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UNEXPECTED EFFECTS OF BANK BAILOUTS: DEPOSITORS NEED NOT APPLY AND NEED NOT RUN

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Unexpected Effects of Bank Bailouts: Depositors Need Not Apply and Need Not Run

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Abstract

A key policy issue is whether bank bailouts weaken or strengthen market discipline. We address this by analyzing how bank bailouts influence deposit quantities and prices of recipients versus other banks. Using TARP bailouts, we find both deposit quantities and prices decline, consistent with substantially reduced *demand* for deposits by bailed-out banks that dominate market discipline supply effects. Main findings are robust to numerous checks and endogeneity tests. However, a deeper dive into depositor heterogeneity suggests nuance. Increases in uninsured deposits, transactions deposits, and deposits in banks that repaid bailout funds early suggest some limited support for weakened market discipline.

JEL Classification: G18; G21; G28.

Key Words: Bailouts, Banking, Depositor Behavior, Market Discipline, Bank Runs.

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"Of course, market discipline can only limit moral hazard to the extent that debt and equity holders believe that, in the event of distress, they will bear costs. In the crisis, the absence of an adequate resolution process for dealing with a failing SIFI left policymakers with only the terrible choices of a bailout or allowing a potentially destabilizing collapse."

"The Crisis as a Classic Financial Panic", Remarks by Ben S. Bernanke Chairman Board of Governors of the Federal Reserve System at the 14th Jacques Polak Annual Research Conference Sponsored by the International Monetary Fund, Washington, D.C., November 8, 2013.

"The prospect of a government bailout reduces market discipline, giving creditors, investors, and counterparties less incentive to monitor vigilantly those institutions that they perceive will not be allowed to fail."

SIGTARP, Statement of Neil Barofsky, Special Inspector General Troubled Asset Relief Program, March 30, 2010

1 Introduction

To address the financial system turmoil and resulting economic recession during the Global Financial Crisis, the U.S. government, Federal Reserve, and other regulatory agencies took some of the most aggressive policies in the history of banking. These actions included numerous bank bailouts, including the prominent Troubled Asset Relief Program (TARP). More than a decade later – now in the midst of the COVID-19 crisis which has resulted in nonfinancial sector bailouts and may yet involve bank bailouts – the topic of bank bailouts still spur significant controversy and debate.

A key unresolved policy issue concerns whether bailed-out banks may be subject to either significantly weakened versus greatly strengthened market discipline by depositors and other creditors, shareholders, and other counterparties. Market participants may face reduced incentives to monitor bank risks and take actions to address these risks due to the perceived safety generated by the bailouts and/or increased expectations of future bailouts (e.g., Acharya, Cooley, Richardson, and Walter, 2011; Balasubramnian and Cyree, 2011; Acharya and Mora, 2015; Hett and Schmidt, 2017). If market discipline does decrease, this may in turn fuel higher bank risk-taking and increase the likelihood of future financial crises. In sharp contrast, others claim to the contrary that bailouts may increase market discipline on the bailed-out institutions. This is because bailouts may stigmatize the recipients, creating doubt about their financial health, and result in market participants exerting more discipline on these banks (e.g., Philippon and Skreta, 2012; Tirole, 2012; Armantier, Ghysels, Sarkar, and Shrader, 2015; Ennis, 2019; Che, Choe, and

Rhee, 2020). Excessive market discipline may also have unfavorable social consequences in terms of substantially reduced confidence in the financial system and curtailments in the supply of credit that may hamper economic recovery. Put in the vernacular, bailouts may either put market participants to sleep or wake them up to bank risks, with both extremes posing significant financial and economic hazards.

To break the tension between these opposing views, we investigate the effects of bailouts on market discipline by depositors using the TARP bailout as a quasi-natural experiment. During 2008:Q4-2009:Q4, the U.S. Treasury infused preferred equity capital into over 700 different banking organizations, including bank holding companies (BHCs), commercial banks, S&Ls, and thrifts. The bailout had several main objectives, including, but not limited to, increasing the stability of the U.S. financial system and improving real economic conditions. Any effects on market discipline would be unintended consequences.

Market discipline is crucially important to the stability of the banking sector, helping to offset bank moral hazard incentives and improve efficiency (see, e.g., Flannery, 1998; Flannery and Nikolova, 2004; Flannery and Bliss, 2019). Market discipline requires that private-sector agents both incur costs that are increasing in bank risks and act on the basis of these costs by charging banks higher interest rates or other prices, or by withdrawing business opportunities to them (Berger, 1991). Moral hazard incentives are particularly strong in banking due to government-backed deposit insurance and other safety net protections in addition to bailouts, making market discipline all the more vital for banks. The international Basel Accords that apply to the U.S. establish market discipline as one of the key pillars for effective bank supervision and regulation, complementing the supervisory and capital elements (Basel Committee on Banking Supervision, 2005; Marsh and Roman, 2018).

We address the market discipline issue by conducting the first empirical investigation of the effects of TARP on market discipline by depositors, the majority debtholders of the banks. Specifically, we analyze how TARP influenced both deposit quantities and prices of TARP recipients relative to other banks, controlling for other factors in a difference-in-difference (DID) setting. Deposit growth rates are taken from Call Reports

and implicit deposit rate premiums are derived from both Call Report and RateWatch survey data. We regress measures of deposit growth rates and interest rate premiums on a TARP recipient dummy interacted with a time indicator for the post-TARP program implementation (the DID term). We also include a strong set of controls for bank and market characteristics, as well as bank and time fixed effects. We conduct our tests on all bank deposits, as well as on a number of subsets of the data.

To identify the effects of bailouts on depositor discipline – a supply effect – we use both quantities and prices to distinguish net demand effects from net supply effects, since a change in quantities or prices alone could be explained by either supply or demand. There are a number of reasons why bailouts might shift the recipient banks' demand for deposits. TARP banks' demand for deposits may shift down because of the increased funding from the Treasury's preferred equity capital injections, choices to increase common equity funding, or reductions in asset portfolios. Conversely, TARP banks might increase deposit demand because of choices to reduce common equity, repay TARP funds early, or increase asset portfolios. Finally, TARP banks' demand for any particular deposit category may decrease or increase due to a desired shift among liability categories.

As will become clear when we discuss our contributions to the literature in Section 2, our treatment of both demand and supply effects from TARP differs quite sharply from both the extant market discipline literature in banking, and the extensive literature on the effects of TARP. The market discipline literature generally focuses on the supply of funds from market participants only, and the TARP literature usually concentrates on demands and supplies of the recipient banks, as opposed their customers' reactions.

Our main empirical findings are quite surprising. Instead of finding that market discipline by depositors significantly declined or increased from the TARP bailouts, we find any such change in depositor discipline is dwarfed by decreases in deposit demand by TARP banks. As illustrated by the second line of the title of our paper, "depositors need not apply and need not run." That is, they need not reduce or increase discipline, given that the TARP banks are actively shunning deposits so strongly. We find that both deposit quantities and rates decrease with bailouts, consistent with a substantial

downward shift in deposit demand, rather than any significant changes in deposit supply from depositor discipline.

Our main results are robust to a number of different checks and alternative specifications. We provide evidence that our results are not driven by reverse causality concerns using an instrumental variable (IV) approach which relies on banks' geography-based political connections as instrumental variables for TARP injections. We also provide several articles of evidence that our results are attributable to the treatment effect of government capital injection rather than selection of TARP banks into the program. In all specifications, we additionally explicitly control for proxies of bank financial health similar to the TARP program selection criteria. We account as well for any time-invariant heterogeneity between TARP and non-TARP recipients using bank fixed effects. We show that our findings hold up to a propensity score analysis, falsification tests, and alternative definitions of the key variables.

While our main findings are strong and robust, some of the results from examination of subsets of the data provide some addition important exceptions. The data for uninsured deposits, transaction deposits, and deposits held by banks that repaid TARP early suggest that deposits in these categories increased in response to TARP in some cases, contrary to our main findings. These exceptions do not allow for strong conclusions, but they are more consistent with weakened, rather than strengthened depositor discipline in these cases. Thus, while our overall conclusion about reduced deposit demand by banks dominates changes in depositor discipline remains unchanged, the subset analyses suggest that for some depositors, depositor discipline was more likely weakened than strengthened.

We make several contributions to the literature. First, this paper contributes to the bank bailout literature by investigating the important concern whether market discipline is increased or decreased by these bailouts. To our knowledge, we are the first to examine the effect of TARP bailouts on depositor discipline. Forssbaeck and Nielsen (2016) is the only other related paper we know that has looked at market discipline effects of bailouts by subordinated debt holders. Second, we contribute to the market discipline literature by incorporating both quantities and prices to distinguish between supply and demand

effects. Our findings also have policy implications for debates on costs and benefits of bailouts.

The rest of the paper proceeds as follows. Section 2 describes our contributions to the literature and Section 3 describes our hypotheses. Section 4 gives the methodology we employ. Section 5 describes the data, identification strategy, model specification, and variable construction. Section 6 presents our main results and robustness tests, while Section 7 describes extensions and additional robustness tests. Section 8 concludes.

2 Contributions to the Literature

Our paper contributes to two distinct literatures, market discipline and bank bailouts.

2.1 Market Discipline

There is a large empirical literature on bank market discipline, focused mainly on two major groups of bank stakeholders that may exert discipline – subordinated debtholders and depositors.[1] Starting with the former, subordinated debtholders lose their investments if the bank defaults and have no additional gains if the bank takes more risk and succeeds, and so are motivated to control default risks. Research focuses on whether subordinated debtholders can recognize bank risks, monitor and influence banks, and provide effective market discipline. Many papers find evidence of market discipline from subordinated debtholders, documenting that subordinated debt yields and premiums are correlated with accounting measures of bank risk, asset portfolio composition, credit agency ratings, regulatory ratings, and likelihood of undercapitalization or failure (e.g., Flannery and Sorescu, 1996; Hancock and Kwast, 2001; Jagtiani and Lemieux, 2001; Morgan and Stiroh, 2001; Bliss and Flannery, 2002; Blum, 2002; Evanoff and Wall, 2002; Jagtiani, Kaufman, and Lemieux, 2002; Sironi, 2003; Flannery and Nikolova, 2004; Krishnan, Ritchken, and Thomson, 2005; Gropp, Vesala, and Vulpes, 2006; Pop, 2006;

[1] Flannery (1998, 2010), Bliss (2014) and Flannery and Bliss (2019) provide surveys of the market discipline literature. Other bank stakeholders that could also exercise discipline are shareholders and bank loan and off-balance sheet customers, but the literatures on these stakeholders are relatively thin.

Evanoff, Jagtiani, and Nakata, 2011; Danisewicz, McGowan, Onali, and Schaeck, 2018, 2020). Some papers find that safety net policies can weaken market discipline for banks (e.g., Balasubramnian and Cyree, 2011; Acharya, Anginer, and Warburton, 2016). Turning to depositors, uninsured depositors have the most to lose from bank default and so have the strongest incentives to monitor bank activities and discipline riskier banks by withdrawing funds or demanding higher interest rates (see, e.g. Chen, Goldstein, Huang, and Vashishtha, 2020). Research finds that depositors react to an increase in the risk of their banks measured by leverage, variability of earnings, asset risk, and expected probability of default (e.g., Hannan and Hanweck, 1988; Ellis and Flannery, 1992; Goldberg and Hudgins, 1996, 2002; Billett, Garfinkel, and O'Neal, 1998; Park and Peristiani, 1998; Maechler and McDill, 2006; Iyer and Puri, 2012; Iyer, Puri, and Ryan, 2016), but finds little or no discipline from depositors on bank managers (e.g., Jordan, Peek, and Rosengren, 1999; Gilbert and Vaughan, 2001; Jagtiani and Lemieux, 2001). Depending on the development and the institutions of the studied banking market, the introduction of explicit deposit insurance is found to either weaken depositor discipline (e.g., Demirgüç-Kunt and Huizinga, 2004; Karas, Pyle, and Schoors, 2013; Ioannidou and de Dreu, 2019) or not weaken this discipline (e.g., Martinez Peria and Schmukler, 2001).

There is also some research about the effects of depositor discipline during and around financial crises, when most of the bank bailouts occur. Some papers find increase in depositor discipline during crises consistent with "wake-up" effects on depositors to bank risks. Opiela (2004) finds that in the period preceding the 1997 crisis in Thailand, depositors increased their market discipline by monitoring banks and finance companies more closely. Karas, Pyle, and Schoors (2010) investigate discipline by Russian depositors around the Russian crisis of 1998, when bailouts were not available for the large majority of banks. They find both firm and household depositors reacted to the crisis by exerting more discipline. Bennett, Hwa, and Kwast (2015) evaluate the FDIC's expectations about losses to creditors at banks that failed during the Global Financial Crisis and find evidence of market discipline from uninsured depositors, insured depositors, and general creditors far enough in advance to signal to both banks and supervisors that corrective

actions can and should be taken. They also find that these market participants are able to distinguish among banks of different risks. Similarly, Lamers (2015) investigates discipline by depositors in local U.S. banking markets around the time of banks failures during the Global Financial Crisis and finds that a wake-up call materializes in those markets where a bank failed. Finally, Acharya and Mora (2012, 2015) examine deposit flows and rates through the first part of 2009. They report that riskier banks about to fail during the crisis experienced large deposit outflows and raised deposit rates to attract new deposits but were only weakly successful.

