Did Saving Wall Street Really Save Main Street? The Real Effects of TARP on Local Economic Conditions*

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Abstract

We investigate whether saving Wall Street through the Troubled Assets Relief Program (TARP) really saved Main Street during the recent financial crisis. Our difference-in-difference analysis suggests that banks' TARP bailouts were followed by improvements in economic conditions in the local markets in which they operate. TARP statistically and economically significantly increased net job creation and net hiring establishments, and statistically and economically significantly decreased business and personal bankruptcies. The results hold up to a variety of robustness tests, including accounting for endogeneity. These results suggest that giving a lifeline to Wall Street via TARP may have helped save Main Street.

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"I was never able to convince the American people that what we did with TARP was not for the banks. It was for them. It was to save Main Street. It was to save our economy from a catastrophe."

Henry Paulson, former Secretary of the Treasury, "Five Years from the Brink",
Bloomberg BusinessWeek, September 2013,
http://www.moneynews.com/FinanceNews/Paulson-crisis-financial-Fed/2013/09/13/id/525579

"To declare TARP a success is revisionist history…TARP was supposed to restore lending, and that didn't happen."

Neil Barofsky, the Special Inspector General for TARP,
"Bailout: An Inside Account of How Washington Abandoned Main Street While Rescuing Wall Street",
http://www.thedailybeast.com/articles/2012/07/24/neil-barofsky-s-bailout-why-tarp-failed.html

1. Introduction

Did saving Wall Street really save Main Street during the recent financial crisis? That is, did bailing out the banks through the Troubled Assets Relief Program (TARP) have a significant positive impact on the economic conditions of average Americans? This was one of the intentions of the program, and it was successful in this respect according to Henry Paulson, the former Secretary of the Treasury who initiated the program. Other observers take the opposite view, including Neil Barofsky, the Special Inspector General for TARP (see quotes above). While there has been a significant amount of research on TARP, to our knowledge, there is no academic research directly supporting either of these views. The purpose of this paper is to provide such evidence.

TARP was one of the largest government interventions in the U.S. during the recent financial crisis. The main component of TARP, the Capital Purchase Program (CPP), was a preferred stock and equity warrant purchase program led by the U.S. Treasury's Office of Financial Stability. We use the name TARP henceforth to refer to CPP, since this is the ultimate name widely used in the media (although CPP is only one of the interventions). The main objectives of TARP were to enhance the overall stability of the financial system, increase the availability of credit, and improve real economic conditions (i.e., save Main Street).

Prior TARP research includes investigations of the effects on bank lending (Black and Hazelwood, 2013; Li, 2013; Puddu and Walchli, 2013; Duchin and Sosyura, 2014), bank risk-taking (Black and Hazelwood, 2013; Li, 2013; Duchin and Sosyura, 2014), bank competition (Koetter and Noth, 2014; Berger and Roman, forthcoming), traded banks' stock market valuations (Veronesi and Zingales, 2010;

Ng, Vasvari, and Wittenberg-Moerman, 2013; Harrisa, Huertab, and Ngob, 2013), traded relationship borrowers' stock market valuations (Liu, 2013; Norden, Roosenboom, and Wang, 2013), and loan contract terms to recipient banks' large customers (Berger, Makaew, and Roman, 2014).

However, the effects of TARP on the real economy and the welfare of average Americans have not been directly studied, perhaps because of the difficulty of disentangling the effects of TARP from those of other government programs and market events which were occurring around the same time. We circumvent this difficulty by studying the effects of TARP on local market economic conditions. Specifically, we look at the changes in local economic conditions as functions of the proportions of the banks that received TARP in their local areas. If saving Wall Street really saved Main Street, then local markets in which more banks received TARP should have improved significantly relative to local markets in which fewer or no banks received TARP.

Ex ante, it is unclear whether TARP would improve or worsen local economic conditions. We formulate and test hypotheses with divergent predictions regarding the effect of TARP on local economic conditions to see which of these hypotheses empirically dominates.

Using the full sample of commercial banks in the U.S. over 2005:Q1-2012:Q4, we test the hypotheses using difference-in-difference (DID) regression models. We use four indicators of local economic conditions that likely affect average Americans – *Net Job Creation / Capita*, *Net Hiring Establishments / Capita*, *Business Bankruptcies / Capita*, and *Personal Bankruptcies / Capita* – as the key dependent variables. The exogenous variables include *TARP Recipient* (the proportion of banks receiving TARP in the local market), *Post TARP* (a dummy equal to one over 2009:Q1-2012:Q4, the period after the TARP program initiation) and a DID term *Post TARP x TARP Recipient* to capture the effect of the TARP treatment. We also control for large numbers of bank-related and state-related characteristics, and state and time fixed effects.

Our results suggest that the TARP program led to improvements in economic conditions in the local markets in which a higher proportion of banks received TARP funds: it statistically and economically significantly increased net job creation and net hiring establishments, and statistically and economically

significantly decreased business and personal bankruptcies. We find that the average market had a quarterly increase in the net job creation of 0.506, given an average TARP recipient value of 0.156. This suggests that over the 16 quarters of the post-TARP period (2009:Q1-2012:Q4), for every 1000 people, 8.09 jobs were created due to TARP. Similarly, we find that on average over the post-TARP period, for every 1000 people, 1.60 more establishments created jobs, and there were 0.052 fewer business bankruptcies and 1.08 fewer personal bankruptcies due to TARP. All of these figures are large relative to their sample means. These measured effects on the economy may be understated because they do not capture any benefits to the economy from possible stabilization of the financial system that may have occurred due to TARP. As a result, we conclude that saving Wall Street may have helped save Main Street to an economically significant extent.

Our results are robust to a number of checks, including an instrumental variable analysis and a Heckman (1979) self-selection model to deal with potential endogeneity and sample selection problems, respectively, a placebo experiment to attempt to rule out the possibility that alternative forces in the local markets may drive our results, and estimation of several alternative econometric models. We also investigate the dynamic effects of TARP on local economic conditions and find that the job creation and hiring establishments effects mostly occur in 2009 and generally dissipate thereafter, but the bankruptcy effects tend to last longer.

We also test for which types of banks and under what local economic conditions TARP was most effective by considering different bank sizes, involuntary versus voluntary participants, stress-tested versus non-stress-tested banks, distinguishing between banks that repaid TARP funds early and those that did not, considering low -capitalized versus high-capitalized banks, states with worse versus better previously existing economic conditions, and states with existing lower versus higher economic freedom. We find a number of important differences across these groups. Overall, the results of this paper add to the literature on TARP by focusing on real economic effects of TARP, and suggest that extending a lifeline to Wall Street via TARP may have helped save Main Street.

The remainder of the paper is organized as follows. In Section 2, we describe TARP. In Section

3, we review the related literature. Section 4 develops the empirical hypotheses. In Section 5, we detail the econometric framework, and in Section 6, we discuss the data. In Section 7, we present the main empirical results. Section 8 focuses on robustness tests. Section 9 draws conclusions and gives policy implications. Appendix X decomposes our four local economic conditions and examines the effects of TARP on each component. Appendix Y shows the subsample analyses of the effects of TARP.

2. Description of the Troubled Asset Relief Program (TARP)

TARP was created in October 2008 in accordance with the Emergency Economic Stabilization Act of 2008 (EESA), one of the largest government interventions to address the recent financial crisis. Its primary goals were to improve financial stability by purchasing up to $700 billion of the banking organizations' "troubled assets" to allow them to stabilize their balance sheets and avoid further losses, encourage them to resume lending, and improve real economic conditions.

Instead of purchasing "troubled assets," the Capital Purchase Program (CPP) of TARP authorized the U.S. Treasury to invest up to $250 billion (out of the $700 billion bailout package) in the preferred equity of selected financial institutions to enhance their capital ratios. This included $125 billion in $10 billion and $25 billion increments to nine large involuntary participants (Citigroup, Bank of America, J.P. Morgan Chase, Wells Fargo, Goldman Sachs Group, Morgan Stanley, Wachovia Corporation, State Street Corporation, and Merrill Lynch) on October 28, 2008. These initial recipients did not follow the formal TARP evaluation process, while the rest of the recipients followed the formal process and applied for TARP funds from the U.S. Treasury. TARP eventually infused capital of $204.9 billion into 709 banking organizations. Approval to receive TARP funds took into account the health of the banking organizations, with viable, healthier ones being more likely to receive capital. In addition, Bayazitova and Shivdasani (2012), Duchin and Sosyura (2012, 2014), Li (2013), Berger, Makaew, and Roman (2014), and Berger and Roman (forthcoming) find that banks with more political and regulatory connections were more likely to receive TARP funds. The size of the TARP investment in preferred shares was determined by the

Treasury, ranging from 1-3% of a firm's risk-weighted assets or $25 billion (whichever was smaller).[1]

In return for the TARP capital infusion, banks provided the Treasury with non-voting preferred stock paying quarterly dividends at an annual yield of 5% for the first five years and 9% thereafter and ten-year life warrants for the common stock, giving taxpayers the opportunity to benefit from the banks' future growth. In addition, TARP participants were subject to compensation restrictions. Some of these were outlined at program inception in October 2008: limiting tax deductibility of compensation for senior executives to $500,000, requiring bonus claw-backs, and restricting golden parachute payments. In February 2009, the Treasury revised the compensation rules and limited total annual compensation for senior executives at TARP banks to $500,000 excluding certain incentive awards, and the American Recovery and Reinvestment Act (ARRA) further prohibited bonuses, retention awards, and incentive compensation other than long-term restricted stock awards that exceeded one-third of annual compensation. As of December 31, 2012, the Treasury had received over $220 billion in total cash back on $204.9 billion TARP investments in banking organizations (more than 100% of the total disbursed).[2]

3. Related Literature

A number of studies focus on the determinants and consequences of the TARP program. First, several studies look at factors that affect the decisions to apply for and receive TARP funds by banks. Duchin and Sosyura (2012) investigate the allocation of TARP capital to publicly listed banks and find that banks with more political connections were more likely to receive TARP funds and these connections are also used in Bayazitova and Shivdasani (2012), Li (2013), Duchin and Sosyura (2014), Berger, Makaew, and Roman (2014), and Berger and Roman (forthcoming). Bayazitova and Shivdasani (2012) also find that banks that posed systemic risk and faced high financial distress costs, but had strong asset quality, were more likely to obtain TARP equity infusions. Cornett, Li, and Tehranian (2013) find that financial characteristics

[1] Exceptions are Bank of America and Citigroup, which initially received $25 billion, but later obtained more funds from the Targeted Investment Program (TIP) (Calomiris and Kahn, forthcoming).

[2] http://www.treasury.gov/initiatives/financial-stability/reports/Pages/Monthly-Report-to-Congress.aspx

related to the probability of receiving TARP differ for the healthiest (‘‘over-achiever’’) versus the least healthy (‘‘under-achiever’’) banks. TARP “under-achievers” had weaknesses in income production and experienced liquidity issues, while the loans of TARP “over-achievers” performed well, but liquidity issues hurt the abilities of these banks to continue lending.

Other papers look at “exit from TARP” decisions. Bayazitova and Shivdasani (2012), Wilson and Wu (2012), and Berger and Roman (forthcoming) find that banks with high prior levels of CEO pay were more likely to exit early, presumably due to the restrictions on executive pay imposed on TARP recipients.

Some researchers look at traded TARP banks’ and traded borrowers’ valuations and loan contract terms for large loans. Ng, Vasvari, and Wittenberg-Moerman (2013) find that traded TARP banks had lower equity returns at program initiation and increased their valuations later. Veronesi and Zingales (2010) estimate the costs and benefits of TARP capital infusions in the ten largest banks up to 2009. They find that this intervention increased the value of banks’ financial claims by $130 billion. Norden, Roosenboom, and Wang (2013) find that TARP led to spillover effects from the banking sector to the corporate sector, leading to a significantly positive impact on traded relationship borrowing firms’ stock returns around the time of TARP capital injections. Liu (2013), in contrast, finds that traded firms with relationships with TARP banks suffered significant valuation losses around the times of TARP approval announcements. Berger, Makaew, and Roman (2014) study the effects of the TARP on loan contract terms for large loans using DealScan and find that TARP generally led to more favorable terms of credit for both relationship and non-relationship customers.

Perhaps the closest to our article are studies that investigate the impact of TARP on bank risk-taking and/or lending, because local economic conditions where banks operate will likely be affected, if at all, through bank lending and off-balance sheet commitments. Duchin and Sosyura (2014) use a sample of 529 publicly traded financial firms (2006-2010), which tend to be the largest firms, and find that TARP banks seemed to approve riskier loans, but find no evidence of a change in credit supply. Black and Hazelwood (2013) analyze risk-taking by bank size using 81 banks from the Survey of Terms of Bank Lending (STBL) survey (2007-2010). They find that risk of commercial and industrial (C&I) loans

originated increased for large TARP banks, but decreased at small TARP banks. They also find that C&I loans increased at small TARP banks, but decreased at large TARP banks relative to non-TARP banks. Li (2013) looks at TARP's effect on bank loan supply using a sample of 7,062 banks (both public and private), out of which 647 are TARP recipients. He focuses on banks with below-median Tier 1 ratios (less well capitalized) because these are more likely to receive TARP, and finds that these TARP banks expanded their credit supply, and this increase was registered in all major types of loans. Puddu and Walchli (2013) look at small business loan supply using a sample of 794 commercial banks that could be matched to the Community Reinvestment Act (CRA) data. They find that TARP banks provide on average 12% more small business loan originations than non-TARP banks.[3] The results in these last two studies were presumably dominated by the effects on small banks, which constitute the vast majority of U.S. banks.

