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

Allen N. Berger†

University of South Carolina
Wharton Financial Institutions Center
European Banking Center

Raluca A. Roman‡

University of South Carolina

September 2014

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 significantly and economically increased net job creation and net hiring establishments, decreased personal bankruptcies, and had no impact on business bankruptcies. These results suggest that giving a lifeline to Wall Street via TARP may have saved the Main Street.

JEL Classification Codes: G18, G21, G28

Keywords: TARP, Banks, Capital Support, Economic Conditions

*The authors thank Orgul Ozturk, Lamont Black, Daniel Hartley, Alan Hess, Dasol Kim, Loretta Mester, Mark Schweitzer, Randy Verbrugge, and seminar participants at the Federal Reserve Bank of Cleveland for helpful comments, and Lamont Black, Christa Bouwman, and Jennifer Dlugosz for data on Discount Window and Term Auction Facility programs.

†Corresponding Author: Moore School of Business, University of South Carolina, 1705 College Street, Columbia, SC 29208, Tel: 803-576-8440, Fax: 803-777-6876, E-mail: aberger@moore.sc.edu.

‡Moore School of Business, University of South Carolina, 1705 College Street, Columbia, SC 29208, E-mail: raluca.roman@grad.moore.sc.edu.

"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). To our knowledge, there is no academic research supporting either of these views. The purpose of this paper is to provide such evidence.

TARP was one of the largest government interventions in US 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 US 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 improve the stability of the financial system, increase availability of credit, and improve real economic conditions.

Prior TARP research includes investigations of the effects of the program on bank lending (Black and Hazelwood, 2013; Li, 2013; Duchin and Sosyura, 2014), bank risk-taking (Black and Hazelwood, 2013; Li, 2013; Puddu and Walchli, 2013; Duchin and Sosyura, 2014), bank

competition (Koetter and Noth, 2014; Berger and Roman, forthcoming), and banks' stock market valuations (Veronesi and Zingales, 2010; Ng, Vasvari, and Wittenberg-Moerman, 2013; Harrisa, Huertab, and Ngob, 2013) and borrowers' stock market valuations (Liu, 2013; Norden, Roosenboom, and Wang, 2013).

However, the effects of TARP on the real economy 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 avoid 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, worsen, or leave relatively unchanged 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 US over 2005:Q1-2012:Q4, we test the hypotheses using a difference-in-difference (DID) regression model. Our empirical model uses indicators of local economic conditions – *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, following Duchin and Sosyura, 2014) and a DID

term *Post TARP x TARP Recipient* to capture the effect of the TARP treatment. We also control for proxies for CAMELS, the declared set of financial criteria used by regulators to assess the health of banking organizations, as well as other bank-related and state-related characteristics and state and time fixed effects.

Our results suggest that 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, statistically and economically increased net hiring establishments, and statistically and economically decreased local personal bankruptcies, and had no significant effect on local business bankruptcies. We find that the average market had a quarterly increase in the net job creation of 1.0405, 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, 16.65 jobs were created due to TARP. Similarly, we find that on average over the whole post-TARP period, for every 1000 people, 3.25 more establishments created jobs, and 0.77 personal bankruptcies were eliminated. 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. 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 to deal with potential endogeneity problems, 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 hiring effects mostly occur in 2009 and generally dissipate thereafter and 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 banks versus non-stress-tested ones, distinguishing between banks that repaid TARP funds early and those that did not, considering high-capitalized versus low-capitalized banks, states with better versus worse 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 describe the econometric framework, and Section 6 discusses 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 provides a decomposition of the local economic conditions and Appendix Y shows subsample analyses.

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

The Troubled Asset Relief Program (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 subprime mortgage 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 banks 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), and Berger and Roman (forthcoming) find that the banks with more political influence 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

[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).

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 initial 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), 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, obtained TARP equity infusions. Cornett, Li, and Tehranian (2013) find that financial characteristics 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 TARP “over-achievers” loans 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 levels of

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

CEO pay were more likely to exit early, presumably due to the restrictions on executive pay imposed on TARP recipients.

Some researchers look at TARP banks' and borrowers' valuations. Ng, Vasvari, and Wittenberg-Moerman (2013) find that 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 borrowing firms' stock returns around the time of TARP capital injections. Liu (2013), in contrast, finds that firms with relationships with TARP banks suffered significant valuation losses around the times of TARP approval announcements.

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 impacted, if at all, through bank lending and 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 banks.

In addition, there are papers that look at the effects of TARP on competition. Berger and Roman (forthcoming) find that TARP gave recipients 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 or deposit growth after TARP, suggesting that the **safety net channel** extends to those banks for which suppliers of funds anticipate bailouts to be more likely.

Finally, there is also a related literature that looks at government interventions 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 or increases in risk-taking, and reductions in credit

[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.

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 their competitors, and such lending and commitments contribute to local economic conditions.

There are several primary channels that 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 due to the selection criteria which targeted "healthy, viable institutions." The safety channel includes the effects of both the banks' decision to apply for TARP and whether the application is accepted. 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.

Fourth, under the **increased moral hazard channel**, there may be reductions in regulatory and market discipline due to the increased probability of future bailouts. These may result in increases in risk taking, which may take the form of increased supply of bank loans and commitments to riskier applicants that might otherwise be rationed (e.g., Stiglitz and Weiss, 1981).

Several primary channels may also worsen local economic conditions through decreases in credit in the local markets. 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' decision to apply for TARP and whether the application is accepted. Under this channel, customers may

[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.

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.

Fourth, 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 by reducing the supply of bank loans and loan commitments to riskier applicants that might otherwise be rationed.

There are also some secondary channels that 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.

These changes can increase or decrease the supply of loan and loan commitments depending on whether the customers are relationship borrowers or transactional borrowers. In particular, 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.

5. Econometric Framework

We test the effects of TARP on local economic conditions using state-level data, considering the proportions of all banking organizations in US in the states in which they operate.[5] The changes in conditions after TARP injections in banks are analyzed using a difference-in-difference (DID) methodology. A DID estimator is 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,

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

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, forthcoming; Berger and Roman, forthcoming). An advantage of this empirical 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:

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

Y_{st} is the dependent variable, an indicator of change in local economic conditions in state *s* between times *t-1* and *t* (*Net Job Creation / Capita*, *Net Hiring Establishments / Capita*, *Business Bankruptcies / Capita*, or *Personal Bankruptcies / Capita*). Turning to the independent variables, *TARP Recipient*$_{st}$ is the weighted proportion of banks receiving TARP capital support in the local markets, where the weights are based on the proportion of deposits of the banks in their local markets.[6] *Post TARP*$_{st}$ 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*$_{st}$ *x TARP Recipient*$_{st}$ is the DID term and captures the effect of the treatment (TARP) after the treatment. A positive coefficient in the *Net Job Creation / Capita* or *Net Hiring Establishments / Capita* equations or a negative coefficient 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 and $Time_t$ represents year and quarter

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

fixed effects, and ε_{st} represents a white noise 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. This aggregation is done for all bank-level variables. 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

[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.

proportions of their deposits in the local markets in which they operate 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 bank deposits.

We obtain quarterly local economic conditions data at the state level from US 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 US Court Filings for the period 2005:Q1 to 2012:Q4.

We also use data from several other sources for additional control variables and instruments: FDIC Summary of Deposits, List of Corrective Actions, and U.S. Census Bureau's Population Distribution, House of Representatives website, Missouri Census Data Center, and the Center for Responsible Politics website, NBER, the Tax Policy Center, and the Fraser Institute. The regressions also lose one quarter of observations because of the use of lagged values for some of the exogenous variables. Our final regression sample contains 1,580 state-quarter observations for 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 from *t-1* to *t* calculated as: (*Gross Job Creation* - *Gross Job Destruction*) / (*Population*/1000).[9] *Gross Job Creation* is the number of jobs created from *t-1* to *t*. It consists of job openings and expansions. Openings are number of jobs created at new establishments.

[9] Our per capita variables are actually per 1000 of population to make the results easier to interpret.

Expansions are number of new jobs created at existing establishments. *Gross Job Destruction* is the number of jobs destroyed from *t-1* to *t*. It consists of job closings and contractions, defined analogously.

