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On the Real Effects of Bank Bailouts: Micro Evidence from Japan[†]

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Exploiting the Japanese banking crisis of the 1990s as a laboratory, we investigate the effects of bank bailouts on the supply of credit and the performance of banks' clients. Our findings indicate that the size of capital injections relative to the initial financial condition of banks is crucial for the success of bank bailouts. Capital injections that are large enough to reestablish bank capital requirements increase the supply of credit and spur investment. In contrast, not only do capital injections that are too small fail to increase the supply of credit, but they also encourage the evergreening of nonperforming loans. (*JEL* E44, G21, G28, G32, G34)

Bank bailouts face stiff resistance for their fiscal implications and the long-term moral hazard costs. Yet, during a financial crisis, few governments refrain from bailing out banks. The potential benefits include guaranteeing the functioning of the payment system, systemic stability, and the flow of credit to the real economy. Governments often mention the beneficial effects on the real economy to justify their interventions. However, not only is the magnitude of these benefits widely disputed, but also the structure of bank bailouts is a subject of debate.

Theory suggests that the real effects of bailouts depend on the size of the recapitalizations, the banks' ex post ability to meet capital requirements and the quality of the banks' clients. Bagehot (1873) argues that government support for bad banks encourages even worse lending decisions. Recent theories help to qualify

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these statements. Government support is desirable only to the extent that banks have specific loan collection skills and information about their clients, who would not have access to external finance for funding profitable investment if their bank were to fail. Under these conditions, as Philippon and Schnabl (forthcoming) and Bhattacharya and Nyborg (2011) note, to be effective, recapitalizations have to be large enough to solve banks' debt overhang problems. Bank recapitalizations would be ineffective in spurring bank lending otherwise. Diamond and Rajan (2000) and Diamond (2001) point out that recapitalizations that are too small may even damage bank lending policies. In their setting, while recapitalizations that remedy bank capital inadequacy also restore incentives to sound lending policies, banks that remain undercapitalized evergreen bad loans to avoid writing them off and becoming officially insolvent. Capital injections allow undercapitalized banks to lend more to impaired borrowers. Such banks may even recall loans to their creditworthy borrowers, as new capital puts the goal of meeting capital requirements within reach. Thus, too small recapitalizations encourage banks' bad lending policies, and may even decrease the availability of loans for borrowers with valuable investment opportunities.

Empirical evidence on whether and under what circumstances bank bailouts benefit the real economy is scarce. In this paper, we exploit the Japanese banking crisis of the 1990s to evaluate the effects of government recapitalizations. Abstracting from any possible systemic effects, we evaluate whether borrowers benefit from the bailouts of their banks. Not only do we quantify the effects of government recapitalizations on firms' access to credit, stock market valuations, employment, and investment, but we also investigate the characteristics of the firms that benefit the most. These distributional issues are crucial to evaluate the effects of bank bailouts on capital allocation.

Japan represents an ideal laboratory for several reasons. First, there are some analogies between the 1990s Japanese banking crisis and the 2008 financial crisis in the United States (Hoshi and Kashyap 2010). Both crises originated from the burst of a real estate bubble, and the Japanese government intervened to recapitalize banks, much as the US administration did. Importantly, the size of the recapitalizations as well as the affected banks' financial conditions vary, enabling us to test the theoretical predictions as to the real effects of the interventions.

Second, we have publicly accessible data on all loans that Japanese listed companies received from different lenders, together with extensive financial information on banks and firms. Thus, we can ask whether nonfinancial firms more closely related to the banks receiving capital injections indirectly benefited more than otherwise similar firms. This allows us to quantify the benefits of specific interventions, abstracting from the effects of concurrent events. Even more important, we are able to isolate the effects of the bailouts on the supply of credit by focusing on firms borrowing from multiple banks and by evaluating whether, after the bailouts, the same firm received larger loans from the affected banks than the unaffected ones.

Our results show that the effects differ dramatically depending on the size of capital injections relative to the banks' initial financial conditions. If banks receive large capital injections, they extend larger loans to creditworthy borrowers with

which they have closer relationships, but reduce their exposure to low-quality firms, which we identify as the "zombie" firms in Caballero, Hoshi, and Kashyap (2008). The effects are reversed for banks that are likely to have remained undercapitalized after the interventions. These banks extend larger loans to zombie firms, but smaller ones—or none—to other borrowers. These findings indicate that poorly designed capital injections may aggravate the problems of loan evergreening and are consistent with the theory of Diamond and Rajan (2000) and Diamond (2001), who show that too small recapitalizations may encourage perverse lending policies and even reduce the supply of credit for borrowers with valuable investment opportunities.

The effects of capital injections on firm performance largely mirror our findings on the supply of credit. Large recapitalizations increase the value and investment of firms with closer relationships with the bailed out banks; however, if capital injections are too small, and banks remain undercapitalized, not only do the positive real effects fail to materialize, but there are also negative effects on capital allocation. With more credit available, zombie firms invest more. Thus, too small recapitalizations in Japan may have favored the survival of zombie firms, which as Caballero, Hoshi, and Kashyap (2008) argue, contributed to poor economic performance.

To the best of our knowledge, this paper provides the first microeconomic evidence on the real effects of bank bailouts. Existing literature analyzes the macroeconomic implications of bank bailouts through case studies, without aiming to establish causal effects (Calomiris, Klingebiel, and Laeven 2005). Notable exceptions are Ng, Vasvari, and Wittenberg Moerman (2010); Veronesi and Zingales (2010); and Bayazitova and Shivdasani (2012), who investigate the effects of the US government's capital infusions on the value of banks' financial claims. Differently from these papers, we consider the effects on banks' clients.

This paper is also related to a growing literature exploring how shocks to bank health affect the supply of credit and bank borrowers (see, for instance, Khwaja and Mian 2008; Paravisini 2008; Schnabl 2012). In particular, Slovin, Sushka, and Polonchek (1993); Bae, Kang, and Lim (2002); and Ongena, Smith, and Michalsen (2003) investigate the stock price reaction of borrowers to bank failures. A number of papers explore the effect of negative shocks to the banking system in the context of the Japanese banking crisis. These papers investigate to what extent shocks to firm collateral and bank assets affect firm investment (Gibson 1995; Kang and Stulz 2000; Gan 2007a, b) or bank lending policies (Peek and Rosengren 2005; Caballero, Hoshi, and Kashyap 2008). Existing literature concludes that negative shocks reduce the supply of credit as well as borrower valuation and investment. Our contribution is to explore the effect of different interventions to restore bank health (that is, a positive shock) on the supply of credit and bank borrowers during a systemic banking crisis. This is important as existing theories suggest that incomplete information and agency problems may create asymmetries in the transmission of shocks to bank capital (Holmstrom and Tirole 1997).

The paper is organized as follows. Section I describes the bank bailouts during the Japanese banking crisis. Sections II and III illustrate the empirical approach and the data, respectively. Section IV presents the results. Section V concludes.

I. Background

A. *Japanese Banking Crisis*

The Japanese banking crisis of the 1990s stemmed from a sharp decline in asset prices, especially land and real estate. Banks were severely hit not only because real estate was often used as collateral, but also because they held stocks and land directly, and real estate loans represented a high proportion of their balance sheets. Although export-oriented firms' growth opportunities are believed to have remained sound, bank lending contracted (Gan 2007a). As a consequence, firms cut investment (see, for instance, Kang and Stulz 2000).

Peek and Rosengren (2005) and Caballero, Hoshi, and Kashyap (2008) document that banks not only reduced the supply of loans, but also misallocated credit by funding the weakest firms. The structure of bank-firm relationships in Japan may have exacerbated this problem, because Japanese firms typically have a close relationship with their main banks, which involves bank shareholdings and board seats for bank representatives besides a lending relationship. In addition, the main bank takes a leading role in restructuring firms in financial distress (Hoshi, Kashyap and Scharfstein 1990).

While social and economic incentives may have strengthened Japanese banks' incentives to lend to severely impaired borrowers, empirical evidence suggests that Japanese banks, forbidden by law to hold equity stakes in excess of 5 percent, are interested in protecting the value of their loans and not shareholders or other stakeholders (Morck, Nakamura, and Shivdasani 2000). For instance, low current earnings and liquidity are more significant than poor stock market performance in explaining the appointment of bank directors to boards (Morck and Nakamura 1999).[1]

In addition, credit misallocation during a banking crisis is not unique to Japan. Banks renewed loans to nonperforming borrowers in the Nordic countries during the banking crisis of the early 1990s (Drees and Pazarbasioglu 1995) and in the United States during the Savings and Loan crisis (Akerlof and Romer 1993). For this reason, we believe that the Japanese experience of bank bailouts can offer insights that go beyond the Japanese economy.

B. *Capital Injections*

We believe that the Japanese experience provides a unique laboratory to explore the effects of bank bailouts for several reasons. First, government recapitalizations were directed mostly to the larger banks with no distinction made on the basis of borrower characteristics or bank specialization. Thus, there is no reason to believe that the recapitalization announcements revealed market participants

[1] Empirical evidence also shows that the fortunes of Japanese top executives are positively related to stock performance and earnings as they are in the United States (Kaplan 1994; Kaplan and Minton 1994; Kaplan and Ramseyer 1996). Anderson and Campbell (2004) show that the negative relation between performance and executive turnover was particularly high for banks during the financial crisis.

information about borrowers, beyond the fact that borrowers would benefit from the improved health of their banks. Second, the recapitalizations were heterogeneous in their size and affected banks with different ex ante financial conditions. Thus, we can test whether the effects of recapitalizations vary in line with existing theories.

Here, we describe the events that we explore in our empirical analysis. Nakaso (2001) and Hoshi and Kashyap (2010) provide a more comprehensive description of the government interventions. The government started to recapitalize banks in 1998, amid political opposition, following the failures of two securities companies and a regional bank. On February 16, 1998, the Diet approved the use of 30 trillion yen of public funds, of which 1.8 trillion yen were used for the recapitalization of 20 major banks through subordinated debt and preferred shares.

Most of the banks received a capital injection of 100 billion yen, although some of the smaller banks involved in the program received between 20 and 60 billion yen. This was, on average, 1.9 percent of bank risk-weighted assets. The main objective in the design of the recapitalizations was to avoid signaling any differences in financial health across the institutions to market participants. Thus, all systemically important banks were recapitalized, and the amount of capital injected was related only to bank size.[2]

With the small size of the capital injections, most of the banks remained undercapitalized, although there was considerable heterogeneity. Some banks reported tier 1 capital ratios above their capital requirements (8 percent for Japanese banks with international branches, and 4 percent for other banks), even before the capital injections. Other banks were severely undercapitalized. Since the size of the recapitalizations was, on average, less than 2 percent of risk-weighted assets, most of the banks with the lowest capital ratios remained undercapitalized. Even in this respect, however, there is heterogeneity, as few banks received a capital infusion of over 4 percent of their risk-weighted capital, and afterward were likely to meet capital requirements.

