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The Effects of Collateral on SME Performance in Japan

ONO Arito
Mizuho Research Institute Ltd.

SAKAI Koji
Institute of Economic Research, Hitotsubashi University

UESUGI Iichiro
RIETI

RIETI
Research Institute of Economy, Trade & Industry, IAA
The Research Institute of Economy, Trade and Industry
http://www.rieti.go.jp/en/

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The Effects of Collateral on SME Performance in Japan[1]

Arito Ono,[2] Koji Sakai,[3] and Iichiro Uesugi[4]

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Abstract

This paper examines how collateral and personal guarantees affect firms' ex-post performance employing the propensity score matching estimation approach. Based on a unique firm-level panel data set of more than 500 small and medium-sized borrower firms in Japan, we find the following: (1) the increase in profitability and reduction in riskiness of borrowers that provide collateral to lenders are more sizeable than of borrowers that do not; (2) On the other hand, the lending attitude and monitoring frequency of borrowers' main banks do not change significantly at the time of collateral being pledged; and (3) The increase in profitability of collateralized borrowers is driven by cost reductions rather than by sales growth. These findings are consistent with the hypothesis that by providing collateral, borrowers curb their own incentives for moral hazard in order to further enhance their creditworthiness.

JEL classification number: D82, G21, G30

Keywords: collateral, moral hazard, propensity score

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[2] Mizuho Research Institute Ltd., arito.ono@mizuho-ri.co.jp
[3] Institute of Economic Research, Hitotsubashi University, sakai-koji@rieti.go.jp
[4] Institute of Economic Research, Hitotsubashi University, iuesugi@ier.hit-u.ac.jp

1. Introduction

How effective are collateral and personal guarantees in improving economic welfare? A substantial number of theoretical studies, including Bester (1985), Boot, Thakor, and Udell (1991), and Stulz and Johnson (1985), suggest that the use of loan securities such as collateral in an environment of asymmetric information leads to possible welfare gains by limiting adverse selection and moral hazard problems. For example, Boot, Thakor, and Udell argue that collateral pledged by risky borrowers induces them to exert greater managerial effort in order to reduce the default probability, and thus attenuates the problem of moral hazard. Stulz and Johnson argue that collateral reduces perverse incentives for borrowers to choose risky projects at creditors' expense (asset substitution). This type of effect should be captured by observing the ex-post performance of borrowers who pledge collateral. However, despite the large number of empirical studies on the use of collateral, little attention has been paid to the effect of collateral on the ex-post performance of borrower firms. Exceptions in this regard are Berger and Udell (1990), and Jiménez and Saurina (2004). These studies, however, do not appropriately deal with possible selection bias problems in the provision of collateral.

Against this background, the present paper represents the first attempt to examine the effect of collateral and personal guarantees on borrowers' ex-post performance, appropriately controlling for possible selection biases. As the subject of our analysis, we focus on small and medium-sized enterprises (SMEs) in Japan, which tend to be heavily dependent on bank loans. Our analysis is based on a unique firm-level panel data set of more than 500 Japanese SMEs covering the years 2001-2005.

For our empirical investigation, we employ the propensity score matching estimation approach proposed by Rosenbaum and Rubin (1983). Propensity score matching has become a popular method for estimating treatment effects and has been widely applied in a diverse range of fields. To our knowledge, however, this paper is the first attempt to apply matching estimation in the domain of small business lending. We first estimate propensity scores, which are the probabilities of a borrower pledging collateral conditional on the relevant covariates, including borrowers' characteristics. Next, we produce a match for each collateralized borrower by choosing non-collateralized borrowers with the "closest" propensity scores to that of the collateralized borrower. We use these collateralized borrowers as the treatment group and the selected uncollateralized borrowers as the control group. Finally, we observe the difference between the treatment group and the control group in terms of their performance one year later.

We find that riskier firms are more likely to pledge collateral and that their performance one year later is improved. This result is consistent with the "moral hazard hypothesis," which predicts that collateral induces risky firms to increase managerial effort to avoid defaulting on a loan, or reduces risky firms' incentives to engage in asset substitution. In either case, collateral improves borrower firms' ex-post performance. Our finding would also be compatible with the alternative hypothesis that collateral enhances lenders' monitoring incentive (monitoring hypothesis) and the hypothesis that collateral improves borrowing firms' access to larger amounts of credit (availability hypothesis). We obtain, however, little empirical support for these hypotheses. To some extent, the above findings also hold for personal guarantees, but the results are weaker.

The structure of the paper is as follows: Section 2 provides a description of the existing theoretical and empirical literature on collateral and personal guarantees; Section 3 posits our empirical hypotheses to be examined; Section 4 explains our data set and the empirical methodology employed; Section 5 presents the empirical results accompanied by a variety of robustness checks; and Section 6 concludes.

