An Econometric Evaluation of Bank Recapitalization Programs with Bank- and Loan-level Data

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Abstract. Public capital injections into the banking system are a comprehensive policy program aimed at reducing the financial risks faced by capital-injected banks, thereby stimulating their lending and profitability. This paper evaluates empirically two large-scale bank capital injections in Japan in 1998 and 1999. We begin by extracting the treatment effects of the public injections using bank-level panel data. Using a difference-in-difference estimator in two-way fixed-effects regression models, we find that the public injections significantly reduced the financial risks faced by the capital-injected banks, but did not stimulate their lending and profitability. Next, we investigate the factors that impeded bank lending following the capital injections using a matched sample of Japanese banks and their borrowers. By employing three-way fixed-effects regression models corresponding to the matched sample, we provide evidence that the deterioration of borrower creditworthiness inhibited not only the capital-injected banks, but also other banks, from lending more.

JEL classification: G01, G21, G28.

Keywords: public capital injection, treatment effect, capital crunch, default risk difference-in-difference estimator, three-way fixed-effects model.

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1. Introduction

Public capital injections into the banking system are a comprehensive policy program aimed at reducing the financial risks of capital-injected banks, thereby stimulating their lending and profitability. The financial crisis after the 2008 Lehman shock and the global recession that followed forced many industrialized countries, including the UK, France, Germany, Ireland, the US, and Switzerland, to implement such bank recapitalization programs. Accordingly, a macroeconomic framework to conceptualize theoretically how this policy program works has been developing (see, e.g., Gertler and Kiyotaki (2010), Kollmann et al. (2012), and Hirakata et al. (2013)), but no empirical consensus exists on whether it has produced the desired results. This paper utilizes two large-scale bank capital injections in Japan in 1998 and 1999, which are regarded as precedents for the European and US public capital injections, as a natural experiment in bank recapitalization policy, and attempts to offer new insights into the actual implementation of public capital injection programs in the banking system.

Theoretically, when asymmetric information exists, an increase in a bank's financial risk can cause its lending behavior to deteriorate. This phenomenon, where a bank restrains its lending because of the increase in its financial risk, is known as a "capital crunch". Indeed, several studies have found evidence supporting the existence of capital crunches in both the US and in Japan in the 1990s (see, e.g., Bernanke and Lown (1991) and Peek and Rosengren (1995) for investigations into capital crunches in the US, and Woo (2003) and Watanabe (2007) for analyses of Japan's experience). Previous studies of the Japanese bank recapitalization programs in 1998 and 1999 mainly focused on whether the two programs resolved the capital crunch of banks requiring capital injection.

The favorable view of the effect of Japan's public capital injections suggests that they reduced the default risk of the capital-injected banks, thereby improving their lending (see Allen et al. (2011) and Giannetti and Simonov (2013)). Figures 1 and 2 display the historical paths of the probability of default and bank loans to domestic enterprises for Japanese banks across two banking groups: the treated group comprises banks involved in bank recapitalization programs, and the control group includes banks that are not. [1]

[1] See Subsection 2.3 for the method of calculating the probability of default and the definition of bank loans.

As shown in Figure 1, the probability of default of the treated group decreased drastically following the two public capital injections in 1998 and 1999, while that of the control group seldom changed before and after the capital injections. In contrast, Figure 2 shows that the bank loans not only of the treated group but also of the control group decreased continuously following the capital injections. Casual observation reveals that the favorable view of bank public capital injections cannot successfully explain why lending by the injected banks did not increase, even though their financial conditions improved substantially.

One promising explanation is that the policy framework for the two capital injection programs that obliged each capital-injected bank to maintain and raise its capital ratio actually ended up impeding its lending, as pointed out by Osada (2011). However, this unfavorable view of bank public capital injections largely ignores the coexistence of the relatively stable financial conditions for non-capital-injected banks and their reduction in loans to domestic enterprises.

Despite the differing implications of the effects of bank public capital injections in Japan, these opposing views share a common premise in that the lending of Japanese banks following the capital injections was determined primarily by lender-side factors, such as bank financial conditions and profitability. However, once we note that the creditworthiness of many borrowers deteriorated during the subsequent severe recession, we cannot simply ascribe the stagnant bank lending after the capital injections to lender-side factors. In other words, the increased default risk and decreased profitability of borrowing firms, as shown in Figure 3, appear to be dominant factors accounting for the stagnant bank lending in the period after the bank public capital injections.

Some theoretical and empirical studies have noted the substantial role that borrower-side factors can play in causing stagnant bank lending during a severe recession. For example, Bernanke and Gertler (1989) and Bernanke et al. (1999) theoretically found that the deterioration of borrower creditworthiness in a severe recession could increase agency costs associated with lending, thereby decreasing the supply of bank credit. An empirical analysis of US capital injections by Berrospide and Edge (2010) likewise concluded that it was not possible to simple attribute the US slowdown in loan growth after the bank capital injections to bank capital positions. On this basis, they suggested that an adequate explanation of bank decision making in lending after the US capital injections needed to consider both borrower- and lender-side factors. Lastly, De Nicolò and Lucchetta (2011) established empirically that bank credit demand shocks are the main drivers of the bank lending cycle in G-7 economies, thereby disproving the conventional wisdom that constraints in the bank credit supply have been a key driver in the sharp downturn in real activity since the 2008 Lehman shock. Together, these studies suggest that any analysis of bank lending in a severe recession following public bank capital injection should include borrower-side factors.

In empirically evaluating bank lending following the bank public capital injections in Japan in 1998 and 1999, we incorporate the notion that public capital injections are a comprehensive policy program designed first to stabilize the banking system and then to stimulate bank lending and profitability. More precisely, we evaluate the two public capital injection programs by addressing the following three issues.

1. To what extent did the public capital injections in 1998 and 1999 contribute to reducing the financial risks of capital-injected banks, including default risk and non-performing loans?

2. If the public capital injections contributed to a decrease in the financial risks of the injected banks, did they also increase their lending to domestic enterprises and profitability?

3. Was there room to improve bank lending to domestic enterprises using the capital injections in the first place? If not, how can we explain the sluggish bank lending after the public capital injections shown in Figure 2?

To address the first and the second issues econometrically, we estimate the treatment effects of the public capital injections using bank-level panel data. To this end, we employ a difference-in-difference estimator in a two-way fixed-effects regression model. The main reason for employing this method is that the two capital injections are arguably representative of a "too big to fail policy," in that the public capital was injected primarily into major but problematic Japanese banks. Therefore, the overlapped region of estimated propensity scores for the treated and control groups is too small to employ propensity score-based methods (e.g., Heckman et al. (1997, 1998), Hirano et al. (2003), and Abadie (2005)). In addition, there is no conventional and tractable method for causal inference other than two-way fixed-effects regression methods in applied panel data analysis. [2]

To address the third issue, we use a matched sample of Japanese banks and their listed borrowing enterprises. By doing so, we can control for not only lender- but also borrower-side factors when investigating bank lending in Japan following the public bank capital injections. Some recent studies of bank lending utilize the same approach, including those of Albertazzi and Marchetti (2010), who used a matched sample of Italian banks and their borrowers, and Jiménez et al. (2012, 2014), who employed a matched sample of Spanish banks and their borrowers. These studies, which controlled for both firm- and bank-level characteristics, examined bank decision making in lending more specifically.

Our approach to evaluating Japan's bank public capital injections differs from that of previous studies as follows. First, existing analyses (Allen et al. (2011), Osada (2011), and Giannetti and Simonov (2013)) measured the responses of bank lending only when public capital injections were taking place. In taking into consideration the characteristics of public capital injections, it is more important to select outcome variables linked to their policy objectives and then to measure their change over time. We attempt to capture this duration effect for Japan's public capital injections in terms of causal inference. [3]

Second, as pointed out by Conley and Taber (2011), when the number of members belonging to the treated group is much smaller than the number of those belonging to the control group, the standard large-sample approximations are not appropriate for conducting the statistical inference of a treatment-effect estimate obtained using a fixed-effects panel model. Following Conley and Taber (2011), we conduct rigorous statistical inference of the treatment-effect estimate based on the empirical distribution.

Third, to assess actual bank lending conditions following the public capital injections, we

[2] In terms of causal inference, Bertrand et al. (2004), Athey and Imbens (2006), and Angrist and Pischke (2009, Ch. 5) considered the application of a difference-in-difference estimator to panel data in two-way fixed-effects regression models. Wooldridge (2005) established statistically the conditions under which two-way fixed-effects regression estimators are consistent for the treatment effect.

[3] Spiegel and Yamori (2003) and Giannetti and Simonov (2013) employed an event study approach to evaluate the bank capital injections in Japan. Unlike our study, these attempted to analyze the very short-term effects by estimating stock market responses to the announcement of the public capital injections into the banking system.

exploit a loan-level matched sample of Japanese banks and their borrowers. Specifically, we include both lender- and borrower-side factors into a bank lending function as time-varying observable and time-invariant unobservable variables. Like our analysis, Giannetti and Simonov (2013) used a matched sample of Japanese banks and their borrowing enterprises to estimate a bank lending function for a post-capital injection period. However, unlike our analysis, they controlled for the primary lender- and borrower-side factors using unobserved fixed effects, but did not estimate the unobserved fixed effects. Accordingly, we are yet to understand fully which factors, lender- or borrower-side, drove the sluggish bank lending after the public capital injections. To address this, we incorporate our bank lending function into the framework of a three-way fixed-effects regression model. We thus reveal the role of lender- and borrower-side factors in the sluggish bank lending after the public capital injections by controlling for the time-varying observable lender- and borrower-side factors along with the two types of unobserved fixed effects. [4]

When estimating our bank lending function from the loan-level data set, we employ the fixed-effects estimation method developed by Abowd et al. (1999) and Andrews et al. (2008). This estimation method yields consistent and unbiased parameter estimates, not only for the time-varying observed covariates, but also for the unobserved fixed effects. [5] For the post-capital injection period, unlike extant studies of bank lending functions, we additionally analyze the nature of the estimated unobserved fixed effects. We then examine in depth whether the sluggish bank lending after the public capital injections was more the result of lender- than borrower-side factors.

The remainder of the paper is organized as follows. Section 2 explains the data sources and discusses a method for the estimation of the treatment effect. Section 3 reports the

[4] Peek and Rosengren (2005) and Gan (2007) included time-varying covariates for both Japanese banks and their borrowers to investigate bank lending in Japan from the early 1990s to the late 1990s. Unlike our study, Peek and Rosengren (2005) included only firm random effects as unobservable components and then employed the random effects probit model after transforming the growth data for bank loans into binary outcome data. Gan (2007) did not include unobservable components, and thus employed a simple pooled ordinary least-squares regression.

[5] To estimate the wage-setting functions, Abowd et al. (1999) and Andrews et al. (2008) applied their fixed-effects estimation method to French and German linked employer–employee data sets, respectively. Davis (2002) developed a method for estimating three- and four-way fixed-effects models, but his estimation method does not allow us to estimate the unobserved fixed effects.

estimated treatment effects obtained using bank-level panel data. We also discuss how the amount of capital injected into each bank influenced its default risk by introducing heterogeneity into the treatment effect. Section 4 reexamines the treatment effects by controlling for borrower-side factors together with lender-side factors in loan-level specifications. Section 5 analyzes which factors impeded bank lending after the capital injections, either lender- or borrower-side. Section 6 provides some concluding remarks. In Appendix I, we discuss the method of statistical inference developed by Conley and Taber (2011). In Appendix II, we explain the construction of the probability of default based on Merton's (1974) structural option-pricing model.

2. Data and Estimation Method In November 1997, four financial institutions (Sanyo Securities, Hokkaido Takushoku Bank, Yamaichi Securities, and Tokuyo City Bank) failed, and Japan experienced its greatest postwar financial crisis. Since then, the Japanese government has decided to use public funds to deal with the financial crisis, although up until then, it had been apprehensive about the effect on public opinion in doing so (see Nakaso (2001) and Hoshi and Kashyap (2010)).

To enable the actual implementation of the bank public capital injections, the Financial Function Stabilization Act (hereafter FFSA) came into effect in February 1998. As shown in Table 1, the first capital injection based on the FFSA was approved in March 1998 for 21 banks, with a total of 1,815.6 billion yen (1,080 billion yen for subordinated debt, 414.6 billion yen for subordinated loans, and 321 billion yen for preferred stock) paid on March 30.

Six months later, in October 1998, the government abolished the FFSA, and replaced it with the Prompt Recapitalization Act (hereafter PRA). Consequently, the limit on the amount of public capital available for banks increased from 13,000 billion yen to 25,000 billion yen. The government approved the second capital injection based on the PRA for 15 banks in March 1999, with a total of 7,459.25 billion yen (1,300 billion yen for subordinated debt and loans and 6,159.3 billion yen for preferred stock), again paid on March 30 of that year.

As discussed, Japan's bank public capital injections in 1998 and 1999 under the FFSA and the PRA are indicative of a "too big to fail policy," and hence we cannot employ propensity score-based methods to evaluate the treatment effects of the two capital injections. In this section, we first explain our data sources and then outline an econometric method for estimating the treatment effects of the two public capital injections.

The FFSA and PRA stipulate the policy objectives for public capital injections, and so the banking supervisory agency supervises a capital-injected bank to ensure that its actions are consistent with the policy objectives. In this section, we also define the financial variables corresponding to the policy objectives stipulated by the FFSA and the PRA, thereby specifying our econometric models more precisely.

2.1. **Data Sources** Our data are from three sources. First, we obtain bank-level panel data (balance sheet and income statements) from the Nomura Research Institute (hereafter NRI). The data are semiannual and based on financial statements reported by Japanese banks for the first half (ending September year t) and full year (ending March year $t+1$) of the fiscal year (hereafter FY) t, with our regression samples covering the period from September 1997 to March 2002. When conducting causal analysis with the bank-level panel data, we adjust the full-year statements of bank income to a semiannual basis.

We evaluate the treatment effects on the 21 banks that received the first capital injection in March 1998 and the 15 banks that received the second capital injection in March 1999, as shown in Table 1. Table 2 provides summary statistics for our bank-level panel data. The sample size for our analysis of the first capital injection program from September 1997 to September 1998 consists of 303 observations for 103 Japanese banks (21 banks in the treated group and 82 banks in the control group) listed on the Tokyo Stock Exchange. provides Summary statistics for our analysis of the second capital injection program from September 1998 to March 2002 consists of 751 observations for 99 Japanese banks (15 banks in the treated group and 84 banks in the control group). During the second subsample period after the second capital injection in March 1999, 17 regional banks in addition to the 15 capital-injected banks sporadically received public capital injections under the PRA. To extract the pure effects of the first and second public capital injections, we initially excluded the data for these 17 regional banks. [6]

[6] Until March 2002, capital injections under the PRA were intermittent. The capital injection in March 1999 was implemented for major banks, while each capital injection after April 1999 was implemented

In the construction of our data set, it is worth noting that by March 2002, after the second capital injection in March 1999, four mergers had taken place among capital-injected banks in the treated group for the second subsample. [7] The four mergers partially changed the composition of the treated group. To control for the merger effect on the composition of the treated group, we use the four continuing banks formed by the mergers before March 2002 as survivors of the premerger banks in the treated group. [8]

The second source of data is the company annual financial statements complied by the NRI, which we use to control for borrower-side characteristics in our loan-level matched sample. We use information on the borrowing firms' capital, total debts, total assets, profits, total interest payments, and investment for our analysis.

Finally, our matched bank–firm loan data, used for the analysis of the postinjection period after March 1998, are from the Corporate Borrowings from Financial Institutions Database compiled by Nikkei Digital Media Inc. The data are annual and report short- (a maturity of one year or less) and long-term (a maturity of more than one year) loans from each financial institution for every listed company on any Japanese stock exchange, which we sum to obtain the total amount of loans outstanding. For our analysis, we include loans from Japanese city, trust, regional, and mutual banks from FY1998 (ending March 1999) through FY2002 (ending March 2003), which is in accordance with data used for estimating the treatment effect of the capital injections with bank-level panel data. Our loan measure comprises all loans received from each financial institution for about 2,500 firms each year. Our data cover all industries, including manufacturing, mining, agriculture, and services.

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for the 17 regional banks based on the subprogram "Basic vision for strengthening the capital bases of regional banks" announced by the Japanese government in June 1999. We exclude the data on the 17 regional banks to extract the pure effects of the capital injection in March 1999, even though the capital injection in March 1999 and each capital injection after April 1999 drew on the PRA. Furthermore, in June 2003, there was a capital injection for the Resona Bank, but this occurred under the Deposit Insurance Law. In our sample, we do not include Resona Bank as a bank that received the first and second capital injections in 1998 and 1999, respectively.

[7] Four mergers took place: Chuo Trust Bank and Mitsui Trust Bank in 2000; Daiichi Kangyo Bank, Fuji Bank, Industrial Bank of Japan, and Yasuda Trust Bank in 2000; Sakura Bank and Sumitomo Bank in 2001; and Sanwa Bank, Tokai Bank, and Toyo Trust Bank in 2001.

[8] Regarding the mergers among Japanese banks that took place in the late 1990s and 2000s, Harada and Ito (2011) found that the merged banks largely inherited the financial conditions of the premerger banks. Similar to our study, they used an indicator of bank fragility based on Merton's (1974) model. According to their findings, our approach to dealing with the four mergers after the second capital injection would not significantly affect our estimation results, as reported in the following section.

