Unintended Consequences of Government Bailouts: Evidence from Bank-dependent Borrowers of Large Banks

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1. Introduction

In response to the global financial crisis of 2008, the US Government instituted the Troubled Asset Relief Program (TARP), to reduce the possibility of collapse of the financial system as well as to stimulate economic growth. In this program, the US Government injected capital directly into financial institutions with the twin objectives of protecting them from bankruptcy and to promote lending by these institutions. In a letter to Congressional leaders dated January 15, 2009, Larry Summers, the then secretary of treasury, specifically highlighted that "this program (TARP) must promote the stability of the financial system and increase lending."

However, an important criticism of TARP, and more specifically the Capital Purchase Program (CPP) within TARP, that emerged in the media soon after it was initiated, was that no conditions were imposed on recipients to actually lend the money that they had received from the program. An article in *The New York Times*, published in January 2009, states "Congress approved the $700 billion rescue plan with the idea that banks would help struggling borrowers and increase lending to stimulate the economy, and many lawmakers want to know how the first half of that money has been spent before approving the second half. But many banks that have received bailout money so far are reluctant to lend, worrying that if new loans go bad, they will be in worse shape if the economy deteriorates."[1] Concerned over the effectiveness of TARP, Paul Krugman suggested nationalization of US banks as a potential remedy to stimulate lending.[2] Even Alan Blinder, the former vice chairman of the Board of Governors of the Federal Reserve System and who argued that TARP was a highly successful program in stabilizing the financial system, held a similar view.[3]

[1] M. McIntire, "Bailout is a windfall to banks, if not to borrowers," *New York Times*, January 7, 2009.
[2] Paul Krugman, New York Times Blog titled "What went wrong," July 9, 2012.
[3] Interview of Alan Blinder, published in the Washington Post, Jan 23, 2013.

These criticisms of the TARP program are not without basis. In fact, the aggregate data on lending growth between the third quarter of 2008 and the end of 2010, provided by the US Treasury, clearly show that TARP banks cut their lending relative to non-TARP banks in all size deciles.[4] This is a finding that even the most unfavorable commentators on TARP have not highlighted. One simple explanation for this outcome is that the aggregate data mask heterogeneity across these two bank types and/or their relationship borrowers. Thus, the above-documented results, that is, reduction in aggregate lending by TARP banks, appear to reflect their poorer financial condition or that of their relationship borrowers. We analyze this possibility in detail.

If a decrease in credit supply were indeed found, there remains the puzzle as to the mechanisms through which this could occur. One possibility is that TARP banks disproportionately increased their precautionary savings, or hoarded liquidity, to improve regulatory capital (Acharya, Shin and Yorulmazer, 2011; Giannetti and Simonov, 2013). TARP also imposed a considerable additional regulatory burden on recipient banks. Such regulatory uncertainty could also result in a reduction in lending (see, for example, American Bankers Association, 2011; *Financial Times*, 2014).[5] Thus, instead of stimulating new lending, CPP injections may have led banks to hoard cash and reduce the size of their loan books.

Motivated by the above debates, this paper seeks to generate more conclusive evidence on the effect of TARP beyond the banking system. Our base data set consists of publicly traded firms (CRSP/Compustat) in the US that had loans reported in the LPC Dealscan database in the five-year window prior to TARP. We focus on the six-year window around TARP to facilitate tests

[4] For example, for banks with assets between $10 billion and $100 billion, TARP banks cut their total lending by 11.87%, whereas non-TARP banks cut their lending by 5.99%. Similarly, for smaller institutions, between $1 billion and $10 billion in size, TARP banks cut their lending by 8.91%, whereas non-TARP banks cut their lending by 1.16%. The largest banks (over $100 billion in size), all of which received TARP money, cut their lending by 9.48%.http://www.treasury.gov/initiatives/financial-stability/TARP-Programs/bank-investment-programs/cap/cpp-report/Documents/Quarterly%20CPP%20Report%20Q4%202010.pdf

[5] This effect could persist even beyond the date on which the recipient banks repaid the TARP funds.

similar to a difference-in-difference setting, such as that used in Giannetti and Simonov (2013). Thus, our primary data analysis contains two periods, namely, a pre-TARP period (2005 to 2007), and a post-TARP period (2009 to 2011). Although LPC Dealscan data covers mainly syndicated loans, which is only a portion of total borrowing by publicly traded US corporations, the detailed loan facility-level information enables us to control for demand-side effects, thus isolating the impact of supply side effects, which is our focus (Khwaja and Mian, 2008; Giannetti and Simonov, 2013).

A central variable of our analysis is the classification of a firm's main bank. We use a period of five years from October 2003 to September 2008 to classify whether a given bank is the main bank (that is, main relationship bank) of a given borrowing firm. Using this classification, we classify firms as having exposure to TARP (henceforth, TARP firms) if their main bank is a TARP bank. Our focus on the impact of TARP on dependent borrowers is motivated by the long literature on relationship banking, which strongly suggests the special value of lender–borrower relationships developed over time (see Boot, 2000) for surveys of this area of the literature). This justifies the use of relationship borrowers to examine the effects of bank recapitalizations. As such, due to reduced information asymmetry between relationship borrowers and lenders, any positive effect of bank bailouts is likely to be most pronounced on their relationship borrowers.

First, we test the effect of TARP on the credit supply provided by TARP banks. We follow the approach suggested by Khwaja and Mian (2008) to evaluate this effect. Specifically, we create lender–borrower pairs for each borrower and lender in our sample, and then compute the difference in lending (scaled by borrower total assets) between each borrower–lender pair in the pre-TARP and the post-TARP periods. Using this scaled difference in lending as the dependent variable, and the TARP dummy as the independent variable, we evaluate the impact of TARP on the change in

lending volumes. We find a significant reduction (around 8% in the baseline specification) in the supply of credit by TARP banks to TARP firms in the post-TARP period. With this specification, any demand-side reduction in lending would be captured by the firm fixed effects and would be common to all lenders.

The reduction in credit supply is higher for banks with lower pre-TARP Tier 1 capital ratios, consistent with the liquidity hoarding theory. Further, borrowers whose banks had experienced the largest changes in liquid assets and Tier 1 capital ratios between the pre-TARP and post-TARP periods had the largest reductions in credit supply.

One puzzle may be that the TARP effects that we document, especially regarding the effect on borrowing firms, last well beyond the duration that a given bank was a participant bank. It was widely publicized that several banks exited TARP due to related to limitations on executive compensation for CPP banks. What is not as well recognized is the fact that TARP recipient banks also had additional legal liabilities that extended beyond their bailout repayment date.[6] Such regulatory uncertainty could have the effect of leading to reductions in lending on the part of banks, similar to the effect on lending resulting from any other increase in economic uncertainty. To test this, we examine the likelihood of a future regulatory fine in the post-TARP period compared with the likelihood of being a TARP recipient.

Our evidence shows a strong positive association between these two variables, suggesting that this may represent an important conduit for the reduction in credit supply. To further confirm this, we include the fitted likelihood of being fined in our baseline credit supply regressions. We find that the TARP effect on credit supply is significantly absorbed by the fitted likelihood of being

[6] A report produced for the US Congress in July 2014, by the Special Inspectorate General for the TARP program (SIGTARP), documents several legal actions undertaken against TARP banks well after the date of exit from the TARP program.

fined, suggesting that the likelihood of a fine plays a significant role in the reduction of credit supply. While the TARP bank effect reduces to almost half of its magnitude relative to the baseline, it continues to remain significant, suggesting that the regulatory cost was an important, but not the exclusive channel for the reduction in lending.

Our baseline effects are potentially impacted by unobservable riskiness of TARP banks relative to non-TARP banks, a selection effect. We have two ways in which we address this. First, we repeat the credit supply regressions exclusively for the TARP firms that received the first capital injection (the so-called first round sample). This sample consists of the largest banks in the US. Anecdotally, these first round banks were forced to accept the TARP funding to avoid any stigma effect. Our results are identical in direction and similar in magnitude for this sample. A second way we address this selection effect is to match each TARP bank with a non-TARP bank with likelihood of being a TARP bank. This also does not change our results.[7]

A second potential criticism is that the reduction in lending would have been even larger in the absence of the TARP injection. If this were indeed the case, we should observe that relationship borrowers of these banks should have a positive stock price reaction to the announcement of the TARP bailout of their banks.. In fact, we find the opposite. Specifically, TARP firms suffer a stock valuation loss of around 1.4%, relative to non-TARP firms, in the 3-day window surrounding their main lender's CPP approval announcement. This finding suggests that there are negative effects, resulting from their banks being bailed out that dominate any potential positive benefits to these firms from the CPP program. It also provides confirmation that the reductions in the supply of credit result in a negative effect on their borrowers. If the bailout resulted in a reduction in the magnitude of reduction in credit supply, borrower stock price reaction should have been positive.

[7] In further tests, we also conduct a similar matching on borrower firms having their relationship banks being a TARP banks. We find similar results here as well.

As a final ex-post test of the reduction in credit supply, we use cash flow sensitivity of cash as a measure of financial constraints afflicting TARP firms, and find that cash holdings become more sensitive to cash flow for TARP firms after the implementation of TARP. We also find that TARP firms significantly reduce the scale of their investments in the post-TARP period.

Our paper makes multiple contributions to the academic literature. First, the first papers to examine the real economic effects of TARP on for relationship borrowers of TARP banks. Bayazitova and Shivdasani (2012) study the effect of TARP announcement on the CPP banks themselves and identify a positive impact. We show that a positive impact on the banking sector can co-exist with a negative impact on bank-dependent firms. While TARP achieved its principal objective of stabilizing the financial system, the secondary goal of stimulating lending via capital injections in the financial sector was not achieved for the set of borrowers we examine.

In addition, our paper adds to the literature evaluating the real effects of government financial interventions during periods of crisis. Diamond and Rajan (2002), Diamond (2001), Hoshi and Kashyap (2010) and Giannetti and Simonov (2013) argue that a recapitalization of inadequate size during a crisis may encourage perverse lending policies, and even decrease the supply of credit made available to borrowers with valuable investment opportunities.

Third, our paper is related to the large literature that documents the negative effects of uncertainty on economic activity (Baker, Bloom and Davis, 2016). Related to our findings, Gissler, Oldfather and Ruffino (2016) find that regulatory uncertainty reduced mortgage lending in the US between 2011 and 2013. However, the specific effect of regulatory uncertainty in the form of increased regulatory scrutiny and increased likelihood of fines that accompanies a bailout has received little attention.

Another stream of relevant literature explores stigma from receiving government funding during a crisis. This literature suggests that receiving such funds conveys a negative signal about a bank's financial health (Hoshi and Kashyap, 2010).[8] However, Bayazitova and Shivdasani (2012) show a positive and significant abnormal return for TARP banks around the point of TARP initiation. This suggests that there was a net positive benefit of a bank being a TARP recipient. Can a stigma be consistent with a positive bank valuation on the bailout? Veronesi and Zingales (2012) suggest that TARP funding had a large element of subsidy for big banks. Thus, any negative perception about a given bank's health may be fully offset by the subsidy. Thus, a positive bailout effect on banks can be fully consistent with a positive or negative effect on their relationship borrowers.