Others find that depositors exhibit either reduced market discipline and/or low sensitivity to bank fundamentals during crises, consistent with "go to sleep" effects. Berger and Turk-Ariss (2015) investigate depositor discipline in the U.S. and EU countries before and during the Global Financial Crisis, which involved many bank bailouts. They find significant depositor discipline prior to the crisis in both the U.S. and EU, but this discipline mostly decreased during the crisis, with the notable exception of small U.S. banks. Levy-Yeyati, Martinez Peria, and Schmukler (2004) look at depositors' reactions during the crisis periods in Argentina and Uruguay and find that depositors become more sensitive to macroeconomic risks but less sensitive to bank-specific factors. Oliveira, Schiozer, and Barros (2015) look at effects on depositors in Brazil during the global turmoil of 2008 and find evidence of depositor run from smaller banks towards systemically important banks, which is better explained by the perception of a too-big-to-fail policy rather than bank fundamentals. Finally, Hasan, Jackowicz, Kowalewski, and Kozłowski (2013) use data from several Central European countries and find that the recent crisis did not seem to change the sensitivity of deposit growth rates to accounting risk measures. Depositors' actions were more strongly influenced by negative press rumors concerning parent companies than by fundamentals.[2]

Our research differs from these papers in that we focus specifically on differences

[2]Also related to crises but not focused on depositors, Hett and Schmidt (2017) use a novel test for market discipline based on changes in debt-to-equity sensitivity for a sample of large financial firms (including banks, insurance companies, and GSEs) to infer market participants' bailout expectations and their discipline during the Global Financial Crisis. They find that market discipline deteriorated over the crisis period, but this trend reversed after the initiation of the Dodd Frank Act.

between banks that are bailed out and those that are not, rather than the general effects of financial crises, which include bailouts and many other factors.

2.2 Bank Bailouts

In the interest of brevity, we only review the findings for TARP, the most studied of all bailouts. As noted, Forssbaeck and Nielsen (2016) is the only paper of which we know that analyzes disciplinary effects on TARP bailouts, and they focus on subordinated debtholders, rather than depositors. They use a sample of 123 BHCs over the period 2004-2013 and find that predicted distress risk from TARP has consistently positive and significant effects on subordinated debt spreads, suggesting an increase in market discipline. However, their results, are transitory and disappear altogether if the largest banks are dropped from the sample. Our study differs significantly in that we study deposits rather than subordinated debt, and in that we include quantities and prices to disentangle demand and supply effects.

There is a very large literature on the consequences of TARP bailouts, and we focus here just on the studies of bank risk-taking effects of these bailouts. Starting with credit risk, Duchin and Sosyura (2014) use a sample of publicly traded financial firms, which tend to be the largest firms, and find that TARP banks approve riskier loans. Berger, Makaew, and Roman (2019) use corporate credits, which are mostly also issued by large banks, and find that TARP generally led to more improvements in the terms of credit for high-risk borrowers. Similarly, Berger and Roman (2017) find that TARP banks shift into commercial real estate lending, which tend to be very risky. Also consistent with risk shifting, Agarwal and Zhang (2018) and Chavaz and Rose (2019) find more future problem loans for TARP banks. Black and Hazelwood (2013) find higher credit risk for large banks and lower credit risk for small banks.[3]

Despite the common finding of increased credit risk, several studies find that TARP banks increased their capital ratios and so they reduced their leverage risks. To be

[3]Many studies also examine the effects of TARP on the quantities of credit supplied, mostly finding positive effects, also consistent with increased credit risk (see Berger and Roman, 2020, for a comprehensive survey).

clear, these are studies of common equity and leverage based on common equity, and do not include preferred equity, which increased mechanically due to the injections by the U.S. Treasury under TARP. Duchin and Sosyura (2014), Calabrese, Degl'Innocenti, and Osmetti (2017) and Berger, Roman, and Sedunov (2020) all find that accounting-based common equity-to-assets ratios increased, decreasing accounting leverage risk under TARP, and also find consistent results for market-based capital ratios and leverage. Berger, Roman, and Sedunov (2020) also document that the systemic risk contributions of TARP banks are reduced relative to those of non-TARP banks, almost entirely due to the decrease in market leverage.

We add to the TARP and other bailout research literatures by focusing on the effects of this bailout on market discipline by depositors, which is heretofore unstudied.

3 Hypothesis Development

We are concerned with whether TARP bailouts increased or decreased market discipline supply effects from depositors, while recognizing that deposit quantities and prices are also determined by bank demands that may also be influenced by TARP. Since TARP may have increased or decreased both deposit supplies and demands, we formulate and test four empirical hypotheses that map the four combinations of changes in quantity and price changes.

There are two cases of relative quantity and price changes that map into the empirical domination of *supply-side hypotheses* in which we can determine if market discipline by depositors decreased or increased:

- **Hypothesis 1** (**"Decreased Discipline"** or **"Depositors Need Not Run"** Hypothesis): Depositors move more deposits toward TARP recipients, as these are now considered safer because of the bailout or because of an increased expected probability of future bailouts (Quantity ↑ Price ↓).
- **Hypothesis 2** (**"Increased Discipline"** or **"Depositors Run"** Hypothesis): Depositors reduce supplies to TARP recipients relative to other banks because these

banks are perceived as riskier or are stigmatized by the bailouts (Quantity ↓ Price ↑).

There are also two cases that map quantity and price changes into the empirical domination of *demand-side hypotheses* in which we can determine if bank demand for deposits from TARP banks decreased or increased:

- **Hypothesis 3** (**"More Deposits Needed"** Hypothesis): Banks increase their demand for deposits to replace other funding sources and/or to increase assets (Quantity ↑ Price ↑).
- **Hypothesis 4** (**"Depositors Need Not Apply"** Hypothesis): Banks reduce their demand for deposits because of additional funds from other sources and/or decreases in asset portfolios (Quantity ↓ Price ↓).

None of these four hypotheses rules out the others. Our empirical analysis simply determines which one is empirically dominant. This may differ across deposit types. For example, market discipline effects may be stronger for uninsured deposits, since these depositors have more of their own funds at risk, so **Hypotheses 1** or **2** may be more likely to dominate for uninsured deposits.

4 Methodology

4.1 Baseline Empirical Framework

Our approach of identifying changes in supply and demand for deposits by examining changes in deposit quantities and prices is in line with the existing literature (e.g., Park and Peristiani, 1998; Martinez Peria and Schmukler, 2001; Demirgüç-Kunt and Huizinga, 2004; Karas, Pyle, and Schoors, 2013; Berger and Turk-Ariss, 2015; Ioannidou and de Dreu, 2019). We estimate two reduced form equations for the net flow of deposits

and the deposit rate premium charged by the bank. The baseline specifications are:

$$\Delta \ln D_{it} = \beta_0 + \beta_1 \text{POST}_t \times \text{TARP}_i + \beta_2 \text{CONTROLS}_{it-1} + \mu_i + \nu_t + \epsilon_{it} \quad (1a)$$

$$r_{it} - r_{ft} = b_0 + b_1 \text{POST}_t \times \text{TARP}_i + b_2 \text{CONTROLS}_{it-1} + m_i + n_t + e_{it} \quad (1b)$$

where $\Delta \ln D_{it}$ represents the quarterly deposit growth rate calculated in log differences for bank i at time t, and $r_{it} - r_{ft}$ is the deposit rate the bank pays in excess of a risk-free rate. Our main variable of interest is the difference-in-difference interaction term $\text{POST}_t \times \text{TARP}_i$, where POST_t is a dummy taking on the value 1 after the start of the TARP program and 0 otherwise, and TARP_i is a dummy taking on the value 1 if the bank received TARP funds and 0 otherwise. The specifications include individual bank and local market control variables to account for bank-specific and market-specific conditions that otherwise might impact the demand and supply for deposits, as well as bank fixed effects (μ_i and m_i) and time fixed effects (ν_t and n_t) to control for remaining unobserved heterogeneity. The variables POST_t and TARP_i do not appear independently in the equation because they are absorbed by the time and bank fixed effects, respectively. Robust standard errors are clustered at the bank level.

Our coefficients of interest are β_1 and b_1, which tell us how deposit flows and rates changed for banks after they received the TARP capital injections relative to non-TARP banks, ceteris paribus. A positive β_1 would indicate that TARP banks see higher inflows or lower outflows of deposits after receiving TARP compared to other banks. Similarly, a positive b_1 would suggest that TARP banks pay higher rates on their deposits after receiving TARP relative to non-recipients. Of course, negative values would have opposing implications. To disentangle supply shocks from demand shocks and see which hypothesis is supported, we jointly inspect β_1 and b_1. A finding of $\beta_1 < 0$ and $b_1 > 0$ would support the empirical dominance of increased discipline under **Hypothesis 2** (**"Depositors Run"**). Alternatively, $\beta_1 > 0$ and $b_1 < 0$ would be in line with empirical dominance of **Hypothesis 1** (**"Depositors Need Not Run"**), $\beta_1 > 0$ and $b_1 > 0$ with **Hypothesis 3** (**"More Deposits Needed"**), and $\beta_1 < 0$ and $b_1 < 0$ with **Hypothesis 4** (**"Depositors**

Need Not Apply").

4.2 Further Endogeneity and Sample Selection Approaches

We estimate our specifications using OLS and instrumental variables (IV). The OLS specifications treat the TARP funds as if they were exogenously determined. However, TARP banks and non-TARP banks may differ from one another in systematic ways. The first eight involuntary participants were very large banks, and others had to apply for the funding. These funds were supposed to be allocated to banks deemed healthy enough by regulators. In reality, there are many potential reverse causality and omitted variable concerns raised in the TARP literature that may create bias in the OLS coefficients and require IV. We follow this literature and instrument TARP capital injections with two political variables – whether the local congressional district representative of the bank's headquarters served on the Financial Institutions Subcommittee or Capital Markets Subcommittee of the House Financial Services Committee in 2008 or 2009 and whether she/he is a Democrat (e.g., Duchin and Sosyura, 2012, 2014; Li, 2013; Berger and Roman, 2015, 2017). Because the TARP variable is binary, we follow the dummy endogenous variable model in Wooldridge (2002) procedure 18.4.1 (pp. 236-237) which involves a 3-step approach and was used in prior TARP literature.[4]

In line with most of the literature, we apply a homogenous treatment in which POST equals 1 starting from 2009:Q1 onwards, and 0 otherwise. We also test whether our results are robust to our choice of the treatment period by rerunning the baseline regressions using the actual dates for each bank.

To further alleviate selection concerns, we also consider a propensity score matched (PSM) sample. We apply Mahalanobis Distance Matching on bank-level CAMELS components and market-level control variables in 2007:Q4 (at least one year before the capital injections start) to match TARP banks with non-TARP banks. We rerun our results on the matched sample.

[4]We first run a probit explaining TARP distribution using political connection variables as well as bank-level and market-level control variables, and in the next stage, we instrument receiving TARP with the probability from the first stage probit regression. A similar approach is recommended in Angrist and Pischke (2009).

Finally, we execute a falsification test for our baseline results, in which we randomly assign TARP to the banks, keeping the proportions of TARP and non-TARP banks equal to the sample proportions. We repeat this random assignment 1,000 times and rerun the baseline OLS regressions to arrive at a distribution of coefficients we can use for statistical inference.

5 Data

5.1 Data and Sample Selection

To address our research question, we require data on deposit holdings and deposit interest rates of both TARP and non-TARP banks, risk-free rates to compute the rate premiums, information on which banks received TARP capital injections, and other data on the banks and their local markets to construct the control variables. We collect our data from multiple sources.

Our main source of bank data are the quarterly Reports of Condition and Income (Call Reports). From these reports, we obtain at a quarterly frequency balance sheet information and income statement data between 2004:Q1 and 2012:Q4 for unconsolidated commercial banks. Since TARP capital was injected at the bank holding company (BHC) level of affiliated banks, we consolidate the Call Report data of all the banks in each BHC. If the commercial bank is independent, however, we keep the data for the commercial bank. For expositional ease, we henceforth use the term "banks" to indicate either entity. We exclude entities that do not refer to commercial banks (RSSD9331 = 1), have missing or incomplete financial data for key financial variables such as total assets or equity, or that failed before 2009:Q1 (i.e., before the observation of TARP effects). Following prior research, to avoid distortions for the Equity to GTA ratio, for all observations with equity less than 0.01 times gross total assets (GTA), we replace Equity with 1 percent of GTA.[5]

The bank financial variables are deflated to 2012:Q4 dollars using the seasonally ad-

[5]Gross total assets (GTA) equals total assets plus the allowance for loan and lease losses and the allocated transfer risk reserve (a reserve for certain foreign loans). Total assets on Call Reports deduct these two reserves, which are held to cover potential credit losses. We add these reserves back to measure the full value of the assets financed.

justed GDP deflator, and winsorized at the 1st and 99th percentiles. As one of our dependent variables is the percentage change in deposits, we also account for mergers by removing the bank-quarter observation when the merger took place. The merger information comes from the bank M&A lists of the Federal Reserve Bank of Chicago. Our final sample is an unbalanced panel comprising 219,206 bank-quarter observations for 8,059 individual banks covering the period 2004:Q1 to 2012:Q4.