Combining these three databases, we can link the characteristics of the individual Japanese firms with those of their individual lenders. When combining the bank-level panel data, we use the fiscal year-end reports by banks. Although the fiscal year for Japanese banks ends on March 31, this is not necessarily the case for their borrowing firms. Hence, we match the bank-side information to the borrower-side information in the same fiscal year.

Our main difficulty in working with the loan-level data was sorting through the various bank mergers and restructurings in our data. We thoroughly recorded the date of all bankruptcies and mergers that took place in the Japanese banking sector. Whenever a bank ceases to exist in our data because of a bankruptcy, firms cease reporting that financial institution as a source of loans. If we could not find any information on a bankruptcy or a merger, we filled in zero-loan data in our data set. On the other hand, if we found evidence of a bankruptcy or merger and firms reported loans from a restructured bank as coming from the prior bank, we recorded these loans as coming from the restructured bank. In order to calculate the loan growth of a restructured bank, we traced to it all banks that predated it. Thus, if banks A and B merged in year t to form bank C, bank C's loans in year $t-1$ would be set equal to the sum of the loans for banks A and B, and the growth rate of bank C's loans in year t would be calculated accordingly.

The loan-level data include about 100 banks, some 2,500 listed firms, and about 20,000 bank–firm relations each year. While our data set does not include all small- and medium-sized enterprises (SMEs), it does cover approximately 65% of the total loans of the Japanese banking sector over our sample period from FY1998 through to FY2002. Altogether, there are 104,840 loan observations (46,332 with the capital-injected banks and 58,508 with the non-capital-injected banks). Table 3 provides summary statistics for our loan-level matched data.

2.2. **Econometric Method with Bank-level Data** In this subsection, we discuss an econometric method to estimate the treatment effects with bank-level panel data. Let t^* denote the time at which public capital is injected into problem banks. Then, we denote $D_{it} = 1$ if bank i belongs to the treated group at time $t = t^* + k$ ($k \geq 0$) in which banks have entered into a recapitalization program at time t^*, and $D_{it} = 0$ if bank i belongs to the control group at time t in which banks have not entered into the program at time t^*.

Let us assume that this indicator variable takes the value $D_{it^*-1} = 0$ for all banks i at time $t^* - 1$.

Given the treatment indicator D_{it}, we introduce the following two-way fixed-effects regression models to estimate the treatment effect on the capital-injected banks:

$$\begin{aligned} \textbf{Model I}: \quad y_{it} &= \mathbf{X_{it-1}}\beta + \gamma_t t + \delta D_{it} + v_i + \varepsilon_{it}, \\ \textbf{Model II}: \quad y_{it} &= \mathbf{X_{it-1}}\beta + \gamma_t t + \delta_t (t \cdot D_{it}) + v_i + \varepsilon_{it}, \end{aligned}$$

where y_{it} is an outcome variable for bank i, and $\mathbf{X_{it-1}}$ are one-period lags of time-varying observed covariates. t is a time dummy variable with time $t^* - 1$ as the reference point of time, where the coefficient parameter γ_t captures the time effect that is common to all banks but that varies across time. v_i is the fixed-effects term for bank i, and ε_{it} is the stochastic error term.

The fixed-effects term v_i plays the role of embodying the unobserved characteristics of bank i, such as the unobserved managerial ability that determines managerial decisions, including the decision about whether the bank enters into the recapitalization program. As in the conventional fixed-effects models, v_i can be correlated not only with the treatment indicator D_{it} but also with the covariates $\mathbf{X_{it-1}}$ and each other.

Now, let us assume that the outcome variable of bank i takes a value of y_{1it} at time $t = t^* + k$ $(k \geq 0)$ if it has received a capital injection at time t^* $(D_{it} = 1)$ and y_{1it} at time t if it has not $(D_{it} = 0)$. Then, we can define the treatment effect on the treated group, denoted by TE, as follows:

$$TE = E(y_{1it} - y_{0it} | D_{it} = 1) = E(y_{1it} | D_{it} = 1) - E(y_{0it} | D_{it} = 1).$$

To measure TE, we estimate $E(y_{0it} | D_{it} = 1)$, which is the expected value of the counterfactual outcome that would be realized if a capital-injected bank has not been recapitalized. However, we cannot estimate the expected value directly from the observational data because the counterfactual outcome is not observable. [9] Then, we introduce the following

[9] If the public recapitalization program is randomly assigned across all banks, $E(y_{0it} | D_{it} = 1) = E(y_{0it} | D_{it} = 0)$ holds for time $t = t^* + k$ $(k \geq 0)$. However, this assumption is not appropriate because the

unconfoundedness assumption into Models I and II:

$$E(y_{0it}|D_{it},\ \mathbf{X_{it-1}},\ t,\ v_i) = E(y_{0it}|\mathbf{X_{it-1}},\ t,\ v_i). \tag{1}$$

Equation (1) implies that the recapitalization program is randomly assigned across banks at time $t = t^* + k$ $(k \geq 0)$ as long as X_{it-1}, t and v_i are conditional. By employing this assumption, the treatment effect at time t is expressed as an estimate of the parameter coefficient δ_t in Model II as follows:

$$\begin{aligned}
\delta_t &= E(y_{1it} - y_{0it}|\ \mathbf{X_{it-1}},\ t,\ v_i) = E(y_{1it} - y_{0it}|\ D_{it} = 1,\ \mathbf{X_{it-1}},\ t,\ v_i) \\
&= \{E(y_{1it}|\ D_{it} = 1,\ \mathbf{X_{it-1}},\ t,\ v_i) - E(y_{1it^*-1}|\ D_{it^*-1} = 0,\ \mathbf{X_{it-2}},\ t^* - 1,\ v_i)\} \qquad (2) \\
&\quad - \{E(y_{0it}|\ D_{it} = 1,\ y_{it-1},\ \mathbf{X_{it-1}},\ t,\ v_i) - E(y_{1it^*-1}|\ D_{it^*-1} = 0,\ \mathbf{X_{it-2}},\ t^* - 1,\ v_i)\},
\end{aligned}$$

where the second equality follows from equation (1). From the third equality in equation (2), we can interpret the estimate of δ_t as a difference-in-difference estimate in which time t^*-1 is the reference point of time. More precisely, the duration effect of the public capital injection, or δ_t, is the difference between the actual variation in the outcome variable (the first brace term) and the counterfactual variation (the second brace term). The difference between the actual and counterfactual variations measures the treatment effect of the capital injection on the outcome variable in terms of causal inference.

The treatment effect in Model I is expressed as an estimate of the parameter coefficient on D_{it} as follows:

$$\delta = E(\delta_t) = E(y_{1it} - y_{0it}|D_{it} = 1,\ \mathbf{X_{it-1}},\ t,\ v_i).$$

In the following, we measure the treatment effect on the capital-injected banks by estimating the parameter coefficients δ and δ_t in Models I and II. For estimation of the parameter coefficients, we use conventional within-group estimation methods.

For consistency of an estimator of coefficient parameters in a two-way fixed-effects regression model, a strict exogeneity condition, which requires that the stochastic error

program is not randomly assigned.

term should be uncorrelated with covariates over time, is necessary. As pointed out by Wooldridge (2005), the strict exogeneity condition is demanding for the use of the dynamic panel specification that includes the lagged dependent variable y_{it-1} in Models I and II. In this paper, we do not use the dynamic panel specifications of Models I and II. [10]

As stated, we estimate the causal effect of bank public capital injections using two fixed-effects regression models: Models I and II. However, Conley and Taber (2011) pointed out that the standard large-sample approximations are not appropriate for conducting statistical inference for a treatment-effect estimate obtained using a fixed-effects regression model when the number of members of the treated group is much smaller than that of the control group. Thus, following Conley and Taber (2011), we conduct statistical inference for estimates of the treatment-effect parameters δ and δ_t in Models I and II. The method of statistical inference developed by Conley and Taber (2011) is based on the empirical distribution calculated using residuals ε_{jt}, generated from the control group equation of a non-capital-injected bank j. In Appendix I, we discuss in detail the procedure for calculating the empirical distribution.

Our bank-level panel data set used for estimation of the treatment effect is semiannual. Hence, each time period t for estimating the treatment effect is associated with March or September. Our sample period for estimating the treatment effect of the first recapitalization program ranges from September 1997 to September 1998 because $t^* =$ March 1998, while that for estimating the treatment effect of the second recapitalization program ranges from September 1998 to March 2002 because $t^* =$ March 1999. The reason the sample period for analyzing the second recapitalization program extends to 2002 is that the third recapitalization program, as based on the Deposit Insurance Law, was for the Resona Bank in 2003. To extract the pure effect of the second recapitalization program before the third program, we set the end of the second subsample period at 2002.

[10] Angrist and Pischke (2009, Ch. 5) proposed using not only a two-way fixed-effects regression model without a lagged dependent variable, but also a lagged dependent variable model without a fixed-effects term, thus checking the robustness of the treatment-effect estimates. Accordingly, we also estimate Models I and II that include the lagged dependent variable y_{it-1} but exclude the fixed-effects term v_i by the ordinary least-squares estimation method. Our estimation results for Models I and II are qualitatively unaffected by whether the lagged dependent variable y_{it-1} is included.

2.3. **Bank-level Data Set and Estimation Model** In this subsection, we define the outcome variable y_{it} and covariates $\mathbf{X_{it}}$ corresponding to the policy objectives of the bank public capital injections in 1998 and 1999, thereby providing a more concrete specification for Models I and II. As detailed above, the public capital injections in 1998 and 1999 were on the basis of the FFSA and the PRA, respectively. To discipline the capital-injected banks, these Acts stipulate the following policy objectives: 1) reduction of the default risk for the capital-injected banks; 2) write-offs of nonperforming loans; 3) improvements in profitability; 4) improvements in bank lending to domestic enterprises, including SMEs; and 5) expenditure cuts through adjustment of employment costs, the number of board members, and the number of branch offices. The FFSA and the PRA discipline capital-injected banks in line with these policy objectives, but the ultimate purpose of the fifth policy objective is to improve the financial condition of capital-injected banks and to revitalize their profitability. Accordingly, we focus on the first to fourth policy objectives and consequently offer the following seven variables as the outcome variable y_{it}.

Related to policy objective 1): a variable measuring the default risk of bank i

1. Probability of default (PD_{it}),

Related to policy objective 2): a variable measuring the nonperforming loans of bank i

2. Nonperforming loan (NPL_{it}),

Related to policy objective 3): a variable measuring the profitability of bank i

3. Return on assets (ROA_{it}),

Related to policy objective 4): a variable measuring loans to enterprises offered by bank i

4. Growth rates of bank loans for domestic enterprises ($\Delta\mathrm{LOAN}_{it}$),
5. Growth rates of loans for SMEs ($\Delta\mathrm{SMELOAN}_{it}$).

The probability of default (PD_{it}) is theoretically based on Merton's (1974) structural option-pricing model. Let V_A represent the bank's asset (market) value, σ_A the asset volatility, and r the risk-free rate. Furthermore, we denote by D the book value of the debt that has maturity equal to T. In the framework of Merton's (1974) structural model, the risk-neutral probability of bank default is calculated as $N(d_1)$, where $d_1 = d_2 - \sigma_A\sqrt{T}$, $d_2 = \frac{\ln(V_A/D)+\left(r+\frac{1}{2}\sigma_A^2\right)T}{\sigma_A\sqrt{T}}$, and N denotes the cumulative density function of the standard normal distribution. We use the risk-neutral probability as a measure of default risk, after converting it to percentage terms. The calculation of the risk-neutral probability requires estimating two unknowns, V_A and σ_A. Appendix II discusses how we estimate them.

Nonperforming loans (NPL_{it}) are defined as the ratio of the reported amount of non-performing to total loans. We use the book values of nonperforming and total loans and include the logarithm of the nonperforming loan ratio in Models I and II. [11]

To obtain the return on assets (ROA_{it}) we divided each bank's net profits by the book value of total assets, expressed in percentage terms.

The growth rates of loans to domestic enterprises ($\Delta\mathrm{LOAN}_{it}$) are the period-by-period growth rates: $\Delta\mathrm{LOAN}_{it} = \left(\mathrm{LOAN}_{it} - \mathrm{LOAN}_{it-1}\right)/\mathrm{LOAN}_{it-1}$. The growth rates of loans to SMEs ($\Delta\mathrm{SMELOAN}_{it}$) are defined in the same manner as $\Delta\mathrm{LOAN}_{it}$. [12]

To complete the specifications of Models I and II, we use the above five outcome variables (PD_{it}, NPL_{it}, ROA_{it}, $\Delta\mathrm{LOAN}_{it}$, and $\Delta\mathrm{SMELOAN}_{it}$) as the time-varying covariates $\mathbf{X_{it-1}}$ to be included into an outcome equation. To this end, we consider the policy transmission mechanism that we expect to work. That is, the reduction in the capital-injected banks' default risk induces a write-off of nonperforming loans, and consequently stimulates their profitability and lending. Doing so allows us to further characterize how Japan's banking system attained the policy objectives for the two public capital injections in 1998 and 1999. According to this expected transmission mechanism, we specify each outcome equation in a way that bank default risk (PD_{it}) can determine nonperforming loans (NPL_{it}) as a covariate, but not vice versa, and that bank financial health (PD_{it} and NPL_{it}) can affect profitability

[11] The book value of nonperforming loans is the sum of loans to borrowers in legal bankruptcy and past-due loans for which there have been no payments of interest or principal for six months or more. We use both bank and trust accounts to calculate nonperforming loans. We also define the nonperforming loan ratio as the ratio of these loans to the book value of total assets. The difference in our definition of the nonperforming loan ratio does not make any qualitative difference to our estimation results.

[12] The book values of the loans to domestic enterprises and loans to SMEs include loans for trust accounts as well as those for bank accounts.

and lending (ROA_{it}, $\Delta\mathrm{LOAN}_{it}$, and $\Delta\mathrm{SMELOAN}_{it}$) as covariates, but not vice versa.

Conversely, in terms of the causal relationships between bank profitability and asset growth, previous studies found that profitability is a prerequisite for future asset growth (e.g., Goddard et al. (2004)). Given that most bank assets are loans, their findings suggest that bank profitability (ROA_{it}) should be included into the lending equations of $\Delta\mathrm{LOAN}_{it}$ and $\Delta\mathrm{SMELOAN}_{it}$ as a promising determinant, but bank loans ($\Delta\mathrm{LOAN}_{it}$ and $\Delta\mathrm{SMELOAN}_{it}$) should not be included in the profit equation for ROA_{it}. When specifying bank profit and lending equations in Models I and II, we adopt this recursive causality assumption.

As discussed, Japan's bank public capital injections in 1998 and 1999 are representative of a "too big to fail policy." Hence, to estimate their treatment effects precisely, we should include, as an important covariate, a proxy for bank size, which is expected not only to be closely associated with the treatment indicator D_{it}, but also to be unvarying before and after the public capital injections. Accordingly, we include the one-period lag of the relative size (RSIZE_{it}) of bank i into all the outcome equations as a covariate. We define the relative size (RSIZE_{it}) of bank i at time t as $\mathrm{RSIZE}_i = \ln\left(V_{Ai} / \sum_{j=1}^{n} V_{Aj}\right)$, where V_A is each bank's asset value as defined in the construction of the probability of default, and n is the number of banks listed on the Tokyo Stock Exchange at time t.

Figures 1, 2 and 4 illustrate the historical path of each financial variable from 1997 to 2002. The solid line indicates the path of the treated group that received the two capital injections, and the dashed line is the path of the control group that did not. We point out the following observations. First, Figure 1 indicates that the default risk of the treated group became much higher than that of the control group immediately before the first injection in 1998 but decreased drastically just after. Second, as shown by Figure 2, the bank loans of both the treated and control groups decreased consistently, irrespective of the public capital injections in 1998 and 1999. Third, Figure 4 suggests that SME loans were always higher in the control group, while nonperforming loans were larger in the treated group before the second injection in 1999, whereas they became smaller after the second injection. The path of the return on assets indicates that bank profitability fell sharply in 1999. The historical path of relative size shows that the firm size of the treated group was always considerably larger than that of the control group.

The sample means of the probability of default (PD_{it}), nonperforming loans (NPL_{it}), the return on assets (ROA_{it}), the rate of change in bank loans (ΔLOAN_{it}), the rate of change in SME loans ($\Delta\text{SMELOAN}_{it}$), and the relative size (RSIZE_{it}) in Table 2 confirm the tendency observed in all three figures.

In the following section, we report the estimation results for Models I and II obtained using our bank-level panel data set.

3. Estimation Results with Bank-level Data The estimation results for Model I are in Table 4. All estimates of the treatment effect δ are initially converted to percentages. For each of the treatment-effect estimates, Table 4 reports its level of significance with asterisks and its 95% confidence interval in parentheses, each based on the empirical distribution constructed following Conley and Taber's (2011) method. For estimates of the covariates, Table 4 reports their 95% confidence intervals in parentheses, each based on the large-sample approximations. When calculating the 95% confidence intervals based on the large-sample approximations in Table 4, we use standard errors clustered by both bank and time, as proposed by Petersen (2008). In Appendix I, we discuss Conley and Taber's (2011) method for constructing the empirical distribution.