We next consider the *Net Hiring Establishments / Capita*, the overall net hiring establishments per capita from *t-1* to *t*, calculated as: (*Gross Hiring Establishments* - *Gross Firing Establishments*) / (*Population*/1000). *Gross Hiring Establishments* is the number of hiring establishments that create jobs from *t-1* to *t*. It consists of establishments that create jobs through job openings and expansions. *Gross Firing Establishments* is the number of establishments that destroy jobs from the *t-1* to *t*. It consists of establishments that destroy jobs through job closings and contractions.

We also examine changes in business and personal 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 at time *t*) / (*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. *Personal Bankruptcies / Capita* is the overall number of personal bankruptcies per capita, calculated as: (Total personal bankruptcy filings at time *t*) / (*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.

6.3 Main Independent Variables

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

6.4 Control Variables

We include a broad set of control variables (bank-related and state-related) to mitigate potential omitted variable problems. For the bank variables, we use the proportions of the variables in the states, where the weights are the proportions of deposits in the different states where banks operate.

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.[10] We 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).[11, 12] We control for *Asset Quality* to account for the overall condition of a bank's portfolio, defined by the fraction of nonperforming loans to total loans. We also 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. We control for *Earnings* because banks that are more profitable may

[10] In Section 8.5, we try removing the proxies for CAMELS to mitigate the possibility that TARP affects local market economic conditions through affecting the health of the recipient banks.

[11] 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.

[12] 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).

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 also 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 also control 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.[13] 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 (e.g., Berger and Bouwman, 2009). Then, we control for *HHI Deposits*, the Herfindahl-Hirschman Index determined using the bank deposit data from the FDIC Summary of Deposits, which may affect the lending strategy of the bank. HHI is weighted by the share of bank deposits in each local market over bank's total deposits over all the markets in which the bank operates. Also, we control for *Metropolitan Dummy* – a dummy equal to 1 if the majority of bank deposits are in MSAs or NECMAs – as banks in metropolitan locations may have more opportunities to increase lending. Finally, *State No Banks* is the logarithm of the total number of banks in the state.

We also control for a number of state-level control variables that could influence local economic conditions. *State Minimum Wage* is the 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. *State Marginal Tax Rate* is the top marginal rate of the state's income tax.

[13] 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.

Finally, *State Economic Freedom Index* is the state-level index of economic freedom. 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. A score of 10 represents the highest level of economic freedom.

.7. Empirical analysis

7.1 Summary Statistics

Table 1 provides definitions and summary statistics for our variables. We present the means, medians, standard deviations, and 25th and 75th percentiles across all banks and states in the sample for the variables used in our analyses. 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 15.6% of the banks across states received TARP money.

Looking at the proxies for CAMELS ratings for the state-level sample, we find that the average state in our sample has aggregated bank *Capital Adequacy* of 0.104, *Asset Quality* of 0.008, *Management Quality* of -0.001, *Earnings* of 0.009, *Liquidity* of 0.058, and *Sensitivity to Market Risk* of 0.143. These statistics suggest that 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. Looking next at other bank variables, we find that in the average state, 24.1% of banks obtained Discount Window and/or TAF funds (*DWTAF*), the average *Bank Size* (logarithm of the GTA) is 14.959 (mean GTA is $86.8 billion), *HHI Deposits* is 588.823, and *Metropolitan Dummy* is 0.336. Also, the average state in our sample has a *State No Banks* (logarithm of the total number of banks) of 4.180

(111.900), a *State Minimum Wage* of $6.769 / hour, a *State Marginal Tax Rate* of 5.214, and a *State Economic Freedom Index* of 6.769.

7.2 Regression Analysis

Table 2 tabulates the main estimation results for analyzing the impact of TARP on local economic conditions using equation (1) and tests Hypotheses H1 and H2 (state and time fixed effects are not shown for brevity). First, regression estimates in column (1) for *Net Job Creation / Capita* and column (2) for *Net Hiring Establishments / Capita* indicate that the DID term, *Post* $TARP_{st}$ * *TARP* $Recipient_{st}$, is 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, results in column (3) for *Business Bankruptcies / Capita* suggest that there is no statistically significant impact on business bankruptcies, and the regression estimate in column (4) for *Personal Bankruptcies / Capita* indicates that the DID term, *Post* $TARP_{st}$ * *TARP* $Recipient_{st}$, is negative and statistically significant, indicating that TARP banks' capital injections were followed by decreases in personal bankruptcies. Overall, 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 dominates economically Hypothesis H2. The coefficient on *Post* $TARP_{st}$ * *TARP* $Recipient_{st}$ of 6.670 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 1.0405, 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, 16.65 jobs were created due to TARP. Similarly, we find that on average over the whole post-TARP period, for every 1000 people, 3.25 more establishments created jobs, and 0.77 personal bankruptcies were eliminated.

Statistically and economically, these results indicate that the banks that received TARP improved local economic conditions, by increasing net job creation and hiring establishments and reducing personal bankruptcies, and had no effect on business bankruptcies. Overall, these results suggest that extending a lifeline to Wall Street via TARP may have saved Main Street to an economically significant extent.

8. Robustness Tests

8.1 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 impact local economic conditions through lending and commitments, yielding a spurious relationship.

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 can 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 *Subcommitee 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 Capital Markets Subcommittee or the Financial Institutions Subcommittee of the House Financial Services Committee (key finance committees involved in drafting and amending TARP) in 2008 or 2009, following Duchin and Sosyura (2014). A firm is considered to be connected to a politician if it is headquartered in the election district of a politician and a politician is considered to be connected to TARP if he or she served on one these two subcommittees. 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 *Subcommitee on Financial Institutions or Capital Market* 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 have also affected a Representative's action about TARP. Republicans are thought to be generally more opposed to government bailouts, while 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).[14] This variable should be positively related to TARP decisions as a bank with a Fed-connection may have been treated more favorably in the Fed's TARP evaluation process. 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.

[14] 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 which is less than 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).

Because the basis of the TARP Recipient variable is binary and we need the instrument to predict the treatment, we employ a dummy endogenous variable model and follow a 3-step approach as in Wooldridge (2002) procedure 18.1.[15] 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 at the state level and use this variable as an instrument for the second stage. Thus, we 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.

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 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

[15] Wooldridge (2002) procedure 18.1 is a useful implementation 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 excluded instrument (this method involves three steps and not just two). We follow this 3-step 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 thus obtained variables as instruments further and not as regressors.

statistically and economically significant improvements in economic conditions. We find that on average over the whole post-TARP period, for every 1000 people, 19.48 jobs were created, 3.57 more establishments created jobs, and 1.29 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.

8.2 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.[16] 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* and we should not find negatively significant results for the DID terms on *Business Bankruptcies / Capita* and *Personal Bankruptcies / Capita*.

The results of the placebo experiments are reported in Table 4. We find that the results are either statistically insignificant or go in the opposite direction. For net job creation, net hiring establishments, and business bankruptcies, the fictional TARP effects are statistically

[16] 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).

insignificant. For 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 help alleviate the concern that omitted unobserved state-specific determinants might be spuriously responsible for our results, we also test robustness using specifications with state random effects. These results are presented in Table 5 Panel A. We also present a model state and time fixed effects and White standard errors which are robust to within-cluster correlation at the state level (Rogers standard errors) in Table 5 Panel B. In Table 5 Panel C, we present a model in which we exclude the proxies for *CAMELS* to mitigate the possibility that TARP affects local market economic conditions through affecting the health of the recipient banks. In Table 5 Panel D, we further present a model in which we exclude all bank-related variables. In Table 5 Panel E, we also present a model in which 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. We remove the two middle quartiles (2 and 3) and reestimate the results using only quartile 1 and quartile 4. These results are shown in Table 6 Panel A. As an alternative test, we also split the states into terciles according to the proportions of TARP recipients in the state and remove tercile 2 from

the estimations. These results are shown in Table 6 Panel B. We find that results are robust to these tests.

8.5 Dynamics of TARP and Competitive Indicators

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 do this by including 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*$_{it}$ *x TARP Recipient*$_{it}$ from equation (1) with interactions of the *TARP Recipient*$_{it}$ with year dummies for each year before and after the TARP.