2. Previous Literature

There is an abundance of theoretical and empirical literature on the use of collateral. We review three strands of literature that are relevant for our analysis on the relationship between collateral and the ex-post performance of borrower firms. These focus, respectively, on the relationship between collateral and borrowers' credit risk, the relationship between collateral and the lender's screening and monitoring activities, and the relationship between collateral and loan availability.

We begin with the literature on the relationship between collateral and borrowers' credit risk. Lending practitioners frequently point out that risky firms are more likely to pledge collateral. That is, financial institutions assess the riskiness of applicant firms in order to determine whether to extend secured loans, unsecured loans, or no loans at all. If a financial institution finds that an applicant qualifies for a secured loan, it will ask the prospective borrower to pledge collateral. Boot, Thakor, and Udell (1991) develop a theoretical model of this "sorting-by-observed risk" practice. They consider the situation in which the quality of the borrower's project is known to both the borrower and the lender but the borrower's effort is private information. The model of Boot, Thakor, and Udell predicts that, in equilibrium, the safer borrower is offered an unsecured loan, while the riskier borrower is offered a collateralized loan, because it is the riskier borrower whose marginal return to effort is larger.[5] Thus,

[5] The "sorting-by-observed risk" hypothesis represents one convincing theory of what determines whether a borrower pledges collateral; however, there is another plausible hypothesis in which borrower riskiness is assumed to be unobservable. This hypothesis is referred to as the "sorting-by-unobserved risk," or signaling hypothesis. Bester (1985), for example, argues that collateral can produce sorting across borrower types when the pledging of collateral is costly. Similarly, Besanko and Thakor (1987) posit that in the case where lenders are at an informational disadvantage regarding borrowers' default probability, collateral can attenuate credit rationing. These theoretical studies suggest that there is a negative relationship between collateral and (unobservable) borrower riskiness, because safer borrowers tend to pledge collateral as a signaling device to inform lenders that they are actually less risky. However, there are few empirical studies that provide support for the "sorting-by-unobserved risk" hypothesis. One exception is the study by Jiménez, Salas, and Saurina (2006), which examines how ex-post borrower riskiness is associated with the use of collateral and finds that, among sub-samples of young firms, safer borrowers are more likely to

collateral provides an incentive to limit moral hazard by the borrower. Stulz and Johnson (1985) argue that collateral is useful in mitigating another type of moral hazard: asset substitution. If the borrower's actions after the loan is extended are private information, then firms with little capital have incentives to choose risky projects or cash payouts at the expense of creditors. Collateral prevents firms from engaging in such asset substitution.

Based on Boot, Thakor, and Udell (1991) and Stulz and Johnson (1985), we can make two empirical predictions. First, the provision of collateral is positively associated with the borrower's ex-ante riskiness. Second, collateralized borrowers become less risky over time, because they make greater efforts to avoid defaulting and/or refrain from asset substitution. There are many studies providing empirical evidence for the first prediction, including the seminal work by Orgler (1970) as well as recent studies such as Berger and Udell (1990; 1995), Brick and Palia (2007), Jiménez, Salas, and Saurina (2006), and Jiménez and Saurina (2004). However, there are few empirical studies that have investigated the ex-post performance of collateralized borrowers, exceptions being Berger and Udell (1990) and Jiménez and Saurina (2004). Both of these studies find that the ex-post performance of collateralized borrowers, such as the probability of default, is worse than that of uncollateralized borrowers, and they attribute this finding to the higher ex-ante riskiness of collateralized borrowers. In other words, there are no empirical studies which find ex-post improvements of collateralized borrowers' performance, presumably because it is difficult to control for the possible selection bias of collateral provision.

The second strand of literature examines the relationship between collateral and the lender's screening and monitoring activities. Manove, Padilla, and Pagano (2001) investigate the relationship between the use of collateral and screening by the lender. They argue that

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pledge collateral.

collateral is considered as a substitute for the evaluation of borrowers' riskiness. Thus, banks that are highly protected by collateral may become "lazy" in the sense that they perform less screening than what is socially optimal for the projects they finance. Longhofer and Santos (2000) and Rajan and Winton (1995), on the other hand, show theoretically that collateral may serve as a contractual device to increase the lender's screening and monitoring incentive. Longhofer and Santos, for example, argue that collateral is effective in raising the bank's seniority in the presence of several creditors and enhances its screening and monitoring. However, the empirical evidence on the relationship between collateral and lenders' screening and monitoring is mixed. Jiménez, Salas, and Saurina (2006), for example, find that banks with a low level of expertise in small business lending use collateral as a substitute for poor evaluation capabilities, while Voordeckers and Steijvers (2006) suggest that the intensity of credit evaluation does not have a significant effect on whether loans are collateralized. Meanwhile, Ono and Uesugi (2008) find evidence that monitoring intensity by the main bank, as measured by the frequency of document submission, is positively associated with collateral being pledged.