3.1. **Estimation Results for Models I and II** Column (1) of Table 4 reports the estimation results for Model I in which our measure of bank default risk is specified as the outcome variable. As indicated by the estimates of the treatment effect δ on the probability of default (PD_{it}), the first and second capital injections significantly reduced the default risks of the public capital-injected banks. [13] The estimated coefficients for relative size

[13] There are two issues concerning the estimates of the treatment effect for the first capital injection, considered to have been implemented based on an ineffective policy scheme in which the capital requirements of the capital-injected banks were not fully tested (see, e.g., Allen et al. (2011)). First, our estimation results for the first capital injection are quite consistent with the movement in the Japanese risk premium, as demonstrated by Hoshi and Kashyap (2010). Indeed, the premium peaked at almost 110 basis points in December 1997. However, it started to fall in January 1998, when the government outlined a policy scheme for injecting public funds into problem banks, and ended up falling below 20 basis points in March 1998. In this way, the premium dropped to a much lower value in March 1998. Second, some studies of US public capital injections based on the Troubled Assets Relief Program (TARP) (see, e.g., Greenspan (2010) and Veronesi and Zingales (2010)) provided evidence that it significantly reduced bank default risk, even though it was implemented without a bank stress test to determine the actual capital requirements

(RSIZE_{it}), while not significant, imply that the default risks of larger banks decreased more than those of smaller banks.

The treatment-effect estimates for NPL_{it} in column (2) of Table 4 indicate that the first and second capital injections both reduced nonperforming loans held by the capital-injected banks, while the second did so more significantly than the first. The parameter estimates for the covariates of the default risk indicator (PD_{it}) imply that Japanese banks with higher (lower) default risks held a larger (smaller) amount of nonperforming loans. [14]

When the measure of bank profitability is the outcome variable in Model I, as shown in column (3) of Table 4, estimates of the treatment effect on ROA_{it} imply that the first and second capital injections did not improve the profitability of the capital-injected banks. The parameter estimates of PD_{it} and NPL_{it} are shown not to be significant, but their negative values imply that the profitability of Japanese banks was negatively associated with their financial risks. The parameter estimates of RSIZE_{it} significantly suggest that larger banks were more likely to have worsening profitability.

Columns (4) and (5) in Table 4 report the estimation results of the loan supply functions for Japanese banks. Regarding the estimation results, there are two issues in relation to previous studies of both of the bank public capital injections.

First, our treatment-effect estimates of $\Delta\mathrm{LOAN}_{it}$ and $\Delta\mathrm{SMELOAN}_{it}$ suggest that the first and second capital injections did not have substantial effects on the lending behavior of the capital-injected banks. These estimation results do not support those of Allen et al. (2011) and Giannetti and Simonov (2013), which showed that the second capital injection in 1999 improved the lending behavior of the capital-injected banks, but they do support those of Osada (2011), which indicated that the first and second capital injections in 1998 and 1999 did not improve lending behavior. However, unlike our study, Allen et al. (2011), Osada (2011), and Giannetti and Simonov (2013) measured the responses of bank lending only at the time of the bank public capital injections. Therefore, our estimation results are

of the injected banks.

[14] Hoshi (2001) analyzed the determinants of the nonperforming loans of Japanese banks in the 1980s and Ogawa (2003, Ch. 2) analyzed those in the 1990s. They found that increases in the number of loans to real estate businesses, the construction industry, and the finance and insurance industry were responsible for increases in the number of nonperforming loans.

not directly comparable. [15]

Second, our parameter estimates of the covariates in Model I indicate that the two indicators of bank fragility (the probability of default and nonperforming loans) did not significantly determine bank lending (ΔLOAN_{it} and $\Delta\text{SMELOAN}_{it}$) for our sample period from 1998 to 2002. [16]

Figure 5 depicts the treatment-effect estimates of PD_{it}, NPL_{it}, ROA_{it}, ΔLOAN_{it}, and $\Delta\text{SMELOAN}_{it}$ obtained using Model II. Overall, the estimates of the time-varying treatment effect δ_t obtained with Model II are consistent with those of the treatment effect δ obtained with Model I. More precisely, Figure 5 illustrates that the two public capital injections reduced the default risks of the capital-injected banks. This figure also shows that the second capital injection in 1999 worked particularly well in reducing the number of nonperforming loans of the capital-injected banks. In contrast, as with the estimates of the treatment effect for ROA_{it}, ΔLOAN_{it}, and $\Delta\text{SMELOAN}_{it}$ obtained with Model I, Figure 5 also provides unfavorable evidence about the effect of Japan's public capital injections. Namely, the first and second capital injections in 1998 and 1999 did not improve the profitability and loans to domestic enterprises, including SME loans, of the capital-injected banks. [17]

3.2. **Heterogeneous Effects on the Probability of Default** The previous subsection observed that the two capital injections in 1998 and 1999 decreased the default risk of

[15] Unlike this analysis, Allen et al. (2011), Osada (2011), and Giannetti and Simonov (2013) used a discrete variable at the time of each of the two capital injections in the one equation; that is, their specifications are not based on the difference-in-difference methodology. Such a specification for identifying policy effects does not involve a particular period as the reference period. Hence, it could lead to an incorrect assessment of the policy effects because the value of the outcome variable serves as the reference value for evaluating subsequent policy.

[16] Ito and Sasaki (2002) estimated a loan supply function in Japan from 1990 to 1993, as did Woo (2003) from 1989 to 1997 and Ogawa (2003, Ch. 2) from 1992 to 1999. They all found that an increase in nonperforming loans caused a decrease in bank loans. Hosono (2006) observed that a decrease in the self-capital ratio caused a decrease in bank loans in the 1990s. Osada (2011) found that his bank fragility indicators (the Tier I ratio and the capital ratio) significantly determined bank lending for the sample period from 1993 to 2006. The key difference is the sample period; those earlier studies used a data set covering much of the 1990s, during which the capital crunch resulting from the deterioration of bank assets was particularly severe.

[17] Even when we use the Tier I and capital adequacy ratios as a measure of bank default risk in Models I and II, the results thus far obtained using the probability of default (PD_{it}) are robust.

the capital-injected banks. This subsection introduces the heterogeneous treatment effect corresponding to the amount of capital injected into each bank reported in Table 1, and thereby examines how this affected the default risk of the capital-injected banks.

More precisely, we identify the heterogeneous effect using the following model:

$$\textbf{Model III}: \quad PD_{it} = \mathbf{X_{it}}\beta + \gamma_t t + \delta_q(D_{it}^q) + v_i + \varepsilon_{it},$$

where dummy variable D_{it}^q is set for each bank i depending on the amount of capital injection, and hence its parameter coefficient δ_q captures heterogeneity in the policy effect corresponding to the injected capital. [18] PD_{it} and $\mathbf{X_{it}}$ indicate the probability of default and the covariates, respectively. t denotes a time dummy variable, and v_i denotes the fixed-effects term for each bank. For covariate $\mathbf{X_{it}}$, this subsection uses one-period lags of RSIZE_{it}. For estimation of Model III, we use the within-group estimation method. In addition, to confirm the robustness of the estimation results, we also estimate a version of Model III that includes PD_{it-1} but does not include v_i as an explanatory variable. Table 5 provides the estimation results for the heterogeneous effect corresponding to the amount of capital injected.

We first report the heterogeneous effect of the first capital injection in 1998. In this, some 100 billion yen was injected into 11 of 21 banks, and hence the first capital injection is often referred to as the "yokonarabi (herd behavior) policy." Nevertheless, we observe that the capital injection of 100 billion yen significantly reduced PD_{it}. Given the significant effect, we infer that the overall effect of the first capital injection, as discussed in the previous subsection, primarily reflects this 100 billion yen capital injection. Conversely, Table 4 suggests that the first capital injection did not reduce the default risk of the Long-term Credit Bank of Japan and the Nippon Credit Bank, the former having received the largest capital injection of 176.6 billion yen and the latter having received 60 billion yen. The estimation results for the two banks are consistent with the fact that they both fell into bankruptcy after the first capital injection. Our estimates of the heterogeneous effect imply that for the first capital injection, the difference in capital injected into each bank

[18] See Wooldridge (2005) for conditions under which fixed-effects regression estimators can yield consistent estimates of the heterogeneous treatment effect.

did not make any quantitative difference to the amount by which the default risk fell.

Next, we report the heterogeneous effect of the second capital injection in 1999. The second injection significantly reduced PD_{it} in more cases than did the first injection. Such a favorable result for the second injection may be because the capital of injected banks was initially adequate. Unlike the first injection in 1998, the second injection was only given after a bank stress test was used to determine each injected bank's capital requirements (see, e.g., Allen et al. (2011) and Hoshi and Kashyap (2010)). On the other hand, although Daiwa Bank and Asahi Bank, which later merged to form Resona Bank, received capital injections of 408 billion yen and 500 billion yen, respectively (see Table 1), the second capital injection did not significantly reduce their default risks. Finally, for the three banks that received the largest capital injections, Sakura Bank (800 billion yen), Daiichi Kangyo Bank (900 billion yen), and Fuji Bank (1,000 billion yen), Table 4 indicates that the second capital injection reduced their default risks significantly. Our empirical results for the second capital injection imply that as long as there was a bank stress test to determine the injected bank's capital requirements, the difference in the size of the capital injection for each bank possibly accounted for a quantitative difference in the amount by which the default risk fell.

3.3. **Robustness Check** As a robustness check, this subsection addresses three issues concerning our difference-in-difference analysis developed in the previous subsections: 1) selection of observed covariates $\mathbf{X}$ included in Models I and II; 2) the validity of the unconfoundedness assumption (1); and 3) the changes in the risk-weighted assets of capital-injected banks.

3.3.1. **Selection of Observed Covariates** If a set of covariates includes variables that are themselves affected by a treatment, there can be bias in the resulting estimators of the treatment effect, as pointed out by Rosenbaum (1984). We thus specify only each bank's relative size (RSIZE_{it}) as a time-varying observed covariate, thereby checking the plausibility of our treatment-effect estimates. Given that larger Japanese banks received the first and second capital injections in 1998 and 1999, relative size, although not directly affected by the capital injections, would be closely associated with a bank's decision about whether it entered the recapitalization programs: D_{it} in Models I and II.

The estimated treatment effects of the four outcome variables (NPL_{it}, ROA_{it}, $\Delta\mathrm{LOAN}_{it}$, and $\Delta\mathrm{SMELOAN}_{it}$) obtained by including only relative size as a time-varying observed covariate in Models I and II are qualitatively the same as those reported in Subsection 3.1. These estimation results indicate that our causal analysis is robust with respect to the selection of the observed covariates. [19]

3.3.2. **Unconfoundedness** Our causal analysis based on the difference-in-difference estimator depends critically on the unconfoundedness assumption (1), but the key assumption is not directly testable. We hence employ the falsification test, thereby alternatively ensuring that the observed changes in the outcome variables are more likely from the public capital injections, as suggested by Imbens (2004) and Roberts and Whited (2012). This test focuses on estimating the causal effect of a treatment on a lagged outcome. If the estimated treatment effect is statistically indistinguishable from zero, it implies that the observed change after the treatment is likely to be because of it, and not to some alternative force; consequently, this reinforces the expectation that unconfoundedness holds.

Table 6 provides falsification test results obtained by adding pretreatment outcome variables to Model I for the sample period from September 1995 to September 1997. As clearly shown, all estimated treatment effects are statistically indistinguishable from zero. This indicates that the unconfoundedness assumption (1) is plausible in our difference-in-difference analysis.

3.3.3. **Risk-weighted Assets** In Subsection 3.1, we found that the two capital injections in 1998 and 1999 did not stimulate lending by the injected banks, although they did significantly reduce financial risk. Here, we reexamine this evidence in terms of the changes in the risk-weighted assets of the banks.

We obtained the estimated treatment effects by using the rate of change in the risk-weighted assets as an outcome variable in Model II that includes only bank relative size (RSIZE_{it}) as a time-varying observed covariate. We found that the risk-weighted assets of

[19] We also use a bank-funding variable including bank deposits as a time-varying observed covariate, but the estimation results are again qualitatively the same as those obtained otherwise.

the capital-injected banks did not increase in all sample periods, except September 2000. [20] This evidence is consistent with the lack of loan expansion by the capital-injected banks.

4. Treatment Effect in Loan-level Specification In the previous section, we observed that the first and second capital injections in 1998 and 1999 likely reduced the financial risk of the capital-injected banks through recapitalization and the write-off of nonperforming loans. Furthermore, we also observed that the two capital injections did not significantly improve the lending behavior of the capital-injected banks.

The estimated coefficients for the time-varying observables in our loan supply functions showed that financial risk factors in Japanese banks, such as the probability of default and nonperforming loans, do not explain their lending behavior after the public capital injections. In fact, the estimation results suggest that overall bank lending after the public capital injections did not depend on financial risk. [21]

Why did the lending behavior of the capital-injected banks not improve, even though their financial conditions did? Was there scope to improve bank lending using the two capital injections in the first place? In this and the following sections, we address these questions. To this end, we thoroughly exploit a matched sample of Japanese banks and their listed borrowing enterprises, thereby elaborating on our specification of the loan supply function. More precisely, we additionally introduce borrower-side factors into the loan supply function to examine bank lending in more depth after the public capital injections.

[20] The estimation results are available from the authors upon request. From this estimation result for risk-weighted assets, we should not simply infer that the risk-taking behavior of the capital-injected banks did not change. This is because their shift toward riskier assets may have occurred within the same asset class, and therefore remained undetected using risk-weighted assets, as demonstrated in the US study by Duchin and Sosyura (2014). They found that US capital-injected banks increased credit issuance to riskier firms, as measured by borrowers' cash flow volatility and interest coverage, and reduced credit issuance to safer firms. Consequently, unlike the bank capital injections in Japan, TARP in the US increased the default risk of the capital-injected banks. Black and Hazelwood (2013) provided evidence that larger capital-injected banks shifted their lending toward riskier loans, as proxied by the banks' own risk rating. In contrast to these US studies, Berger et al. (2014) analyzed public capital injections in Germany, showing that these reduced the risk taking of capital-injected banks, and did not contribute to liquidity creation.

[21] Montgomery (2005) demonstrated theoretically that a bank's lending does not depend on its financial risk when the bank is not strictly subject to capital ratio regulation, while an increase in a bank's financial risk reduces its lending when the capital ratio is small or the supervision of the capital ratio is strict.

4.1. **Loan-level Specification and Estimation Method** Here, we start by reexamining the treatment effect of the public capital injections on the lending behavior of the capital-injected banks using our loan-level data set. In particular, we focus on the effect of the second recapitalization program in March 1999, because some studies of Japanese bank public capital injections, including those of Allen et al. (2011), Osada (2011), and Giannetti and Simonov (2013), have argued the pros and cons of the effect of the second program, as discussed in Subsection 3.1.

Matching lender-side information to borrower characteristics helps us to examine which factors determine bank lending because it allows us to exploit the cross-sectional heterogeneity in both lenders and borrowers. To exploit the advantage of a matched sample, we specify a loan supply function in a three-way fixed-effects regression model. To include the concept of the treatment effect of Model II, proposed in Subsection 2.2, we specify our loan supply function as follows:

$$\textbf{Model IV}: \Delta\mathrm{LOAN}_{it}^{j} = \mathbf{X_{it-1}}\beta + \mathbf{X_{t-1}^{j}}\beta^{*} + \mathbf{X_{it-1}^{j}}\beta^{**} + \gamma_t t + \delta_t(t \cdot D_{it}) + v_i + v^j + \varepsilon_{it}^{j},$$

where the definition of the treatment indicator D_{it} conforms to that defined in Subsection 2.2. $\Delta\mathrm{LOAN}_{it}^{j}$ indicates the growth rate of the total amount of loans outstanding between domestic listed company j and bank i at time t. [22] $\mathbf{X_{it-1}}$ and $\mathbf{X_{it-1}^{j}}$ are one-period lags of time-varying observed covariates to capture the financial risks and the profitability of bank i and listed enterprise j that borrows from bank i, respectively. $\mathbf{X_{it-1}^{j}}$ are time-varying observables used to capture the characteristics of the bank–firm relationship. t is a time dummy variable to control for the common factors for the Japanese bank loan market at time t, and v_i and v^j are bank and firm fixed effects to capture the respective time-invariant unobserved characteristics. The fixed-effects terms v_i and v^j could be correlated not only with the time-varying covariates, but also with each other.

The crucial difference between our Model IV and those developed by Giannetti and

[22] It is noteworthy that we cannot include bank- or firm-level variables as an outcome variable in the loan-level specification of Model IV in place of $\Delta\mathrm{LOAN}_{it}^{j}$, and hence we cannot conduct robustness checks of treatment-effect estimates for a bank's financial risks (PD_{it} and NPL_{it}), profitability (ROA_{it}), and SME loans ($\Delta\mathrm{SMELOAN}_{it}$) obtained in Section 3.

Simonov (2013), who also used a matched sample of Japanese banks and their borrowers, is that unlike Giannetti and Simonov (2013), Model IV includes lender- and borrower-side time-varying covariates as $\mathbf{X_{it}}$ and $\mathbf{X^j_{it}}$. Another difference is that Model IV measures the duration effect of the public capital injection, while they measured the responses of bank lending only at the time of the public capital injection. Accordingly, our treatment-effect estimates are not directly comparable with the results in Giannetti and Simonov (2013).