In addition, some papers look at the effects of TARP on competition. Berger and Roman (forthcoming) find that TARP recipients got competitive advantages and increased both their market shares and measured market power, and that these results may be driven primarily by the **safety channel** (TARP banks may be perceived as safer), which is partially offset by the **cost disadvantage channel** (TARP funds may be relatively expensive). These competitive advantages are primarily due to TARP banks that repaid early. Koetter and Noth (2014) find competitive distortions as a result of TARP for unsupported banks. They find that higher bailout expectations for the unsupported banks increase loan rates, reduce deposit rates, and are associated with larger loan and deposit growth after TARP, suggesting that the **safety net channel** may extend to those banks for which customers anticipate bailouts to be more likely.

Finally, there is also a related literature that looks at government bailouts in other nations on bank risk-taking, lending, and liquidity creation (e.g., Brandao-Marques, Correa, and Sapriza, 2012; Dam and Koetter, 2012; Hryckiewicz, 2012; Berger, Bouwman, Kick, and Schaeck, 2014) and find either reductions

[3] Chang, Contessi, and Francis (2014) find that banks that received TARP funds maintained lower cash-to-assets ratios (and thus lower excess reserves ratios), consistent with the view that the TARP capital injection possibly resulted in more lending for TARP beneficiaries.

or increases in risk-taking, and reductions in credit growth and liquidity creation. Others look at effects on competition (e.g., Cordella and Yeyati, 2003; Gropp, Hakenes, and Schnabel, 2011;, Calderon and Schaeck, 2012) and find less aggressive competitive conduct when banks are subject to bailouts, and lower market power or more aggressive conduct for competitors of bailed out institutions.

4. Hypothesis Development

It is unclear ex ante whether TARP would improve or worsen local market economic conditions. We describe here a number of primary and secondary channels through which TARP may influence local market conditions, and develop two hypotheses from these channels. The primary channels are also hypothesized to affect market share and market power in Berger and Roman (forthcoming), but they apply here as well because they affect the quantity of lending and loan commitments issued by recipient banks, and such lending and commitments contribute to local economic conditions.

There are three primary channels through which TARP may improve local economic conditions through increases in credit in the local markets. First, the **predation channel** (Telser, 1966; Fudenberg and Tirole, 1986) suggests that TARP capital may have made banks better capitalized and these banks may have used the additional capital to act aggressively in the market and increase their loans and commitments.

Second, under the **safety channel**, TARP banks may be perceived as safer due to the bailout and/or the selection criteria which targeted "healthy, viable institutions." The safety channel includes the effects of both the banks' decisions to apply for TARP and whether the applications are approved. Under this channel, customers may demand more loans and loan commitments from TARP banks because these banks are less likely to fail or become financially distressed. Also, bank creditors may supply more funds and/or charge them lower rates because TARP banks are more likely to pay back. In reaction to the greater availability of loanable funds and/or reduction in funding costs, TARP banks may also supply additional credit. Thus, both demand for and supply of credit may be increased through this channel.

Third, under the **cost advantage channel**, TARP funds may be cheaper than non-TARP funds,

in which case TARP banks have an incentive to expand loans and loan commitments more because they are more cheaply funded.

There are also three other primary channels under which TARP may worsen local economic conditions through decreases in local market credit. First, under the **charter value / quiet life channel** (Hicks, 1935; Keeley, 1990; Cordella and Yeyati, 2003), the extra capital from the bailout may increase charter value and/or allow for a "quiet life," decreasing incentives for aggressive behavior and risk taking and reducing the supply of loans and commitments by the TARP banks.

Second, under the **stigma channel**, TARP banks may be perceived as riskier due to the bailouts.[4] The **stigma channel** is the opposite of the **safety channel**, and only one can hold for a given bank at a given time. The **stigma channel** includes the effects of both the banks' decisions to apply for TARP and whether the applications are approved. Under this channel, customers may demand less credit from TARP banks because these banks are more likely to fail or become financially distressed. Also, bank creditors may supply them less funds and/or charge them higher rates because TARP banks are less likely to repay. In reaction to the reduced availability of loanable funds and/or increase in funding costs, TARP banks may supply less credit. Thus, both demand for and supply of credit may be decreased through this channel.

Third, under the **cost disadvantage channel**, TARP funds may be more expensive than non-TARP funds. This is the opposite of the **cost advantage channel**, and only one can hold for a given bank at a given time. Here, TARP banks decrease the supply of loans and loan commitments because costs of funds are higher.

There are also two primary channels introduced by Berger and Roman (forthcoming) which could either increase or decrease the amount of credit to the local markets. Under the **increased moral hazard channel**, there may be increases in risk taking because of a perceived increased probability of future bailouts. The increases in risk taking may take the form of increased supplies of bank loans and

[4] Hoshi and Kashyap (2010), in their study about lessons from the recent Japanese crisis to consider for US, mention that a bank may refuse government assistance if the capital injection generates stigma or an adverse signal that the bank is expected to have high future losses.

commitments to riskier applicants, decreased supplies to safer applicants, or shifts from safer to riskier applicants without changing the overall quantity of loans. Alternatively, under the **decreased moral hazard channel** (the opposite of the **increased moral hazard channel)**, the increase in capital from the TARP injections may result into shifts into safer portfolios, again with an ambiguous effect on the overall credit supply.

There are also two secondary channels under which TARP may either improve or worsen local economic conditions through changes in credit in the local markets. As discussed in Berger and Roman (forthcoming), there may also be either an increase or decrease in the market power of TARP banks due to the primary channels described above. The changes in market power can either increase or decrease the net supply of loan and loan commitments, depending in part on the proportions of relationship borrowers versus transactional borrowers. An increase in market power may increase the supply of credit to relationship borrowers because limits on competition help banks enforce implicit contracts with relationship borrowers that result in greater credit availability (e.g., Petersen and Rajan, 1995). In contrast, an increase in market power may decrease the supply of credit to transactional borrowers under the structure-conduct-performance hypothesis. These channels are reversed if market power is decreased. Thus, the change in market power has an ambiguous effect on the total supply of credit in the local markets.

Finally, bailouts may result in changes in the behavior by the competitors to TARP banks that may partially offset or accentuate the increase or decrease in credit supply by the TARP banks (Hakenes and Schnabel, 2010; Gropp, Hakenes, and Schnabel, 2011; Koetter and Noth, 2014).

These primary and secondary channels lead us to our opposing hypotheses:

Hypothesis H1: A higher proportion of TARP banks is associated with improvements in local economic conditions.

Hypothesis H2: A higher proportion of TARP banks is associated with deteriorations in local economic conditions.

These hypotheses are not mutually exclusive. One can hold in some local markets and the other

can hold in other markets. We test whether one of these hypotheses empirically dominates the other overall.

5. Econometric Framework

We test the effects of TARP on local economic conditions using state-level data.[5] The changes in conditions after TARP injections in banks are analyzed using a difference-in-difference (DID) methodology. DID estimators are commonly used in the program evaluation literature (e.g., Meyer, 1995) to compare a treatment group to a control group both before and after treatment, and has been recently utilized in the banking literature (e.g., Beck, Levine, and Levkov, 2010; Schaeck, Cihak, Maehler, and Stolz, 2012; Berger, Kick, and Schaeck, 2014; Berger, Makaew, and Roman, 2014; Berger and Roman, forthcoming). An advantage of this approach is that by analyzing the time difference of the group differences, the DID estimator can account for omitted variables that affect treated and untreated groups alike. The DID regression model has the following form, which accounts for Hypotheses H1 and H2:

$$\begin{aligned} Y_{st} = {} & \beta_0 + \beta_1 \cdot TARP\ Recipient_{st} + \\ & + \beta_2 \cdot Post\ TARP_t + \beta_3 \cdot Post\ TARP_t \times TARP\ Recipient_{st} \\ & + \beta_4 \cdot X_{st-1} + \beta_5 \cdot State_s + \beta_6 \cdot Time_t + \varepsilon_{st} \end{aligned} \quad (1)$$

Y_{st} is an indicator of local economic conditions in state *s* at time *t* (*Net Job Creation / Capita*, *Net Hiring Establishments / Capita, Business Bankruptcies / Capita*, or *Personal Bankruptcies / Capita*). *TARP Recipient*$_{st}$ is the weighted proportion of banks receiving TARP capital support in the state, where the weights are based on the proportions of deposits of the banks in the state *s* in quarter *t*.[6] *Post TARP*$_t$ is a dummy equal to one in 2009:Q1-2012:Q4, the period after the TARP program started (following Duchin and Sosyura, 2014, but considering a longer period). *Post TARP*$_t$ *x TARP Recipient*$_{st}$ is the DID term and captures the effect of the treatment (TARP) when it occurs. Positive coefficients on the DID terms in the *Net Job Creation / Capita* or *Net Hiring Establishments / Capita* equations or negative coefficients on the

[5] To the extent that customers borrow from out-of-state banks which may or may have not received TARP funds, our estimates are biased toward finding no effects because these are not captured by our independent variables.

[6] Deposits and branches are the only banking variables for which locations are available.

DID terms in the *Business Bankruptcies / Capita* or *Personal Bankruptcies / Capita* equations would show favorable changes in the local economic conditions as functions of the proportions of the banks that received TARP in their local areas, and vice-versa. X_{st-1} are bank control variables based upon the weighted average of the banks in the state or state-level controls, $State_s$ represents state fixed effects, $Time_t$ represents year and quarter fixed effects, and ε_{st} represents an error term.

6. Data and Sample

6.1 Data Sources

Data are collected from multiple sources. We obtain TARP transactions data for the period October 2008 to December 2010 and TARP recipients list from the Treasury's website.[7] We match by name and location the institutions in the list with their corresponding RSSD9001 (Call Reports ID) where available. The TARP report includes 572 bank holding companies (BHCs) and 87 commercial banks.[8]

We obtain bank data from quarterly Call Reports for the period 2005:Q1 to 2012:Q4. Given that the majority of TARP recipients are BHCs, we aggregate Call Report data of all the banks in the BHC at the holding company level if the BHC has more than one commercial bank owned. If the commercial bank is independent, we keep the data for the commercial bank. For convenience, we will use the term bank to mean either type of entity. We exclude firm-quarter observations that do not refer to commercial banks (RSSD9331 different from 1), have missing or incomplete financial data for total assets or common equity, have missing or negative data for the income statement items such as interest expenses, personnel expenses, and non-interest expenses, or if the bank failed before 2009:Q1 (i.e., before observation of TARP effects). In addition, we normalize all financial variables using the seasonally-adjusted GDP deflator to be in real 2012:Q4 dollars. We merge the TARP data with the Call Report data. We then convert these data to the state level based on the proportions of their deposits in the local markets in which they operate

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

[8] The TARP report also includes 48 thrifts and 2 S&Ls. However they do not have comparable Call Report information and their lending behavior is very different (focus on residential mortgages), so we exclude them from the estimation.

as reported in the FDIC's Summary of Deposits (SoD) database. Thus, for the vast majority of banks which operate only in one state, we include the percentage of the state's deposits that are in that bank to the state's *TARP Recipient* value. For multi-state banks, we assume that the TARP effects are geographically distributed according to the locations of the banks' deposits.

We obtain quarterly local economic conditions data at the state level from the U.S. Department of Labor (the Quarterly Business Dynamics Statistics (BDS) and the Quarterly Census of Employment and Wages (QCEW) datasets) and quarterly business and personal bankruptcies data at the state level from American Bankruptcy Institute and U.S. Court Filings for the period 2005:Q1 to 2012:Q4.

We use data from several other sources for additional control variables and instruments: List of Corrective Actions, U.S. Census Bureau's Population Distribution, House of Representatives website, Missouri Census Data Center, Center for Responsible Politics website, National Bureau of Economic Research, Tax Policy Center, and Fraser Institute. The regressions lose one quarter of observations because of the use of lagged values for some of the exogenous variables. We also lose one observation due to a missing value for one of our state controls in one quarter. Our final regression sample contains 1,580 state-quarter observations for 31 quarters and 51 states (including Washington, D.C., as a state).

6.2 Main Dependent Variables

For dependent variables, we first consider *Net Job Creation / Capita,* the overall net job creation per capita calculated as: (*Gross Job Creation* - *Gross Job Destruction*) / (*Population*/1000). Our per capita variables are actually per 1000 of state population to make the results easier to interpret. *Gross Job Creation* is the number of jobs created and consists of job openings and expansions. Openings are number of jobs created at new establishments. Expansions are number of new jobs created at existing establishments. *Gross Job Destruction* is the number of jobs destroyed and consists of job closings and contractions, defined analogously.

We next consider the *Net Hiring Establishments / Capita,* the overall net hiring establishments per capita, calculated as: (*Gross Hiring Establishments* - *Gross Firing Establishments*) / (*Population*/1000).

Gross Hiring Establishments is the number of hiring establishments that create jobs. It consists of establishments that create jobs through job openings and expansions. *Gross Firing Establishments* is the number of establishments that destroy jobs and consists of establishments that destroy jobs through job closings and contractions.

We also examine bankruptcies for each state, as bankruptcies can be costly (e.g., Altman, 1984; Hotchkiss, 1995; Weiss, 1990; Wruck, 1990; Weiss and Wruck, 1998; Bris, Welch, and Zhu, 2006), and may reflect resource misallocation in the local markets (Meyer and Pifer, 1970). We look at both business and personal bankruptcies. *Business Bankruptcies / Capita* is the overall number of business bankruptcies per capita, calculated as: (Total business bankruptcy filings) / (*Population*/1000). Business bankruptcies consist of Chapter 7 filings (corporate liquidations), Chapter 11 filings (large corporate reorganizations), Chapter 12 filings (corporate reorganizations for farms and fisheries), and Chapter 13 filings (orderly plan for small debt repayment). *Personal Bankruptcies / Capita* is the overall number of personal bankruptcies per capita, calculated as: (Total personal bankruptcy filings) / (*Population*/1000). Personal bankruptcies consist of Chapter 7 filings (straight bankruptcy or liquidation), Chapter 11 filings (personal reorganization), and Chapter 13 filings (wage earner plan for debt repayment).