Finally, many of the theoretical studies that provide a positive rationale for collateral predict that collateral increases loan availability. For example, Bester (1985) and Besanko and Thakor (1987) show that under informational asymmetry, collateral serves as a sorting device for borrowers' ex-ante riskiness and thus attenuates credit rationing, enabling them to implement projects with positive net present value (NPV). Similarly, Stulz and Johnson (1985) argue that by providing collateral to creditors, borrowers become less likely to suffer from the underinvestment problem. Focusing on lenders' incentives, Inderst and Mueller (2007) show that collateral raises the probability of loan approval and thus facilitates the financing of projects with positive NPV.[6]

3. Empirical Hypotheses

Based on the theoretical literature on how ex-post borrower characteristics are affected by the use of collateral, we have several testable hypotheses. Our main focus is on the association between borrower riskiness and collateral. We posit the following theoretical predictions:[7]

Hypothesis 1 (Moral Hazard Hypothesis): Borrowers with high observed riskiness are more likely to pledge collateral and improve their performances afterward by exerting greater effort, resulting in increased profitability, and/or by refraining from asset substitution, resulting in reduced riskiness. Alternatively, if the lender requires observably risky borrowers to pledge collateral in order to reduce its risk exposure and to substitute for its monitoring effort, then there is no reduction in the ex-post riskiness of collateralized borrowers.

The reasoning underlying the first part of this hypothesis is based on Boot, Thakor, and Udell (1991) and Stulz and Johnson (1985). Boot, Thakor, and Udell argue that it is riskier borrowers that tend to provide collateral, and then choose a higher level of managerial effort. The reason for this incentive effect is that collateralized borrowers would lose their pledged assets upon default, and hence strive to decrease their default probability by increasing their efforts. Moreover, because the decrease in default probability by choosing a higher level of effort is larger for riskier firms, it is riskier borrowers that are more likely to pledge collateral.

[6] In contrast, the model of Manove, Padilla, and Pagano (2001) shows that “lenient” provision of secured credit may be bad for society because lenders approve projects with negative NPV.
[7] As noted in footnote 5, there exists another important hypothesis, the “sorting-by-unobserved risk” hypothesis, which predicts that borrowers with low unobserved riskiness are more likely to pledge collateral. However, since it is difficult to identify borrowers’ unobservable riskiness in our data set, we do not explicitly examine this hypothesis. We will briefly discuss how the “sorting-by-unobserved risk” hypothesis is related to our empirical results in footnote 15.

Stulz and Johnson consider the agency problem between firm owners and debt holders. Because of limited liability, firm owners may prefer to choose risky projects at the expense of lenders, with this agency problem being particularly acute for financially distressed firms. Collateral will prevent a distressed firm from engaging in such asset substitution, and thus riskier firms tend to pledge collateral more often.

The latter part of the hypothesis, which is based on the model by Manove, Padilla, and Pagano, emphasizes lenders' incentive to reduce their exposure to borrowers' credit risk. In this case, lenders who are protected by collateral may perform less screening and monitoring than what is socially optimal, which results in a deteriorating ex-post performance of borrowers.

The next two hypotheses concern how collateralized borrowers improve their performance.

Hypothesis 2 (Monitoring Hypothesis): The performance of collateralized borrowers improves after the loan is made because they are subject to more intensive monitoring by financial institutions.

Hypothesis 3 (Availability Hypothesis): Collateralized borrowers experience a greater improvement in performance because they find it easier to gain approval for larger loan amounts.

Hypothesis 2 is based on the theoretical models of Longhofer and Santos (2000) and Rajan and Winton (1995), in which collateral serves as a contractual device to increase the lender's monitoring incentive. More intensive monitoring by the lender attenuates the problem of borrower moral hazard and thus improves borrowers' creditworthiness and performance.

Another possible path for borrowers to improve their performance is through better access to funds in order to implement projects with positive net present value, which we posit in Hypothesis 3. Besanko and Thakor (1987), for example, show that collateral mitigates the problem of credit rationing and thus improves credit availability. Similarly, if collateral reduces the agency problem between shareholders and lenders, it also increases the provision of credit (Stulz and Johnson, 1985). By investing funds in projects with positive net present value, collateralized borrowers will improve their profitability and creditworthiness.