To identify the treatment effect δ_t of the second public capital injection using Model IV, the unconfoundedness assumption (1) proposed in Subsection 2.2 should be changed as follows:

$$E(y^j_{0it}|D_{it},\ \mathbf{X_{it-1}},\ \mathbf{X^j_{t-1}},\ \mathbf{X^j_{it-1}},\ t,\ v_i,\ v^j) = E(y^j_{0it}|\mathbf{X_{it-1}},\ \mathbf{X^j_{t-1}},\ \mathbf{X^j_{it-1}},\ t,\ v_i,\ v^j),$$

where $y^j_{0it} = \Delta\mathrm{LOAN}^{\mathrm{j}}_{\mathrm{0it}}$, the counterfactual loan growth that would be realized if a capital-injected bank was not recapitalized.

When estimating the loan supply function based on the above three-way fixed-effects regression model, we employ the estimation method developed by Abowd et al. (1999) and Andrews et al. (2008). This estimation method yields consistent and unbiased parameter estimates, for not only the time-varying observed covariates of both the lender- and borrower-side factors, but also for their two types of unobserved fixed effects. [23]

As pointed out by Abowd et al. (1999) and Andrews et al. (2008), using dummy variables to estimate Model IV in the full least-squares estimation of the parameter vector $\left[\beta',\ \beta^{*\prime},\ \beta^{**\prime},\ \gamma_t,\ \delta_t,\ v_i,\ v^j\right]'$ is not feasible because the dimension of the parameter vector is too large. In the framework of Model IV, the fixed-effects estimation method of Abowd et al. (1999) and Andrews et al. (2008) suggests that explicitly including dummy variables

[23] However, a cost of the estimation method is that it requires a strict exogeneity condition, namely:

$$E(\varepsilon^j_{it}|\mathbf{X_{i1}},\ldots,\mathbf{X_{iT-1}},\ \mathbf{X^j_1},\ldots,\mathbf{X^j_{T-1}},\ \mathbf{X^j_{i1}},\ldots,\mathbf{X^j_{iT-1}},\ t,\ t\cdot D_{it},\ v_i,\ v^j) = 0.$$

This exogeneity condition implies that bank and firm matches are exogenous. Therefore, Model IV cannot deal with endogeneity biases that might arise if bank and firm matches are not random. Endogenous matching in bank–firm relationships is an important econometric problem, but in the following analyses, we maintain the assumption of exogenous matching because we are most interested in revealing the role of borrower-side factors in bank decisions on lending after the public capital injections. Abowd et al. (1999) examined the issue of omitted variable bias in estimating wage-setting functions.

for bank heterogeneity v_i but sweeping out the firm heterogeneity v^j by forming within-firm mean deviations for all the variables in Model IV gives consistent and unbiased estimates for the six parameters $\left[\hat{\beta}', \ \hat{\beta}^{*\prime}, \ \hat{\beta}^{**\prime}, \ \hat{\gamma}_t, \ \hat{\delta}_t, \ \hat{v}_i\right]'$. After estimating the within-firm transformed equation, the firm heterogeneity v^j can be recovered as follows:

$$v^j \;=\; \Delta\mathrm{LOAN}^{(j)} - \mathbf{X_i^{(j)}}\hat{\beta} - \mathbf{X^{(j)}}\hat{\beta}^{*} - \mathbf{X_i^{j(j)}}\hat{\beta^{**}} - \gamma^{(j)} - \delta_i^{(j)} - v_i^{(j)},$$

where $\Delta\mathrm{LOAN}^{(j)}$, $\mathbf{X_i^{(j)}}$, $\mathbf{X^{(j)}}$, $\mathbf{X_i^{j(j)}}$, $\gamma^{(j)}$, $\delta_i^{(j)}$ and $v_i^{(j)}$ average $\Delta\mathrm{LOAN}_{it}^j$, $\mathbf{X_{it-1}}$, $\mathbf{X^j_{t-1}}$, $\mathbf{X^j_{it-1}}$, $\hat{\gamma}_t t$, $\hat{\delta}_t(t{\cdot}D_{it})$, and $\hat{v}_i$ over time for each firm j, respectively. Following Andrews et al. (2008), we refer to this estimation procedure as fixed-effects least-squares dummy variable (hereafter FELSDV) estimation. The estimation results reported in the following subsections employ the FELSDV estimation method.

4.2. **Loan-level Data Set and Estimation Results** We define LOAN_{it}^j as the total amount of loans outstanding by adding short-term debt with a maturity of one year or less to long-term debt with a maturity of more than one year and then define its growth rate as $\Delta\mathrm{LOAN}_{it}^j = (\mathrm{LOAN}_{it}^j - \mathrm{LOAN}_{it-1}^j)/\mathrm{LOAN}_{it-1}^j$.

For the lender-side covariates $\mathbf{X_{it-1}}$, we use the one-period lags of the logarithmic values of the leverage ratio ($\mathrm{LEV}_{it} = \ln(D_{it}/V_{Ait})$) and the asset volatility ($\ln\sigma_{Ait}$), as defined in Subsection 2.3, to examine which components of PD_{it} are responsible for bank lending after the public capital injections. Additionally, we use the one-period lag of NPL_{it}, defined in Subsection 2.3, as another proxy for bank i's financial risks. As a proxy for the profitability of bank i, we include the one-period lag of ROA_{it}. Furthermore, we use the one-period lag of the logarithmic value of the bank's assets ($\mathrm{SIZE}_{it} = \ln V_{Ait}$) to control for bank size.

For the borrower-side covariates $\mathbf{X^j_t}$, we use the one-period lags of the logarithmic values of the leverage ratio (LEV_t^j) and the asset volatility ($\ln\sigma_{At}^j$), which are the main components of the probability of the default of borrower j (PD_t^j). As another proxy for the default risk of domestic listed company j, we also include its interest coverage ratio (ICR_t^j). The interest coverage ratio is defined by dividing EBIT, or the borrower's earnings before interest and taxes, by total interest payments, expressed in percentage terms. We additionally use the one-period lag of the return on assets (ROA_t^j) to examine whether the profitability of borrower j determines the lending behavior of bank i. To control for the firm size of borrower j, we include the one-period lag of the logarithmic value of the borrower's assets ($\text{SIZE}_t^j = \ln V_{At}^j$). The procedure for constructing these covariates for borrower j is the same as that for those of lender i, which is discussed in Subsection 2.3.

We also include the one-period lag of the investment of borrower j (INVEST_t^j) as a borrowers' loan demand factor in the borrower-side covariates $\mathbf{X_t^j}$. If the estimated coefficients on INVEST_t^j are not significant, but those on the financial risk factors for borrower j, including the leverage ratio and asset volatility, are significant, we expect that deterioration in borrower creditworthiness and banks' increased perception of the riskiness of lending prevented banks from lending more. We define the investment of borrower j by taking the log-differences of fixed assets.

To examine whether the public capital injections allowed so-called "zombie firms," which received subsidized credit in terms of their interest payments, to borrow more, we include a zombie firm dummy (ZOMBIE_{t-1}^j) among the covariates $\mathbf{X_t^j}$. Several existing studies have pointed out the potential misallocation of bank loans in Japan (see, e.g., Peek and Rosengren (2005) and Caballero et al. (2008)). In particular, Giannetti and Simonov (2013) showed the possibility that undercapitalized banks after Japan's public capital injections extended their loans to zombie firms. Unlike these studies, we do not classify overcapitalized and undercapitalized banks; thus, we simply assess whether banks increased their supply of credit to zombie firms. Our construction of the zombie firm dummy follows that of Caballero et al. (2008) to identify the zombie firms and uses the interest payment gap between the actual interest payments made by the firms and the hypothetical minimum interest payments proposed by Caballero et al. (2008). [24] If the interest payment gap of borrowing firm j takes a negative value, the firm is defined as a zombie: $\text{ZOMBIE}_{t-1}^j = 1$. If the interest payment gap takes a positive value, the zombie dummy variable is set as

[24] The minimum interest payments for each year proposed by Caballero et al. (2008) are from the average short-term prime rate as the lower bound of short-term bank loan prices, the average long-term prime rate as that for long-term bank loan prices, and the minimum observed coupon rate on any convertible corporate bond as that for bond prices. To construct the minimum interest payments, we use the average yield on Moody's Aaa-rated corporate bonds with a remaining maturity of 10 years in place of the minimum coupon rate on convertible corporate bonds. The data on average short- and long-term prime rates each year are from the Bank of Japan. The data for the average yield on Moody's Aaa-rated corporate bonds are from NRI.

$\text{ZOMBIE}^j_{t-1} = 0$.

The relationship factors $\mathbf{X^j_{it-1}}$ contain the one-period lags of bank i's lending exposure to firm j (EXPLEND^j_{it}) and firm j's borrowing exposure from bank i (EXPBORROW^j_{it}). The former is calculated as bank i's loans to firm j as a percentage of its total loans to firm j, while the latter is calculated as firm j's loans from bank i as a percentage of its total loans from bank i. In addition to including the two exposure variables, the relationship factors also include the one-period lag of the duration of the relationship between lender i and its borrowing firm j (DURATION^j_{it}) calculated as its logarithmic value. [25]

Considering the fact that the second public capital injection was conducted at $t^* =$ March 30, 1999 (the end of FY1998), it is reasonable to set the reference point at $t^* - 1 =$ FY1998 for applying the difference-in-difference estimation method to Model IV. Thus, our sample period for estimation of Model IV ranges from $t =$ FY1998 to $t =$ FY2002 and hence we measure the treatment effect δ_t from $t =$ FY1999 to $t =$ FY2002.

Table 7 reports the estimation results for Model IV. All estimates of the treatment effect δ_t are initially converted to percentages. Furthermore, for each of the estimates, Table 7 reports its level of significance with asterisks and its 95% confidence interval in parentheses, each based on the empirical distribution constructed following Conley and Taber's (2011) method. For estimates of the covariates, Table 7 reports their 95% confidence intervals in parentheses, each based on the large-sample approximations and their standard errors clustered by the lender–borrower relationship as well as time. In Appendix I, we discuss Conley and Taber's (2011) method for constructing the empirical distribution.

Our estimation results for Model IV in columns (1)–(3) clearly show that all the treatment-effect estimates are not statistically significant at the 10% level. These estimation results for the treatment effect of the second recapitalization on the lending behavior of the capital-injected banks are consistent with those obtained using our bank-level panel

[25] Peek and Rosengren (2005) focused on the relative importance of a borrowing firm from the lender's viewpoint in the estimation of their loan supply equation, thus using the bank's lending exposure with a matched sample of Japanese banks and their borrowers. From the borrower's viewpoint, Dass and Massa (2011) focused on the relative importance of a firm's bank loans, using the firm's loan-to-asset ratio with US firm-level panel data, but not using the firm's borrowing exposure as in our study. Ongena and Smith (2001) analyzed the duration of bank relationships using a matched sample of Norwegian banks and their borrowing firms.

data, as reported in Section 3.

Also note that bank loans to domestic listed companies are not determined by lender-side financial risks (LEV_{it}, $\ln \sigma_{Ait}$ and NPL_{it}) but by borrower-side financial risks (LEV_t^j, $\ln \sigma_{At}^j$ and ICR_t^j) in the postinjection period. These estimation results obtained using our loan-level data are consistent with those obtained using bank-level panel data in Section 3 in that lender-side financial risks do not explain the lending behavior of Japanese banks after the second public capital injection. [26]

The estimated coefficients on the time-varying observed covariates yield significant evidence that bank loans to zombie firms decreased (ZOMBIE_t^j) and bank–firm relationships of longer duration decreased the supply of credit more (DURATION_t^j). In contrast, the investment motives of borrower j (INVEST_t^j) do not significantly determine the loans as a loan demand factor. This implies that we cannot attribute sluggish bank lending after the second public capital injection to the decrease in borrowers' investment motives.

Table 7 also provides the sample means of the estimated bank and firm fixed effects. The sample means of the estimated firm fixed effects, accompanied by substantially negative values, are much smaller than the estimated bank fixed effects. In Section 5, we explore the implications of the estimated bank and firm fixed effects in depth.

This subsection remeasured the treatment effects of the second recapitalization program in the loan-level specification. As with the bank-level specification, the loan-level specification suggests that the second recapitalization program did not improve bank lending in terms of causal inference.

4.3. **Parsimonious Specification in Loan-level Data** Khwaja and Mian (2008) developed a fixed-effects approach to identify the causal effects of bank financial shocks with loan-level matched data. [27] In this subsection, we extend Khwaja and Mian's (2008) approach using the framework of our three-way fixed-effects methodology based on the FELSDV estimation in Abowd et al. (1999) and Andrews et al. (2008), thereby checking

[26] We additionally estimate Model IV using the probability of default (PD_t^j and PD_t^j of banks and firms) and the capital adequacy ratio of banks as a proxy of financial risk, but the estimation results obtained using the alternative financial variables do not differ qualitatively from those shown in Table 7.

[27] Giannetti and Simonov (2013) employed Khwaja and Mian's (2008) fixed-effects approach to identify the effects of Japan's public capital injections.

the plausibility of our causal analysis. To this end, we introduce the following parsimonious loan supply model:

$$\textbf{Model V}: \Delta\text{LOAN}_{it}^{j} = \mathbf{X_{it-1}}\beta + \mathbf{X_{it-1}^{j}}\beta^{*} + \delta_t(t \cdot D_{it}) + v_i + v^j \cdot t + \varepsilon_{it}^{j},$$

where all potential borrower-side factors are embodied as a time-varying firm unobservable: $v_t^j = v^j \cdot t$. This method of controlling for borrower-side factors on the basis of firm fixed effect v^j is due to the ingenuity of Khwaja and Mian's (2008) approach because the method can identify the causal effects of the public capital injections using the difference-in-difference estimator, if the time-varying firm unobservable v_t^j can fully control for all potential borrower-side factors. [28] The advantage of our three-way fixed-effects methodology over Khwaja and Mian's (2008) fixed-effects approach is that our methodology can estimate the time-varying firm unobservable, $v_t^j = v^j \cdot t$, using the FELSDV estimation method, thus allowing us to examine the role that borrower-side factors played in determining loan supply.

Table 8 reports the estimation results for Model V obtained using the total sample from t = FY1999 to t = FY2002 (the left-side panel) and the subsample consisting of borrowing firms that have multiple relationships with both the capital-injected and non-capital-injected banks (the right-side panel). Khwaja and Mian (2008) originally employed the latter subsample approach. The difference-in-difference estimator based on the subsample can control for potential borrower-side factors more fully than that based on the total sample because it can compare changes in the loan supplies of the capital-injected and non-capital-injected banks before and after the second capital injection for each firm. Therefore, although the subsample approach substantially reduces the number of observations, as shown in Table 8, it allows us to identify more accurately the effects of the capital injections.

[28] We found that some borrower-side factors are highly correlated with each other (e.g., ROA_t^j and INVEST_t^j). Hence, this could be responsible for the insignificance of the borrower-side observable, as reported in Table 7. The parsimonious model allows us to avoid correlation problems arising from the inclusion of numerous borrower-side observables. Hosono and Miyakawa (2014) employed Khwaja and Mian's (2008) fixed-effects approach with Japanese loan-level matched data, thereby identifying the effects of monetary policy on bank loan supply.

As shown in Table 8, although R^2 greatly improves Model IV, the estimation results reported therein do not appear to be qualitatively different from those reported in Subsection 4.2. That is, the estimated treatment effects of the second capital injection in 1999 are not statistically significant and the lender-side covariates do not determine bank lending after the second capital injection.

Regarding the estimation results, there are two additional remarks. First, the estimation results from the total sample are qualitatively the same as those from the subsample consisting of firms that borrow from both the capital-injected and non-capital-injected banks. Second, and more importantly, the sample means of the borrower-side factor, $v_t^j = v^j \cdot t$, have substantially negative values, indicating that borrower-side factors would contribute to suppressing the supply of bank loans.

In sum, even when controlling for lender-side characteristics with Khwaja and Mian's (2008) fixed-effects approach, this subsection confirms the evidence thus far that the second recapitalization program did not improve bank lending.

5. Bank Lending after the Public Capital Injection We have so far empirically demonstrated that Japan's public capital injections in 1998 and 1999 did not improve the lending behavior of the capital-injected banks. In this section, we scrutinized which factors impeded bank lending after the public capital injections, lender- or borrower-side factors. To this end, when estimating the loan supply functions, we divide the sample of banks into capital-injected and non-capital-injected banks to check whether the lending behavior of these banks differed. Thus, we focus on the coefficient estimates of not only the observed but also the unobserved covariates.