6.3 Main Independent Variables

We use *TARP Recipient, Post TARP,* and the interaction term *Post TARP x TARP Recipient* as the key independent variables for our regression analysis. These are defined above in Section 5.

6.4 Control Variables

We include a broad set of bank-related and state-related control variables to mitigate potential omitted variable problems. Starting with the bank-related variables, we control for proxies for CAMELS (the declared set of financial criteria used by regulators for evaluating banks) as in Duchin and Sosyura (2014) because these are widely perceived as good indicators of a bank's financial health. We specifically control for *Capital Adequacy* to account for the extent to which a bank can absorb potential losses and increase lending and commitments. This is constructed as the ratio of equity capital divided by gross total assets

(GTA).[9,10] We control for *Asset Quality* to account for the condition of a bank's portfolio, defined by the fraction of nonperforming loans to total loans. We control for *Management Quality* using a dummy taking a value of -1 if a bank had a corrective action by its primary federal regulator during the quarter, which may result in reduced lending. We control for *Earnings* because banks that are more profitable may be in better positions to lend and improve local economic conditions. It is proxied by return on assets (ROA), and is measured as the ratio of the annualized net income to GTA. We account for bank *Liquidity*, proxied by the ratio of cash over total deposits. Finally, *Sensitivity to Market Risk* is defined as the ratio of the absolute difference (gap) between short-term assets and short-term liabilities to bank GTA.

We control as well for other bank variables which may also affect credit extension. We use *DWTAF*, the proportion of banks using discount window loans and/or Term Auction Facility (TAF) funding during the crisis. Berger, Black, Bouwman, and Dlugosz (2014) find that banks using these funds increased their lending significantly.[11] We also control for *Bank Size*, the natural log of GTA, because larger banks may have a greater capacity to increase lending and/or liquidity creation, which includes loan commitments (e.g., Berger and Bouwman, 2009). We control as well for *HHI Deposits*, the Herfindahl-Hirschman Index determined using deposit data from the FDIC Summary of Deposits, which may affect bank lending strategy. Also, we include *State No Banks*, the logarithm of the total number of banks in the state, another measure of competition. Finally, we control for *Metropolitan* – the weighted proportion of banks having the majority of bank deposits (50% or more) in metropolitan areas in the state – as banks in metropolitan locations may have more opportunities to increase lending.

In addition, we control for a number of state-level control variables that could influence local

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

[10] To avoid distortions for the Equity to GTA ratio, for all observations with equity less than 0.01 * GTA, we replace equity with 1% of GTA (as in Berger and Bouwman, 2009).

[11] Data on these programs during the crisis were made public due to the Freedom of Information Act (FOIA) requests and a provision of the Dodd-Frank Act, and the data were generously provided to us by those authors.

economic conditions. We first control for *State Minimum Wage*, which is the minimum wage mandated by state law in $/hour. If there is no minimum wage law in the state, we use the Federal minimum wage. We control for the *State Marginal Tax Rate*, the top marginal rate of the state's income tax. Berger and Sedunov (2014) find significant effects of *State Minimum Wage* and *State Marginal Tax Rate* on state output.[12] In addition, we control for *State Economic Freedom Index*, the state-level index of economic freedom, which is found to impact growth and employment (Gwartney, Lawson, and Block, 1996; Easton and Walker, 1997; Garrett and Rhine, 2010). The index is obtained from the Fraser Institute and comprised of a number of factors selected to capture three main elements of economic freedom: the size of government, taxation, and labor market. Finally, we control for *State House Price Inflation* since this may have contributed to the financial instability during the recent financial crisis due to banks being able to only partially recover collateral in defaulted mortgage loans when house price inflation was significantly negative. This is calculated using the quarterly change in the state's seasonally-adjusted Federal Housing Financing Agency (FHFA) house price index.

7. Empirical Analysis

7.1 Summary Statistics

Table 1 provides definitions and summary statistics for our variables. In terms of local economic conditions indicators, *Net Job Creation / Capita* has an average of 0.207, *Net Hiring Establishments / Capita* has an average of -0.157, while *Business Bankruptcies / Capita* has an average of 0.038 and *Personal Bankruptcies / Capita* averages 1.036. The *TARP Recipient* variable shows that on average, 15.6% of the banks in a state received TARP money.

Looking at the CAMELS proxies, we find that the average state over our sample period has aggregated bank *Capital Adequacy* of 0.105, *Asset Quality* of 0.007, *Management Quality* of -0.001, *Earnings* of 0.022, *Liquidity* of 0.079, and *Sensitivity to Market Risk* of 0.146. These statistics suggest that

[12] Data on these state indicators were generously provided to us by the authors.

on average over the sample period, states had banks that were well capitalized and did not have many performance problems, although the means mask problems for individual banks at different points in time. We find that in the average state, 24.1% of banks obtained Discount Window and/or TAF funds (*DWTAF*), average *Bank Size* (logarithm of the GTA) is 14.959 (mean GTA is $91.9 billion), *HHI Deposits* is 588.823, average *State No Banks* (logarithm of the total number of banks) is 4.180 (mean number of banks is 115.900), and *Metropolitan* mean is 0.336. Also, the average state in our sample has *State Minimum Wage* of $6.757 / hour, *State Marginal Tax Rate* of 5.214, *State Economic Freedom Index* of 6.769, and *State House Price Inflation* of 0.519%.

7.2 Regression Analysis

Table 2 tabulates the main estimation results for equation (1) and tests Hypotheses H1 and H2 (state and time fixed effects are not shown for brevity). Regression estimates in column (1) for *Net Job Creation / Capita* and column (2) for *Net Hiring Establishments / Capita* indicate that the DID terms, *Post* $TARP_t$ * *TARP* $Recipient_{st}$, are positive and statistically significant at 1% level, suggesting that TARP banks' capital injections were followed by increases in net job creation and net hiring establishments. Second, the DID estimates in column (3) for *Business Bankruptcies / Capita* and in column (4) for *Personal Bankruptcies / Capita* are negative and statistically significant, indicating that TARP banks' capital injections were followed by decreases in business and personal bankruptcies. These results are consistent with the statistical empirical dominance of Hypothesis H1 over Hypothesis H2.

The improvements in the local economic conditions are also economically significant, suggesting that Hypothesis H1 also economically dominates Hypothesis H2. The coefficient on *Post* $TARP_t$ * *TARP* $Recipient_{st}$ of 3.243 in the *Net Job Creation / Capita* equation in column (1) suggests that the average market had a quarterly increase in the net job creation of 0.506, given an average TARP recipient value of 0.156. This suggests that over the 16 quarters of the post-TARP period (2009:Q1-2012:Q4), for every 1000 people, 8.09 jobs were created due to TARP. Similarly, we find that on average over the whole post-TARP period, for every 1000 people, 1.60 more establishments created jobs, and there were 0.052 fewer

business bankruptcies and 1.08 fewer personal bankruptcies due to TARP. All of these figures are large relative to their sample means. Overall, these results suggest that extending a lifeline to Wall Street via TARP may have saved Main Street to an economically significant extent.[13] In unreported results, we also confirm these results at the MSA/NECMA level, finding that the quarterly unemployment rate declined significantly as a result of TARP capital injections.[14,15]

8. Robustness Tests

8.1 Endogeneity and Sample Selection Concerns

8.1.1 Instrumental Variable Analysis

The potential endogeneity of our *TARP Recipient* variable could bias our findings. For example, TARP capital might be more often provided to the strongest banks, which may be more likely to improve local economic conditions through increased credit, yielding a spurious relation.

We therefore conduct an instrumental variable (IV) analysis to isolate the causal impact of TARP on local economic conditions. The research discussed above suggests that bank's political and regulatory connections affect the bank's probability of receiving TARP funds. We use several political and regulatory instruments for the *TARP Recipient* variables. First, we consider the *Subcommitees on Financial Institutions or Capital Markets*, a variable which takes a value of 1 if a bank is headquartered in the election district of a House member who served on the Financial Institutions Subcommittee or Capital Markets

[13] Results are robust to using an alternative Post TARP period, which is equal to 1 for the period 2008:Q4 – 2012:Q4, immediately from the quarter that TARP started to be distributed.

[14] In our unreported results, we use the quarterly unemployment rate at the MSA/NECMA level. This is the only variable for which we find quarterly data available at the metropolitan level and it is not available for rural areas. The model includes MSA/NECMA and time fixed effects and all bank controls from our main specification, but no MSA/NECMA level controls due to data limitations. We consider our state-level analysis to be our main analysis because it is inclusive of both metropolitan and rural areas, we are able to include more state-level controls, and the unemployment rate is a less reliable indicator of labor market conditions, as it changes significantly with labor force participation rates.

[15] In unreported results, we also try to control for State Expenditures per capita, total spending by the state's government per capita and results are robust. However, the data for this control is not available for Washington D.C., which reduces the number of states in our analysis, so we do not include this variable in our main specification.

Subcommittee of the House Financial Services Committee in 2008 or 2009.[16] As shown in Duchin and Sosyura (2014), these subcommittees played a direct role in the development of EESA and were charged with preparing voting recommendations for Congress on authorizing and expanding TARP. Members of these subcommittees were shown to arrange meetings between banks and the Treasury, write letters to regulators, and write provisions into EESA to help particular firms. While these arguments indicate that *Subcommitees on Financial Institutions or Capital Markets* should be positively related to TARP decisions, the distribution of House seats and the pool of House members are likely outside of the control of a given firm as they are determined in nationwide elections. Second, we consider *Democrat*, a variable which takes a value of 1 if a bank's local Representative was a Democrat in the 2007-2008 campaign election, following Li (2013). As noted in Li (2013), ideology might affect a Representative's actions about TARP, with Republicans thought to be generally more opposed to government bailouts, and Democrats more in favor. While this indicates that *Democrat* should be positively related to TARP bailout decisions, the distribution of representatives in districts are likely outside of the control of a given firm. Third, we consider *Fed Director*, a variable which takes a value of 1 if one of the bank's directors was on the board of directors of one of the 12 Federal Reserve Banks or their branches in 2008 or 2009, following Bayazitova and Shivdasani (2012), Duchin and Sosyura (2014), and Li (2013). The Federal Reserve Bank evaluated the TARP applications of all its member banks and all bank holding companies. Therefore, this variable may be positively related to TARP decisions[17] However, the decision of a director to serve on the board of directors of one of the Federal Reserve Banks or branches likely occurred in the past and thus it would be outside the control of a given firm.

[16] We use the MABLE/Geocorr2k software on the Missouri Census Data Center website to associate banks with congressional districts by using the zip codes of their headquarters. The final regression sample for this test is 174,510 bank-quarter observations, less than for the main regression sample. This is due to some of the banks that could not be mapped into a congressional district (either due to an invalid headquarters zipcode or because there is not an exact match to a congressional district), a problem reported also by Li (2013).

[17] Banking organizations first had to submit their TARP applications to their primary federal regulators, which reviewed the applications and sent them together with their recommendations to the U.S. Treasury, which made the final decisions on whether or not to make the capital purchases (e.g., Li, 2013).

Because the basis of the TARP Recipient variable is binary and we need instruments to predict the treatment, we employ a dummy endogenous variable model and follow a 3-step approach as in Wooldridge (2002) procedure 18.4.1. For the first stage, we use a bank-level probit model in which we regress the TARP Recipient dummy (equal to one if the bank was provided TARP capital support) on political and regulatory instruments and all bank controls from the main regression model for predicting the probability of receiving TARP. We then aggregate the TARP recipient dummy fitted value from the first stage weighted by the banks' deposits proportions in the states and use this variable as an instrument for the second stage.[18]

The IV regressions are reported in Table 3. We report the first-stage results in Table 3 Panel A, and the final-stage results for the IV specification in Table 3 Panel B, with columns (1) and (2) for net job creation and net hiring establishments, and columns (3) and (4) for business and personal bankruptcies, respectively. The first-stage results in column (1) indicate that the instrumental variables are positively related to TARP injections, and the first-stage *F*-test suggests that instruments are valid.

The final stage results in Panel B show that the main results continue to hold. There are statistically and economically significant improvements in economic conditions. We find that on average over the post-TARP period, for every 1000 people, 7.86 jobs were created, 1.46 more establishments created jobs, and 0.069 business bankruptcies and 1.33 personal bankruptcies were eliminated due to TARP. Based upon the IV estimates, we again conclude that saving Wall Street may have helped save Main Street.

8.1.2 Heckman's (1979) Two-Stage Self-selection Model

[18] Wooldridge (2002) procedure 18.4.1 is useful when the potentially endogenous variable X is binary, since the estimation is typically woefully inefficient when 2SLS is used directly for this case. Wooldridge's method is also suggested in Angrist and Pischke (2009), who argue that the conditional expectation function of the first 2SLS stage is probably nonlinear when an endogenous variable is dichotomous. Improved efficiency may be obtained by first regressing X on the included and excluded instruments via probit or logit, predicting the probability $\hat{X}$, and using $\hat{X}$ as the single instrument (this method involves three steps and not just two). We follow this approach and use a probit for predicting the probability of the TARP Recipient dummy and instrument our *TARP Recipient* variable by the weighted TARP Recipient dummy fitted value and *Post TARP x TARP Recipient* by the product of the Post TARP dummy and the weighted TARP Recipient dummy fitted value. As indicated in Wooldridge (2002, pp. 236-237), this method is not the same as the forbidden regression, as we use the obtained variables as instruments, and not as regressors.

