On the Real Effects of Bank Bailouts: Micro-Evidence from Japan

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Abstract. Exploiting the Japanese banking crisis as a laboratory, we investigate the effects of bank bailouts on the supply of credit and on bank clients' valuations and real performance. We find that government recapitalizations increase the supply of credit and result in positive abnormal returns, especially for bank-dependent borrowers. However, our results also highlight that during periods of high uncertainty, such as systemic banking crises, the positive effects of bank bailouts on the real economy are muted by firms' desire to accumulate cash for precautionary reasons. Furthermore, the costs of government recapitalizations are increased by the bailed out banks' propensity to extend larger loans to low and high quality firms alike.
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Bank bailouts face stiff resistance because of their fiscal implications and their long-term moral hazard costs. Nevertheless, during financial crises, few governments refrain from bailing out banks. The potential benefits from doing so include guaranteeing the functioning of the payment system, systemic stability, and the flow of credit to the real economy. The beneficial effects on the real economy are often mentioned by governments to justify their interventions: Saving Wall Street is considered a necessary step to help Main Street.

While many believe that bank bailouts benefit the real economy, others argue that the bailout of a few banks is unlikely to restore the confidence in the credit market because uncertainty makes banks reluctant to lend and induces economic agents to hold back investment (Bebchuk and Goldstein, 2008). How to structure bank bailouts is also the subject of long-standing debates. Governments can buy undervalued bank assets, directly recapitalize banks and acquire their stocks, encourage mergers of troubled banks with sounder ones, or promote issues of bank equity to private investors.[1]

Empirical evidence on whether and to what extent different mechanisms for bailing out banks benefit the real economy is scarce. In this paper, we exploit the Japanese banking crisis to evaluate this important issue. Abstracting from any possible systemic effects, we evaluate whether borrowers benefit from the bailouts of their banks. Not only we quantify the effects of different measures on firms' stock market valuations, access to credit, employment, sales, and investment, but we also investigate the characteristics of the firms that benefit 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 U.S. (Hoshi and Kashyap, 2008). Not only both crises originated from the burst of a real estate bubble, but also, in the response to the banking crisis, the Japanese government implemented different interventions that have parallels with the ones enacted by the U.S. administration: Some banks were recapitalized by the government, while others were induced to merge or to issue equity to private investors.

Second, there are publicly accessible data on all loans that Japanese listed companies receive from different lenders, together with extensive financial information on banks and firms. Thus, we can ask to what extent the clients of the banks affected by the interventions indirectly

[1] Bebchuck (2008), Blanchard (2008), and Diamond and Rajan (2009) illustrate vividly the debate surrounding the implementation of the plans for addressing the credit crisis in the U.S.

benefit. In particular, we compare the response of non-financial firms that are clients of banks affected by the interventions with that of similar firms whose banks are not affected. This allows us to quantify the benefits of specific interventions, abstracting from concurrent events and macroeconomic news whose effects may be confounded with the bailouts. Even more importantly, we are able to isolate the effects of the bailouts on the supply of credit by focusing on firms borrowing from multiple banks and evaluating to what extent, after the bailouts, the same firm receives larger loans from the affected banks in comparison to the unaffected ones.

Our results show that government recapitalizations easy access to credit for the affected banks' existing borrowers. Government capital injections increase the value of the banks' clients, especially if they are dependent on bank financing. However, firms appear to exploit their increased access to bank loans especially to increase their cash balances. This suggests that in periods of high uncertainty and financial shortage firms' propensity to save for precautionary reasons increases, as is consistent with the theoretical models of Gamba and Triantis (2008) and Riddick and Whited (2009). Possibly thanks to the higher working capital, bank-dependent firms are able to increase sales, but they appear to use the revenues to further increase cash. Only after recapitalizations that are accompanied by the perception of a significant reduction in systemic risk, bank-dependent firms invest and increase employment more than comparable firms that did not indirectly benefit from the capital injections. Investment, employment and sales of firms less dependent of bank loans are unaffected by the recapitalizations. Thus, the overall real effects of bank bailouts during systemic banking crises appear limited.

The empirical evidence also suggests that government recapitalizations allow banks to extend larger loans to low and high quality firms alike. This suggests that avoiding bank failures, and the benefits that this involves, may come at the expense of further capital misallocation. Low quality firms (i.e., real estate firms whose over-investment contributed to the financial crisis) experience higher abnormal returns than other firms upon the announcement of their banks' recapitalization. Interestingly, capital infusions by private investors appear to lead to less capital misallocation: After private recapitalizations, banks are more likely to terminate relationships with low quality firms. Moreover, real estate firms experience negative abnormal returns upon the announcement of private recapitalizations. However, private investors recapitalizations also bring no benefits in terms of an increased supply of bank loans to high quality firms as banks reduce their loans to all firms.

Finally, we show that (domestic) bank mergers, engineered to avoid bank failures, involve hidden costs as the announcement of the merger results in negative abnormal returns for the clients of the stronger bank. Consistently with the initial stock price reaction, these firms subsequently obtain smaller loans.

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 Bayazitova and Shivdasani (2009), Ng, Vasvari, and Wittenberg Moerman (2010), and Veronesi and Zingales (2010) who investigate the effects of the U.S. government's capital infusions on the value of banks' financial claims. Differently from these papers, we consider the effects on the banks' clients.

This paper is also related to a growing literature exploring how shocks to bank health affect the supply of credit and the banks' borrowers (see, for instance, Khwajan and Mian, 2008; Paravisini, 2008; Chava and Purnanandam, 2010). In particular, Slovin, Sushka and Polonchek (1993), Bae, Kang and Lim (2002) and Ongena, Smith and Michalsen (2003) investigate the stock price reaction of the 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 and 2007b) or bank lending policies (Peek and Rosengren, 2005; Caballero, Hoshi and Kashyap, 2008).[2] Existing literature mostly focuses on negative shocks to the banking system and finds that they decrease the supply of credit and affect negatively borrowers' valuation and investment.[3] Our contribution is to explore the effectiveness of different interventions to restore bank health (that is, a positive shock) on the supply of credit and banks' borrowers during a systemic banking crisis. This is a relevant exercise 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 remainder of this paper is organized as follows. Section I describes the bank bailouts during the Japanese banking crisis. Section II and III illustrate the empirical approach and the data, respectively. Section IV, V, and VI present the results. Finally, Section VII concludes.

[2] Yamori and Murakami (1999) and Brewer, Genay, Hunter, and Kaufman (2003) document borrowers' negative abnormal returns upon the announcement of Japanese banks' failures.

[3] Only Slovin, Sushka and Polonchek (1993) also explore the reaction to the announcement of a bank bailout, but their sample includes the borrowers of just one large U.S. bank and can shed no light on the effectiveness of different measures to resolve systemic banking crises.

I. The Japanese Banking Crisis and the Interventions for Bank Rehabilitation

The Japanese banking crisis of the 1990s stemmed from a sharp increase in asset prices, especially land and real estate, in the second half of the 1980s, and their subsequent decline. Banks were heavily exposed not only because they held stocks and land directly, but also because real estate loans constituted a large fraction of their balance sheets. Thus, when between 1990 and 1993 real estate prices halved, banks experienced a negative shock. Losses were not realized on bank balance sheets and the book capital over-estimated the actual capital at least until 2002. Although firms' growth opportunities are believed to have remained sound, banks contracted lending and this led to a credit crunch (Gan, 2007a). As a consequence, starting from the first half of the 1990s, firms cut investment (see, for instance, Kang and Stulz, 2000).

Peek and Rosengren (2005) and Caballero, Hoshi, and Kashyap (2009) document that not only banks 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 particularly close relationship with their main bank, which involves bank shareholdings, board seats for bank representatives as well as a lending relationship (Eser and Peek, 2006). 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 allocate credit to severely impaired borrowers, empirical evidence suggests that Japanese banks, being forbidden to hold equity stakes in excess of 5 percent by the law, 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 the board (Morck and Nakamura, 1999). [4]

In addition, credit misallocation is not peculiar to Japan. During systemic banking crises, banks are often unwilling to recognize losses and evergreen non-performing loans. For instance, Banerjee, Cole, and Duflo (2009) and Velasco (1991) document bank reluctance to settle non-performing loans in India and Chile, respectively. Nor is loan evergreening limited to developing

[4] Empirical evidence also shows that the fortunes of Japanese top executives are positively related to stock performance and earnings like in the U.S. (Kaplan, 1994; Kaplan and Minton, 1994; Kaplan and Ramseyer, 1996). Anderson and Campbell (2004) show that the negative relation between performance and turnover was particularly high for banks during the financial crisis. Thus, it appears that Japanese executives face incentives to maximize profits as those who fail risk losing their jobs.

economies. Banks renewed loans to non-performing borrowers to avoid defaults in the Nordic countries during the banking crisis of the early nineties (Drees and Pazarbasioglu, 1995) and in the U.S. 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 (we address this issue empirically in Sections V and VI).

Nakaso (2001) and Hoshi and Kashyap (2008) provide a detailed account of the unfolding of the Japanese banking crisis and of the government's response. Here, we focus on describing the measures that we exploit in the empirical tests and summarize in Table 1.

The banking problems heightened at the end of 1997 with the failures of two securities companies and a regional bank. These failures prompted the discussion about how to prevent further bank failures. On February 16, 1998, the Diet approved the use of JPY 30 trillion of public funds, of which JPY 13 trillion were dedicated to bank recapitalizations. In the following days, JPY 1.8 trillion 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, less than 1 percent of bank assets (and some 20 percent of bank capital). The main objective in the design of the recapitalization was not to signal to market participants any differences in financial health between financial institutions. Thus, all systemically important banks were recapitalized and the amount of capital injected was only subordinated to bank size.[5] Due to the small size of the capital injection, this first attempt to stabilize the banking system was considered unsuccessful at the time.

This first recapitalization was followed in March 1999 by a second recapitalization (through preferred shares) that benefited 15 of the banks that had already been recapitalized the year before. The amount injected was more than four times as large as the previous injection. Each bank received between 200 and 1,000 billion Yen, which was approximately 2 percent of bank assets (and 70 percent of bank capital). Probably for its larger size, this second recapitalization was deemed more successful in reestablishing the capital ratios of banks. Finally, the third recapitalization occurred in June 2003 when the government took Resona bank over by injecting nearly 2 trillion Yen of new capital, over 13 percent of the bank's assets, through preferred and common shares.

[5] In particular, no restrictions were imposed on bank lending policies or corporate governance.

The above three recapitalizations were important steps in Japan's response to the banking crisis. For instance, Peek and Rosengren (2001) show that the premium paid by the Japanese banks in the interbank market decreased significantly at their announcement, suggesting that the probability of bank defaults was revised downward. Thus, it makes sense to consider whether and to what extent firms benefited in the aftermath.

All three government recapitalizations were mostly directed to the largest banks. Recapitalized banks had a larger exposure to the real estate sector. However, no differences were made on the basis of borrower characteristics or lending specialization of the banks. Thus, there is no reason to believe that the recapitalization announcements revealed market participants positive (or negative) information about the borrowers, besides the fact that the borrowers may have been expected to benefit from the improved health of their lending banks.

Government recapitalizations were not the only interventions enacted to promote the stability of the banking system. Japan experienced a bank merger wave in the early 2000s. The central bank typically induced banks to acquire weaker banks in order to avoid failures without injecting any public funds (Harada and Ito, 2008). Other banks merged with the aim of becoming too big to fail, as suggested by the empirical evidence showing that less profitable and cost efficient banks were more likely to be acquirers as well as targets. Mergers were not aimed at exploiting scale economies and only slowly improved bank profitability. The goal of improving bank capitalization through mergers was not necessarily reached as consolidated banks suffered from decreasing capital ratios and increasing non-performing loans for three to four years after the merger (Hosono, Sakai, and Tsuru, 2007). Nevertheless, banks' stock price reaction to merger announcements was generally positive suggesting that the perceived probability of default decreased as larger banks were considered more likely to be bailed out.