We fine-tune Model IV, as proposed in Subsection 4.1, and introduce our loan supply function as follows:

$$\textbf{Model IV}^*: \quad \Delta\mathrm{LOAN}_{it}^j \;=\; \mathbf{X_{it-1}}\beta + \mathbf{X^j_{t-1}}\beta^* + \mathbf{X^j_{it-1}}\beta^{**} + \theta r_{it} + \gamma_t t + v_i + v^j + \varepsilon_{it}^j.$$

Model IV* newly includes the lending interest rate of lender i, r_{it}, as a proxy of the price of bank loans. The lender's total lending rate r_{it} is constructed by dividing total interest revenues by the book value of loans for domestic enterprises and is expressed in percentage terms. Given the fact that the growth rate of a bank loan, ΔLOAN^j_{it}, is endogenously determined depending on the price of bank loans, we should include a loan interest rate offered by bank i to borrower j, r^j_{it}, by using bank i's interest revenue from borrower j. However, we have limited access to data about each bank's interest revenue from each borrower. Thus, we use the best available substitute: the total lending interest rate. [29] We construct the lender's total lending rate by dividing its total interest revenues by the book value of its loans for domestic enterprises, and we express it in percentage terms.

For the estimation of Model IV* that includes the lender's total lending rate r_{it}, to deal with the endogeneity of the lending rate, we employ two-stage least-squares estimation in the framework of the FELSDV estimation methodology. We use the one-period lag of the lending interest rate, r_{it-1}, as an instrumental variable. For estimation of Model IV* that does not include the lending interest rate, we simply employ the FELSDV estimation method.

We estimate various specifications of Model IV* as a robustness check. For example, as a proxy for bank financial health, we additionally use its capital surplus (CAP^i_{t-1}), defined by subtracting the target capital ratio (8% for international banks and 4% for domestic banks) from the reported capital ratio. The sample period used for estimation of Model IV* is from FY1998 to FY2002.

5.1. **Estimation Results for Injected and Noninjected Banks** Tables 9 and 10 report the estimation results for Model IV*. We make the following remarks concerning the estimation results of Model IV*.

First, as shown in columns (1)–(6) of Tables 9 and 10, bank loans to domestic listed companies are not determined by lender-side financial risk (PD_{it}, LEV_{it}, $\ln\sigma_{Ait}$, CAP^i_{t-1} and NPL_{it}) but by borrower-side financial risk (PD^j_t, LEV^j_t, $\ln\sigma^j_{At}$, and ICR^j_t). As for lender- and borrower-side profitability, the growth rates of bank loans after the public capital injections are not significantly determined by the profitability of both Japanese banks (ROA_{it}) and their borrowing enterprises (ROA^j_t). This indicates that the deterioration of

[29] The use of the total lending rate, r_{it}, is based on our two expectations; namely, (i) bank i sets its lending interest rates at the same level, and (ii) changes in our proposed proxy are highly correlated with changes in the loan interest rate offered by bank i to borrower j, r^j_{it}.

lender-side and borrower-side profitability shown in Figure 3 would not be responsible for the sluggish bank lending after the public capital injections.

Second, and most importantly, the above results for the injected and noninjected banks are not qualitatively different from each other; lending, not only by the injected banks but also by the noninjected banks, is more sensitive to borrower-side financial risks. [30]

Third, as reported in columns (1)–(6) of Table 10, the investment motives of borrower j (INVEST_t^j) do not significantly determine the loans made by both the injected and noninjected banks as a loan demand factor.

For the zombie firm dummy (ZOMBIE_t^j), columns (3) and (6) in Table 10 show that both the capital-injected and non-capital-injected banks significantly decreased their supply of credit to zombie firms. From our estimation results for the zombie firm dummy, we suggest that Japanese banks did not provide subsidized credit to zombie firms during the postinjection period, but in fact actively decreased loans to them.

Regarding the estimation results of the three relationship factors, bank i's lending exposure to firm j (EXPLEND_{it}^j) and firm j's borrowing exposure to bank i (EXPBORROW_{it}^j) do not significantly determine the loans of the injected and noninjected banks, while the duration of the relationship between lender i and its borrowing firm j (DURATION_{it}^j) significantly determines them. [31]

Finally, the estimated coefficients on our proxy of the prices of bank loans (r_{it}) in columns (1) and (4) of Table 10, although not significant, are positive, and therefore consistent with theory.

Our estimation results indicate that we can attribute the sluggish loan supply of Japanese banks shown in Figure 2 to the deterioration in the creditworthiness of their borrowing

[30] Based on the estimation results reported in Tables 9 and 10, we conducted a cross-model Wald test of the equality of the estimated coefficients across capital-injected and non-capital-injected banks. All the lender-side factors except for SIZE_{it} did not produce significantly different estimates between the two groups, while all the borrower-side factors except for INVEST_t^j produced significantly different estimates.

[31] For the bank's lending exposure, Peek and Rosengren (2005), who used a matched sample of Japanese banks and their borrowing firms, reported positive and significant coefficients for the sample period from 1993 to 1999. Our estimation results for the lending exposure, based on the sample period after FY1998 (that is to say, March 1999), are different from theirs. For the duration of the bank relationship, on the other hand, our negative and significant coefficients for the duration are consistent with the finding of Ongena and Smith (2001) suggesting that the value of relationships declines over time.

firms, as reflected by the deterioration in borrower-side financial risk shown in Figure 3, but not to the decrease in borrowers' investment motives.

5.2. **Unobserved Heterogeneities** This subsection reports the roles that the bank and firm fixed effects, v_i and v^j, played in determining bank loans after the public injections. Tables 7 to 10 contain the sample means of the estimated bank and firm fixed effects.

These tables clearly show that for both the capital-injected and non-capital-injected bank loans, the sample means of the estimated firm fixed effects, accompanied by substantially negative values, are much smaller than the estimated bank fixed effects. Also, note that, as shown in Tables 9 and 10, the estimated firm fixed effects for the injected banks' loans are smaller than the noninjected banks' loans. To explore the implications of the estimated bank and firm fixed effects, this subsection reports the intercorrelations among components of the growth rate of bank loans. Each of the components is calculated using the parameter estimates of Model IV* reported in columns (3) and (6) of Table 10.

The firm fixed effect v^j can be decomposed into two components: one part is due to an industry effect attributed to an industry group to which borrowing firm j belongs and the other is due to its purely unobserved characteristics. Hence, we also report the industry effect $v^j_{industry}$ and the purely unobserved characteristics v^{j*}. To decompose the firm fixed effect v^j, following Abowd et al. (1999) and Andrews et al. (2008), we estimate the auxiliary regression:

$$v^j = \mathrm{INDUSTRY}^{\mathrm{j}}\eta + u^j,$$

where $\mathrm{INDUSTRY}^{\mathrm{j}}$ is a vector of industry dummy variables indicating an industry group to which borrowing firm j belongs. u^j is the stochastic error term. After estimating the auxiliary regression using the generalized least-squares estimation method, we compute the industry effect $v^j_{industry}$ as $\mathrm{INDUSTRY}^{\mathrm{j}}\hat{\eta}$ and the purely unobserved effect v^{j*} as $v^j - \mathrm{INDUSTRY}^{\mathrm{j}}\hat{\eta}$. We set up industry dummy variables according to the 33 industry sectors defined by the Securities Identification Code Committee in Japan.

The firm fixed effect v^j and its purely unobserved part v^{j*} are the components of bank loans that are most highly correlated with the growth rate of bank loans (0.442 to 0.491, depending on the injected or noninjected bank loans). On the other hand, the bank fixed effect v_i is much less important in the determination of bank loans after the public capital injections (0.012 or 0.051, depending on the injected or noninjected bank loans). The bank and firm fixed effects, v_i and v^j, are negatively correlated: -0.030 for the injected banks' loans and -0.066 for the noninjected banks' loans. Therefore, the estimated correlation between the two unobserved heterogeneities is not large. Also note that although the firm fixed effect v^j and the industry effect $v^j_{industry}$ are positively correlated, the industry effect is not substantive in the determination of the bank loans.

The estimated correlation between the bank loans and the time-varying observable factors is smaller than that between the bank loans and the firm-specific unobserved heterogeneity v^{j*}. Nevertheless, among the time-varying observable factors, the lender-side factors $\mathbf{X^j_{t-1}}\hat{\beta}^*$ are the most important and substantive in the determination of the bank loans (0.307 or 0.335, depending on the injected or noninjected bank loans). On the other hand, the time-varying bank and relationship factors, $\mathbf{X^j_{t-1}}\hat{\beta}^*$ and $\mathbf{X^j_{it-1}}\hat{\beta}^{**}$, are much less important in explaining the bank loans. It is also noteworthy that the time-varying lender-side and borrower-side factors are positively highly correlated with the bank- and firm-specific unobserved heterogeneities, respectively.

We report the intercorrelations among the bank- and firm-specific unobservables (v_i and v^{j*}) and the time-varying observables. The firm-specific unobserved heterogeneity v^{j*}, which is the most substantive in explaining bank lending after the public capital injections, is highly negatively correlated with the two financial risk factors of the leverage LEV^j_{t-1} and the volatility $\ln \sigma^j_{At-1}$, and highly positively correlated with the firm size SIZE^j_{t-1}. Therefore, the value of the firm unobserved characteristic v^{j*} decreases according to the increases in its financial risk and according to the decrease in its size.

Similarly, the bank-specific unobserved heterogeneity v_i is highly negatively correlated with leverage LEV_{it-1} and volatility $\ln \sigma_{Ait-1}$. The unobserved heterogeneity is highly negatively correlated with bank size SIZE_{it-1} for the injected banks' loans, but it is highly positively correlated with bank size for the noninjected banks' loans. Hence, the bank-specific unobservables, whose sample means take positive values in Tables 7 to 10, are likely to embody the decrease in financial risk faced by relatively small injected banks and relatively large noninjected banks, although they are not important in explaining bank lending after the public capital injections.

Additional observations include the fact that the firm-specific unobservables positively correlate with the duration of the lender–borrower relationship. Given that the sample means of the estimated firm fixed effects are substantially negative, the firm-specific unobserved heterogeneity after public capital injections is likely to embody the increase in the financial risk faced, particularly by relatively small listed firms whose relationships with banks have a relatively short duration.

The analysis conducted in this subsection suggests that borrowers' unobserved characteristics played a role in determining bank loans after the public capital injections, comparable to or more substantive than their time-varying observed covariates. Previous studies of bank lending functions have ignored such a role for borrowers' unobserved characteristics. Given that lender-side factors are much less important, as not only time-varying observables, but also as time-invariant unobservables, borrower-side factors including their time-varying observables, and also as time-invariant unobservables, appear to be more critical in explaining Japan's sluggish bank lending after the public capital injections.

5.3. **Insights into Japan's Capital Injections** Our estimation results for the time-varying observed covariates, as discussed in Subsection 5.1, indicate that the increased perception by lenders of the riskiness of lending based on deterioration in the creditworthiness of borrowers caused by the increase in financial risk is primarily responsible for impeding lending, not only of the injected banks, but also of the noninjected banks. This insight is robust because borrowers' unobserved heterogeneities are the most important in explaining bank loans after the public capital injections, and the substantive negative values of their sample means likely embody the increase in borrowers' financial risk, as reported in Subsection 5.2.

The empirical study of bank lending after TARP-related capital injections by Berrospide and Edge (2010) attributed the US slowdown in loan growth after the capital injections to the US recession and banks' accompanying increased perception of riskiness of lending, but not to their capital position. [32] Their insight into US bank lending after the capital injections highlights the theoretical view suggested by Bernanke and Gertler (1989) and Bernanke et al. (1999): the deterioration of borrower creditworthiness in a severe recession can lead to an increase in agency costs associated with lending, thus resulting in a decrease in the bank credit supply. We share this theoretical view in explaining Japan's bank lending during the postinjection period. Our estimation results obtained by utilizing Japan's two large-scale capital injections in 1998 and 1999 as a natural experiment provide empirical support for Berrospide and Edge's (2010) view regarding the US capital injections.

6. Conclusion This paper draws three substantive conclusions.

First, the first and second bank capital injections reduced the default risks of the capital-injected banks and their nonperforming loans. We therefore conclude that the two public capital injections significantly reduced financial risk in the capital-injected banks.

Second, the two injections did not substantially improve the profitability of the capital-injected banks and their lending behavior.

Third, the main reason that the lending of the capital-injected banks did not increase is most likely that the borrowers' default risks increased during the severe recession after the two injections. In addition, the borrowers' increased default risks would impede not only the injected but also the non-capital-injected banks from lending more. In other words, the deterioration of borrower creditworthiness because of Japan's severe recession and the accompanying increased perception by banks of the riskiness of lending would impede overall bank lending to domestic enterprises after the two public injections.

The two capital injections in Japan probably had a favorable effect in terms of decreasing the financial risks of the capital-injected banks. Such a favorable effect is likely to have substantially stabilized the Japanese banking system. Conversely, the public capital injections would not have successfully stimulated the lending and profitability of the injected banks. We carefully extracted such an effect of the public capital injections through exploiting both bank-level and loan-level data sets.

[32] Duchin and Sosyura (2014) also found that TARP did not result in the credit expansion of capital-injected banks. However, unlike Berrospide and Edge (2010), they attributed the sluggish bank lending of capital-injected banks to a shift from safer toward riskier lending.

We did not address two issues in our empirical investigation. First, we did not consider the issue of endogeneity biases that might arise in loan-level specification of a loan supply function if bank and firm matches are not random (see footnote 23). Given that laws stipulate the policy objectives of public capital injections, such as the write-off of non-performing loans and improvements in bank lending, and hence the banking supervisory agency supervises a capital-injected bank to ensure that its actions are consistent with the policy objectives, the assumption of exogenous matching in lender–borrower relationships is highly demanding. Examining the extent to which the achievement of the policy objectives of public capital injection would arise from endogenous matching is a matter for future analysis.

Second, we did not address some specific issues about how recapitalization should be conducted: what amount of recapitalization is optimal to maintain viable relationships between lenders and their borrowing firms (see, e.g., Diamond and Rajan (2000), Diamond (2001), and Giannetti and Simonov (2013)), what measures to infuse capital are the most effective (see, e.g., Hoshi and Kashyap (2010) and Bayazitova and Shivdasani (2012)), and how should the banking supervisory agency supervise a capital-injected bank in terms of its risk taking (see, e.g., Osada (2011), Black and Hazelwood (2013), Berger et al. (2014), and Duchin and Sosyura (2014)). We should reassess these issues by exploiting not only bank-level but also loan-level data.

Appendix I: Statistical Inference of the Estimated Treatment Effect Conley and Taber (2011) demonstrated that standard large-sample theory is not appropriate for statistical inference of the treatment effect estimated using the within-group estimation method for a fixed-effects regression model when the number of members of the treated group, N_1, is much smaller than that of the control group, N_0. Accordingly, Conley and Taber (2011) suggested an alternative method for statistical inference that employs information about members of the control group. More precisely, their method employs the empirical distribution constructed using residuals generated from a control group equation in a fixed-effects regression model. Following Conley and Taber's (2011) method, we then conduct statistical inference based on the empirical distribution constructed by the following procedures. [33]

1. Estimate Model I using the within-group estimation method.

2. Generate residuals ε_{ht} from an estimated equation for bank h ($h = 1, \ldots, N_0$) that belongs to the control group, and then calculate the centered residuals $\tilde{\varepsilon}_{ht} = \varepsilon_{ht} - \bar{\varepsilon}_h$. N_0 denotes the number of banks belonging to the control group.

3. Construct the empirical distribution of the estimated treatment effect using the centered residuals as follows:

$$\frac{\sum_{i=1}^{N_1} \sum_{t=t^*-1}^{T} (D_{it} - \bar{D}_i)\tilde{\varepsilon}_{ht}}{\sum_{i=1}^{N_1} \sum_{t=t^*-1}^{T} (D_{it} - \bar{D}_i)^2} \quad (h = 1, \ldots, N_0),$$

   where $\bar{D}_i = (T - t^*)^{-1} \sum_{t=t^*-1}^{T} D_{it}$, and T indicates the end point of the sample period. The 95% confidence intervals of the treatment effect δ reported in Tables 4 and 6 are obtained as "a point estimate of δ plus the 2.5 percentage quantile of the empirical distribution" and "the point estimate plus the 97.5 percentage quantile."

4. To test the null hypothesis $\delta = 0$, estimate Model I, imposing the parameter restriction $\delta = 0$, and then construct the empirical distribution of the null hypothesis following procedures 2 and 3 above. If a point estimate of δ falls into the rejection region of this empirical distribution at the required level of significance, reject the null hypothesis. Tables 4 and 6 report the 1, 5, and 10%levels of significance with the corresponding number of asterisks.

5. The 90% confidence intervals of the treatment effect δ_t at each time $t = t^* + k$ ($k \geq 0$), shown in Figure 5, are constructed by modifying the above procedures in the following way. First, in procedures 1 and 2, we estimate Model II and generate centered residuals $\tilde{\varepsilon}_{ht} = \varepsilon_{ht} - \bar{\varepsilon}_h$ for bank h ($h = 1, \ldots, N_0$) that belongs to the control group. Next, in procedure 3, using the centered residuals, we calculate the empirical

[33] Unlike the standard asymptotic distributions, the empirical distributions are not symmetric. Therefore, we must use the confidence intervals for statistical inference of estimated treatment effects, as reported in Tables 4 to 10.

distribution at each time $t = t^* + k$ $(k \geq 0)$ as follows:

$$\frac{\sum_{i=1}^{N_1}(D_{it} - \bar{D}_i)\tilde{\varepsilon}_{ht}}{\sum_{i=1}^{N_1}(D_{it} - \bar{D}_i)^2} \quad (h = 1, \ldots, N_0).$$

Finally, the 90% confidence intervals of the treatment effect δ_t at time $t = t^* + k$ $(k \geq 0)$ are obtained as "a point estimate of δ_t at time $t = t^* + k$ $(k \geq 0)$ plus the five percentage quantile of the empirical distribution" and "the point estimate plus the 95 percentage quantile."