Our results are not fully consistent with several papers that find positive economic effects of TARP, such as Berger and Roman (2014) and Li (2014). While these authors focus on aggregate data, we focus on individual bank-borrower data, which allows for finer inferences. On the other hand, the structure of our data (LPC Dealscan) implies that most banks in our sample (TARP and non-TARP) are relatively large. Consistent with this, Berger and Roman (2014) find that most of the positive effects of TARP are concentrated in smaller and mid-size banks, with little benefit for CPP injections in large banks. Thus, our results are more applicable to larger banks, for which the liquidity hoarding motive is likely to be important, as hypothesized in Acharya, Shin and Yorulmazer (2011). Further, risk shifting (Duchin and Sosyura, 2014, Drecshler et al, 2016) is also more likely to be prevalent in large banks, as they have a greater capacity to shift assets to trading

[8] In fact, the Chairman of Signature Bank of New York, one of the very first banks to repay the TARP funds, stated, "We don't want to be touched by the stigma attached to firms that had taken the money." New York Times, April 1, 2009, article titled "Four small banks are the first to pay back TARP funds."

desks. Finally, large banks are very likely to have been the targets of increased regulatory scrutiny due to the political environment in the post-TARP period.

The remainder of this paper proceeds as follows. Section 2 presents the institutional background of TARP. In Section 3, we review the scholarly literature and set forth our hypotheses. Section 4 discusses the data and defines the variables. Section 5 lays out and discusses our empirical findings. Section 6 presents our conclusions.

2. Institutional Background of TARP

The financial crisis of 2008 in the United States started with the collapse of the investment banking giant Lehman Brothers on September 15, 2008. In response, the US Government proposed emergency measures collectively referred to as TARP, with the twin objectives of stabilizing the financial system and promoting lending.[9] Although, prior to the passage of the act, the principal stated objective was to purchase mortgage-backed securities, after its passage, this was quickly changed to a principal objective of recapitalizing the banks.[10] Thus, of all the subprograms in TARP, the recapitalization program (or CPP), was the most prominent, and we use this setting to study the impact of capital injection on lending. CPP attracted a great deal of media attention due to its perceived (or real) favoritism shown toward large Wall Street firms.

As of December 2009, through the mechanism of the CPP, the Treasury had invested $204.9 billion in 707 financial institutions in 48 states. These investments came in stages as the financial crisis developed. The first round of CPP equity injection went to nine financial institutions on

[9] http://www.treas.gov/press/releases/hp1177.htm

[10] "If you remember what the acronym stands for, it's the Troubled Assets Relief Program. It was sold to Congress as a government program that was going to buy up these toxic assets and then sell them back at a profit. Instead Hank Paulson, who was then the secretary of the Treasury, *decided to use it only for injecting capital into banks, largely by buying preferred stocks***.** I didn't agree with the choice, we could have done both, and it kind of gave it a bait and switch aura. He never purchased any troubled assets with it," Alan Blinder in an interview published in *The Washington Post*, Jan 23, 2013.

October 14, 2008, and involved a total injection of $125 billion. These nine institutions were Goldman Sachs, Morgan Stanley, Bank of America, Merrill Lynch, Citigroup, JP Morgan, Bank of New York Mellon, State Street, and Wells Fargo. From October 15 through November 14 of the same year, an additional 53 banks received a further $50.3 billion in CPP capital. Finally, in the period between November 15 and April 24, 2009, 419 additional banks received equity infusions totaling $14 billion. To account for the possibility that the attributes of CPP recipients changed over time, we classify the initial set of 9 institutions as "round 1 institutions;" those who received CPP funding between October 14, 2008, and November 14, 2008 are classified as "round 2 institutions;" and those who received CPP funding after November 14, 2008, are classified as "round 3 institutions."

Under CPP, US Treasury investment in these financial institutions took the form of the purchase of non-voting preferred shares, and the size of each investment was between 1% and 3% of the firm's risk-weighted assets.[11] For the first round of funding, the list of institutions was approved directly by the US Treasury, following private negotiations with the banks in question. This approach was ostensibly followed to avoid any potential stigma effect that otherwise may have resulted in creditor runs for the bailed-out financial institutions. Anecdotal evidence suggests that the US Government applied informal pressure on several financial institutions to accept the CPP funds.

For subsequent rounds, the US Government announced guidelines to apply for CPP capital injections. Specifically, the criteria stated that a financial institution needed to be a US domestic bank, bank holding company, savings association, or savings and loan holding company (SNL), to be eligible. In each case, a formal application was to be submitted to the institution's primary

[11] The maximum threshold was set at the lower of 3% of risk-weighted assets or $25 billion.

regulator, such as the Federal Reserve or Federal Deposit Insurance Corporation, by November 14, 2008. The application would then be subject to an initial review, via the Camels rating system, and successful applications would then be forwarded to the Treasury for final approval. Approved banks received CPP funding in the form of preferred stock, which was designed not to dilute outstanding common shares. Recipient banks were required to pay a 5% dividend to the Treasury on a quarterly basis for the first 5 years, and 9% thereafter. In addition, the Treasury also received warrants, valid for 10 years, to purchase common stock for an amount of up to 15% of the preferred share investment.

All CPP recipients needed to comply with restrictions attached to the program, such as limitations on executive compensation. This may explain why several banks repaid the CPP funds within a few months of receiving their capital injections. For instance, on March 31, 2009, four banks announced repayment of all preferred shares issued to the US Treasury. On June 9, 2009, a further 10 CPP recipient banks announced that they had decided to repay the funds in full and exit the program. The banks in question, including Goldman Sachs, JP Morgan Chase, American Express, and Morgan Stanley, were granted permission to repay a total of $68 billion at that point and thereby free themselves of the restrictions in place under TARP.

3. Hypotheses Development and Related Literature

As mentioned in the introduction, capital injection via TARP had two objectives – to stabilize the financial system, and to increase lending. We focus on the latter objective, specifically examining credit supply changes to relationship borrowers. Our motivation for examining relationship borrowers stems from the fact that there is a long literature in lending that suggests

repeated lending between such borrowers and lenders. Thus, any effects of bank bailout should be the strongest for its relationship borrowers.

We first elaborate the channels through which a positive effect can result. First, the bailout can reduce the cost of funding for the bank. This may occur due to subsidization of the cost of funding (a direct effect) or market participants reducing the cost of funding due to the perception of an implicit government guarantee. If the banks were to pass such benefits on to their clients (Allen et al., 2014; Hubbard et al., 2002), their relationship borrowers would so enjoy a lower cost of funding and an increased supply of credit.

A second channel for a positive effect on the relationship borrowers comes from reduced bankruptcy risk for the bank itself. Slovin et al. (1993) and Megginson et al. (2012) show that bank bankruptcy is a costly event for its relationship borrowers. Since a capital injection reduces the likelihood of bank bankruptcy, this should also be a positive valuation event for relationship borrowers.

On the other hand, capital injections for banks may have non-positive or even negative effects on their borrowers. Theoretically, Diamond and Rajan (2011) point out that capital injections into weak institutions with illiquid assets may increase the risk of fire sales, and aggravate the credit rationing problem. Partial support for this is found in the work of Giannetti and Simonov (2013), who show, in the case of Japan, that only large capitalizations work, and that inadequate capitalizations result in liquidity hoarding. Acharya et al. (2011) show that banks may choose to hold on to added liquidity for potential use in future acquisitions; particularly at times of financial crisis, it may be possible to acquire weaker financial institutions at fire-sale prices.[12]

[12] It is important to note that these negative effects of TARP may persist even after the institutions repay the TARP money. For example, the effects predicted in Diamond and Rajan (2011) and Acharya et al (2011), including the acquisition motive, would persist if the bailout program were to be discontinued after a while. Similarly, the increased regulatory scrutiny would likely last beyond the duration of the time during which the bank received the bailout money, as institutions continue to be liable for their past actions. Therefore, liquidity hoarding, due

Liquidity hoarding could also be induced by increased regulatory cost after a bailout. Anecdotal evidence for a regulatory effect includes several banks exiting TARP, citing a high regulatory burden and governmental interference in their operations as motives. What is not well noted is that this regulatory burden, as well as liquidity hoarding incentives, can persist even after a bank exits TARP.[13] A *Wall Street Journal* article in July 2014, reported that the top-6 banks in the United States experienced a 9.7% reduction in revenue between 2009 and 2013, while non-interest expenses rose by 9.6% in the same period.[14] The article attributes a large part of the increase in expenses to greater regulatory costs. Indeed, J.P. Morgan is reported to have increased its compliance staff by 13,000 employees between 2012 and 2014, despite heavy cuts in the number of employees in other divisions.

In sum, the real economic consequences of TARP on the dependent borrowers of TARP recipient banks remain unclear, which motivates us to study these effects in this paper.

4. Data and Variables

Our main dataset is drawn from the LPC Dealscan database, widely used in studies of banking and relationship lending. Our empirical strategy requires identification of all US borrowing firms with past lending relationships with banks that participated in the TARP program. To achieve this, we start by identifying all borrowing firms available in Dealscan, and then match

to concerns over future shocks either from the economy or from the regulator would continue to impede the transfer of beneficial effects of TARP from banks to bank clients.

[13] As an illustration, in a report to the US Congress in July 2014, SIGTARP (Office of the Special Inspectorate General for the TARP Program) indicated that they pressed charges against over 196 individuals and institutions that had participated in the TARP program. A reading of a number of these reports makes it quite clear that the time-period for several of these alleged offenses was actually prior to the time that the bank concerned accepted the TARP program in the first place. This suggests that institutions that accepted TARP money, regardless of their repayment status, acquired a legal liability that extended beyond the time period during which they were recipients of TARP funding.

[14] A. Johnson, "Bank cost cuts fall short as growth stays tepid: Rising Pay, Regulatory costs offset recent efforts," *Wall Street Journal*, 7 July 2014. Available at: http://www.wsj.com/articles/banks-cost-cuts-fall-short-as-growth-stays-tepid-1404775556

these borrowers to firms in the Compustat and CRSP databases.[15] Financial firms and utility firms are excluded, and we require firms to have financial information available in Compustat from 2005 to 2007. This yields a total of 2,591 distinct borrowing firms, all of which are publicly listed in the US.

Next, we identify key relationship banks for this list of firms, based on all loan facilities originated within a 5-year window between October 2003 and September 2008. In making this classification, we consider only banks that are retained in a lead role in respect of a given loan.[16] A bank is defined as a main bank for a given borrower firm if that institution secures the largest share of lending, by dollar value, of all loans taken out by that borrower within the 5-year window. If two banks have the same share of lending, then both are classified as main banks. For this set of main banks, each is classified as either a TARP bank or a non-TARP bank. A TARP bank is defined as one that received a government equity injection via the CPP, whereas a non-TARP bank is one that received no such funding. Finally, each borrower firm is defined as a TARP firm if its main bank is a TARP bank, or as a non-TARP firm if its main bank is a non-TARP bank.[17] Out of the 2,591 sample firms, there are 1,124 TARP firms and 1,467 non-TARP firms.