To address potential self-selection bias, we use Heckman's (1979) two-step procedure. This approach controls for self-selection bias induced by banks obtaining TARP capital by incorporating the TARP injection decision into the econometric estimation. In the first step, we use the same probit model from the IV estimation to regress the *TARP Recipient* dummy on all control variables from our main specification and our instrumental variables. In the second stage, the local economic indicators are the dependent variables, and we include the self-selection parameter (inverse Mills ratio) estimated from the first stage weighted by the banks' deposits' proportions in the states at the state level.

The second-stage results are reported in Table 3 Panel C. The coefficients on the inverse Mills ratio are not statistically significant, suggesting that sample selection bias is not an issue. Nevertheless, controlling for potential self-selection bias, the results of the two-step estimation model continue to suggest that TARP is associated with statistically and economically significant improvements in local economic conditions. In the outcome equation, the economic indicators suggest that on average over the post-TARP period, for every 1000 people, 7.87 jobs were created, 1.61 more establishments created jobs, and 0.069 business bankruptcies and 1.05 personal bankruptcies were eliminated due to TARP, consistent with our prior findings.

8.2 Placebo Experiment

We are also concerned that alternative forces may drive our main results. We therefore conduct a placebo experiment following Puddu and Walchli (2013). We fictionally assume that the TARP participation took place four years earlier, while still distinguishing between banks that received TARP and those that did not according to the "true" TARP program. To mimic our main analysis, we use an eight-year period immediately preceding the TARP program from 2001-2008, and assume that the fictional *Post TARP* period begins four years before the actual program. Thus, we rerun the regressions using the placebo sample (2001-2008) and define *Placebo Post TARP* as a dummy equal to one in 2005-2008, the period

after the fictional TARP program initiation.[19] If our main results reflect the true program, we should not find positively significant results for the DID terms on *Net Job Creation / Capita* and *Net Hiring Establishments / Capita,* nor should we not find negatively significant results for the DID terms on *Business Bankruptcies / Capita* and *Personal Bankruptcies / Capita*.

The placebo experiment results are reported in Table 4. We find that the DID coefficients are either statistically insignificant or go in the opposite direction of our main results. For net job creation and net hiring establishments, the fictional TARP effects are statistically insignificant. For business and personal bankruptcies, the fictional TARP effects are positive and statistically significant (opposite direction of main results). In the markets where more TARP banks were located, there may have been worse economic conditions at the beginning of the financial crisis, which corresponds to part of the fictional *Post TARP* period in the placebo experiments. Thus, it appears that our main results are not driven by alternative forces.

8.3 Alternative Econometric Specifications

To account for possible correlation among error terms at the state level, in Table 5 Panel A, we present a model with standard errors clustered at the state level. In Table 5 Panel B, we test robustness using specifications with state random effects in place of state fixed effects. In Table 5 Panel C, we exclude all bank-related variables to mitigate the possibility that TARP affects local market economic conditions through affecting the characteristics and health of the recipient banks. In Table 5 Panel D, we exclude all state-related variables. In all specifications, we continue to find support for our earlier results.

8.4 Other Robustness Tests

To get a clearer distinction between states with more or less proportions of TARP banks, we split states into quartiles according to the proportions of TARP recipients in the state. In Table 6 Panel A, we remove the two middle quartiles (2 and 3) and reestimate the results using only quartiles 1 and 4. As an alternative

[19] In these regressions, we include all controls as in our main analysis, except that we are not able to include *Management Quality* because of data limitations on enforcement actions (only available from 2005 onwards).

test, in Table 6 Panel B, we split the states into terciles and remove tercile 2 from the estimations. We find that results are robust to these tests.

8.5 Dynamics of TARP Effects on Local Economic Conditions

We next examine the dynamics of the relation between TARP and local economic conditions in a similar fashion to Beck, Levine, and Levkov (2010). We include a series of dummy variables in the standard regression to trace out the year-by-year effects of TARP. In the regressions, we replace the DID term *Post* $TARP_t$ *x TARP Recipient*$_{st}$ from equation (1) with interactions of the *TARP Recipient*$_{st}$ with year dummies for each full year before and after TARP initiation.

$$\begin{aligned} Y_{st} = {} & \lambda_0 + \lambda_1 \cdot TARP\ Recipient_{st} + \lambda_2 \cdot Post\ TARP_t \\ & + \lambda_3 \cdot D^{2006}{}_t\, x\ TARP\ Recipient_{st} + \lambda_4 \cdot D^{2007}{}_t\, x\ TARP\ Recipient_{st} + ... \\ & + \lambda_9 \cdot D^{2012}{}_t\ x\ TARP\ Recipient_{st} + \\ & + \lambda_{10} \cdot X_{st-1} + \lambda_{11} \cdot State_s + \lambda_{12} \cdot Time_t + \zeta_{st} \end{aligned} \tag{2}$$

where Y_{st}, *TARP Recipient*$_{st}$, *Post* $TARP_t$, X_{st-1}, $State_s$, and $Time_t$ are defined as above, the "*D*s" are year dummies, and ζ_{st} represents a white noise error term.[20] We plot the DID coefficients with their 95% confidence intervals in Figure 1, Graphs A, B, C, and D.

Graphs A and B of Figure 1 illustrate that there are immediate increases in net job creation and net hiring establishments in 2009, but these increases are short-lived, only lasting through 2010. In Graphs C and D of Figure 1, we find that there are decreases in business and personal bankruptcies immediately after TARP injections. This decline is slow and steady over the whole post-TARP period.

8.6 Decomposition of Local Economic Conditions

In Appendix X, we decompose our four indicators of local economic conditions and examine the effects of TARP on each of the components. We find that the net job creation findings are due to both an increase in gross job creation and a decrease in gross job destruction, and that the net hiring establishment findings

[20] We use the X11 procedure developed by the U.S. Census Bureau to deseasonalize the dependent variables for this procedure, following Beck, Levine, and Levkov (2010).

are due to both an increase in gross hiring establishments and a decrease in gross firing establishments. We find that TARP had effects on business bankruptcies via Chapter 7 (liquidations) and 13 filings (adjustments of debts – small amounts), and that the personal bankruptcies findings are primarily due to reductions in bankruptcies through Chapter 7 (liquidations).

8.7 Subsample Analyses

In Appendix Y, we conduct several subsample analyses to see for which types of banks and under what local economic conditions TARP was most effective. The data suggest that: 1) only the medium and large TARP banks have statistically significant results, particularly the medium banks; 2) in most cases, the voluntary and non-stress-tested banks appear to be responsible for most of the gains; 3) most of the gains are due to TARP banks that did not repay early; and 4) improvement results are primarily due to banks in the states with poor economic conditions and states with low economic freedom.

9. Conclusions

Did saving Wall Street through TARP really save Main Street during the recent financial crisis? We provide the first empirical evidence on this important question and the answer appears to be yes. Our difference-in-difference analysis suggests that TARP led to statistically and economically significant improvements in economic conditions in the local markets in which it was applied. These measured effects may be understated because they do not capture any benefits to the economy from possible stabilization of the financial system due to TARP that may have occurred.

This paper contributes to the research and policy debates on the costs and benefits of the TARP program. Among the costs identified in the prior research are any increases in moral hazard incentives to take on excessive risk because of the increased expectation of future bailouts, which may have occurred for large banks (Black and Hazelwood, 2013; Duchin and Sosyura, 2014), a possible reduction in lending by large banks (Black and Hazelwood, 2013), distortion in competition caused by the bailouts of some banks and not others (Koetter and Noth, 2014; Berger and Roman, forthcoming), distortion caused by the bailouts being partially distributed according to political and regulatory connections (Bayazitova and

Shivdasani, 2012: Duchin and Sosyura, 2012, 2014; Li, 2013: Berger, Makaew, and Roman, 2014; Berger and Roman, forthcoming), any reductions in the market values of the TARP recipient banks' traded customers (Liu, 2013), and the small profit to the Treasury that likely did not compensate for the risks. Among the benefits identified in the literature are the possible increase in lending and reduction in risk by small banks (Black and Hazelwood, 2013; Li, 2013; Puddu and Walchli, 2013), increases in the market values of traded recipient banks (Veronesi and Zingales, 2010; Ng, Vasvari, and Wittenberg-Moerman, 2013; Harrisa, Huertab, and Ngob, 2013), any increases in the market values of recipient banks' traded relationship customers (Norden, Roosenboom, and Wang, 2013), and better large loan contract terms for both relationship and non-relationship borrowers (Berger, Makaew, and Roman, 2014).[21]

However, the two potentially most important effects of TARP are not measured in the research literature: the impact on the overall stability of the financial system and the effects on the lives of average Americans – i.e., Main Street. Both of these outcomes are difficult to measure because so many other government programs and market events occurred around the same time period. While the impact of TARP on overall financial stability may not be possible to assess, we attempt to measure the second set of effects by studying the effects of TARP on local market economic conditions. Our study adds to the debate on benefits and costs of TARP by offering the first evidence on the effect on local economic conditions, which appears to be a benefit. Overall, the results suggest that saving Wall Street through TARP may have helped save Main Street during the recent financial crisis.

[21] For a more detailed discussion of TARP benefits and costs, see Calomiris and Khan (forthcoming).

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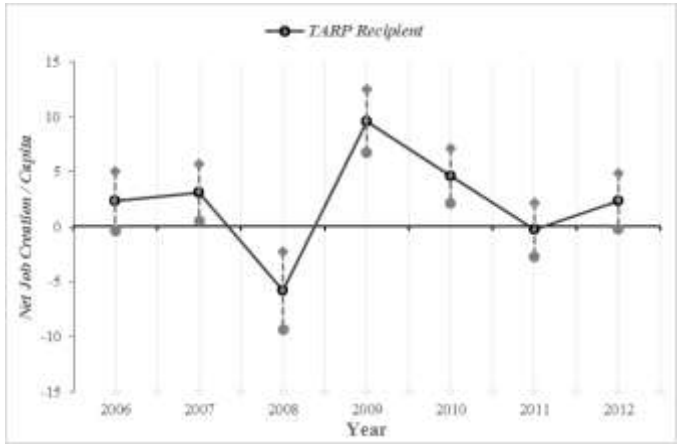
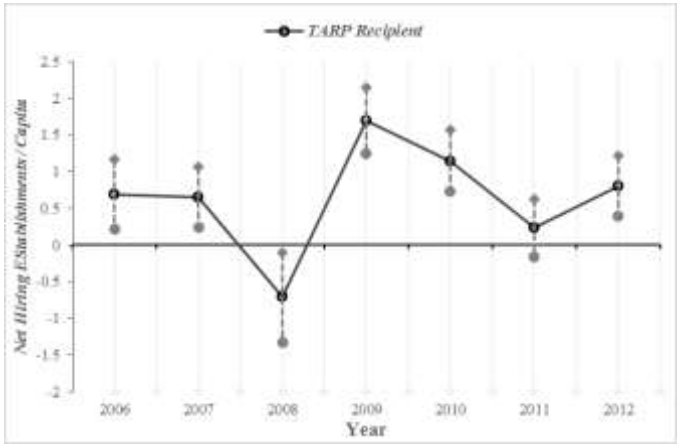
Figure 1: The Dynamic Impacts of TARP on Local Economic Conditions

Graph A of Figure 1 plots the coefficients for the dynamic impact of TARP on Net Job Creation / Capita, adjusted for seasonality (represented by small circles), with their 95% confidence intervals (represented by the dashed lines). The coefficients are the interactions of the TARP Recipient variable with year dummies for each full year before and after the TARP program. Graph B of Figure 1 plots the coefficients for the dynamic impact of TARP on Net Hiring Establishments / Capita, adjusted for seasonality (represented by small circles), with their 95% confidence intervals (represented by the dashed lines). Graph C of Figure 1 plots the coefficients for the dynamic impact of TARP on Business Bankruptcies / Capita, adjusted for seasonality (represented by small circles), with their 95% confidence intervals (represented by the dashed lines). Graph D of Figure 1 plots the coefficients for the dynamic impact of TARP on Personal Bankruptcies / Capita, adjusted for seasonality (represented by small circles), with their 95% confidence intervals (represented by the dashed lines).

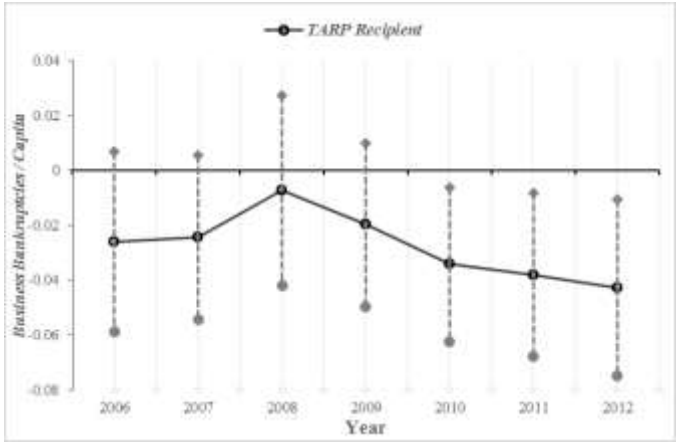
Graph A. The Dynamic Impacts of TARP on Net Job Creation / Capita



Graph B. The Dynamic Impacts of TARP on Net Hiring Establishments



Graph C. The Dynamic Impacts of TARP on Business Bankruptcies / Capita



Graph D. The Dynamic Impacts of TARP on Personal Bankruptcies / Capita

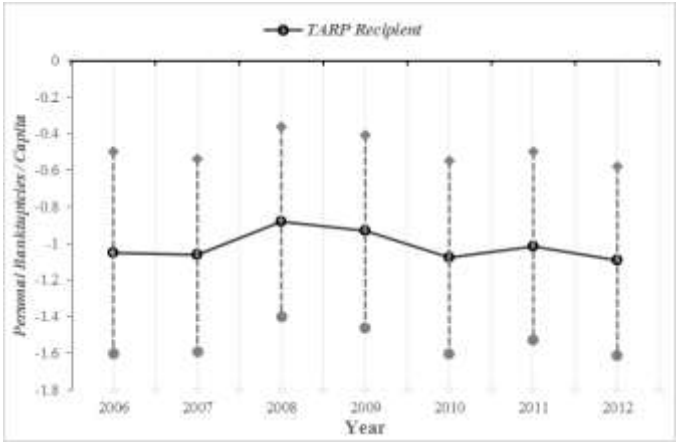


Table 1: Definitions and Summary Statistics

This table reports summary statistics for the full U.S. bank sample. This table reports summary statistics of the variables for the full sample. All variables using dollar amounts are expressed in real 2012:Q4 dollars using the implicit GDP price deflator.