6. The 95% confidence intervals related to the treatment effect δ_q reported in Table 5 are constructed by modifying procedures 1 to 3. First, we obtain centered residuals $\tilde{\varepsilon}_{jt}$ $(j = 1, \ldots, N_0)$ of the control group by estimating Model III. Next, we construct the empirical distribution using the following equation:

$$\frac{\sum_{i=1}^{N_1}\sum_{t=t^*-1}^{T}(D_{it}^q - \bar{D}_i^q)\tilde{\varepsilon}_{jt}}{\sum_{i=1}^{N_1}\sum_{t=t^*-1}^{T}(D_{it}^q - \bar{D}_i^q)^2} \quad (j = 1, \ldots, N_0),$$

where $\bar{D}_i^q = (T - t^*)^{-1}\sum_{t=t^*-1}^{T} D_{it}^q$. The confidence intervals are obtained as "a point estimate of δ_q plus the 2.5 percentage quantile of the empirical distribution" and "the point estimate plus the 97.5 percentage quantile." To test the null hypothesis of δ_q, we estimate Model III and impose the restriction $\delta_q = 0$, and then construct the empirical distribution of the null hypothesis.

7. The 95% confidence intervals of the treatment effect δ_t at each time $t = t^* + k$ $(k \geq 0)$, shown in Tables 7 and 8, are constructed in the following way. First, in procedures 1 and 2, we estimate Model IV and generate centered residuals $\tilde{\varepsilon}_{ht}^j = \varepsilon_{ht}^j - \bar{\varepsilon}_h^j$ for bank h $(h = 1, \ldots, N_0)$ and its borrowing firm j $(j = 1, \ldots, N_0^F)$ that belongs to the control group. Next, in procedure 3, using the centered residuals, we calculate the empirical distribution at each time $t = t^* + k$ $(k \geq 0)$ as follows:

$$\frac{\sum_{i=1}^{N_1}(D_{it} - \bar{D}_i)\tilde{\varepsilon}_{ht}^j}{\sum_{i=1}^{N_1}(D_{it} - \bar{D}_i)^2} \quad (h = 1, \ldots, N_0, \quad j = 1, \ldots, N_0^F).$$

Finally, the 95% confidence intervals of the treatment effect δ_t at time $t = t^* + k$ $(k \geq 0)$ are obtained as "a point estimate of δ_t at time $t = t^* + k$ $(k \geq 0)$ plus the 2.5

percentage quantile of the empirical distribution" and "the point estimate plus the 97.5 percentage quantile."

Appendix II: Construction of the Probability of Default The probability of default, defined in Subsections 2.3 and 4.2, is theoretically based on Merton's (1974) structural option-pricing model. According to Merton (1974), the market value of equity V_E can be thought of as a call option on the asset value V_A with the time to maturity of debt T, and hence it plays the role of the strike price of the call option. The market value of equity V_E and the volatility of equity valuation σ_E are then given by the Black and Scholes (1973) formula for call options:

$$V_E = V_A N(d_1) - De^{-rT} N(d_2), \qquad \sigma_E = \left(\frac{V_A}{V_E}\right) N(d_1)\sigma_A, \tag{3}$$

where D indicates the book value of the debt that has maturity equal to T. $d_1 = d_2 - \sigma_A\sqrt{T}$, $d_2 = \frac{\ln(V_A/D)+\left(r+\frac{1}{2}\sigma_A^2\right)T}{\sigma_A\sqrt{T}}$, and N denotes the cumulative density function of the standard normal distribution. In the framework of Merton's (1974) structural model, once the numerical value of d_1 is obtained, the risk-neutral probability of default is calculated as $N(d_1)$.

To compute the risk-neutral probability of default, it is necessary to estimate two unknowns—the market value of asset V_A and asset volatility σ_A—using data for each period of the five observables: the market value of equity V_E, the volatility of equity valuation σ_E, the book value of debt liabilities D, the time to maturity of the debt T, and the risk-free rate r, from the two nonlinear simultaneous equations (3). To solve this system, we employ the reduced gradient method and use the market value of equity V_E calculated from both the daily stock-price data and the number of shares outstanding provided by NRI. [34] To estimate the volatility of equity valuation σ_E, we calculate the standard deviation of the market value of equity V_E for the past 20 business days of each trading day. In addition, we express the estimated volatility of the equity valuation at annual rates as in the following

[34] The number of shares outstanding used for our empirical analysis is adjusted according to a TOPIX-type computation from the secondary capital transfer data.

equation:

$$\sigma_{Et} = \sqrt{\frac{1}{20-1} \times \sum_{i=t}^{t-19} (ret_i - \overline{ret_t})^2} \times \sqrt{240},$$

where t denotes a trading day. $ret_t = \ln(V_{Et}) - \ln(V_{Et-1})$ denotes the daily rate of change in equity valuation, and $\overline{ret_t}$ is the average rate of change in equity valuation of the previous 20 days.

The book value of debt liabilities D is obtained from semiannual published accounts (unconsolidated basis) compiled by NRI and is linearly interpolated to yield daily observations. The time to maturity of the debt T is set at one year, which is the conventional assumption in constructing a measure of default risk theoretically based on Merton's (1974) model including the distance to default marketed by the Moody/KMV Corporation (see Crosbie and Bohn (2003) for details). [35] For the risk-free rate r, the one-year swap rate observed for each trading day is used. More precisely, we construct the swap rates based on the average rate of offers and bids quoted by Yagi Euro, one of the major dealers in the interest rate swap market in Japan. Finally, we compute the monthly average of the probability of default to ensure consistency with our data set.

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Table 1: The Size of Public Capital Injections

Bank name	The first capital injection based on the Financial Function Stabilization Act			The second capital injection based on the Prompt Recapitalization Act		
	Preferred shares	Subordinated bonds and loans	Total	Preferred shares	Subordinated bonds and loans	Total
Daiichi Kangyo	99	-	99	700	200	900
Fuji	-	100	100	800	200	1000
Industrial Bank of Japan	-	100	100	350	250	600
Yasuda Trust	-	150	150	-	-	-
Sakura	-	100	100	800	-	800
Sumitomo	-	100	100	501	-	501
Tokyo Mitsubishi	-	100	100	-	-	-
Mitsubishi Trust	-	50	50	200	100	300
Sanwa	-	100	100	600	100	700
Tokai	-	100	100	600	-	600
Toyo Trust	-	50	50	200	-	200
Asahi	-	100	100	400	100	500
Daiwa	-	100	100	408	-	408
Sumitomo Trust	-	100	100	100	100	200
Mitsui Trust	-	100	100	250.3	150	400.3
Chuo Trust	32	28	60	150	-	150
Yokohama	-	20	20	100	100	200
Hokuriku	-	20	20	-	-	-
Ashikaga	-	30	30	-	-	-
Long-Term Credit Bank of Japan	130	46.6	176.6	-	-	-
Nippon Credit Bank	60	-	60	-	-	-
Total	321	1494.6	1815.6	6159.3	1300	7459.3

∗ Data are expressed in billions of yen.

Table 2: Summary Statistics for Bank-level Data: September 1997 - March 2002

Periods	Variables	Total Sample					Capital-injected Banks					Noncapital-injected Banks				
		Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
The First Public Capital Injection (1997:9 ∼1998:3)	PD_{it}	306	0.795	3.819	0.000	58.84	63	2.122	7.502	2.26E-06	58.84	243	0.451	1.839	0.000	24.31
	$Tier_{it}$	304	6.257	2.323	-13.13	13.00	63	5.003	1.935	-8.784	7.551	241	6.584	2.307	-13.13	13.00
	$RATIO_{it}$	305	8.739	2.204	-4.259	13.67	63	9.642	1.187	6.648	13.67	242	8.504	2.344	-4.259	13.65
	NPL_{it}	306	0.029	0.024	0.003	0.164	63	0.046	0.029	0.016	0.164	243	0.024	0.020	0.003	0.141
	ROA_{it}	306	-0.032	0.201	-2.116	0.032	63	-0.048	0.214	-1.640	0.027	243	-0.028	0.198	-2.116	0.032
	$\Delta LOAN_{it}$	304	-0.006	0.030	-0.134	0.093	63	-0.029	0.029	-0.134	0.017	241	-0.0003	0.027	-0.128	0.093
	$\Delta SMELOAN_{it}$	303	-0.010	0.088	-1.000	0.389	63	-0.025	0.030	-0.131	0.023	240	-0.006	0.098	-1.000	0.389
	$RSIZE_{it}$	306	-5.453	1.116	-7.263	-2.258	63	-3.698	0.893	-5.480	-2.258	243	-5.908	0.597	-7.263	-4.497
Periods	Variables	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
The Second Public Capital Injection (1998:9 ∼2002:3	PD_{it}	762	1.205	2.788	0.000	37.74	94	3.765	3.567	0.003	15.15	668	0.845	2.458	0.000	37.74
	$Tier_{it}$	762	7.040	2.203	-10.83	16.36	94	6.496	1.060	4.608	9.533	668	7.116	2.310	-10.83	16.36
	$RATIO_{it}$	762	9.414	2.246	-12.15	16.47	94	11.28	1.217	8.897	15.15	668	9.150	2.233	-12.15	16.47
	NPL_{it}	757	0.081	0.753	0.003	14.86	94	0.044	0.019	0.016	0.122	663	0.086	0.804	0.003	14.86
	ROA_{it}	762	-0.015	0.099	-1.640	0.228	94	-0.006	0.037	-0.227	0.036	668	-0.016	0.105	-1.640	0.228
	$\Delta LOAN_{it}$	751	0.001	0.097	-0.173	1.576	94	0.008	0.190	-0.126	1.576	657	0.0006	0.075	-0.173	1.188
	$\Delta SMELOAN_{it}$	752	0.015	0.371	-1.000	9.569	94	0.126	1.021	-0.909	9.569	658	-0.0009	0.087	-1.000	1.259
	$RSIZE_{it}$	762	-5.290	1.050	-8.200	-1.334	94	-3.414	0.781	-5.415	-1.334	668	-5.554	0.780	-8.200	-2.107

∗ See Subsection 2.1 for the data source. For the definition of each variable, see Subsection 2.3.

Table 3: Summary Statistics for Loan-level Data: FY1998 - FY2002

Variables		Total Sample					Capital-injected Banks					Noncapital-injected Banks				
		Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Dependent Variable	$\Delta\mathrm{LOAN}_{it}^{j}$	104840	0.516	14.19	-0.999	2499	46332	0.495	13.25	-0.998	1930	58508	0.532	14.89	-0.999	2499
Factor of bank i	PD_{it}	95365	1.333	2.056	0	24.85	49356	1.842	2.047	2.63e-07	10.92	46009	0.786	1.920	0	24.85
	LEV_{it}	95424	94.97	2.503	86.01	99.98	49356	95.50	2.308	89.28	99.39	46068	94.40	2.579	86.012	99.98
	σ_{it}	95365	2.054	1.053	0.028	10.66	49356	2.087	0.989	0.249	4.071	46009	2.020	1.116	0.028	10.66
	NPL_{it}	107716	4.382	3.640	0.275	51.99	49356	4.590	2.058	2.009	13.90	58360	4.207	4.562	0.275	51.99
	ROA_{it}	111911	-0.207	0.770	-45.09	1.614	49356	-0.235	0.398	-1.947	0.255	62555	-0.185	0.967	-45.09	1.614
	SIZE_{it}	112465	16.75	1.288	11.98	18.22	49356	17.44	0.477	15.64	18.17	63109	16.21	1.454	11.98	18.22
Factor of borrower j	PD_{t}^{j}	102507	0.000978	0.1071767	0	14.50458	44742	0.0000371	0.00402	0	0.483	46009	0.786	1.920	0	24.85
	LEV_{t}^{j}	105911	64.26	20.31	0.758	99.84	46266	61.91	20.28	1.761	99.64	59645	66.09	20.15	0.758	99.84
	σ_{t}^{j}	102738	16.42	11.80	0.139	282.7	44841	16.71	11.02	0.139	200.7	57897	16.20	12.36	0.139	282.7
	ICR_{t}^{j}	106954	1428	20295	-77000	1803800	47142	1153	12226	-77000	603315	59812	1645	24872	-72356	1803800
	ROA_{t}^{j}	109341	0.240	5.295	-372.9	157.1	47830	0.378	4.991	-123.3	56.10	61511	0.132	5.518	-372.9	157.1
	SIZE_{t}^{j}	109341	11.12	1.622	4.812	16.46	47830	11.01	1.565	4.812	16.46	61511	11.21	1.660	5.771	16.46
	INVEST_{t}^{j}	104614	0.039	0.165	-4.079	4.494	45390	0.042	0.160	-2.822	4.494	59224	0.036	0.169	-4.079	4.494
	ZOMBIE_{t}^{j}	97103	0.328	0.469	0.000	1.000	42393	0.345	0.475	0.000	1.000	54710	0.315	0.464	0.000	1.000
Relationship Factor of lender i and borrower j	$\mathrm{EXPLEND}_{it}^{j}$	100607	0.712	3.121	0.00002	100.0	43962	0.142	0.470	0.00002	14.05	56645	1.154	4.084	3.14E-05	100.0
	$\mathrm{EXBORROW}_{it}^{j}$	100667	12.06	14.66	0.0007	100.0	43945	13.20	14.32	0.001	100.0	56722	11.18	14.869	0.0007	100.0
	$\mathrm{DURATION}_{it}^{j}$	104840	12.65	8.404	1.000	25.00	46332	12.73	8.328	1.000	24.00	58508	12.60	8.464	1.000	25.00
Price of bank loan	r_{t}^{i}	110376	1.158	0.747	0.0006	77.31	48725	1.189	0.736	0.0006	8.028	61651	1.133	0.755	0.0006	77.31

$*$ See Subsection 2.1 for the data source. For the definition of each variable, see Subsections 2.3 and 4.2.

Table 4: Estimation Results of Model I

The first capital injection (September 1997 - September 1998)

	Outcome variable : y_t^i				
	(1) PD	(2) NPL	(3) ROA	(4) ΔLOAN	(5) ΔSMELOAN
Treatment effect : δ	-2.658** (-5.546, -0.354)	-10.27** (-20.30, -2.947)	-0.960 (-3.221, 1.300)	0.421 (-0.800, 1.790)	0.021 (-2.899, 2.801)
PD_{t-1}^i	-	0.004* (-0.001, 0.008)	-0.002 (-0.008, 0.004)	-0.0002 (-0.0007, 0.0002)	-0.0001 (-0.0007, 0.0003)
NPL_{t-1}^i	-	-	-0.036 (-0.085, 0.013)	-0.016 (-0.037, 0.004)	-0.019 (-0.051, 0.013)
ROA_{t-1}^i	-	-	-	0.036*** (0.028, 0.045)	0.041*** (0.025, 0.057)
$RSIZE_{t-1}^i$	-3.759 (-25.46, 17.94)	1.012** (0.125, 1.900)	-0.153* (-0.327, 0.020)	0.153* (-0.020, 0.327)	0.396 (-0.043, 0.835)
Within R^2	0.028	0.165	0.096	0.251	0.039

The second capital injection (September 1998 - March 2002)

	Outcome variable : y_t^i				
	(1) PD	(2) NPL	(3) ROA	(4) ΔLOAN	(5) ΔSMELOAN
Treatment effect : δ	-1.177** (-2.399, -0.105)	-27.65** (-58.00, -1.298)	0.029 (-0.046, 0.100)	-1.412 (-4.043, 1.508)	-1.986 (-7.543, 3.502)
PD_{t-1}^i	-	0.009** (0.001, 0.018)	-0.009 (-0.026, 0.008)	-0.002 (-0.006, 0.001)	-0.001 (-0.005, 0.002)
NPL_{t-1}^i	-	-	-0.032 (-0.089, 0.025)	-0.008 (-0.026, 0.010)	-0.048 (-0.120, 0.024)
ROA_{t-1}^i	-	-	-	0.053 (-0.025, 0.133)	0.072 (-0.243, 0.388)
$RSIZE_{t-1}^i$	-1.868 (-5.093, 1.355)	-0.691*** (-0.950, -0.433)	-0.232*** (-0.353, -0.112)	-0.002 (-0.043, 0.039)	-0.162** (-0.306, -0.018)
Within R^2	0.114	0.427	0.058	0.075	0.031

1. We conduct the within-group estimation method for estimating Model l.
2. For the estimates of the treatment effect δ, the 95% confidence intervals calculated using Conley and Taber's (2011) method are in parentheses. See Appendix I for Conley and Taber's (2011) method. For the estimates of the covariates, the 95% confidence intervals calculated based on large-sample approximation and its standard error clustered by both bank and time dimensions are in parentheses.
3. *, ** and *** indicate the 10%, 5% and 1% levels of significance, respectively.