Hypotheses 2 and 3 are compatible with the first part of Hypothesis 1 in that all three hypotheses expect that the performance of collateralized borrowers will improve; in other words, they are not mutually exclusive. For example, lenders are eager to provide more funds to those who pledge collateral (Hypothesis 3) because the moral hazard incentive for such borrowers is curbed (Hypothesis 1). However, Hypotheses 2 and 3 contrast with the first part of Hypothesis 1 in that the improvements in borrowers' performance are driven by the lenders' incentives and actions such as their monitoring frequency and their willingness to supply larger loan amounts, while Hypothesis 1 focuses *solely* on borrowers' incentives.

4. Data Set and Empirical Approach

4.1 Data

We construct a firm-level panel data set to analyze the ex-post performance of borrowers. The data set is based on the *Surveys of the Financial Environment* (SFE) implemented by the Small and Medium Enterprise Agency of Japan in the years 2001-2004, and the *Financial Information Database* (FID) that covers the years 2001-2005 and is collated by Tokyo Shoko Research Inc., a commercial credit research firm. The yearly SFE survey is based on a sample drawn from the FID that contains the balance sheets and income statements of firms. The SFE asks a variety of detailed questions regarding the financial transactions between a borrower and its main bank, such as the usage of collateral and personal guarantees. By combining the SFE and the FID, we have a rich firm-level data set that includes not only the financial statements of surveyed firms, but also qualitative information regarding their financial transactions.

For our analysis, we need information for at least three periods in order to examine the effect of collateral on borrowers' ex-post performance. We also need to know whether collateral is provided in periods *t-1* and *t*, and the development of borrowers' performance between periods *t* and *t+1*. Using the data in period *t-1* and *t*, we produce a "match" for each collateralized borrower with non-collateralized borrowers by estimating a probit model that takes account of the various factors that are likely to affect whether borrowers pledge collateral. Then, using the data from periods *t* and *t+1*, we measure the effect of collateral on borrowers' ex-post performance by observing the difference between matched collateralized borrowers and non-collateralized borrowers.

Combining the SFE and FID, and using five years of data, we construct three panel data sets for the years 2001-2003, 2002-2004, and 2003-2005. We then concatenate these three data sets into one panel data set. The initial year of each panel data set is labeled year *t-1*, the second year is year *t*, and the final year is year *t+1*. We add dummies representing the initial year in order to distinguish these three panel data sets with different starting years.

For our analysis, we exclude the following observations from our data set. First, observations where one or more of the variables (described in the next subsection) fall into either the upper or lower 0.5 percentile of the total distribution were omitted from the sample. Second, in order to focus on firms that mostly depend on bank loans for their financing, the sample is restricted to borrowers that fulfill the legal definition of SMEs in Japan, which is a firm that has either 300 or fewer employees, or 300 million yen or less of registered capital.

Third, the sample is confined to firms with positive borrowings outstanding; that is, firms whose short-term loans, long-term loans and discounted bills outstanding add up to zero are omitted from the sample. Fourth, in order to identify the year in which borrowers newly pledged collateral (personal guarantees), observations of firms that used collateral (personal guarantees) in period *t-1* are omitted.

Lastly, and most importantly, the effects of government credit guarantees on collateral need to be isolated. We deal with this issue by omitting from the sample all observations of borrowers that made use of government guarantee programs in any way.[8] We do so because loans covered by the credit guarantee program enjoy a 100 percent guarantee of principal and interest, meaning that lenders bear no credit risk for the guaranteed loan amounts and thus have no incentive to require borrowers to pledge collateral.

4.2 Variables

After screening our data as aforementioned, we are left with 543 observations for the analysis on collateral and 766 observations for the analysis on personal guarantees.[9] The variables we use are as follows.

First, in order to distinguish whether a borrower's loan is collateralized or not, we use two binary variables: whether collateral is used ($COLL_t$) or whether personal guarantees are used ($GUAR_t$). The SFE defines collateral as physical assets or securities that the creditor can sell in the event that the borrower defaults. A personal guarantee refers to a contractual

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[8] Note, however, that the borrowers not covered by government guarantees in any way are larger and less risky. Hence, limiting our sample to borrowers that do not use government credit guarantee programs at all may introduce a size bias in the sample. As a robustness check, we also employ another data set which excludes only borrowers whose *entire* loans were covered by government credit guarantees (see Section 5.3).