Variable Definitions and Summary Statistics for the Full Sample (2005-2012)

Type	*Variable*	*Definition*	*Mean*	*Median*	*SD*	*p25*	*p75*
Net Job Creation and Hiring Establishments Variables *(Source: US Department of Labor)*	***Net Job Creation/ Capita***	The overall net job creation per capita calculated as: (Gross Job Creation - Gross Job Destruction) / (Population / 1000). Gross Job Creation is the number of jobs created. It consists of job openings and expansions. Openings are number of jobs created at new establishments. Expansions are number of new jobs created at existing establishments. Gross Job Destruction is the number of jobs destroyed. It consists of job closings and contractions, defined analogously.	0.207	0.645	3.613	-1.256	2.257
	Components of Net Job Creation / Capita						
	Gross Job Creation / Capita	The gross job creation per capita is the number of jobs created calculated as: calculated as: (Gross Job Creation) / (Population / 1000). It consists of job openings and expansions.	24.853	23.711	5.367	21.337	26.786
	Components of Gross Job Creation / Capita						
	Gross Job Creation - Openings / Capita	The job openings per capita, calculated as: (Number of new jobs created at new establishments) / (Population / 1000).	4.750	4.474	1.347	3.839	5.326
	Gross Job Creation - Expansions / Capita	The expansions per capita, calculated as: (Number of new jobs created at existing establishments that expand their operations) / (Population / 1000).	20.102	19.303	4.325	17.444	21.510
	Gross Job Destruction / Capita	The gross job destruction per capita is the number of jobs destroyed, calculated as: ((Number of jobs destroyed) / (Population / 1000). It consists of job closings and contractions.	24.646	23.873	5.080	21.258	26.880
	Components of Gross Job Destruction / Capita						
	Gross Job Destruction - Closings / Capita	The job closings per capita, calculated as: (Number of jobs lost due to closing establishment closings) / (Population / 1000).	4.432	4.191	1.288	3.636	5.031
	Gross Job Destruction - Contractions / Capita	The contractions per capita, calculated as: (Number of jobs lost due to existing establishments that contract their operations) / (Population / 1000).	20.213	19.553	4.163	17.415	21.953
	Net Hiring Establishments / Capita	The overall net hiring establishments per capita, calculated as: (Gross Hiring Establishments - Gross Firing Establishments) / (Population / 1000). Gross Hiring Establishments is the number of hiring establishments that create jobs. It consists of establishments that create jobs through job openings and expansions. Gross Firing Establishments is the number of establishments that destroy jobs. It consists of establishments that destroy jobs through job closings and contractions.	-0.157	-0.074	0.642	-0.467	0.227
	Components of Net Hiring Establishments / Capita						
	Gross Hiring Establishments / Capita	The gross hiring establishments per capita is the number of hiring establishments that create jobs, calculated as: (Number of establishments that created jobs) / (Population / 1000). It consists of establishments that created jobs through openings and expansions.	6.649	6.291	1.372	5.690	7.343
	Components of Gross Hiring Establishments						
	Gross Hiring Establishments - Openings / Capita	The hiring establishments that create jobs via openings per capita, calculated as: (Number of new establishments that created jobs via openings) / (Population/1000).	1.299	1.217	0.373	1.021	1.565
	Gross Hiring Establishments - Expansions / Capita	The hiring establishments that create jobs via expansions per capita, calculated as: (Number of establishments that created jobs via operations expansions) / (Population/1000).	5.350	5.084	1.085	4.627	5.823
	Gross Firing Establishments / Capita	The gross firing establishments per capita is the number of firing establishments that create jobs, calculated as: (Number of establishments that destroyed jobs) / (Population/1000). It consists of establishments that destroyed jobs through closings and contractions.	6.806	6.432	1.333	5.882	7.498
	Components of Gross Firing Establishments						
	Gross Firing Establishments - Closings / Capita	The firing establishments that destroy jobs via closings per capita, calculated as: (Number of establishments that destroyed jobs via closings) / (Population / 1000).	1.291	1.206	0.371	1.020	1.532
	Gross Firing Establishments - Contractions / Capita	The firing establishments that destroy jobs via contractions per capita, calculated as: (Number of establishments that destroyed jobs via contractions) / (Population / 1000).	5.515	5.243	1.058	4.783	6.026

Variable Definitions and Summary Statistics for the Full Sample (2005-2012)

Type	*Variable*	*Definition*	*Mean*	*Median*	*SD*	*p25*	*p75*
Bankruptcies Variables *(Source: American Bankruptcy Institute, US Court Filings)*	***Business Bankruptcies / Capita***	The overall number of business bankruptcies per capita, calculated as: (Total business bankruptcy filings) / (Population / 1000). Business bankruptcies consist of Chapter 7 filings (corporate liquidations), Chapter 11 filings (large corporate reorganizations), Chapter 12 filings (corporate reorganizations for farms and fisheries), and Chapter 13 filings (orderly plan for small debt repayment) filings.	0.038	0.030	0.040	0.021	0.042
	Components of Business Bankruptcies / Capita						
	Business Bankruptcies - Chapter 7 / Capita	Number of Chapter 7 business bankruptcies per capita, calculated as: ((Total Chapter 7 business bankruptcy filings) / (Population / 1000).	0.025	0.021	0.018	0.014	0.030
	Business Bankruptcies - Chapter 11 / Capita	Number of Chapter 11 business bankruptcies per capita, calculated as: ((Total Chapter 11 business bankruptcy filings) / (Population / 1000).	0.009	0.005	0.033	0.003	0.008
	Business Bankruptcies - Chapter 12 / Capita	Number of Chapter 12 business bankruptcies per capita, calculated as: ((Total Chapter 12 business bankruptcy filings) / (Population / 1000).	0.001	0.000	0.001	0.000	0.001
	Business Bankruptcies - Chapter 13 / Capita	Number of Chapter 13 business bankruptcies per capita, calculated as: ((Total Chapter 13 business bankruptcy filings) / (Population / 1000).	0.003	0.002	0.003	0.001	0.004
	Personal Bankruptcies/ Capita	The overall number of personal bankruptcies per capita, calculated as: (Total personal bankruptcy filings) / (Population / 1000). Personal bankruptcies consist of Chapter 7 filings (straight bankruptcy or liquidation), Chapter 11 filings (personal reorganization), and Chapter 13 filings (wage earner plan for debt repayment) filings.	1.036	0.832	0.810	0.539	1.303
	Components of Business Bankruptcies / Capita						
	Personal Bankruptcies - Chapter 7 / Capita	Number of Chapter 7 personal bankruptcies per capita, calculated as: ((Total Chapter 7 personal bankruptcy filings) / (Population / 1000).	0.760	0.602	0.672	0.378	0.907
	Personal Bankruptcies - Chapter 11 / Capita	Number of Chapter 11 personal bankruptcies per capita, calculated as: ((Total Chapter 11 personal bankruptcy filings) / (Population / 1000).	0.001	0.000	0.001	0.000	0.001
	Personal Bankruptcies - Chapter 13 / Capita	Number of Chapter 13 personal bankruptcies per capita, calculated as: ((Total Chapter 13 personal bankruptcy filings) / (Population / 1000).	0.275	0.211	0.243	0.109	0.330

Variable Definitions and Summary Statistics for the Full Sample (2005-2012)

Type	*Variable*	*Definition*	*Mean*	*Median*	*SD*	*p25*	*p75*
Control Variables *(Source: Call Reports, Summary of Deposits, Bank List with Corrective Actions, US Census website, NBER, Tax Policy Center, FHFA)*	*CAMELS Proxy: Capital Adequacy*	The weighted proportion of the bank capitalization ratio in the local markets. Capitalization ratio is defined as equity capital divided by GTA. Capital adequacy refers to the amount of a bank's capital relative to its assets. Broadly, this criterion evaluates the extent to which a bank can absorb potential losses.	0.105	0.101	0.021	0.093	0.112
	CAMELS Proxy: Asset Quality	The weighted proportion of the bank asset quality in the local markets. Asset quality evaluates the overall condition of a bank's portfolio and is typically evaluated by a fraction of nonperforming assets and assets in default. Noncurrent loans and leases are loans that are past due for at least ninety days or are no longer accruing interest. Higher proportion of nonperforming assets indicates lower asset quality.	0.007	0.002	0.015	0.001	0.006
	CAMELS Proxy: Management Quality	The weighted proportion of the bank management quality in the local markets. Management quality is the negative of the number of corrective actions that were taken against bank executives by the corresponding banking regulator during the sample period 2005-2012 (FED, OTS, FDIC, and OCC).	-0.001	0.000	0.003	0.000	0.000
	CAMELS Proxy: Earnings (ROA)	The weighted proportion of the bank earnings in the local markets. Return on assets (ROA) is measured as the ratio of the annualized net income to GTA.	0.022	0.019	0.100	0.009	0.033
	CAMELS Proxy: Liquidity	The weighted proportion of the bank liquidity in the local markets. Liquidity is defined as cash divided by bank total deposits.	0.079	0.059	0.107	0.040	0.089
	CAMELS Proxy: Sensitivity to Market Risk	The weighted proportion of the bank sensitivity to interest rate risk in the local markets. The sensitivity to interest rate risk is defined as the ratio of the absolute difference (gap) between short-term assets and short-term liabilities to GTA.	0.146	0.099	0.546	0.045	0.187
	DWTAF	The weighted proportion of banks receiving Discount Window loans and/or Term Auction Facility (TAF) funding during the crisis in the local markets.	0.241	0.162	0.215	0.085	0.348
	Bank Size	The weighted proportion of the bank size in the local markets. Bank size is the log value of GTA.	14.959	13.834	3.151	12.421	18.231
	HHI Deposits	The weighted proportion of banks' deposits concentration, measured by the Herfindahl-Hirschman Deposits Index and determined using the bank deposit data from the FDIC Summary of Deposits. Higher values show greater market concentration.	588.823	450.622	449.055	258.668	851.763
	State No Banks	The logarithm of the total number of banks in the state.	4.180	4.369	1.225	3.401	5.106
	Metropolitan	The weighted proportion of banks having the the majority of bank deposits (50% or more) in metropolitan areas in the state.	0.336	0.276	0.233	0.160	0.461
	State Minimum Wage	Minimum wage mandated by state law in $/hour. If there are no minimum wage laws in the state, then minimum wage equals the Federal minimum wage.	6.757	7.161	0.985	5.855	7.337
	State Marginal Tax Rate	Top marginal rate of the state's income tax.	5.214	5.830	2.929	3.400	7.050
	State Economic Freedom Index	The the state-level index of economic freedom.	6.769	6.800	0.568	6.400	7.200
	State House Price Inflation	The the state-level house price inflation calculated as the quarterly change of the seasonally-adjusted Federal Housing Financing Agency (FHFA) house price index for the states.	-0.519	-0.440	2.379	-1.906	0.869
	State Fixed Effects	State fixed effects, represented by dummy variables for each state of the sample period.					
	Time Fixed Effects	Time fixed effects, represented by year and quarter dummy variables for the sample period.					
Instrumental Variables: *(Sources: Center for Responsive Politics, House of Representatives website, Missouri Census Data Center)*	*Subcommittees on Financial Institutions or Capital Markets*	A dummy which takes a value of 1 if a firm is headquartered in a district of a House member, who served on the Capital Markets Subcommittee or the Financial Institutions Subcommittee of the House Financial Services Committee in 2008 or 2009.	0.088	0.000	0.228	0.000	0.000
	Democrat	A dummy variable which takes a value of 1 if a bank's local Representative was a Democrat in the 2007-2008 campaign election cycle.	0.429	0.000	0.495	0.000	1.000
	Fed Director	A dummy that equals 1 if a bank's director sat on the board of directors of a Federal Reserve Bank (FRB) or its branch in 2008 or 2009.	0.013	0.000	0.111	0.000	0.000

Table 2: Effects of TARP on Local Economic Conditions: Main Results

This table reports estimates from difference-in-difference (DID) regression estimates for analyzing the impact of TARP on local economic conditions. The measures of local conditions are *Net Job Creation / Capita*, *Net Hiring Establishments / Capita*, *Business Bankruptcies / Capita*, and *Personal Bankruptcies / Capita*. *TARP Recipient* is the weighted proportion of TARP banks receiving TARP in the local markets, *Post TARP* is a dummy equal to one in 2009-2012, the period after TARP program initiation. All models include state and time fixed effects. The estimation results are for 2005-2012. All variables are defined in Table 1. *, **, and *** denote significance at 10%, 5%, and 1% level.