Besides bank mergers, other private sector solutions were encouraged to increase bank capitalization. Between 1998 and 2005, 64 banks made 98 equity issues to private investors. These recapitalizations were generally performed by the existing shareholders and did not alter banks' control structure.[6] The average (median) amount of capital injected was 75 (28) billion Yen. This was on average slightly more than 1 (44) percent of bank assets (capital). Capital injections by private investors may lead to better outcomes if private investors can evaluate bank

[6] Japanese banks have diffuse ownership (with the top holders holding around 5 percent of the shares and the top 10 holders less that 30 percent). Their stocks are mostly held by other financial institutions and industrial companies. Financial institutions predominate among the top five shareholders.

balance sheets and monitor bank lending policies more effectively than the government. For these reasons, besides the fact that they do not rely on the taxpayer, capital injections by private investors are often favored in policy debates. However, during banking crises, private investors are often unable to evaluate the extent of bank losses. It is thus an empirical question whether capital infusions by private investors are more effective than the ones performed by the government.

The three government recapitalizations, together with the bank mergers and private recapitalizations, are the main focus of our analysis. For these interventions, we can compare the performance and access to credit of the clients of the banks that were affected with the clients of other banks. During the banking crisis, the government also implemented actions that affected all banks and therefore all firms in our sample. These include: a law that allowed the government to recapitalize all systemically important banks in June 2003 (Fourth recapitalization); a second law that allowed the government to recapitalize all banks without the requirement to be systemically important in June 2004 (Fifth recapitalization); the creation of two asset management companies to purchase bad loans from (any) banks in April 1999 and May 2003; and a plan formulated by the prime minister Heizo Takenaka calling for a rigorous evaluation of bank assets and an increase in transparency in 2002. In some specifications, we explore firms' reaction to these interventions as well. However, we recognize that in these cases we do not have a control sample of firms that were not affected and cannot fully control for concurrent events.

II. Empirical Approach

The identification of the effects of bank bailouts on the borrowing firms presents two main challenges: First, during banking crises, there may be changes in the macroeconomic environment, policy initiatives, and economic news, whose effects may be confounded with those of the bailouts. Second, the clients of the banks being bailed out may differ from the rest of the borrowers along some unobserved dimensions that affect their response to the bailouts. Thus, selection problems could bias the estimates.

We proceed as follows. First, to avoid the confounding effects of concurrent events, our (main) tests rely on difference-in-difference estimates comparing the performance (and access to credit) of the clients of the banks affected by the interventions with that of the clients of the unaffected banks. In this way, we control for any events affecting all firms and focus on the

effects on bank clients. This also implies that we abstract from any systemic effects on the economy.

Second, selection problems are unlikely to be a major problem in our sample because the banks' troubles originate from a drop in real estate prices that left largely unchanged the borrowers' growth opportunities (Gan 2007a and 2007b). In addition, as we explain in Section I, the bailed-out banks were not selected on the basis of their borrowers. Consistently with this observation, in Section III, we show that the clients of the banks being bailed out have similar observable characteristics to the other firms in the sample. Although selection problems are unlikely to be a major issue in our sample, most of our tests are designed in a way that the estimates would be unaffected by firms' unobserved heterogeneity. The implementation of the different tests and the identifying assumptions are discussed below.

A. Firms' valuations

We investigate the announcement effects of bank bailouts on firms' valuations using an event study. As argued in Section I, the implementation of the bailouts was such that, besides the capital infusion in their lending banks (or the merger), no new information about banks' borrowers was communicated to market participants. Thus, as firms' pre-announcement stock prices reflect any differences across borrowers known to market participants, the announcement abnormal returns can only capture the (average) effect of the bailouts. In what follows, we explore how borrowers with different characteristics react to the announcements. In addition, we explore whether the reaction of borrowers with different characteristics is consistent with changes in the supply of loans, which –as explained in Subsection II.B– we are able to isolate from differences in the demand for credit.

Since the government interventions and, to a large extent, also the bank mergers and the bank equity issues to private investors were preceded by lengthy discussions, we surmise that the market started to incorporate the news into prices already 10 days before the actual events. Furthermore, immediately after the announcement of the government recapitalizations, there was some uncertainty on when banks would have received the capital infusions, which were realized within a few days. It may also have taken some time for market participants to recognize which firms were related to a given bank. Therefore, we allow up to 5 days after the announcement for the news to be incorporated in the stock prices. While most of the analysis focuses on an event

window of [-10,+5], we also explore the robustness of our results to the use of event windows of [-10,+10], [-5,+5] and [-5,+3].

For any firm *i*, we estimate daily expected returns using the standard market model: $R_{it} - R_{ft} = \alpha_i + \beta_i \left(R_{mt} - R_{ft} \right) + \varepsilon_{it}$, where R_{it} and R_{mt} are the day *t* returns on firm *i* and the market portfolio, respectively, R_{ft} is the return on the risk free asset, which we measure with the return of 60 days 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*: $AR_{i,t} \equiv \hat{\varepsilon}_{it} = R_{it} - R_{ft} - \hat{\alpha}_i - \hat{\beta}_i \left(R_{it} - R_{ft} \right)$. 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.[7]

We define 9 event dummies, Intervention_j$_t$, one for each of the 9 interventions listed in Table 1. These event dummies take value 1 for days inside the relevant event window and zero otherwise. We regress firms' daily abnormal returns on the event dummies as follows:

$$AR_{it} = a^{AR} + \sum_{j=1}^{9} \mathrm{b}_j^{AR} * \text{Related-firm}_{ij} * \text{Intervention-j}_t + u_{it}^{AR},$$

where Related-firm$_{ij}$ is a dummy variable that takes value 1 if firm *i* is related to a bank benefiting from intervention *j* and Intervention-j$_t$ is the event dummy for the intervention *j*, which takes value 1 if *t* belongs to the event window of intervention *j*.

A statistically significant coefficient b_j^{AR} indicates that firm abnormal returns are significantly different from zero for intervention *j*. Cumulative abnormal returns are then obtained multiplying the coefficient estimate by the number of days in the event window. Within this empirical framework, we can easily investigate whether the announcement effects differ across subsamples of firms, by identifying the specific subsample with a dummy variable and interacting the dummy variable with the event dummy. A statistically significant coefficient for the interaction term indicates that abnormal returns are indeed different for the subsample of firms identified by the dummy variable.

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

Although it is customary in event studies to evaluate the effects of events without a control sample (see, for instance, MacKinlay, 1997), during financial crises, many different events may affect markets. For this reason, where possible, we use a difference-in-difference approach. In practice, we evaluate the effect of a given intervention as the average cumulative abnormal return of the firms related to the bailed out banks *minus* the average cumulative abnormal returns of the unrelated firms. Our sample period goes from 1998 to 2004. Thus, our control sample includes the firm's abnormal return outside of the event window and the abnormal returns of firms that are not affected by the event.

As the announcements of the government bailouts occur on the same calendar dates for all firms, problems arise with the cross-sectional correlation of the events, which could inflate our t-statistics. With a control sample of firms that are unaffected by the events, the cross-sectional correlation of the firms' abnormal returns could also bias our results against finding any differences between related and unrelated firms. Nevertheless, we cannot assume that abnormal returns are cross-sectionally independent. Therefore, we cluster standard errors across months as well as across firms, as suggested by Petersen (2009).

B. Firms' access to bank loans

We also examine whether the supply of bank loans increases after the bailouts. This is generally a challenging task because events that prompt the bailouts may be accompanied by changes in the demand for credit. We pursue the same identification strategy that Khwaja and Mian (2008) suggest to isolate the effect of negative liquidity shocks on bank lending. 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.

We estimate the following equation:

$$\frac{\Delta Loan_{ikt+1}}{FinDebt_{it}} = a^{L} + \sum_{j=1}^{5} \mathrm{b}_{j}^{L} * \text{Intervention-j}_{ikt} + \text{Firm}_{i}^{L} * \text{Year}_{t}^{L} + Bank_{k} + u_{ikt}^{L}.$$

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. The dummies $\text{Intervention-j}_{ikt}$ takes the value one if the *k*-th bank of firm *i* benefits from intervention *j* in year *t*. Thus, a positive coefficient b_{j}^{L} indicates that in the year of intervention *j* firms have an

abnormal increase in the loans they receive from the banks affected by intervention *j* in comparison to the remaining ones. Note that here we can identify only the effects of the five interventions for which we have subsamples of related and unrelated firms; the effect of the remaining interventions is absorbed by the year effects. 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 spans from 1998 to 2004.

C. Firms' corporate policies

Finally, we investigate the effects of the bank bailouts on the employment, the investment, and the sales of the banks' clients. Conditionally on the bailed out banks increasing the supply of credit, we should observe an increase in investment and employment only if firms are financially constrained and unable to substitute bank loans to pursue their investment opportunities. We recognize that the effects on corporate policies may be delayed and, for this reason, we allow up to a two-year lag in the response. Also in this case, our sample spans from 1998 to 2004, but our unit of analysis here is the firm-year. We estimate the following equation:

$$\Delta y_{it+2} = a^y + \sum_{j=1}^{5} \mathrm{b}_j^y * \text{Related-firm}_{ij} * \text{Intervention-j}_t + Firm_i^y + Year_t^y + u_{it}^y,$$

Year fixed effects ($Year_t^y$) control for systematic shocks affecting all firms in a given year, while firm fixed effects ($Firm_i^y$) capture systematic differences in the average growth rates and investment opportunities across firms. In some specifications, we also include interactions of industry and year fixed effects to capture industry time-varying growth opportunities. Related-firm_{ij} and Intervention-j_t are defined as in Subsection II.A, but now the frequency of Intervention-j_t is yearly. Also here we can identify only the effects of the five interventions for which we have subsamples of related and unrelated firms.

Since we include firm fixed effects, we compare the changes in the growth rates of various outcomes with firms that are not related to the banks that benefit from the interventions and with the firms related to banks that benefit from the interventions before and after these occur. Our specifications capture whether in the two years following the interventions (in unreported and qualitatively similar specifications, in the year following the interventions) the treated firms have temporarily higher growth in the variables of interest.

In these tests, the validity of the estimates is subject to the assumption, typical of difference-in-difference estimates, that shocks contemporaneous to the interventions affect treatment and control groups similarly. This assumption can be problematic if treatment and control groups are dissimilar on observables (and potentially unobservable) characteristics. If this assumption is satisfied, any significant effect of the event dummies suggests that bank bailouts affect firm performance. We provide empirical evidence supporting the identifying assumption in Section III, where we show that there exist no preexisting differences in observable characteristics between firms related and unrelated to the banks benefiting from the government's bailouts.

III. Data

A. The sample

Our main data source is the Nikkei NEEDS Financial dataset. We obtain 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 and capital reductions. In addition, we reconstruct the sequence of government interventions and obtain the list of recapitalized banks from Nasako (2001), Kashyap and Hoshi (2008), the website of the deposit insurance corporation of Japan,[8] and news searches in LexisNexis and Factiva.