[9] The corresponding numbers of observations when we count borrowers whose entire portfolio of loans from their main banks is not completely guaranteed by the government are 701 (collateral) and 928 (personal guarantees). Descriptive statistics for this data set are presented in the Appendix Table.

obligation of the business representative to repay loans in the event of a default. The dummy variable $COLL_t$ ($GUAR_t$) equals one if the borrower does not pledge collateral (personal guarantees) in year *t-1* but newly pledges collateral (personal guarantees) in year *t* to its main bank; $COLL_t$ ($GUAR_t$) equals zero if the borrower does not pledge collateral (personal guarantees) in either year *t-1* or *t*. In our sample, the number of borrowers that newly pledge collateral (personal guarantees) in year *t* is 115 (179), while the number of borrowers that continue not to pledge collateral (personal guarantees) is 428 (587).

Second, we use two sets of variables to evaluate ex-post firm performance and to examine Hypothesis 1. The first set of variables measures firm performance: the profitability of a firm as measured by the return on assets (ROA_t: the ratio of pre-tax operating profits to total assets) and the interest coverage ratio ($ICOVER_t$: the ratio of pre-tax operating profits to interest expenses), while the creditworthiness of a firm as measured by the capital ratio (CAP_t: the ratio of capital to total assets). The second set of variables captures whether a firm is in financial distress. For this, we use several binary dummy variables, including a dummy indicating whether a borrower defaults on a loan in year *t* ($DEFAULT_t$), a dummy for a negative ROA (ROA_NG_t) meaning the firm is in deficit, a dummy for interest coverage being less than or equal to unity ($ICOVER\_SM_t$) meaning operating profit is insufficient to cover interest expenses, and a dummy for a negative capital ratio (CAP_NG_t), that is, the firm has negative net worth.

Third, in order to investigate Hypotheses 2 and 3, the following variables are employed. Hypothesis 2 relates ex-post firm performance to the lender's monitoring activity. Our proxy variable for the lender's monitoring is the frequency of a firm's document submissions to its main bank (DOC_t). DOC_t ranges from a minimum index value of 1 for the lowest document submission frequency (once a year) to a maximum of 4 for the highest document submission frequency (once every one or two months). Hypothesis 3 attributes ex-post firm performance to the availability of credit to the borrower firm. We measure the availability of credit by an index variable of the main bank's response to a borrower's loan application (RES_t) and the fixed-asset ratio ($FIXED_t$). RES_t takes a value of 1 if an application was rejected or the loan amount was reduced, a value of 2 if a loan application was approved, and a value of 3 if the lender solicited the borrower to increase the loan amount.[10] $FIXED_t$ is the ratio of fixed tangible assets to total assets. We use this variable because if the collateral increases loan availability for borrower firms, then it is likely to result in increases in their fixed investment.

Finally, the following are the proxy variables we employ for borrower characteristics and the borrower-lender relationship that determine the use of collateral and personal guarantees in period *t*. Firm characteristic variables include the log of the number of employees ($LnEMP_t$), the log of total borrowings ($LnLIAB_t$), the long-term borrowing ratio ($LONG_t$: the ratio of long-term loans, with maturity greater than one year, to total assets), the land ratio ($LAND_t$: the ratio of real estate holdings to total assets), the cash ratio ($CASH_t$: the ratio of cash holdings to total assets), and the interest payment rate ($RATE_t$: interest expenses divided by the total amount of borrowing). These variables are constructed from a firm's financial statements in the FID. To measure the extent of commingling risk that is likely to be positively associated with the use of personal guarantees, we construct a dummy variable that equals one if a family member of the CEO of the borrower firm is a major shareholder ($OWNER_t$). As proxies for the borrower-lender relationship, we use the number of banks with which the borrower has transactions ($BANKS_t$) and the log of the duration of the borrower-main bank relationship

[10] We also employ an alternative definition of RES_t, where it is a binary dummy variable that takes a value of 1 if an application was rejected or the loan amount reduced, and 0 otherwise. The empirical results obtained are qualitatively the same in both cases; therefore, we only report the results for our main definition of RES_t.

($LnDURATION_t$). The underlying assumption is that the smaller the number of banks that a borrower has transactions with and the longer the years that a borrower has transactions with the main bank, the more solid are the borrower-lender relationships. Year, industry, and regional dummies as well as a dummy for the type of main bank are also included.

4.3 Empirical Approach

Using the data set just described, we proceed to examine the effect of borrowers' pledging of collateral. Note, however, that a simple comparison of the ex-post performance of collateral users and non-users is not appropriate because of possible selection bias. For example, if the borrower firms that pledge collateral are riskier than those not pledging collateral, then a simple comparison of the ex-post performance between the two groups confounds ex-ante riskiness and ex-post riskiness (changes in borrowers' riskiness after the loan is extended). To circumvent the problem, we need to control for any possible selection bias in our estimation. To do so, we employ the matching estimation approach. The procedure is as follows:

(i) We implement a probit estimation that models the probability of borrowers' pledging collateral in year *t* conditional on covariates observed in the same year. Borrowers that pledge collateral ($COLL_t = 1$) are labeled treatment observations. We then attach a propensity score to each observation. The propensity score $e(\cdot)$ is defined as

$$e(X_t) \equiv \Pr(COLL_t = 1 | X_t) \tag{1}$$

where X_t is a vector of covariates in the collateral equation.