The first recapitalization was followed in March 1999 by a second (through preferred shares) that benefited 15 of the banks that had been recapitalized during the previous year, a confirmation that the banks affected by the first recapitalization were in different financial conditions. The amount injected was more than four times as much as in the previous injection. Each bank received between 200 and 1,000 billion yen, approximately 5.1 percent of bank risk-weighted assets. The amount of capital injected differed across banks. With its larger size, the second recapitalization produced a large reduction in the premium paid by Japanese banks in the interbank market (Peek and Rosengren 2001). Finally, a third recapitalization occurred in June 2003, when the government took Resona bank over by injecting nearly 2 trillion yen of new capital (over 17.5 percent of the bank's risk-weighted assets) through preferred and common shares. Notwithstanding their larger size, these recapitalizations hardly solved all banking problems. In fact, some Japanese banks still showed serious signs of undercapitalization in 2002 (Kashyap 2002; Hoshi and Kashyap 2010).

[2] No restrictions were imposed on bank lending policies or corporate governance.

In our analysis, we exploit heterogeneity (across and within) different rounds of government recapitalizations, and also consider private recapitalizations. Between 1998 and 2005, 64 banks made 98 equity issues to private investors. The average (median) amount of capital injected was 75 (28) billion yen. This was, on average, slightly more than 2.07 percent of bank risk-weighted assets. The equity issues were generally taken by reluctant current shareholders, who were invited to provide capital by the authorities and the banks themselves, and left banks' control structure unaltered.[3]

C. *Other Events*

Finally, a number of bank mergers occurred after 2000. Our sample includes 71 bank mergers affecting 58 banks. In a few instances, the central bank induced banks to acquire weaker banks to avoid failures (Harada and Ito 2008). Other banks merged, with the aim of becoming "too big to fail." The mergers did not improve bank capitalization (Hosono, Sakai, and Tsuru 2007). We control for bank mergers because they may have weakened relationships with the clients of target banks (this is unlikely to affect our results because the mergers mostly occurred after the first two rounds of government recapitalizations), but the analysis of their effects is beyond the scope of this paper.

II. Empirical Approach

Our objective is to test whether firms with stronger lending relationships with the recapitalized banks were able to obtain larger loans and whether the effects depend on the size of the recapitalizations relative to the banks' financial conditions. The main challenge is that the firms maintaining stronger relationships with the recapitalized banks may have different demand for credit, which would affect their response to the recapitalizations.

Luckily, we can design our main tests on the supply of credit so that the estimates are unaffected by firms' unobserved heterogeneity and can quantify the extent of selection problems, which appear to be limited. The ability to identify the effects of capital injections on the supply of credit helps us to interpret the rest of our findings.

A. *Supply of Credit*

Our first step is to examine whether the supply of bank loans increases after the bailouts. This is generally a challenging task because the events that prompt a bailout may be accompanied by changes in the demand for credit. We use the identification strategy of Khwaja and Mian (2008). Since we observe multiple bank relationships for each firm in a given year, we can evaluate the effects of bank bailouts using a within-firm estimator that compares the amount of funding provided by the affected and the unaffected banks, before and after the bailouts, to the same firm. This allows us to hold loan demand constant.

[3] Japanese banks have diffuse ownership. The top shareholders hold around 5 percent of the shares and the top 10 percent hold less than 30 percent. Their stocks are mostly held by other financial institutions and industrial companies. Financial institutions predominate among the top five shareholders.

We estimate the equation

$$(1)\quad \frac{\Delta Loan_{ikt+1}}{Loan_{ikt}} = a^L + \sum_{j\in J} b_j^L \times Intervention - j_{ikt} \times \%Loans_{ikt} + c^L \times \%Loans_{ikt} + Firm_i^L \times Year_{t+1}^L + Bank_k + u_{ikt+1}^L.$$

The dependent variable $\Delta Loan_{ikt+1}/Loan_{ikt}$ is the increase in bank loans that firm i receives from bank k during the year following the recapitalization. In all equations, our unit of analysis is the bank-firm-year, and we include interactions of firm $(Firm_i^L)$ and year $(Year_t^L)$ fixed effects. In this way, we fully absorb firm heterogeneity. In these specifications, we also include bank fixed effects $(Bank_k)$ to control for systematic differences across banks and cluster errors at the firm level. Our sample covers 1998–2004.

The variable *Intervention-*j_{ikt} is a dummy that takes a value of one if the kth bank of firm i benefits from intervention j in year t, and a value of zero otherwise. We capture the strength of the relationship of firm i with bank k using the proportion of loans that firm i received from bank k in the past $(\%Loans_{ikt})$, and include this variable as a control because the intensity of a firm's relationship with the bank may affect loan provision. We test whether firms with closer relationships benefit more from the recapitalizations by interacting the intervention dummies with the proportion of bank loans that firm i received from bank k in the year prior to the recapitalization. In other words, we allow the effects of the interventions to vary continuously with the strength of the bank relationship. A positive coefficient b_j^L indicates that firms with a higher proportion of loans from bank k in the past receive larger loans from bank k if it was affected by intervention j during the past year. Our results are invariant if we simply consider whether a firm received loans from bank k prior to the recapitalization and not the strength of the lending relationship. Using a continuous measure for the strength of bank relationships, however, gives us more cross-sectional variation because most of the firms borrowed from the banks recapitalized by the government, but the proportions of loans they received from these banks vary greatly. This helps us to identify the effects of the interventions especially in the tests described below, where the unit of observation is the firm instead of the firm-bank.

While in equation (1) we allow the impact of the different rounds of capital injections to differ, to have a more systematic understanding of the differences, we test the theoretical predictions on the size of recapitalizations and on banks' ex post ability to meet capital requirements. We surmise that any heterogeneity in the impact of the capital injections on the supply of credit depends on the size of the capital injections (relative to the banks' risk-weighted assets). Banks that benefit from larger capital injections should be better able to increase the supply of loans to their most important borrowers. We estimate the following equation:

$$(2)\quad \frac{\Delta Loan_{ikt+1}}{Loan_{ikt}} = a^{L'} + b^{L'} Injection\ Size_{kt} \times \%Loans_{ikt} + c^{L'} Injection\ Size_{kt} \times \%Loans_{ikt} \times Undercapitalized\ Bank_{kt} + d^{L'} \times \%Loans_{ikt} + Firm_i^{L'} \times Year_{t+1}^{L'} + Bank_k' + u_{ikt+1}^{L'}.$$

The interaction term $\%Loans_{ikt} \times Injection\ Size_{kt}$ measures how the size of the capital injection relative to the risk-weighted assets of bank k affects firms that are more or less related to the bank. We expect that $b^{L'} > 0$. Furthermore, we conjecture that banks that still fail to meet their capital requirements after a recapitalization are less inclined to lend. To assess the relevance of this mechanism, we interact $\%Loans_{ikt} \times Injection\ Size_{kt}$ with $Undercapitalized\ Bank_{kt}$, a dummy variable that takes a value of 1 if bank k is still undercapitalized after the capital injection, and a value of zero otherwise. We expect that $c^{L'} < 0$.

We then test whether undercapitalized banks extend larger loans to impaired borrowers, as theories would predict. As we explain in Section IIIB, we denote impaired borrowers as zombie and identify them with a dummy variable, $zombie_{it}$, which takes a value of one for impaired borrowers, and a value of zero otherwise. We estimate the following equation:

$$
\begin{aligned}
(3)\quad \frac{\Delta Loan_{ikt+1}}{Loan_{ikt}} = {} & a^{L''} + b_1^{L''} Injection\ Size_{kt} \times \%Loans_{ikt} \\
& + b_2^{L''} zombie_{it} \times Injection\ Size_{kt} \times \%Loans_{ikt} \\
& + c_1^{L''} Injection\ Size_{kt} \times \%Loans_{ikt} \times Undercapitalized\ Bank_{kt} \\
& + c_2^{L''} zombie_{it} \times Injection\ Size_{kt} \times \%Loans_{ikt} \\
& \times Undercapitalized\ Bank_{kt} + d^{L''} \times \%Loans_{ikt} \\
& + Firm_i^{L''} \times Year_{t+1}^{L''} + Bank_k'' + u_{ikt+1}^{L''}.
\end{aligned}
$$

If bank capitalization matters for the allocation of credit after the capital injections, as the theory of Diamond and Rajan (2000) and Diamond (2001) implies, we expect that banks that are able to meet capital requirements after the recapitalization increase the supply of loans to all borrowers ($b_1^{L''} > 0$), with the exception of zombie firms ($b_2^{L''} \leq -b_1^{L''}$). We expect the opposite to hold for banks that remain undercapitalized (i.e., $c_2^{L''} > 0$ and $c_1^{L''} \leq -c_2^{L''}$).

B. *Firm Valuations*

We investigate the announcement effects of bank bailouts on firm valuations using an event study. As noted in Section I, beyond the capital infusion to their lending banks, the implementation of the bailouts communicated no new information about banks' borrowers to market participants. Since firms' preannouncement stock prices reflect any differences across borrowers known to market participants, the announcement abnormal returns can only capture the (average) effect of the bailouts on firms' expected discounted cash flows. We explore how borrowers react to the announcements and whether the effects depend on the characteristics of the recapitalizations and of the borrowers in a way that is consistent with the effects on the supply of credit.

While the government recapitalizations were preceded by lengthy discussions, the names of the banks participating in the program were announced only shortly before the actual capital injections. For this reason, and to avoid contamination from other events, in most of our analysis, we use an event window starting three days before the capital injections and ending one day after the capital injections. We explore the robustness of our results to the use of alternative event windows.

For any firm i, we estimate daily expected returns using the standard market model: $R_{it} - R_{ft} = \alpha_i + \beta_i(R_{mt} - R_{ft}) + \varepsilon_{it}$, where R_{it} and R_{mt} are day t returns on firm i and on the market portfolio, respectively; R_{ft} is the return on the risk-free asset, which we measure with the return of 60-day Japanese Treasury bills; and ε_{it} is a zero-mean disturbance term. Abnormal returns of firm i on day t are computed as firm i's actual return on day t minus its expected return on day t: $AR_{i,t} \equiv \hat{\varepsilon}_{it} = R_{it} - R_{ft} - \hat{\alpha}_i - \hat{\beta}_i(R_{it} - R_{ft})$. The parameters $\hat{\alpha}_i$ and $\hat{\beta}_i$ are estimated using ordinary least squares in the window $[t-280, t-20]$, as long as we have at least 100 observations for daily returns.[4]

We estimate the following model for firm abnormal returns:

$$(4) \qquad AR_{it} = a^{AR} + \sum_{j \in J} b_j^{AR} \times \textit{Exposure Intervention-}j_i + c^{AR} X_{it-1} + u_{it}^{AR},$$

where *Exposure Intervention-*j_i is the proportion of loans that in the year preceding the intervention firm i received from any of the banks benefiting from intervention j. This variable measuring the intensity of all these relationships captures firm i's exposure to intervention j; it is allowed to differ from zero only for days within the relevant event window.[5] By construction abnormal returns are expected to be equal to zero outside the event window. A statistically significant coefficient b_j^{AR} indicates that firms that receive more loans from banks affected by intervention j experience large abnormal returns upon the announcement of intervention j. We also include a vector of firm controls, X_{it-1}, which are measured at the end of each year. We include these controls because exposure to systematic risk factors may vary with firm characteristics (such as size and market to book) in a way that systematically affects firm returns. Furthermore, news affecting firm abnormal returns may be revealed during the sample period, especially for firms with certain characteristics, such as low interest rate coverage. Our controls account for these effects, but the results are invariant if we exclude these controls.