Our sample includes a maximum of 3,160 non-financial companies and 239 banks and other lending institutions. The panel is unbalanced as the sample includes currently listed companies as well as companies that used to be listed but ceased to exist (together with their banks). Most of firms in Japan end the fiscal year in March. Approximately 20 percent of the sample firms, however, terminate their fiscal years in other months of the year. To avoid timing problems, when we consider changes of variables based on the firms' financial statements (i.e., financial debt, sales, cash, employment, and investment), we limit the sample to those firms with a fiscal year that ends in March, which is by far the date most commonly used by Japanese firms. Also the data for loans outstanding to individual firms from each lender are based on the firm's

[8] See http://www.dic.go.jp/english/.

fiscal year. Since in those specifications we compare the loans offered by different banks to the same firm, we keep the whole sample. Our results are, however, invariant if we exclude the firms whose fiscal year does not end in March.[9]

Since several interventions happen during the year, we explore the effect of interventions in year *t* on investment and growth between year *t-1* and *t+1*. In this way, we are sure that the benchmark year (*t-1*) is not affected by the interventions. The results are, however, similar to the ones we report if we consider growth rates between *t* and *t+2* or *t* and *t+1*, which may be more appropriate for interventions enacted during the first quarter.

In Table 1, we list the specific events we investigate and the number of firms that are related and unrelated to the banks affected by each event. A few comments are in order. We consider a firm to be related to a given bank if the firm obtains at least 5 percent of its loans from that bank. However, the magnitude and statistical significance of the results is similar if we consider as related all firms that receive a loan from a given bank.[10] For the first three recapitalizations, we have a subsample of firms that are unrelated to the recapitalized bank. In addition, mergers and capital injection by private investors occur at different dates. Table 1 lists the number of firms whose banks do not merge or issue equity to private investors during the sample period. Naturally, the number of firms that are not affected by each bank merger or equity issue is much larger. For the first two recapitalizations, however, we have many more subjects in the treatment group (firms related to the recapitalized banks) as compared to the control group. In our robustness checks, we make sure that this does not affect our results by comparing the performance of different subsamples of treated firms with the firms in the control group.

In Table 2, we describe the main variables and the salient features of the sample. Our sample includes large listed companies, which have a median number of 1,300 employees. Real estate and construction firms, the firms in the industries that are most affected by the crisis, are slightly less than 10 percent of the sample. Panel B of Table 2 presents the event dummies at yearly frequency. Firms are considered to be affected by each intervention (i.e., the dummy variable is set equal to one) in the year in which this occurs and are considered as unaffected in the remaining years. The descriptive statistics of the event dummies show that, when the time-

[9] Our results are also invariant if in the tests concerning loans and corporate policies, where timing issues may arise, we restrict our attention to interventions occured in the first quarter of each year.
[10] In this case, the number of related firms is just slightly larger.

series and cross-sectional variation are exploited, only a minority of firms are affected by the interventions.

It is also important to note that we construct three sets of event dummies for bank mergers. In Table 2, we make no distinction between the merging banks or between target and bidder. In the empirical analysis, however, we also distinguish between the weaker and the stronger bank involved in the merger. We define a bank to be stronger if it is larger (this is often the case in our sample as many of the acquired banks are quite small and do not lend to listed companies) or if it has a lower proportion of loans to the real estate sector. We proceed in this way, instead of using information on non-performing loans, because losses are generally reported on bank balance sheets with a significant delay.

While bank financing is important for all firms in Japan, firms rely to different extents on bank loans: Firms that fund less than 10 percent of their assets with bank loans and make large use of the bond market coexist with firms for which bank loans are over 50 percent of total assets. We consider firms with a ratio of bank loans to total assets above the top quartile in 1997 as highly dependent on bank loans and explore whether they are affected to a larger extent by the bank bailouts. In addition, the median firm has nine bank relationships suggesting that not all banks are equally important. However, 15 percent of the sample firms receive over half of their loans from a single bank. We consider firms that receive more than half of the loans from a single bank as highly dependent on that bank and explore whether these firms are more affected by the interventions benefiting their most important bank.

B. Selection issues

Table 3 compares the characteristics of the firms related to the banks being bailed out with those of the remaining firms, in 1997, the year before any of the interventions for bank recapitalizations.[11] It reveals that, as is consistent with the argument that bank distress was determined by the drop in the real estate prices, a relatively higher fraction of the related firms are in the real estate and construction sectors. However, the proportion of real estate firms is small enough –less that 10 percent– to allow a comparison of the effects of bank bailouts on the remaining firms. Relatively more bank-dependent firms are included in the control sample.

More importantly, we do not observe any differences in size or performance between the firms related to the banks recapitalized by the government and firms in the control sample. Only

[11] This and other time-invariant firm characteristics are absorbed by firm fixed effects.

for firms related to banks that merge or are recapitalized by private investors, there exist some ex ante differences. In particular, the borrowers of banks benefiting from private sector solutions have higher sales growth, slower accumulation of cash holdings, and debt before the sample period. While statistically significant, the differences are not necessarily economically relevant (as for sales growth it is less than 5 percent of the standard deviation). Furthermore, since differently from government recapitalizations these events happen at different times, this is less of a problem: Firms related to banks that merge or are recapitalized by private investors serve as a control for all firms that merge or are recapitalized by private investors at different dates.

Table 3 also reveals that the level of capitalization and (different categories of) non-performing loans are similar in 1997 for banks that benefit from different interventions. Ex ante differences in bank health are therefore unlikely to be a first order determinant of any differences in the effects of the measures for restoring bank health that we may find.

IV. Results

A. Announcement effects on bank valuation

In Panel A of Table 4 (column 1), we explore the effects of the interventions on banks. On average, the announcement of the government recapitalizations did not produce positive abnormal returns for the banks' shareholders.[12] This is unsurprising because the recapitalizations produce two counteracting effects: They dilute existing shareholders, but at the same time they increase the probability of survival of the bank. Consistently with this explanation, Veronesi and Zingales (2010) find that the US government's equity infusions on October 2008 benefited banks' debtholders, but not shareholders.

Only the fifth recapitalization results in positive abnormal returns. This may depend on the fact that, on this occasion, the government announced the willingness to inject capital in any bank, thus reducing uncertainty in the interbank markets. Also bank mergers do not result in positive abnormal returns thus suggesting that the market does not anticipate large improvements in efficiency. The lower probability of failure due to the large size of the bank does not seem to benefit shareholders most likely because acquisitions are often encouraged by the government to avoid the failure of relatively smaller banks. In unreported specifications, we also explore

[12] We have no price data for Resona bank in the period of its recapitalization. For this reason, when we consider the announcement effect of bank recapitalizations on (bank) stock prices, we exclude the recapitalization of Resona bank (third recapitalization).

whether there are any additional benefits for the weaker bank involved in the merger, but we do not find any evidence supporting this conjecture.

More surprisingly, since purchases often occur at above-market prices, the creation of the asset management companies purchasing bad loans from banks does not produce positive abnormal returns. This suggests that the announcement may have happened at a time of intensification of the financial crisis and indicates the importance of having a control sample for evaluating the effects of the interventions. Finally, the announcement of the Takenaka's program has no effect on bank valuations. Thus, increased transparency and rigorous evaluation of bank assets are not expected to affect bank shareholders, who, most likely, are already aware of the financial health of the banks.

B. Announcement effects on firm valuation

B.1. Government recapitalizations

Estimates in Table 4 (Panel A, columns 2 to 4) show that the announcements of the government recapitalizations produce significantly positive abnormal returns for the firms related to the recapitalized banks. The effects are large. For instance, the first recapitalization produces cumulative abnormal returns for the firms related to the recapitalized banks over the 16 days event window of $0.13 \times 16 \approx 2.08$ percent. The second and third government recapitalization appear to have an even larger impact.

Empirical evidence (e.g., Chava and Purnanandam, 2010; Khwaja and Mian, 2008) shows that adverse shocks to banks hurt bank-dependent borrowers to a larger extent. We explore whether bank-dependent firms benefit to a larger extent from the government recapitalizations. Estimates in column 6 of Table 4 show that bank-dependent firms indeed experience higher abnormal returns upon the announcement of the government recapitalizations.

Since in our dataset firms engage multiple banks, we also surmise that the interventions may affect more strongly firms that are highly dependent on a single bank when this is affected by the event (i.e., the recapitalization or the merger). We define firms that receive more than half of their loans from one bank only as highly dependent on a single bank. Estimates in column 7 confirm that firms experience larger abnormal returns if they are highly dependent on a bank benefiting from a government recapitalization. The effects are large: Firms that are highly

dependent on a single bank being recapitalized experience over 7 percent cumulative abnormal returns after all three government recapitalizations.

B.2. Private recapitalizations

A common concern with government recapitalizations is that they come with poor governance as government officials are unwilling or unable to monitor recapitalized banks. Capital injections by private investors instead could come with more monitoring and lead to sounder bank lending policies. Also, private investors could be more capable than the government in ascertaining the quality of bank portfolios. This could allow private capital to reach banks with better lending opportunities.

Our results in Table 4 suggests that capital injections performed by private investors do have different effects on bank borrowers, but that on average they do not produce positive abnormal returns. As we show in Subsection C.IV, this is likely to depend on the different effects of government and private recapitalizations on the supply of credit.

B.3. Mergers

On average, bank mergers do not appear to affect the valuations of the merging banks' clients (columns 2 and 3 in Panel A of Table 4). Differently from recapitalizations, mergers bring no new cash on the banks' balance sheets. Therefore, it is not surprising that they are not viewed as positive news for bank clients. This finding may also depend on the fact that expectations of improved financial health come about with a higher probability of termination of the bank relationship after the merger. The latter is a common concern in episodes of bank consolidations in non-crisis periods (see, for instance, Karceski, Ongena and Smith, 2005 and Sapienza, 2002).

To better evaluate the effects of bank mergers, we distinguish between the clients of the weaker and the stronger merging banks (column 4). We find that the clients of the stronger banks react negatively to the announcement of the merger. Upon the announcement, they lose over 1 percent of their stock market valuation. The abnormal returns of the clients of the weaker banks are not distinguishable from zero. This suggests that avoiding bank failures by favoring the consolidation of the banking system may be costly for the real sector as the healthier banks are expected to lend less to their clients.

B.4. Other events

In column 5, we include event dummies for the remaining events that affect all firms in the sample. Not surprisingly, given our earlier findings on the effects of government

recapitalizations, the announcements of government willingness to provide capital to systemically important banks (Fourth recapitalization) and then to any bank (Fifth recapitalization) produce positive abnormal returns.

More interestingly, also the announcement of the creation of the asset management companies produces positive abnormal returns. When the asset management companies are announced, it is still unknown which banks will benefit from the purchase of bad loans and, for this reason, we cannot distinguish the effect of the announcement between related and unrelated firms as we do for first three government recapitalizations. We conjecture that banks with large exposures to the real estate sector should be more likely to have bad loans to sell. Thus, their clients should be expected to indirectly benefit to a larger extent, because the ability to sell bad loans may increase their bank's ability to extend loans. In column 5, we show that the clients of banks with a percentage of loans to the real estate sector larger than the median indeed benefit more from the creation of asset management companies.

Finally, the Takenaka's market reform requiring banks to improve transparency on the quality of bank loans appears to have a negative effect on firm valuation, implying that it is expected to further decrease bank lending.[13]

B.5. Robustness

We perform a battery of robustness checks. First, as discussed in Subsection IV.B.4., in column 5 of Panel A of Table 4, we control for other significant events related to the resolution of the banking crises that affect all firms in the sample. Besides providing some interesting insights, this exercise allow us to perform a first test for the robustness of the estimates. While our estimates are largely invariant, we no longer find a positive announcement effect for the third recapitalization. This depends on the fact that its announcement on May 19, 2003 was preceded by the creation of the second asset management company on May 1, 2003 and followed by the announcement of the government willingness to provide capital to any systemically important bank on June 2, 2003. All firms experience positive abnormal returns in the latter two occurrences and this makes it difficult to identify any additional benefits from the recapitalization of Resona bank. Importantly, if we evaluate the cumulative abnormal returns of the firms related

[13] In unreported specifications, we also consider interactions of the high bank dependence dummies with the event dummies for the asset management companies, the Takenaka's market based reform, and distinguish between the clients of stronger and weaker banks in mergers (the latter exercise involves a triple interaction term). Since these additional interaction terms are not significant, for brevity, we do not report these specifications.

to Resona bank during the event window corresponding to the third recapitalization, as is common in event studies, we continue to find a positive announcement effect.