5.2 Deposit Variables

For every bank for every quarter, we collect the stock of total deposits, as well as insured deposits and uninsured deposits. Total deposits can be directly obtained from the Call Reports. To calculate uninsured deposits, we use Call Report Schedule RC-O and take all the funds in accounts that are partially insured and subtract off the amount that is insured, while carefully considering separate treatment for several time periods affected by changes in deposit insurance limits over our sample period.

Specifically, for the period *2004:Q1-2006:Q1*, we calculate the uninsured deposits as the amount of bank deposit accounts (demand, savings, and time) with a balance on the report date of more than $100K minus the number of such deposit accounts multiplied by $100K. For the period *2006:Q2-2009:Q2*, we take into account the different treatment of deposit retirement accounts versus the rest. Thus, we calculate the uninsured deposits as the amount of bank deposit accounts (demand, savings, and time, excluding retirement accounts) with a balance on the report date of more than $100K minus the number of such deposit accounts multiplied by $100K plus the amount of bank deposit retirement accounts with a balance on the report date of more than $250K minus the number of such deposit accounts multiplied by $250K.

For the period *2009:Q3* onwards, we account for the deposit insurance limit increase from $100K to $250K for all deposits except foreign ones. Thus, we calculate the uninsured deposits as the amount of bank deposit accounts (demand, savings, and time, including retirement accounts) with a balance on the report date of more than $250,000 minus the number of such deposit accounts multiplied by $250,000. While the last change

in deposit insurance took place in October 2008, the Call Report did not change to reflect this increase in limit until 2009:Q3.[6] We further subtract from the uninsured deposits the deposits that fell under the FDIC Transaction Account Guarantee Program (TAGP) during the Global Financial Crisis over October 14, 2008 to December 31, 2010 and further extended to December 31, 2012 by the Dodd-Frank Act, providing unlimited deposit insurance for non-interest-bearing transaction accounts. Finally, for all time periods, we also add the foreign deposits to the uninsured deposits because foreign deposits are not covered by the FDIC deposit insurance.

The deposit rates are derived from multiple sources. We construct the implicit interest rate using Call Report data as interest expense minus service charges divided by the amount of total deposits. We also employ data from RateWatch, a weekly survey collecting rates on a number of standardized deposit products. Since RateWatch data is collected at bank-branch level, we first link the RateWatch data to the Summary of Deposits database using the unique branch identifier, *uninumbr*. We then select the last-recorded rate for each bank in each quarter as the deposit rate. For banks on which we have data from multiple branches, we aggregate the deposit rates up to the bank-level using the deposit share from the FDIC Summary of Deposits.

We select deposit rates with the highest availability throughout the sample. Following Ben-David, Palvia, and Spatt (2017) and Drechsler, Savov, and Schnabl (2017), we select two products from the RateWatch data, namely the CD rate for accounts of \$10K with a maturity of 12 months, as well as the CD rate for accounts of \$100K with a maturity of 12 months. The \$10K CDs cover 77 percent of our bank-quarter observations and the \$10K CDs cover 38 percent. The implicit interest rate, as well as the two CD rates are taken in excess of a risk-free rate, for which we use the 1-Year Treasury Constant Maturity Rate.

[6]If all banks are affected equally by this increase in deposit insurance limit, including time fixed effects would be sufficient to correct for this issue. However, Lambert, Noth, and Schüwer (2017) show that this was not the case. In robustness analyses, we have excluded 2009:Q3 and the results are robust to the exclusion of this quarter. Since deposit flows are obtained from the deposit stocks, this issue disappears after this quarter.

5.3 Bailout Variables and Instruments

We collect information on the TARP bailouts under the Capital Purchase Program (CPP) from the U.S. Treasury website.[7] We acquire the bank name, exact date of purchase, amount of purchase, repayment dates, and information on whether the bank has missed divided payments. In total we have data on injections in 709 unique institutions, 572 of which are bank holding companies and 87 are commercial banks, while the rest are S&Ls and thrifts. We match by name and location the institutions in the U.S. Treasury list with their corresponding RSSD9001 (Call Report ID) where available using the National Information Center database. We exclude S&Ls and thrifts because their data is not comparable with banks and their business models are different. The majority of TARP injections were repaid by 2011, although 297 banks had not fully repaid TARP at the end of our sample period in 2012:Q4.

To instrument TARP with political connections, we collect data from the Center for Responsive Politics and House of Representatives websites information on whether banks are headquartered in a congressional district of a House member who served on the Capital Markets Subcommittee or the Financial Institutions Subcommittee of the House Financial Services Committee in 2008 or 2009. We also obtain information on whether the bank was headquartered in a district where the local representative was a Democrat in the 2007/2008 campaign election cycle. Finally, we use the MABLE/Geocorr2k software on the Missouri Census Data Center website to associate banks with congressional districts by using the zip codes of their headquarters.

5.4 Bank and Market Control Variables

We employ several bank and local economic variables as controls. We include proxies for the regulatory CAMELS ratings components that were used in the TARP approval process and proxy for the bank financial health and riskiness.[8] These are important to

[7] `http://www.treasury.gov/initiatives/financial-stability/Pages/default.aspx`

[8] CAMELS is a confidential supervisory rating system used by U.S. regulatory authorities to rate the financial condition of banks, according to the six factors represented by its acronym: Capital adequacy, Asset quality, Management Quality, Earnings, Liquidity, and Sensitivity to Market Risk. The ratings in each category, ranging from 1 (best) to 5 (worst), are assigned based on financial ratios and onsite

include because banks in more precarious condition and riskier (lower levels of capital, asset quality, management quality, earnings, liquidity, and having a higher sensitivity to market risks) are expected to pay higher interest rates on their deposits or see a lower inflow of deposits. We follow standard definitions in the literature and proxy *Capital Adequacy* (C) by the simple (unweighted) equity ratio, *Asset Quality* (A) is proxied by the ratio of nonperforming loans to total loans, *Management Quality* (M) is measured by the cost-to-income ratio, *Earnings* (E) are proxied by the return-on-assets, *Liquidity* (L) is proxied by the ratio of liquid assets to total assets, and *Sensitivity to Market Risk* (S) is measured by the absolute gap between short term assets and short term liabilities divided by GTA. However, we recognize that our CAMELS proxies may be imperfect measures of the true regulatory CAMELS scores because they do not capture the content of onsite examinations or other intangible characteristics. Other bank controls include bank size, measured by the natural logarithm of GTA, the age of the oldest bank in the organization, and whether the bank is a BHC.

We also control for local economic conditions to which the banks are exposed as banks present in low poverty areas, high median household income areas, or high gross regional domestic product per capita areas may see a higher inflow of deposits. We collect county-level data on the poverty rate, median household income, and the gross regional domestic product (GDP) per capita from the U.S. Census Bureau and the Bureau of Economic Analysis (BEA), and aggregate this information to the bank level by using deposit shares in the bank's markets of operation from the FDIC Summary of Deposits.

Finally, we proxy for the local bank concentration with the county-level Herfindahl-Hirschman index (HHI) of bank deposits using again data from the FDIC Summary of Deposits and aggregate the index to the bank level using the deposit shares of the affiliated branches.

examinations.

5.5 Summary Statistics for Key Variables

Table 1 shows summary statistics for our key variables in the final dataset. Of the 8,059 unique banks between 2004:Q1 and 2012:Q4, 646 banks received TARP, accounting for 9 percent of the bank-quarter observations.[9] At the time of the capital injections, the bailout size was equivalent to 2.5 percent of the risk-weighted assets and 22 percent of risk-based capital.

Regarding the key endogenous variables, growth in deposits and deposit rate premia, banks' total deposits grew on average 1.53 percent per quarter and insured and uninsured deposits grew by 1.35 percent and 2.48 percent, respectively. However, large variations can be seen in the data. The excess implied interest rate is always negative. This may reflect the fact that the implied interest rate excludes the value to depositors of transaction services, safekeeping and other services implicitly included in the deposit contract. The excess interest rates from RateWatch on the $10K and $100K CDs are on average positive, with the $10K CD having an excess rate of 0.17 percent on average and the $100K CD having an excess rate of 0.50 percent on average. However, the sample size for the latter rate decreases considerably due to the scarce availability of these rates in the RateWatch survey.

As for control variables, Table 1 shows that banks in our sample were in good health. Banks appeared to be well capitalized as the simple equity rate equals 11.4 percent, and only an average of 1.75 percent of their loan portfolios was nonperforming.

5.6 Parallel Trends Analysis

Figure 1 plots the evolution over time of the simple average, unconditional deposit flows and deposit rates we use in our baseline analysis for TARP and non-TARP banks. The deposit flows exhibit seasonality, with slightly lower quarter-on-quarter flows of deposits in each second quarter compared to higher flows in the fourth quarter. Such seasonality is accounted for in the regression models with the time fixed effects.

[9]This excludes S&Ls and thrifts as well as a few other TARP institutions in the official list with missing financial data and/or due to other data restrictions mentioned above.

Figure 1A shows that before TARP implementation, TARP banks experienced average higher inflows of deposits than non-TARP banks, but this reverses after program initiation. Figure 1B shows the same pattern for the insured deposits, but it is less apparent for uninsured deposits in Figure 1C. Turning to interest rates in Figure 1D-F, the deposit rates on the 12-month 10K CDs and 12-month 100K CDs from RateWatch suggest that TARP banks pay lower interest rates on their deposits compared to non-TARP banks after initiation. The implicit interest rate from the Call Reports shows that the gap between rates of TARP banks and non-TARP banks has become smaller in the TARP period.

The aggregate movements of both lower quantities and lower prices for TARP banks relative to non-TARP banks after TARP was initiated provide some preliminary evidence consistent with the empirical domination of a negative demand shift for deposits under Hypothesis 4 above. The figures also suggest that the parallel trends assumptions are not violated for either deposit flows or the deposit rates.

Of course, these aggregate trends are only mildly suggestive, and neither show individual bank behavior nor include control variables. In the next section, we investigate our question more rigorously in our DID model, controlling for different demand and supply factors, and addressing potential endogeneity concerns.

6 Empirical Results

6.1 Main Results

We start with our baseline findings from estimating Equations 1a and 1b with OLS. Table 2 shows regressions of the inflow of total deposits – as well as insured and uninsured deposits – and the different measures of the deposit rate on the DID term $\text{POST}_t \times \text{TARP}_i$. All regressions include bank and time fixed effects as well as time-varying bank-level controls described in Section 4, and standard errors are clustered at the bank-level.

Our first result in column (1) is that total deposits decline significantly for TARP banks after they receive preferred equity injections compared to non-TARP banks. In

the POST period, TARP banks have average lower inflows of 0.589 percent per quarter. These results are economically large, as the average inflow of deposits is 1.175 percent per quarter in the post-TARP period. The decrease in deposits is mainly driven by a decrease in insured rather than uninsured deposits.

Turning to deposit rates in columns (4)-(6), we see that all DID terms have negative signs and are statistically significant. The pricing result is also economically sizable. TARP banks have a lower implicit deposit rate premium of 7.5 basis points in the post-TARP period, compared to an average implicit deposit rate premium of 6.2 basis points in the post-TARP period. The negative findings for both quantities and prices together support **Hypothesis 4**, **"Depositors Need Not Apply"** suggesting reduced demand for deposits by TARP banks relative to non-TARP banks.

The results for the controls suggest that banks with higher capital ratios, banks with better asset quality, smaller banks, and banks with branches in more affluent markets see higher inflows of insured and uninsured deposits. For deposit rates, the signs on the control variables are less consistent in magnitude and in sign, consistent with the mixed findings in the literature (e.g. Maechler and McDill, 2006; Acharya and Mora, 2015; Bennett, Hwa, and Kwast, 2015; Ben-David, Palvia, and Spatt, 2017).

6.2 Selection and Endogeneity

Table 3 addresses the robustness of our results. In Panel A, we show the results of our IV estimates using political variables to exogenize the bank level treatment with TARP. In the first stage (Table A2 in the Appendix), the coefficients on the political variables have positive signs and are statistically significant, in line with prior literature. The second stage confirms our baseline results – the coefficients on the difference-in-differences terms are negative, statistically significant and economically sizable for both deposit flows and deposit prices, confirming our baseline OLS findings.[10] Our baseline finding that the effect is mainly driven by a reduction in the demand for insured deposits

[10]Since the IV approach estimates the local average treatment effect (LATE) of banks whose reception of TARP funds is more likely determined by their political connections, the IV estimates are a factor of 4 to 9 times larger in magnitude than the OLS estimates. Such an increase in magnitude of the LATE is consistent with the literature (e.g. Jiang, 2017).

is confirmed. The results are, in sum, fully in line with the baseline analysis and consistent with **Hypothesis 4**, **"Depositors Need Not Apply"** that banks reduced their demand for deposits.