In the same vein of the tests on the supply of credit, we consider the role of the size of the recapitalizations and of banks' ex post ability to meet capital requirements by estimating the equation:

$$(5) \quad AR_{it} = a^{AR'} + b^{AR'} \times \textit{Injection Exposure}_{it} + c^{AR'} \times \textit{Undercapitalized Injection Exposure}_{it} + d^{AR'} \times X_{it-1} + u_{it}^{AR'}.$$

[4] In unreported specifications, we estimate the market model using Scholes-Williams betas (Scholes and Williams 1977). The results are virtually identical to the ones we report.

[5] In unreported alternative specifications, we capture the effects of intervention j using a dummy variable that captures whether the firm has a relationship with any of the banks benefiting from intervention j or the proportion of loans from the main bank if this is affected by intervention j. The results are qualitatively similar to the ones we report.

In equation (5), we test whether firms experience larger abnormal returns when the recapitalized banks from which they borrow most receive larger capital injections, using the following proxy:

$$(6) \qquad \textit{Injection Exposure}_{it} \equiv \sum_{k} \%\textit{Loans}_{ikt} \times \textit{Injection Size}_{kt}.$$

We expect that $b^{AR'} > 0$. We also test whether firms experience lower abnormal returns when the capital injections are unsuccessful in reestablishing banks' capital requirements. We use the following proxy:

$$(7) \qquad \textit{Undercapitalized Injection Exposure}_{it} \equiv \sum_{k} \%\textit{Loans}_{ikt} \times \textit{Injection Size}_{kt} \times \textit{Undercapitalized Bank}_{kt}.$$

Here, we expect that $c^{AR'} < 0$. As before, these two variables are allowed to differ from zero only for the days included in the relevant event window.

Within this empirical framework, we can then investigate whether the announcement effects differ across subsamples of firms, as follows:

$$\begin{aligned}(8) \qquad AR_{it} = {} & a^{AR''} + b_1^{AR''} \times \textit{Injection Exposure}_{it} \\ & + b_2^{AR''} \times \textit{zombie}_{it} \times \textit{Injection Exposure}_{it} \\ & + c_1^{AR''} \times \textit{Undercapitalized Injection Exposure}_{it} \\ & + c_2^{AR''} \times \textit{zombie}_{it} \times \textit{Undercapitalized Injection Exposure}_{it} \\ & + d^{AR'} \times X_{it-1} + u_{it}^{AR'}.\end{aligned}$$

Also here if bank capitalization matters for the allocation of credit, we expect $b_1^{AR''} > 0$, $b_2^{AR''} < 0$, $c_1^{AR''} < 0$ and $c_2^{AR''} > 0$.

Our sample period runs from 1998 through 2004. As cross-sectional correlation of the events could inflate our *t*-statistics, we cluster standard errors across months as well as across firms, as suggested by Petersen (2009).

C. *Firms' Corporate Policies*

Finally, we investigate the effects of bank bailouts on bank clients' corporate policies (such as changes in the use of financial debt and cash holding, investment, and employment growth). Conditionally on the bailed out banks increasing the supply of credit, we should observe an increase in investment or employment only if firms are financially constrained and unable to substitute bank loans with other sources of funds to pursue their investment opportunities. Also in this case, our sample spans

from 1998 to 2004, but our unit of analysis is the firm-year. We estimate the following equation:

$$(9) \qquad y_{it+1} = a^{y} + \sum_{j \in J} b_{j}^{y} \times Exposure\ Intervention\text{-}j_{it} + c^{y} X_{it} + Firm_{i}^{y} + Year_{t+1}^{y} + u_{it+1}^{y}.$$

The dependent variable y_{it+1} is any of the firm outcomes we consider in turn. Year fixed effects ($Year_{t+1}^{y}$) control for systematic shocks affecting all firms in a given year, while firm fixed effects ($Firm_{i}^{y}$) capture systematic differences across firms. We also include a vector of time-variant firm controls, X_{it}, and in some specifications interactions of industry and year fixed effects to capture industry time-varying growth opportunities. The variables *Exposure Intervention-*j_{it} capturing the firms' exposure to intervention j are defined as in Section IIB, but now the frequency is yearly. In particular, a firm's exposure to an intervention is different from zero only in the year following the intervention. Also, here we test whether the effects of the interventions depend on the size of the capital injections and the banks' ex post ability to meet the capital requirements. We estimate the following equation:

$$(10) \qquad y_{it+1} = a^{y'} + b^{y'} \times Injection\ Exposure_{it} + c^{y'} \times Undercapitalized\ Injection\ Exposure_{it} + d^{y'} \times X_{it} + Firm_{i}^{y'} + Year_{t+1}^{y'} + u_{it+1}^{y'},$$

where the variables *Injection Exposure*$_{it}$ and *Undercapitalized Injection Exposure*$_{it}$ are defined similarly to the event study. We also test whether the effect of the exposure to capital injections differs for impaired borrowers by estimating the following equation:

$$(11) \qquad y_{it+1} = a^{y'} + b_{1}^{y''} \times Injection\ Exposure_{it} + b_{2}^{y''} \times zombie_{it} \times Injection\ Exposure_{it} + c_{1}^{y''} \times Undercapitalized\ Injection\ Exposure_{it} + c_{2}^{y''} \times zombie_{it} \times Undercapitalized\ Injection\ Exposure_{it} + d^{y''} \times X_{it} + Firm_{i}^{y''} + Year_{t+1}^{y''} + u_{it+1}^{y''}.$$

Since we include firm fixed effects, our specifications capture whether in the year following the interventions, the firms with closer relationships with the banks

benefiting from the interventions have temporarily stronger performance in the variables of interest (such as investment or employment growth).[6]

In these tests, the validity of the estimates is subject to the assumption, typical of difference-in-differences estimates, that the strength of a firm's relationships with the recapitalized banks is not related to unobserved firm characteristics affecting the changes in firm performance after the interventions. This assumption can be problematic if firms that receive different proportions of loans from the recapitalized banks are dissimilar on observable (and potentially unobservable) characteristics.

This is a common problem in studies attempting to evaluate the real effects of shocks to bank capital (see, for instance, Khwaja and Mian 2008; Paravisini 2008; Schnabl 2012). Similarly to these papers, we present empirical evidence that the strength of firms' relationships with recapitalized banks affects firm performance in the year of the recapitalizations, but is unrelated to firm performance in the years preceding and following the recapitalizations, suggesting that unobserved heterogeneity is unlikely to bias our estimates. Furthermore, using the loan regressions, in which we are able to fully absorb firm heterogeneity, we are able to quantify the extent of possible biases. The tests described below suggest that any biases are small. Finally, and perhaps most important, we can interpret the effects on firm outcomes in the light of our findings on the credit supply, which do not suffer from selection problems. The consistency of the findings indicates that the difference-in-differences estimates are reliable.

III. Data

A. *Data Sources and Sample*

Our main data source is the Nikkei NEEDS Financial dataset, which provides price, accounting, and loan information for all listed companies in Japan. Crucially for our study, NEEDS Bank Loan data allow us to observe loans outstanding to individual firms from each lender at the end of the firm's fiscal year. We also obtain bank financial statements, bank merger announcement dates, major shareholders, firms' and banks' shareholdings, and information on capital increases. From Bankscope, we extract information on tier 1 capital ratios, total capital ratios, and risk-weighted assets for all Japanese banks. Finally, we reconstruct the sequence of government interventions and obtain the list of recapitalized banks from Nasako (2001), Kashyap and Hoshi (2010), and the website of the deposit insurance corporation of Japan.[7]

The sample includes at most 3,160 nonfinancial companies and 239 banks and other lending institutions. The panel is unbalanced as the sample includes currently listed companies as well as delisted companies in the years during which they were listed. Most firms in Japan end their fiscal year in March. Approximately 20 percent of the sample firms, however, end their fiscal years in other months. To avoid timing problems, when we consider changes of variables based on firms' financial statements (i.e., financial debt, sales, cash, employment, and investment), we limit

[6] Unreported tests reveal, as seems plausible, that there are no permanent effects on these growth rates.
[7] See http://www.dic.go.jp/english/.

the sample to firms whose fiscal year ends in March. Data for loans outstanding to individual firms from each lender are also based on firms' fiscal years. Since in those specifications we compare the loans offered to the same firm by different banks, we keep the whole sample. The results, however, are invariant if we exclude firms whose fiscal year does not end in March. Furthermore, since the first and second government recapitalization occurred in the first quarter, we explore the effect of interventions in year t on investment and growth, including loan growth, between fiscal year $t-1$ and fiscal year t.[8] For clarity, in the paper, we refer to calendar years.

B. *Main Variables*

Our first challenge is to characterize the capital injections in a way that is consistent with the theories described in the introduction. Table 1 summarizes the main features of the recapitalizations and the affected banks. Unsurprisingly, banks receiving government and private capital injections had lower tier 1 capital ratios than other banks. Most importantly, financial conditions, as measured by the tier 1 capital ratio, differed markedly even for the banks that were recapitalized. Some banks started from significantly better ex ante conditions, even for the first recapitalization. Private capital injections were smaller than government capital injections, suggesting that they may have left many of the banks in which they occurred unable to meet capital requirements.

In what follows, we exploit this heterogeneity in the amount of capital injected and the banks' ability to meet capital requirements after the capital injections (both across and within different rounds of recapitalizations) to test whether these factors influence the real effects of bank bailouts. First, we measure the size of the recapitalization using the size of the capital infusion relative to the bank's risk-weighted assets. We expect larger capital infusions to lead to a higher supply of credit.

Second, we distinguish between banks that after the recapitalizations were more or less likely to meet capital requirements. Banks' ability to meet capital requirements is difficult to measure, as during banking crises banks tend to report capital ratios that most likely overstate their net wealth. This was particularly true in Japan, where banks were known to underprovide for nonperforming loans and to include deferred tax credit (which, given their dire economic conditions, they were unlikely to ever be able to exploit) in the computation of bank capital.