Next, we elaborate the screens we use to classify banks as TARP and non-TARP banks for the LPC data set. We start with a dataset of 559 TARP recipient banks, obtained from Bayazitova and Shivdasani (2012), to identify announcement dates for each financial institution that received

[15] We thank Michael Roberts for providing our Dealscan and Compustat link table, which is an updated version as used in Chava and Roberts (2008). Please see Michael Roberts' website for the data: http://finance.wharton.upenn.edu/~mrrobert/styled-9/styled-12/index.html

[16] We use the following roles to classify a bank as a lead bank: 'Agent', 'Arranger', 'Admin agent', 'Lead bank', 'Sole lender', 'Book runner', 'Co-agent', 'Co-arranger', 'Co-lead arranger', 'Co-lead manager', 'Lead arranger', 'Lead manager', 'Managing agent', 'Mandated Lead arranger', 'Mandated arranger', 'Senior lead manager', 'Syndications agent', 'Co-syndications agent'. Following prior literature, we assume that lead banks share the same proportion of the total loan amount in each facility.

[17] For firms with multiple main banks, a firm is classified as a TARP firm if at least one of their main banks is a TARP bank.

funding under the CPP.[18] For banks that received multiple TARP fund injections, we use only the earliest announcement date in the analysis.[19] Out of the 559 TARP recipient banks, we successfully identify TARP approval announcement dates for 393 banks (approximately 70% of the total). Next, we match these 393 banks, using LPC Dealscan, to all banks ever listed as lead banks by historical loan origination. This further reduces the sample of TARP recipient banks to 101, and the sample is further reduced to 89 after manually matching with the Bankscope database. For these 89 banks, we further require: (1) that the institutions in question serve as main bank (as defined) for any facilities between October 2003 and September 2008; (2) that such facilities can be matched to Compustat; (3) that bank characteristics information is available from Bankscope for any year from 2005 to 2007.

This yields a sample of 43 TARP banks that are used as the basis for our analysis. Indeed, as highlighted in the title of paper, these 43 banks are the largest TARP recipient banks. There are 386 non-TARP control banks selected from Dealscan, following similar criteria (1, 2, and 3, as above). In total, there are 429 sample banks. Table 1 presents summary statistics of our sample banks.

Notwithstanding the relatively small number of banks in this sample relative to the total universe, this sample does provide a good representation of the lending effect of TARP for borrowers who use the syndicated loan market, particularly as the market share of these banks is very considerable. Out of the total loan volume of all identified TARP banks within Dealscan, the percentages of total loan volume from these 43 TARP banks are at least 95% in 2005, 95% in 2006,

[18] We thank Bayazitova and Shivdasani (2012) for sharing their data on announcement date of TARP approval for participating banks, and we manually verify the data with Factiva.
[19] We need the announcement dates in conducting announcement effect tests for the borrowing firms, which we use to supplement the credit supply analysis.

and 97% in 2007. Thus, banks that were omitted from our final sample account for only a very small fraction of lending in the syndicated loan market.

Our first measure of a potential benefit (or cost) to borrower firms arising from the TARP injection is simply a dummy for being a TARP firm itself. In addition to the above dataset, we create two additional measures of exposure to TARP that assess the strength of the relationship between a TARP bank and its borrowing firm. These are: (1) the proportion of loans (by dollar value) received by the firm in question from its TARP bank between October 2003 and September 2008; and (2) the proportion of loans (based on the number of loans) provided by the TARP bank to the firm between October 2003 and September 2008. If a firm has relationships with multiple TARP banks, then these measures are added up.

The rationale for these additional measures is that a given borrowing firm may have varying degrees of dependence on its main bank, which would not be captured by the dummy variable alone. Another important advantage of these variables is that they allow for variations in relationship strength between borrowers and banks. Such variation may help to identify differences in exposure to TARP injection within TARP firms.

Table 2 presents summary statistics for our sample firms. Note that financial information is obtained from annual financial filings in Compustat, averaging across the period 2005 to 2007. On average, TARP and non-TARP firms are comparable across many performance, liquidity, and financial structure variables. This suggests that those firms whose banks received TARP funding are quite similar to those firms whose banks did not receive bailout cash injections. Nevertheless, we formally account for possible selection biases in our empirical design.

5. Empirical Results

Before analyzing our sample, it is useful to consider available US Treasury data on aggregate lending by all US financial institutions; these data are segregated into CPP recipient institutions and non-CPP recipient institutions.[20] Within its reports, the Treasury defines financial institutions as belonging to one of four categories: (1) institutions with total assets above $100 billion; (2) institutions with total assets between $10 billion and $100 billion; (3) institutions with total assets between $1 billion and $10 billion; and (4) institutions with total assets of less than $1 billion. The full details for this are present in Online Appendix 1 and 2 that are linked here. (Click Here)

In virtually every size category (with the exception of the top category of banks, valued above $100 billion, where all the institutions are CPP recipients), CPP recipients show negative loan growth relative to non-CPP recipients of a similar size. This pattern is also present in the case of loans in the commercial and industrial category, which are more likely to be loans for public companies, which we seek to examine in greater detail.

Next, we compare regulatory ratios, as well as bank performance measures, for the third quarter of 2008 and fourth quarter of 2010, the last time period for which the data are available. As is the case with the pattern of loan growth, the performance of TARP banks is significantly worse relative to that of non-TARP banks, in this case with the exception of the smallest size category. This holds true for Return on Assets as well as for Return on Equity, which are measured at the end of 2010. A similar pattern is true even for the third quarter of 2008, although the differences are smaller in magnitude. Current losses (measured by the percentage of loans that are charged off), and expected losses (percentage of loans that are not current, that is, are behind on

[20] US Treasury, "Quarterly Analysis of Institutions in the Capital Purchase Program Fourth Quarter 2010." Key information is contained within Exhibits I to VII. Available at: http://www.treasury.gov/initiatives/financial-stability/TARP-Programs/bank-investment-programs/cap/cpp-report/Documents/Quarterly%20CPP%20Report%20Q4%202010.pdf The Treasury discontinued this report after this time period.

payments) are also higher for TARP banks. Thus, TARP recipients are demonstrably in worse financial condition, even at the time of the granting of TARP funds by the US government.

Overall, the US Treasury aggregate summary statistics present a mixed picture. On the one hand, it is quite clear that TARP recipient banks reduced their lending significantly, relative to non-TARP banks, post-bailout. Further, the decreases in lending to industrial and commercial customers, which are more likely to be large publicly traded firms that are part of our sample, are even greater relative to the overall decrease in lending.

On the other hand, some of these differences may be accounted for by the different lending portfolios of TARP and non-TARP banks prior to the bailout, as well as differences in the level of demand for loans by customers of these banks. Our analysis below focuses again on the LPC data sample, for which we have detailed information on the relationship customers of these banks. A significant advantage of our approach is that it allows us to control for the demand-side effects of credit (Khwaja and Mian, 2008; Giannetti and Simonov, 2013). Interestingly, the US Treasury report, cited above, mentions this as one of the important challenges faced in evaluating the impact of TARP on the lending behavior of banks.

5.1. Bank Credit Supply

We illustrate our empirical model by closely following Khwaja and Mian, (2008). Suppose there is a shock on bank *k* at time *t* and the change in the supply of loans from bank *k* to firm *i* at time *t*+1 is defined as $\Delta L_{i,k}$. To examine the change in loan supply in *t*+1 due to the shock on *t*, we typically run the following regression:

$$\Delta L_{i,k} = \beta_0 + \beta_1 TARP\ BANK_k + \eta_i + \varepsilon_{ik} \quad (1)$$

where *TARP BANK*$_k$ captures the shock on bank *k* and η_i represents the loan demand (productivity shock) of firm *i*. However, note that η_i can be unobservable and the estimate $\hat{\beta}^{OLS}$ will be biased if the correlation between the assignment of treatment and borrower-specific loan shock ρ(*TARP BANK*$_k$, η_i) ≠ 0. To completely rule out the demand-side effect, Khwaja and Mian (2008) suggest controlling for firm fixed effect *after first-differencing* the data. In this regard, firm fixed effect would absorb all firm-specific credit demand shocks η_i.

In our setting, *TARP BANK*$_k$ is simply a dummy for whether a given bank is a TARP recipient. Thus, we use the Khwaja-Mian approach to test whether the same firm borrowing from two different banks experiences a larger decline in lending from the TARP recipient bank. Note that the comparison is across banks for the same firm. We can further allow for variations within the TARP bank by introducing the interaction term between *TARP BANK*$_k$ and *BANK_CHAR*$_k$, thus yielding the following empirical specification:

$$\frac{\Delta loan_{i,k,}}{Asset_i} = \beta_0 + \beta_1 TARP\ BANK_k + \beta_2 BANK\_CHAR_k \times TARP\ BANK_k + \beta_3 Lend\_Rel_{i,k} + \eta_i + \varepsilon_{i,k}. \quad (2)$$

The dependent variable is the *change* in lending from bank *k* to firm *i* after implementation of TARP in 2009 (that is, $\Delta L_{i,k}$ in Equation 1). The change in lending is scaled by the pre-TARP total assets of the sample firm. For each sample firm, we create a set of bank–firm pairs from across the 429 sample banks, as defined in the data section. This results in a sample of 1,111,539 (429 × 2,591) firm–bank pairs. Then, for each bank–firm pair, we identify lending activities from Dealscan, classifying loans originated between 2005 and 2007 as pre-TARP lending, while loans originated between 2009 and 2011 are classified as post-TARP lending. Lending activities in 2008

are omitted so as to avoid any interference from the abnormal credit supply due to the onset of the Global Financial Crisis that year.

Note that, for each bank–firm pair, we have only one value for the change in lending subsequent to TARP. By controlling for firm fixed effects in the regression, all firm-level variables are *perfectly collinear* with firm fixed effect (Khwaja and Mian, 2008). A similar approach, taken by Giannetti and Simonov (2013), is to control for *firm × year fixed effect* in panel data.

We report the estimated results in Table 3. In Column 1, the coefficient on the indicator variable *TARP* is negative and significant (-0.002, *se* < 0.001), suggesting a statistically significant reduction in the supply of credit from TARP banks in the post-bailout period (2009–2011). This translates to an approximately 8% (0.002/0.024) reduction in their supply of credit relative to that of non-TARP banks. This finding also suggests that the inference from aggregate data on lending provided by the US Treasury is not driven by a demand-side effect. In Column 2, we find that injection size does not have incremental predictive power on ex-post credit supply reduction. One explanation for this is that injection size in TARP was restricted to a relatively narrow range, between 1% and 3% of banks' risk-weighted assets, meaning that the incremental information contained in the actual size is very limited.