Second, to benchmark the statistical significance of our results against any random period, we undertake a bootstrapping exercise. We re-estimate our basic specification of the equation for firm abnormal returns considering a randomly generated event of 16 contiguous days. We perform this exercise 1000 times for 1000 different randomly generated events of 16 contiguous days. Then, we compare the t-statistics of our actual event dummies with the empirically generated distribution obtained from the significance of the random events. In column 3 of Panel A of Table 4, we report the statistical significance of the estimates compared to this empirical distribution. The results confirm the statistical significance of our findings.

Third, we consider the possibility that the interventions arrive at the end of a period of particularly weak stock market performance. In this case, the positive effects of the government recapitalization dummies could capture a reversal in stock returns or more in general the fact that the bailouts follow a period of unusual prior performance. To address this issue, in column 1 of Panel B of Table 4, we control for the firms' cumulative abnormal returns between t-55 and t-25.[14] We find no evidence of return reversals as past cumulative abnormal returns appear to be positively correlated with the firm's current abnormal returns. More importantly, the impact of the government recapitalizations, the private recapitalizations, and the bank mergers is invariant.

Fourth, for the government recapitalizations, our control sample is relatively smaller than the treated group of related firms raising concerns that this may drive our estimates. We address this issue as follows. In column 2, we include only related firms whose most important bank receives a capital infusion from the government. Observations for all other related firms are excluded during the dates of the events. In this way, the treatment group is significantly reduced and includes 972; 539; and 373 firms for the first, second and third government recapitalizations, respectively. Nevertheless, our estimates remain qualitatively invariant. In a different specification that we do not report for brevity, we keep in the sample only 10 percent randomly selected firms related to banks benefiting from the first recapitalization. This shrinks the sample of treated firms to the point that its size is slightly smaller that the control group available for the first recapitalization. In this subsample, the percentage of treated firms for the second

[14] In other specifications, we explore other windows for previous cumulative abnormal returns. The results are similar to the ones we report here.

recapitalization is 40 percent. Our estimates are invariant, both qualitatively and quantitatively, also for this random subsample.

Fifth, in column 3, we explore whether our results are driven by the peculiarity of bank-firm relationships in Japan. Banks and firms are often part of the same business group (keiretsu). Banks may be more inclined to support their clients if these belong to the same keiretsu. In fact, it is often claimed that in Japan, banks and firms within the same keiretsu sustain each other without necessarily taking into account the profitability of their actions (Aoki, 1990).To check this, we identify the sample firms belonging to keiretsu in 1998 using data from Peek and Rosengren (2005).[15] In our sample, 289 firms belong to a keiretsu. If we exclude all observations related to these firms, our results are unaffected.

Finally, we explore the robustness of our results to the use of different event windows (columns 4, 5, and 6). Once again, our estimates appear robust to the use of longer (-10,+10) and shorter ((-5,+5) and (-5,+3)) event windows.

C. Access to loans

The empirical evidence in Subsection IV.B consistently suggests that government interventions have a positive effect on the valuation of related firms, while capital injections by private investors are ineffective and bank mergers hurt at least some bank clients. A mechanism consistent with these findings is that government recapitalizations increase the supply of credit, while bank mergers reduce it, at least for the borrowers of the stronger bank.

Since we have information on bank loans, we can shed light on this mechanism. Importantly, as mentioned in Section III, we are able to identify the effects of the interventions on the loan supply. In fact, we have data on the loans that firms obtain from multiple banks at a yearly frequency. We include interactions of firm and year fixed effects to fully absorb firm heterogeneity. Our estimates benchmark changes in the loans that a firm receives from the affected banks to the ones that the same firm obtains from its unaffected banks. This allows us to hold constant firms' loan demand.

The inclusion of year fixed effects forces us to focus on the first three recapitalizations, private recapitalizations, and bank mergers (for which we observe both related and unrelated

[15] We focus on keiretsu centered around banks (financial keiretsu) and exclude keiretsu 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).

firms). For the remaining events affecting all sample firms, we would not be able to distinguish the impact of the government interventions from time effects (However, we include an interaction of the dummy for the creation of asset management companies with the exposure to the real estate sector of the related banks).

Estimates in Table 5 show that not all the interventions tend to increase the availability of credit. The first two recapitalizations unambiguously increase the supply of bank loans of both short- and long-term maturity. The effect is not only statistically significant but also sizable as in the year following the recapitalizations the growth rate of loans obtained from an affected bank is more than 3 times larger than the mean loan growth rate. The effect of the recapitalization and government takeover of Resona bank (Third recapitalization) on the supply of loan is either negative or statistically insignificant. This is consistent with the lack of statistical significance in some of the previous results on firm abnormal returns. As before, it is likely to depend on the fact that during the same year all banks benefited from a reduction in uncertainty due to the government's commitment to inject capital in systemically important banks and to purchase bad loans. These interventions may have benefited all banks and contributed to increase loan supply to a larger extent than the recapitalization of Resona bank. It is important to note, however, that, as we show in Section V, the third recapitalization eases access to credit on the extensive margin, as the relationships with existing borrowers are less likely to be interrupted afterwards. This effect alone is sufficient to explain the positive impact on the stock prices of related firms.

Private recapitalizations decrease the supply of bank loans although they are also followed by a decrease in the probability of terminations of the relationships with existing borrowers. Importantly, the difference from the first two government recapitalizations cannot be interpreted to depend on borrower heterogeneity –which is fully controlled for– or on banks' time invariant effects as these are captured by bank fixed effects. It is possible that stronger involvement of private investors, as well as the change of control implied by the privatization of Resona bank, leads to more restrictive bank lending policies. Perhaps more plausibly, in these recapitalizations the investors were more successful in increasing the bank's bargaining power with existing borrowers. As the model of Diamond and Rajan (2000) suggests, if capital increases to the point that banks remain solvent in case of their borrowers' default, banks may successfully obtain some partial repayments, which are not necessarily efficient from a social point of view. Probably, bank bargaining power was affected differently by the third recapitalization and the private

recapitalizations: the third recapitalization was much larger than the previous ones and private investors may have more flexibility and acumen in deciding how much capital to inject

The clients of the stronger banks receive smaller loans after mergers; on the contrary the supply of bank loans increases for the clients of the weaker banks. These effects are fully consistent with the results of the event study and confirm our conjecture that mergers, not bringing new cash to the merged bank, cannot benefit all borrowers. Finally, we note that the clients of banks with higher exposure to the real estate sector are offered larger loans after the creation of asset management companies purchasing bad loans.

It is important to note that the point estimates of the effects of recapitalizations are similar when we only include firm and year fixed effects (column 1) and when we fully control for firm time-varying heterogeneity by including interactions of firm and year fixed effects (column 2). The small differences that arise can give us some insights on the direction of selection biases determined by borrowers' unobserved heterogeneity in the specifications in which we are unable to fully absorb it. For the first two government recapitalizations, private recapitalizations, and bank mergers, there appear to be a (small) negative correlation between being a firm related to the banks affected by the interventions and the error term in the loan equation, as the estimates of the event dummies are larger when we fully control for firm heterogeneity in column 2 (the contrary is true for the third recapitalization). Unsurprisingly, the interventions appear to affect weaker banks lending to weaker firms. These firms should have more difficulties in switching banks and accessing alternative sources of external finance.[16] It is therefore remarkable that, as we find in the next subsection, they appear to use loans to exploit investment and employment to a limited extent.

The magnitude of the event dummies coefficients obtained using the within-firm estimator represents 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 as long as the unaffected banks do not decrease 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 of the actual effect in the, probably unlikely, case in which the affected banks' loan increase is fully wiped out by the loans' decrease of the unaffected banks. We can provide evidence, however, that the supply of loans from the unaffected banks does not change. In a

[16] For the clients of banks benefiting from private recapitalizations and bank mergers, the actual bias appears to be opposite to what firm characteristics, such as higher growth of sales, in Section III suggest.

specification that we omit for brevity, we exclude the loans from the affected banks and evaluate if after the events 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 5 capture the actual effect of the interventions on the firm's access to bank loans. We confirm this result exploring firms' overall access to financial debt in Subsection IV.D.

D. Firm employment and investment

Here, we explore whether the different interventions promote firm growth and affect firm policies. Importantly, our results in column 1 and 2 of Table 6 show that larger (smaller) loans from the banks affected by the first two government recapitalizations (private recapitalizations) imply greater (lower) access to financial loans for the related firms. On average, the financial debt of firms related to banks recapitalized by the government grows by 200 percent (at the mean) faster than for other firms. The magnitude of the increase is consistent with the results from the within-firm estimation. This result confirms that larger loans from the banks benefiting from the capital injections are not substituted by lower credit from the other banks; 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 was accompanied by a decrease in the average interest rate expenses, suggesting that the interest rate does not decrease. Under these conditions, and holding constant growth opportunities, unconstrained borrowers should not respond to recapitalizations by increasing the use of financial debt.

Firms appear to use the larger loans to accumulate cash on their balance sheets. From an economic point of view the effect is extremely large as the cash growth rate of the firms related to the banks benefiting from government capital infusions in the two years following the interventions is nearly three times larger than the mean cash growth rate. These results suggest that in periods of high uncertainty and financial shortage, firms increase cash for precautionary reasons, as is consistent with theories of precautionary cash stocks (Gamba and Triantis, 2008; Riddick and Whited, 2009). Not surprisingly, the effect is larger for bank-dependent firms, that is, for the firms for which the uncertainty related to bank health is more relevant. These effects of

bailouts are also consistent with the findings of Ivashina and Scharfstein (2010) and Campello, Graham and Harvey (2009) that, during the global credit crisis of 2008, firms have been drawing down credit lines in order to build cash reserves and insulate themselves from credit supply shocks.

This empirical evidence does not imply that firms have no growth opportunities. Far from it, firms anticipating financing constraints in the future may respond to these potential constraints by hoarding cash. Holding cash is costly if higher cash savings require reductions in current, valuable, investment. Nonetheless, as Almeida, Campello, and Weisbach (2004) show, constrained firms optimally choose to hoard cash to balance the profitability of current and future investments.

We explore the effects of the interventions on firm performance and corporate policies other than cash. We find limited effects on firm performance that here we proxy using the growth of sales. Only for bank-dependent firms the sales appear to grow faster during the two years after the first two government recapitalizations, suggesting that they benefit from using cash as working capital. The increased revenues from sales may then allow these firms to save even more cash.

There is limited evidence that the clients of recapitalized banks increase employment or investment. Only during the two years after the second recapitalization, related firms with higher bank dependence increase investment by almost 150 percent at its mean. Similarly, the growth rate of employment increases by over 3 times. These results suggest that the real effects of bank bailouts are limited to bank-dependent firms. The real effects may be large if the interventions are successful in reducing the uncertainty on bank health. This appears to have been the case after the second government recapitalization in Japan, because the premium paid by Japanese banks in the interbank market disappeared (Peek and Rosengren, 2001).

Interestingly, the interventions that determine a decrease in the supply of bank loans such as private recapitalizations do not result in a decrease in cash, confirming that during financial crises cash hoarding may lead to important asymmetries in the response to shocks. Instead, firms appear to decrease investment. More surprisingly, the effect seems to be concentrated in firms that are less bank-dependent. Also, after bank mergers, as is consistent with the finding that the supply of credit decreases for the clients of the stronger merging bank, firms decrease investment.