For this reason, we use three alternative proxies to capture the likelihood that banks were still undercapitalized after the capital infusions. Our first two measures rely on tier 1 capital only, as creative accounting is more likely to make the tier 2 capital uninformative. We conjecture that a bank was still undercapitalized after the capital infusion if the tier 1 capital ratio was at least 2 percentage points lower than its capital requirements that we obtain from Peek and Rosengren (2005). Since the capital injections occurred in the form of preferred shares or subordinated debt,

[8] Our results are invariant if in the tests concerning loans and corporate policies, where timing issues may arise, we restrict our attention to interventions that occurred in the first quarter of each year.

Table 1—Japanese Banks and Capital Injections

	Observations	Mean	Median	SD
Banks' tier 1 capital ratio	860	7.40	7.07	5.98
Government capital injections	17	5.94	6.00	1.93
Private capital injections	76	5.74	5.67	1.75
Unaffected banks	776	7.59	7.24	6.24
Capital injection size				
Government capital injections	17	4.13	2.56	4.26
Private capital injections	76	2.07	1.67	1.53
Banks still undercapitalized—Def 1 (tier 1 ratio $<$ capital requirement $-$ 2%)				
Government capital injections	17	0.24		
Private capital injections	76	0.50		
Banks still undercapitalized—Def 2 (tier 1 ratio $+$ capital injection size $<$ capital requirement)				
Government capital injections	17	0.12		
Private capital injections	76	0.51		
Banks still undercapitalized—Def 3 (total capital ratio $<$ capital requirement $+$ 2%)				
Government capital injections	17	0.12		
Private capital injections	76	0.71		
Relationships strength	151,697	0.081	0.035	0.124
Government capital injections	8,458	0.165	0.126	0.127
Private capital injections	14,281	0.098	0.055	0.120
Unaffected banks	131,925	0.075	0.030	0.123

Notes: Table 1 reports the tier 1 capital ratio from Bankscope for the recapitalized banks in the year following the recapitalization and for the unaffected banks and the affected banks in the years that are not preceded by a capital injection. It also reports the size of government and private capital injections relative to the bank's risk-weighted assets also obtained from Bankscope. We then present three alternative conditions aiming to capture whether banks are still undercapitalized after the capital injections. The first condition defines a bank as still undercapitalized after the capital injection if the tier 1 capital ratio is low enough. The second definition adds the size of the capital injection relative to the bank's risk-weighted assets to the bank's tier 1 capital ratio. A bank is defined as still undercapitalized if this quantity is below the capital requirements. The third definition classifies a bank to be undercapitalized if the total capital ratio is below the bank's capital requirements. Finally, we provide statistics for the proportion of loans that the sample firms receive from banks benefiting from different capital infusions as a proxy for the strength of lending relationships.

their amount did not enter in the tier 1 capital ratio. If the tier 1 capital ratio was low enough, given the relatively small size of the recapitalizations and the banks' under-provisioning for nonperforming loans, we can conjecture that the capital infusion left the bank below its capital requirement.

The second definition adds the size of the capital injection relative to the bank's risk-weighted assets to the bank's tier 1 capital ratio. A bank is defined as still undercapitalized if this amount is below the capital requirements. The third definition uses the total capital ratio, which includes any funds obtained in the recapitalization. To take into account that tier 2 capital often includes dubious items, such as deferred tax credit, similarly to Peek and Rosengren (2005), we define a bank as undercapitalized, if the total capital ratio was below the bank's capital requirements plus 2 percentage points.

As Table 1 shows, whether a bank is classified as able or unable to meet capital requirements is not highly sensitive to the definition we use. Furthermore, our results are similar for the different classifications. Table 1 also shows how important recapitalized banks were for bank clients, using the proportion of loans that clients receive from the recapitalized banks in the year prior to the recapitalization. Government

Table 2—Descriptive Statistics

	Observations	Mean	SD	Median	1st	99th
Panel A. Firm-bank-time specific variables						
ΔLoan (%)	151,697	5.84	42.49	0.000	−84.72	100.00
Loan increase	151,697	0.324	0.467			
%Loans *k*	151,697	0.081	0.124	0.035	0.000	0.595
First recapitalization bank *k* × %Loans *k*	151,697	0.005	0.036	0.000	0.000	0.175
Second recapitalization bank *k* × %Loans *k*	151,697	0.004	0.031	0.000	0.000	0.139
Third recapitalization bank *k* × %Loans *k*	151,697	0.001	0.013	0.000	0.000	0.000
Private recapitalization bank *k* × %Loans *k*	151,697	0.012	0.052	0.000	0.000	0.266
Recapitalization bank *k* × %Loans *k*	151,697	0.009	0.048	0.000	0.000	0.247
Undercapitalized bank—Def 1	151,697	0.080	0.272			
Undercapitalized bank—Def 2	151,697	0.004	0.063			
Undercapitalized bank—Def 3	151,697	0.097	0.296			
Bidder bank *k* × %Loans *k*	151,697	0.014	0.057	0.000	0.000	0.293
Target bank *k* × %Loans *k*	151,697	0.001	0.014	0.000	0.000	0.000
Tier1 capital ratio of bank *k*	151,697	7.249	4.849	7.250	2.310	11.270
Tier1 capital ratio of bank *k* × %Loans *k*	151,697	0.570	1.011	0.239	0.000	4.628
Firm shareholdings of bank *k*	151,697	0.000	0.001	0.000	0.000	0.004
Bank *k* shareholdings of the firm	151,697	0.012	0.027	0.000	0.000	0.012
Same Keiretsu	151,697	0.009	0.093			

(*Continued*)

recapitalizations affected banks that, on average, provided a larger share of loans to their clients and may have had greater benefits.

Table 2 lists the main variables we use in the analysis, distinguishing between variables that refer to the bank-firm relationships and firm-specific variables. The latter include the proxies for firms' exposure to the various interventions, which we measure using the proportion of loans a firm received from the affected banks. Clearly, a firm can be considered exposed to the intervention only when the intervention occurs. We also consider firms' exposure to banks that benefited from capital injections and were still likely to be undercapitalized afterward. For brevity, we present only descriptive statistics (and most of the results) based on our first definition of undercapitalized banks in Table 1.

Table 2 also describes firm performance measures and characteristics, including our proxy for low-quality firms, the zombie-firm dummy, which we define as in Caballero, Hoshi, and Kashyap (2008). We could try to capture firm quality using profitability, productivity, or dependence on bank loans, but all of these characteristics vary across industries and may just end up capturing industry effects. The zombie classification aims to identify firms that are receiving subsidized credit, using publicly available information. It is based on anecdotal evidence showing that Japanese banks granted interest rate concessions, moratoriums on loan principal or interest, and other direct interest subsidies to nonperforming borrowers in order to keep their loans on the books and avoiding writing off capital.

The zombie-firm dummy is constructed as follows. First, we compute the "required minimum interest rate expenses," a lower bound, assuming that the borrower pays the average short-term prime rate in year *t*, the average long-term prime rate in year *t* and zero coupons, respectively, on short-term bank loans, long-term bank loans, and total bonds outstanding during year *t*. We define a firm as zombie if its actual interest payments were lower than this lower bound. As Caballero, Hoshi,

TABLE 2—DESCRIPTIVE STATISTICS (*Continued*)

	Observations	Mean	SD	Median	1st	99th
Panel B. Firm-time specific variables						
Firm abnormal return (daily, %)	3,722,512	0.043	3.398	−0.037	−9.158	11.04
ΔFinDebt/TA	10,308	−0.016	1.135	−0.003	−0.896	0.774
ΔCash/TA	10,781	0.002	0.051	0.000	−0.118	0.145
Investment	10,546	0.012	0.104	0.001	−0.159	0.309
Growth of employment	9,999	−0.000	0.041	−0.004	−0.108	0.144
Size	13,580	8.684	4.021	10.218	0.000	12.37
Mkt to book	13,580	1.212	1.433	0.825	0.000	7.689
Interest rate coverage	13,457	0.524	4.456	0.130	0.000	2.500
Exposure						
First recapitalization	13,580	0.039	0.138	0.000	0.000	0.710
Exposure						
Second recapitalization	13,580	0.028	0.107	0.000	0.000	0.581
Exposure						
Third recapitalization	13,580	0.005	0.040	0.000	0.000	0.182
Exposure						
Private recapitalizations	13,580	0.097	0.178	0.000	0.000	0.734
Injection exposure	13,580	0.285	0.781	0.000	0.000	3.731
Undercapitalized injection exposure	13,580	0.080	0.515	0.000	0.000	2.052
Exposure bidder	13,580	0.127	0.213	0.000	0.000	0.876
Exposure target	13,580	0.008	0.042	0.000	0.000	0.222
Zombie firm	13,580	0.119	0.324			

Notes: Table 2 reports descriptive statistics for the main variables. ΔLoan is the percentage change in loans from bank k to firm i between t and $t+1$. Loan Increase is a dummy that takes a value of one if the loan from bank k to firm i increases between t and $t+1$ and takes a value of zero otherwise. %Loans k is the proportion of loans that firm i receives from bank k at time t. First Recapitalization Bank k, Second Recapitalization Bank k, Third Recapitalization Bank k, Private Recapitalization Bank k, Recapitalization Bank k, Bidder Bank k, and Target Bank k are dummy variables that take a value of one if in year t bank k has been affected by any of these interventions and are equal to zero otherwise. Undercapitalized Bank-Def 1, Undercapitalized Bank-Def 2 and Undercapitalized Bank-Def 3 are dummy variables that take a value of one if at t bank k is defined as undercapitalized according to each of the three definitions presented in Table 1. Tier1 Capital Ratio of Bank k is the tier 1 capital ratio of bank k at time $t-1$. Firm Shareholdings of Bank k is the fraction of shares that firm i holds in bank k at time t. Bank k Shareholdings of the Firm is the fraction of shares that bank k holds in firm i. It is winsorized at the ninety-ninth percentile. Same Keiretsu is a dummy variable that takes a value of one if firm i belongs to the same financial keiretsu of bank k and a value of zero otherwise. Firm abnormal return is the difference between the actual return of firm i on day t minus the expected return predicted using the CAPM. The CAPM regression coefficients are computed with daily data using a $(t-280, t-20)$ estimation window for each firm-day. We discard observations with fewer than 100 days to compute expected returns. ΔFinDebt/TA is the change in financial debt of firm i between year t and $t+1$ divided by the firm's total assets at time t; to improve the readability of the tables we multiply this variable by 100,000. ΔCash/TA is the change in the cash of firm i between year t and $t+1$ divided by the firm's total assets at time t. Investment is the growth rate of fixed assets of firm i between t and $t+1$. Growth of Employment is the growth rate in the number of employees of firm i between year t and $t+1$; we censor the observations of this variable at the first and the ninety-ninth percentiles. Size is the logarithm of the firm market capitalization. Mkt to Book is the ratio of firm i market capitalization and total assets at year t; we remove observations with negative book values as well as observations above the ninety-ninth percentile. Interest Rate Coverage is the interest rate coverage ratio of firm i at year t defined as earnings before interest and taxes divided by interest expense. Loans from Banks Affected by Intervention j is the proportion of loans that firm i receives from each of the banks affected by intervention j in the year prior to the recapitalization. These variables capture the firms' exposures to the interventions and are allowed to differ from zero only during the year following the interventions. Injection Exposure is defined as

$$\sum_k \textit{Loans from Bank}_{ikt} \times \textit{Injection Size}_{kt}.$$

Undercapitalized Injection Exposure is defined as

$$\sum_k \textit{Loans from Bank}_{ikt} \times \textit{Injection Size}_{kt} \times \textit{Still Undercapitalized Bank}_{kt}.$$

Zombie Firm is a dummy variable that takes a value of one if during year t the actual interest payments of firm i are lower than if the firm paid the prime rate on long-term and short-term debt and zero coupons on its bonds; the dummy variable takes a value of zero otherwise. All variables are reported at yearly frequency with the exception of Firm Abnormal Return, which is reported at daily frequency.

and Kashyap (2008) argue, this measure is conservative because the minimum interest rates are extremely advantageous to the firm, and because the actual interest payments include interest expenses on items, such as trade credit, which are not included in the computation of the required minimum interest rate expenses.