We further explore variations in credit supply stemming from banks' ex-ante and ex-post characteristics.[21] We find that TARP banks with higher regulatory capital (Tier 1 capital ratio) are associated with a lower ex-post reduction in credit supply. However, we find that banks with a larger increase in Tier 1 capital between the pre-TARP and post-TARP periods reduce the supply of credit to a greater extent. In addition, banks with a larger increase in liquid asset ex-post also

[21] Cornett, McNutt, Strahan, and Tehranian (2011) show that banks relied more heavily on core deposit and equity financing continue to lend relative to other banks, whereas banks held more illiquid assets on their balance sheet, in contrast, increased asset liquidity and reduce lending.

reduce the supply of credit much more in the post-TARP period. These results appear to suggest that the reduction in credit supply can be partially attributed to liquidity hoarding by TARP banks. Banks that are healthier in the pre-TARP period have a lower reduction in their supply of credit to borrowers, since they have a lower incentive to indulge in risk shifting or liquidity hoarding.

5.2. Impact of Regulatory Scrutiny

We had earlier presented results that liquidity hoarding by banks was one potential channel that may explain our findings. An conjecture for such liquidity hoarding behavior is that banks anticipate increased monitoring by the US Government as a result of their participation in TARP. To mitigate such regulatory risk, banks may choose to hold more liquidity. We explore this conjecture by examining the effect of TARP injection on the likelihood of a bank being fined by the regulatory authorities in a three-year window after 2008. Bank fine data are obtained from the *Financial Times*, yielding 63 bank fine events involving 27 unique banks in the period 2009 to 2011. From our 429 sample banks, we identify banks for which bank fine data are also available, and run the test according to the specifications set forth in Table 4.

In Columns 1 and 2 of Table 4, we allow for multiple fine events for each bank. We find positive and significant coefficients on the TARP bank dummy (2.069, *se* = 0.261 in Column 1; 1.507, *se* = 0.350 in Column 2). In Column 3, we exclude multiple observations for any single bank, and find qualitatively similar results (1.130, *se* = 0.396).

Overall, our findings suggest that TARP participation increases a bank's likelihood of being fined. We argue that this could be one important reason why TARP banks hoard liquidity, even after receiving such significant government capital injections.

In the second panel of this table, we test if the TARP effect on reduction in credit supply operates solely through the regulatory channel or if there are other alternative channels for the reduction in credit supply. To this end, we use the fitted likelihood of being fined from model 3, Panel A of Table 4as an additional explanatory variable We examine if the TARP effect on credit availability continues to persist after controlling for the fitted likelihood of being fined. Since the specification used is identical to that in Table 3, with the additional explanatory variable, the change in the coefficient on '*TARP bank*' is arguably driven by the fitted likelihood of being fined. We find that use of the likelihood of being fined reduces the coefficient estimate for the TARP effect by almost half (i.e., reduces from −0.002 to −0.001); however, it continues to remain significant. This finding suggests that regulatory uncertainty was an important mechanism through which TARP reduced credit supply; however, other mechanisms such as liquidity hoarding play a role as well.

5.3. Selection Effects in Bank Credit Supply

5.3.1 Propensity Score Matching

The identification strategy in subsection 5.1 relies on changes in the supply of credit from TARP versus non-TARP banks to the same borrower. As a secondary test of TARP effects, we match TARP and non-TARP banks by propensity score matching, where the likelihood of being a TARP recipient bank is modeled. Specifically, for each TARP bank, we find a nearest-neighborhood match, from the pool of non-TARP banks, based on the propensity score, defined as the probability of receiving TARP funds conditional on a set of covariates. We choose covariates for the propensity score following Duchin and Sosyura (2012). These are: bank size; Tier1 ratio;

missing Tier1 ratio[22]; loan-to-asset ratio; ROA; liquid asset ratio; nonperforming loan ratio; leverage; and deposit-to-asset ratio. All these variables are calculated based on mean value from 2005 to 2007.

We conduct both a one-to-one matching method and a one-to-five matching method. The details of the matched sample are presented in Online Figure 1 that is hypelinked here (Click Here). Note that some of the matched control banks are foreign banks, as TARP banks in our sample are among the largest banks in the US. Giannetti and Luc (2012) show that lenders in the syndicated loan market rebalance their loan portfolios in favor of domestic borrowers during the crisis period. In this regard, using foreign banks as a benchmark may lead to an underestimation of the reduction in credit supply by TARP banks. We find a qualitatively similar result in Panel A, Table 5, employing a one-to-one matching method. In Panel B, we conduct a one-to-five matching method and find a consistent result.

One concern may be that TARP firms are systematically different from non-TARP firms. Note that controlling for firm fixed effect in a first difference regression should eliminate such cross-firm variation. Therefore, it is unlikely that this concern affects our result. Nevertheless, we match each TARP firm with a unique non-TARP firm, according to the nearest propensity score estimated by firm size, market-to-book ratio, cash holdings, leverage, ROA, and interest coverage. Then, we re-estimate Equation (2) using this alternative sample and obtain qualitatively similar results, shown in Panel C, Table 5. [23]

[22] This is a relevant variable as several investment banks established commercial bank subsidiaries overnight, with a view toward being able to tap the TARP funds. These banks did not report a Tier 1 capital ratio in the pre-TARP period, as they were not commercial banks during this period. However, many of these investment banks were significant players in the syndicated lending market since the 1990's.

[23] In unreported tables, we further restrict the sample to TARP borrowers only and examine the within TARP firm variations. Using the prior borrowings of a TARP firm from all TARP banks as a proxy for a TARP firm's exposure to the TARP, we find a negative and significant association between the credit supply and the TARP exposure during the post TARP period.

5.3.2 Using only Round 1 TARP Banks

An alternative approach to dealing with unobservable selection effects would be to focus solely on Round 1 TARP banks. As mentioned previously, the set of round 1 banks were chosen by the government to essentially be the largest size banks in the US. Anecdotally, this was decided in a hurried fashion by the US Treasury Secretary Hank Paulson, in an overnight meeting where the banks CEO's had little choice (Calomiris and Khan, 2013). As a result, for this set of banks, selection effects are likely to be less severe since all large banks were forced to accept the bailout money.

We re-estimate our baseline credit supply regression using this sample alone (Panel D). The magnitude of our results is very similar to that using the total sample, suggesting that unobservable riskiness of TARP banks does not drive our results.

5.3.3 Excluding Foreign Banks

As we discussed before, one may concern that most of the control banks are foreign banks. Our findings could be biased due to the differences between foreign banks and domestic banks. In Panel E of Table 5, we delete all foreign banks from the whole sample and re-estimate the main regression. The results show that excluding foreign banks from the sample would not change the inference of our estimation,

5.3.4. Borrower's Stock Price Reaction to TARP

While both of the above tests deal with selection, one could still conjecture that TARP banks would have cut their lending even more if they had not been given capital injections. To test this,

we examine the stock market effect of the TARP injections on the relationship borrowers of the TARP banks. If TARP prevented deeper cuts in lending by TARP banks, then their relationship borrowers' stock prices should react positively to the TARP announcement. If TARP had no impact on lending decisions, i.e., the liquidity hoarding or regulatory costs, would have been present even in the absence of the bailout (unobservable bank characteristics driving both), the announcement return should be insignificantly different from zero. On the other hand, if TARP induced higher liquidity hoarding and/or regulatory costs, and this resulted in the expectation of a lower credit supply to the relationship borrowers, then TARP announcement should result in a negative stock price reaction to the borrowers.

Unlike Norden, Roosenboom and Wang (2013) who examine stock price reactions of relationship borrowers on the actual capital infusion dates, we examine 19 unique event dates for TARP approval announcements relating to the 43 TARP banks in our sample[24]. A 260-day estimation window is implemented, that is, [Day -290, Day -31], and firms are required to have available return data for all working days between day -5 to day +5 in the period immediately surrounding the announcement.

We construct the sample based on all the matched borrowing firms with lending relationships available, that is, our sample of 2,591 borrowing firms (TARP and non-TARP firms). The control sample consists of all observations relating to all non-TARP firms available in LPC on the same date.

We use this control sample to account for unobservable heterogeneity in the sample of firms that access the syndicated loan market, that is, the firms present in the LPC Dealscan database.

[24] Arguably, the information about TARP recipients will be immediately released to the market upon approval announcements. Since the actual capital infusion dates are much later, the event study based on these infusion dates may capture the announcement drift or reversal.

This set of firms may be more bank-dependent compared to other firms. By including only such firms in the analysis, we control for any such effect. Later in this section, we also use the CRSP-Compustat sample (except for the TARP firms) as control firms and find similar results.

Table 6, Panel A provides univariate results for cumulative abnormal returns (CARs) for TARP and non-TARP firms in the period after the bank bailout. Our results show consistently negative CARs for TARP firms across different event windows and different model specifications. In Panel A, with an adjusted market model, we find that TARP firms experience an average negative CAR of 3.01% over the three-day window around bank bailout announcement. Although non-TARP firms also experience a significant and negative CAR of 1.33% in the same period, the magnitude of CAR reduction is significantly smaller than for TARP firms. If we use a 21-day window [-10, +10] moreover, to account for potential information leakage of the identities of TARP banks, the return becomes even more negative for TARP firms, while it becomes a small positive number for non-TARP firms. If we use the Fama–French four-factor model, we find that, within the 21 day window, there is a much greater negative return of -3.58% for TARP firms, compared to -0.35% for non-TARP firms. Across most specifications, TARP firms have a significantly larger negative return relative to non-TARP firms.

We conduct multivariate analyses of CAR in Panel B of Table 6. The dependent variable is CAR [-1, +1] of firm *i* around its bank's approval event. We include industry fixed effects as well as firm characteristics that may potentially amplify a firm's reaction to the TARP announcement.

In Columns 1 and 2 of Panel B, we show consistently negative and significant coefficients on the *TARP_EXP* indicator, with a magnitude of around -1%. Firm leverage has a large negative impact on abnormal return, and firm size has a consistent positive effect on this abnormal return. Next, we substitute the exposure dummy, with exposure variables measured by the proportion of

past loan amounts (numbers) with TARP banks to total loan amounts (numbers) from all banks. Consistent with our finding in the dummy measure, this estimation from the OLS regression reflects a -1.5% sensitivity in terms of *CAR* [-1, +1] to a firm's exposure to TARP injection, measured by past lending relationship amount. A similar level of coefficient obtains when the relationship measure is based on number exposure.

Further, we examine whether there is any heterogeneity in announcement effects across the several rounds of TARP injections by dividing our sample into three subgroups, according to the bailout announcement date of the financial institution in question. Round 1 includes all sample observations relating to TARP announcements on October 14, 2008. This includes eight banks[25], namely, Bank of America, Citigroup, BNY, Wells Fargo, State Street, JP Morgan, Morgan Stanley, and Goldman Sachs. In the OLS regression, shown in column 5, interaction terms between TARP and round dummies are added. A significant valuation loss is found for firms that were relationship firms of banks involved in the round 1 and round 2 bailouts, suggesting that the negative price reaction to main bank TARP approval announcement is not driven by involvement in a particular round of TARP injection, but associated with the entire program.