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-3.444**	-0.916***	0.047**	0.295*
	(-2.398)	(-3.689)	(2.332)	(1.727)
Post TARP	-0.385	-0.081	-0.011**	-0.931***
	(-0.796)	(-1.080)	(-2.264)	(-16.879)
Post TARP x TARP Recipient	3.243***	0.640***	-0.021***	-0.432***
	(4.233)	(5.049)	(-2.859)	(-5.385)
Capital Adequacy	-2.673	-0.081	-0.104**	-0.462
	(-0.730)	(-0.141)	(-2.089)	(-1.126)
Asset Quality	-6.698	-1.684	0.062	0.473
	(-0.965)	(-1.114)	(1.459)	(1.037)
Management Quality	-24.680	-0.145	0.103	-1.761
	(-1.176)	(-0.039)	(0.712)	(-0.928)
Earnings	14.865***	2.618***	0.004	0.031
	(5.237)	(5.033)	(0.128)	(0.117)
Liquidity	-1.862	-0.251	0.009	-0.028
	(-1.437)	(-1.068)	(0.444)	(-0.219)
Sensitivity to Market Risk	-2.368***	-0.441***	-0.003	0.004
	(-4.458)	(-4.746)	(-0.419)	(0.076)
DWTAF	0.243	0.439	0.015	0.274
	(0.143)	(1.501)	(0.705)	(1.556)
Bank Size	0.169	0.039	-0.005***	-0.081***
	(0.993)	(1.541)	(-3.026)	(-4.640)
HHI Deposits	0.001*	0.000**	0.000	0.000
	(1.672)	(2.423)	(1.155)	(0.991)
State No Banks	0.504	-0.114*	-0.001	0.006
	(1.134)	(-1.715)	(-0.286)	(0.152)
Metropolitan	-3.557	-1.120***	0.017	1.192***
	(-1.310)	(-2.700)	(0.669)	(4.707)
State Minimum Wage	-0.082	-0.037	0.001	-0.018
	(-0.580)	(-1.630)	(0.764)	(-1.254)
State Marginal Tax Rate	-0.045	-0.004	0.002	0.020
	(-0.343)	(-0.174)	(1.581)	(1.535)

State Economic Freedom Index	-0.665	-0.045	-0.017**	-0.038
	(-1.171)	(-0.459)	(-2.122)	(-0.406)
State House Price Inflation	0.302***	0.057***	-0.001*	-0.012***
	(6.534)	(8.837)	(-1.823)	(-3.385)
Intercept	4.974	1.171	0.160**	2.181***
	(1.115)	(1.521)	(2.228)	(3.193)
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.485	0.571	0.626	0.874

Table 3: Effects of TARP on Local Economic Conditions – Instrumental Variable Analysis and Heckman Selection Model

This table shows difference-in-difference (DID) regression estimates for analyzing the impact of TARP on local economic conditions using an instrumental variable approach as in Wooldridge Section 18.4.1 (Panels A and B) and Heckman Selection Model (Panels A and C). We use as instruments several political and regulatory connections variables: *Subcommittees on Financial Institutions or Capital Markets*, *Democrat*, and *Fed Director*. *Subcommittees on Financial Institutions or Capital Markets* is a variable, which takes a value of 1 if a firm is headquartered in a district of a House member, who served on the Capital Markets Subcommittee or the Financial Institutions Subcommittee of the House Financial Services Committee in 2008 or 2009. *Democrat* is a variable, which takes a value of 1 if a bank's local Representative was a Democrat in the 2007-2008 campaign election cycle. *Fed Director* is a variable that equals 1 if a bank's director sat on the board of directors of a Federal Reserve Bank (FRB) or of a branch of a FRB in 2008 or 2009. The measures of local conditions are *Net Job Creation / Capita*, *Net Hiring Establishments / Capita*, *Business Bankruptcies / Capita*, and *Personal Bankruptcies / Capita*. *TARP Recipient* is the weighted proportion of TARP banks receiving TARP in the local markets, *Post TARP* is a dummy equal to one in 2009-2012, the period after TARP program initiation. All models include state and time fixed effects. The estimation results are for 2005-2012. All variables are defined in Table 1. *, **, and *** denote significance at 10%, 5%, and 1% level.

Panel A: First Stage – IV (as in Wooldridge (Section 18.4.1) and Heckman Selection Model

Dependent Variable:	***TARP Recipient***
Independent Variables:	**(1)**
Subcommittees on Financial Institutions or Capital Markets	0.108***
	(5.687)
Democrat	0.027***
	(2.913)
FED Director	0.361***
	(12.577)
Bank Controls	**YES**
Time Fixed Effects	**YES**
Observations	172,002
Pseudo R-squared	0.216

Panel B: IV 2SLS – Final Stage as in Wooldridge (Section 18.4.1)

Dependent Variable:	*Net Job Creation / Capita*	*Net Hiring Establishments / Capita*	*Business Bankruptcies / Capita*	*Personal Bankruptcies / Capita*
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-3.147*	-0.673**	0.022	0.324**
	(-1.770)	(-2.009)	(1.149)	(2.077)
Post TARP	-0.375	-0.077	-0.009**	-0.917***
	(-0.797)	(-1.041)	(-2.089)	(-17.151)
Post TARP x TARP Recipient	3.148***	0.585***	-0.028***	-0.534***
	(3.992)	(4.361)	(-3.625)	(-5.827)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.485	0.571	0.624	0.874
First Stage Kleibergen-Paap rk Wald F-test	372.388***	372.388***	372.388***	372.388***

Panel C: Heckman Selection Model – Outcome Equation

Dependent Variable:	*Net Job Creation / Capita*	*Net Hiring Establishments / Capita*	*Business Bankruptcies / Capita*	*Personal Bankruptcies / Capita*
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-4.126**	-0.869***	0.051*	0.368***
	(-2.492)	(-2.842)	(1.701)	(2.701)
Post TARP	-0.385	-0.081	-0.011**	-0.931***
	(-0.798)	(-1.080)	(-2.267)	(-16.871)
Post TARP x TARP Recipient	3.153***	0.646***	-0.021***	-0.422***
	(4.003)	(5.022)	(-2.614)	(-5.128)
Inverse Mills Ratio	0.756	-0.052	-0.004	-0.081
	(0.692)	(-0.272)	(-0.298)	(-0.664)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.485	0.571	0.624	0.874

Table 4: Effects of TARP on Local Economic Conditions: Placebo Experiment

This table reports difference-in-difference (DID) regression estimates for analyzing the impact of TARP on local economic conditions. We use placebo experiments, in which we fictionally assume that the TARP participation took place four years earlier and we still distinguish between banks that received TARP and those that did according to their "true" TARP program. We define *Placebo Post TARP* as a dummy equal to one in 2005-2008, the period after the fictional TARP program initiation and we run the regressions by using the placebo-sample (2001-2008). The measures of local conditions are *Net Job Creation / Capita*, *Net Hiring Establishments / Capita*, *Business Bankruptcies / Capita*, and *Personal Bankruptcies / Capita*.. *TARP Recipient* is the weighted proportion of TARP banks receiving TARP in the local markets. All models include state and time fixed effects. The estimation results are for 2005-2012. All variables are defined in Table 1. *, **, and *** denote significance at 10%, 5%, and 1% level.

Panel A: Placebo Experiment (TARP Participation is Assumed to Have Taken Place Four Years Earlier)

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-0.798	-0.279	-0.025**	-0.242
	(-0.728)	(-1.606)	(-2.119)	(-1.509)
Placebo Post TARP	1.534***	-0.175***	-0.008**	-0.674***
	(4.148)	(-3.230)	(-2.059)	(-15.040)
Placebo Post TARP x TARP Recipient	-0.125	-0.048	0.011*	0.448***
	(-0.169)	(-0.400)	(1.794)	(5.096)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,632	1,632	1,632	1,632
Adjusted R-squared	0.450	0.518	0.517	0.890

Table 5: Alternative Econometric Models

This table reports difference-in-difference (DID) regression estimates for the impact of TARP on local economic conditions using alternative econometric models: state and time fixed effects with errors clustered at the state level in Panel A, state random effects in Panel B, state and models excluding all bank-related controls in Panel C, and models excluding all state-related controls in Panel D. The measures of local conditions are *Net Job Creation / Capita*, *Net Hiring Establishments / Capita*, *Business Bankruptcies / Capita*, and *Personal Bankruptcies / Capita*. *TARP Recipient* is the weighted proportion of TARP banks receiving TARP in the local markets, *Post TARP* is a dummy equal to one in 2009-2012, the period after TARP program initiation. All models include time fixed effects. The estimation results are for 2005-2012. All variables are defined in Table 1. *, **, and *** denote significance at 10%, 5%, and 1% level.

Panel A: Regression Parameters –Error Clustering by State

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-3.444*	-0.916***	0.047	0.295**
	(-1.834)	(-3.063)	(1.561)	(2.128)
Post TARP	-0.385	-0.081	-0.011**	-0.931***
	(-0.742)	(-0.798)	(-2.267)	(-10.371)
Post TARP x TARP Recipient	3.243***	0.640***	-0.021**	-0.432***
	(3.928)	(4.685)	(-2.436)	(-4.724)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
State Clusters	51	51	51	51
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.485	0.571	0.626	0.874

Panel B: Regression Parameters – State Random Effects instead of State Fixed Effects

Dependent Variable:	*Net Job Creation / Capita*	*Net Hiring Establishments / Capita*	*Business Bankruptcies / Capita*	*Personal Bankruptcies / Capita*
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-3.005*	-0.719**	0.045	0.302**
	(-1.839)	(-2.398)	(1.571)	(2.248)
Post TARP	-0.240	-0.039	-0.005	-0.929***
	(-0.706)	(-0.693)	(-1.395)	(-11.247)
Post TARP x TARP Recipient	3.316***	0.666***	-0.021**	-0.430***
	(4.056)	(4.865)	(-2.436)	(-4.765)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Random Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.466	0.545	0.113	0.267

Panel C: Regression Parameters – Excluding All Bank-Related Controls

Dependent Variable:	*Net Job Creation / Capita*	*Net Hiring Establishments / Capita*	*Business Bankruptcies / Capita*	*Personal Bankruptcies / Capita*
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-1.394*	-0.342**	0.017***	0.302***
	(-1.664)	(-2.540)	(2.788)	(3.063)
Post TARP	-0.423	-0.025	-0.011***	-0.888***
	(-0.975)	(-0.360)	(-2.897)	(-18.345)
Post TARP x TARP Recipient	3.248***	0.650***	-0.016***	-0.508***
	(4.230)	(5.128)	(-2.790)	(-5.971)
Bank-Related Controls	**NO**	**NO**	**NO**	**NO**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,631	1,631	1,631	1,631
Adjusted R-squared	0.470	0.548	0.609	0.866

Panel D: Regression Parameters – Excluding All State-Related Controls

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-3.583**	-0.952***	0.044**	0.332**
	(-2.466)	(-3.758)	(2.349)	(2.040)
Post TARP	-0.547	-0.170***	-0.001	-0.904***
	(-1.537)	(-3.179)	(-0.386)	(-17.923)
Post TARP x TARP Recipient	3.363***	0.658***	-0.021***	-0.431***
	(4.290)	(4.987)	(-2.786)	(-5.388)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**NO**	**NO**	**NO**	**NO**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,611	1,611	1,611	1,611
Adjusted R-squared	0.459	0.540	0.623	0.872

Table 6: Additional Robustness Tests

This table reports difference-in-difference (DID) regression estimates for the impact of TARP on local economic conditions from additional robustness tests. Panel A reports estimates when using only quartiles 1 and 4 of the proportions of TARP recipients in the state. Panel B reports estimates when using only terciles 1 and 3 of the proportions of TARP recipients in the state. Panel C reports estimates when excluding state-quarters with no TARP banks. The measures of local conditions are *Net Job Creation / Capita*, *Net Hiring Establishments / Capita*, *Business Bankruptcies / Capita*, and *Personal Bankruptcies / Capita*. *TARP Recipient* is the weighted proportion of TARP banks receiving TARP in the local markets, *Post TARP* is a dummy equal to one in 2009-2012, the period after TARP program initiation. All models include time fixed effects. The estimation results are for 2005-2012. All variables are defined in Table 1. *, **, and *** denote significance at 10%, 5%, and 1% level.

Panel A: Regression Parameters – Quartiles 1 & 4

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-1.864	-0.929***	0.078**	0.453**
	(-0.950)	(-2.719)	(2.143)	(2.424)
Post TARP	-0.711	-0.088	-0.018**	-1.029***
	(-1.077)	(-0.809)	(-2.382)	(-14.980)
Post TARP x TARP Recipient	3.684***	0.663***	-0.026***	-0.413***
	(4.313)	(4.583)	(-3.046)	(-4.671)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	768	768	768	768
Adjusted R-squared	0.488	0.555	0.582	0.877

Panel B: Regression parameters – Terciles 1 & 3

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-2.185	-0.868***	0.063**	0.281
	(-1.326)	(-3.056)	(2.477)	(1.304)
Post TARP	-0.763	-0.098	-0.012**	-0.975***
	(-1.363)	(-1.064)	(-2.051)	(-15.926)
Post TARP x TARP Recipient	3.388***	0.672***	-0.027***	-0.403***
	(4.200)	(4.999)	(-3.286)	(-4.736)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,034	1,034	1,034	1,034
Adjusted R-squared	0.493	0.566	0.613	0.861

APPENDIX X – EFFECTS OF TARP ON THE COMPONENTS OF LOCAL ECONOMIC CONDITIONS

In Table X.1, we decompose our four indicators of local economic conditions and examine the effects of TARP on each component. We first decompose *Net Job Creation / Capita* into *Gross Job Creation / Capita* (openings and expansions) and *Gross Job Destruction / Capita* (closings and contractions) to shed light on the sources of the net job creation effects. Results in Panel A columns (1) and (4) suggest that our main net job creation findings are due to both an increase in gross job creation and a decrease in gross job destruction. In columns (2) and (3), we further decompose *Gross Job Creation / Capita* into its subcomponents of *Gross Job Creation - Openings / Capita* (job openings or jobs created at new establishments) and *Gross Job Creation - Expansions / Capita* (expansions or jobs created at existing establishments). We find that job expansions are the most important to explain the increase in gross job creation. Similarly, in columns (5) and (6), we further decompose *Gross Job Destruction / Capita* into its subcomponents of *Gross Job Destruction – Closings / Capita* (job closings or jobs lost due to closing establishments) and *Gross Job Destruction – Contractions / Capita* (contractions or jobs lost at existing establishments that contract operations). Job contractions appear to be the most important to explain the decrease in gross job destruction.