In Panel B, we verify whether our results are robust to using the actual purchase dates for each bank as the start of the TARP period rather than 2009:Q1. The coefficients are quantitatively and qualitatively similar to our baseline analyses.

In Panel C, we report the results of our matched sample analysis. In Table A3 in the Appendix, we show more information on the matching procedure and matching outcome. The coefficients on the deposit quantities in our matched sample analysis are negative and consistent with the baseline results. For the rates, we do see robustness of the result on the implicit interest rate, but a bit less so on the RateWatch rates. Part of this can be explained by the fact that not all matched banks have RateWatch survey rates available in the pre-TARP period (especially for the 100K CD rate where the sample drops from 82,158 to 15,871 observations), thereby considerably reducing the power of the test for those variables in this setting. Still, our results remain broadly consistent with the empirical dominance of **Hypothesis 4**, **"Depositors Need Not Apply"** and do not support any of the other three hypotheses.

Finally, in Panel D, we show the results of our falsification experiment in which we randomly assign the TARP treatment to banks 1,000 times in 2009:Q1, and re-estimate our main results for each repetition. We then report the mean DID coefficients together with the corresponding 5^{th} and 95^{th} percentiles (shown in brackets). The confidence band around the mean coefficient includes zero in all cases, indicating that the random assignment of TARP has no significant effect on deposit flows or deposit rates, again supporting our baseline findings.

6.3 Dynamics of the Effects

To examine dynamics of our main effects over time, in Table 4, we split the POST variable into four year-dummy variables that take on the value 1 for each year in the treatment period. Panels A and B show OLS and IV findings, respectively.

The results for total deposits, insured deposits and all deposit rates are generally consistent with our baseline analyses, but the dynamics do show slight differences across the years for the uninsured deposits. In 2009, TARP banks see an increase in the uninsured deposits compared to those banks that did not receive TARP, although this is offset by negative flows in later years, rendering the uninsured deposits coefficient in our main analysis above insignificant. Since we are not able to observe rates on uninsured deposits, we are unfortunately unable to distinguish whether this might be because of a deposit rate increase for uninsured deposits. However, the finding corresponds with previous literature stating that banks can offset losses of uninsured deposits with increased inflows of insured deposits, and vice versa (see, e.g., Billett, Garfinkel, and O'Neal, 1998; Martin, Puri, and Ufier, 2018).

One potential interpretation is that TARP banks see a decrease in depositor discipline for uninsured deposits in the first year of the TARP program, which then gives way to a downward demand shift in later years. Thus, although we do not have separate pricing results for uninsured deposits, we can say that the 2009 increase in the quantity of uninsured deposits is at least consistent with a temporary decline in depositor discipline by uninsured depositors and is not consistent with any increase in such discipline. That is, it is more consistent with **Hypothesis 1** than with **Hypothesis 2** in the short run for uninsured deposits.

6.4 Early Repayment

Following Berger and Roman (2015, 2017), we also test whether the results differ for banks that repaid TARP early in 2009 or 2010 compared to those that did not. TARP banks that repaid early were generally stronger institutions that were able to raise enough capital to convince authorities that they were safe and were likely able to get rid of limits on executive compensation. Table 5 splits TARP banks into those that repaid early versus those that did not repay early, with Panels A and B showing OLS and IV results, respectively.[11]

[11] Because we consider here the TARP recipients that did and did not repay early separately, following prior research on TARP, we include one additional instrumental variable that accounts for exit from the

Our baseline results are only consistently found in the group of banks that did not repay early. Banks that repaid early show some evidence of increased deposit quantities, particularly for uninsured deposits, and mixed results for deposit rates. While we cannot draw strong conclusions from this mixed evidence, the increases in deposit quantities for the banks that repaid early are at least suggestive that there was more likely a decline than an increase in depositor discipline for these banks. That is, the findings are more consistent with **Hypothesis 1** than with **Hypothesis 2**, since **Hypothesis 2** would require a decrease in deposits by these banks.

6.5 Unpacking the Results by Deposit Type

We next conduct a deeper analysis by deposit type, using Call Reports to obtain more detailed data on the level of different deposit holdings of banks as well as their interest rates. We follow Martin, Puri, and Ufier (2018) and distinguish non-demandable term deposits (total, insured and uninsured), non-maturity savings deposits, non-maturity checking transaction deposits and non-core brokered deposits.[12] In our sample, term deposits make up 41.6 percent of total deposits (28.3 percent are insured and 13.3 percent are uninsured), savings deposits make up 25.5 percent, and checking transaction deposits make up 24.3 percent. Brokered deposits make up the remaining 8.6 percent but we exclude this category here because not all banks use brokered deposits. We also use the Call Reports to construct implicit interest rates for each deposit category so that we can investigate price movements.

We replicate the OLS results for the different categories in Table 6 Panel A and the IV results in Panel B. Most categories show negative effects for both quantities and prices, again consistent with baseline results favoring the empirical dominance of **Hypothesis 4**. For transaction deposits, however, we find a positive significant coefficient for quantities, along with a negative price coefficient that becomes significant in the IV estimation.

TARP program: *CEO Compensation $500K+*. *CEO Compensation $500K+* is a dummy which takes a value of one if bank's CEO had a total compensation greater than $500,000 in 2008 because banks with high CEO compensation are more likely to exit the program, following Wilson and Wu (2012) and Bayazitova and Shivdasani (2012). First stage results are reported in Table A2 in the Appendix.

[12] See Table A1 in the Appendix for more information on the variable definitions.

These findings support **Hypothesis 1**, a weakening of market discipline, for transaction accounts.[13]

We cannot say with certainty why **Hypothesis 4** dominates for all of the other deposit categories and for deposits overall, but not for transaction deposits, but we postulate why this might be true. Many transaction deposits are either non-interest-bearing or pay very little explicit interest, so TARP banks that are otherwise reducing other deposit categories by varying deposit rates are more limited in this ability for transaction deposits. Our general finding of empirical dominance for **Hypothesis 4** does not mean that the other hypotheses are not functioning, but rather that the reduced demand for deposits by TARP overwhelms them. Our finding of reduced depositor discipline for transaction accounts may occur as well in the other categories but remains undetectable because TARP banks are able to more effectively reduce their holdings of the other deposit types.

6.6 Channels Behind the Downward Shift in Deposit Demand

We next investigate some of the channels behind our main finding of the dominance of **Hypothesis 4**, **"Depositors Need Not Apply"**. Specifically, we investigate whether the reduced demand for deposits by TARP banks may have originated on the left or right hand sides of their balance sheets. As discussed above, TARP banks may need fewer deposits because they may shrink their assets or shift from deposits to other liabilities or equity funding. In Table 7, we examine the effects of TARP on growth in these other balance sheet entries, using the same control variables as in the earlier equations to gain insights into which of these channels may be active. That is, we check whether assets decline or other liabilities or equity increase with TARP, although we cannot claim cause-and-effect from these variables to the deposit decline.

Panels A and B show the OLS and IV findings, respectively. In column (1), we find evidence that TARP banks reduce their assets compared to non-TARP banks after receiving the capital injection, and the results are statistically significant and hold for both OLS and IV. These results are consistent with a channel in which TARP banks

[13]The tests are also robust to using the exact TARP dates as treatment period and a falsification test where we randomly assign TARP 1,000 times to banks in 2009:Q1, the other robustness checks in Table 3.

reduce their demands for deposits relative to non-TARP banks because they want to fund fewer assets. Column (2) shows a reduction in other liabilities and so does not support a shift from deposits to other liabilities. Column (3) shows contradictory signs for equity, and so does not support a switch from deposits to equity as fueling the decline in deposits. Thus, the data are only consistent with the channel in which a desire for fewer assets helps explain the reduced demand for deposits, but as indicated, we cannot claim cause-and-effect from this channels analysis.

7 Extensions and Additional Robustness Tests

In this section, we tie up several loose ends. We discuss additional robustness checks for our main results, investigate the effects of TARP at the intensive margin, and conduct cross-sectional tests based on bank characteristics. For brevity, we only replicate baseline DID results – additional IV findings are available upon request.

7.1 Additional Robustness Tests

In Table 8, we run additional robustness tests dealing with alternative choices regarding control variables, fixed effects, and estimation sample. In Panel A, we run the baseline regression with bank and time fixed effects but without potentially collinear control variables. In Panel B, we remove the bank fixed effects to be able to use between-bank variation instead of within-bank variation. To be able to construct the difference-in-differences estimator, we include a TARP dummy variable as well as the $\text{POST}_t \times \text{TARP}_i$ variable of interest. In Panels C-E, we test whether certain banks drive the main estimation results. In Panel C, we exclude the eight banks that were involuntary TARP recipients and therefore may have different demands for deposits. In Panel D, we exclude the 19 banks that participated in the 2009 Supervisory Capital Assessment Program (SCAP) stress tests, and may be under greater scrutiny. Panel E excludes small banks with GTA below $100 million as these banks may be faced with different deposit market conditions. Panels F and G remove two different quarters from our sample which may drive the re-

sults. Panel F deletes observations from 2008:Q4, the quarter which shortly followed the Lehman Brothers failure shock that may have significantly affected deposit flows, and Panel G, removes 2009:Q3, the quarter in which the Call Report redenomination of the insured and uninsured deposits took place (see Table A1 in the Appendix).

In all cases, our results are robust to these alternative choices, and indicate an overall reduced demand for deposits by TARP banks, consistent with **Hypothesis 4**, **"Depositors Need Not Apply"**.

7.2 Effect of TARP at the Intensive Margin (TARP-iness)

Our main results identify an average shift in deposit demand for TARP banks, regardless of the size of the capital injection. In Table 9, we investigate whether TARP banks receiving a more sizeable injection change their demand for deposits to a larger extent.

We rerun our main regression specification using three different treatment intensity measures instead of the TARP dummy variable before. These measures are the natural logarithm of the TARP capital injection (Panel A), the ratio of the capital injection to the bank's risk-based (Tier 1 and Tier 2) capital (Panel B), and the ratio of the capital injection to the bank's risk-weighted assets (Panel C). We present the results in Table 9.

In all cases, deposit quantities and rates consistently show negative coefficients, suggesting that banks lower their demand for deposits after the TARP capital injection, again consistent with **Hypothesis 4**, **"Depositors Need Not Apply"**. These results further show that not only the treatment but also the intensity of the treatment matters, e.g., the demand for deposits decreases more for banks that received higher TARP injection amounts.

7.3 Cross-Sectional Tests

In Table 10, we assess whether depositors exhibit differential responses to TARP capital injections that depend on their banks' characteristics or circumstances. TARP banks that are larger, better capitalized, and operate in better local economic conditions

may attract more deposits or experience reduced discipline due to a perception of higher safety and soundness.

We re-estimate our baseline regressions using sample splits based on the median values of bank size, bank capitalization, and market-level economic conditions.

In Panel A, we find that the demand effect we identified earlier is present for both small and large banks. For large banks, we find strong effects for both insured deposits and total deposits, whereas for small banks we only find an effect for insured deposits. This may be because the coefficient for uninsured deposits is positive, albeit not significant, for small banks, indicating that the demand effect of **Hypothesis 4**, **"Depositors Need Not Apply"** is marginally dominating the weakened market discipline effect of **Hypothesis 1**, **"Depositors Need Not Run"**.

In Panels B and C, we find that our main results hold for banks with both high and low levels of capitalization, as well as for banks faced with both good and bad local economic conditions. We do find, however, that the downward deposit demand shift is more pronounced for highly capitalized banks, which need less deposit funding, *ceteris paribus*.

8 Conclusions

Understanding whether bank bailouts significantly weaken or strengthen market discipline is an important research and policy question. Theoretically, the results could go either way, and there is little in the way of prior empirical evidence on this point. We address this under-investigated issue by conducting the first empirical study of the effects of the massive TARP bailout of U.S. banks on market discipline by depositors, who account for the majority of bank debt. Using an extensive dataset, we measure the effects of the bailouts on deposit quantities and prices in order to distinguish supply effects caused by changes in market discipline from demand effects caused by bank reactions to bailouts.

We find very surprising results overall. Banks that received TARP registered outflows of deposits and lower deposit prices compared to non-recipient banks, consistent

with demand effects by banks dominating supply effects or market discipline decreases or increases by depositors. These baseline results are quite robust to a wide array of estimation strategies and robustness tests including instrumental variables, falsification tests, propensity score matching, and many others. Our channels analysis is suggestive of the reason that TARP recipients choose to demand less deposits – they appear to shrink their assets in response to TARP and thus require fewer deposits.

