSA-3zips. These areas typically lie in rural geographical areas as zip codes in urban areas often span multiple congressional districts.

After collapsing the FM data by cohort, district, and month, and merging it with market shares from HMDA, we estimate the following model:

$$\%(Default)_{c,d,m}$$
$$= \beta \cdot \%(TARP \cap Home)_{c,d} + \gamma \cdot Controls_{c,d} + District \cdot Month_{d,m}$$
$$+ Cohort \cdot Month_{c,m} + \varepsilon_{c,d,m}$$

where:

- $\%(Default)_{c,d,m}$ is the (value-weighted) cumulative share of FM-purchased mortgages from cohort c and originated in congressional district d which are in default during month m. We use cumulative shares in order to avoid the attrition bias that would result from the fact that mortgages drop out of FM data after they default. Alternatively we use the (value-weighted) share of non-performing (90 or more days past-due) mortgages from a cohort-district in a given month. This measure is more prone to attrition bias; but it measures mortgage quality in a continuous way, and is less susceptible to strategic default decisions or differences in recourse and foreclosure regulation across states and time.

- $\%(TARP \cap Home)_{c,d}$ is the (value-weighted) share of HMDA-reported mortgages from cohort c and district d which are originated by banks headquartered in district d and participating in TARP at the time of the origination of cohort c. Banks first enter TARP in 2008q4. Therefore, $\%(TARP \cap Home)_{c,d}$ is zero for cohorts between 2007q1 and 2008q3. We alternatively measure this variable using (i) all HMDA-reported originations by banks or (ii) all HMDA-reported originations by banks sold to FM. The second approach has the

benefit of zooming onto loans more likely to be covered in FM data. But in practice this advantage might be limited because HMDA only report FM sales if they are executed during the year of origination. This mechanically lowers data accuracy for loans issued towards the end of the year, for instance mortgages issued when banks entered TARP in 2008q4.

- $Controls_{c,d}$ is a set of controls for the loan or mortgage-market characteristics. Following Agarwal et al. (2015), we include the cohort-district averages of: mortgage size and maturity, FICO score, owner-occupier and condo dummies, loan-to-value ratio, and interest rate, as well as the (log) number of mortgages in the district-cohort, all at the origination time. We also add $\%(TARP)_{c,d}$ and $\%(Home)_{c,d}$ – the (value-weighted) share of mortgages from cohort c originated by TARP participants and banks headquartered in district d, respectively – alternatively measured using all HMDA originations or FM sales.

- $District \cdot Month$ is a set of district-month fixed effects controlling for unobservable performance determinants common to a district and/or month (local unemployment rate in a given month, etc.).

- $Cohort \cdot Month$ is a set of cohort-month fixed effects controlling for unobservable performance determinants common to a cohort and/or month (unobserved mortgage underwriting quality of a cohort, vulnerability of a cohort to changes in economy-wide economic circumstances in a month, etc.).

Table 6 reports the estimation results of the model above. A higher presence of home-district TARP banks in a cohort and district is associated with a significantly larger share of non-performing (columns 1 and 3) and defaulted (columns 3 and 4) mortgages. The results are similar when using either all HMDA-reported originations (columns 1-2) or only FM purchases (columns 3-4) to measure market shares. The results reported in column one suggest that a 10% higher market share of home-district TARP lenders is associated with a 0.3% higher share of non-performing loans – 10% of the average non-performing rate for the 2008q4 cohort (3.1%).

These results suggest that districts more exposed to the home-district effect have poorer mortgage performance. Our comprehensive set of fixed effects ensures that this result is not driven by unobserved determinants of mortgage performance across time and district. This said, this interpretation should be taken with caution, given the limitations of the FM data and the difficulty of its coherence with HMDA data. In particular, there is no precise way to ascertain that the non-performing loans we observe in FM are originated by TARP banks. Our results could thus be driven by indirect effects; for instance, increased aggressiveness by TARP banks in their home-district market could lead other banks to loosen their underwriting standards.

6. Conclusion

Government-funded programs are necessarily shaped and approved by legislatures, and allocated by public bodies with discretion. This leaves scope to politicians to influence the availability and terms of funds to firms with which they are connected. In this paper, we have examined the consequences of a large political intervention for the allocation of corporate investment across political constituencies. We have 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. We have also provided evidence that suggests that this higher lending improved the electoral prospects of incumbents while also reducing the quality of banks' mortgage portfolios. Succinctly, political interference associated with the TARP raised the quantity of mortgage lending by politically connected banks while also lowering its quality.