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

where Y_{st}, *TARP Recipient*$_{it}$, *Post TARP*$_{st}$, X_{st-1}, *State*$_s$, and *Time*$_t$ are defined as above. The "*D*s" are dummies for the years from 2006 to 2012, and ζ_{st} represents a white noise error term. We plot the coefficients, adjusted for seasonality, with their 95% confidence intervals for the local economic indicators in Figure 1, Graphs A, B, C, and D. [17]

Graphs A and B of Figure 1 illustrates that there is an immediate increase in net job creation and hiring establishments in 2009, but this increase is short-lived, only lasting until 2010. In Graphs C and D of Figure 1, we find that there is a decrease in business and personal bankruptcies immediately after TARP injections. This decline is slow and steady over the whole post-TARP period.

[17] To deseasonalize the data, we follow use the X11 procedure developed by the U.S. Census Bureau.

8.6 Decomposition of Local Economic Conditions

In Appendix X, we decompose our four indicators of local economic conditions. 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 are due to a decrease in gross firing establishments. We also find that the personal bankruptcies findings are primarily due to reductions in bankruptcies through Chapters 7 (liquidations). In addition, we find that TARP had impact on business bankruptcies as well via Chapter 12 (adjustments of debts) and 13 filings (adjustments of debts – small amounts).

8.7 Subsample Analyses

In Appendix Y, we conduct several subsample analyses to see in which types of banks and under what local economic conditions TARP was most effective. We have several important findings: 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 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 that may have occurred.

This paper contributes to the policy debate on the benefits and costs of the TARP program. Among the benefits appear to be the possible increase in lending and reduction in risk by small banks (Black and Hazelwood, 2013; Li, 2013; Puddu and Walchli, 2013), the increases in the market values of 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' customers (Norden, Roosenboom, and Wang, 2013), any improvement in the short-run overall stability of the financial system, which is difficult to document because so many government programs and market events occurred around the same time period, and the improvements in local economic conditions which are documented by this paper.

Among the costs are the potential increase 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), any reduction in lending by large banks (Black and Hazelwood, 2013), any distortion in competition caused by the bailouts of some banks and not others (Koetter and Noth, 2014; Berger and Roman, forthcoming), any distortion caused by the bailouts being partially distributed according to political connections (Bayazitova and Shivdasani (2012), Duchin and Sosyura (2012, 2014), Li (2013), Berger and Roman (forthcoming, 2014)), any reductions in the market values of the TARP recipient banks' customers (Liu, 2013), and the small profit to the Treasury that did not compensate for the risks.[18]

Our study adds to this debate on benefits and costs of TARP by offering the first evidence on the effect on local market 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.

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

References

Altman, E. I., 1984. A Further Empirical Investigation of the Bankruptcy Cost Question, Journal of Finance 39, 1067–1089.

Angrist, J.D., Pischke, J.S., 2009. Mostly Harmless Econometrics. Princeton University Press, Princeton, N.J.

Barofsky, N., 2012. An Inside Account of How Washington Abandoned Main Street While Rescuing Wall Street, www.thedailybeast.com/articles/2012/07/24/neil-barofsky-s-bailout-why-tarp-failed.html

Bayazitova, D., Shivdasani, A., 2012. Assessing TARP. Review of Financial Studies 25, 377-407.

Beck, T., Levine, R., Levkov, A., 2010. Big Bad Banks? The Winners and Losers from Bank Deregulation in the United States. Journal of Finance 65, 1637–67.

Berger, A. N., Black, L. K., Bouwman, C. H. S., Dlugosz, J. L., 2014. The Federal Reserve's Discount Window and TAF Programs: "Pushing on a String?." Working Paper, University of South Carolina.

Berger, A. N., Bouwman, C. H. S., 2009. Bank Liquidity Creation. Review of Financial Studies 22, 3779-3837.

Berger, A. N., Bouwman C. H. S., Kick T. K., Schaeck K., 2014. Bank Risk Taking and Liquidity Creation Following Regulatory Interventions And Capital Support. Working Paper, University of South Carolina.

Berger, A. N., Dick, A., 2007. Entry into Banking Markets and the Early-Mover Advantage. Journal of Money, Credit and Banking 39, 775-807.

Berger, A. N., Imbierowicz, B., Rauch, C., 2014. The Roles of Corporate Governance in Bank Failures during the Recent Financial Crisis. Working Paper, University of South Carolina.

Berger, A. N., Kick T. K., Schaeck K. Forthcoming. Executive Board Composition and Bank Risk Taking. Journal of Corporate Finance.

Berger, A. N., Roman R. A., Forthcoming. Did TARP Banks Get Competitive Advantages?. Journal of Financial and Quantitative Analysis.

Black, L., Hazelwood, L., 2013. The Effect of TARP on Bank Risk-taking. Journal of Financial Stability 9, 790-803.

Brandao-Marques, L., Correa, R., Sapriza, H., 2012. International Evidence on Government Support and Risk-Taking in the Banking Sector. IMF Working Paper.

Bris, A., Welch, I., Zhu, N., 2006. The Costs of Bankruptcy: Chapter 7 Liquidation versus Chapter 11 Reorganization. The Journal of Finance 61: 1253-1303.

Calderon C., Schaeck, K., 2012. Bank bailouts, competitive distortions, and consumer welfare. Working Paper. World Bank.

Calomiris, C. W., Khan, U., Forthcoming. An Assessment of TARP Assistance to Financial Institutions. Journal of Economic Perspectives.

Chang, S - H., Contessi, S., Francis, J. L., 2014. Understanding the Accumulation of Bank and Thrift Reserves during the US Financial Crisis. Journal of Economic Dynamics and Control, 43, 78-106.

Cordella, T., Yeyati, E. L., 2003. Bank Bailouts: Moral Hazard versus Value Effect. Journal of Financial Intermediation 12, 300-330.

Cornett, M. M., Li, L., Tehranian, H., 2013. The Performance of Banks around the Receipt and Repayment of TARP Funds: Over-achievers versus Under-achievers, Journal of Banking & Finance 37, 730–746.

Dam, L., Koetter, M., 2012. Bank Bailouts and Moral Hazard: Empirical Evidence from Germany. Review of Financial Studies 25, 2343-2380.

Duchin D., Sosyura D. 2012. The Politics of Government Investment. Journal of Financial Economics 106, 24-48.

Duchin R., Sosyura, D., 2014. Safer Ratios, Riskier Portfolios: Banks' Response to Government Aid. Journal of Financial Economics 113, 1-28.

Fudenberg, D., Tirole, J., 1986. A 'Signal-Jamming' Theory of Predation. Rand Journal of Economics 17, 366-376.

Gropp, R., Hakenes, H., Schnabel, I., 2011. Competition, Risk-Shifting, and Public Bail-Out Policies. Review of Financial Studies 24, 2084-120.

Hakenes, H., Schnabel, I., 2010. Banks without Parachutes: Competitive Effects of Government Bail-Out Policies. Journal of Financial Stability 6, 156-168.

Hicks, J., 1935. Annual Survey of Economic Theory: The Theory of Monopoly. Econometrica 3, 1-20.

Holmstrom, B., Tirole, J., 1997. Financial Intermediation, Loanable Funds, and the Real Sector. Quarterly Journal of Economics 112, 663-691.

Hoshi, T., Kashyap, A.,. 2010. Will the U.S. Bank Recapitalization Succeed? Eight Lessons from Japan. Journal of Financial Economics 97, 398–417.

Hotchkiss, E. S., 1995. Post-Bankruptcy Performance and Management Turnover, Journal of Finance 50, 3–21.

Hryckiewicz, A. 2012. Government Interventions - Restoring or Destroying Financial Stability in the Long Run?, Working Paper. Goethe University of Frankfurt.

Keeley, M. C., 1990. Deposit Insurance, Risk, and Market Power in Banking. American Economic Review 80, 1183-1200.

Koetter, M. and Noth, F., 2014. Bank Bailouts and Competition. Working Paper, Frankfurt School of Finance and Management.

Li, L., 2013. TARP Funds Distribution and Bank Loan Supply, Journal of Banking and Finance 37: 4777-4792.

Liu, X., 2013. Transmission of Stigma in Financial Markets: Evidence from Trouble Asset Relief Program (TARP). Working Paper. National University of Singapore.

Meyer, B. D., 1995. Natural and Quasi-Experiments in Economics. Journal of Business and Economic Statistics 13, 151-161.

Meyer, P. A., Pifer, H. W., 1970. Prediction of Bank Failures. The Journal of Finance, 25, 853-868.