These overall results, however, mask some interesting findings for three subsets of the data – uninsured deposits, transaction deposits, and deposits held by banks that repaid TARP early. Regression analyses suggest that these deposits increase in response to TARP in some cases. While we cannot draw strong conclusions from these exceptions, these deposit increases are consistent with weakened market discipline and are not consistent with strengthened discipline by depositors in these cases. Thus, while we cannot draw strong conclusions regarding overall changes in depositor discipline from TARP, the results of investigating subsets of the data are more suggestive of pockets of weakened rather than strengthened discipline.

Our paper contributes to the bank bailout literature by investigating for the first time the effect of TARP bailouts on depositor discipline. We also contribute to the market discipline literature by assessing effects on both quantities and prices to distinguishing between supply effects of market participants and demand effects of the banks themselves.

Our results have important implications for the policy debate on costs and benefits of bank bailouts and other resolution regimes for banks in general, and the costs and benefits of the TARP bailouts in particular (e.g., Calomiris and Khan, 2015; Berger and Roman, 2020). Among the costs identified in the literature is potential weakening of market discipline, including by depositors. Our empirical findings are only suggestive of weakened depositor discipline in some of our subsamples, with any overall change in depositor discipline empirically dominated by decreases in deposit demand by the bailed out banks.

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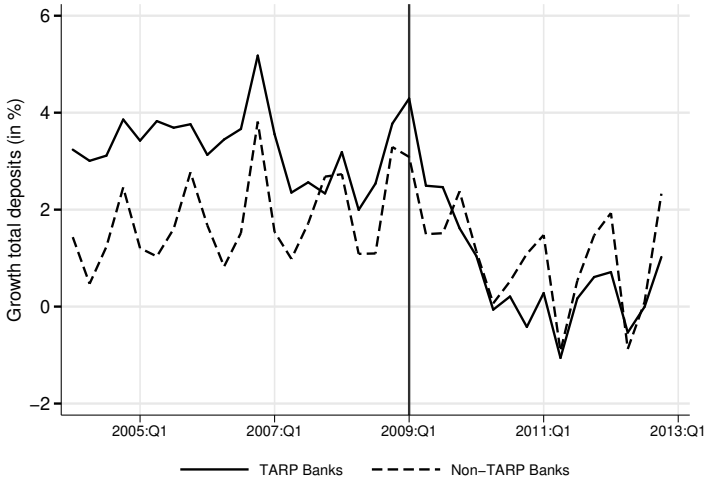
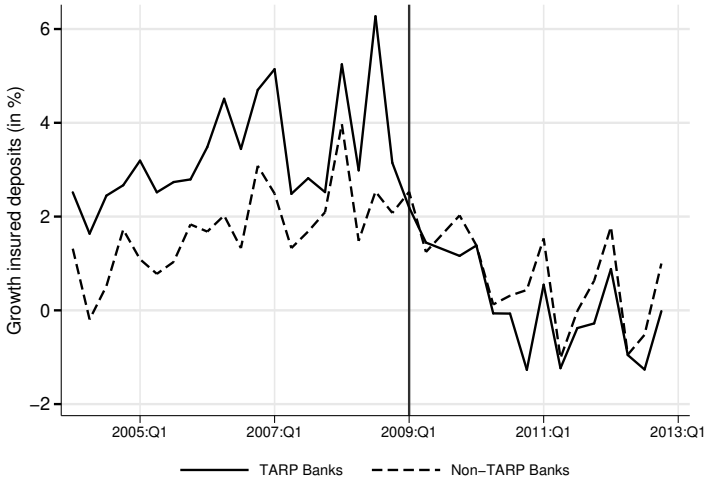
Figure 1: Deposit Flows and Interest Rates for TARP vs. Non-TARP Banks

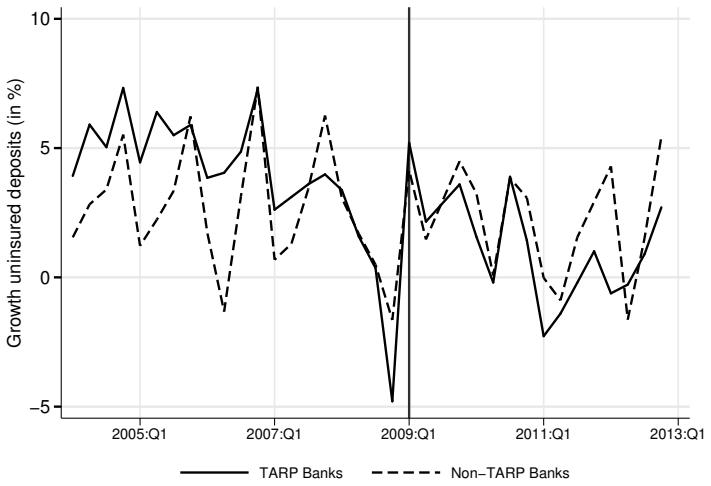
Panel A: Total Deposits



Panel B: Insured Deposits



Panel C: Uninsured Deposits

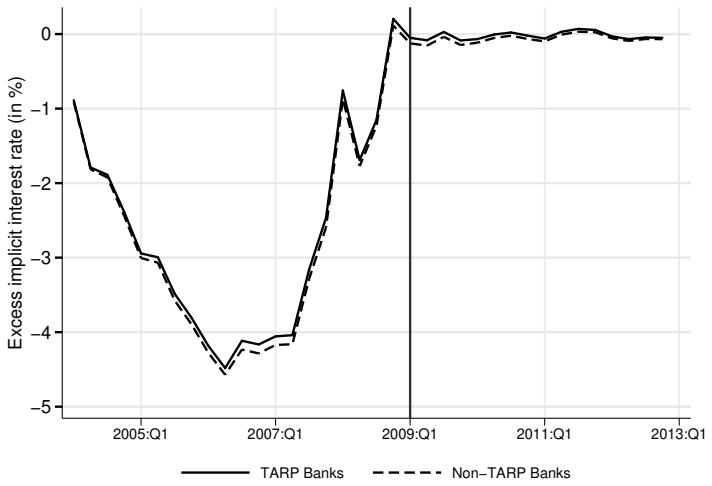


(continued on next page)

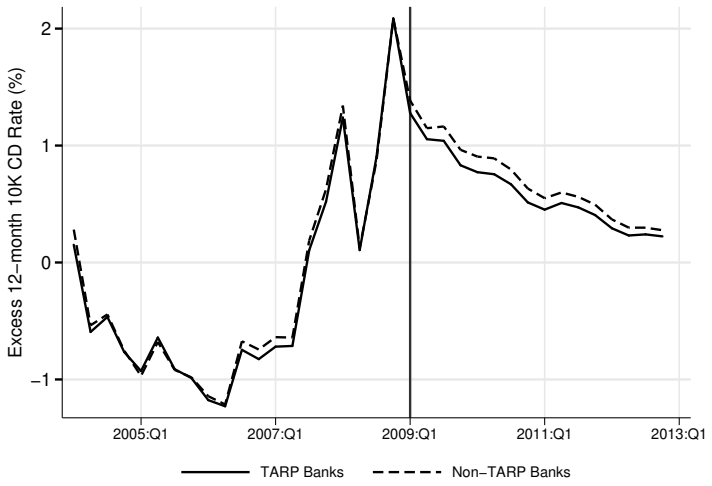
The figure shows (simple average) deposit flows and deposit rates for TARP and non-TARP banks per quarter. The vertical line corresponds to the first quarter of 2009, the start of the treatment period.

Figure 1: Deposit Flows and Interest Rates for TARP vs. Non-TARP Banks (Continued)

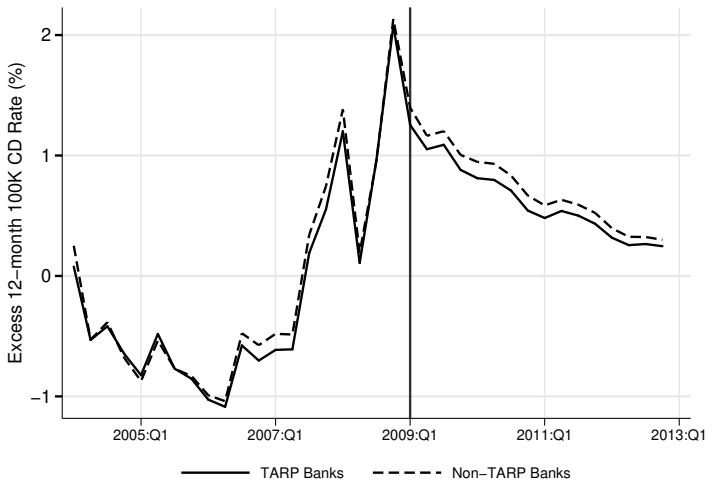
Panel D: Excess Implicit Interest rate



Panel E: Excess 12 month-10K CD rate



Panel F: Excess 12 month-100K CD rate



The figure shows (simple average) deposit flows and deposit rates for TARP and non-TARP banks per quarter. The vertical line corresponds to the first quarter of 2009, the start of the treatment period.

Table 1: Summary Statistics

	N	Mean	Std. Dev.	p25	p50	p75
Deposit Flows and Interest Rates						
Δ ln (Total Deposits) (%)	216,803	1.53	6.79	−1.98	0.63	3.71
Δ ln (Insured Deposits) (%)	210,451	1.35	7.73	−1.86	0.23	2.77
Δ ln (Uninsured Deposits) (%)	209,645	2.49	26.67	−7.02	1.35	10.90
Excess Implicit Interest Rate (%)	218,359	−1.64	1.71	−3.26	−1.02	−0.07
Excess 12-month 10K CD Rate (%)	168,346	0.17	0.94	−0.51	0.30	0.80
Excess 12-month 100K CD Rate (%)	83,312	0.50	0.68	0.22	0.54	0.88
Bailout Variables						
TARP	219,206	0.09	0.29	0.00	0.00	0.00
ln(1 + Bailout Amount)	219,206	0.88	2.81	0.00	0.00	0.00
Bailout Amount / Risk-Based Capital (%)	219,204	3.53	85.52	0.00	0.00	0.00
Bailout Amount / Risk-Weighted Assets (%)	206,037	0.32	1.46	0.00	0.00	0.00
Bank and Local Economic Conditions Variables						
Capital Adequacy (%)	219,206	11.40	5.85	8.64	10.02	12.16
Asset Quality (%)	217,470	1.78	2.47	0.26	0.91	2.23
Management Quality (%)	219,165	74.71	30.44	59.44	68.93	80.83
Earnings (%)	219,193	0.16	0.40	0.10	0.22	0.33
Liquidity (%)	219,206	6.40	6.42	2.55	4.06	7.59
Sensitivity to Market Risk (%)	219,206	10.48	9.28	3.12	8.11	15.38
Ln(Total Assets)	219,206	11.99	1.32	11.13	11.87	12.67
Age	219,187	67.97	43.64	23.00	79.00	104.00
BHC	219,206	0.74	0.44	0.00	1.00	1.00
Local Poverty Rate (%, weighted by deposit share)	215,502	14.64	4.92	11.26	13.65	17.04
Local Median Household Income (1000$, weighted by deposit share)	215,502	45.43	10.20	38.44	44.48	51.55
Gross Regional Domestic Product per Capita (1000$, weighted by deposit share)	215,502	45.86	6.37	41.95	45.65	50.32
Local Deposit HHI (weighted by deposit share)	215,502	0.23	0.13	0.14	0.19	0.27
Instruments						
Local Representative on House Financial Services Committee in 2008/2009	219,206	0.08	0.21	0.00	0.00	0.00
Local Representative a Democrat in 2007/2008 Election Cycle	219,206	0.37	0.48	0.00	0.00	1.00
CEO Compensation $500K+	219,206	0.03	0.16	0.00	0.00	0.00

This table shows summary statistics for variables used in the analysis. Banks are consolidated into the highest holding company, and data is between 2004 and 2012. The unit of observation is the bank-quarter level, and income statement variables are based on quarterly data. All data are winsorized at the 1^{st} and 99^{th} percentile, and bank-quarters in which a merger has taken place are removed. The merger information comes from the *Mergers and Acquisitions* list from the Federal Reserve Bank of Chicago. The implicit interest rate, 12-month 10K CD rate, and 12-month 100K CD rate are in excess of the 1 year Treasury rate. Capital adequacy is defined as the simple equity ratio, asset quality is proxied by the nonperforming loan ratio, management quality is proxied by the cost to income ratio, earnings are the return on assets, liquidity is the ratio of liquid assets to total assets, and sensitivity to market risk is the short-term asset and liability mismatch to total assets ratio. See Table A1 in the Appendix for definitions and sources of the data.