The key contribution of these findings is to provide evidence that investment decisions by beneficiaries of government funds are subject to political influences. Our findings also show that political forces matter, despite the maturity of the American political and financial systems and the absence of formal channels 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. Other government funding programs, like procurement contracts, may constitute insightful laboratories for further research.

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).

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 a country 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.

References

Agarwal, Sumit, Efraim Benmelech, Nittai Bergman, and Amit Seru (2012) "Did the Community Reinvestment Act (CRA) lead to risky lending?" Unpublished.

Agarwal, Sumit, Yongheng Deng, Chenxi Luo, and Wenlan Qian (2015) "The Hidden Peril: The Role of the Condo Loan Market in the Recent Financial Crisis" *Review of Finance* 20(2) (2016).

Agarwal, Sumit, Gene Amromin, Itzhak Ben-David, and Serdar Dinç (2016) "The politics of foreclosures" Unpublished.

Akey, Pat, Heimer, Rawley Z., and Stefan Lewellen (2016) "Politicizing consumer credit" Unpublished.

Amore, Mario Daniele, and Morten Bennedsen (2013) "The value of local political connections in a low-corruption environment" *Journal of Financial Economics* 110 (2), 387-402.

Bayazitova, Dinara, and Anil Shivdasani (2012) "Assessing TARP" *Review of Financial Studies* 25 (2), 377-407.

Black, Lamont K., and Lieu N. Hazelwood (2013) "The effect of TARP on bank risk-taking" *Journal of Financial Stability* 9 (4), 790-803.

Chavaz, Matthieu (2016) "Dis-integrating credit markets – Diversification, securitization and lending in a recovery" *Bank of England Staff Working Paper* 617.

Cohen, Lauren, Joshua Coval, and Christopher Malloy (2011) "Do powerful politicians cause corporate downsizing?" *Journal of Political Economy* 119 (6), 1015-1060.

Cohen, Lauren, and Christopher J. Malloy (2014) "Friends in high places" *American Economic Journal: Economic Policy* 6 (3), 63-91.

Dinç, Serdar (2005) "Politicians and banks: Political influences on government-owned banks in emerging markets" *Journal of Financial Economics* 77 (2), 453-479.

Duchin, Ran, and Denis Sosyura (2012) "The politics of government investment" *Journal of Financial Economics* 106 (1), 24-48.

Duchin, Ran, and Denis Sosyura (2014) "Safer ratios, riskier portfolios: Banks′ response to government aid" *Journal of Financial Economics* 113 (1), 1-28.

Faccio, Mara, Ronald W. Masulis, and John McConnell (2006) "Political connections and corporate bailouts" *Journal of Finance* 61 (6), 2597-2635.

Giannetti, Mariassunta, and Luc Laeven (2012) "The flight home effect: Evidence from the syndicated loan market during financial crises" *Journal of Financial Economics* 104 (1), 23-43.

Gilje, Erik P., Elena Loutskina, and Philip E. Strahan (2016) "Exporting liquidity: Branch banking and financial integration" *Journal of Finance* 71 (3), 1159-1184.

Landier, Augustin, Vinay B. Nair, and Julie Wulf (2007) "Trade-offs in staying close: Corporate decision making and geographic dispersion" *Review of Financial Studies* 22.3: 1119-1148.

Li, Lei (2013) "TARP funds distribution and bank loan supply" *Journal of Banking & Finance* 37 (12), 4777-4792.

Mackenzie, John (2009) "Gerrymandering and legislator efficiency" Unpublished.

Mian, Atif, Amir Sufi, and Francesco Trebbi (2010) "The political economy of the US mortgage default crisis" *American Economic Review* 100 (5), 1967-1998.

Petersen, Mitchell A., and Raghuram G. Rajan (2002). "Does distance still matter? The information revolution in small business lending" *Journal of Finance* 57 (6), 2533-2570.

Rose, Andrew K., and Tomasz Wieladek (2014) "Financial protectionism? First evidence" *The Journal of Finance* 69 (5), 2127-2149.

Schoenherr, David (2017) "Political connections and allocative distortions" Unpublished.

Veronesi, Pietro, and Luigi Zingales (2010) "Paulson's gift" *Journal of Financial Economics* 97 (3), 339-368.