Table 5: The Heterogeneous Effect on the Probability of Default: Model III

The first capital injection			The second capital injection		
Size	Treatment effect : δ^q FE estimation	OLS estimation	Size	Treatment effect : δ^q FE estimation	OLS estimation
20 billion yen	-0.781 (-5.224, 3.048)	-0.438 (-1.556, 0.680)	150 billion yen	-0.320 (-6.341, 6.328)	-2.120*** (-3.241, -0.999)
30 billion yen	-0.747 (-7.554, 5.004)	-0.405 (-1.707, 0.897)	200 billion yen	-1.100 (-5.820, 2.426)	-1.415** (-2.608, -0.222)
50 billion yen	0.594 (-4.592, 7.307)	0.775 (-0.197, 1.353)	300 billion yen	-1.496** (-2.591, -0.121)	-1.301*** (-2.196, -0.406)
60 billion yen	-1.730 (-6.709, 2.040)	-0.700 (-2.496, 1.096)	400.3 billion yen	-3.424 (-11.43, 3.900)	-1.322*** (-2.225, -0.419)
99 billion yen	-0.422 (-7.905, 9.065)	-0.190 (-0.822, 0.442)	408 billion yen	2.691 (-4.121, 7.309)	4.791*** (4.227, 5.355)
100 billion yen	-4.206*** (-7.762, -2.954)	-4.594*** (-6.915, -2.273)	450 billion yen	-4.339 (-9.655, 0.330)	0.210 (-0.543,0.963)
150 billion yen	0.041 (-7.708, 7.529)	0.677 (-0.927, 2.281)	500 billion yen	1.499 (-4.122, 7.684)	3.738*** (3.311, 4.165)
176.6 billion yen	2.820*** (0.528, 3.774)	2.520*** (1.795, 3.245)	501 billion yen	-0.988* (-1.865, 0.032)	-0.551* (-1.179, 0.077)
-	-	-	600 billion yen	-1.811** (-3.622, -0.155)	-0.981** (-1.863, -0.099)
-	-	-	700 billion yen	-1.222 (-7.521, 4.439)	0.001 (-0.545, 0.556)
-	-	-	800 billion yen	-5.492* (-11.94, 0.399)	-2.409*** (-2.986, -1.832)
-	-	-	900 billion yen	-1.179 (-2.313, 0.299)	-2.111*** (-2.645, -1.577)
-	-	-	1000 billion yen	-6.890*** (-7.848, -5.869)	-2.579*** (-3.117, -2.041)

1. We conduct the within-group estimation (FE estimation) method for estimating Model III with a fixed effect term v_i, and the ordinary least squares estimation (OLS estimation) method for estimating a version of Model III that include PD_{it-1} but does not include v_i as an explanatory variable.
2. For the FE estimation, the numbers in parentheses are the 95% confidence interval calculated using Conley and Taber's (2011) method. See Appendix I for details. For the OLS estimation, the numbers in parentheses are the 95% confidence interval calculated based on the large-sample approximation and its standard error clustered by both bank and time dimensions.
3. *, ** and *** indicate the 10%, 5% and 1% levels of significance, respectively.

Table 6: Falsification Test Results: Model I

Pre-treatment Period (September 1995 - September 1997)

	Outcome variable : y_t^i (1) PD	(2) NPL	(3) ROA	(4) ΔLoan	(5) ΔSMELOAN
Treatment effect : δ	-0.896 (-2.215, 0.357)	-0.002 (-0.070, 0.061)	-0.085 (-0.221, 0.049)	0.012 (-0.004, 0.018)	-0.011 (-0.046, 0.037)

1. We conduct the within-group estimation method for estimating Model I in the pre-treatment sample period from September 1995 to September 1997.
2. For the estimates of the treatment effect δ, the 95% confidence intervals calculated using Conley and Taber's (2011) method are in parentheses. See Appendix I for Conley and Taber's (2011) method.

Table 7: Estimation Results of the Loan Supply Function: Models IV (FY 1998 - FY 2002)

Dependent variable	$\Delta\text{LOAN}_{it}^{j}$	(1)	(2)	(3)
Factor of bank i	LEV_{it-1}	0.045 (-0.027, 0.118)	0.029 (-0.052, 0.112)	0.015 (-0.068, 0.100)
	σ_{Ait-1}	0.097 (-0.019, 0.213)	0.096 (-0.031, 0.224)	0.091 (-0.045, 0.228)
	NPL_{it-1}	-0.042 (-0.105, 0.019)	-0.069 (-0.165, 0.026)	-0.051 (-0.127, 0.023)
	ROA_{it-1}	-0.092 (-0.232, 0.047)	-0.048 (-0.154, 0.057)	-0.045 (-0.158, 0.066)
	SIZE_{it-1}	0.091 (-0.327, 0.509)	0.040 (-0.414, 0.495)	0.136 (-0.349, 0.621)
Factor of borrower j	LEV_{t-1}^{j}	-0.044** (-0.082, -0.006)	-0.049** (-0.095, -0.003)	-0.048* (-0.102, 0.006)
	σ_{At-1}^{j}	-0.038*** (-0.067, -0.009)	-0.040** (-0.076, -0.005)	-0.041* (-0.087, 0.004)
	ICR_{t-1}^{j}	1.7×10^{-5}* ((-1.9×10^{-6}, 3.0×10^{-5})	1.0×10^{-5}** (8.4×10^{-7}, 2.0×10^{-5})	2.7×10^{-5}* (-5.1×10^{-6}, 6.0×10^{-5})
	ROA_{t-1}^{j}	-0.0004 (-0.012, 0.011)	0.004 (-0.007, 0.016)	0.0005 (-0.013, 0.014)
	SIZE_{t-1}^{j}	-1.115*** (-1.670, -0.620)	-1.110*** (-1.746, -0.474)	-0.716** (-1.391, -0.040)
	INVEST_{t-1}^{j}	-	0.291 (-0.266, 0.849)	0.372 (-0.533, 1.277)
	ZOMBIE_{t-1}^{j}	-	-	-0.475*** (-0.791, -0.157)
Relationship Factor of lender i and borrower j	$\text{EXPLEND}_{it-1}^{j}$	-	-0.004 (-0.021, 0.012)	-0.007 (-0.028, 0.012)
	$\text{EXPBORROW}_{it-1}^{j}$	-	0.001 (-0.003, 0.007)	0.002 (-0.003, 0.008)
	$\text{DURATION}_{it-1}^{j}$	-	-0.057*** (-0.077, -0.037)	-0.062*** (-0.085, -0.037)
Treatment Effect δ_t	t =FY1999	-8.082 (-20.53, 4.182)	-10.16 (-27.77, 6.398)	-9.091 (-31.55, 12.90)
	t =FY2000	3.991 (-1.020, 9.252)	3.761 (-1.790, 9.281)	3.040 (-2.998, 9.088)
	t =FY2001	-0.189 (-0.693, 0.295)	-0.192 (-0.761, 0.390)	-0.161 (-0.703, 0.395)
	t =FY2002	1.762 (-6.729, 10.45)	2.293 (-6.409, 10.79)	2.576 (-6.889, 10.35)
Fixed Effect of lender i and borrower j	v_i	0.096	0.150	0.032
	v^j	-10.76	-8.778	-11.52
R^2		0.337	0.380	0.381
Observations		91921	77334	44529

1. We employ the fixed-effects least-squares dummy-variable estimation method proposed by Abowd et al. (1999) and Andrews et al. (2008).
2. Estimates of the time dummy variables are not reported.
3. For the bank and firm fixed effects, v_i and v^j, the sample means of estimated fixed effects are reported.
4. For the estimates of the treatment effect δ_t, the 95% confidence intervals calculated using Conley and Taber's (2011) method are in parentheses. See Appendix I for Conley and Taber's (2011) method. For the estimates of the covariates, the 95% confidence intervals calculated based on the large-sample approximation and its standard error clustered by lender-borrower relationship and time are in parentheses.
5. *, ** and *** indicate the 10%, 5% and 1% levels of significance, respectively.

Table 8: Estimation Results of the Loan Supply Function: Models V (FY1998 - FY2002)

		Total Sample Set		Firms that borrow from both capital-injected and noncapital-injected banks	
Dependent variable	$\Delta\text{LOAN}_{it}^{j}$	(1)	(2)	(1)	(2)
Factor of bank i	LEV_{it-1}	-0.046 (-0.103, 0.010)	-0.073 (-0.177, 0.031)	-0.036 (-0.970, 0.898)	-0.011 (-0.106, 0.084)
	σ_{Ait-1}	-0.068 (-0.164, 0.028)	-0.098 (-0.229, 0.033)	-0.069 (-0.240, 0.103)	-0.107 (-0.282, 0.067)
	NPL_{it-1}	-0.013 (-0.044, 0.017)	-0.022 (-0.059, 0.014)	-0.035 (-0.076, 0.006)	-0.023 (-0.065, 0.017)
	ROA_{it-1}	0.003 (-0.099, 0.106)	0.036 (-0.106, 0.178)	0.023 (-0.163, 0.209)	0.011 (-0.177, 0.199)
	SIZE_{it-1}	-0.191 (-0.494, 0.111)	-0.157 (-0.681, 0.367)	-0.066 (-0.595, 0.463)	-0.062 (-0.594, 0.470)
Relationship Factor of lender i and borrower j	$\text{EXPLEND}_{it-1}^{j}$	-	-0.001 (-0.023, 0.023)	-	0.006 (-0.019, 0.031)
	$\text{EXPBORROW}_{it-1}^{j}$	-	0.001 (-0.003, 0.006)	-	-0.003 (-0.009, 0.002)
	$\text{DURATION}_{it-1}^{j}$	-	-0.058*** (-0.071, -0.045)	-	-0.056*** (-0.069, -0.042)
Treatment Effect δ_t	t =FY1999	-17.86 (-37.61, 3.824)	-19.44 (-45.58, 7.198)	-14.58 (-58.46, 29.22)	-14.86 (-60.18, 30.55)
	t =FY2000	5.850 (-1.630, 12.78)	5.673 (-1.488, 12.54)	4.355 (-1.978, 10.96)	3.900 (-4.992, 12.79)
	t =FY2001	-0.129 (-0.499, 0.211)	-0.148 (-0.899, 0.577)	-0.131 (-1.415, 1.486)	-0.207 (-0.892, 0.421)
	t =FY2002	1.798 (-1.290, 4.508)	2.879 (-2.339, 5.296)	1.306 (-3.721 , 6.224)	1.331 (-3.422, 6.563)
Fixed Effect of lender i	v_i	0.042	0.028	0.042	0.014
Factor of borrower j	$v^j \times t$	-11.59	-11.99	-13.87	-14.39
Correlation of v_i and $v^j \times t$		-0.024	-0.021	-0.049	-0.015
R^2		0.687	0.716	0.840	0.883
Observations		91921	77334	44529	37531

1. We employ the fixed-effects least-squares dummy-variable estimation method proposed by Abowd et al. (1999) and Andrews et al. (2008).
2. For the bank fixed effect v_i and the borrower-side factor $v^j \times t$, their sample means are reported.
3. For the estimates of the treatment effects δ_t, the 95% confidence intervals calculated using Conley and Taber's (2011) method are in parentheses. See Appendix I for Conley and Taber's (2011) method. For the estimates of the covariates, the 95% confidence intervals calculated based on the large-sample approximation and its standard error clustered by lender-borrower relationship and time are in parentheses.
4. *, ** and *** indicate the 10%, 5% and 1% levels of significance, respectively.

Table 9: Estimation Results of the Loan Supply Function: Model IV* (FY1998 - FY2002)

		Capital-injected bank's loan			Noncapital-injected bank's loan		
Dependent variable	$\Delta\mathrm{LOAN}^j_{it}$	(1)	(2)	(3)	(4)	(5)	(6)
Factor of bank i	PD_{it-1}	-0.010 (-0.114, 0.095)	-	-	-0.005 (-0.044, 0.034)	-	-
	LEV_{it-1}	-	-0.124 (-0.368, 0.120)	-	-	-0.009 (-0.068, 0.050)	-
	σ_{Ait-1}	-	-0.304 (-0.703, 0.095)	-	-	-0.039 (-0.139, 0.060)	-
	CAP_{it-1}	-	-	-0.007 (-0.170, 0.156)	-	-	0.025* (-0.004, 0.055)
	NPL_{it-1}	-0.061 (-0.147, 0.024)	-0.630 (-0.147, 0.021)	-0.062 (-0.146, 0.021)	0.004 (-0.031, 0.039)	0.008 (-0.026, 0.042)	-0.0008 (-0.027, 0.026)
	ROA_{it-1}	0.182 (-0.468, 0.832)	0.100 (-0.569, 0.769)	0.193 (-0.449, 0.835)	-0.008 (-0.109, 0.093)	-0.014 (-0.117, 0.089)	-0.031 (-0.118, 0.056)
	SIZE_{it-1}	3.376** (0.682, 6.070)	3.079** (0.301, 5.857)	3.361** (0.599, 6.124)	-0.066 (-0.415, 0.284)	-0.117 (-0.504, 0.270)	-0.289** (-0.560, -0.018)
Factor of borrower j	PD^j_{t-1}	-2.609*** (-4.709, -0.509)	-	-2.602*** (-4.618, -0.526)	-2.298*** (-4.301, -0.193)	-	-2.247*** (-4.347, -0.114)
	LEV^j_{t-1}	-	-0.073*** (-0.098, -0.046)	-	-	-0.024*** (-0.034, -0.015)	-
	σ^j_{At-1}	-	-0.053*** (-0.079, -0.026)	-	-	-0.020*** (-0.030, -0.010)	-
	ICR^j_{t-1}	1.6×10^{-5}* (-4.3×10^{-7}, 5.1×10^{-5})	1.5×10^{-5} (-7.2×10^{-6} , 3.8×10^{-5})	1.6×10^{-5}* (-4.2×10^{-6} , 5.8×10^{-5})	7.7×10^{-6}* (-1.7×10^{-6} , 7.0×10^{-6})	6.2×10^{-6} (-2.7×10^{-7}, 1.5×10^{-5})	9.5×10^{-7}* (-0.7×10^{-6} , 9.2×10^{-6})
	ROA^j_{t-1}	0.019 (-0.012, 0.049)	0.002 (-0.029, 0.032)	0.019 (-0.012, 0.049)	0.009* (-0.0008, 0.018)	0.005 (-0.005, 0.014)	0.005 (-0.001, 0.014)
	SIZE^j_{t-1}	-0.960 (-2.281, 0.360)	-0.736 (-2.040, 0.569)	-0.961 (-2.282, 0.360)	-0.869*** (-1.288, -0.499)	-0.751*** (-1.168, -0.334)	-0.755*** (-1.091, -0.419)
Fixed effect of lender i and borrower j	v^i	0.634	0.629	0.614	0.364	0.014	0.277
	v^j	-23.04	-22.80	-23.05	-10.61	-13.14	-13.91
R^2		0.220	0.258	0.260	0.209	0.214	0.224
Observations		41100	40981	41067	58508	51750	51860

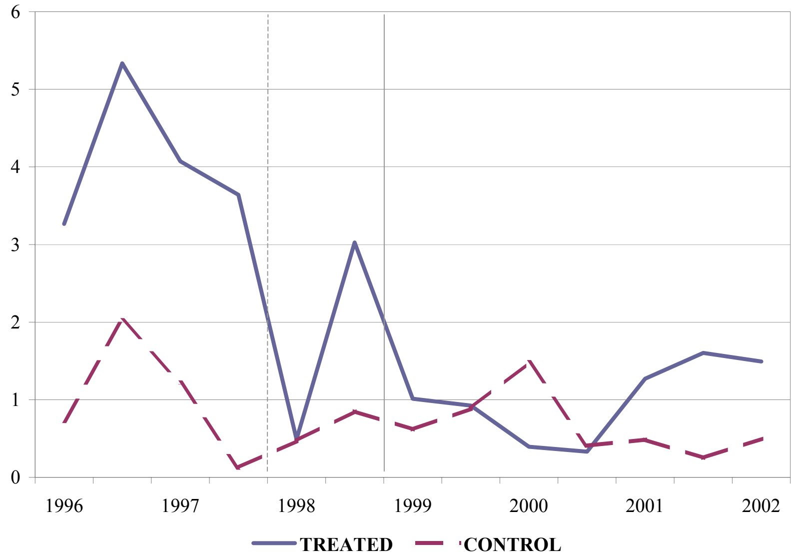
1. We employ the fixed-effects least-squares dummy-variable estimation method proposed by Abowd et al. (1999) and Andrews et al. (2008).
2. Estimates of the time dummy variables are not reported.
3. The capital surplus (CAP^i_{t-1}) is defined by subtracting the target capital ratio (8% for international banks and 4% for domestic banks) from the reported capital ratio.
4. For the bank and firm fixed effects, v_i and v^j, the sample means of estimated fixed effects are reported.
5. The numbers in parentheses are the 95% confidence interval calculated with a standard error clustered by lender-borrower relationship and time.
6. *, ** and *** indicate the 10%, 5% and 1% levels of significance, respectively.