The round 1 results are particularly pertinent in this respect, because round 1 TARP banks had no selection biases (anecdotally, all the top 10 banks by assets were forced to accept the government funding). This again demonstrates that selection effects do not drive our results. The insignificant effect on the firm valuation of those companies associated with round 3 funding being paid to their banks is partially due to the limited number of observations of TARP firms (that is, 23 observations) in this case.

[25] For the purpose of examining ex-post real effects, we consider Merrill Lynch and Bank of America both as Bank of America.

In the final specification, we use the entire CRSP-Compustat sample as the reference against which to compare the abnormal returns of TARP firms. The magnitude of the coefficient (-1.1%) in this case is similar to that relative to other LPC Dealscan firms, which further confirms that our results are not dependent on sample selection.

In Panel C of Table 6, we repeat the tests using several additional subsamples. In Columns 1 and 2, subsamples are obtained from methods of one-to-one matching, and one-to-five matching at the bank level. In Column 3, a subsample is obtained from the method of one-to-one matching at the firm level. Overall, the results are qualitatively the same as those in Panel B.

5.3.5. Are the Measured Bank Effects Because of Borrower Credit Risk?

An alternative explanation for the above findings is that TARP borrowers could have a higher degree of credit risk relative to non-TARP borrowers. In this case, the reduction in lending by TARP banks to their relationship borrowers could reflect the higher risk of these borrowers. Note that our identification strategy does account for this, since the higher credit risk should result in cuts in lending to these borrowers by both TARP and non-TARP banks.

Nevertheless, to confirm that credit risk is not driving the results, we examine the time series of credit risk of TARP and non-TARP firms using two popularly used measured – the Altman Z score and the option implied distance to default. For estimating distance to default, we use the Moody's KMV method using short-term debt + ½ of the long-term debt in the liabilities. The actual data is obtained using the Global Default Database collected by the Credit Research Initiative.[26] For both variables (Z score and Distance to Default), a higher value implies a lower default probability.

[26] See http://rmicri.org/cms/about/cri/.

Figure 1 presents this time series of both variables for the two sets of firms. Unsurprisingly, both sets of firms have an increase in the risk of default during the credit crisis. Using distance to default, TARP firms almost always have a lower probability of default relative to non-TARP firms. Using Z-score, the same pattern continues to hold for the crisis period. Given this figure, the negative allocation of banks Therefore, the reduction in credit allocation is unlikely to be explained by increased credit risk of TARP firms.

5.3.6. Are the Measured Bank Effects Because of Withdrawals of Line of Credit?

One may further argue that TARP borrowers are able to withdraw more line of credit, and therefore have a weaker demand for new loans. However, withdrawals of the line of credit would inevitably result in an increase in bank debt in a borrower's balance sheet. In the unreported tables, we find that the ratio of bank debt to total debt decreases rather than increases in the post-TARP period. This finding suggests that withdrawals of the line of credit unlikely drive the reduction in credit supply by TARP banks. [27]

In summary, the results from this entire section suggest that main banks' participation in TARP is perceived as "bad news," which is consistent with our finding on the reduction in credit supply from TARP banks to their dependent borrowers.

5.4. Real Effects of Reduction in Bank Credit Supply: A Perspective from Dependent Borrowers

Thus far, we employ the methodology of Khwaja and Mian (2008) to establish the negative supply side effect on bank credit due to TARP. The event study results presented in the previous section suggest that this was negative news to the borrowing firms. Here, we quantify two potential

[27] In particular, we find that the coefficient on the interaction term between Exposure to TARP indicator and Post TARP indicator is negative and significant (−0.017, *se* = 0.007).

real effects of the reduction in credit supply, thus providing further evidence the possible channels that caused the market to react negatively. Our underlying economic rationale follows Chava and Purnanandam (2011) who articulate that the adverse shocks to banks would affect their borrowers' performance negatively via the 'credit channel'. To this end, we direct examine the TARP effects using borrowing firms (as opposed to lender-borrower pair) as the unit of observation.

First, we focus on the real effects, using a model as specified as Equation (3):

$$Real\ Effects = \beta_0 + \beta_1 TARP\_EXP_k + \beta_2 Post_t \times TARP\_EXP_k + \gamma F_i + \varepsilon_{i,t}. \quad (3)$$

There is an important difference between the empirical specification in this section and that in the previous section. Here, we focus on TARP bank-dependent firms (demand side) instead of TARP banks (supply side). To this end, we develop multiple measures to proxy for *exposure* of a given borrower to TARP banks. The first measure is an indicator variable that equals to 1 if the at least one main bank of the firm is a TARP bank. The second measure is the continuous variable, which is equal to the proportion of the loan amount obtained from all TARP banks to the total borrowing amount over the period October 2003 to September 2008. The third measure is the proportion of the number of loans obtained from all TARP banks to the total number of borrowings over the period October 2003 to September 2008.

First, we directly test the real effect of a reduction in credit supply on borrowing firms' financial constraints via the credit channel. Two factors are tested in particular: cash flow sensitivity of cash and investment. Almeida et al. (2004) suggest that firms that are financially constrained are more likely to save cash out of cash flow. These authors argue that cash flow sensitivity of cash is free from the Kaplan and Zingales (1997) criticism, and better captures financial constraints, because the explanatory power of cash flow in determining cash policy is

less likely to link to future investment demand. Following their argument, we investigate whether these TARP firms become more financially constrained following their main banks' participation in TARP, using a measure of cash flow sensitivity to cash. The following model is employed:

$$\Delta Cash\ to\ asset_{i,t} = \beta_0 + \beta_1\ TARP\_EXP_i + \beta_2\ Post_t \times TARP\_EXP_i + \beta_3\ TARP\_EXP_i \times Cashflow_{i,t} + \beta_4\ TARP\_EXP_i \times Post_{i,t} \times Cashflow_{i,t} + \beta_5\ Post_{i,t} \times Cashflow_{i,t} + \beta_6\ Cashflow + \gamma F_i + \varepsilon_{i,t}. \quad (4)$$

The dependent variable in each case is yearly change in cash scaled by lagged value of total assets. The coefficient on the triple interaction term (β_4) is the variable of interest, and our hypothesis predicts a positive coefficient. In Columns 1 to 3 of Table 7, we use different measures of exposure to TARP and find positive and significant coefficients on the triple interaction term, suggesting that TARP firms become more financially constrained in the post-bailout period. Other firm characteristics, such as market-to-book ratio, leverage, and size, are controlled for in the regressions. Industry and year fixed effects are also added to account for potential omitted variable problems.

In Columns 4 to 6 of Table 7, we repeat the above tests with subsamples, obtained via propensity score matching at the main bank level and firm level, respectively. Across the subsamples, we find that TARP firms experience significant increases in cash flow sensitivity of cash in the post-TARP period, whereas control firms experience insignificant changes in cash flow sensitivity of cash in the post-TARP period. The findings across the subsamples reinforce the earlier evidence from the full sample suggesting that TARP firms become more financially constrained after government capital injections to their main banks.

Next, we examine the investment levels of TARP firms, as an alternative indicator of financial constraint. In this case, our empirical specification is based on the Q-theory, which suggests that investment is a function of marginal Q ratio, which can be proxied by market-to-book ratio. We augment the model with firm-specific financial variables, such as internal cash flow (Fazzari et al., 1988), and value of investment in the preceding year. Year and firm fixed effects are used to account for unobservable time and firm heterogeneity. Firm investment level is measured using capital expenditure, scaled by net property plant and equipment in the preceding year. We estimate the following equation:

$$CAPEX_{i,t} = \beta_0 + \beta_1 CAPEX_{i,t-1} + \beta_2 TARP\_EXP_i + \beta_3 Post_{i,t} \times TARP\_EXP_i + \gamma F_i + \varepsilon_{i,t}. \quad (5)$$

Three measures of firm exposure to TARP are adopted in the estimations. The variables chosen to reflect overall firm characteristics were selected based on the literature in this area. For example, the selection of cash flow is motivated by the extensive literature showing an association of cash flow with investment, which is usually interpreted as evidence of the way in which financial constraints impact firm investment. Firms with high leverage are also likely to be more financially constrained or distressed. Firm size is posited to be inversely related to financial constraint, while ROA is deemed an alternative proxy for future growth opportunities, although a high ROA could also mean that the firm has more cash at its disposal and is, therefore, less financially constrained. Under both interpretations of ROA, one should expect a positive impact on investment.

Table 8 sets out the results relating to firm investment levels. In Columns 1 to 3, point estimates of the coefficient on the interaction term $Post_{i,t} \times TARP\_EXP_i$ are negative and significant at the 10% level across all three specifications, suggesting that TARP firms reduce their level of

capital investment in the period following their main bank's receipt of TARP funds. The coefficient is -1.6% with the dummy measure of exposure to TARP, which translates into a 24% (0.016/0.067) reduction in capital investment. We argue that these results support the conjecture that TARP firms reduce investment activity in this period because they became more financially constrained.

In addition, the coefficients on market-to-book are all positive and significant. The positive and significant coefficient for cash flow reflects the fact that firms are sensitive to cash flow fluctuations, suggesting that financial frictions do play a role in determining firm investment.

Subsample robustness checks for this finding are provided in Columns 4 to 6 of Table 8. These subsamples are constructed using propensity score matching at the bank level (matched one-to-one and one-to-five). In both cases, the coefficients of the interaction term between TARP and the post dummy are negative. The significance level of the point estimates increases from 5% to 1% in the one-to-five matched subsample. However, for the propensity score matching at the firm level, the coefficient estimate becomes insignificantly negative.

Thus, the results set out in Table 8 suggest that firms significantly reduced their level of capital investment in the period after their main banks commenced participation in TARP. Together with the previous findings on cash flow sensitivity, this suggests that TARP firms increased financial constraints in the post-TARP period, despite the fact that their main bank received a significant capital injection.

6. Conclusion

This paper investigated the effect of TARP on the relationship firms of recipient banks. We found that TARP banks reduced the supply of credit to their customers after the bailout. This

reduction was more pronounced when the TARP banks are associated with worse levels of ex-ante profitability and in cases where there is more ex-post increase in their liquidity assets. In addition, we highlight the role of regulatory uncertainty in the form of heightened scrutiny and possibility of finance, a topic not studied extensively in the prior literature.

Our results highlight the fundamental contradiction in regulatory objectives of the bailout of banks during a financial crisis. On one hand, regulators would like to see banks and the entire banking system stabilized, and one way for the banks themselves is to hoard liquidity during the crisis. On the other hand, regulators would like banks to pass on the benefits of the bailout to the non-financial system, which requires banks not to hoard cash. Purely from a liquidity perspective, these objectives are fundamentally opposed to each other for a bank receiving the bailout money.

Our results contrast with papers that identify positive economic effects from the TARP bailout. For example, Berger and Roman (2014) indicate a positive effect of TARP on local economic conditions. Li (2014) finds a beneficial effect on the aggregate supply of bank loans. Both these papers use the political connections of TARP banks as an instrument to avoid the endogeneity concern that TARP bailout likelihood could be correlated with future economic growth or loan supply. However, there are potential problems with this approach. It is possible, for instance, that politicians could equally direct other bailout mechanisms to assist their constituencies. For this reason, it is difficult to rule out the possibility that there were other political factors at work, apart from TARP, that resulted in improvement in local economic conditions. More importantly, the demand-side effect could not be properly controlled for in these studies given limitations in the data used.