Table 1: Summary Statistics for the Benchmark Sample

	(1) Mean	(2) Std. Dev.
Panel A: Bank-year variables		
TARP	0.212	0.41
(Log) total assets	14.10	2.76
Tier-1 capital (% Total assets)	0.08	0.03
Cash (% Total assets)	0.02	0.02
Charge-offs (% Total assets)	0.01	0.01
Repossessed real estate (% Total assets)	0.00	0.01
Deposits (% Total assets)	0.73	0.14
Non-performing loans (% Total assets)	0.02	0.02
(Log) bank age	66.55	42.33
Return on equity	0.06	0.15
Exposure to local shocks	-0.16	1.62
Panel B: Bank-county-year variables		
Δ (log) mortgage lending	0.015	1.12
Home	0.22	0.41
TARP x Home	0.020	0.14
Borrower loan-to-income	2.10	1.07
(Log) borrower income	4.48	0.61
(Log) loan size	4.94	0.82
Borrower tract (log) median income	4.68	0.27
Non-white dummy	0.06	0.18
Non-male dummy	0.19	0.26
Panel C: Additional bank-level variables		
Exit	0.13	0.34
Eligible for 1st round	0.39	0.49
Deposit-to-asset ratio	0.72	0.14
TARP supporter	0.63	0.48
Financial contributions	11.99	0.98
District market share	0.09	0.14
Powerful politician	0.35	0.48

This table reports the mean (column 1) and standard deviation (column 2) of variables of the main regression model (1) for all observations included in the benchmark sample. Annual American data 2006-2010, for all loans given to census tracts adjacent to a within-state congressional district border. All HMDA- and Call Reports-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. See section 4 (bank-year and bank-county-year variables) and section 6 (additional bank-level variables) for other variable definitions.

Table 2: Estimates of Home-district Effect: Effect of TARP participation on home-district mortgage lending

	(1) Baseline	(2) All census tracts	(3) All banks	(4) Without gerrymandered districts	(5) TARP-county-year fixed effects	(6) TARP x Close to HQ control	(6) TARP x Home-County control	(7) Placebo
TARP x Home	0.22**	0.21**	0.16*	0.22**	0.17**	0.18**	0.20**	-0.03
	(0.07)	(0.07)	(0.08)	(0.08)	(0.06)	(0.07)	(0.09)	(0.08)
TARP	-0.05	-0.08	-0.11	-0.04		-0.06	-0.06	0.07
	(0.09)	(0.08)	(0.09)	(0.08)		(0.09)	(0.11)	(0.10)
Observations	93,671	220,192	133,629	87,433	93,671	93,671	93,671	74,888
Adjusted R^2	0.40	0.34	0.35	0.39	0.43	0.39	0.59	0.42
TARP x Home + TARP	0.18	0.12	0.04	0.18	0.17	0.12	0.14	0.04
(p-value)	(0.00)	(0.01)	(0.52)	(0.00)	(0.00)	(0.04)	(0.08)	(0.60)

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- and Call Reports-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 median income. Bank-home and county-year fixed effects included but not recorded.

Table 3: Variation of the "Home-district" effect across time, banks, and politicians

Interaction:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Timing		*Bank characteristics*		*Politician characteristics*			
	Around injection	Exit	Eligible for 1st round	Deposit-to-asset ratio	TARP supporter	Financial Contributions	District market share	Powerful Politician
TARP x Home	0.54**	0.28**	0.03	1.80*	-0.04	-0.65*	0.05	0.08
	(0.18)	(0.08)	(0.07)	(0.82)	(0.09)	(0.33)	(0.07)	(0.05)
TARP	-.18	-0.12	0.06	-1.38**	0.25*	1.08**	0.06	0.07
	(0.18)	(0.09)	(0.08)	(0.50)	(0.12)	(0.33)	(0.08)	(0.07)
TARP x Home x Interaction		-0.40*	0.32*	-2.34*	0.38*	0.08*	2.03**	0.31*
		(0.16)	(0.14)	(1.10)	(0.16)	(0.03)	(0.75)	(0.14)
TARP x *Interaction*		0.49**	-0.15	2.04**	-0.41**	-0.010**	-0.78**	-0.26*
		(0.15)	(0.10)	(0.71)	(0.15)	(0.03)	(0.15)	(0.12)
Observations	93,671	93,671	93,671	93,671	82,984	82,774	86,723	93,671
Adjusted R^2	0.39	0.39	0.39	0.39	0.40	0.40	0.39	0.39