(ii) Next, for each treatment observation, we identify matched observations from the uncollateralized borrower sample. The matched observations are those that have the "closest" propensity scores to a particular treatment observation and are labeled control observations.

These matched observations are chosen from the same calendar year as the treatment observation. It should also be noted that we use a non-treated observation more than once as a control, that is, a non-treatment observation may be used as a control for one treatment observation and as a control for another treatment observation at the same time. There are several matching algorithms to find the “closest” control observations. As a baseline for our analysis, we employ *k* nearest matching, in which the arbitrarily determined *k* observations whose propensity scores are the closest to each treatment observation are chosen.[11]

(iii) Finally, we compare the change (yearly difference) in the ex-post performance variables of the treatment and the control group from year *t* to year *t+1*. To be precise, to test Hypothesis 1 we use the difference-in-difference (DID) estimator regarding firms’ ex-post performance variables described above, where the DID estimator is defined as $\Delta Y_{t+1}^{T} - \Delta Y_{t+1}^{C}$ where Y indicates the performance variable and uppercase T and C stand for the treatment and the control group, respectively. We expect an improvement in the DID estimators if the moral hazard hypothesis applies. To examine the validity of Hypotheses 2 and 3, we calculate the DID with respect to the monitoring and availability variables. For Hypotheses 2 and 3, we measure the changes from year *t-1* to *t* because these hypotheses presume contemporaneous relationships between the proxy variables and the provision of collateral. We expect a more sizable increase of document submission frequency, a larger improvement of the main bank’s lending attitude, and a more sizable increase of the fixed-asset ratio if these hypotheses hold.

One of the benefits of employing propensity score matching estimation is that we can match treatment and control observations using the scalar propensity score. The propensity

[11] In this paper we use *k*=5. Because the results of our estimation may be sensitive to the matching algorithm we choose, as a robustness check, in Section 5.3 we also report results using different matching algorithms: 10-nearest matching, radius matching, and kernel matching.

score, which is the conditional probability of being treated given the value of observed characteristics, is a very useful variable in dealing with a highly dimensional vector of covariates. Rosenbaum and Rubin (1983) show that treatment observations (in our case those who pledged collateral) and control observations (those who did not pledge collateral) with the same propensity score value have the same distribution of the full vector of covariates. It is thus sufficient to match firms in terms of the propensity score in order to obtain the same probability distribution of covariates for treatment and control observations.

In propensity score matching, an assumption known as unconfoundedness has to be satisfied so that the differences in ex-post performance variables between the treated observations and the controlled observations with the same values for covariates are attributable to the treatment effect of providing collateral (Becker and Ichino, 2002; Caliendo and Kopeinig, 2008). To ensure this, the following balancing condition of pretreatment variables given the propensity score must be satisfied:

$$COLL_t \perp X_t | e(X_t) \qquad (2)$$

In other words, for a given propensity score, treatment observations are randomly chosen, and therefore, the treatment sample and the non-treated sample are identical on average.

In order to verify that (2) holds, we implement the following testing procedure after the first step of the matching observation: (i) based on the estimated probit model, we split the sample such that the average propensity scores of the treated and non-treated groups do not differ, and (ii) within all intervals, test that the means of every element of X_t do not differ significantly between treated and non-treated observations. If there are no statistically significant differences between the two, we can then proceed to estimate the treatment effect in the second step with some confidence.

5. Results

5.1 Propensity Score Estimation

We start from the propensity score estimation. Table 1 lists the means of the variables we use in this estimation.

(Insert Table 1)

In our model, the propensity score is the conditional probability of a borrower pledging collateral and personal guarantees to its main bank in year t given the values of observed borrower characteristics and the borrower-bank relationship in the same year. There are two dependent variables, a binary dummy variable on the use of collateral in year t ($COLL_t$) and a binary variable on the use of personal guarantees in year t ($GUAR_t$). Note that as a result of the way we have constructed our data set, the values of $COLL_{t-1}$ and $GUAR_{t-1}$ are zero.