In what follows, we ask to what extent the recapitalizations of related banks increased or reduced credit for zombie firms and the extent to which the conclusion depends on the size of the capital injections and banks' ex post ability to meet capital requirements.

IV. Results

A. *Supply of Credit*

We start by exploring the effects of capital injections on the supply of credit. Estimates in panel A of Table 3 show that not all the interventions increased the availability of credit. The first two recapitalizations unambiguously increased the supply of bank loans. The effect is not only statistically significant, but also sizable as in the year following each of the first two government recapitalizations, a recapitalized bank that was extending a firm 20 percent of the loans increased credit by 10 percent (with respect to the mean loan increase). This effect is present also when we focus on the probability that a loan is increased, as Peek and Rosengren (2005) do. However, the effects of the recapitalization of Resona bank (third recapitalization) and of private recapitalizations on the supply of loans are insignificant or even negative.[9]

The difference from the first two government recapitalizations cannot be interpreted to depend on borrower heterogeneity, which is fully controlled for, or on time-invariant bank characteristics, captured by bank fixed effects. Differences may rather depend on the size of the capital injections and banks' ex post ability to meet capital requirements. Due to the persistence of debt overhang problems after private capital injections and the third recapitalization, most banks may not have found it optimal to lend, as Philippon and Schnabl (2012) suggest. Diamond and Rajan (2000) and Diamond (2001) indicate that undercapitalized banks may even reduce lending to viable borrowers. For these banks, the recapitalizations may make it more within reach to meet capital requirements. In these cases, banks may attempt to shrink their balance sheets by recalling loans to viable borrowers, even if this is inefficient from a social point of view.

In panels B and C of Table 3, we directly test the above theories considering the size of the recapitalizations and the banks' ex post ability to meet capital requirements. We surmise that borrowers with stronger lending relationships should have more access to credit when banks receive a larger capital injection. However, this effect should be smaller or even absent for banks that are likely to have remained

[9]The coefficient of % Loan *k* suggests that loans from banks that provided more loans in the past increase to a lower extent. This is contrary to the result reported by Peek and Rosengren (2005). However, their specifications include neither firm nor interactions of firm and year fixed effects and could be driven by the fact that loans to firms with a closer relationship with a single bank grow faster. Furthermore, we use a different and longer sample period. If loans from banks that provided a higher fraction of loans grew faster, all firms should have only one bank. This is clearly counterfactual.

Table 3—The Supply of Credit

	ΔLoan (1)	Loan increase (2)	ΔLoan (3)	Loan increase (4)
Panel A. Recapitalization rounds				
First recapitalization bank $k \times$ %Loans k	49.16***	0.57***	57.27***	0.63***
	(3.37)	(0.05)	(3.55)	(0.05)
Second recapitalization bank $k \times$ %Loans k	58.19***	0.66***	61.74***	0.67***
	(3.66)	(0.05)	(3.72)	(0.05)
Third recapitalization bank $k \times$ %Loans k	−2.48	−0.24**	−9.04	−0.35***
	(9.30)	(0.10)	(9.52)	(0.10)
Private recapitalization bank $k \times$ %Loans k	−12.36***	−0.06*	−12.62***	−0.07**
	(2.45)	(0.030)	(2.54)	(0.03)
%Loans k	−94.58***	−0.42***	−94.48***	−0.37***
	(2.10)	(0.02)	(2.30)	(0.02)
Bidder bank $k \times$ %Loans k	8.19***	0.097***	12.24***	0.092***
	(2.48)	(0.028)	(2.59)	(0.029)
Target bank $k \times$ %Loans k	−1.76	0.04	4.66	0.10
	(6.42)	(0.08)	(6.91)	(0.08)
Fixed effects	Firm, bank, year	Firm, bank, year	Firm × year, bank	Firm × year, bank
Observations	151,697	151,697	151,697	151,697
R^2	0.189	0.152	0.300	0.278
	ΔLoan	ΔLoan	ΔLoan	ΔLoan
Panel B. The size of capital injections				
%Loans $k \times$ government injection size	2.86***	8.74***		
	(0.59)	(0.99)		
%Loans $k \times$ government injection size* undercapitalized bank-Def 1		−8.03***		
		(1.17)		
%Loans $k \times$ injection size			1.98***	3.70***
			(0.43)	(0.62)
%Loans $k \times$ injection size undercapitalized bank-Def 1				−3.34***
				(0.85)
Private recapitalization bank $k \times$ %Loans k	−2.22	−2.69		
	(2.43)	(2.54)		
%Loans k	−97.86***	−97.32***	−97.84***	−97.86***
	(2.89)	(2.89)	(2.48)	(2.48)
Bidder bank $k \times$ %Loans k	4.97**	6.15**	1.92	3.08
	(2.45)	(2.47)	(2.30)	(2.33)
Target bank $k \times$ %Loans k	−4.37	−3.46	−10.23	−9.43
	(6.82)	(6.83)	(6.39)	(6.40)
Tier1 capital ratio of bank k	−0.20***	−0.19***	−0.16***	−0.16***
	(0.03)	(0.03)	(0.03)	(0.03)
Tier1 capital ratio of bank $k \times$ %Loans k	1.17***	1.07***	1.03***	0.97***
	(0.30)	(0.29)	(0.24)	(0.24)
Fixed effects	Firm × year, bank	Firm × year, bank	Firm × year, bank	Firm, year, bank
Observations	151,697	151,697	151,697	151,697
R^2	0.298	0.299	0.187	0.187

(*Continued*)

TABLE 3—THE SUPPLY OF CREDIT (*Continued*)

	ΔLoan (1)	Loan increase (2)	ΔLoan (3)	ΔLoan (4)
Panel C. The size of capital injections; alternative specifications				
%Loans k × injection size	4.40***	6.60***		4.88***
	(0.61)	(0.82)		(0.59)
%Loans k × injection size × undercapitalized bank-Def 1	−3.76***	−5.97***		
	(0.86)	(1.05)		
Recapitalization bank k × %Loans k			0.51***	
			(0.03)	
Recapitalization bank k × %Loans k × undercapitalized bank-Def 2			−0.17*	
			(0.09)	
%Loans k × injection size × undercapitalized bank-Def 1				−4.89***
				(0.86)
%Loans k	−97.50***	−0.35***	−105.91***	−97.12***
	(2.85)	(0.03)	(2.96)	(2.84)
Bidder bank k × %Loans k	6.38***	0.02	14.39***	6.60***
	(2.42)	(0.03)	(2.54)	(2.41)
Target bank k × %Loans k	−3.56	0.01	5.02	−3.48
	(6.82)	(0.08)	(6.91)	(6.81)
Tier1 capital ratio of bank k	−0.19***	−0.001***	−0.21***	−0.19***
	(0.03)	(0.0003)	(0.03)	(0.03)
Tier1 capital ratio of bank k × %Loans k	1.05***	0.00	1.41***	1.00***
	(0.30)	(0.00)	(0.30)	(0.29)
Fixed effects	Firm × year, bank	Firm × year, bank	Firm × year, bank	Firm × year, bank
Observations	151,697	151,697	151,697	151,697
R^2	0.298	0.277	0.300	0.298

(*Continued*)

undercapitalized. The estimates are strongly supportive of the theory and indicate that only capital injections to banks that ended up able to meet capital requirements increased the supply of credit. The estimates in column 2 imply that a firm that in the past received 8.1 percent of its loans (the sample mean) from a recapitalized bank benefiting from an average government capital injection of 4.13 percent of risk-weighted bank assets would receive 2.9 percent more loans from the recapitalized bank. The effect on the supply of credit is statistically indifferent from zero, however, if this same bank remains severely undercapitalized.

Both differences across government recapitalizations (columns 1 and 2 of panel B) and differences across government and private capital injections (columns 3 to 4 of panel B and columns 1 and 2 of panel C) can be explained by the size of the capital injections and the banks' ex post ability to meet capital requirements. While the magnitude of the coefficients capturing the effects of the capital injections drops when we lump together government and private recapitalizations, this is partially due to the increase in the standard deviation of the corresponding variable, which increases by about 20 percent. Private recapitalizations may have less of an effect also because they are perceived not to insure banks against future financial difficulties as much as government assistance. Importantly, the effects of recapitalizations

TABLE 3—THE SUPPLY OF CREDIT (*Continued*)