By contrast, we use individual loan and borrower relationships as our identification strategy. Thus, our tests rely on the heterogeneity of borrowers within TARP banks; the actual likelihood

of being a TARP recipient is far less a concern in our tests. Moreover, consistent with our results, even Berger and Roman (2014) find that most of the positive effects of TARP are associated with smaller and mid-size banks, with little beneficial effect on the lending of larger banks. In contrast, the structure of our data implies that most of our banks are larger, as these are the banks that are active in the syndicated lending market. Thus, our results are more applicable to larger banks, for which the liquidity hoarding motive is likely to be important, as hypothesized in Acharya, Shin and Yorulmazer (2011). Further, risk shifting is more likely to be prevalent in large banks, since they have greater capacity when it comes to shifting assets into trading desks.

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Table 1. Descriptive Statistics of Sample Banks

This table reports the descriptive statistics of 429 sample banks. Out of 429 sample banks, 43 are TARP banks, while 386 are non-TARP banks. We require sample banks to have served as lead arranger for facilities in Dealscan between October 2003 and September 2008. Further, we manually match these banks with Bankscope to obtain their financial information. Panel A and Panel B report the descriptive statistics for TARP banks and non-TARP banks, respectively. All these variables are calculated based on the mean value over the period 2005-2007. Detailed definitions of variables are presented in Appendix 1.

	N	Mean	Std. Dev.
Panel A: TARP Banks			
Size	43	10.01	2.33
Tier 1 Capital	39	9.91%	2.74%
Loan-to-asset ratio	43	61.97%	20.01%
ROA	43	1.13%	0.60%
Nonperforming loan to total loan	40	0.70%	0.77%
Margin	43	3.31%	1.09%
Loan loss provisions	42	0.47%	0.60%
Liquid asset ratio	43	10.30%	12.10%
Cash to asset	43	2.80%	1.73%
Leverage	43	89.70%	4.38%
Deposit-to-asset ratio	42	63.90%	19.20%
Panel B: Non-TARP Banks			
Size	386	9.65	2.59
Tier 1 Capital	304	11.40%	5.52%
Loan-to-asset ratio	380	54.87%	19.03%
ROA	381	0.98%	0.88%
Nonperforming loan to total loan	322	2.69%	2.83%
Margin	374	2.28%	1.20%
Loan loss provisions	362	0.50%	0.63%
Liquid asset ratio	386	18.30%	14.20%
Cash to asset	353	2.62%	2.78%
Leverage	386	90.50%	6.86%
Deposit-to-asset ratio	367	66.70%	22.20%

Table 2. Summary Statistics of Sample Firms

This table reports summary statistics for the sample firms. Financial characteristics reported are the average figures for 2005–2007. Financial and utility firms are excluded in the sample. Panel A and Panel B report descriptive statistics for TARP firms (treatment) and non-TARP firms (control), respectively. Financial information is obtained from Compustat Annual Fundamental, and bank debt/asset, bank debt/total debt, and undrawn credit line ratio figures are calculated from information obtained from Capital IQ. Detailed definitions of variables are provided in Appendix 1.

	N	Mean	Std. Dev.
Panel A: TARP Firms			
Total asset	1,124	4,752	16,663
Market value of equity	1,124	5,442	17,976
Market-to-book	1,098	2.09	1.19
Cash/asset	1,123	14.00%	12.70%
Market leverage	1,123	0.16	0.18
Book leverage	1,123	0.20	0.20
Working capital ratio	1,109	2.74	2.05
Interest coverage	995	82.62	386.90
R&D/asset	1,124	0.05	0.09
Capex/asset	1,121	0.07	0.08
ROA	1,121	0.07	0.17
COGS/asset	1,123	0.71	0.67
Sale growth	1,117	1.20	0.32
Bank debt/asset	467	15.30%	27.40%
Bank debt/total debt	458	64.05%	37.60%
Undrawn credit line ratio	101	56.00%	30.40%
Panel B: Non-TARP Firms			
Total asset	1,467	5,362	14,009
Market value of equity	1,467	6,282	15,986
Market-to-book	1,420	1.90	1.01
Cash/asset	1,461	8.76%	8.70%
Market leverage	1,465	0.21	0.18
Book leverage	1,465	0.25	0.19
Working capital ratio	1,429	2.09	1.25
Interest coverage	1,407	55.18	273.80
R&D/asset	1,467	0.02	0.05
Capex/asset	1,467	0.06	0.07
ROA	1,466	0.14	0.10
COGS/asset	1,467	0.80	0.64
Sale growth	1,464	1.17	0.28
Bank debt/asset	937	12.00%	18.60%
Bank debt/total debt	924	46.17%	36.77%
Undrawn credit line ratio	212	68.90%	28.20%

Table 3. Supply of Credit and TARP Injection

Table 3 reports the effect of TARP participation on banks' supply of credit to firms. The sample period starts from 2005 and ends in 2011 (2008 is excluded). We first create a panel with bank–firm pairs, out of 2,591 sample firms and 429 sample banks. Next, we aggregate total lending amount from bank k to firm i in 2005-2007 and in 2009-2011, respectively. The dependent variable is equal to $\Delta\ Loan_{i,k,(t,t-1)} / Asset_{t-1,}$ where *t* refers to post-TARP period (2009-2011), and *t*-1 refers to pre-TARP period (2005-2007). We scale the change in loan amount by the firm's average total asset level in 2005-2007. The key independent variable is TARP dummy, which equals 1 if the bank is a TARP recipient bank, and zero otherwise. To control for demand-side effects, we follow Khwaja and Mian (2008), which covers firm fixed effects. Robust standard errors standard errors are computed using within-firm clustering. Detailed variable definitions are given in Appendix 1. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively. Standard errors are reported in parentheses.

	$\Delta\ Loan_{i,k,(t,t-1)} / Asset_{i,t-1}$					
	(1)	(2)	(3)	(4)	(5)	(6)
TARP bank	**-0.002*** **	**-0.002*** **	**-0.008*** **	**-0.001*** **	**-0.002*** **	**-0.001*** **
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Injection size		0.002				
		(0.003)				
TARP bank × Tier 1 ratio			**0.058*** **			
			(0.005)			
Tier 1 ratio			0.000			
			(0.000)			
TARP bank × Δ Tier 1 cap				**-0.002*** **		**-0.002*** **
				(0.000)		(0.000)
Δ Tier 1 capital				-0.000***		-0.000***
				(0.000)		(0.000)
TARP bank × Δ Liquid asset					**-0.010*** **	**0.001**
					(0.002)	(0.002)
Δ Liquid asset					0.001***	0.000
					(0.000)	(0.000)
Lend relationship (Amt)	-0.638***	-0.638***	-0.616***	-0.623***	-0.642***	-0.623***
	(0.021)	(0.021)	(0.022)	(0.022)	(0.021)	(0.022)
Constant	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Obs.	1,111,539	1,111,539	888,713	766,936	1,036,400	766,936
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Cluster at firm level	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-square	0.231	0.231	0.230	0.235	0.234	0.235

Table 4. TARP Injection and Likelihood of Bank Fine

This table provides results of the effects of TARP injection on banks' subsequent likelihood of being fined. The sample period runs from 2009 to 2011, and the sample includes all sample banks. The dependent variable equals 1 if a bank is fined by the US government, and zero otherwise. Other controls include bank characteristics, such as bank size, Tier 1 ratio, loan-to-asset ratio, ROA, NPL ratio, and liquid asset ratio. Since some banks experience multiple fines, in columns one and two, we include multiple fines as observations. In column three, we delete duplicate observations for particular banks. In column four, we use OLS regression instead and re-run on the sample, which excludes multiple fine observations. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A

		Fine=0/1	
	Probit	Probit	Probit
Variables	(incl. multiple fines)	(incl. multiple fines)	(excl. multiple fines)
TARP bank	**2.069*** **	**1.507*** **	**1.130*** **
	(0.261)	(0.350)	(0.396)
Ln(bank size)	0.545***	0.415***	0.300***
	(0.088)	(0.081)	(0.080)
Tier 1 ratio		-0.078	-0.070
		(0.087)	(0.085)
Loan/Asset		-0.008	-0.013
		(0.014)	(0.016)
Bank ROA		8.603**	6.743**
		(3.439)	(3.367)
NPL ratio		-19.420	-21.470
		(18.072)	(22.294)
LIQ/Asset		2.780*	1.739
		(1.455)	(1.525)
Constant	-8.315***	-5.907***	-4.166***
	(1.176)	(1.638)	(1.530)
Obs.	460	345	328
Pseudo R-square	0.566	0.596	0.422

Panel B

		$\Delta\ Loan_{i,k,(t,t-1)} / Asset_{i,t-1}$	
VARIABLES	(1)	(2)	(3)
TARP bank	**-0.0020*** **		**-0.0010*** **
	(0.0002)		(0.0001)
Δ(Implied fine prob)		**-0.0277*** **	**-0.0225*** **
		(0.0037)	(0.0036)
Other Controls (As in Table 3)			
Observations	761,754	761,754	761,754
Firm FE	Yes	Yes	Yes
Cluster at firm level	Yes	Yes	Yes
Adjusted R-squared	0.2360	0.2361	0.2362

Table 5. Robustness Tests on Supply of Credit by TARP Banks

Table below provides robustness tests on three sets of subsamples. Robust standard errors are computed using within-firm clustering. Regression specifications follow Table 3. The "*Other controls"* includes *Tier 1 ratio, Δ Tier 1 capital, Δ Liquid asset* and *Lend relationship (Amt)*. The five sets of subsamples include: (1) one-to-one matching on bank-level characteristics; (2) one-to-five matching on bank-level characteristics; (3) one-to-one matching on firm-level characteristics; (4) round 1 banks; (5) excluding foreign banks. All specifications include firm fixed effects and other control variables as in Table 3. Please refer to online appendix for details on matching.