We cannot find statistically significant differences between the clients of weak and strong banks (results omitted).

A possible concern is that firm selection biases our results against finding positive real effects. The firms borrowing from the related banks may not only have difficulties in accessing bank loans, as we suggest comparing the ordinary least square estimates and the ones obtained from the within-firm estimator, but they may also experience negative productivity shocks. Thus, weak growth opportunities for the related firms or other sources of unobserved time-varying firm heterogeneity could explain why the interventions appear to leave investment and employment policies largely unaffected. Such an interpretation is not consistent with firms' high propensity to hold cash, which is often interpreted as an empirically useful measure of financial constraints (Almeida, Campello, and Weisbach, 2004). It is also well-known that cash is particularly valuable for firms with growth opportunities and risky cash flows (Opler, Pinkowitz, Stulz, and Williamson, 1999; Faulkender and Wang, 2006), as firms' cash flows certainly are during a financial crisis.

Nonetheless, we address the above concerns in two ways. First, firms in the same industry are likely to have similar growth opportunities. Therefore, we reestimate all the equations in columns 3 to 10 of Table 6 by including interactions of 34 industry dummies with year dummies. The estimates exhibit the same statistical significance and magnitude of the ones we report. Also including time-varying firm level controls for firm size, financial leverage, and profitability leaves the results invariant. If unobservable characteristics drove our results, one would expect that increasing the set of controls would have a large impact on our estimates. The fact that it does not suggests that firm time-varying heterogeneity is unlikely to bias our results.

Second, we check whether the interventions increase investment and employment to a larger extent in subsamples of supposedly higher quality firms, such as firms with returns on assets above the median or firms with growing exports. The results are qualitatively equivalent to the ones we report in Table 6 and are therefore omitted. The fact that our results are qualitatively and quantitatively invariant for the subsample of exporting firms –whose demand and investment opportunities depend on the international environment– also suggests that our estimates of the effects of recapitalizations are unlikely to depend on the low domestic demand, low interest rates, and other peculiar features of the Japanese macroeconomic situation during the sample period.

Overall, the high propensity of firms to hold cash in periods of high uncertainty can explain the limited real effects of the capital infusions. Thus, it appears that economic uncertainty constraints investment and employment and that restoring bank capital may be a necessary, but not a sufficient condition to stimulate the real economy.

V. Effects on Capital Allocation

So far, we have shown that the government recapitalizations are often successful in increasing the supply of bank loans, but have limited effects on employment and investment because firms prefer to accumulate cash. Another, possibly complementary reason, for the limited real effects is that the recapitalized banks fail to funnel credit to financially constrained firms with growth opportunities.

The way in which loans are allocated is at least as important as the quantity of bank credit in order to determine to which extent recapitalizations are desirable. Distressed banks are likely to have both viable and unviable borrowers. Government recapitalizations are desirable only to the extent that more credit reaches viable borrowers that are unable to substitute their lending banks due to problems of asymmetric information.

Existing theories, however, suggest that banks with low capitalizations not necessarily have the incentives to extend (larger) loans to the most profitable borrowers. Diamond and Rajan (2000) show that banks may prefer to lend to poorly performing borrowers rather than to more profitable firms. In addition, banks that risk insolvency because they are close to their capital requirements have an incentive to renew loans to insolvent borrowers to avoid writing down their book capital.

These problems may have been particularly severe in Japan. Peek and Rosengren (2005) and Caballero, Hoshi, and Kashyap (2008) show that, during the banking crisis, Japanese banks, on average, allocated credit to the weakest firms to avoid the realization of losses on their own balance sheets and to comply with capital requirements. None of these papers studies the effects of bank bailouts on bank lending policies.

To answer these questions we ask which types of firms the market expect to benefit most from the interventions and whether after the interventions, banks offer more credit to certain types of borrowers.

Since the Japanese banking crisis was caused by over-investment in the real estate sector, to explore this issue, we surmise that firms in the real estate and construction sectors (henceforth, real estate firms) did not have growth opportunities during the banking crisis. This is certainly the case as real estate prices never recovered and continued to decrease in Japan after the burst of the bubble.[17] If real estate firms benefited more than manufacturing firms from bank recapitalizations, this would suggest that avoiding bank failures and the gains that this involves for viable firms may come at the expense of further capital misallocation. In addition, similarly to Peek and Rosengren (2005), we consider firms with profitability below the median as less efficient and we ask whether these less efficient firms benefit more than other firms from the interventions for bank rehabilitation.

Panel A of Table 7 suggests that the valuations of troubled real estate firms as well as those of firms with low profitability benefit more than average from the first two government recapitalizations.[18] The effect is not only statistically significant, but also economically large. When we include the interaction firms, it appears that only low quality firms experience positive cumulative abnormal returns at the announcement of the recapitalizations. The effect is very large: At the announcement of the first recapitalization, real estate firms experience cumulative abnormal return of over 9 percent. Interestingly, cumulative abnormal returns are even negative for the average (non-low quality) firm in column 6, where we identify low quality firms as firms with return on assets below the median (In specifications that we do not report for brevity, we find that the effect is negative only for high-quality firms that are not bank-dependent). This apparently counterintuitive finding is consistent with the theory of Diamond and Rajan (2000) and Diamond (2001). The recapitalization may increase the bargaining power of the banks with viable borrowers: More profitable borrowers may be forced to make larger payments to the bank because after the recapitalization their foreclosure would no longer cause the bank to default. Expectations of a possible decrease in access to bank loans may produce negative abnormal returns for profitable firms. More in general, high quality firms may expect to be able to substitute ailing banks with other sources of external or even internal funds. It is therefore not

[17] Contrary to the U.S. administration, the Japanese government never aimed to sustain real estate prices during the financial crisis.

[18] We do not include interactions with the asset management companies and the Takenaka's market based program dummies because these events are not the main focus of our analysis and these interaction terms are not statistically significant.

surprising that the anticipated effects of bank bailouts are smaller for them and even negative if they expect increased competition from low quality firms.[19]

From the point of view of the allocation of bank credit, we find little evidence that banks select borrowers depending on their type. After the first two government recapitalizations, all firms receive larger loans irrespectively of their type. After the third government recapitalization, the largest and only one to be followed by nationalization, loan supply decreases. The larger recapitalization, together with the change in ownership following the government takeover, may have stopped the process of evergreening of bank loans. While bank mergers result in an increase in the supply of bank loans, private recapitalizations appear to decrease the credit supply. In columns 6 and 8, it even appears that after the private capital injections, bank loans to low profitability firms decrease to a lower extent. Thus, at best banks do not distinguish between low and high quality firms and either they supply larger loans across the board or they reduce the supply of bank loans for all firms. At worse, they seem to favor low quality firms.

A possible concern with our interpretation of the results is that credit misallocation may depend on the organizational structure typical of Japanese business groups and would not be informative about the real effects of bank bailouts in other institutional contexts. However, our results are once again invariant if we exclude firms belonging to keiretsu, indicating that the peculiar structure of Japanese business groups is not driving our results. Consequently, Japanese banks' reactions to government interventions should be informative about the possible effects of bank bailouts elsewhere.

In Panel B of Table 7, we focus on the effects on presumably high quality firms. We conjecture that manufacturing firms with a large proportion of sales exported to foreign countries have stronger growth opportunities and experience more severe financing constraints during the credit crunch. The reasons are twofold. First, while domestic demand in Japan was low, export markets had high demand for Japanese products during the sample period (Madsen, 2004). Second, Amiti and Weinstein (2009) show that in Japan export-oriented firms suffered from negative shocks to bank health more than other firms, probably because international trade requires higher working capital than domestic sales. To fund investment opportunities, the loans

[19] The negative effects of bank bailouts on the valuation of firms with profitability above the median in columns 5 and 7 of Panel A of Table 7 are consistent with the finding of Caballero, Hoshi, and Kashyap (2008) that congestion created by the continued operations of insolvent firms affects negatively healthy firms. In this respect, our results suggest that not only capital injections may contribute to perpetrate credit misallocation, but through this channel they may also retard industry restructuring.

of recapitalized banks should reach these firms. Consistently with our previous results, however, we do not find that these firms, which are arguably higher quality than average, experience higher abnormal returns or receive larger loans than other firms after recapitalizations and bank mergers. We also consider that some firms may have no demand for bank loans because they have access to the bond market. If we exclude firms with access to the bond markets before the interventions, our results are qualitatively invariant. We still find that export-oriented firms do not receive larger loans than other firms nor experience higher abnormal returns. If anything, they receive smaller loans and are expected to perform worse that other related firms after the interventions. After mergers, however, export-oriented firms do not appear to experience negative abnormal returns suggesting that, at least in this respect, mergers may have improved capital allocation (as we show in Table 8, bank relationships with export-oriented firms are less likely to be interrupted after mergers).

Overall, these results suggest that interventions for bank rehabilitation, at best, affect the supply of credit equally for low and high quality firms and are seldom translated in performance improvements because firms prefer to accumulate cash in uncertain economic times.

While our interpretation of the empirical evidence is consistent with the findings of previous literature suggesting that Japanese banks severely misallocated credit before the bailouts, the results in Table 7 can also be viewed as evidence that the increase in the supply of bank loans benefit marginal borrowers with less productive investment opportunities. The fact that, in some specifications that we do not report for brevity, we find no evidence of higher investment, growth in employment, or performance improvements for these marginal borrowers lends no support to this interpretation.

To further evaluate the merit of this argument, however, we investigate the effects of the interventions on the terminations and initiations of relationships. Considering loan provision on the extensive margin can help us to sharpen the results. If credit allocation was indeed efficient and the marginal borrowers were low quality borrowers, we would expect that not only the lending banks lend more and are less likely to terminate relationships with them, but that also other banks that had previously made no loans initiate relationship with these borrowers. We find no evidence of this.

After the government recapitalizations and private recapitalizations, all relationships are less likely to be terminated (Panel A of Table 8). This is consistent with an increase in the loan

supply that affects loan provision on the intensive as well as on the extensive margins. It is more troubling, however, that after several of the recapitalizations, either public or private, relationships with low quality firms seem even less likely to be terminated. The effects are economically large: The unconditional probability that a relationship is terminated is 13 percent in our sample; Government recapitalizations decrease this probability by something between 50 and 100 percent and the probability that the relationships with real estate firms are terminated after the second government recapitalization is 50 percent lower than for other probably more deserving borrowers. Only after mergers, banks are more likely to terminate relationships with all borrowers and with low quality firms, in particular. Consistently with the theoretical results of Diamond and Rajan (2000), relationships with high quality firms are also more likely to be terminated (after all the interventions, except the second government recapitalization), possibly because these firms are the only ones to be able to reimburse their loans.

None of the interventions appear to favor the formation of new relationships (Panel B of Table 8). New relationships appear to be initiated only with exporting firms after the second and third government recapitalizations and after private recapitalizations, suggesting that the demand for credit of these firms is not exhausted. More importantly, it clearly emerges that banks that were not previously engaging certain low quality borrowers are less likely (or not more likely) to establish relationships with them after all the interventions. This suggests that the incentives to extend credit depend on the amount of outstanding loans. As Dewatripont and Maskin (1995) show, banks may have an incentive to lend to non viable borrowers if they consider a previously extended loan as a sunk cost. Other banks, with no outstanding exposure to these borrowers, would not extend credit, as we indeed observe for low quality firms. Thus, the positive abnormal returns for low quality firms upon the announcement of the government recapitalizations is not only explained by the larger loans, but to an even larger extent by the higher likelihood of maintaining the relationships with the existing banks, the only ones willing to engage them.