Table 10: Estimation Results of the Loan Supply Function: Model IV*
(FY1998 - FY2002)

		Capital-injected bank's loan			Noncapital-injected bank's loan		
Dependent variable	$\Delta\text{LOAN}_{it}^{j}$	(1)	(2)	(3)	(4)	(5)	(6)
Factor of bank i	LEV_{it-1}	-0.139 (-1.004, 0.726)	-0.186 (-0.467, 0.095)	-0.184 (-0.491, 0.124)	-0.0005 (-0.112, 0.114)	-0.009 (-0.076, 0.057)	-0.028 (-0.099, 0.042)
	σ_{Ait-1}	-0.0004 (-0.174, 0.173)	-0.321 (-0.777, 0.135)	-0.302 (-0.799, 0.196)	-0.044 (-0.144, 0.055)	-0.059 (-0.172, 0.054)	-0.073 (-0.192, 0.046)
	NPL_{it-1}	-0.046 (-0.127, 0.035)	-0.069 (-0.165, 0.026)	-0.076 (-0.180, 0.027)	0.017 (-0.034, 0.069)	0.011 (-0.027, 0.050)	0.004 (-0.037, 0.045)
	ROA_{it-1}	0.147 (-0.159, 0.454)	0.115 (-0.649, 0.880)	0.127 (-0.713, 0.968)	0.001 (-0.001, 0.003)	-0.008 (-0.124, 0.109)	-0.015 (-0.139, 0.109)
	SIZE_{it-1}	0.315 (-5.241, 5.872)	2.880 (-0.267, 6.028)	2.562 (-0.865, 5.989)	-0.194 (-0.986, 0.599)	-0.222 (-0.658, 0.214)	-0.099 (-0.561, 0.362)
Factor of borrower j	LEV_{t-1}^{j}	-0.025*** (-0.036, -0.012)	-0.082*** (-0.113, -0.052)	-0.086*** (-0.120, -0.052)	-0.025*** (-0.043, -0.006)	-0.023*** (-0.034, -0.012)	-0.020*** (-0.032, -0.008)
	σ_{At-1}^{j}	-0.015*** (-0.027, -0.005)	-0.059*** (-0.090, -0.027)	-0.067*** (-0.105, -0.028)	-0.021*** (-0.039, -0.003)	-0.018*** (-0.029, -0.006)	-0.017*** (-0.030, -0.004)
	ICR_{t-1}^{j}	1.5×10^{-5}** (2.4×10^{-6}, 2.7×10^{-5})	1.3×10^{-5}** (2.1×10^{-6}, 2.4×10^{-5})	1.6×10^{-5}** (2.4×10^{-6}, 2.6×10^{-5})	3.2×10^{-5}** (3.2×10^{-6}, 6.4×10^{-5})	1.5×10^{-5}* (-1.6×10^{-6}, 3.3×10^{-5})	7.2×10^{-5}** (3.6×10^{-5}, 1.0×10^{-4})
	ROA_{t-1}^{j}	0.013 (-0.013, 0.040)	0.010 (-0.026, 0.047)	0.003 (-0.042, 0.048)	0.005 (-0.007, 0.017)	0.005 (-0.007, 0.017)	0.005*** (0.001, 0.003)
	SIZE_{t-1}^{j}	-0.675 (-1.567, 0.217)	-0.204 (-1.781, 1.373)	-0.021 (-1.791, 1.749)	-0.815*** (-1.238, -0.391)	-0.752*** (-1.254, -0.250)	0.002*** (0.001, 0.003)
	INVEST_{t-1}^{j}	0.368 (-0.098, 0.836)	1.053 (-1.057, 3.163)	0.862 (-0.412, 2.135)	0.266 (-0.171, 0.704)	0.153 (-0.233, 0.540)	0.286 (-0.139, 0.710)
	ZOMBIE_{t-1}^{j}	-	-	-0.865*** (-1.336, -0.395)	-	-	-0.386*** (-0.551, -0.221)
Relationship Factor of lender i and borrower j	$\text{EXPLEND}_{it-1}^{j}$	-	-0.122 (-0.564, 0.321)	-0.070 (-0.531, 0.391)	-	-0.013 (-0.030, -0.004)	-0.011 (-0.029, 0.007)
	$\text{EXPBORROW}_{it-1}^{j}$	-	0.007 (-0.005, 0.019)	0.009 (-0.005, 0.023)	-	-0.003 (-0.008, -0.001)	-0.004 (-0.009, 0.001)
	$\text{DURATION}_{it-1}^{j}$	-	-0.958*** (-1.335, -0.582)	-0.138*** (-0.185, -0.091)	-	-0.206*** (-0.304, -0.107)	-0.026*** (-0.037, -0.014)
Price of bank loan	r_{it}	0.139 (-0.726, 1.004)	-	-	1.902 (-3.080, 6.884)	-	-
Fixed effect of lender i and borrower j	v^{i}	0.691	0.870	0.857	0.223	0.106	0.180
	v^{j}	-21.93	-20.46	-23.41	-8.329	-15.87	-13.92
R^2		0.250	0.273	0.286	0.219	0.241	0.254
Observations		41110	41060	41095	51914	51851	51894

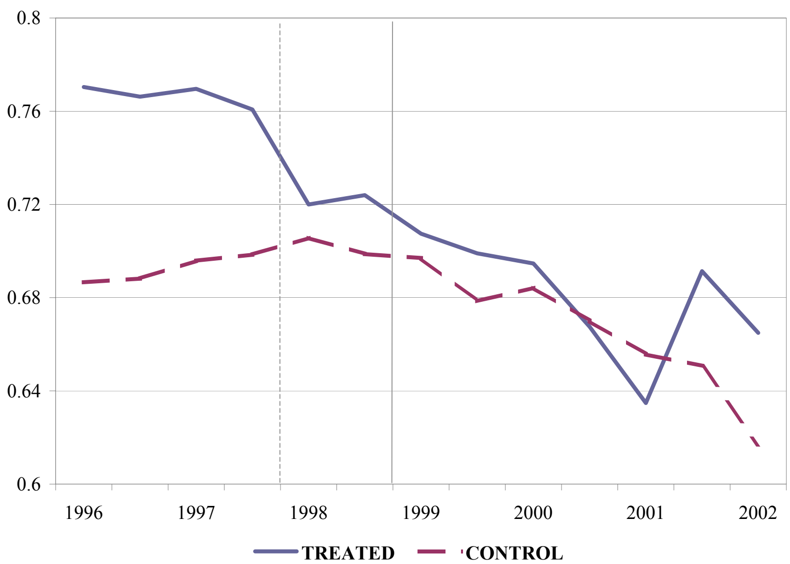
1. We employ the fixed-effects least-squares dummy-variable estimation method proposed by Abowd et al. (1999) and Andrews et al. (2008).
2. Estimates of the time dummy variables are not reported.
3. For the bank and firm fixed effects, v_i and v^j, the sample means of estimated fixed effects are reported.
4. The numbers in parentheses are the 95% confidence interval calculated with a standard error clustered by lender-borrower relationship and time.
5. *, ** and *** indicate the 10%, 5% and 1% levels of significance, respectively.

Figure 1: The Probability of Default of Japanese Banks



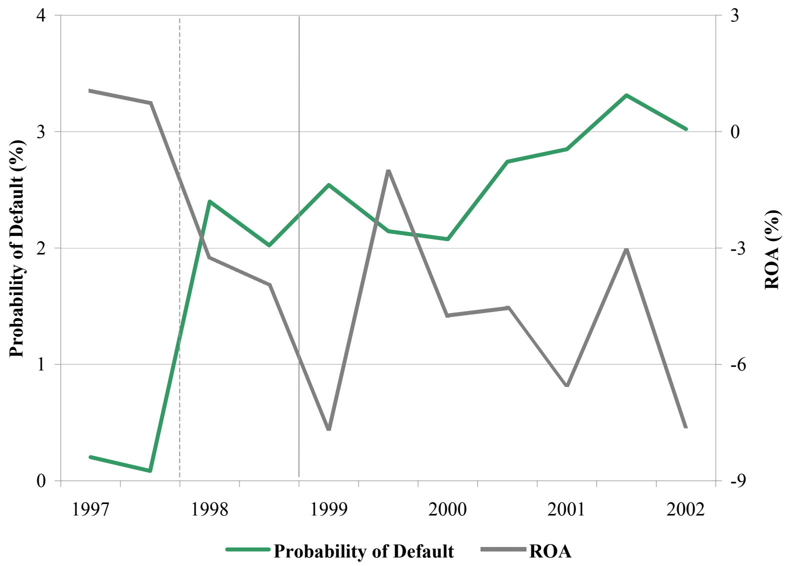
The vertical dotted line indicates the first injection period, and the vertical solid line indicates the second injection period. The solid line indicates the path of the injected banks (treated group), and the dashed line indicates that of the noninjected banks (control group). The probability of default is calculated using Merton's (1974) structural model for option pricing. See Subsection 2.3 for details.

Figure 2: Bank Loans to Domestic Enterprises



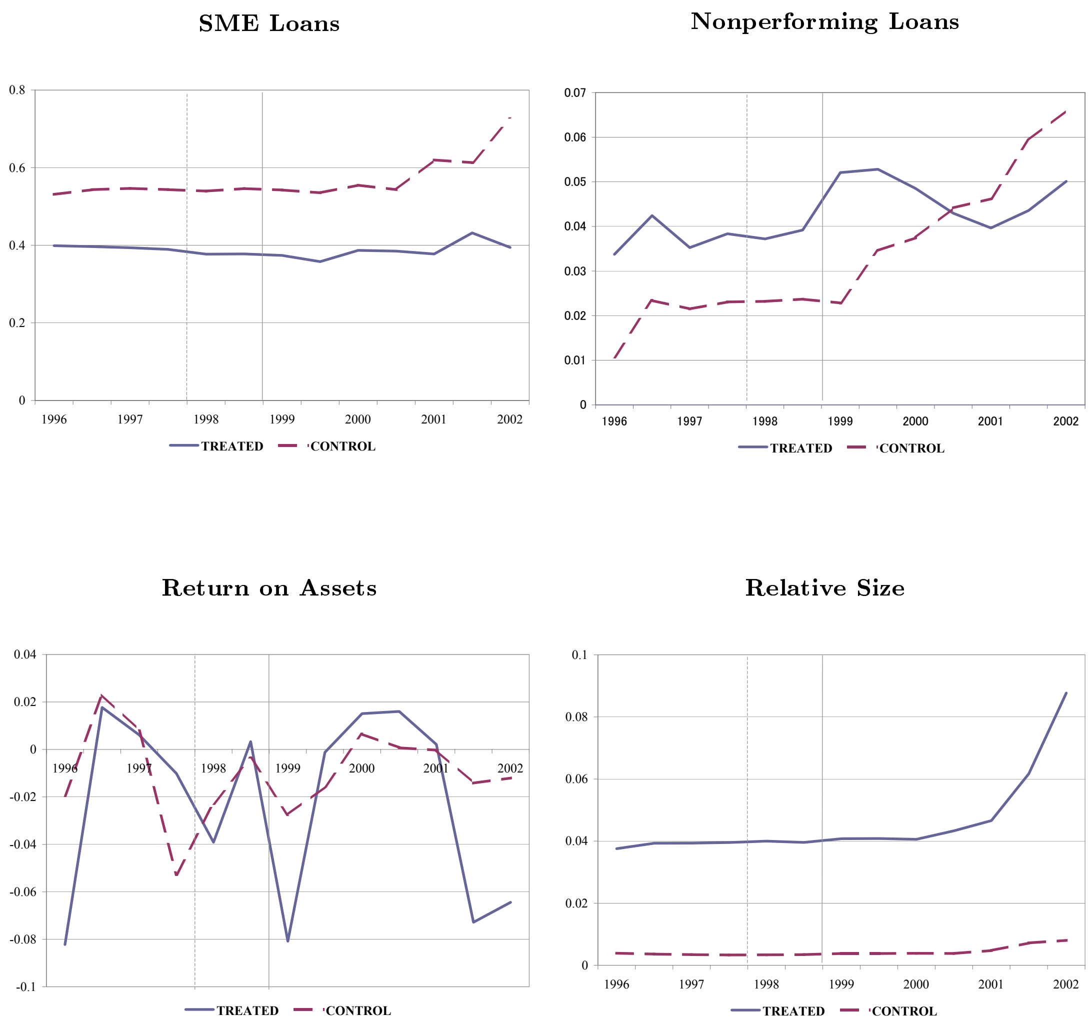
The vertical dotted line indicates the first injection period, and the vertical solid line indicates the second injection period. The solid line indicates the path of the injected banks (treated group), and the dashed line indicates that of the noninjected banks (control group). Bank loans is defined as the ratio of loans for domestic enterprises to total assets. See Subsection 2.3 for details.

Figure 3: The Default Risk and Profitability of Borrowing Firms



The vertical dotted line indicates the first injection period, and the vertical solid line indicates the second injection period. The probability of default of borrowing firms is calculated using Merton's (1974) structural model for option pricing. ROA (return on assets) is defined as $\frac{\text{net profits}}{\text{total assets}} \times 100$. See Subsection 4.2 for details.

Figure 4: Historical Paths of Target Variables



1. The vertical dotted line indicates the first injection period, and the vertical solid line indicates the second injection period.

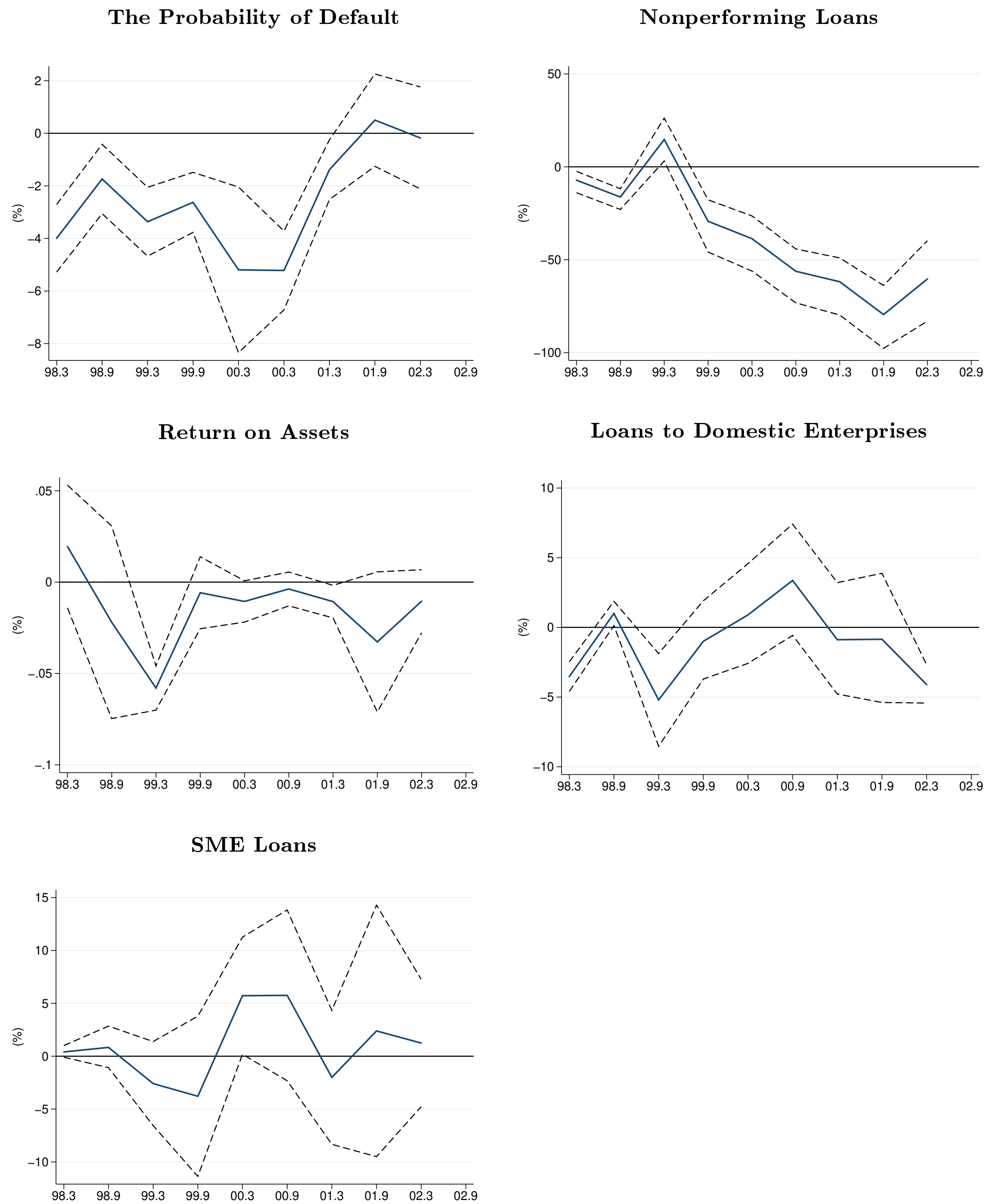
2. The solid line indicates the path of the injected banks (treated group), and the dashed line indicates that of the noninjected banks (control group).

3. SME loans and nonperforming loans are defined as the ratio of loans for small and medium enterprises to total assets and the ratio of nonperforming loans to total loans, respectively.

4. Return on assets is defined as $\frac{\text{net profits}}{\text{total assets}} \times 100$.

5. Relative size is defined as $V_{Ai}/\sum_{j=1}^{n} V_{Aj}$, where V_{Ai} is bank i's asset value and n is the number of banks listed on the Tokyo Stock Exchange at each time.

Figure 5: The Treatment Effects on Target Variables: Model II



1. The solid line indicates point estimates, and the dashed line indicates 90% confidence intervals.

2. The confidence intervals are calculated using the method of Conley and Taber (2011). See Appendix I for details.