Explanatory variables are as follows. First, we employ the following borrower characteristic variables: the log of the number of employees ($LnEMP_t$), the log of total borrowings ($LnLIAB_t$), the capital-asset ratio (CAP_t), the long-term borrowing ratio ($LONG_t$), the land ratio ($LAND_t$), profitability in terms of ROA (ROA_t) and the interest coverage ratio ($ICOVER_t$), liquidity as measured by the cash-to-asset ratio ($CASH_t$), the interest payment rate ($RATE_t$), and the dummy indicating whether a family member of the CEO is a major shareholder ($OWNER_t$). In addition, considering the possibility that very risky firms newly pledge collateral more often than the other firms, we use three dummy variables to identify borrowers in financial distress. These is a dummy for a negative ROA (ROA_NG_t), a dummy indicating whether the interest coverage is less than or equal to unity ($ICOVER\_SM_t$), and a dummy indicating whether the capital ratio is negative (CAP_NG_t). Second, two variables representing the intimacy of borrower-lender relationships are used: the number of banks ($BANKS_t$) and the log of the duration (in years) of the borrower-lender relationship

($LnDURATION_t$). Third, to capture whether the collateral (personal guarantee) is pledged in tandem with the provision of personal guarantees (collateral), dummy variables for personal guarantees (collateral) in year *t-1* and *t* are included as explanatory variables in the collateral (personal guarantee) equation. Finally, year, industry, and regional dummies as well as dummies for the type of main bank are included.

The probit estimation results on the use of collateral and personal guarantees are presented in Table 2. In the estimation on the use of collateral, there are several significant coefficients. $LONG_t$ is positive and significant, which is consistent with practitioners' observation that long-term loans often finance purchases of machinery or equipment and these physical assets tend to be used as collateral. Similarly, $LAND_t$ is positive and significant, indicating that firms whose real estate holdings make up a larger share of their assets are more likely to pledge collateral. Regarding the firm performance variables, a significant positive coefficient is obtained for $ICOVER\_SM_t$, while the coefficient on $ICOVER_t$ is insignificant. The significant positive coefficient on $ICOVER\_SM_t$ suggests that whether borrowers fall below the threshold where their operating profits are sufficient to cover interest expenses is a critical determinant of whether lenders require collateral. $LnDURATION_t$ is positive and significant, implying that borrowers who have established a longer-term relationship with their main bank are more likely to pledge collateral. Finally, the coefficient on $GUAR_t$ is positive and significant, meaning that borrowers who pledge a personal guarantee to their main bank are likely to newly pledge collateral simultaneously.

(Insert Table 2)

In the estimation on the use of personal guarantees, there are several differences from the results for the collateral equation. Both $LnEMP_t$ and $LnLIAB_t$ have significant negative coefficients, implying that firms with fewer employees and smaller amounts of borrowing outstanding tend to pledge personal guarantees more often. In addition, the dummy variable $OWNER_t$ has a significant positive coefficient. Owner-CEOs are less hesitant to pledge personal guarantees since they have a sizable stake in the firm. Taken together, the result suggests that firms' main bank tends to require personal guarantees from smaller firms that can be easily controlled by the owner-CEO, presumably in order to attenuate the risk of the commingling of representatives' personal wealth and business assets. Although only weakly significant, the coefficient for the capital-asset ratio CAP_t is negative, indicating that firms with less capital are more likely to pledge personal guarantees. Finally, similar to the result for the collateral equation, the coefficient on $COLL_t$ is positive and significant. Thus, borrowers who pledge collateral to their main bank are likely to newly pledge a personal guarantee simultaneously.

5.2 Treatment Effect Estimation

Having obtained the propensity score for each observation, we match each treatment observation of a borrower that pledges collateral (personal guarantees) in year *t* with control observations of borrowers that do not pledge collateral (personal guarantees) in that year. There are 115 treatment observations that newly pledge collateral and 179 treatment observations that newly pledge personal guarantees. We choose five neighboring control observations for each treatment observation in the same calendar year, in terms of the distance measured by the propensity scores.

For these treatment and control observations that are matched, in order to examine Hypothesis 1 we use several variables to measure the change in borrowers' performance between year *t* and year *t+1*. First, for both the treatment and the control group, the change in the performance variables is measured by ΔROA_{t+1}^{j} ($\equiv ROA_{t+1}^{j} - ROA_{t}^{j}$), ΔCAP_{t+1}^{j}, and