Panel A: 1:1 Matching on Bank Level

	$\Delta\ Loan_{i,k,(t,t-1)} / Asset_{i,t-1}$					
	(1)	(2)	(3)	(4)	(5)	(6)
TARP bank	**-0.003*** **	**-0.003*** **	**-0.009*** **	**-0.001*** **	**-0.002*** **	**-0.001*** **
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Injection size		0.003				
		(0.005)				
TARP bank × Tier 1 ratio			0.068***			
			(0.007)			
TARP bank × Δ Tier 1 cap				-0.002***		-0.002***
				(0.001)		(0.001)
TARP bank × Δ Liquid asset					-0.012***	0.001
					(0.004)	(0.003)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	755,469	755,469	604,023	521,256	704,400	521,256
Adjusted R-squared	0.246	0.246	0.246	0.248	0.247	0.248

Panel B: 1:5 Matching on Bank Level

	(1)	(2)	(3)	(4)	(5)	(6)
TARP bank	**-0.003*** **	**-0.003*** **	**-0.010*** **	**-0.002*** **	**-0.002*** **	**-0.002*** **
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Injection size		0.001				
		(0.004)				
TARP bank × Tier 1 ratio			0.077***			
			(0.007)			
TARP bank × Δ Tier 1 cap				-0.003***		-0.003***
				(0.001)		(0.001)
TARP bank × Δ Liquid asset					-0.013***	0.001
					(0.003)	(0.003)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	818,961	818,961	654,787	565,064	763,600	565,064
Adjusted R-square	0.235	0.235	0.232	0.236	0.238	0.236

Panel C: 1:1 Matching on Client Firm Level

	$\Delta\ Loan_{i,k,(t,t-1)} / Asset_{i,t-1}$					
	(1)	(2)	(3)	(4)	(5)	(6)
TARP bank	-**0.002***** (0.000)	-**0.003***** (0.000)	-**0.008***** (0.001)	-**0.001***** (0.000)	-**0.002***** (0.000)	-**0.001***** (0.000)
Injection size		0.003 (0.004)				
TARP bank × Tier 1 ratio			0.061*** (0.006)			
TARP bank × Δ Tier 1 cap				-0.002*** (0.001)		-0.002*** (0.001)
TARP bank × Δ Liquid asset					-0.009*** (0.003)	0.001 (0.002)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	849,849	849,849	679,483	586,376	792,400	586,376
Adjusted R-square	0.235	0.235	0.233	0.240	0.239	0.240
Panel D: Round 1 Banks Only						
TARP bank	-**0.003***** (0.000)	-**0.003***** (0.000)	-**0.008***** (0.001)	-**0.002***** (0.000)	-**0.003***** (0.000)	-**0.002***** (0.000)
Injection size		0.006 (0.006)				
TARP bank × Tier 1 ratio			0.059*** (0.007)			
TARP bank × Δ Tier 1 cap				-0.002** (0.001)		-0.002*** (0.001)
TARP bank × Δ Liquid asset					-0.007* (0.004)	0.009** (0.004)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	555,555	555,555	444,185	383,320	518,000	383,320
Adjusted R-square	0.267	0.267	0.274	0.277	0.269	0.277
Panel E: Exclude Foreign Banks						
TARP bank	-**0.002***** (0.000)	-**0.002***** (0.000)	-**0.009***** (0.001)	-**0.001***** (0.000)	-**0.002***** (0.000)	-**0.001***** (0.000)
Injection size		0.002 (0.003)				
TARP bank × Tier 1 ratio			0.067*** (0.005)			
TARP bank × Δ Tier 1 cap				-0.003*** (0.000)		-0.003*** (0.000)
TARP bank × Δ Liquid asset					-0.013*** (0.002)	0.002 (0.002)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	349,785	349,785	292,783	264,282	323,875	264,282
Adjusted R-square	0.249	0.249	0.248	0.254	0.252	0.254

Table 6. Stock Price Reactions to TARP

This table reports abnormal returns of firms around the TARP approval announcement date for their respective banks. In Panel A, we report descriptive statistics for univariate analysis first. The treatment group includes firms that have their main bank receive approval for TARP on a certain event date, whereas the control group includes firms that have none of their main banks receiving approval for TARP on the specific event date. The median of cumulative abnormal returns (CARs) are reported. We require sample firms to be non-financial and non-utility firms. 3-day, 21-day, and 12-day event windows are implemented in computing CAR. We use a 260-day estimation window, that is, [Day -290, Day -31] and require firms to have non-missing returns on all days during the period between day -5 and day +5. Mean CAR and t-statistics are reported. Different models in calculating CAR are adopted. Various model specifications are considered, including the market model, Fama–French Three-Factor model and Four-Factor model. The Wilcoxon rank test is conducted on median CARs and significance between groups is reported in the last column. In Panel B, we report results of multivariate analysis. In columns 1-5, OLS regression with different measures of a firm's exposure to the announcement of a bank's approval for TARP on a certain date. In column 6, we use the entire universe of firms both in Compustat and CRSP as a sample to retest our results. In Panel C, we retest our results in different subsamples. Robust standard errors standard errors are corrected for two-way clustering at bank and announcement date. ***, **, and * indicate statistical significance at 1%, 5%, and 10% level, respectively. Detailed variable definitions are stated in Appendix 1.

Panel A: Univariate Analysis

	(1) Treatment		(2) Control		Difference
	Median CAR	Significance	Median CAR	Significance	
No. Obs.	1,821		15,180		
Panel A1: Market Model Adjusted Abnormal Returns					
[-1, +1]	-3.01%	***	-1.33%	***	***
[-10, +10]	-10.19%	***	0.01%	***	***
[-10, +1]	-5.13%	***	-1.24%	***	***
Panel A2: Fama–French Three-Factor Model Adjusted Abnormal Returns					
[-1, +1]	-0.87%		-0.80%		
[-10, +10]	-3.58%	***	-0.35%		***
[-10, +1]	-2.59%	***	-1.17%	***	***
Panel A3: Fama–French Four-Factor Model Adjusted Abnormal Returns					
[-1, +1]	-0.90%		-0.79%		
[-10, +10]	-4.24%	***	-0.42%		***
[-10, +1]	-3.00%	***	-1.19%	***	***

Table 6 Panel B: Multivariate Analysis

	CAR[-1,+1]					
	(1)	(2)	(3)	(4)	(5)	Compustat /CRSP
TARP EXP (Dum)	**-0.014*** **	**-0.010** **				**-0.011*** **
	(0.004)	(0.004)				(0.004)
TARP EXP (Amt)			-0.015**			
			(0.006)			
TARP EXP (Num)				-0.015**		
				(0.006)		
TARP EXP (Dum)× *Round 1*					-0.009*	
					(0.005)	
TARP EXP (Dum)× *Round 2*					-0.027***	
					(0.006)	
TARP EXP (Dum)× *Round 3*					0.030	
					(0.022)	
Cash		0.009	0.010	0.010	0.009	0.004
		(0.009)	(0.009)	(0.009)	(0.009)	(0.005)
M/B		-0.001	-0.001	-0.001	-0.001	-0.001
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Firm size		0.001	0.001	0.001	0.001	-0.000
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
ROA		0.009	0.009	0.009	0.009*	0.005
		(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
LEV		-0.023	-0.023	-0.023	-0.023	-0.024**
		(0.014)	(0.014)	(0.014)	(0.014)	(0.012)
INTCOV		0.000	0.000	0.000	0.000	-0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-0.016***	-0.023**	-0.027**	-0.027**	-0.006	-0.016
	(0.005)	(0.010)	(0.011)	(0.011)	(0.011)	(0.012)
Obs.	15180	13188	13188	13188	13188	20625
Industry fixed effects	No	Yes	Yes	Yes	Yes	Yes
Two-way cluster at bank and announcement date	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-square	0.002	0.012	0.012	0.012	0.012	0.007

Table 6 Panel C: Subsample

	CAR[-1,+1]		
	1:1 Bank Matching	1:5 Bank Matching	1:1 Client Firm Matching
TARP EXP(Dum)	**-0.005**	**-0.012****	**-0.013*****
	(0.007)	(0.005)	(0.004)
Cash	-0.035*	-0.033	0.005
	(0.020)	(0.022)	(0.010)
M/B	0.005***	0.003***	0.000
	(0.001)	(0.001)	(0.001)
Firm size	0.003**	0.004*	0.002
	(0.001)	(0.002)	(0.002)
ROA	-0.036***	-0.008	-0.002
	(0.012)	(0.011)	(0.004)
LEV	-0.002	-0.030	-0.028
	(0.011)	(0.023)	(0.019)
INTCOV	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
Constant	-0.045***	-0.039**	-0.077***
	(0.013)	(0.014)	(0.007)
Obs.	2581	4978	12512
Two-way cluster at bank and announcement date	Yes	Yes	No
Two-way cluster at firm and announcement date	No	No	Yes
Adj. R-square	0.001	0.006	0.012

Table 7. Cash Flow Sensitivity and TARP Injection

This table provides results of the effect of TARP injection on relationship firms' cash flow sensitivity to cash. The sample period in this table is between 2005 and 2011 (year 2008 is excluded). The dependent variable is the changes in cash holding for firm i at year t, scaled by total assets in year t-1. Post equals 1 if fiscal year is after 2009 (including 2009), and zero otherwise. We regress the change of cash scale by pre-TARP total asset level on measures of exposure to TARP and their interactions with post dummy and cash flow post TARP injection. The "*Other controls*" in this table includes market-to-book, sales growth, cash flow, size, leverage, cash flow × post dummy, and measures of relationship with TARP bank. Regression results in the full sample and propensity score matched subsamples are both reported. Detailed variable definitions are stated in Appendix 1 and robust standard errors are corrected for within-firm clustering. Industry and year fixed effects are controlled in the regressions. ***, **, and * indicate statistical significance at 1%, 5%, and 10% level, respectively.

	Δ *Cash* $_{i,(t,t-1)}$ / *Asset* $_{i,t-1}$					
Variables	Full sample	Full sample	Full sample	1:1 Bank Matching	1:5 Bank Matching	1:1 Firm Matching
TARP EXP (Dum)	0.002			-0.015	-0.003	-0.001
	(0.004)			(0.011)	(0.006)	(0.004)
TARP EXP (Dum) × Post	-0.018***			-0.021	-0.020	-0.016**
	(0.006)			(0.024)	(0.012)	(0.006)
TARP EXP (Dum) × Cash flow	-0.124***			-0.049	-0.085*	-0.096***
	(0.030)			(0.084)	(0.049)	(0.032)
TARP EXP (Dum) × Cash flow × Post	**0.222*** **			**0.367***	**0.297*** **	**0.200*** **
	(0.049)			(0.201)	(0.105)	(0.052)
TARP EXP (Amt)		-0.000				
		(0.004)				
TARP EXP (Amt) × Post		-0.017**				
		(0.007)				
TARP EXP (Amt) × Cash flow		-0.120***				
		(0.035)				
TARP EXP (Amt) × Cash flow × Post		**0.234*** **				
		(0.056)				
TARP EXP (Num)			-0.001			
			(0.004)			
TARP EXP (Num) × Post			-0.017**			
			(0.007)			
TARP EXP (Num) × Cash flow			-0.116***			
			(0.035)			
TARP EXP (Num) × Cash flow × Post			**0.238*** **			
			(0.057)			
Post	0.015***	0.014***	0.014***	0.016	0.016	0.013***
	(0.004)	(0.004)	(0.004)	(0.024)	(0.012)	(0.005)
Cash flow × Post	-0.061*	-0.052	-0.052	-0.202	-0.132	-0.039
	(0.035)	(0.034)	(0.034)	(0.198)	(0.099)	(0.040)
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13341	13341	13341	7967	9026	12661
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Clustered at firm level	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-square	0.120	0.119	0.119	0.096	0.098	0.110