We next decompose *Net Hiring Establishments / Capita* into *Gross Hiring Establishments / Capita* and *Gross Firing Establishments / Capita.* Results in Table X.1 Panel B columns (1) and (4) suggest that our main net hiring establishment findings are due to both an increase in *Gross Hiring Establishments / Capita* and a decrease in *Gross Firing Establishments / Capita*. In columns (2) and (3), we further decompose *Gross Hiring Establishments / Capita* into its subcomponents of *Gross Hiring Establishments - Openings / Capita* (establishment openings or new establishments that create jobs) and *Gross Hiring Establishments - Expansions / Capita* (establishment expansions or establishments that expand their operations and create jobs). We find statistically significant increases in establishment expansions. Similarly, in columns (5) and (6), we further decompose *Gross Firing Establishments / Capita* into its subcomponents of *Gross Firing Establishments - Closings / Capita* (closing establishments that destroy

jobs) and *Gross Firing Establishments - Contractions / Capita* (contractions or continuing establishments that destroy jobs). We find that establishment contractions are the most important to explain the decrease in gross firing establishments.

As shown in Bris, Welch, and Zhu (2006), there may be differences among the different incentives and conditions that lead a firm to choose one bankruptcy filing over another. Therefore, we decompose *Business Bankruptcies / Capita* into its components: *Business Bankruptcies / Capita - Chapter 7* (liquidations), *Business Bankruptcies / Capita - Chapter 11* (corporate reorganizations), *Business Bankruptcies / Capita - Chapter 12* (adjustments of debts), and *Business Bankruptcies / Capita - Chapter 13* (adjustments of debts – small amounts), where the first two types of filings are typically for large corporations. Results in Panel C columns (1) - (4) suggest that there are statistically significant reductions in bankruptcies through Chapters 7 and 13 filings. The reduction in bankruptcies through Chapter 7 is also large relative to the sample mean. We also decompose *Personal Bankruptcies / Capita* into its components: *Personal Bankruptcies / Capita - Chapter 7*, *Personal Bankruptcies / Capita - Chapter 11*, and *Personal Bankruptcies / Capita - Chapter 13*. Results in Panel D columns (1) - (3) suggest that TARP led to statistically and economically significant decreases in personal bankruptcies via Chapter 7 (liquidations) filings only.

Table X.1: Effects of TARP on Local Economic Conditions: Sources

This table reports estimates from difference-in-difference (DID) regression estimates for the impact of TARP on local economic conditions components. Panel A shows the decomposition of *Net Job Creation / Capita*, Panel B shows the decomposition of *Net Hiring Establishments / Capita*, Panel C shows the decomposition of *Business Bankruptcies / Capita* and Panel D shows the decomposition of *Personal Bankruptcies / Capita*. *TARP Recipient* is the weighted proportion of TARP banks receiving TARP in the local markets, *Post TARP* is a dummy equal to one in 2009-2012, the period after TARP program initiation. All models include state and time fixed effects. The estimation results are for 2005-2012. All variables are defined in Table 1. *, **, and *** denote significance at 10%, 5%, and 1% level.

Panel A: Decomposition of *Net Job Creation / Capita*

Dependent Variable:	***Gross Job Creation / Capita***	*Gross Job Creation - Openings / Capita*	*Gross Job Creation - Expansions / Capita*	***Gross Job Destruction / Capita***	*Gross Job Destruction Closings / Capita*	*Gross Job Destruction Contractions / Capita*
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
TARP Recipient	-2.296**	-0.341	-1.955**	1.148	-0.149	1.312
	(-2.340)	(-0.945)	(-2.378)	(1.232)	(-0.492)	(1.506)
Post TARP	-3.544***	-0.731***	-2.814***	-3.160***	-0.786***	-2.372***
	(-9.685)	(-5.862)	(-9.623)	(-9.019)	(-4.952)	(-9.081)
Post TARP x TARP Recipient	1.205**	0.160	1.045**	-2.038***	0.119	-2.167***
	(2.409)	(0.901)	(2.432)	(-3.802)	(0.689)	(-4.423)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580	1,580	1,580
Adjusted R-squared	0.885	0.755	0.884	0.866	0.684	0.870

Panel B: Decomposition of *Net Hiring Establishments / Capita*

Dependent Variable:	***Gross Hiring Establishments / Capita***	*Gross Hiring Establishments – Openings / Capita*	*Gross Hiring Establishments – Expansions / Capita*	***Gross Firing Establishments / Capita***	*Gross Firing Establishments – Closings / Capita*	*Gross Firing Establishments – Contractions / Capita*
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
TARP Recipient	-0.611***	-0.172***	-0.439***	0.305**	0.040	0.265**
	(-3.755)	(-2.649)	(-3.440)	(2.128)	(0.639)	(2.157)
Post TARP	-0.541***	-0.047**	-0.493***	-0.459***	-0.017	-0.442***
	(-9.778)	(-2.249)	(-11.347)	(-9.708)	(-0.648)	(-12.449)
Post TARP x TARP Recipient	0.281***	0.023	0.258***	-0.359***	-0.043	-0.316***
	(3.328)	(0.680)	(3.884)	(-4.550)	(-1.128)	(-4.835)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580	1,580	1,580
Adjusted R-squared	0.958	0.897	0.958	0.959	0.862	0.961

Panel C: Decomposition of *Business Bankruptcies / Capita*

Dependent Variable:	***Business Bankruptcies - Chapter 7 / Capita***	***Business Bankruptcies - Chapter 11 / Capita***	***Business Bankruptcies - Chapter 12 / Capita***	***Business Bankruptcies – Chapter 13 / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	0.015***	0.029	0.000	0.002
	(2.592)	(1.559)	(1.479)	(1.547)
Post TARP	-0.008***	-0.001	-0.000	-0.002***
	(-4.716)	(-0.224)	(-0.374)	(-4.213)
Post TARP x TARP Recipient	-0.010***	-0.009	-0.000	-0.002**
	(-2.964)	(-1.464)	(-1.045)	(-2.249)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.723	0.565	0.415	0.640

Panel D: Decomposition of *Personal Bankruptcies / Capita*

Dependent Variable:	***Personal Bankruptcies – Chapter 7/ Capita***	***Personal Bankruptcies – Chapter 11 / Capita***	***Personal Bankruptcies – Chapter 13 / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**
TARP Recipient	0.269	0.000	0.026
	(1.634)	(0.797)	(0.916)
Post TARP	-0.866***	0.000**	-0.066***
	(-15.958)	(2.361)	(-5.775)
Post TARP x TARP Recipient	-0.412***	-0.000	-0.019
	(-5.393)	(-0.256)	(-1.117)
Bank-Related Controls	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580
Adjusted R-squared	0.842	0.498	0.932

APPENDIX Y – SUBSAMPLE TESTS

We conduct several subsample analyses to see in which types of banks and under what local economic conditions TARP was most effective.

Y.1 Effects by Bank Size Classes

As shown in the TARP literature (e.g., Black and Hazelwood, 2013; Li, 2013; Puddu and Walchli, 2013; Duchin and Sosyura, 2014), different bank sizes may exhibit different lending behavior after TARP capital disbursements, which may have different effects on local economic conditions.

We therefore examine separately the proportions of different TARP bank sizes in the local markets: small TARP banks (GTA ≤ $1 billion), medium TARP banks ($1 billion ≤ GTA < $3 billion), and large TARP banks (GTA > $3 billion) and create three variables: *SMALL TARP Recipient, MEDIUM TARP Recipient,* and *LARGE TARP Recipient,* as well as DID interaction terms between these TARP variables and the *Post TARP* dummy.[25] Table Y.1 Panel A1, columns (1)-(4), present the results.

We find that all effects are concentrated in the medium and large banks, particularly the medium banks. The proportions of large and medium TARP banks in the local markets statistically and economically increase net job creation and hiring establishments more than the proportion of the small TARP banks and lead to a statistically significant decrease in business and personal bankruptcies. Also, the *t*-tests for the differences in coefficients among the proportions of the three TARP bank size groups reported in Panel A2 show that the differences between the effects of the proportions of small and large TARP banks are not statistically significant. However, the differences between the small and medium TARP banks are statistically significant for the net job creation, net hiring establishments, and business bankruptcies, while the differences between medium and large TARP banks are statistically significant for net hiring establishments and business bankruptcies. As shown below, the weaker findings for large banks compared to medium banks may be related to the involuntary nature of TARP participation or the stress tests of most of the largest institutions. Alternatively, it may be because many of the large banks are multistate, and the

[25] Out of the TARP bank recipients, 67% are small banks, 16% are medium banks, and 17% are large banks.

effects of TARP for these banks may not align well with the distributions across states of their deposits, as is assumed in our analyses.

Y.2 Involuntary and Voluntary Participants

As discussed above, some banks were required to participate in TARP at its inception. We classify the following eight banks as involuntary participants: Citigroup, JP Morgan, Wells Fargo, Morgan Stanley, Goldman Sachs, Bank of New York, Bank of America, and State Street Bank.[26] We consider separately the proportions of TARP involuntary and voluntary banks and we interact these variables with our *Post TARP* dummy. Regression estimates are shown in Table Y.1 Panel B1, columns (1)-(4). We find that results continue to hold and are primarily due to voluntary TARP participants. The only exception is business bankruptcies, for which only involuntary banks play a more important role in the reduction of bankruptcies.

Y.3 TARP Banks Subject to Stress Tests (SCAP and CCAR) and Those That Are Not

The 2009 U.S. Banks Stress Tests aka Supervisory Capital Assessment Program (SCAP) was a mandatory program applied to 19 banking organizations with assets exceeding $100 billion that cover about 2/3 of U.S banking assets and about half of loans.[27] It was conducted by Federal Bank Regulatory Agencies (FED, FDIC, OCC) from February 25, 2009 to late April 2009 and it was designed to ensure that large banking organizations had enough capital to withstand the recession and a more adverse scenario that might occur over the rest of 2009 and 2010. These organizations had to have or raise enough capital to meet capital requirements under the more adverse scenario, or the Treasury would provide the capital. In later years, this became the Comprehensive Capital Analysis and Review (CCAR). Given this special treatment of stress-tested banks, we would like to rule out the possibility that our main results may be determined by this subsample of banks.

[26] We exclude Merrill Lynch from the original 9 involuntary recipients because it is not a bank.

[27] These 19 banking organizations are Bank of America, Citigroup, Goldman Sachs, JP Morgan Chase, Morgan Stanley, Wells Fargo, Bank of NY Mellon, BB&T, Fifth Third Bancorp, Keycorp, PNC Financial, Regions Financial, SunTrust Banks, US Bancorp, Ally Financial, American Express Company, Capital One Financial, Metlife, and State Street.

We examine separately the proportions of TARP stress-tested and non-stress-tested banks and interact these variables with our *Post TARP* dummy. Regression estimates are shown in Table Y.1 Panel C1, columns (1)-(4). We find that results continue to hold and in most cases, the non-stress tested banks appear to be responsible for more of the gains in job creation and hiring establishments. One possible reason may be that the stress tests were successful and TARP was not needed for these banks. However, with regard to business and personal bankruptcies, stress-tested banks generally tend to contribute more to the reduction in both business and personal bankruptcies..

Y.4 TARP Banks that Repaid Early and TARP Banks that Did Not

We also test whether TARP may have been more or less effective in improving local economic conditions for TARP banks that repaid early in 2009 or 2010 versus other recipients. Berger and Roman (forthcoming) find that the competitive benefits of TARP are primarily or entirely due to TARP recipients that repaid early.

We rerun our tests by differentiating between TARP banks that repaid early and those that did not. Table Y.1 Panel D1, columns (1) - (4) report the estimation results. The results indicate that most of the gains are due to TARP banks that did not repay early: the proportions of TARP banks that repaid early lead to higher increase in net job creation and hiring establishments and higher decreases in business and personal bankruptcies. The *t*-tests for the difference in coefficients between the two groups reported in Panel D2 shows that the difference between proportions of TARP banks that repaid early and those that did not is statistically significant for personal bankruptcies, but not for the others.

Y.5 Banks with Low and High Capital Ratios (2008:Q3)

Banks with lower capital ratios prior to infusion may expand loans and loan commitments more because TARP injections relieved them from capital constraints that prevented them from lending. Alternatively, banks with higher capital ratios prior to infusion may have better abilities to use the extra capital from the infusion to expand loans and loan commitments and thus alter local economic conditions. Therefore, we consider separately the proportions of TARP banks with low equity to assets ratio (*EQCAP_08Q3* $\leq$

median) and high equity to assets ratio (*EQCAP_08Q3* > median) before the TARP program started. Regression estimates are shown in Table Y.1 Panel E1, columns (1)-(4).