Table 2: Main Results

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta \ln D_{i,t}$			$r_{i,t} - r_{f,t}$		
	Total Deposits	Insured Deposits	Uninsured Deposits	Implicit Interest Rate	12-month 10K CD	12-month 100K CD
POST* × *TARP	-0.589***	-1.395***	-0.233	-0.075***	-0.064***	-0.048*
	[-4.57]	[-8.73]	[-0.81]	[-10.23]	[-3.39]	[-1.66]
Capital Adequacy	0.532***	0.591***	0.955***	-0.001***	0.007***	0.005***
	[39.61]	[35.58]	[30.31]	[-3.02]	[6.37]	[3.90]
Asset Quality	-0.368***	-0.367***	-0.474***	-0.005***	-0.011***	-0.007***
	[-31.51]	[-27.03]	[-12.94]	[-9.59]	[-9.35]	[-5.48]
Management Quality	0.002	0.001	0.006	0.000***	-0.000	0.000
	[1.33]	[0.76]	[1.44]	[9.29]	[-1.44]	[0.87]
Earnings	-0.062	-0.745***	1.155***	-0.017***	-0.003	-0.001
	[-0.88]	[-8.15]	[3.85]	[-8.29]	[-0.61]	[-0.23]
Liquidity	-0.131***	-0.059***	-0.338***	-0.001***	-0.002***	-0.004***
	[-22.25]	[-9.79]	[-17.86]	[-2.80]	[-4.37]	[-7.06]
Sensitivity to Market Risk	0.013***	0.004	0.066***	0.001***	0.000	0.001*
	[4.35]	[1.35]	[6.45]	[2.65]	[0.24]	[1.67]
Ln(Total Assets)	-5.900***	-4.184***	-10.972***	0.114***	-0.057***	-0.003
	[-30.19]	[-18.82]	[-25.87]	[13.64]	[-3.77]	[-0.16]
Age	0.003	-0.006	0.029**	-0.000*	0.000	0.001
	[0.63]	[-1.10]	[2.50]	[-1.68]	[0.70]	[0.94]
BHC	0.018	-0.024	-0.598	0.032***	-0.061**	0.001
	[0.08]	[-0.10]	[-1.17]	[2.97]	[-2.29]	[0.02]
Local Poverty Rate	-0.052***	-0.060***	-0.002	-0.002**	-0.005***	-0.000
	[-3.47]	[-3.43]	[-0.04]	[-2.51]	[-2.84]	[-0.22]
Local Median Household Income	0.034***	0.049***	0.123***	0.001***	0.007***	0.003**
	[2.69]	[3.53]	[3.43]	[2.84]	[5.50]	[2.26]
Gross Regional Domestic Product per Capita	0.061***	0.065***	-0.007	-0.001	0.003*	0.009***
	[3.76]	[3.49]	[-0.16]	[-1.40]	[1.93]	[4.05]
Local Deposit HHI	0.681	1.018	-0.216	-0.015	0.030	0.013
	[1.01]	[1.31]	[-0.11]	[-0.55]	[0.43]	[0.13]
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.167	0.117	0.026	0.050	0.014	0.012
No. of obs	206,845	200,858	199,810	206,844	162,787	82,158
No. of clusters	7,662	7,659	7,661	7,662	6,437	5,430

This table shows results for estimations analyzing the impact of TARP on the supply and demand for deposits. Columns (1)-(3) show results for different deposit flows, and columns (4)-(6) show results for different deposit rates. TARP is a dummy variable equal to 1 if the bank received TARP support under the Capital Purchase Program, and POST is a dummy variable equal to 1 between 2009-2012. All estimations include lagged bank-level and lagged market-level control variables, as well as bank and time fixed effects to control for unobserved heterogeneity. The control variables are the same as shown in Table 1 and are defined in Table A1 in the Appendix. T-statistics are reported in square brackets, based on standard errors clustered by bank. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 3: Endogeneity and Sample Selection Analyses

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta \ln D_{i,t}$			$r_{i,t} - r_{f,t}$		
	Total Deposits	Insured Deposits	Uninsured Deposits	Implicit Interest Rate	12-month 10K CD	12-month 100K CD
Panel A: IV						
POST* × *TARP	-1.795***	-5.502***	0.088	-0.355***	-0.402***	-0.431***
	[-5.29]	[-11.37]	[0.11]	[-12.86]	[-8.52]	[-6.79]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Cragg-Donald Wald F statistic	38679.3	37645.9	37547.1	38680.8	35415.1	20341.7
Kleibergen-Paap rk Wald F statistic	564.9	562.7	563.0	564.9	508.6	291.4
Within-$\bar{R}^2$	0.166	0.109	0.026	-0.064	-0.007	-0.028
No. of obs	206,845	200,858	199,810	206,844	162,787	82,158
No. of clusters	7,662	7,659	7,661	7,662	6,437	5,430
Panel B: Exact TARP Injection Dates						
POST-EXACT* × *TARP	-0.562***	-1.262***	-0.518*	-0.076***	-0.061***	-0.046
	[-4.32]	[-8.03]	[-1.79]	[-10.32]	[-3.22]	[-1.61]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.167	0.116	0.026	0.050	0.014	0.012
No. of obs	206,845	200,858	199,810	206,844	162,787	82,158
No. of clusters	7,662	7,659	7,661	7,662	6,437	5,430
Panel C: Matched Sample Regression						
POST* × *TARP	-0.428**	-0.761***	-0.112	-0.049***	-0.006	0.017
	[-2.47]	[-3.30]	[-0.28]	[-5.17]	[-0.23]	[0.40]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
$\bar{R}^2$	0.192	0.116	0.053	0.063	0.013	0.013
No. of obs	31,745	30,772	30,689	31,744	25,814	15,871
No. of clusters	1,033	1,033	1,033	1,033	936	893
Panel D: Falsification Test						
POST* × *TARP	-0.001	-0.001	-0.005	0.000	0.000	0.001
90% Confidence Interval	$[-0.204; 0.208]$	$[-0.235; 0.224]$	$[-0.553; 0.496]$	$[-0.011; 0.010]$	$[-0.030; 0.028]$	$[-0.055; 0.057]$
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

This table shows robustness tests dealing with endogeneity and sample selection issues for analyzing the impact of TARP on the supply and demand for deposits. Panel **A** shows results for instrumental variable estimations, where TARP is instrumented in a first stage probit regression with political connection variables shown in Table A2 in the Appendix. Panel **B** shows the estimation when redefining the POST period to be the quarter in which the bank received TARP, instead of letting TARP start in 2009:Q1. Panel **C** shows the results for matched sample regressions, where we obtain a matched sample of TARP and non-TARP banks based on Mahalanobis Distance Matching on bank-level and market-level control variables in 2007:Q4. See Table A3 in the Appendix for more information on the matching procedure and outcome. Panel **D** shows the results for a falsification test where we randomly assign TARP 1,000 times to banks in 2009:Q1, keeping the proportion of banks that received TARP similar, and afterwards run the baseline OLS regression. From the 1,000 repetitions we show the mean, the 5th and 95th percentile coefficients. In Panels **A**, **B** and **C** T-statistics are reported in square brackets, based on standard errors clustered by bank. In Panel **D** 90% Confidence Intervals are shown in square brackets. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 4: Dynamics of the Effects by Year

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta \ln D_{i,t}$			$r_{i,t} - r_{f,t}$		
	Total Deposits	Insured Deposits	Uninsured Deposits	Implicit Interest Rate	12-month 10K CD	12-month 100K CD
Panel A: OLS Estimates						
2009 × TARP	0.199	-1.228***	1.197**	-0.052***	-0.080***	-0.059*
	[1.27]	[-4.57]	[2.15]	[-7.28]	[-4.03]	[-1.95]
2010 × TARP	-0.663***	-1.296***	0.257	-0.074***	-0.084***	-0.074**
	[-4.21]	[-6.87]	[0.56]	[-10.06]	[-4.17]	[-2.44]
2011 × TARP	-0.920***	-1.431***	-0.816**	-0.083***	-0.055***	-0.039
	[-5.79]	[-7.77]	[-2.02]	[-10.65]	[-2.70]	[-1.34]
2012 × TARP	-1.073***	-1.614***	-1.404***	-0.095***	-0.036*	-0.015
	[-6.52]	[-9.08]	[-3.66]	[-11.40]	[-1.73]	[-0.51]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.167	0.117	0.026	0.050	0.014	0.013
No. of obs	206,845	200,858	199,810	206,844	162,787	82,158
No. of clusters	7,662	7,659	7,661	7,662	6,437	5,430
Panel B: IV Estimates						
2009 × TARP	0.011	-5.526***	2.078	-0.291***	-0.447***	-0.453***
	[0.03]	[-8.02]	[1.42]	[-11.45]	[-8.78]	[-6.30]
2010 × TARP	-2.316***	-5.172***	0.661	-0.365***	-0.540***	-0.573***
	[-6.15]	[-9.82]	[0.54]	[-13.10]	[-10.42]	[-8.43]
2011 × TARP	-2.627***	-5.926***	0.420	-0.373***	-0.361***	-0.400***
	[-6.16]	[-11.08]	[0.38]	[-12.84]	[-7.36]	[-6.37]
2012 × TARP	-2.452***	-5.410***	-2.675**	-0.399***	-0.234***	-0.277***
	[-5.42]	[-9.76]	[-2.47]	[-12.49]	[-4.87]	[-4.53]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Cragg-Donald Wald F statistic	9516.1	9163.7	9135.2	9516.2	8768.0	4735.0
Kleibergen-Paap rk Wald F statistic	141.8	139.9	139.9	141.8	128.2	103.6
Within-$\bar{R}^2$	0.166	0.109	0.026	-0.065	-0.010	-0.038
No. of obs	206,845	200,858	199,810	206,844	162,787	82,158
No. of clusters	7,662	7,659	7,661	7,662	6,437	5,430

This table shows the dynamics of the impact of TARP on the supply and demand for deposits by splitting the POST period in underlying years. As the unit of observation is at the bank-quarter level, these coefficients show the average quarterly deposit flows and deposit rates that TARP banks experience compared to non-TARP banks in specific years of the TARP period. T-statistics are reported in square brackets, based on standard errors clustered by bank. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 5: Early Repayment of TARP Capital

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta \ln D_{i,t}$			$r_{i,t} - r_{f,t}$		
	Total Deposits	Insured Deposits	Uninsured Deposits	Implicit Interest Rate	12-month 10K CD	12-month 100K CD
Panel A: OLS Estimates						
POST* × *TARP Not Repaid Early	-0.820***	-1.705***	-0.543*	-0.073***	-0.077***	-0.047
	[-5.88]	[-9.62]	[-1.72]	[-8.84]	[-3.73]	[-1.45]
POST* × *TARP Repaid Early	0.381	-0.086	1.072*	-0.084***	-0.016	-0.053
	[1.36]	[-0.27]	[1.85]	[-6.17]	[-0.35]	[-0.89]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.167	0.117	0.026	0.050	0.014	0.012
No. of obs	206,845	200,858	199,810	206,844	162,787	82,158
No. of clusters	7,662	7,659	7,661	7,662	6,437	5,430
Panel B: IV Estimates						
POST* × *TARP Not Repaid Early	-6.271***	-13.732***	-5.230***	-0.654***	-0.995***	-0.662***
	[-8.04]	[-11.18]	[-2.99]	[-10.52]	[-8.04]	[-4.49]
POST* × *TARP Repaid Early	4.525***	6.473***	7.252***	0.122	0.510**	-0.084
	[3.74]	[3.05]	[3.60]	[1.24]	[2.43]	[-0.49]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Cragg-Donald Wald F statistic	4189.4	4140.7	4141.0	4189.7	3288.3	1638.6
Kleibergen-Paap rk Wald F statistic	54.8	55.4	55.7	54.8	38.0	23.1
Within-$\bar{R}^2$	0.150	0.058	0.025	-0.381	-0.130	-0.072
No. of obs	206,845	200,858	199,810	206,844	162,787	82,158
No. of clusters	7,662	7,659	7,661	7,662	6,437	5,430

This table shows the differential impact of TARP on the supply and demand for deposits by banks that repaid TARP early and those that did not. The banks that receive TARP are split in (1) banks that repaid TARP early in 2009 or 2010, and (2) banks that did not repay early. In Panel **A** we perform this analysis with the baseline OLS regressions, and in Panel **B** we use the variables Local representative on House Financial Services Committee in 2008/2009, Local representative a Democrat in 2007/2008 election cycle, and CEO compensation as instruments for TARP. The first-stage probit estimations can be found in Table A2 columns (2) and (3). T-statistics are reported in square brackets, based on standard errors clustered by bank. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 6: Additional Deposit Categories