	ΔLoan (1)	Loan increase (2)	ΔLoan (3)	ΔLoan (4)	ΔLoan (5)
Panel D. Types of bank client					
%Loans k × injection size	4.76***	6.96***	4.36***	4.26***	3.23**
	(0.62)	(0.84)	(0.62)	(0.64)	(1.04)
%Loans k × injection size × undercapitalized bank-Def 1	−4.09***	−6.27***	−3.67***	−3.58***	−3.70**
	(0.94)	(0.11)	(0.87)	(0.90)	(1.63)
Zombie firm × %Loans k × injection size	−11.79***	−11.58***			
	(3.34)	(4.12)			
Zombie firm × %Loans k × injection size × undercapitalized bank-Def 1	11.67***	11.30**			
	(3.63)	(4.52)			
Same Keiretsu × %Loans k × injection size			0.67		
			(3.49)		
Firm shareholdings of bank k × %Loans k × injection size				4.01	
				(5.92)	
Firm shareholdings of bank k × %Loans k × injection size × undercapitalized bank-Def 1				−6.10	
				(11.65)	
Bank k shareholdings of the firm × %Loans k × injection size					−15.71
					(14.89)
Bank k shareholdings of the firm × %Loans k × injection size × undercapitalized bank-Def 1					22.04
					(21.79)
%Loans k	−97.39***	−0.35***	−98.35***	−99.12***	−132.90***
	(2.85)	(0.03)	(2.86)	(2.86)	(3.64)
Bidder bank k × %Loans k	6.31***	0.02	5.79**	6.25***	−4.38*
	(2.41)	(0.03)	(2.41)	(2.42)	(2.29)
Target bank k × %Loans k	−3.63	0.00	−3.16	−4.49	−15.52**
	(6.82)	(0.08)	(6.82)	(6.86)	(6.62)
Tier1 capital ratio of bank k	−0.19***	−0.001***	−0.19***	−0.19***	−0.23***
	(0.03)	(0.000)	(0.03)	(0.03)	(0.03)
Tier1 capital ratio of bank k × %Loans k	1.05***	0.00	1.04***	1.02***	1.70***
	(0.30)	(0.00)	(0.30)	(0.29)	(0.33)
Same Keiretsu			10.20***		
			(1.13)		
Firm shareholdings of bank k × %Loans k				76.06***	
				(11.67)	
Bank k shareholdings of the firm × %Loans k					8.33***
					(0.34)
Fixed effects	Firm × year, bank	Firm × year, bank	Firm × year, bank	Firm × year, bank	Firm × year, bank
Observations	151,697	151,697	151,697	151,697	151,697
R^2	0.298	0.277	0.299	0.299	0.306

Notes: The dependent variable is either the loan growth of firm i from bank k between t and $t+1$ or Loan Increase, a dummy variable that takes a value of one if bank k increases the amount of loans it provides to firm i between t and $t+1$ and takes a value of zero otherwise. All variables are defined in Tables 1 and 2, and the dependent variable is indicated on each column. Parameters are estimated either by using a within-firm estimator (i.e., we include firm × year fixed effects in all equations and estimate the parameters by ordinary least squares) or by using a firm fixed effects estimator. The constant is included in all regressions, but the coefficient is omitted. Standard errors presented in parentheses are corrected for heteroskedasticity and clustered at the firm level. All estimates in panel A and estimates for %Loans k, Bidder Bank k × %Loans k, Target Bank k × %Loans k, Tier1 Capital Ratio of Bank k, Tier1 Capital Ratio of Bank k × %Loans k are multiplied by 100.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

are qualitatively similar across specifications regardless of whether we include interaction of firm and year fixed effects, to completely absorb firm heterogeneity, or only firm and year fixed effects, as in column 4 of panel B. The effects are invariant also when we model the probability of a loan increase (column 2 of panel C), using a linear probability model, instead of the loan growth rate, as in the remaining equations.

So far we have defined undercapitalized banks considering only the tier 1 capital ratio, the first definition (Def 1) in Table 1.[10] In columns 3 and 4 of panel C, similar patterns emerge when we use the two alternative definitions of undercapitalized banks. In particular, in column 4 of panel C, where we define a bank as undercapitalized if its tier 1 capital ratio plus the injected capital relative to the risk-weighted assets is less than the capital requirements, we do not let the effect of the intervention depend on the size of the recapitalizations. The estimates imply that a firm that in the past received 8.1 percent of the loans from a recapitalized bank would receive 4.1 percent more loans from the recapitalized bank in the year following the recapitalization, but the effect is not statistically different from zero if this same bank is still severely undercapitalized.

Across different specifications, it emerges clearly that banks that are still likely to be undercapitalized do not increase the supply of credit. In other words, capital injections that are too small relative to the banks' ex ante financial conditions appear ineffective, as is consistent with theories of debt overhang (Philippon and Schnabl 2010; Bhattacharya and Nyborg 2011), as well as with Diamond (2001) and Diamond and Rajan (2000).

We also consider the effects of recapitalizations on capital allocation. This allows us to differentiate theories implying that large capital injections are needed to solve debt overhang problems and the model of Diamond and Rajan (2000) and Diamond (2001), which focuses on bank capital and lending policies. This model implies that capital injections may encourage loan evergreening of insolvent borrowers, when banks fail to meet capital requirements and want to avoid capital write-downs. To test whether banks allocate more or less credit to insolvent borrowers after a capital injection, we focus on zombie firms.

Columns 1 and 2 in panel D of Table 3 show that, after the capital injection, banks that meet capital requirements decrease the amount of loans they extend to zombie firms, while extending more credit to the other borrowers. The coefficients have opposite signs for undercapitalized banks, which appear to lend more only to zombie firms, most likely in order not to write off their loans and further damage their reported capitalization. Importantly, the effects are not only highly statistically significant, but also economically large. According to the estimates in column 1, a firm that in the past received 8.1 percent of the loans from a bank, benefiting from an average recapitalization of 4.13 percent of risk-weighted bank assets, obtains 1.6 percent more credit if the bank is ex post well-capitalized. If the bank remains undercapitalized, however, the effect on the supply of loans is not different from zero, because we cannot reject the null that the coefficients of $\%Loans\ k \times Injection\ Size \times Undercapitalized\ Bank\text{-}Def\ 1$ and $\%Loans_{ikt} \times Injection\ Size_{ikt}$ are equal

[10] We use dichotomous variables to capture bank capitalization because the theories predict that bank behavior varies discretely if a bank is above or below its capital requirements. The implications of the analysis would be invariant if we used a bank's tier 1 capital ratio as a (continuous) proxy for bank health.

in absolute value. More importantly, healthy banks offer 2.35 percent less credit $(=(4.755 - 11.789) \times 0.0413 \times 0.081)$ to zombie firms after the recapitalization, while banks that remain undercapitalized increase credit to zombie firms by 0.18 percent $(=(4.755 - 4.091 - 11.789 + 11.67) \times 0.0413 \times 0.081)$.

These results strongly indicate that capitalization matters for lending policies, above and beyond whatever effects it may have on debt overhang problems and support the model of Diamond and Rajan (2000) and Diamond (2001). That is, capital infusions that are large enough not only achieve the goal of increasing the supply of credit, but also make capital allocation more efficient as banks that are able to meet capital requirements refrain from lending to zombie firms. The opposite is true if the amount of capital injected is too small.

Finally, it is interesting to note that the process of bank consolidation appears to benefit clients of bidders. As is often discussed in the literature (e.g., Sapienza 2002), after bank mergers, bidders favor their own clients over the clients of the target bank.[11] Our results, however, do not depend on the fact that some recapitalizations affect the targets of bank mergers, as parameter estimates (not reported) do not change if we exclude all clients of these firms.

The estimates reported in Table 3 are qualitatively invariant when we perform a number of robustness checks. First, we compare estimates obtained with the within-firm estimator with those obtained including only firm fixed effects. The point estimates of the effects of recapitalizations are similar when we include only firm (together with year and bank) fixed effects in columns 1 and 2 of panel A and column 4 of panel B, and when we fully control for firm time-varying heterogeneity by including interactions of firm and year fixed effects (together with bank fixed effects) in columns 3 and 4 of panel A and the rest of panels B, C, and D.

The small differences that arise can give us some insights into the direction of selection biases caused by borrowers' unobserved heterogeneity in the specifications on corporate policies, where we are unable to use the within-firm estimator. For the first two government recapitalizations and for capital injections to relatively well-capitalized banks, there appears to be a (small) negative correlation between a firm's relationship to the affected banks and the error term in the loan equation, as the estimated effects of recapitalizations are slightly larger when we fully control for firm heterogeneity. This means that these banks have closer relationships with borrowers with lower demand for credit. Therefore, we expect the estimates of the effects of these recapitalizations on employment and investment in Section IVC to be slightly downward biased. The contrary is true for the third recapitalization, the private recapitalizations, and the recapitalizations of undercapitalized banks, for which we expect our estimates of the effects on corporate policies to be, if anything, upward biased. These biases make our estimates of the effects of recapitalizations on investment conservative and our findings even more remarkable.

Second, the effects of the recapitalizations obtained using the within-firm estimator represent the proportional credit supply increase from the affected banks in comparison to the other banks. This is the actual increase in the supply of credit if

[11] For all mergers in our sample, bidder and target banks remain separate entities in our dataset. We thus consider separately the loans that firms receive from the target or the bidder.

the unaffected banks do not reduce the supply in response to the interventions. If not, the estimated effect on the actual supply would be upward biased. The bias can cause the estimated coefficient to be at worse double the actual effect if the affected banks' loan increase were fully wiped out by a reduction in the loans of the unaffected banks. We find, however, that the supply of loans from the unaffected banks does not change. In specifications that we omit for brevity, we exclude loans from the affected banks and estimate equations, such as those presented in panel A of Table 3, to evaluate if after the recapitalizations the unaffected banks change the supply of credit to the clients of the affected banks. In these specifications, besides bank fixed effects, we are able to include firm and year fixed effects, but not their interactions. We find that the loans from the unaffected banks do not change after the bailouts, indicating that our estimates in Table 3 capture the actual effect of the interventions on firms' access to bank loans. We confirm this result when we explore firms' overall access to financial debt in Section IVC.[12]

Third, we explore to what extent our results may be driven by the peculiarity of bank-firm relationships in Japan, where banks and borrowers are often part of the same business group (*keiretsu*). Banks may be more inclined to support clients in the same keiretsu. In fact, it is often claimed that Japanese banks and firms in the same keiretsu sustain each other without necessarily taking into account the profitability of their actions (Aoki 1990). To explore this, we identify sample firms belonging to a keiretsu in 1998, using data from Peek and Rosengren (2005).[13] In our sample, 289 firms belong to a keiretsu. None of the keiretsu banks can be classified as undercapitalized by the first definition. If we exclude all observations related to firms belonging to a keiretsu, our results are unaffected. This indicates that the unusual structure of Japanese business groups is not driving our results.

Furthermore, we include an interaction of the variable capturing the size of the recapitalization with a dummy that takes a value of one for firms that belong to the same keiretsu as the bank, and a value of zero otherwise. In column 3 of panel D, our estimates are invariant. Interestingly, like Peek and Rosengren (2005), we find that firms belonging to the same keiretsu as the bank are granted larger loans. Keiretsu relationships, however, do not affect changes in the supply of credit after the recapitalizations.

We similarly investigate whether the fact that firms own bank shares and vice versa might drive our results. Again, in this case, we first include interactions of our main variables of interest with the firm's shareholdings in the bank. Column 4 of panel D shows the newly added interaction terms are insignificant, indicating that our results are driven by the strength of lending relationships and bank capitalizations. Results once again indicate that lending relationships are more important

[12] Arguments that the bailouts may have harmed the banks that were not bailed out are implausible in this context for several reasons. First, these arguments apply to normal times expectations of bank bailouts, which decrease the ex ante cost of credit for banks that benefit from guarantees. When bailouts occur, they may be perceived as a negative signal on the health of the banks receiving them. Second, the Japanese bailouts were announced as part of a wider program that signaled that no bank would be allowed to fail, thus implicitly benefiting all banks. Finally, we show that the supply of bank credit increases for the clients of bailed out banks.