VI. Further robustness and extensions

A. Bank shareholdings

A peculiarity of Japan is that banks hold shares in industrial companies. Although this phenomenon is diffused (89 percent of firms have banks as shareholders in our sample), banks' equity stakes are generally small (on average, 2 percent of firm capital) and are constrained to be

less than 5 percent by the law. Thus, banks' exposure as shareholders is much smaller than their exposure as creditors. For this reason, equityholdings are believed not to affect bank incentives (Morck and Nakamura, 1999). Nevertheless, we test whether a firm benefits more from the interventions if the recapitalized bank is both a lender and a shareholder. If this were the case, equity ties – and not perverse incentives related to low capitalization – could explain why banks increase loans to low and high quality firms alike after the recapitalizations. We find no evidence that bank shareholdings explain firms' abnormal returns upon the announcement of the recapitalizations or access to bank loans.

Overall, this confirms our earlier result that our estimates are not driven by the peculiar nature of bank-firm relationships in Japanese business groups or by bank shareholdings.

B. Other events

Besides the events we discuss above, we also explore other events related to the banking crisis and its resolution. The consideration of these additional events not only allows us to evaluate their role in the resolution of the crisis, but also to further test whether our estimates may spuriously capture events concurrent to the ones on which we focus.

Between 1999 and 2003, 9 listed banks in our sample reduced their capital. We explore the effects of the announcement of these banks' capital reductions by including a capital-reduction event dummy defined similarly to the other event dummies. We cannot detect any effects of these announcements on firm abnormal returns or bank lending policies suggesting that the explicit realization of bank losses does not affect bank behavior and that it is not expected to, possibly because bank losses were already common knowledge for market participants. For brevity, we do not report the results.

C. Other borrowers

For reasons of data availability, we focus on the reaction of listed companies to the bailouts of their related banks. However, it is possible that the bailouts have even larger benefits on borrowers that we do not observe.[20] Fortunately, we have yearly information on the number of borrowers as well as on the number of small business borrowers and the amount of loans to small business borrowers for all listed and the largest unlisted banks. Hence, we can ask whether after

[20] We also test whether banks use the capital injections to hoard cash, but we find no evidence of that.

capital injections or bank mergers, the number of borrowers, the number of small business borrowers or the amount of loans to small business borrowers increase.

Since we want to be able to include year fixed effects to control for the macroeconomic environment as well as bank fixed effects, we focus only on the events that affect a subset of banks. Estimates in Table 9 show that the recapitalizations do not increase the number of bank borrowers, whether large or small, or the amount of loans to small business borrowers. The effects are similar for government and private recapitalizations. After the recapitalization of Resona bank, the number of borrowers even decreases, apparently driven by a decrease in the number of small business borrowers. Overall, it is unlikely that capital injections have large effects on sectors of the economy that we do not capture.

After mergers, banks appear to have more borrowers (but not more loans to small business borrowers). This may be a mechanical effect due to the fact that when a bank acquires a much smaller bank we may not observe the number of borrowers of the client. Indeed, in Panel A of Table 8, the probability that, after the merger, the bank terminates the relationships with the listed companies for which we observe bank loans increases.

VII. Conclusions

To the best of our knowledge, this paper provides the first micro-evidence on the effects of bank bailouts on firm valuation, access to credit, and subsequent investment and performance. We show that the announcement of banks' recapitalizations by the government affects positively the valuation of those banks' clients and the supply of credit. However, most of firms do not expand employment or have higher growth of sales than comparable firms in the two years following the recapitalization of related banks. Firms mostly use the larger loans to accumulate cash.

Furthermore, our empirical evidence uncovers that recapitalizations allow banks to supply larger loans to low and high quality firms alike and that the positive effects on the valuation of low-quality firms are much larger. This suggests that capital injections may increase the misallocation of credit. Private sector solutions, such as bank mergers or capital injections by private investors, are not immune from inefficiencies in the allocation of credit during banking crises.

Overall, our analysis highlights that, during periods of high uncertainty, such as systemic banking crises, the positive effects of bank bailouts on the real economy may be muted by firms' desire to accumulate cash for precautionary reasons. At the same time, the costs of bank bailouts may be increased by the misallocation of credit. These effects may have depended on the fact that even after the recapitalizations, Japanese banks' ability to face shocks with buffer reserves remained limited (Hoshi and Kashyap, 2008). The inability to withstand shocks may not only have affected banks' lending policies, but also the overall sense of economic uncertainty. While this may be quite frequent, as also in the U.S. the balance sheets of many banks continued to be fragile after the TARP injections (Congressional Oversight Panel, 2009), we believe that it is an exciting area for future research to explore whether more decisive interventions to tackle problems of capital shortage may eliminate the limitations associated with capital injections uncovered in this paper.

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Table 1
Interventions for Bank Rehabilitation

This table provides a list of the main interventions for bank rehabilitation we focus on. The events in bold are the ones that affect only a subsample of banks at the same time and are the main focus of our analysis. We define a firm to be related to a bank if it obtains at least 5 percent of the loans from that bank.

Events	Date	Description	Related firms	Unrelated firms
First government recapitalization	**February 16, 1998**	**20 major banks are recapitalized**	1979	214
Second government recapitalization	**March 1, 1999**	**15 major banks are recapitalized**	1708	536
Third government recapitalization	**May 19, 2003**	**Resona Bank is recapitalized**	472	1680
Fourth recapitalization	June 2, 2003	Government is allowed to provide capital to any bank that is considered systemically important	2193	
Fifth recapitalization	June 2, 2004	Government is allowed to provide capital to any bank	2148	
Private recapitalizations	**Different dates**	**98 capital injections by private investors affecting 64 banks between 1998 and 2005**	2437	723
Merger	**Different dates**	**71 bank mergers affecting 58 banks between 1998 and 2005**	2490	670
Asset management companies	April 1, 1999 May 1, 2003	Two different asset management companies are created with the goal of purchasing bad loans from banks	2577	
Takenaka's market based program	October 30, 2002	Banks are requested to rigorously evaluate assets and to improve transparency	2237	

Table 2
Descriptive Statistics

This table reports descriptive statistics for the main variables. *Bank* (*firm*) *abnormal return* is the difference between the actual return of the bank (firm) 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 less than 100 days to compute expected returns. *Loan growth* is the change in the loan of firm *i* from bank *j* between years *t* and *t+2* divided by the total financial debt of firm *i* at *t*. *Employment growth* is the growth rate of employment defined as defined as $ln(Employment_{i,t+1}/Employment_{i,t-1})$, where $Employment_{i,t}$ is the number of employees of firm *i* at time *t*. *Cash, Financial Debt* and *Sales growth* are defined similarly. *Investment* is defined as the growth rate of fixed assets. In the empirical analysis, all performance measures are winsorized at approximately the 1st and the 99th percentiles of their distribution. *Terminated (New) relationship* is a dummy variable that takes value 1 if a bank-firm relationship that existed (did not exist) at *t-1* has been terminated (initiated) at *t*. *Real estate* is a dummy variable which equals one if the firm is in the Construction or Real Estate industry. *Bank-dependent* is a dummy variable that equals 1 if the firm has a ratio of bank loans to total assets in the top quartile of the sample in 1997. The *recapitalization* dummies take value 1 if any bank providing more than 5 percent of the loans to the firm during the previous year was affected by the government recapitalization at a given date, and 0 otherwise. The *merger* dummy takes value one if any bank providing more than 5 percent of the loans to the firm during the previous year merged with another financial institution at a given date and zero otherwise. The *private recapitalization* dummy takes value 1 if if any bank providing more than 5 percent of the loans to the firm during the previous year has issued equity to private investors at a given date and 0 otherwise. With the exception of abnormal returns all other variables are presented at yearly frequency. Abnormal returns have daily frequency.

Panel A: Firm Characteristics

	Obs	Mean	Std.Dev	Median	1st percentile	99th percentile
Firm abnormal return (%)	3723000	0.224	3.462	0.177	-9.440	11.061
Bank abnormal return (%)	175247	0.201	2.540	0.172	-6.756	7.684
Loan growth x 100	142102	0.090	0.913	0.000	-2.533	3.923
Employment growth (2 years)	10692	0.009	0.211	-0.011	-0.531	0.699
Financial debt growth (2 years)	10662	-0.109	0.664	-0.067	-2.536	1.741
Cash growth (2 years)	11269	0.004	0.520	0.005	-1.393	1.471
Investment (2 years)	11286	0.027	0.294	-0.003	-0.734	1.014
Sales growth (2 years)	11315	0.027	0.226	0.019	-0.558	0.731
Terminated relationship	243996	0.13				
New relationship	262569	0.11				
Real estate	15971	0.074				
Bank-dependent	15971	0.341				

Panel B. Event dummies

	Obs	Mean
First recapitalization	15971	0.124
Second recapitalization	15971	0.107
Third recapitalization	15971	0.030
Fourth recapitalization	15971	0.137
Fifth recapitalization	15971	0.135
Private recapitalization	15971	0.390
Merger	15971	0.490
Asset management companies	15971	0.408
Takenaka's market based program	15971	0.153

Table 3
Selection Issues

We compare the characteristics of the firms (and the banks) benefiting from different interventions with those of the firms (and the banks) that are not affected by those interventions. The control group for firms related to banks receiving government recapitalizations is the group of unrelated firms. Mergers and private recapitalizations however occur at different dates. Thus for one of such events happening at date *t*, the control group at date t includes unrelated firms as well as the firms related to banks experiencing mergers and private recapitalizations at different dates.

		Related firms	Unrelated firms	T-stat H_0(Unrelated-Related=0)
Firm characteristics				
Fraction of real estate companies	Government recapitalizations	0.09	0.07	-3.46**
	Mergers	0.09	0.06	-6.43***
	Private recapitalizations	0.09	0.06	-6.06***
Fraction of bank-dependent firms	Government recapitalizations	0.37	0.44	4.84***
	Mergers	0.35	0.49	13.33***
	Private recapitalizations	0.34	0.48	13.24***
Sales	Government recapitalizations	174900	178009	0.30
	Mergers	124366	117427	-2.03**
	Private recapitalizations	123701	118940	-1.37
Sales/fixed assets	Government recapitalizations	2.48	2.46	-0.37
	Mergers	2.45	2.49	1.50
	Private recapitalizations	2.44	2.49	1.76*
Employment	Government recapitalizations	4963	5700	1.38
	Mergers	4157	4389	1.35
	Private recapitalizations	4145	4363	1.25
Average sales growth 95-97	Government recapitalizations	0.03	0.03	1.45
	Mergers	0.04	0.03	-3.69***
	Private recapitalizations	0.04	0.03	-2.67**
Average fin. debt growth 95-97	Government recapitalizations	0.08	0.08	0.46
	Mergers	0.08	0.08	0.57
	Private recapitalizations	0.08	0.08	1.82*
Average cash growth 95-97	Government recapitalizations	0.10	0.10	0.02
	Mergers	0.08	0.09	3.53***
	Private recapitalizations	0.08	0.09	3.39***
Average asset growth 95-97	Government recapitalizations	0.09	0.08	-0.37
	Mergers	0.08	0.08	-0.35
	Private recapitalizations	0.08	0.09	1.07
Average employment growth 95-97	Government recapitalizations	0.06	0.07	0.61
	Mergers	0.07	0.07	0.39
	Private recapitalizations	0.06	0.07	1.47
Bank characteristics				
Capital/Assets	Government recapitalizations	1.200	0.998	-1.88*
	Mergers	1.236	0.967	-1.29
	Private recapitalizations	1.114	0.878	-1.94
Non-performing loans to assets	Government recapitalizations	3.585	2.160	-3.15***
	Mergers	2.916	2.163	-1.55
	Private recapitalizations	2.740	1.647	-4.10***
Loans past due to assets	Government recapitalizations	0.360	0.373	0.20
	Mergers	0.491	0.343	-1.62
	Private recapitalizations	0.398	0.332	-0.84
Restructured loans to assets	Government recapitalizations	1.484	0.945	-2.40**
	Mergers	1.132	0.97	-0.92
	Private recapitalizations	1.179	0.731	-3.16***