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Term Deposits		Insured Term Deposits		Uninsured Term Deposits		Savings Deposits		Transaction Deposits	
	$\Delta \ln D_{i,t}$	$r_{i,t} - r_{f,t}$	$\Delta \ln D_{i,t}$	$r_{i,t} - r_{f,t}$	$\Delta \ln D_{i,t}$	$r_{i,t} - r_{f,t}$	$\Delta \ln D_{i,t}$	$r_{i,t} - r_{f,t}$	$\Delta \ln D_{i,t}$	$r_{i,t} - r_{f,t}$
Panel A: OLS Estimates										
POST* × *TARP	-2.095***	-0.012***	-2.952***	-0.019***	-1.200***	-0.174***	-0.126	-0.043***	0.882***	-0.004
	[-11.13]	[-2.74]	[-12.10]	[-5.26]	[-4.26]	[-4.42]	[-0.58]	[-8.40]	[4.47]	[-0.69]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.116	0.007	0.102	0.026	0.033	0.016	0.038	0.021	0.028	0.003
No. of obs	206,327	206,027	200,135	206,351	196,410	202,414	205,784	205,804	206,491	206,494
No. of clusters	7,648	7,644	7,643	7,649	7,634	7,635	7,631	7,631	7,650	7,650
Panel B: IV Estimates										
POST* × *TARP	-8.472***	-0.039***	-12.934***	-0.083***	-3.958***	-1.333***	-1.664***	-0.176***	3.860***	-0.033***
	[-14.71]	[-3.08]	[-17.09]	[-7.73]	[-5.34]	[-12.82]	[-3.56]	[-11.19]	[6.72]	[-2.76]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cragg-Donald Wald F statistic	38647.9	38383.8	37336.3	38631.3	37083.3	38051.0	38808.4	38821.8	38637.6	38636.7
Kleibergen-Paap rk Wald F statistic	564.1	557.5	552.6	563.9	560.1	562.1	566.4	566.7	564.8	564.8
Within-$\bar{R}^2$	0.103	0.005	0.079	0.014	0.032	-0.034	0.037	-0.012	0.027	0.001
No. of obs	206,327	206,027	200,135	206,351	196,410	202,414	205,784	205,804	206,491	206,494
No. of clusters	7,648	7,644	7,643	7,649	7,634	7,635	7,631	7,631	7,650	7,650

This table shows analyses on the impact of TARP on different categories of deposit flows and their implicit interest rates, namely term deposits (total, insured and uninsured), savings deposits and transaction account deposits. Panel **A** shows baseline OLS regressions, and Panel **B** shows instrumental variables estimates. T-statistics are reported in square brackets, based on standard errors clustered by bank. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 7: Underlying Channels Analysis

	(1)	(2)	(3)
	Δ Gross Total Assets	Δ Nondeposit Liabilities	Δ Equity
Panel A: OLS Estimates			
POST* × *TARP	-0.633***	-1.496***	0.512***
	[-5.63]	[-3.95]	[4.25]
Bank and Market Controls	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Within-$\bar{R}^2$	0.160	0.008	0.077
No. of obs	206,848	205,943	206,848
No. of clusters	7,662	7,657	7,662
Panel B: IV Estimates			
POST* × *TARP	-2.124***	-0.829	-1.114***
	[-6.92]	[-0.86]	[-3.11]
Bank and Market Controls	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Cragg-Donald Wald F statistic	38690.0	38476.5	38690.0
Kleibergen-Paap rk Wald F statistic	565.1	564.0	565.1
Within-$\bar{R}^2$	0.157	0.008	0.075
No. of obs	206,848	205,943	206,848
No. of clusters	7,662	7,657	7,662

This table explores the different ways in which TARP banks alter their capital structure. Specifically, we look at whether TARP banks' Gross Total Assets (column 1), Nondeposit Liabilities (column 2) and Equity (column 3) change compared to non-TARP banks. In Panel **A** we show the results from baseline OLS regressions and in Panel **B** we show the instrumental variables regressions. T-statistics are reported in square brackets, based on standard errors clustered by bank. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 8: Additional Robustness Tests

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta \ln D_{i,t}$			$r_{i,t} - r_{f,t}$		
	Total Deposits	Insured Deposits	Uninsured Deposits	Implicit Interest Rate	12-month 10K CD	12-month 100K CD
Panel A: Exclude Controls						
POST* × *TARP	-1.819***	-2.377***	-2.234***	-0.057***	-0.082***	-0.053*
	[-9.79]	[-10.19]	[-6.38]	[-7.78]	[-4.45]	[-1.84]
Bank and Market Controls	No	No	No	No	No	No
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.002	0.002	0.000	0.004	0.001	0.001
No. of obs	216,692	210,338	209,533	218,242	168,203	83,190
No. of clusters	7,867	7,847	7,864	7,891	6,520	5,469
Panel B: Exclude Bank Fixed Effects						
TARP	0.967***	1.121***	1.272***	0.045***	0.011	0.012
	[9.32]	[9.18]	[7.49]	[4.48]	[0.49]	[0.39]
POST* × *TARP	-1.301***	-1.837***	-1.389***	-0.074***	-0.086***	-0.062**
	[-10.94]	[-11.98]	[-5.79]	[-10.57]	[-4.42]	[-2.03]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	No	No	No	No	No	No
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.148	0.120	0.018	0.147	0.043	0.055
No. of obs	206,957	200,969	199,921	206,956	162,933	82,290
No. of clusters	7,774	7,770	7,772	7,774	6,583	5,562
Panel C: Remove Involuntary Banks						
POST* × *TARP	-0.602***	-1.432***	-0.255	-0.073***	-0.064***	-0.047
	[-4.65]	[-8.97]	[-0.88]	[-9.97]	[-3.34]	[-1.61]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.167	0.117	0.026	0.051	0.014	0.012
No. of obs	206,666	200,691	199,637	206,665	162,695	82,070
No. of clusters	7,654	7,651	7,653	7,654	6,432	5,425
Panel D: Remove SCAP Participants						
POST* × *TARP	-0.601***	-1.413***	-0.265	-0.071***	-0.061***	-0.041
	[-4.61]	[-8.86]	[-0.91]	[-9.73]	[-3.16]	[-1.39]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.168	0.117	0.026	0.051	0.014	0.012
No. of obs	206,402	200,433	199,379	206,401	162,479	81,860
No. of clusters	7,644	7,641	7,643	7,644	6,424	5,417

(continued on next page)

Table 8: Additional Robustness Tests (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta \ln D_{i,t}$			$r_{i,t} - r_{f,t}$		
	Total Deposits	Insured Deposits	Uninsured Deposits	Implicit Interest Rate	12-month 10K CD	12-month 100K CD
Panel E: Exclude Small Banks (< $ 100 million)						
POST* × *TARP	-0.376***	-1.144***	-0.016	-0.054***	-0.034*	-0.025
	[-2.80]	[-6.80]	[-0.05]	[-8.43]	[-1.69]	[-0.84]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.118	0.051	0.024	0.039	0.010	0.012
No. of obs	133,380	129,398	129,160	133,379	110,814	62,340
No. of clusters	5,471	5,468	5,469	5,471	4,696	4,054
Panel F: Exclude 2008:Q4						
POST* × *TARP	-0.606***	-1.386***	-0.405	-0.078***	-0.061***	-0.043
	[-4.57]	[-8.38]	[-1.42]	[-9.97]	[-3.10]	[-1.40]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.169	0.119	0.026	0.051	0.014	0.012
No. of obs	200,875	194,890	193,851	200,874	158,187	80,827
No. of clusters	7,658	7,655	7,657	7,658	6,432	5,427
Panel G: Exclude 2009:Q3						
POST* × *TARP	-0.666***	-1.395***	-0.233	-0.077***	-0.063***	-0.049*
	[-5.12]	[-8.73]	[-0.81]	[-10.34]	[-3.31]	[-1.69]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.168	0.117	0.026	0.050	0.014	0.012
No. of obs	200,934	200,858	199,810	200,933	158,048	79,766
No. of clusters	7,662	7,659	7,661	7,662	6,432	5,417

This table shows multiple robustness tests for choices regarding control variables, fixed effects, and estimation sample. Panel **A** reports regressions with both bank and time fixed effects but without controls variables. Panel **B** reports regressions without bank fixed effects but with time fixed effects and the bank and market control variables, and in these regressions the TARP dummy is included next to the POST × TARP variable of interest. In Panels **C** - **E** we test whether certain banks drive the main estimations results, and run baseline regressions while excluding the 8 banks that were involuntary recipients of TARP (Panel **C**), the 19 banks that participated in the 2009 Supervisory Capital Assessment Program (SCAP) stress tests (Panel **D**), and small banks with gross total assets below $100 million (Panel **E**). Furthermore, in Panel **F** we remove the crisis quarter 2008Q4, and due to the increase in deposit insurance level and subsequent change in reporting in 2009:Q3 (see Table A1 for more information) we run the baseline regressions while excluding 2009:Q3 in Panel **G**. T-statistics are reported in square brackets, based on standard errors clustered by bank. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 9: TARP Intensive Margin (TARP-iness)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta \ln D_{i,t}$			$r_{i,t} - r_{f,t}$		
	Total Deposits	Insured Deposits	Uninsured Deposits	Implicit Interest Rate	12-month 10K CD	12-month 100K CD
Panel A: Ln(1 + Bailout Amount)						
POST* × *LN(1 + BAILOUT)	-0.036***	-0.085***	-0.014	-0.005***	-0.004***	-0.003*
	[-4.71]	[-8.81]	[-0.83]	[-11.13]	[-3.28]	[-1.85]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.167	0.117	0.026	0.050	0.014	0.012
No. of obs	206,845	200,858	199,810	206,844	162,787	82,158
No. of clusters	7,662	7,659	7,661	7,662	6,437	5,430
Panel B: Bailout / Risk-based Capital						
POST* × *BAILOUT/Risk-Based Capital	-2.849***	-4.388***	-3.456***	-0.210***	-0.145**	-0.092
	[-6.16]	[-6.86]	[-2.81]	[-7.28]	[-2.46]	[-1.39]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.167	0.116	0.026	0.047	0.014	0.012
No. of obs	206,843	200,856	199,808	206,842	162,786	82,158
No. of clusters	7,662	7,659	7,661	7,662	6,437	5,430
Panel B: Bailout / Risk-weighted Assets						
POST* × *BAILOUT/Risk-Weighted Assets	-0.334***	-0.512***	-0.311***	-0.020***	-0.019***	-0.012
	[-9.03]	[-10.25]	[-3.66]	[-6.30]	[-3.12]	[-1.48]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.170	0.114	0.027	0.050	0.014	0.012
No. of obs	197,879	192,201	191,563	197,878	158,693	80,943
No. of clusters	7,426	7,426	7,426	7,426	6,312	5,341

This table explores the intensive margin of the TARP bailouts, by allowing the size of the treatment to vary. The panel shows regressions when defining the TARP bailouts to be equal to (1) Ln(1 + Bailout Amount), (2) Bailout Amount / Risk-based Capital (Tier 1 + Tier 2), and (3) Bailout Amount / Risk-Weighted Assets. We obtain similar results when using Bailout / Gross Total Assets or Bailout / Total Equity. T-statistics are reported in square brackets, based on standard errors clustered by bank. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Table 10: Cross-Sectional Tests

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta \ln D_{i,t}$			$r_{i,t} - r_{f,t}$		
	Total Deposits	Insured Deposits	Uninsured Deposits	Implicit Interest Rate	12-month 10K CD	12-month 100K CD
Panel A: Sample Split by Median Bank Size in 2008:Q3						
Small Banks						
POST* × *TARP	-0.089	-1.254***	1.320	-0.116***	-0.197***	-0.305***
	[-0.22]	[-2.78]	[1.38]	[-3.16]	[-4.70]	[-4.05]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.179	0.148	0.024	0.075	0.016	0.022
No. of obs	103,820	100,931	100,082	103,820	76,143	30,980
No. of clusters	4,547	4,543	4,544	4,547	3,570	2,577
Large Banks						
POST* × *TARP	-0.523***	-1.166***	-0.411	-0.049***	-0.014	-0.019
	[-4.02]	[-6.88]	[-1.39]	[-7.03]	[-0.63]	[-0.61]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.082	0.030	0.018	0.033	0.010	0.011
No. of obs	102,868	99,767	99,568	102,867	86,509	51,074
No. of clusters	4,338	4,333	4,334	4,338	3,740	3,282
Panel B: Sample Split by Median Bank Capitalization in 2008:Q3						
Lowly Capitalized Banks						
POST* × *TARP	-0.398***	-0.942***	0.043	-0.056***	-0.025	-0.011
	[-2.69]	[-5.60]	[0.11]	[-6.48]	[-1.03]	[-0.27]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.072	0.022	0.014	0.058	0.009	0.008
No. of obs	103,486	100,395	99,924	103,485	84,473	42,249
No. of clusters	5,511	5,501	5,497	5,511	4,631	3,573
Highly Capitalized Banks						
POST* × *TARP	-0.616**	-1.864***	-0.060	-0.096***	-0.096***	-0.094**
	[-2.45]	[-5.75]	[-0.11]	[-9.41]	[-2.94]	[-2.06]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.181	0.145	0.029	0.037	0.014	0.015
No. of obs	102,824	99,919	99,342	102,824	77,835	39,427
No. of clusters	5,773	5,762	5,762	5,773	4,695	3,677

(continued on next page)

Table 10: Cross-Sectional Tests (Continued)

	$\Delta \ln D_{i,t}$			$r_{i,t} - r_{f,t}$		
	Total Deposits	Insured Deposits	Uninsured Deposits	Implicit Interest Rate	12-month 10K CD	12-month 100K CD
Panel C: Sample Split by Median Gross Regional Domestic Product per Capita in 2008:Q3						
Low Gross Regional Domestic Product per Capita						
POST* × *TARP	-0.796***	-1.307***	-0.824**	-0.067***	-0.043*	0.002
	[-4.62]	[-5.89]	[-2.00]	[-9.48]	[-1.72]	[0.05]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.172	0.119	0.025	0.054	0.023	0.030
No. of obs	106,530	103,179	102,735	106,529	86,614	41,349
No. of clusters	4,472	4,468	4,468	4,472	3,837	3,051
High Gross Regional Domestic Product per Capita						
POST* × *TARP	-0.374*	-1.452***	0.217	-0.081***	-0.089***	-0.095***
	[-1.95]	[-6.28]	[0.52]	[-6.40]	[-3.12]	[-2.75]
Bank and Market Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Within-$\bar{R}^2$	0.155	0.105	0.025	0.047	0.011	0.009
No. of obs	100,286	97,649	97,043	100,286	76,150	40,791
No. of clusters	4,308	4,305	4,307	4,308	3,499	2,873

This table shows the differential impact of TARP on the supply and demand for deposits by banks based on their observable characteristics by performing split sample regressions. We base the split samples on the median observation of bank characteristics in 2008:Q3, so before the TARP bailouts were in place. Panel **A** shows split sample regressions for large and small banks, based on the size distribution of 2008:Q3. Panel **B** shows split sample regressions for high and lowly capitalized banks based on the simple equity ratio distribution in 2008:Q3. Panel **C** shows split sample regressions for banks that are exposed to mainly to markets with a low or high gross regional domestic product per capita based on the distribution in 2008:Q3. T-statistics are reported in square brackets, based on standard errors clustered by bank. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.