[13] We focus on keiretsu centered around banks (financial keiretsu) and exclude those centered around industrial companies (industrial keiretsu) as only in the former it can be argued that the nature of bank-firm relationships is different in Japan (Morck and Nakamura 1999).

than stockholdings if we similarly consider the banks' shareholdings in the firms (column 5 of panel D).[14]

B. *Firm Valuations*

Announcement effects of recapitalizations on firm valuations largely mirror their effect on the supply of credit. Columns 1 and 2 of Table 4 clearly show that the first government recapitalization produced significantly positive abnormal returns for the clients of recapitalized banks, while the third one, which reduced the supply of credit, was associated with negative abnormal returns. We find no statistically significant effects for the private recapitalizations and, perhaps more surprisingly, for the second recapitalization. However, given our relatively short event window, the effects of the latter may have been anticipated by market participants. The economic effect of the first recapitalization on firm valuation is large. The cumulative abnormal returns of a firm receiving 20 percent of its loans from banks receiving a capital infusion equal to 1 percent of their assets are 75 basis points over our 5-day event window $(0.75 \times 0.2 \times 1 \times 5)$. The cumulative abnormal return is 1.9 percent for a similar firm receiving half of its loans from the recapitalized banks.

In column 3, we explore whether the size of capital injections and banks' ex post ability to meet capital requirements produce announcement effects that are consistent with the evidence on the supply of credit. It emerges clearly that the positive announcement effect increases with the size of the capital injections and that the effects are weaker for firms with stronger relationships with banks that remain undercapitalized. The difference in cumulative abnormal returns between a firm receiving all loans from a bank benefiting from an average capital injection of 4.13 percent, and a firm with no loans from recapitalized banks over the 5-day event window, is 6.5 percent $(=0.0413 \times 31.52 \times 5)$. The cumulative abnormal returns are only 3.25 percent if the firm receives only half of its loans from such a bank. Furthermore, for a firm receiving all its loans from a bank benefiting from an average recapitalization, but that remains still undercapitalized, the cumulative abnormal returns are only 0.56 percent (and statistically indifferent from 0).

Estimates in column 4 also suggest that a smaller announcement effect of large recapitalizations of banks that succeed in meeting capital requirements for zombie firms. While the effect is not statistically significant at conventional levels, we cannot reject the hypothesis that the announcement effect is zero for zombie firms (that the coefficient of *Zombie Firm* × *Injection Exposure* is equal in absolute value to the coefficient of *Injection Exposure* cannot be rejected at a confidence level of 0.8142). The opposite appears to be true if the banks remain undercapitalized. Their clients that we classify as zombie benefit the most, while the remaining borrowers do not benefit at all from recapitalizations (that the coefficient of *Undercapitalized Injection Exposure* is equal in absolute value to the coefficient of

[14] Finally, in unreported specifications, we include interaction terms to explore whether our results are driven by the firm's main bank, defined as the bank providing the highest proportion of loans. We find no consistent evidence of that.

Table 4—Firm Abnormal Returns

	[−3, +1] (1)	[−3, +1] (2)	[−3, +1] (3)	[−3, +1] (4)	[−5, +1] (5)	[−10, +1] (6)
Exposure First recapitalization	0.75***	0.75***				
	(0.13)	(0.12)				
Exposure Second recapitalization	−0.15	−0.15				
	(0.37)	(0.37)				
Exposure Third recapitalization	−0.66***	−0.66***				
	(0.05)	(0.05)				
Exposure Private recapitalizations	0.50	0.49				
	(0.38)	(0.38)				
Injection exposure			31.5***	32.4***	29.9***	23.9***
			(4.5)	(3.6)	(3.9)	(4.3)
Undercapitalized injection exposure			−28.8***	−28.7***	−23.5***	−18.2***
			(3.3)	(3.1)	(3.1)	(4.6)
Zombie firm × injection exposure				−25.2		
				(17.2)		
Zombie firm × undercap injection exposure				19.3		
				(19.2)		
Exposure bidder	−0.01	−0.01	−0.01	−0.01	−0.01	−0.02
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Exposure target	−0.08	−0.09	−0.09	−0.09	−0.09	−0.09
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Size		−0.17	−0.17	−0.17	−0.17	−0.18
		(0.18)	(0.18)	(0.18)	(0.18)	(0.18)
Mkt to book		0.13	0.13	0.13	0.13	0.13
		(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Interest rate coverage		0.09*	0.09**	0.09**	0.09**	0.09**
		(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
Zombie firm				−0.01		
				(0.02)		
Observations	3,722,512	3,722,512	3,722,512	3,722,512	3,722,512	3,722,512
R^2	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002

Notes: We explore the response of firms' daily abnormal returns to the interventions. In columns 1–4, the event window is [−3, +1] (i.e., we allow the exposure to intervention *j* captured by Loans from Banks affected by Intervention *j* to be different from zero 3 days before the event until 1 day afterwards). All variables are defined in Tables 1 and 2. Parameters are estimated by ordinary least squares. The constant is included in all regressions, but the coefficient is omitted. The coefficients of size, mkt to book, and interest rate coverage are multiplied by 100. Estimates are obtained by ordinary least squares. Standard errors presented in parentheses are corrected for heteroskedasticity and clustered at the firm and year level.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Zombie Firm × Undercapitalized Injection Exposure cannot be rejected at a confidence level of 0.9433).

We conclude by evaluating the robustness of our results to changes in the event window. As the government recapitalizations were widely discussed and their effects may have been anticipated, we extend the window to 5 and 10 days before the official announcement. The estimates in columns 5 and 6 are largely invariant, which

strongly indicates that the effects of capital injections on firm valuations depend on their size and the banks' ex post ability to meet capital requirements.

C. *Corporate Policies*

We finally explore to what extent the capital injections affect corporate policies. Column 1 in panel A of Table 5 shows that access to financial loans, as measured by the leverage, increases for firms receiving half of their loans from banks benefiting from the first or second recapitalization, while most of the sample firms decrease their leverage. This result indicates that unaffected banks do not decrease the supply of loans in response to the capital injections; it also indicates that larger bank loans do not substitute for market debt. The increase in the use of financial debt also suggests that before the recapitalizations, firms were financially constrained. In fact, we find no evidence that the increase in financial debt is accompanied by a decrease in average interest rate expenses, suggesting that the interest rate did not decrease.

When we distinguish across the rounds of recapitalizations, we find some evidence that firms use the larger loans to increase their cash holdings (although the effects are not statistically significant at conventional levels), but we find no evidence of real effects. Our empirical models gain statistical power once we exploit the heterogeneity of the capital injections with respect to banks' risk-weighted assets and banks' ex post ability to meet capital requirements. This indicates, as we noted before, that there is significant heterogeneity in the effects of recapitalizations and that a banks' financial condition after the capital injections plays an important role.

In panel B of Table 5, it is apparent that firms with closer lending relationships with banks receiving larger capital injections are able to increase their use of financial loans. Also in this case, the effect appears smaller for the clients of banks that remain undercapitalized after the capital injections. We find similar effects on cash-holdings, which are however insignificant at conventional levels.[15]

Most importantly, although we continue to find no effect on employment, in panel C, larger recapitalizations are associated with higher investment if the bank is able to meet capital requirements after the capital infusion. The effects are large. A firm with average investment receiving 50 percent of its loans from a bank benefiting from an average capital injection increases its investment by approximately 1.6 percent. The effect increases to 3.2 percent if the size of the capital injection is doubled. The effect is statistically indistinguishable from zero, however, if the firm is exposed to banks that fail to meet capital requirements after the capital injection (the hypothesis that the sum of the coefficients of *Injection Exposure* and *Undercapitalized Injection Exposure* in column 1 of panel C is zero cannot be rejected at the 37 percent level).

Differences in investment across firms are also consistent with our previous findings on the allocation of credit. Zombie firms more exposed to banks that benefited from capital injections and that remained undercapitalized appear to invest more. The contrary is true for other bank clients. In contrast, large capital injections

[15] The effects become statistically significant for either of the other two definitions of an undercapitalized bank in Table 1.

Table 5—Corporate Policies

	ΔFinDebt/TA (1)	ΔCash/TA (2)	Growth of employment (3)	Investment (4)
Panel A. Recapitalization rounds				
Exposure First recapitalization	0.11**	0.00	−0.00	0.01
	(0.05)	(0.01)	(0.01)	(0.01)
Exposure Second recapitalization	0.28***	0.01	0.00	0.02
	(0.09)	(0.01)	(0.01)	(0.02)
Exposure Third recapitalization	−0.12	−0.01	0.01	0.00
	(0.08)	(0.01)	(0.01)	(0.03)
Exposure Private recapitalizations	−0.04	−0.00	−0.01	0.00
	(0.08)	(0.01)	(0.01)	(0.01)
Exposure bidder	0.07	−0.00	−0.00	−0.02*
	(0.05)	(0.01)	(0.01)	(0.01)
Exposure target	0.29	0.01	−0.01	0.01
	(0.18)	(0.02)	(0.01)	(0.03)
Size	1.85**	−0.00	−0.03	−0.09
	(0.76)	(0.02)	(0.02)	(0.07)
Mkt to book	−2.92	0.00	−0.05	0.29
	(4.04)	(0.14)	(0.06)	(0.18)
Interest rate coverage	0.17	0.01	−0.01	0.01
	(0.13)	(0.01)	(0.03)	(0.02)
Fixed effects	Firm, year	Firm, year	Firm, year	Firm, year
Observations	10,308	10,781	9,999	10,546
R^2	0.304	0.224	0.367	0.296

	ΔFinDebt/TA (1)	ΔCash/TA (2)	Growth of employment (3)	Growth of employment (4)
Panel B. The size of capital injections				
Injection exposure	4.45***	0.16	0.02	0.03
	(1.68)	(0.10)	(0.14)	(0.15)
Undercapitalized injection exposure	−5.85**	−0.14	−0.01	0.00
	(2.44)	(0.13)	(0.18)	(0.18)
Zombie firm × injection exposure				−0.17
				(0.40)
Zombie firm × undercapitalized injection exposure				0.02
				(0.44)
Exposure bidder	0.06	0.00	−0.00	−0.00
	(0.05)	(0.00)	(0.00)	(0.00)
Exposure target	0.27	0.01	−0.01	−0.01
	(0.18)	(0.02)	(0.01)	(0.01)
Size	1.851**	−0.00	−0.03*	−0.03*
	(0.758)	(0.02)	(0.02)	(0.02)

(*Continued*)

reestablishing bank capital requirements reduce investment of zombie firms whose survival, as Caballero, Hoshi, and Kashyap (2008) argue, may dampen the productivity and growth of the economy. The effects are large. According to the estimates

Table 5—Corporate Policies (*Continued*)