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- and Call Reports-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. In column 1 (*Around injection*), *TARP* is the sum of the coefficients for $TARP_t$ (1 the year the bank receives TARP, 0 otherwise) and $TARP_{t+1}$ (1 the year after the bank receives TARP, 0 otherwise), and *TARP x Home* is the sum of $TARP_t$ *x Home* and $TARP_{t+1}$ *x Home*; $TARP_{t+2}$ and $TARP_{t+2}$ *x Home* included but not reported. *Exit* is 1 if the bank's representative was not re-elected in the November 2008 congressional election or if the bank has reimbursed TARP funds, and 0 otherwise. *Eligible for 1st round* is 1 if the bank is a public, non-Corporation S bank, and 0 otherwise. *Deposit-to-asset ratio* is the bank's deposit funding as percentage of total assets as of 2008q3. *TARP supporter* is 1 if a bank's home representative voted in favor of EESA in Congress (October 2nd 2008 roll call), and 0 otherwise. *Financial contributions* is log contributions made by financial industry to 110th Congress home representative (up to November 2008). *District market share* is the bank's share of total mortgage lending in its home district in 2006-2007. *Powerful politician* is 1 in 2008 if a bank's home representative is member of the 110th Congress House financial committee; 1 after 2008 if the representative is member of 111th Congress House financial, ways and means, judicial or oversight and government reform committees; and 0 otherwise. 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 median income. Bank-home and county-year fixed effects included but not recorded.

Table 4: Aggregate and electoral effects

	(1)	(2)	(3)	(4)
	District-level effect			
Model:	IV Stage 1		IV Stage 2	
Dependent variable:	Δ Mortgage volume	Δ% Accepted applications	Yes if incumbent wins 2010 midterm	
%(TARP ∩ Home)$_d$	0.42**	0.06**		
	(0.16)	(0.02)		
Δ Mortgage volume			1.87*	
			(0.92)	
Δ % Accepted applications				12.32*
				(5.47)
TARP$_i$ x %(Home)$_i$				
Additional controls:				
State fixed effects	Yes	Yes	Yes	Yes
Borrower characteristics	Yes	Yes	Yes	Yes
Δ Economic conditions	Yes	Yes	Yes	Yes
Election characteristics	Yes	Yes	Yes	Yes
Bank characteristics				
Observations	392	392	392	392
R^2	0.76	0.67	0.13	0.19

Coefficients, with robust standard errors in parentheses; one (two) asterisk(s) indicates significantly different from zero at .05 (.01) level. Regressands are: 2006-2007 to 2008-2009 change in log mortgage origination volume for a district (column 1); 2006-2007 to 2008-2009 change in accepted mortgage applications (% total) for a district (column 2); 1 if incumbent candidate wins 2010 midterm House of Representatives election, 0 otherwise (columns 3-4); 2006q4-2007q4 mean to 2010q4 change in non-performing loans (% total loans) for a bank (column 5). %(*TARP ∩ Home)$_d$* is 2006-2007 mortgage volume originated by TARP participants headquartered in the district (%district total). TARP$_i$ is 1 for TARP participants, 0 otherwise; %(Home)$_i$ is 2006-2007 home-district mortgage volume (% bank total). Borrower controls are district (columns 1-4) or bank (column 5) 2006-2007 averages of: log income; loan-to-income; log loan size; non-*white* dummy; non-male dummy; tract median income. Δ Economic conditions controls are district (columns 1-4) or bank (column 5) average of 2006-2007 to 2008-2009 log borrower income growth and log loan size growth. Election controls are: 2006 and 2008 winner margin; 2008 and 2010 log candidates number; 2006-2008 representative Republican dummy; 2010 incumbent Republican dummy, log terms served, and 2008 general election vote %; financial industry employees (% total employees); home-district mortgages (%total mortgages). Bank controls are TARP$_i$, %(Home)$_i$, and 2006q4-2007q4 mean of: home-district mortgages (%total mortgages); TARP dummy (1 if bank is future recipient, 0 otherwise); non-performing loans (%Total Loans); 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. Districts where the incumbent candidate does not run in 2010 are excluded from columns 1-4. Annual American data for all loans. All HMDA- and Call Reports-reporting commercial banks active as of 2007q4 are included.