Appendix A

Table A1: Variable Definitions

Variable	Definition	Source
Bailout Variables		
TARP	A dummy variable equal to 1 if the bank received TARP capital support and 0 otherwise.	U.S. Department of Treasury
TARP Repaid Early	A dummy variable equal to 1 for banks that received TARP and repaid early in 2009 or 2010, and 0 otherwise.	U.S. Department of Treasury
TARP Not Repaid Early	A dummy variable equal to 1 for banks that received TARP and did not repaid early in 2009 or 2010, and 0 otherwise.	U.S. Department of Treasury
Ln(1+Bailout Amount)	The natural logarithm of one plus the dollar bailout amount for banks receiving TARP. This is 0 for non-TARP banks.	U.S. Department of Treasury
Bailout Amount / Risk Based Capital	The dollar bailout amount divided by the sum of Tier 1 and Tier 2 regulatory capital.	U.S. Department of Treasury, Call Reports
Bailout Amount / Risk Weighted Assets	The dollar bailout amount divided by the amount of Risk-Weighted Assets.	U.S. Department of Treasury, Call Reports
POST	A dummy variable equal to 1 in the period 2009-2012 and 0 otherwise.	
Deposit Flows and Interest Rates		
Δ *ln (Total Deposits)*	Quarterly log change in bank total deposits from $t-1$ to t.	Call Reports
Δ *ln (Insured Deposits)*	Quarterly log change in insured deposits from $t-1$ to t. Insured deposits are calculated as the amount in deposit accounts under the deposit insurance limit (before 2008Q4 \$100K, afterwards \$250K) plus the number of deposit accounts above the deposit insurance limit multiplied with the insured amount. After 2008:Q4 the insured deposits also include the deposits under the Transaction Account Guarantee (TAG) program which provided unlimited deposit insurance for non-interest bearing transaction accounts. With the increase in deposit insurance limit only being reflected in Call Report items in 2009:Q3, we remove this quarter when calculating the quarterly insured and uninsured growth rates.	Call Reports
Δ *ln (Uninsured Deposits)*	Quarterly log change in uninsured deposits from $t-1$ to t. Uninsured deposits are calculated as the amount in deposit accounts over the deposit insurance limit (before 2008Q4 \$100,000, afterwards \$250,000) minus the number of deposit accounts above the deposit insurance limit multiplied with the insured amount. After 2008Q4 the uninsured deposits are adjusted to exclude the deposits under the Transaction Account Guarantee (TAG) program which provided unlimited deposit insurance for non-interest bearing transaction accounts. With the increase in deposit insurance limit only being reflected in Call Report items in 2009:Q3, we remove this quarter when calculating the quarterly insured and uninsured growth rates.	Call Reports
Implicit Interest Rate	Total interest expense minus services charges on deposit accounts divided by total deposits. In excess of the 1-Year Treasury Constant Maturity Rate (DGS1).	Call Reports, St. Louis FRED Economic Data
12-month 10K CD Rate	The 12 month 10K CD obtained from RateWatch. The rates are assigned to branch offices in the Summary of Deposits using the unique branch identifier *uninumbr*. The branch-level rates are aggregated to the bank-level by weighting with the last known branch deposit share to total bank deposits from the Summary of Deposits. In excess of the 1-Year Treasury Constant Maturity Rate (DGS1).	RateWatch, Summary of Deposits, St. Louis FRED Economic Data
12-month 100K CD Rate	The 12 month 100K CD obtained from RateWatch. The rates are assigned to branch offices in the Summary of Deposits using the unique branch identifier *uninumbr*. The branch-level rates are aggregated to the bank-level by weighting with the last known branch deposit share to total bank deposits from the Summary of Deposits. In excess of the 1-Year Treasury Constant Maturity Rate (DGS1).	RateWatch, Summary of Deposits, St. Louis FRED Economic Data

Table A1: Variable Definitions
(Continued)

Variable	Definition	Source
Additional Deposit Categories		
Δ *ln (Term Deposits)*	Quarterly log change in term deposits from $t-1$ to t. Term deposits are calculated as the sum of insured and uninsured term deposits. Due to a change in the reporting of term deposits in the Call Reports in 2010Q1, we remove this quarter when calculating the quarterly growth rates.	Call Reports
Term deposits rate	Total term deposit interest expense divided by total term deposits. In excess of the 1-Year Treasury Constant Maturity Rate (DGS1).	Call Reports
Δ *ln (Insured Term Deposits)*	Quarterly log change in insured term deposits from $t-1$ to t. Due to a change in the reporting of term deposits in the Call Reports in 2010Q1, we remove this quarter when calculating the quarterly growth rates.	Call Reports
Insured term deposits rate	Total insured term deposit interest expense divided by total insured term deposits. In excess of the 1-Year Treasury Constant Maturity Rate (DGS1).	Call Reports
Δ *ln (Uninsured Term Deposits)*	Quarterly log change in uninsured term deposits from $t-1$ to t. Due to a change in the reporting of term deposits in the Call Reports in 2010Q1, we remove this quarter when calculating the quarterly growth rates.	Call Reports
Uninsured term deposits rate	Total uninsured term deposit interest expense divided by total uninsured term deposits. In excess of the 1-Year Treasury Constant Maturity Rate (DGS1).	Call Reports
Δ *ln (Savings Deposits)*	Quarterly log change in savings deposits from $t-1$ to t. Savings deposits are calculated as the sum of money market deposit accounts and other non-transaction savings deposits.	Call Reports
Savings Deposits Rate	Total savings deposit interest expense divided by total savings deposits. In excess of the 1-Year Treasury Constant Maturity Rate (DGS1).	Call Reports
Δ *ln (Transaction Deposits)*	Quarterly log change in transactions deposits from $t-1$ to t.	Call Reports
Transaction Deposits Rate	Total transaction deposit interest expense divided by total transaction deposits. In excess of the 1-Year Treasury Constant Maturity Rate (DGS1).	Call Reports
Bank and Market Control Variables		
Capital Adequacy	Capitalization ratio calculated as total equity capital divided by gross total assets.	Call Reports
Asset Quality	Ratio of non-performing loans (90 days or more past due and loans that are no longer accruing interest) to total loans.	Call Reports
Management Quality	Ratio of non-interest expenses to total interest income and non-interest income.	Call Reports
Earnings	Ratio of annualized net income to total assets.	Call Reports
Liquidity	Ratio of bank liquid assets to gross total assets	Call Reports
Sensitivity to Market Risk	The absolute difference (gap) between short term assets and short term liabilities divided by gross total assets.	Call Reports
Ln(Total Assets)	Natural logarithm of gross total assets (total assets plus the loan allowance for loan and lease losses and the allocated transfer risk reserve) deflated to 2012Q4 using a GDP deflator.	Call Reports

Table A1: Variable Definitions (Continued)

Variable	Definition	Source
Bank and Local Economic Conditions Variables		
Age	The age (in years) of the bank or the oldest bank owned by the bank holding company based on its reported opening date.	Call Reports
BHC	Dummy variable equal to 1 if the bank is a Bank Holding Company (BHC).	Call Reports
Local Poverty Rate	The U.S. Census county poverty rate, aggregated to the bank-level by weighting with the last known branch deposit share to total bank deposits from the Summary of Deposits.	Summary of Deposits, U.S. Census
Local Median Household Income	The U.S. Census median county household income, aggregated to the bank-level by weighting with the last known branch deposit share to total bank deposits from the Summary of Deposits.	Summary of Deposits, U.S. Census
Gross Regional Domestic Product per Capita	The Bureau of Economic Analysis (BEA) Gross Domestic Product by country, aggregated to the bank-level by weighting with the last known branch deposit share to total bank deposits from the Summary of Deposits.	Summary of Deposits, BEA
Local Deposit HHI	Deposit based Herfindahl-Hirschman Index of local market concentration per county, aggregated to the bank-level by weighting with the last known branch deposit share to total bank deposits from the FDIC Summary of Deposits.	Summary of Deposits
Instruments		
Local representative on House Financial Services Committees in 2008/2009	A dummy variable equal to 1 if the bank is headquartered in a district of a House member who served on the Capital Markets Subcommittee or the Financial Institutions Subcommittee of the House Financial Services Committee in 2008 or 2009.	Center for Responsive Politics, House of Representatives website, Missouri Census Data Center
Local Representative a Democrat in 2007/2008 Election Cycle	A dummy variable equal to 1 if the bank is headquartered in a district where the local representative was a Democrat in the 2007/2008 campaign election cycle.	Center for Responsive Politics, House of Representatives website, Missouri Census Data Center
CEO Compensation ($500k+)	A dummy variable equal to 1 if the bank's CEO had compensation above $500,000 in 2008.	ExecuComp, SEC Edgar DEF14A Filings

Table A2: First Stage Probit Models

	(1)	(2)	(3)
	TARP	TARP Not Repaid	TARP Repaid Early
House Subcommittee on Financial Markets or Capital Markets 2008/2009	0.099***	0.071***	0.156***
	[5.34]	[3.67]	[4.96]
Democrat as Local Representative 2007/2008	0.089***	0.066***	0.056***
	[9.96]	[7.15]	[3.41]
CEO Compensation ($500k+)		0.183***	1.065***
		[8.89]	[41.13]
Bank and Market Controls	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Pseudo- R^2	0.212	0.153	0.315
No. of obs	209,070	209,070	209,070

This table shows the first stage probit models which serve as an input for the instrumental variables approach. The following instruments are used to predict which banks received TARP: *House Subcommittee on Financial Markets or Capital Markets 2008/2009* is a dummy variable equal to 1 if the bank is headquartered in a district of a House member who served on the Capital Markets Subcommittee or the Financial Institutions Subcommittee of the House Financial Services Committee in 2008 or 2009, and *Democrat as local representative 2007/2008* is a dummy variable equal to 1 if the bank is headquartered in a district where the local representative was a Democrat in the 2007/2008 campaign election cycle. Following Berger and Roman (2015), in the second stage 2SLS regression TARP is instrumented by $\widehat{\text{TARP}}$ from the first stage. Moreover, we use *CEO Compensation $500K* to instrument for the early repayment of TARP. *CEO Compensation $500K* is a dummy variable equal to 1 if the bank's CEO had a total compensation greater than $500K, as banks with a high CEO compensation have been shown to be more likely to exit the TARP program (see, e.g. Wilson and Wu, 2012).

Table A3: Comparison Control and Treatment Group, Mahalanobis Distance Matching in 2007:Q4

	Control	Treated	Difference
Capital Adequacy	10.76	10.75	0.01
Asset Quality	0.91	0.95	-0.04
Management Quality	75.83	76.69	-0.86
Earnings	0.13	0.10	0.03
Liquidity	3.27	3.23	0.04
Sensitivity to Market Risk	8.54	9.27	-0.73
Ln(Total Assets)	12.87	13.27	-0.40***
Age	52.94	53.27	-0.33
BHC	0.81	0.83	-0.02
Local Poverty Rate	12.78	12.80	-0.02
Local Median Household Income	50.90	51.99	-1.09
Gross Regional Domestic Product per Capita	47.84	48.18	-0.34
Local Deposit HHI	0.19	0.19	0.00

This table shows the outcome of the matching to obtain a matched sample for analyses in Table 3C. We matched the banks on the above characteristics in 2007:Q4 using Mahalanobis Distance matching, and track the treated and control banks throughout the sample period. The matching procedure could not find a match for all treated banks, so the sample is not balanced. We perform simple *t*-tests to see if the matched banks are similar in 2007:Q4 and report the outcome of the *t*-tests in the last column. * significant at 10 percent; ** significant at 5 percent; *** significant at 1 percent.