	ΔFinDebt/TA (1)	ΔCash/TA (2)	Growth of employment (3)	Growth of employment (4)
Panel B. The size of capital injections				
Mkt to book	−2.938	0.00	−0.05	−0.05
	(4.060)	(0.14)	(0.06)	(0.06)
Interest rate coverage	0.1741	0.01	−0.01	−0.01
	(0.1283)	(0.01)	(0.03)	(0.03)
Zombie firm				0.20
				(0.19)
Fixed effects	Firm, year	Firm, year	Firm, year	Firm, year
Observations	10,308	10,781	9,999	9,999
R^2	0.304	0.224	0.367	0.367
	Investment (1)	Investment (2)	Investment (3)	Investment (4)
Panel C. Corporate investment and the size of capital injections				
Injection exposure	0. 78**	0.84***	1.01***	−0.59
	(0.32)	(0.32)	(0.36)	(1.65)
Undercapitalized injection exposure	−0.69*	−0.78**	−0.95**	0.48
	(0.36)	(0.37)	(0.40)	(1.68)
Zombie firm × injection exposure		−1.67**	−1.43**	
		(0.68)	(0.71)	
Zombie firm × undercapitalized injection exposure		1.85**	1.56*	
		(0.76)	(0.80)	
Exposure bidder	−0.01	−0.01	−0.01*	0.01*
	(0.01)	(0.01)	(0.01)	(0.01)
Exposure target	0.01	0.01	0.00	0.03
	(0.03)	(0.03)	(0.03)	(0.04)
Size	−0.10	−0.10	−0.09	−0.09
	(0.07)	(0.07)	(0.07)	(0.07)
Mkt to book	0.29	0.30*	0.27	0.30*
	(0.18)	(0.18)	(0.18)	(0.18)
Interest rate coverage	0.01	0.01	0.01	0.01
	(0.02)	(0.02)	(0.02)	(0.02)
Zombie firm		−0.62	−0.50	
		(0.43)	(0.42)	
Fixed effects	Firm, year	Firm, year	Firm, industry × year	Firm, year
Observations	10,546	10,546	10,546	10,546
R^2	0.297	0.298	0.307	0.296

Notes: We study the effects of interventions on the corporate policies indicated in each column (i.e., ΔFinDebt/TA, ΔCash/TA, Growth of Employment and Investment). All variables are defined in Tables 1 and 2. All regressions include the constant, and firm and year fixed effects, whose coefficients are not reported. In column 3 of panel C, we classify firms in 13 industries and also include interactions of industry and year fixed effects. In column 4 of panel C, we perform a placebo test by fictitiously applying the capital infusions two years after they occurred. Parameters are estimated by ordinary least squares. The coefficients of zombie firm, size, mkt to book and interest rate coverage are multiplied by 100. Standard errors presented in parentheses are corrected for heteroskedasticity and clustered at the firm level.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

in column 2 of panel C, nonzombie firms, with half of their loans from banks, receiving an average capital injection and no exposure to undercapitalized banks, are able to increase their investment by 1.73 percent, while firms with similar exposure to undercapitalized banks reduced their investment by 1.71 percent. Moreover, zombie firms exposed to banks that became able to meet capital requirements after the capital injection increase their investment by only 0.12 percent. The effect is somewhat larger for zombie firms with similar exposure to banks that fail to meet the capital requirements. These firms increase their investment by 0.50 percent.

Since in these specifications we are able to control for firm heterogeneity simply by including firm fixed effects, concerns arise that undercapitalized banks may have worse borrowers, which for this reason benefit less from recapitalization. Yet, our findings are fully consistent with the effects of capital injections on the supply of credit, which cannot be driven by selection problems because firm heterogeneity is fully absorbed by the interaction of firm and year fixed effects. In addition, when we quantify unobserved borrower heterogeneity by comparing the estimates obtained with the within-firm estimator and with firm fixed effects, it emerges that the effects of unobserved heterogeneity are minimal. In particular, those estimates imply an attenuation bias for the (positive) effects of capital injections when the lending banks meet capital requirements and an upward bias for the effect of recapitalization of banks that remain undercapitalized. Thus, our conclusion that capital injections have positive real effects only if banks are fully recapitalized is conservative.

To further mitigate any doubts that our results are biased by selection problems, we reestimate all models in panels B and C by including interactions of industry and year fixed effects. The point estimates of our variables of interest are invariant (for brevity, we report only the investment equation). Our estimates are also invariant if we sort firms into size quintiles and include size quintile dummies and year interactions in the above equations (estimates omitted). Any unobserved heterogeneity correlated with industry or size quintile dynamics should lead to large changes in our estimates. It is thus comforting that our results are unchanged when we compare firms within the same industry (or size quintile) in a given year.

We finally run a placebo test. We reestimate all equations in panels B and C assuming (fictitiously) that the effects of our main variables of interest, *Injection Exposure* and *Undercapitalized Injection Exposure*, occur two years later (in other words, we use two-year lags of these variables and set their values equal to zero in the first two years of the sample). For brevity, we report only the investment equation (column 4 in panel C of Table 5). In all cases, we find that our variables of interest are statistically insignificant. This confirms that the conclusions based on our estimates of the effects of capital injections are conservative and unlikely to be driven by selection problems.

D. *Aggregate Implications*

Hereafter, we consider: the aggregate effects of the first two government recapitalizations (i.e., the ones that involved more banks); and how much larger the costs and benefits of the recapitalizations would have been if no bank had been left

undercapitalized in 1998. While answering these questions is challenging, even a tentative back-of-the-envelope calculation based on our microevidence is quite revealing.

Consider a firm receiving 8.1 percent of its loans from a bank benefiting from an average capital injection in 1998 and 1999 (respectively, 1.89 percent and 5.08 percent of the bank's risk-weighted assets). The estimates in column 5 in panel B of Table 3 imply that the bank would increase credit to the firm by 0.57 percent in 1998 and 1.85 percent in 1999 if the bank met capital requirements after the recapitalization and only by 0.03 percent in 1998 and 0.08 percent in 1999 if the bank did not meet capital requirements after the recapitalization. Since the average amount of a loan both in 1998 and 1999 was approximately 4 billion yen, this translates into an actual increase in credit from the recapitalized bank to the firm of 23 (74) million yen in 1998 (1999), if the bank met the capital requirements. The actual increase in credit to the firm from a bank that remained undercapitalized after the capital injection is only JPY 1.2 million yen (3 million) in 1998 (1999). Since in 1998 (1999) we have 6,828 (6,743) relationships with banks that met capital requirements after the recapitalization and 1,388 (761) with banks that remain undercapitalized, the aggregate increase in credit is 158 billion yen (503 billion).

Clearly, these estimates considerably understate the aggregate effects on the supply of credit, especially because we observe only loans to listed companies, and many of the firms benefiting from the capital injections may have been unlisted. However, since the government injected 1.8 (2) trillion yen in 1998 (1999) in the banking system, the increase in the supply of credit to listed companies appears quite small in both instances.

The fact that some banks remained undercapitalized also had negative consequences on the allocation of credit. In the year prior to the recapitalization of 1998 (1999), "good" firms received from the recapitalized banks 0.099 (0.088) of their loans. Similarly, zombie firms received 0.075 (0.063) of their loans from the recapitalized banks. The estimates in column 1 of panel D of Table 3 imply that in 1998 (1999) the average loan increased by 0.62 percent (2.01 percent) for a good firm borrowing from an adequately capitalized bank and a mere 0.08 percent (0.32 percent) if the same firm borrowed from a still undercapitalized bank. Zombie firms borrowing from adequately capitalized banks suffered a reduction in credit of 0.93 percent (2.38 percent) in 1998 (1999), but their loans increased by 0.06 percent (0.14 percent) in 1998 (1999) if they borrowed from a still undercapitalized bank.

In 1998 (1999), the number of bank-firm relationships that were affected, distinguishing between good firms and zombie firms were, respectively, 6,560 (6,462) and 268 (281) with banks that met capital requirements after the capital injections, and 1,333 (720) and 55 (41) with banks that did not meet the capital requirements after the capital injections. Thus, the banks that met the capital requirements increased credit to good firms by 163 (520) billion yen and reduced credit to zombie firms by 10 (2.68) billion yen. The total increase in loans to good firms from banks that remained undercapitalized was only 4.27 (9.22) billion yen. In the aggregate, these banks increased loans to zombie firms by 132 (230) million yen.

Thus, capital injections had less of a positive effect on the supply of credit because many banks remained undercapitalized. While in the aggregate the amount of credit

that these banks allocated to zombie firms is relatively small, the most significant cost appears to be that the undercapitalized banks did not provide more credit to good firms. Also, the main difference does not seem to be between the first and the second recapitalizations, as Hoski and Kashyap (2010) without using microdata argue, but rather between banks. To the extent that it was larger, the second recapitalization appears to have involved even greater costs in terms of missed credit.

In what follows, we quantify how much the supply of credit to good firms would have increased if all banks met the capital requirements after the first intervention in 1998. We compute how much larger capital injections should have been to bring all the banks' capital 2 percentage points above the official capital requirement. While we estimate that achieving this would have required an additional disbursement of about 1.28 trillion yen, it is plausible to conjecture that if all banks had been well capitalized after the 1998 capital injection, no recapitalization would have been needed in 1999, saving 2 trillion yen. Thus, increasing the budget for the first round of capital injections and avoiding the second round in 1999 would have been approximately budget-neutral. It would, however, have brought significant benefits.

In 1998, the average capital injection would have increased to 2.67 percent. The amount of credit supplied to good firms would have been over 418 billion yen, that is, 2.5 times the amount of credit good firms actually received in 1998. Furthermore, banks would have decreased loans to zombie firms by 19 billion yen. This vividly illustrates the importance of the size of capital injections relative to banks' initial financial conditions for the success of bailouts.

V. Conclusions

To the best of our knowledge, this paper provides the first microevidence on the effects of bank bailouts on firm access to credit, valuation, and subsequent investment. We conclude that the size of capital injections and banks' ex post ability to meet capital requirements are crucial determinants of the effects of capital injections. Only if recapitalizations are large and banks are able to meet capital requirements, will firms have easier access to bank loans, experience positive abnormal returns, and ultimately be able to invest more. The real effects of bank bailouts are weaker if recapitalizations are smaller, and they do not materialize at all if banks remain unable to meet capital requirements. In fact, injecting capital into banks that remain unable to meet capital requirements has undesirable effects on the allocation of credit and investment, as larger loans are directed to unviable zombie firms, which as a consequence invest more.

Our results strongly suggest that too small capital injections may exacerbate the misallocation of credit and raise concerns because the size of government recapitalizations is often constrained by fiscal and political considerations. For instance, also in the United States, the balance sheets of many banks continued to be fragile after the TARP injections (Congressional Oversight Panel 2009). It is an exciting area for future research to explore whether more decisive interventions to tackle problems of capital shortage enhance the positive effects uncovered in this paper.

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