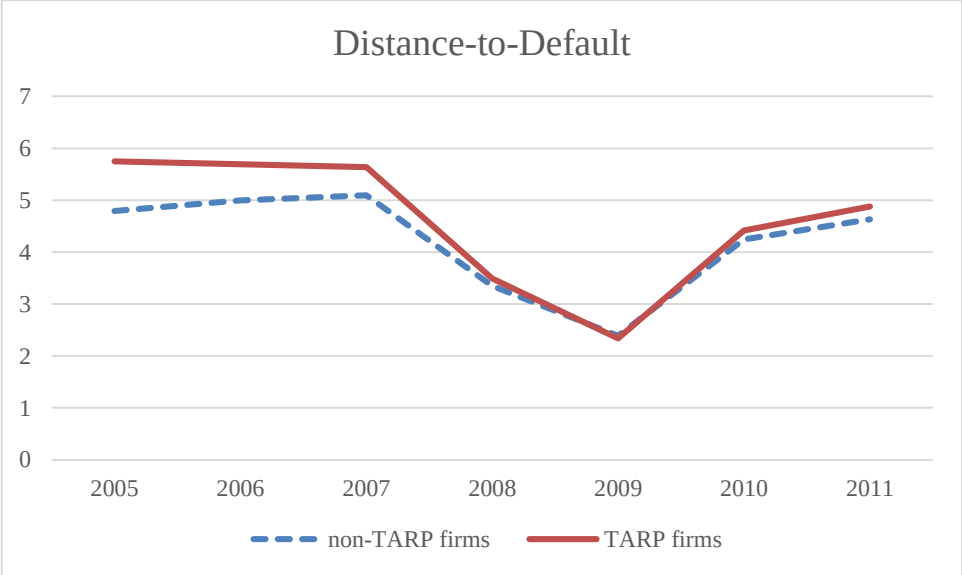
Table 8. Firm Investment and TARP Injection

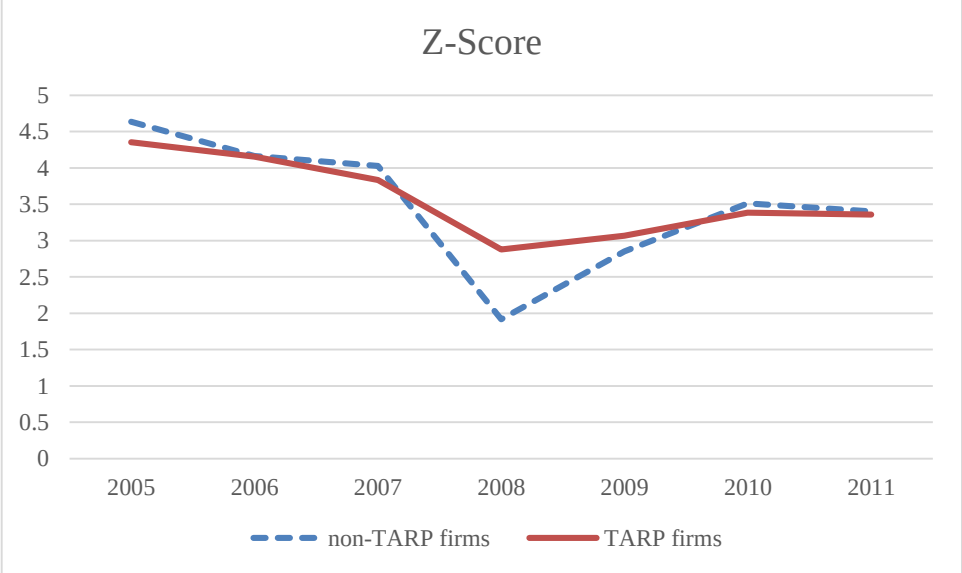
This table provides results relating to the effects of TARP injection on borrower firm investment levels. The sample period in this table is between 2005 and 2011 (excluding 2008). The dependent variable is sample firms' capital expenditure scaled by total assets at year t. Independent variables include measures of firm exposure to TARP recipient banks, post dummy, and their interaction terms. Other controls include lag of investment, market-to-book, sales growth, and cash flow. Year and industry fixed effects are controlled. We winsorize the sample at 1% based on the value of Capex. Detailed variable definitions are stated in Appendix 1. Robust standard errors are corrected for within-firm clustering. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively.

	Capex $_{i,t}$ / Net PPE $_{i,t-1}$					
	Full sample	Full sample	Full sample	1:1 Bank Matching	1:5 Bank Matching	1:1 Firm Matching
TARP EXP (Dum) × Post	**-0.016***			**-0.074****	**-0.042*****	**-0.014**
	(0.009)			(0.033)	(0.014)	(0.009)
TARP EXP (Dum)	0.001			0.012	0.012	0.002
	(0.008)			(0.028)	(0.013)	(0.008)
TARP EXP (Amt) × Post		**-0.019***				
		(0.010)				
TARP EXP (Amt)		0.000				
		(0.009)				
TARP EXP (Num) × Post			**-0.019***			
			(0.010)			
TARP EXP (Num)			-0.001			
			(0.009)			
Post	-0.009	-0.010	-0.010	0.055*	0.021	-0.011
	(0.007)	(0.007)	(0.007)	(0.032)	(0.013)	(0.008)
Capex $_{i,t-1}$ / Net PPE $_{i,t-2}$	0.311***	0.311***	0.311***	0.340***	0.336***	0.325***
	(0.015)	(0.015)	(0.015)	(0.021)	(0.020)	(0.016)
M/B	0.027***	0.027***	0.027***	0.032***	0.031***	0.028***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
Cash flow	0.102***	0.103***	0.103***	0.101**	0.076	0.091**
	(0.039)	(0.039)	(0.039)	(0.049)	(0.047)	(0.039)
Firm size	-0.003**	-0.003***	-0.003***	-0.005***	-0.003***	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Lev	-0.059***	-0.060***	-0.060***	-0.040***	-0.045***	-0.056***
	(0.011)	(0.011)	(0.011)	(0.013)	(0.012)	(0.011)
Sales growth	0.009	0.010	0.010	-0.009	0.009	0.010
	(0.032)	(0.032)	(0.032)	(0.041)	(0.040)	(0.033)
Cash holding	0.124***	0.124***	0.124***	0.126***	0.119***	0.119***
	(0.010)	(0.010)	(0.010)	(0.014)	(0.013)	(0.010)
Constant	0.010	0.010	0.010	0.012	0.021	-0.002
	(0.019)	(0.019)	(0.019)	(0.028)	(0.027)	(0.020)
Obs.	7831	7831	7831	4802	5418	7453
Industry Fix effects	Yes	Yes	Yes	Yes	Yes	Yes
Cluster at firm level	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.338	0.338	0.338	0.384	0.371	0.346

Figure 1. Credit Risk of TARP and Non-TARP Firms

This figure provides compares the Distance to Default and the Z score for TARP and non-TARP firms over the sample time period.





Appendix 1: Variable Definitions and Constructions

Variable	Definition
Main bank	A bank is defined as the main bank of a given borrowing firm if it has the strongest banking relationship, by fraction of dollar value of loans taken by the firm between October 2003 and September 2008. We further require that such lending relationship fraction of the main bank be greater or equal to 20% of total borrowing to eliminate observations where firms have no obvious main bank. For firms with two banks that have an equal fraction of lending, both are taken as main banks.
TARP firms	US public firms in Dealscan that have TARP banks as their main bank.
Non-TARP firms	All remaining US public firms in Dealscan that are not classified as TARP firms.

Panel A: Measures of Connections

TARP EXP (Dum)	In announcement effect regressions, on each announcement date, equals 1 if there is any past lending relationship between announcing TARP bank k and TARP firm i over the period October 2003 and September 2008, and zero otherwise. We consider such relations only if the announcing TARP bank is the firm's main bank. In real effect regressions, this equals 1 if the firm's main bank is a TARP bank, and zero otherwise.
TARP EXP(Amt)	In announcement effect regressions, on each announcement date, this variable equals the past lending relationship (calculated from lending amount) of a firm with its main bank(s) that received approval for TARP on that date. In real effect regressions, this equals the firm's aggregate lending relationship (calculated from lending amount) with all of its main banks that received TARP.
TARP EXP (Num)	In announcement effect regressions, on each announcement date, this variable equals the past lending relationship (calculated from number of lending facilities) of a firm with its main bank(s) that received approval for TARP on that date. In real effect regressions, this equals the aggregate lending relationship (calculated from number of lending facilities) with all of its main banks that received TARP.
TARP Bank	Equals 1 if a bank is a TARP recipient, and zero otherwise.
Round 1	Equals 1 if TARP announcement is on Oct. 14, 2008, and zero otherwise.
Round 2	Equals 1 if TARP announcement is between Oct. 21, 2008 and Nov. 14, 2008, and zero otherwise.
Round 3	Equals 1 if TARP announcement does not belong to either round 1 or round 2, and zero otherwise.
Post dummy	Equals 1 if year is later than 2008, and zero otherwise.

Panel B: Measures of Financial Characteristics

Firm size	Natural logarithm of book value of total assets. Calculated from Compustat data as ln(at).
MVE	End of fiscal year closing stock price (prcc_f) multiplied by total shares outstanding (csho).
M/B	Calculated from Compustat data as (at-ceq-txdb+prcc_f*csho)/at.
Cash/assets	Calculated from Compustat data (ch/at).
Market leverage	Book value of debts over market value of total assets. Calculated from Compustat data as (dltt+dlc)/(dltt+dlc+prcc_f*csho).
Book leverage	Book value of debts over book value of total assets. Calculated from Compustat data as(dltt+dlc)/at.
INTCOV	EBIT over interest expense. Calculated from Compustat data as (ebit/xint).
ROA	Operating income before depreciation, scaled by book value of total assets. Calculated from Compustat data as (ebitda/at).
WCR	Current assets/current liabilities. Calculated from Compustat data as (act/lct).
Sales growth	Percentage change in sales over the preceding fiscal year.
R&D/assets	R&D value over total assets. Calculated from Compustat data as (xrd/at). Missing R&D value is considered to equal zero.
Capital investment	Calculated from Compustat data as (capex/lag(PPE)).
COGS/Asset	Calculated from Compustat data as (cogs/at).
Cash flow	Operating cash flow divided by total assets in year t-1 (oancf/l.at).
Industry	Fama–French 48 industry classification.

Panel C: Measures of Bank Characteristics

Bank size	Calculated from Bankscope data as Ln(data2025).
Tier 1 ratio	Obtained from Bankscope data (data2130), which is calculated as Tier 1 capital over risk-weighted asset value.
ROA	Calculated from Bankscope data as (data2115/data2025).
Cash holding	Calculated from Bankscope data as (data5580/data2025).
LLP	Calculated from Bankscope data as (data2095/data2001).
Liquid /Asset	Obtained from Bankscope data as (data2075/data2025).
Δ Tier 1 Cap	Obtained from Bankscope data as (data2140–Lag(data2140)) /Lag(data2140).
Δ liquid asset	Obtained from Bankscope data as (data2075– Lag(data2075)) /Lag(data2025).