Table 4
Market Response to the Interventions for Bank Rehabilitation

Panel A. Basic specifications

We explore the response the response of banks' and firms' daily abnormal returns to the interventions. The event window is [-10,+5] (i.e., the event dummy for intervention *j* take value one 10 days before the event until 5 days afterwards). All variables are defined in Table 2 and the dependent variable is indicated on each column. AR refers to abnormal returns. Parameters are estimated by ordinary least squares. The constant is included in all regressions, but the coefficient is omitted. Estimates are obtained by ordinary least squares. Standard errors presented in parentheses are corrected for heteroskedasticity and clustering at the firm and month level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent, respectively. We also evaluate statistical significance by performing a bootstrapping exercise for the specification in column 2. x, xxx denotes significance at the 5% and 1% level using the bootstrapped standard errors.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Bank AR	Firm AR	Firm AR	Firm AR	Firm AR	Firm AR	Firm AR
						High dependence = Bank-dependent	High dependence =Bank provides more than 50% of loans
First recapitalization	-0.104***	0.122***	0.129***	0.130***	0.139***	0.087***	0.098***
	(0.030)	(0.025)	(0.038) xxx	(0.039)	(0.033)	(0.033)	(0.036)
First recapitalization * High dependence						0.092***	0.284***
						(0.012)	(0.023)
Second recapitalization	0.002	0.143***	0.155***	0.156***	0.165***	0.134***	0.118**
	(0.012)	(0.026)	(0.054) xx	(0.055)	(0.044)	(0.049)	(0.053)
Second recapitalization * High dependence						0.045***	0.362***
						(0.016)	(0.026)
Third recapitalization		0.330***	0.326***	0.326***	-0.023	0.254***	0.305***
		(0.026)	(0.033) xxx	(0.033)	(0.092)	(0.031)	(0.032)
Third recapitalization * High dependence						0.121***	0.142***
						(0.011)	(0.016)
Private recapitalization	0.069		-0.023	-0.023	-0.023	-0.016	-0.013
	(0.079)		(0.099)	(0.100)	(0.077)	(0.094)	(0.103)
Private recapitalization * High dependence						-0.012	-0.044
						(0.021)	(0.029)
Merger	0.307	-0.063	-0.064			-0.062	-0.064
	(0.201)	(0.053)	(0.053)			(0.053)	(0.055)
Merger*Strong bank				-0.067***	-0.062***		
				(0.021)	(0.024)		
Merger*Weak bank				-0.010	-0.005		
				(0.022)	(0.020)		
Merger* High dependence						-0.002	0.005
						(0.037)	(0.096)
Fourth recapitalization	-0.078***				0.248***		
	(0.013)				(0.038)		
Fifth recapitalization	0.070***				0.205***		
	(0.011)				(0.035)		
Asset management companies	-0.052				0.306***		
	(0.043)				(0.110)		
Asset management estate exposure					0.109*		
					(0.058)		
Takenaka's market based program	-0.130				-0.256***		
	(0.011)				(0.039)		
High Dependence						0.011	0.002
						(0.011)	(0.011)
Observations	164723	3721994	3721994	3721994	3721994	3721994	3721994
R-squared	0.0001	0.0001	0.0001	0.0001	0.0005	0.0001	0.0001
F-test:Merger*(Strong-Weak)				21.72	16.78		
p-value				0.000	0.000		

Panel B: Robustness tests

The dependent variable is the firm's daily abnormal return. All independent variables are defined in Table 2 with the exception of *lagged CAR* which is the firm's cumulative abnormal return between t-55 and t-25. In column 2, we remove from the sample all observations of related firms if the bank affected by the government recapitalizations is not the firm's main loan provider. In column 3, we exclude firms belonging to industrial keiretsu. In columns 1 to 3, we use our default event window [-10,+5]. In column 4 to 6, we use event windows of (-10,+10), (-5,+5) and (-5,+3) days, respectively. Estimates are obtained by ordinary least squares. Standard errors presented in parentheses are corrected for heteroskedasticity and clustering at the firm and month level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent, respectively.

	(1)	(2) Subsample with main related bank only	(3) Subsample without keiretsu firms	(4) Event window (-10,+10)	(5) Event window (-5,+5)	(6) Event window (-5,+3)
First recapitalization	0.129***	0.149***	0.139***	0.333***	0.332***	0.569***
	(0.038)	(0.039)	(0.038)	(0.044)	(0.054)	(0.047)
Second recapitalization	0.155***	0.159***	0.175***	0.267***	0.338***	0.232***
	(0.054)	(0.054)	(0.054)	(0.056)	(0.079)	(0.081)
Third recapitalization	0.326***	0.386***	0.327***	0.312***	0.263***	0.210***
	(0.033)	(0.032)	(0.031)	(0.033)	(0.032)	(0.032)
Private recapitalization	-0.021	-0.021	-0.027	-0.034	-0.031	-0.028
	(0.101)	(0.098)	(0.097)	(0.095)	(0.096)	(0.099)
Merger	-0.064	-0.064	-0.065	-0.065	-0.063	-0.064
	(0.053)	(0.053)	(0.052)	(0.050)	(0.052)	(0.052)
Lagged CAR	0.056					
	(0.130)					
Observations	3721879	3255966	3395306	3721879	3721879	3721879
R-squared	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001

Table 5
Loans and Interventions for Bank Rehabilitation

The dependent variable is the loan growth of firm *i* from bank *k* between *t* and *t+1*. All variables are defined in Table 2 and the dependent variable is indicated on each column. Parameters are estimated by using a within-firm estimator (i.e., we include firm*year fixed effects in all equations and estimate the parameters by ordinary lest squares). The constant is included in all regressions, but the coefficient is omitted. Standard errors presented in parentheses are corrected for heteroskedasticity and clustering at the firm level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent, respectively. All estimates are multiplied by 100.

	(1)	(2)	(3)	(4)	(5)	(6)
	All loans	All loans	All loans	Long-term loans	Short-term Loans	All loans, Subsample without keiretsu firms
First recapitalization	0.164***	0.227***	0.235***	0.105***	0.122***	0.228***
	(0.024)	(0.026)	(0.027)	(0.022)	(0.023)	(0.027)
Second recapitalization	0.362***	0.373***	0.398***	0.226***	0.147***	0.368***
	(0.030)	(0.031)	(0.030)	(0.024)	(0.026)	(0.031)
Third recapitalization	-0.193**	-0.220**	-0.226**	-0.089	-0.131	-0.220**
	(0.097)	(0.099)	(0.098)	(0.074)	(0.083)	(0.099)
Merger	0.028*	0.091***		0.018	0.073***	0.091***
	(0.016)	(0.024)		(0.018)	(0.016)	(0.024)
Private recapitalization	-0.164***	-0.143***	-0.156***	-0.079***	-0.064***	-0.145***
	(0.012)	(0.013)	(0.013)	(0.010)	(0.010)	(0.013)
Merger*Strong bank			-0.064***			
			(0.015)			
Merger*Weak bank			0.147***			
			(0.016)			
Asset management companies*			0.079***			
High real estate exposure bank			(0.014)			
Bank fixed effects	Y	Y	Y	Y	Y	Y
Firm , Year fixed effects	Y	N	N	N	N	N
Firm*Year fixed effects	N	Y	Y	Y	Y	Y
Observations	142949	142949	142949	142949	142949	141684
R-squared	0.118	0.212	0.214	0.194	0.194	0.213
F:			132.0			
Prob > F			0.000			

Table 6
Interventions for Bank Rehabilitation and Corporate Policies

We study the effects of the interventions on the two-year changes of the firm variables indicated in each column (i.e., Financial debt; Cash; Sales; Investment; and Employment). All dependent variables are defined in Table 2. Parameters are estimated by ordinary least squares. The constant is included in all regressions, but the coefficient is omitted. Estimates are obtained by ordinary least squares. Standard errors presented in parentheses are corrected for heteroskedasticity and clustering at the firm level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Financial Debt		Cash		Sales		Investment		Employment	
First recapitalization	0.119***	0.119**	0.109***	0.076**	-0.003	-0.017	-0.020	-0.021	0.009	0.002
	(0.045)	(0.046)	(0.036)	(0.038)	(0.013)	(0.013)	(0.015)	(0.015)	(0.012)	(0.012)
First recapitalization * Bank-dependent		-0.015		0.104***		0.042***		-0.006		0.019
		(0.031)		(0.039)		(0.014)		(0.017)		(0.013)
Second recapitalization	0.091**	0.096**	0.117***	0.097***	0.000	-0.009	0.018	0.001	0.006	-0.006
	(0.039)	(0.040)	(0.031)	(0.033)	(0.011)	(0.012)	(0.014)	(0.014)	(0.012)	(0.012)
Second recapitalization * Bank-dependent		-0.025		0.058*		0.026**		0.045***		0.034**
		(0.026)		(0.035)		(0.013)		(0.016)		(0.013)
Third recapitalization	-0.031	-0.018	-0.000	-0.029	0.009	0.007	-0.005	0.0159	0.00240	0.004
	(0.034)	(0.043)	(0.033)	(0.042)	(0.013)	(0.017)	(0.015)	(0.0152)	(0.0111)	(0.0149)
Third recapitalization * Bank-dependent		-0.028		0.067		0.006		-0.041		-0.001
		(0.063)		(0.064)		(0.025)		(0.030)		(0.022)
Private recapitalization	-0.094	-0.129*	0.044	0.041	0.000	-0.003	-0.027*	-0.047***	-0.005	-0.012
	(0.066)	(0.073)	(0.036)	(0.039)	(0.014)	(0.015)	(0.016)	(0.017)	(0.011)	(0.013)
Private recapitalization* Bank-dependent		0.108		0.006		0.008		0.046**		0.014
		(0.094)		(0.054)		(0.019)		(0.023)		(0.017)
Merger	0.022	0.021	-0.001	-0.012	-0.005	-0.006	0.004	-0.001	-0.007	-0.011*
	(0.016)	(0.018)	(0.015)	(0.018)	(0.005)	(0.006)	(0.007)	(0.008)	(0.005)	(0.006)
Merger* Bank-dependent		0.003		0.029		0.003		0.011		0.011
		(0.028)		(0.028)		(0.009)		(0.012)		(0.009)
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	10662	10662	11269	11269	11315	11315	11286	11286	10692	10692
R-squared	0.384	0.385	0.267	0.268	0.484	0.485	0.397	0.399	0.499	0.500

Table 7
Recapitalizations and Capital Allocation

Panel A. Low quality firms

Distinguishing by firm type, we explore the effects of the interventions on firms' abnormal returns (AR) and the loan growth of firm *i* from bank *k* between *t* and *t+1*. In columns 1 to 4, we consider real estate firms as low quality firms. In columns 5 to 8, we consider firms with profitability below the median as low quality firm. All dependent variables are defined in Table 2 with the following exception. Low profitability firm is a dummy variable that takes value 1 if the profitability of the firm is below the median. The dependent variable is indicated on each column. AR refers to abnormal returns. The constant is included in all regressions, but the coefficient is omitted. Parameters are estimated by ordinary least squares; for the loan growth, we use a within-firm estimator. Standard errors presented in parentheses are corrected for heteroskedasticity and clustering at the firm level in the loan regressions and at the firm and the month level in the abnormal return regressions. ***, **, and * denote statistical significance at the 1, 5, and 10 percent, respectively. All estimates are multiplied by 100.