Table 5: Application-level evidence for the home-district effect of TARP participation on mortgage lending

	(1)	(2)
Model:	Within census tracts	Across census pairs
TARP x Home	0.04**	0.03**
	(0.01)	(0.01)
TARP	-0.03	-0.02
	(0.02)	(0.01)
Fixed effects:		
Tract-Year	Yes	Yes
Bank-Home	Yes	
Bank-Pair		Yes
Observations	767,397	1,632,856
Adjusted R^2	0.23	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- and Call Reports-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 6: Freddie Mac mortgages impairment rates and the home-district effect

	(1)	(2)	(3)	(4)
Dependent variable:	%(Non-Performing)$_{c,d,m}$	%(Default)$_{c,d,m}$	%(Non-Performing)$_{c,d,m}$	%(Default)$_{c,d,m}$
Bank % measured using:	All HMDA originations		Freddie Mac Sales only	
%(Home)$_{c,d}$	0.012	0.012	-0.004	0.021**
	(0.020)	(0.010)	(0.01)	(0.01)
%(TARP)$_{c,d}$	-0.004	-0.017	0.003	-0.007
	(0.005)	(0.009)	(0.004)	(0.004)
%(TARP ∩ Home)$_{c,d}$	0.031*	0.053**	0.023**	0.028**
	(0.014)	(0.009)	(0.008)	(0.006)
Mortgage Size	0.043**	0.042**	0.043**	0.044**
	(0.008)	(0.007)	(0.001)	(0.007)
Mortgage Maturity	-0.021*	-0.024**	-0.022*	-0.025**
	(0.010)	(0.007)	(0.01)	(0.007)
FICO Score	-0.062	-0.043	-0.062	-0.044
	(0.037)	(0.025)	(0.037)	(0.026)
Owner-Occupier	0.017	0.018*	0.017	0.019*
	(0.010)	(0.008)	(0.01)	(0.01)
Condo	0.037**	0.021*	0.038**	0.021**
	(0.013)	(0.009)	(0.013)	(0.0079)
Loan-To-Value	-0.0005	-0.0004**	-0.0005	-0.0004**
	(0.0003)	(0.0001)	(0.0003)	(0.0001)
Interest Rate	0.046**	0.006	0.046**	0.008
	(0.015)	(0.005)	(0.015)	(0.005)
Mortgage Number	0.0015	0.017**	0.001	0.017**
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	389,139	389,139	389,139	389,139
R^2	0.36	0.64	0.36	0.64

Coefficients, with standard errors (clustered by cohort) in parentheses; one (two) asterisk(s) indicates significantly different from zero at .05 (.01) level. The data includes all Freddie-Mac purchased mortgages which can be attributed to a unique congressional district, and tracks their performance between origination month and 2017m12. %(Non-Performing) is the volume-weighted share of mortgages from a district-cohort which are 90 or more days past-due in a given month. %(Default) is the cumulative volume-weighted share of mortgages from a district-cohort which default between the origination month and 2017m12. %(Home) is the value-weighted share of all HMDA-reported originations sold to Freddie-Mac in a district-cohort originated by banks headquartered in the district. %(TARP) is the value-weighted share of HMDA-reported originations in a district-cohort originated by TARP participants. %(TARP ∩ Home) is the value-weighted share of HMDA-reported originations in a district-cohort originated by banks headquartered in the district and participating in

TARP. %(Home), %(TARP) and %(TARP Home) are measured using yearly HMDA data; where necessary, we assume that a bank's origination volume in a given year is split equally across the four quarters of this year. In columns 1-2, these shares are computed using all HMDA-reported originations by banks; in columns 3-4 they are computed using originations by banks sold to Freddie Mac during the origination year only. Mortgage Size is the log mortgage volume; Maturity is the log mortgage maturity, in years; (Log) FICO score is the log FICO score; Owner-Occupier is 1 if a mortgage is for an owner-occupier, 0 otherwise; Condo is 1 if a mortgage is for a condominium, and 0 otherwise; Loan-To-Value is the loan-to-value ratio for a mortgage; Interest Rate is the rate on a mortgage; mortgage number is the log number of mortgages in the district-cohort; these characteristics are measured at origination using Freddie Mac data.

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 Oklahoma County. Thin red or gray lines delineate census tract borders. Thick black lines delineate 110th 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 110th Congressional district borders. Authors' illustration based on original maps from the Census Bureau.