Ng, J., Vasvari, F. P., Wittenberg-Moerman, R., 2013. The Impact of TARP's Capital Purchase Program on the Stock Market Valuation of Participating Banks. Working Paper. University of Chicago.

Norden, L., Roosenboom, P., Wang, T., 2013. The Impact of Government Intervention in Banks on Corporate Borrowers' Stock Returns. Journal of Financial and Quantitative Analysis 48, 1635-1662.

Petersen, M. A., Rajan, R. G., 1995. The Effect of Credit Market Competition on Lending Relationships. Quarterly Journal of Economics, 110, 407-443.

Paulson, H., 2013. Five Years from the Brink, Bloomberg BusinessWeek, September 2013, www.moneynews.com/FinanceNews/Paulson-crisis-financial-Fed/2013/09/13/id/525579

Puddu, S., Wälchli, A., 2013. TARP Effect on Bank Lending Behaviour: Evidence from the Last Financial Crisis, Working Paper, Université de Neuchâtel.

Schaeck, K., Cihak, M., Maehler, A. M., Stolz, S. M., 2012. Who Disciplines Bank Managers?, Review of Finance, 16:197-243.

Stiglitz, J. E., Weiss, A., 1981. Credit Rationing in Markets with Imperfect Information, American Economic Review 71: 393-410.

Telser, L. G., 1966. Cutthroat Competition and the Long Purse. Journal of Law and Economics 9, 259-77.

Veronesi, P., Zingales L., 2010. Paulson's Gift, Journal of Financial Economics 97, 339-36.

Weiss, L. A., 1990. Bankruptcy Resolution, Journal of Financial Economics 27, 285–314.

Weiss, L. A., Wruck, K.H., 1998. Information Problems, Conflicts of Interest, and Asset Stripping: Chapter 11's Failure in the Case of Eastern Airlines, Journal of Financial Economics 48, 55–97.

Wilson, L., Wu, Y.W., 2012. Escaping TARP. Journal of Financial Stability 8, 32–42.

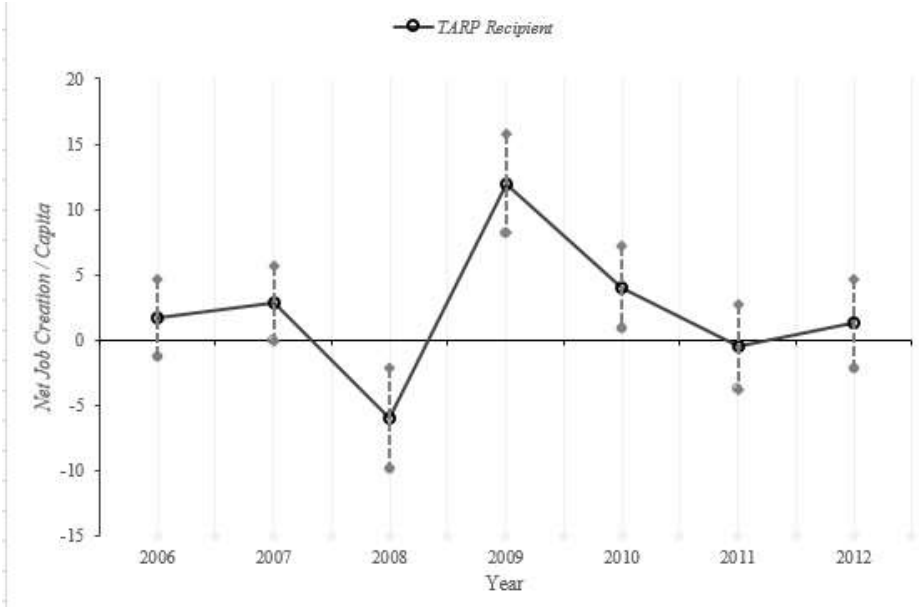
Wooldridge, J. M. 2002. Econometric Analysis of Cross Section and Panel Data. Cambridge, MA: MIT Press, 623-25.

Wruck, K. H., 1990. Financial Distress, Reorganization, and Organizational Efficiency. Journal of Financial Economics, 27: 419-444.

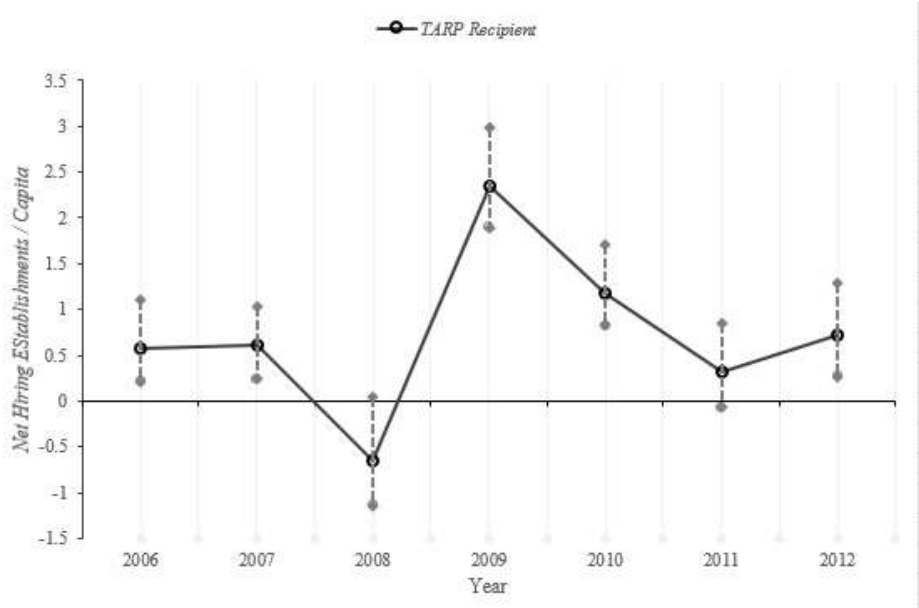
Figure 1: The Dynamic Impacts of TARP on Local Economic Conditions

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 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).

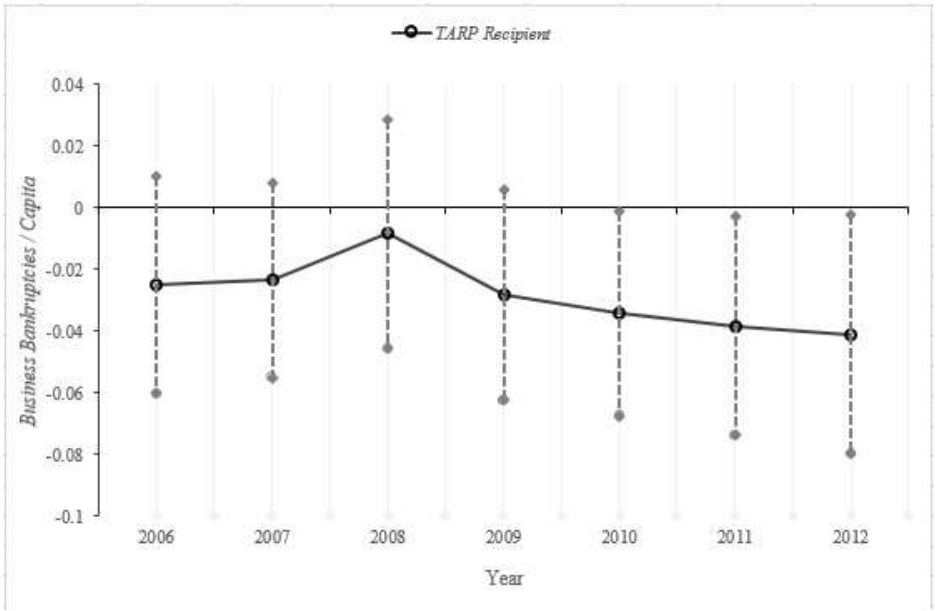
Graph A. The Dynamic Impacts of TARP on Net Job Creation / Capita