	Low quality firm=Real Estate				Low quality firm = Low Profitability			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample		Subsample without keiretsu firms		Full Sample		Subsample without keiretsu firms	
	Firm AR	All loans	Firm AR	All loans	Firm AR	All loans	Firm AR	All loans
First recapitalization	0.043	0.220***	0.054	0.221***	-0.080***	0.237***	-0.056*	0.243***
	(0.044)	(0.028)	(0.044)	(0.028)	(0.028)	(0.043)	(0.029)	(0.044)
First recapitalization * Low quality firm	0.578***	0.047	0.570***	0.049	0.462***	-0.016	0.442***	-0.026
	(0.018)	(0.082)	(0.022)	(0.085)	(0.024)	(0.052)	(0.023)	(0.053)
Second recapitalization	0.091	0.373***	0.115*	0.372***	0.046	0.388***	0.081**	0.396***
	(0.062)	(0.031)	(0.063)	(0.032)	(0.039)	(0.056)	(0.040)	(0.057)
Second recapitalization * Low quality firm	0.268***	-0.055	0.240***	-0.106	0.212***	-0.026	0.190***	-0.047
	(0.017)	(0.103)	(0.019)	(0.098)	(0.029)	(0.064)	(0.027)	(0.064)
Third recapitalization	0.322***	-0.217**	0.323***	-0.218**	0.225***	-0.119	0.225***	-0.117
	(0.031)	(0.105)	(0.029)	(0.105)	(0.026)	(0.131)	(0.025)	(0.132)
Third recapitalization * Low quality firm	0.044	-0.019	0.050	-0.018	0.201***	-0.206	0.206***	-0.210
	(0.030)	(0.277)	(0.031)	(0.278)	(0.031)	(0.184)	(0.031)	(0.184)
Merger	-0.048	0.093***	-0.050	0.093***	-0.066	0.111***	-0.067	0.111***
	(0.045)	(0.024)	(0.045)	(0.024)	(0.047)	(0.035)	(0.044)	(0.035)
Merger* Low quality firm	-0.014	-0.018	-0.008	-0.015	0.003	-0.045	0.001	-0.045
	(0.032)	(0.069)	(0.030)	(0.070)	(0.018)	(0.045)	(0.020)	(0.045)
Private recapitalization	0.037	-0.149***	0.0338	-0.152***	-0.032	-0.176***	-0.037	-0.177***
	(0.111)	(0.014)	(0.109)	(0.014)	(0.094)	(0.020)	(0.093)	(0.020)
Private recapitalization * Low quality firm	-0.201**	0.030	-0.201**	0.040	0.013	0.056**	0.014	0.056**
	(0.099)	(0.033)	(0.098)	(0.033)	(0.027)	(0.023)	(0.025)	(0.023)
Low quality firm	-0.003		-0.000637		0.012		0.0118	
	(0.019)		(0.0208)		(0.017)		(0.016)	
Firm*Year fixed effects	N	Y	N	Y	N	Y	N	Y
Observations	3721634	142949	3394969	141684	3721634	142949	3394969	141684
R-squared	0.0001	0.215	0.0001	0.215	0.0001	0.213	0.0001	0.213

Panel B. High quality firms

Distinguishing by firm type, we explore the effects of the interventions on firms' abnormal returns (AR) and the loan growth of firm *i* from bank *k* between *t* and *t+1*.We consider firms with large exports as high quality firms. The dependent variable is indicated on each column and all variables are defined in Table 2 with the exception of high export firm, which is a dummy that takes value one 1 if the ratio of export to sales is above the 75th percentile. The constant is included in all regressions, but the coefficient is omitted. Parameters are estimated by ordinary least squares; for the loan growth, we use a within-firm estimator. Standard errors presented in parentheses are corrected for heteroskedasticity and clustering at the firm level in the loan regressions and at the firm and the month level in the abnormal return regressions. ***, **, and * denote statistical significance at the 1, 5, and 10 percent, respectively. All estimates are multiplied by 100.

	(1)	(2)
	Firm AR	All loans
First recapitalization	0.166***	0.253***
	(0.039)	(0.032)
First recapitalization * High export firm	-0.126***	-0.090*
	(0.015)	(0.051)
Second recapitalization	0.151***	0.403***
	(0.055)	(0.038)
Second recapitalization * High export firm	0.014	-0.098*
	(0.019)	(0.058)
Third recapitalization	0.346***	-0.187*
	(0.032)	(0.109)
Third recapitalization * High export firm	-0.0894***	-0.147
	(0.013)	(0.226)
Merger	-0.076	0.118***
	(0.050)	(0.025)
Merger* High export firm	0.0528***	-0.198***
	(0.011)	(0.071)
Private recapitalization	-0.029	-0.140***
	(0.099)	(0.015)
Private recapitalization * High export firm	0.030	-0.018
	(0.022)	(0.022)
High export firm	-0.018	
	(0.012)	
Observations	3721879	142949
R-squared	0.0001	0.213

Table 8
Bank Bailouts and Access to Credit on the Extensive Margin

Panel A. Relationship terminations

The dependent variable is the terminated relationship dummy, which takes value 1 if a relationship existing at time *t* is terminated at *t+1* and value 0 otherwise. All the other variables are defined in the Table 2 with the following exceptions. Low profitability firm is a dummy variable that takes value 1 if the profitability of the firm is below the median. High export firm is a dummy that takes value one 1 if the ratio of export to sales is above the 75th percentile. % of loans is the percentage of bank loans that the firm receives from a given bank at time t. Estimates are obtained from a linear probability model; we exploit only within firm variation because we include firm*year fixed effects. Standard errors presented in parentheses are clustered at the firm level and corrected for heteroskedasticity. ***, **, and * denote statistical significance at the 1, 5, and 10 percent, respectively.

	(1)	(2) Firm quality=Real estate	(3) Firm quality=Low profitability	(4) Firm quality=High export
First recapitalization	-0.065***	-0.063***	-0.067***	-0.059***
	(0.004)	(0.004)	(0.005)	(0.004)
First recapitalization * Firm quality		0.009	0.004	-0.012***
		(0.012)	(0.006)	(0.005)
Second recapitalization	-0.135***	-0.130***	-0.125***	-0.134***
	(0.005)	(0.005)	(0.007)	(0.006)
Second recapitalization * Firm quality		-0.058***	-0.020**	-0.001
		(0.016)	(0.009)	(0.007)
Third recapitalization	-0.130***	-0.125***	-0.130***	-0.150***
	(0.023)	(0.024)	(0.027)	(0.025)
Third recapitalization * Firm quality		-0.027	0.001	0.055***
		(0.052)	(0.049)	(0.021)
Private recapitalization	-0.046***	-0.043***	-0.042***	-0.055***
	(0.003)	(0.003)	(0.003)	(0.005)
Private recapitalization * Firm quality		-0.007	-0.010**	0.011**
		(0.008)	(0.005)	(0.005)
Merger	0.012***	0.011***	0.011***	0.015***
	(0.002)	(0.002)	(0.003)	(0.003)
Merger*Firm quality		0.032***	0.002	-0.005**
		(0.005)	(0.003)	(0.003)
% of loans	-0.283***	-0.284***	-0.283***	-0.284***
	(0.008)	(0.008)	(0.008)	(0.008)
Firm*Year fixed effects	Y	Y	Y	Y
Bank fixed effects	Y	Y	Y	Y
Observations	207477	207471	207477	207477
R-squared	0.215	0.215	0.215	0.215

Panel B. New relationships

The dependent variable is the new relationship dummy, which takes the value *1* if a bank-firm relationship reported at *t+1* was no reported at *t*; the dummy takes value 0 if the relationship was already reported at *t*. All the other variables are defined in the Table 2 with the following exceptions. Low profitability firm is a dummy variable that takes value 1 if the profitability of the firm is below the median. High export firm is a dummy that takes value one 1 if the ratio of export to sales is above the 75th percentile. % of loans is the percentage of bank loans that the firm receives from a given bank at time t. Estimates are obtained from a linear probability model; we exploit only within firm variation because we include firm*year fixed effects. Standard errors presented in parentheses are clustered at the firm level and corrected for heteroskedasticity. ***, **, and * denote statistical significance at the 1, 5, and 10 percent, respectively.

	(1)	(2)	(3)	(4)
		Firm quality=Real estate	Firm quality=Low profitability	Firm quality=High export
First recapitalization	-0.029***	-0.029***	-0.031***	-0.021***
	(0.003)	(0.004)	(0.005)	(0.004)
First recapitalization * Firm quality		-0.029***	0.006	-0.018***
		(0.007)	(0.006)	(0.004)
Second recapitalization	0.001	0.000	-0.004	-0.019***
	(0.004)	(0.005)	(0.007)	(0.005)
Second recapitalization * Firm quality		-0.004	0.010	0.052***
		(0.011)	(0.008)	(0.006)
Third recapitalization	-0.010	-0.011	-0.018	-0.012
	(0.015)	(0.016)	(0.021)	(0.014)
Third recapitalization * Firm quality		-0.001	0.020	0.283***
		(0.036)	(0.026)	(0.027)
Private recapitalization	-0.024***	-0.024***	-0.024***	-0.039***
	(0.002)	(0.002)	(0.003)	(0.004)
Private recapitalization * Firm quality		-0.018***	-0.001	0.011***
		(0.006)	(0.004)	(0.004)
Merger	-0.021***	-0.020***	-0.021***	-0.016***
	(0.002)	(0.002)	(0.003)	(0.003)
Merger* Firm quality		-0.021***	0.001	-0.010***
		(0.003)	(0.003)	(0.002)
% of loans	-0.139***	-0.139***	-0.140***	-0.140***
	(0.008)	(0.008)	(0.008)	(0.008)
Firm*Year fixed effects	Y	Y	Y	Y
Bank fixed effects	Y	Y	Y	Y
Observations	226192	226192	226192	226192
R-squared	0.195	0.195	0.195	0.196

Table 9
Lending to Listed and Unlisted Companies

We focus on yearly bank lending and our unit of analysis is the bank-year. The dependent variable is the one- (or two-) year growth rate in the bank's number of borrowers and of small business borrowers for each bank in columns (1) to (4) and column (6). In column (5), the dependent variable is the one year growth rate in the amount of loans to small business borrowers of each bank. Parameters are estimated by ordinary least squares. The constant is included in all regressions, but the coefficient is omitted. Standard errors presented in parentheses are corrected for heteroskedasticity and clustering at the bank level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent, respectively.

	(1) One year, all borrowers	(2) One year, all borrowers	(3) One year, small borrowers	(4) One year, small borrowers	(5) One year, loans to small borrowers	(6) Two years, small borrowers
First recapitalization	0.0362	0.0340	0.0363	0.0341	-0.0469*	0.0415
	(0.0381)	(0.0353)	(0.0387)	(0.0359)	(0.0248)	(0.110)
Second recapitalization	-0.0301	-0.0528	-0.0296	-0.0524	0.0333	0.132
	(0.0464)	(0.0521)	(0.0466)	(0.0523)	(0.0438)	(0.225)
Third recapitalization	-0.176***	-0.170***	-0.177***	-0.171***	-0.311***	-0.331***
	(0.0195)	(0.0202)	(0.0193)	(0.0200)	(0.0152)	(0.0412)
Private recapitalization		0.0256		0.0257	0.0150	0.0191
		(0.0206)		(0.0206)	(0.0156)	(0.0310)
Merger	0.0783*	0.0799*	0.0785*	0.0801*	0.0983***	0.141*
	(0.0416)	(0.0419)	(0.0417)	(0.0420)	(0.0281)	(0.0815)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	860	860	860	860	872	838
R-squared	0.251	0.255	0.254	0.257	0.274	0.328