The results are mixed. The job creation and hiring establishments effects are primarily due to the proportions of well capitalized TARP banks, as indicated by the positive coefficients for their DID terms. However, the bankruptcy effects are primarily due to the proportions of poor-capitalized TARP banks. Also, the *t*-tests for the difference in coefficients between the effects of the proportions of the two TARP groups reported in Panel E2 are statistically significant for all but personal bankruptcies. In addition, the reported improvements in local conditions are economically significant for all the economic indicators except business bankruptcies.

Y.6 States in Poor and Good Conditions (2008:Q3)

It is also possible that the states with worse economic conditions may improve their conditions more or less after TARP relative to those with better economic conditions. We measure the economic conditions using the *Coincident Index* from Philadelphia Federal Reserve website. This index combines four state-level economic indicators – nonfarm payroll employment, average hours worked in manufacturing, the unemployment rate, and wage and salary disbursements deflated by the consumer price index – into a single statistic. We differentiate between proportions of TARP banks in the states with low coincident index before the TARP program started (2008:Q3) (*Coincident Index 2008:Q3* ≤ median) and those with high coincident index before the TARP program started (*Coincident Index 2008:Q3* > median). Regression estimates are shown in Table Y.1 Panel F1, columns (1)-(4).

We find that results are primarily due to the proportions of TARP banks in the states with poor conditions (low coincident indices), which helped statistically and economically significantly increase net job creation and hiring establishments, and decrease business and personal bankruptcies. The *t*-tests for the difference in coefficients between the two groups reported in Panel F2 shows that the difference between states with low and high coincident indices is statistically significant for all but business bankruptcies.

Y.7 States with Low and High Economic Freedom (2008:Q3)

States with less economic freedom may have improved their conditions more or less after TARP relative to those with higher economic freedom. States with high economic freedom (freer competition, better enforcement of contracts, etc.) may have a higher ability to stabilize their local markets without intervention from governments and regulators because their economy is closer to the market economy. Alternatively, banks in states with low economic freedom may have more room for improvement, so they may gain more from the TARP bailouts. We differentiate between proportions of TARP banks in the states with low economic freedom indices *(Economic Freedom Index* 2008:Q3 ≤ median) and those with high economic freedom indices before the TARP program started *(Economic Freedom Index* 2008:Q3 > median). Regression estimates are shown in Table Y.1 Panel G1, columns (1)-(4).

We find that results are primarily due to proportions of TARP banks in the states with low economic freedom indices, which helped statistically and economically significantly increase net job creation and hiring establishments and decrease business and personal bankruptcies. The *t*-tests for the difference in coefficients between the two groups reported in Panel G2 shows that the difference between states with low and high economic freedom indices is statistically significant for personal bankruptcies, but not for the others.

Table Y.1: Effects of TARP on Local Economic Conditions: Other Robustness Tests

This table shows additional subsample tests for analyzing the impact of TARP on local economic conditions. Panel A reports difference-in-difference (DID) regression estimates when considering the proportions of different TARP banks size classes in the local markets: *SMALL TARP Recipient (GTA ≤ 1 Billion*), *MEDIUM TARP Recipient* (*1 Billion < GTA ≤3 Billion)* and *LARGE TARP Recipient* (*GTA > 3 Billion*). Panel B reports difference-in-difference (DID) regression estimates for the proportions of TARP banks that are involuntary and those that are voluntary participants. Panel C reports difference-in-difference (DID) regression estimates for the proportions of TARP banks that are subject to stress-tests and those that were not. Panel D reports difference-in-difference (DID) regression estimates for the proportions of TARP banks that repaid early and those that did not. Panel E reports difference-in-difference (DID) regression estimates for the proportions of TARP banks with low capitalization (*EQCAP_08Q3* ≤ median*)* versus those with high capitalization (*EQCAP_08Q3* > median). Panels F reports difference-in-difference (DID) regression estimates for the proportions of TARP in states with low coincident index in 2008:Q3 (≤ median*)* and in states with high coincident index in 2008:Q3 (> median). Panels G reports difference-in-difference (DID) regression estimates for the proportions of TARP in states with low economic freedom index in 2008:Q3 (≤ median*)* and in states with high economic freedom index in 2008:Q3 (> median).). The measures of local conditions are *Net Job Creation / Capita, Net Hiring Establishments / Capita*, *Business Bankruptcies / Capita*, and *Personal Bankruptcies / Capita*. *TARP Recipient* is the weighted proportion of TARP banks receiving TARP in the local markets, *Post TARP* is a dummy equal to one in 2009-2012, the period after TARP program initiation. All models include state and time fixed effects. The estimation results are for 2005-2012. All variables are defined in Table 1. *, **, and *** denote significance at 10%, 5%, and 1% level.

Panel A: Effects by Bank Size Classes
Panel A1: Regression Estimates

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
SMALL TARP Recipient	14.685	1.054	0.033	1.955*
	(1.093)	(0.516)	(0.531)	(1.677)
MEDIUM TARP Recipient	-18.102*	-3.497***	0.104**	0.349
	(-1.938)	(-2.593)	(2.109)	(0.352)
LARGE TARP Recipient	-3.143**	-0.880***	0.045**	0.273
	(-2.193)	(-3.513)	(2.338)	(1.532)
Post TARP	-0.328	-0.057	-0.011**	-0.911***
	(-0.642)	(-0.725)	(-2.441)	(-16.138)
Post TARP x SMALL TARP Recipient	-5.916	-1.609	0.035	-0.787
	(-0.713)	(-1.078)	(0.487)	(-0.851)
Post TARP x MEDIUM TARP Recipient	23.244***	2.726**	-0.117***	-1.590*
	(2.911)	(2.248)	(-2.591)	(-1.862)
Post TARP x LARGE TARP Recipient	2.928***	0.610***	-0.020**	-0.408***
	(3.749)	(4.726)	(-2.533)	(-5.061)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.487	0.572	0.626	0.874

Panel A2: Tests of the Equality of the Effects of TARP for Different Types of TARP Recipients

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
***t*-stat:** *Post TARP x SMALL TARP Recipient = Post TARP x LARGE TARP Recipient*	1.068	1.493	0.735	0.412
***t*-stat:** *Post TARP x SMALL TARP Recipient = Post TARP x MEDIUM TARP Recipient*	2.202**	1.949*	1.916*	0.728
***t*-stat:** *Post TARP x MEDIUM TARP Recipient = Post TARP x LARGE TARP Recipient*	0.316	1.729*	2.046*	1.371

Panel B: TARP Involuntary and Voluntary Participants

Panel B1: Regression Estimates

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient x INVOL	-1.967	-0.634**	0.041*	0.331*
	(-1.155)	(-2.179)	(1.850)	(1.664)
TARP Recipient x VOL	-5.206***	-1.247***	0.049**	0.314
	(-3.164)	(-4.579)	(1.972)	(1.553)
Post TARP	-0.365	-0.076	-0.012**	-0.918***
	(-0.752)	(-1.014)	(-2.451)	(-16.798)
Post TARP x TARP Recipient x INVOL	2.088*	0.436**	-0.030*	-0.257**
	(1.752)	(2.326)	(-1.928)	(-2.458)
Post TARP x TARP Recipient x VOL	4.873***	0.929***	-0.010	-0.673***
	(3.802)	(4.539)	(-1.020)	(-4.064)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.485	0.572	0.627	0.875

Panel B2: Tests of the Equality of the Effects of TARP for Different Types of TARP Recipients

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
***t*-stat:** Effect for TARP involuntary participants = effect for TARP non-involuntary participants	1.407	1.619	0.894	1.918*

Panel C: Banks Subject to the Stress Tests and those that are not (SCAP and CCAP)

Panel C1: Regression Estimates

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient x Stress-Tested	-1.995	-0.765***	0.036**	0.350**
	(-1.301)	(-2.815)	(2.069)	(2.036)
TARP Recipient x NON Stress-Tested	-9.393***	-1.518***	0.082**	0.074
	(-3.471)	(-3.999)	(2.018)	(0.215)
Post TARP	-0.406	-0.072	-0.015***	-0.928***
	(-0.820)	(-0.938)	(-2.867)	(-16.856)
Post TARP x TARP Recipient x Stress-Tested	2.517***	0.604***	-0.032***	-0.446***
	(2.899)	(4.240)	(-3.511)	(-5.223)
Post TARP x TARP Recipient x NON Stress-Tested	7.590***	0.784*	0.070***	-0.368
	(2.827)	(1.887)	(2.887)	(-1.053)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.487	0.572	0.635	0.874

Panel C2: Tests of the Equality of the Effects of TARP for Different Types of TARP Recipients

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
***t*-stat:** Effect for TARP banks subject to Stress Tests = effect for TARP banks not subject to Stress Tests	1.685*	0.387	3.604***	0.200

Panel D: Distinguishing by Early Repayment

Panel D1: Regression Estimates

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient_Repaid	-2.260	-0.800***	0.045**	0.295
	(-1.494)	(-3.016)	(2.208)	(1.442)
TARP Recipient_Not Repaid	-7.918***	-1.362***	0.051**	0.268
	(-3.271)	(-3.674)	(1.981)	(1.125)
Post TARP	-0.375	-0.076	-0.010**	-0.919***
	(-0.773)	(-1.007)	(-2.300)	(-16.753)
Post TARP x TARP Recipient_Repaid Early	2.392**	0.593***	-0.017	-0.329***
	(2.565)	(3.891)	(-1.403)	(-3.618)
Post TARP x TARP Recipient_Not Repaid Early	6.762***	0.805**	-0.043*	-0.944***
	(2.869)	(2.156)	(-1.727)	(-3.997)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.486	0.572	0.626	0.875

Panel D2: Tests of the Equality of the Effects of TARP for Different Types of TARP Recipients

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
***t*-stat:** Effect for TARP banks that repaid early = effect for TARP banks that did not repay early	1.568	0.489	0.787	2.317**

Panel E: Capitalization Level (2008:Q3)

Panel E1: Regression Estimates

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient x HIGHCAP	-8.053***	-1.678***	0.051**	0.249
	(-4.539)	(-5.647)	(2.072)	(1.177)
TARP Recipient x LOWCAP	-1.281	-0.565**	0.043**	0.315*
	(-0.853)	(-2.163)	(2.071)	(1.750)
Post TARP	-0.379	-0.080	-0.011**	-0.931***
	(-0.788)	(-1.071)	(-2.254)	(-16.870)
Post TARP x TARP Recipient x HIGHCAP	5.850***	1.199***	-0.000	-0.379***
	(4.477)	(5.634)	(-0.043)	(-2.612)
Post TARP x TARP Recipient x LOWCAP	1.615	0.265	-0.039***	-0.470***
	(1.356)	(1.447)	(-3.150)	(-4.372)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.490	0.576	0.628	0.874

Panel E2: Tests of the Equality of the Effects of TARP for Different Types of TARP Recipients

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
***t*-stat:** Effect for TARP banks with low capitalization = effect for TARP banks with high capitalization	2.090**	2.992***	2.383**	0.469

Panel F: Coincident Index 2008:Q3

Panel F1: Regression Estimates

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient x LOWCOINCIDENT	-4.017**	-1.051***	0.052***	0.357**
	(-2.429)	(-3.885)	(2.613)	(2.034)
TARP Recipient x HIGHCOINCIDENT	-2.926**	-0.803***	0.042**	0.239
	(-2.006)	(-3.071)	(2.029)	(1.250)
Post TARP	-0.354	-0.076	-0.011**	-0.935***
	(-0.735)	(-1.011)	(-2.295)	(-16.898)
Post TARP x TARP Recipient x LOWCOINCIDENT	4.771***	0.890***	-0.032***	-0.593***
	(4.732)	(5.591)	(-4.724)	(-5.381)
Post TARP x TARP Recipient x HIGHCOINCIDENT	1.933**	0.436***	-0.012	-0.294***
	(2.093)	(2.775)	(-1.058)	(-2.970)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.487	0.573	0.627	0.875

Panel F2: Tests of the Equality of the Effects of TARP for Different Types of States

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
***t*-stat:** Effect for states with low coincident index = effect for states with high coincident index	2.437**	2.379**	1.640	2.223**

Panel G: Economic Freedom Index 2008:Q3

Panel G1: Regression Estimates

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient x LOWECFREEDOM	-4.189***	-0.965***	0.053***	0.311
	(-2.812)	(-3.715)	(2.601)	(1.574)
TARP Recipient x HIGHECFREEDOM	-3.056**	-0.858***	0.042**	0.274
	(-1.991)	(-3.259)	(2.079)	(1.517)
Post TARP	-0.364	-0.078	-0.011**	-0.932***
	(-0.752)	(-1.038)	(-2.307)	(-16.852)
Post TARP x TARP Recipient x LOWECFREEDOM	3.722***	0.806***	-0.032***	-0.499***
	(4.200)	(5.384)	(-4.153)	(-3.770)
Post TARP x TARP Recipient x HIGHECFREEDOM	2.979***	0.513***	-0.013	-0.380***
	(3.090)	(3.271)	(-1.300)	(-4.289)
Bank-Related Controls	**YES**	**YES**	**YES**	**YES**
State-Related Controls	**YES**	**YES**	**YES**	**YES**
State Fixed Effects	**YES**	**YES**	**YES**	**YES**
Time Fixed Effects	**YES**	**YES**	**YES**	**YES**
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.485	0.572	0.626	0.874

Panel G2: Tests of the Equality of the Effects of TARP for Different Types of States

Dependent Variable:	***Net Job Creation / Capita***	***Net Hiring Establishments / Capita***	***Business Bankruptcies / Capita***	***Personal Bankruptcies / Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
***t*-stat:** Effect for states with low economic freedom index = effect for states with high economic freedom index	1.568	0.489	0.787	2.317**