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

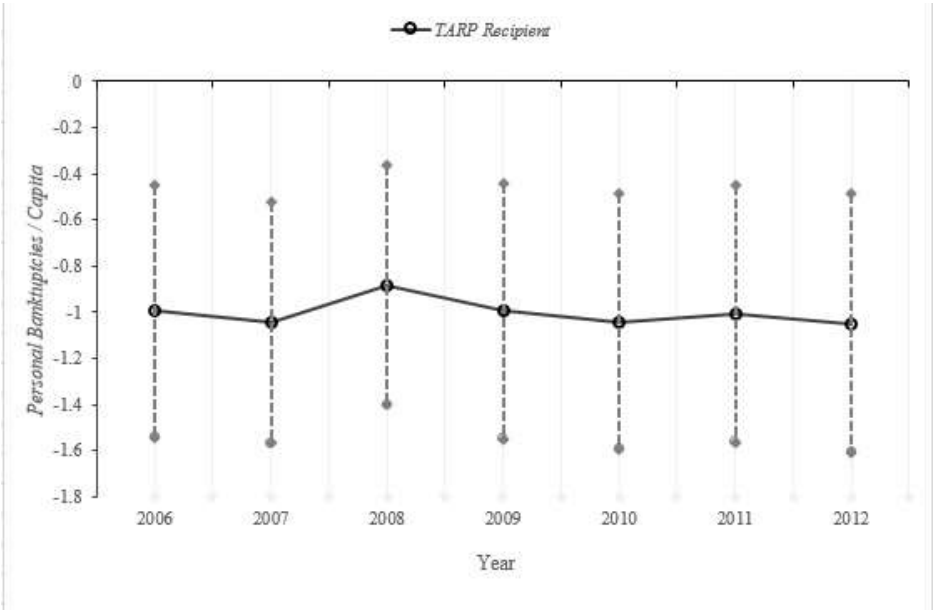


Table 1: Definitions and Summary Statistics

This table reports summary statistics for the full US 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 Variables***						
	Net Job Creation/ Capita	The overall net job creation per capita from t-1 to t calculated as: (Gross Job Creation - Gross Job Destruction)/(Population/1000). Gross Job Creation is the number of jobs created from t-1 to t. 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 from t-1 to t. It consists of job closings and contractions, defined analogously.	0.207	0.645	3.613	-1.256	2.257
	Gross Job Creation / Capita	The gross job creation per capita is the number of jobs created from t-1 to t 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
	Gross Job Creation - Openings/ Capita	The job openings from the prior quarter to the current quarter per capita, calculated as: (Number of new jobs created at new establishments from the previous quarter to current quarter) / (Population/1000).	4.750	4.474	1.347	3.839	5.326
	Gross Job Creation - Expansions/ Capita	The expansions from the prior quarter to the current quarter per capita, calculated as: (Number of new jobs created at existing establishments that expand their operations from the previous quarter to current quarter) / (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 from the prior quarter to the current quarter, calculated as: ((Number of jobs destroyed from the previous quarter to current quarter) / (Population/1000). It consists of job closings and contractions.	24.646	23.873	5.080	21.258	26.880
	Gross Job Destruction - Closings/ Capita	The job closings per capita from the prior quarter to the current quarter, calculated as: (Number of jobs lost due to closing establishment closings from the previous quarter to current quarter) / (Population/1000).	4.432	4.191	1.288	3.636	5.031
	Gross Job Destruction - Contractions/ Capita	The contractions from the prior quarter to the current quarter per quarter, calculated as: (Number of jobs lost due to existing establishments that contract their operations from the previous quarter to current quarter) / (Population/1000).	20.213	19.553	4.163	17.415	21.953
	Net Hiring Establishments Variables						
	Net Hiring Establishments/ Capita	The overall net hiring establishments per capita from t-1 to t, calculated as: (Gross Hiring Establishments - Gross Firing Establishments)/(Population/1000). Gross Hiring Establishments is the number of hiring establishments that create jobs from t-1 to t. It consists of establishments that create jobs through job openings and expansions. Gross Firing Establishments is the number of establishments that destroy jobs from the t-1 to t. It consists of establishments that destroy jobs through job closings and contractions.	-0.157	-0.074	0.642	-0.467	0.227
	Gross Hiring Establishments/ Capita	The gross hiring establishments per capita is the number of hiring establishments that create jobs from the prior quarter to the current quarter, calculated as: (Number of establishments that created jobs from the previous quarter to current quarter) / (Population/1000). It consists of establishments that created jobs through openings and expansions.	6.649	6.291	1.372	5.690	7.343
	Gross Hiring Establishments - Openings/ Capita	The hiring establishments that create jobs via openings per capita from the prior quarter to the current quarter, calculated as: (Number of new establishments that created jobs via openings from the previous quarter to current quarter) / (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 from the prior quarter to the current quarter, calculated as: (Number of establishments that created jobs via operations expansions from the previous quarter to current quarter) / (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 from the prior quarter to the current quarter, calculated as: (Number of establishments that destroyed jobs from the previous quarter to current quarter) / (Population/1000). It consists of establishments that destroyed jobs through closings and contractions.	6.806	6.432	1.333	5.882	7.498
	Gross Firing Establishments - Closings/ Capita	The firing establishments that destroy jobs via closings per capita from the prior quarter to the current quarter, calculated as: (Number of establishments that destroyed jobs via closings from the previous quarter to current quarter) / (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 from the prior quarter to the current quarter, calculated as: (Number of establishments that destroyed jobs via contractions from the previous quarter to current quarter) / (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*
	Business Bankruptcies Variables						
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 at time t) / (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
	Business Bankruptcies - Chapter 7/ Capita	Number of Chapter 7 business bankruptcies per capita, calculated as: ((Total Chapter 7 business bankruptcy filings from the prior quarter to current quarter) / (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 from the prior quarter to current quarter) / (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 from the prior quarter to current quarter) / (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 from the prior quarter to current quarter) / (Population/1000).	0.003	0.002	0.003	0.001	0.004
	Personal Bankruptcies Variables						
	Personal Bankruptcies/ Capita	The overall number of personal bankruptcies per capita, calculated as: (Total personal bankruptcy filings at time t) / (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
	Business Bankruptcies - Chapter 7/ Capita	Number of Chapter 7 personal bankruptcies per capita, calculated as: ((Total Chapter 7 personal bankruptcy filings from the prior quarter to current quarter) / (Population/1000).	0.760	0.602	0.672	0.378	0.907
	Business Bankruptcies - Chapter 11/ Capita	Number of Chapter 11 personal bankruptcies per capita, calculated as: ((Total Chapter 11 personal bankruptcy filings from the prior quarter to current quarter) / (Population/1000).	0.001	0.000	0.001	0.000	0.001
	Business Bankruptcies - Chapter 13/ Capita	Number of Chapter 13 personal bankruptcies per capita, calculated as: ((Total Chapter 13 personal bankruptcy filings from the prior quarter to current quarter) / (Population/1000).	0.275	0.211	0.243	0.109	0.330
TARP Variables *(Source: US Department of the Treasury)*	*TARP Recipient*	The weighted proportion of TARP banks receiving TARP in the local markets. It is the product of the TARP recipient dummy which is 1 if a bank received TARP capital support with the bank's deposits' weight in the local market.	0.156	0.083	0.183	0.035	0.203
	Post TARP	An indicator equal to 1 in 2009 -2012 and 0 in 2005-2008. Similar to Sosyura and Durchin (2012) but using an extended time period.	0.516456	1	0.4998874	0	1

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)*	*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	0.102	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.	0.008	0.002	0.016	0.001	0.008
	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.	0.023	0.021	0.100	0.009	0.035
	CAMELS Proxy: Liquidity	The weighted proportion of the bank liquidity in the local markets. Liquidity is defined as cash divided by bank total deposits.	0.058	0.042	0.079	0.031	0.067
	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.	0.143	0.096	0.546	0.040	0.184
	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
	Metropolitan Dummy	The weighted proportion of banks having the the majority of bank deposits (50% or more) in metropolitan areas.	0.336	0.276	0.233	0.160	0.461
	State No Banks	The logarithm of the total number of banks in the state.	4.180	4.369	1.225	3.401	5.106
	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
Instrumental Variables: *(Sources: Center for Responsive Politics, House of Representatives website, Missouri Census Data Center)*	*Subcommittee 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: Effect 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	-5.660***	-1.468***	0.039*	0.159
	(-3.288)	(-5.143)	(1.817)	(0.771)
Post TARP	-0.962**	-0.195**	-0.009**	-0.921***
	(-1.979)	(-2.571)	(-2.081)	(-16.612)
Post TARP x TARP Recipient	6.670***	1.302***	-0.020	-0.309***
	(5.206)	(6.172)	(-1.392)	(-2.596)
Capital Adequacy	-14.217	-5.130*	-0.325	-3.480**
	(-0.890)	(-1.926)	(-1.314)	(-2.166)
Asset Quality	-5.386	2.468	-0.134	-0.967
	(-0.302)	(0.920)	(-0.637)	(-0.704)
Management Quality	-27.336	-1.202	0.166	-1.336
	(-1.259)	(-0.301)	(1.029)	(-0.677)
Earnings	65.418***	14.100***	-0.199**	-0.385
	(4.859)	(6.233)	(-2.077)	(-0.396)
Liquidity	1.786	0.713	0.083*	1.006**
	(0.407)	(0.991)	(1.821)	(2.345)
Sensitivity to Market Risk	10.449**	1.970**	0.012	0.743
	(2.089)	(2.261)	(0.154)	(1.406)
DWTAF	-0.674	0.264	0.013	0.179
	(-0.372)	(0.806)	(0.585)	(0.913)
Bank Size	0.137	0.046	-0.004*	-0.070***
	(0.696)	(1.526)	(-1.766)	(-3.924)
HHI Deposits	0.001**	0.000**	0.000	0.000
	(2.009)	(2.429)	(1.148)	(1.122)
Metropolitan Dummy	-4.374	-1.177***	0.033	1.299***
	(-1.559)	(-2.716)	(1.201)	(4.655)
State No Banks	0.252	-0.151**	-0.001	0.023
	(0.545)	(-2.180)	(-0.104)	(0.555)
State Minimum Wage	-0.150	-0.046*	0.001	-0.020
	(-1.053)	(-1.933)	(0.661)	(-1.390)
State Marginal Tax Rate	-0.044	0.001	0.002	0.018
	(-0.315)	(0.053)	(1.333)	(1.352)

State Economic Freedom Index	-0.733	-0.066	-0.017**	-0.056
	(-1.271)	(-0.665)	(-2.180)	(-0.591)
Intercept	6.978	1.550**	0.152**	2.210***
	(1.522)	(1.974)	(2.149)	(3.156)
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	0.454	0.541	0.625	0.874

Table 3: Effect of TARP on Local Economic conditions – Instrumental Variable Analysis

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. We use as instruments several political and regulatory connections variables: *Subcommittee on Financial Institutions or Capital Markets, Democrat,* and *Fed Director*. *Subcommittee 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: IV First Stage as in Wooldridge (Section 18.4.1)

First Stage (Probit Model)	
Dependent Variable:	***TARP Recipient (Dummy)***
Independent Variables:	**(1)**
Subcommitee on Financial Institutions or Capital Markets	0.099***
	(5.177)
Democrat	0.050***
	(5.208)
FED Director	0.379***
	(11.932)
Bank Controls	**YES**
Time Fixed Effects	**YES**
Observations	172,002
Pseudo R-squared	0.2365

Panel B: Last Stage as in Wooldridge (Section 18.4.1)

	Last Stage (IV 2SLS)			
Dependent Variable:	***Net Job Creation/ Capita***	***Net Hiring Establishments/ Capita***	***Business Bankruptcies/Capita***	***Personal Bankruptcies/Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-8.920***	-1.778***	-0.012	0.038
	(-3.106)	(-3.356)	(-0.472)	(0.129)
Post TARP	-0.998**	-0.199***	-0.008*	-0.910***
	(-2.102)	(-2.684)	(-1.904)	(-16.868)
Post TARP x TARP Recipient	7.804***	1.432***	-0.038**	-0.518***
	(5.413)	(5.887)	(-2.389)	(-3.165)
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	0.452	0.540	0.619	0.874
First Stage Kleibergen-Paap rk Wald F-test	142.743***	142.743***	142.743***	142.743***

Table 4: Effect of TARP on Bank Competition: Placebo Experiments

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	-1.031	-0.344*	-0.056***	-0.349*
	(-0.823)	(-1.717)	(-2.871)	(-1.890)
Placebo Post TARP	0.177	-0.433***	-0.001	-0.545***
	(0.499)	(-8.213)	(-0.499)	(-13.043)
Placebo Post TARP x TARP Recipient	0.049	-0.066	0.011	0.527***
	(0.059)	(-0.497)	(0.998)	(4.867)
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	0.412	0.463	0.520	0.886

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 random effects in Panel A, state and time fixed effects with errors clustered at the state level in Panel B, models excluding the proxies for CAMELS in Panel C, models excluding all bank-related controls in Panel D, and models excluding all state-related controls in Panel E. 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 – State Random 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	-4.970***	-1.253***	0.037	0.160
	(-2.587)	(-4.359)	(1.447)	(0.871)
Post TARP	-0.814**	-0.153**	-0.004	-0.920***
	(-2.193)	(-2.307)	(-1.039)	(-10.847)
Post TARP x TARP Recipient	6.235***	1.275***	-0.018	-0.295**
	(3.929)	(4.835)	(-1.291)	(-2.022)
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	0.443	0.531	0.211	0.657

Panel B: Regression parameters –State Fixed Effects & 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	-5.660**	-1.468***	0.039	0.159
	(-2.554)	(-4.864)	(1.457)	(0.826)
Post TARP	-0.962*	-0.195*	-0.009*	-0.921***
	(-1.683)	(-1.732)	(-1.973)	(-9.911)
Post TARP x TARP Recipient	6.670***	1.302***	-0.020	-0.309**
	(3.814)	(4.571)	(-1.418)	(-2.061)
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**
State Clusters	51	51	51	51
Observations	1,580	1,580	1,580	1,580
Adjusted R-squared	0.454	0.541	0.625	0.874

Panel C: Regression parameters – Excluding the Proxies for *CAMELS*

Dependent Variable:	***Net Job Creation/ Capita***	***Net Hiring Establishments/ Capita***	***Business Bankruptcies/Capita***	***Personal Bankruptcies/Capita***
Independent Variables:	**(1)**	**(2)**	**(3)**	**(4)**
TARP Recipient	-3.395**	-0.910***	0.046**	0.294*
	(-2.202)	(-3.422)	(2.294)	(1.730)
Post TARP	-0.903*	-0.177**	-0.009**	-0.914***
	(-1.884)	(-2.350)	(-2.037)	(-16.789)
Post TARP x TARP Recipient	3.321***	0.659***	-0.022***	-0.442***
	(4.047)	(4.840)	(-3.013)	(-5.577)
Bank-Related Controls other than CAMELS proxies	**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	0.444	0.525	0.623	0.874

Panel D: 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	-1.698*	-0.407***	0.018***	0.286***
	(-1.856)	(-2.760)	(2.951)	(2.992)
Post TARP	-0.867*	-0.152**	-0.012***	-1.003***
	(-1.793)	(-2.040)	(-2.711)	(-18.265)
Post TARP x TARP Recipient	3.656***	0.732***	-0.017***	-0.456***
	(4.489)	(5.443)	(-2.925)	(-5.681)
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	1,580	1,580	1,580	1,580
Adjusted R-squared	0.442	0.520	0.616	0.872

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	-5.679***	-1.515***	0.039*	0.276
	(-3.490)	(-5.584)	(1.949)	(1.394)
Post TARP	-0.742**	-0.178***	-0.001	-0.902***
	(-2.079)	(-3.438)	(-0.304)	(-18.088)
Post TARP x TARP Recipient	6.336***	1.248***	-0.020	-0.427***
	(5.393)	(6.429)	(-1.479)	(-3.528)
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	0.450	0.533	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 quartile 1 and 4 of the proportions of TARP recipients in the state. Panel B reports estimates when using only tercile 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 – Quartile 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	-5.980**	-1.775***	0.065*	0.500**
	(-2.549)	(-4.319)	(1.691)	(2.257)
Post TARP	-1.294*	-0.200*	-0.017**	-1.026***
	(-1.881)	(-1.756)	(-2.379)	(-14.728)
Post TARP x TARP Recipient	8.421***	1.633***	-0.022	-0.351**
	(5.237)	(6.138)	(-1.345)	(-2.254)
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	0.439	0.517	0.576	0.876

Panel B: Regression parameters – Tercile 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	-5.141**	-1.455***	0.046*	0.170
	(-2.563)	(-4.188)	(1.689)	(0.616)
Post TARP	-1.439**	-0.237**	-0.011**	-0.957***
	(-2.516)	(-2.509)	(-1.996)	(-15.615)
Post TARP x TARP Recipient	7.438***	1.397***	-0.016	-0.292**
	(5.273)	(5.898)	(-1.123)	(-2.237)
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	0.461	0.533	0.612	0.860

APPENDIX X – DECOMPOSITIONS

In Table X.1, we decompose our four indicators of local economic conditions. We first decompose *Net Job Creation / Capita* into its components, *Gross Job Creation / Capita* (consisting of openings and expansions) and *Gross Job Destruction / Capita* (consisting of 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* from column (1) into its subcomponents of job openings (jobs created at new establishments) and expansions (jobs created at existing establishments that expand operations). 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* from column (4) into its subcomponents of job closings (jobs lost due to closing establishments) and contractions (jobs lost due to 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 its components, *Gross Hiring Establishments / Capita* and *Gross Firing Establishments/ Capita.* Results in 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* from column (1) into its subcomponents of establishment openings (new establishments that create jobs) and establishment expansions (establishments that expand their operations and create jobs). We find statistically significant increases in both establishment openings and expansions, with expansions being the larger in magnitude. Similarly, in columns (5) and (6), we decompose *Gross Firing*

Establishments / Capita from column (4) into its subcomponents of establishment closings (closing establishments that destroy jobs) and contractions (continuing establishments that contract their operations and 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 between the different incentives and conditions that lead a corporation to choose one bankruptcy filing over another. Therefore, we decompose *Business Bankruptcies / Capita* into its subcomponents: *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.[22] Results in Panel C columns (1) - (4) suggest that there are statistically significant reductions in bankruptcies through Chapters 12 and 13 filings, however the coefficients are very small. We also decompose *Personal Bankruptcies / Capita* into its subcomponents: *Personal Bankruptcies / Capita - Chapter 7, Personal Bankruptcies / Capita - Chapter 11,* and *Personal Bankruptcies / Capita - Chapter 13.* Consistent with our main results, results in Panel D columns (1) - (3) suggest that TARP led to statistically significant decreases in personal bankruptcies via Chapter 7 (liquidations) filings.

[22] As noted in Bris, Welch, and Zhu (2006), there may be significant differences between Chapter 7 and Chapter 11 filings for large corporate bankruptcies. First, firms are more likely to file for Chapter 11 when they are large. The propensity to reorganize is strongest in the $100,000 to $1 million category, and is still positive but diminishing when assets are above $1 million. Second, firms that have a large number of secured creditors are more likely to file for Chapter 11 than Chapter 7 liquidation. This could point to coordination problems among creditors, with debtors recognizing that Chapter 11 could overcome this type of obstacle and result in a viable reorganized firm after bankruptcy. Third, firms in which a bank is a creditor — especially an unsecured creditor — are more likely to choose liquidation over reorganization. This is consistent with the view that pre-bankruptcy negotiations are more likely to occur when the main creditor is a bank, and this bank has already shown itself unwilling to compromise. Finally, firms that are more underwater tend to prefer Chapter 11 reorganization over Chapter 7 liquidation. This could imply that firms not underwater that filed for Chapter 7 did so under economic, rather than financial, distress.

Table X.1: Effect 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	-4.423***	-0.466	-3.957***	1.237	-0.001	1.252
	(-3.593)	(-1.026)	(-3.997)	(1.122)	(-0.003)	(1.293)
Post TARP	-3.957***	-0.809***	-3.148***	-2.995***	-0.748***	-2.245***
	(-10.936)	(-6.513)	(-10.882)	(-8.510)	(-4.715)	(-8.636)
Post TARP x TARP Recipient	3.560***	0.174	3.386***	-3.110***	-0.062	-3.058***
	(4.100)	(0.602)	(4.828)	(-3.366)	(-0.238)	(-3.863)
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	0.885	0.860	0.678	0.865

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	-1.088***	-0.263***	-0.825***	0.380**	0.105	0.275*
	(-5.882)	(-3.177)	(-5.836)	(2.217)	(1.407)	(1.912)
Post TARP	-0.603***	-0.066***	-0.537***	-0.408***	-0.003	-0.406***
	(-11.160)	(-3.257)	(-12.514)	(-8.683)	(-0.106)	(-11.661)
Post TARP x TARP Recipient	0.871***	0.132***	0.739***	-0.431***	-0.079	-0.352***
	(6.390)	(2.590)	(7.052)	(-3.047)	(-1.435)	(-3.198)
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	0.957	0.857	0.960

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	0.011*	0.026	0.001*	0.001
	(1.925)	(1.304)	(1.869)	(0.893)
Post TARP	-0.007***	-0.000	-0.000	-0.002***
	(-4.296)	(-0.098)	(-0.090)	(-3.637)
Post TARP x TARP Recipient	-0.004	-0.015	-0.000*	-0.002**
	(-0.858)	(-1.118)	(-1.666)	(-2.115)
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	0.725	0.564	0.415	0.638

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	0.131	0.000	0.027
	(0.658)	(0.688)	(0.726)
Post TARP	-0.856***	0.000***	-0.066***
	(-15.759)	(2.718)	(-5.724)
Post TARP x TARP Recipient	-0.274**	0.000	-0.035
	(-2.384)	(0.298)	(-1.358)
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	0.931

APPENDIX Y – OTHER ROBUSTNESS 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 literature on TARP (e.g., Black and Hazelwood, 2013; Li, 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. Thus, we next consider the effects of TARP on local economic conditions by bank size.

We consider 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 interaction terms between these TARP variables and the *Post TARP* dummy. 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 the net job creation and hiring establishments relatively more compared to the proportion of the small TARP banks and lead to a statistically significant decrease in business and personal bankruptcies. Also, the *t*-test for the difference in coefficients between the proportions of the three TARP bank size groups reported in Panel A2 shows that the differences between the effects of the proportions of small and large TARP banks and those between the small and medium size TARP banks are statistically significant for the net job creation and hiring establishments, but not for business or personal bankruptcies. Overall, these findings runs counter

to the effects of TARP on lending found in the literature.

Y.2 Involuntary and Voluntary Participants

Most of the banks voluntarily participated in the TARP program, however there are a few that were required to participate in the program 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.[23] Therefore, 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 and improvements are statistically and economically significant.

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

The US 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.[24] 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 more adverse scenario, or the Treasury would provide the capital. In later years, this became the Comprehensive Capital Analysis and

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

[24] These 19 banks included 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.

Review (CCAR). Given this special treatment of banks under SCAP and CCAR, we worry that the local economic conditions results may be driven by this subsample of banks.

Therefore, we consider separately the proportions of TARP stress-tested and non-stress-tested banks and we 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.

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.[25]

We rerun our tests by differentiating between TARP banks that repaid early and those that did not. We replace the *TARP Recipient*$_{st}$ variable with *TARP Recipient_Repaid Early*$_{st}$ (proportions of TARP banks that repaid early in 2009-2010 in the local markets) and *TARP Recipient_Not Repaid Early*$_{st}$ (proportions of TARP banks that did not repay early in the local markets) and similarly for the interaction terms. 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. All are economically significant, except for the business bankruptcies, for which coefficient is very small. Also, the *t*-tests for the differences in coefficients between the effects of the proportions of the two TARP groups reported in Panel D2 are statistically significant for all but the business

[25] Berger and Roman (forthcoming) find that the competitive benefits of TARP are primarily or entirely due to TARP recipients that repaid early.

bankruptcies.

Y.5 Capitalization Ratio (2008:Q3)

Banks with a higher level of capital prior to infusion may have a better ability to use the extra capital from the infusion to expand loans and loan commitments and thus alter local economic conditions. Alternatively, 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. Therefore, we consider separately the proportions of TARP banks with low equity to assets ratio (*EQCAP_08Q3* ≤ median) and high equity to assets ratio (*EQCAP_08Q3* > median) before the TARP program started and create *LOWCAP TARP Recipient* and *HIGHCAP TARP Recipient*, and interact these variables with our *Post TARP* dummy. Regression estimates are shown in Table Y.1 Panel E1, columns (1)-(4).

We find that results are mixed. The job creation effects are primarily due to the proportions of well capitalized TARP banks that statistically significantly help increase net job creation and hiring establishments as indicated by the positive coefficients for the DID terms. As for the bankruptcy effects, these are primarily due to the proportions of TARP poor-capitalized banks, that help statistically significantly decrease business and personal bankruptcies. 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

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) and create the following two variables: *LOWCOINCIDENT TARP Recipient* and *HIGHCOINCIDENT TARP Recipient* and interact these variables with our *Post TARP* dummy. 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 help statistically significantly increase net job creation and hiring establishments, and help statistically significantly decrease business and personal bankruptcies. All effects are also economically significant, except for business 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 in Low and High Economic Freedom Conditions

It is possible that the 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) and create the following two variables: *LOWECFREEDOM TARP Recipient* and *HIGHECFREEDOM TARP Recipient* and interact these variables with our *Post TARP* dummy. 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 help statistically and economically significantly increase net job creation and hiring establishments and help statistically significantly decrease 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 net hiring establishments, but not for the others.

Table Y.1: Effect 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	16.441	1.305	0.048	1.944*
	(1.184)	(0.605)	(0.782)	(1.689)
MEDIUM TARP Recipient	-13.977	-2.576*	0.096*	0.233
	(-1.472)	(-1.850)	(1.930)	(0.233)
LARGE TARP Recipient	-5.410***	-1.452***	0.036*	0.117
	(-3.175)	(-5.014)	(1.752)	(0.541)
Post TARP	-0.944*	-0.177**	-0.009**	-0.899***
	(-1.834)	(-2.251)	(-2.188)	(-15.729)
Post TARP x SMALL TARP Recipient	-2.293	-0.801	0.023	-0.647
	(-0.267)	(-0.517)	(0.321)	(-0.683)
Post TARP x MEDIUM TARP Recipient	28.227***	3.644***	-0.111**	-1.499*
	(3.539)	(2.981)	(-2.505)	(-1.732)
Post TARP x LARGE TARP Recipient	6.104***	1.258***	-0.017	-0.246**
	(4.679)	(5.809)	(-1.090)	(-2.200)
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	0.456	0.541	0.624	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*	0.985	1.334	0.519	0.424
***t*-stat:** *Post TARP x SMALL TARP Recipient = Post TARP x MEDIUM TARP Recipient*	2.291***	1.954**	1.676	0.781
***t*-stat:** *Post TARP x MEDIUM TARP Recipient = Post TARP x LARGE TARP Recipient*	2.727***	1.916**	1.902**	1.480

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	-3.905*	-1.223***	0.016	0.196
	(-1.946)	(-3.579)	(0.638)	(0.731)
TARP Recipient x VOL	-7.338***	-1.743***	0.047*	0.227
	(-3.892)	(-5.808)	(1.915)	(1.033)
Post TARP	-0.975**	-0.203***	-0.011***	-0.905***
	(-1.998)	(-2.668)	(-2.646)	(-16.501)
Post TARP x TARP Recipient x INVOL	4.955***	0.950***	-0.035	-0.056
	(2.885)	(3.380)	(-1.431)	(-0.442)
Post TARP x TARP Recipient x VOL	8.762***	1.694***	-0.014	-0.522***
	(5.575)	(6.798)	(-1.379)	(-2.708)
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	0.455	0.542	0.874	0.627

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:** *Post TARP x TARP Recipient x INVOL = Post TARP x TARP Recipient x VOL*	1.792*	2.252**	0.872	0.989

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	-4.242**	-1.411***	0.020	0.207
	(-2.224)	(-4.372)	(1.062)	(0.978)
TARP Recipient x NON Stress-Tested	-9.776***	-1.637***	0.083**	0.031
	(-3.487)	(-4.052)	(2.055)	(0.088)
Post TARP	-1.054**	-0.200**	-0.014***	-0.917***
	(-2.106)	(-2.572)	(-2.869)	(-16.583)
Post TARP x TARP Recipient x Stress-Tested	5.621***	1.250***	-0.039**	-0.308***
	(3.935)	(5.307)	(-2.351)	(-2.851)
Post TARP x TARP Recipient x NON Stress-Tested	11.279***	1.532***	0.064**	-0.319
	(4.001)	(3.478)	(2.542)	(-0.867)
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	0.455	0.540	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:** *Post TARP x TARP Recipient x Stress-Tested = Post TARP x TARP Recipient x NON Stress-Tested*	1.778*	0.566	3.461***	0.988

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	-4.522**	-1.430***	0.035	0.169
	(-2.489)	(-4.634)	(1.452)	(0.631)
TARP Recipient_Not Repaid	-9.294***	-1.657***	0.053**	0.219
	(-3.702)	(-4.275)	(2.195)	(0.911)
Post TARP	-1.020**	-0.207***	-0.009**	-0.907***
	(-2.086)	(-2.721)	(-2.155)	(-16.430)
Post TARP x TARP Recipient_Repaid Early	5.278***	1.149***	-0.012	-0.175
	(3.715)	(4.935)	(-0.626)	(-1.325)
Post TARP x TARP Recipient_Not Repaid Early	12.198***	1.926***	-0.050**	-0.862***
	(4.812)	(4.701)	(-2.225)	(-3.661)
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	0.455	0.541	0.625	0.874

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:** *Post TARP x TARP Recipient_Repaid Early= Post TARP x TARP Recipient_Not Repaid Early*	2.431***	1.720*	1.044	2.612***

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	-10.003***	-2.151***	0.043*	0.125
	(-4.883)	(-6.441)	(1.817)	(0.532)
TARP Recipient x LOWCAP	-2.615	-1.048***	0.023	0.152
	(-1.427)	(-3.379)	(0.990)	(0.662)
Post TARP	-0.938*	-0.192**	-0.009**	-0.921***
	(-1.936)	(-2.538)	(-2.145)	(-16.630)
Post TARP x TARP Recipient x HIGHCAP	9.414***	1.877***	0.006	-0.215
	(5.309)	(6.653)	(0.466)	(-1.157)
Post TARP x TARP Recipient x LOWCAP	5.157***	0.966***	-0.039**	-0.372***
	(3.392)	(3.940)	(-2.160)	(-2.909)
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	0.460	0.545	0.629	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:** *Post TARP x LOWCAP TARP Recipient = Post TARP x HIGHCAP TARP Recipient*	2.035**	2.827***	2.742***	0.645

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	-6.263***	-1.597***	0.043**	0.240
	(-3.401)	(-5.454)	(2.047)	(1.168)
TARP Recipient x HIGHCOINCIDENT	-5.126***	-1.351***	0.034	0.083
	(-2.876)	(-4.421)	(1.508)	(0.365)
Post TARP	-0.972**	-0.195***	-0.009**	-0.922***
	(-2.016)	(-2.593)	(-2.110)	(-16.610)
Post TARP x TARP Recipient x LOWCOINCIDENT	8.488***	1.606***	-0.029***	-0.465***
	(6.271)	(7.255)	(-2.585)	(-3.289)
Post TARP x TARP Recipient x HIGHCOINCIDENT	4.862***	0.998***	-0.011	-0.154
	(3.354)	(4.156)	(-0.575)	(-1.191)
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	0.457	0.543	0.625	0.874

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:** *Post TARP x LOWCOINCIDENT TARP Recipient = Post TARP x HIGHCOINCIDENT TARP Recipient*	3.023***	3.151***	1.382	2.408***

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	-5.979***	-1.444***	0.045**	0.186
	(-3.244)	(-4.681)	(2.105)	(0.835)
TARP Recipient x HIGHECFREEDOM	-5.297***	-1.411***	0.034*	0.134
	(-3.019)	(-4.846)	(1.659)	(0.626)
Post TARP	-0.953*	-0.195**	-0.009**	-0.922***
	(-1.956)	(-2.567)	(-2.118)	(-16.602)
Post TARP x TARP Recipient x LOWECFREEDOM	7.485***	1.441***	-0.030***	-0.364**
	(5.409)	(6.388)	(-2.605)	(-2.204)
Post TARP x TARP Recipient x HIGHECFREEDOM	5.691***	1.055***	-0.010	-0.250**
	(4.042)	(4.529)	(-0.523)	(-2.075)
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	0.454	0.542	0.625	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:** *Post TARP x LOWECFREEDOM TARP Recipient = Post TARP x HIGHECFREEDOM TARP Recipient*	0.354	2.010**	0.401	0.645