$\Delta ICOVER_{t+1}^{j}$, where $j=\{T,C\}$ and T and C stand for the treatment and the control group, respectively. Because the change in the interest coverage ratio is also affected by the change in the interest payment rate, we also check $\Delta RATE_{t+1}^{j}$. Second, we examine the change in the probability of financial distress. We use several ways to define borrower financial distress, including default, a negative capital ratio, interest coverage less than or equal to unity, and a negative ROA. We measure the probability of a borrower falling into a certain type of distress and then take the difference in this probability between year *t* and year *t+1*. Hence, the change in distress probabilities is measured by $\Delta p_{t+1}^{j}(DEFAULT=1)$, [12] $\Delta p_{t+1}^{j}(CAP\_NG=1)$, $\Delta p_{t+1}^{j}(ICOVER\_SM=1)$, and $\Delta p_{t+1}^{j}(ROA\_NG=1)$, where $j=\{T,C\}$. Finally, for both performance variables and financial distress variables, we take differences in the change in these variables between the treatment and the control group; for example, in the case of ROA we measure $\Delta ROA_{t+1}^{T}-\Delta ROA_{t+1}^{C}$. We then use these to estimate the treatment effect of pledging collateral or personal guarantees.

Turning to the results, we begin with the treatment effect of pledging collateral, shown in Table 3. Among the borrower performance variables, ΔROA_{t+1}^{T} is higher than ΔROA_{t+1}^{C} by 1.2 percentage points, which is the treatment effect and statistically significant at the 5 percent level. ΔCAP_{t+1}^{T} is higher than ΔCAP_{t+1}^{C} by 1.9 percentage points, and the difference is also statistically significant at the 5 percent level. $\Delta ICOVER_{t+1}^{T}$ is higher than $\Delta ICOVER_{t+1}^{C}$, but the effect is not statistically significant. Note that $\Delta RATE_{t+1}^{T}$ is higher than $\Delta RATE_{t+1}^{C}$ by a statistically significant 0.7 percentage points, implying that borrowers pledging collateral face a larger increase in interest payment rates than those who do not. Looking at the variables of financial distress probabilities, we find that all of them show that borrowers in the treatment

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[12] Since we do not have data on defaults in year *t*, $\Delta p_{t+1}^{j}(DEFAULT=1)$ is actually $p_{t+1}^{j}(DEFAULT=1)$.

group see a greater reduction in their probability of falling into financial distress than those in the control group. Specifically, the differences between the treatment and the control group are significant at the 1% level for $\Delta p_{t+1}^{j}(ICOVER\_SM = 1)$ and significant at the 5% level for $\Delta p_{t+1}^{j}(ROA\_NG = 1)$.

Finally, in order to examine Hypotheses 2 and 3, we estimate the treatment effect measuring the changes in financial conditions from year *t-1* to year *t*. We do so because these hypotheses presume contemporaneous relationships between the proxy variables employed and the pledging of collateral and personal guarantees. Table 3 shows that the difference between the treatment and the control group is negative for ΔDOC_{t}^{j}, meaning that collateralized borrowers are somewhat less frequently monitored by their main banks than non-collateralized borrowers. This contradicts the monitoring hypothesis, although the effect is statistically insignificant. Turning to the credit availability variables, consistent with the prediction that collateralized borrowers will experience an improvement in credit availability, the differences between the treatment and the control group are positive, but statistically insignificant, for both the index variable indicating main banks' response to borrowers' loan applications, ΔRES_{t}^{j}, and for the fixed asset ratio $\Delta FIXED_{t}^{j}$,. Thus, we cannot state that for collateralized borrowers the main bank exerts either more or less intensive monitoring effort, or that the bank becomes either more or less accommodative in approving loans.

(Insert Table 3)

Next, we present the results for the treatment effect of pledging personal guarantees, shown in Table 4. In comparison with the results obtained for collateral, the number of variables for which the difference between the treatment and the control group is statistically significant is limited. Among the borrower performance variables, ΔROA_{t+1}^{T} and ΔCAP_{t+1}^{T} are slightly higher than the control group counterparts, but the difference is not significant. Moreover,

$\Delta ICOVER_{t+1}^{T}$ is lower than $\Delta ICOVER_{t+1}^{C}$ and the effect is weakly significant at the 10% level, hinting at a deterioration in the ex-post performance of borrowers that pledge personal guarantees. Among the variables representing the probabilities of financial distress, the probabilities of falling into negative net worth (ΔCAP_{t+1}^{j}) and a profit deficit (ΔROA_{t+1}^{j}) display a larger decrease for the treatment group than the control group, but the margins are not significant. Finally, regarding the variables on monitoring frequency and credit availability, the coefficient obtained for ΔDOC_{t}^{j} is consistent with Hypothesis 2, but the effect is not significant and the signs of the coefficients on ΔRES_{t}^{j} and $\Delta FIXED_{t}^{j}$ are contradictory to Hypothesis 3.

(Insert Table 4)

5.3 Robustness Check

The results presented in the previous subsection are based on a sample which completely excludes borrowers that received government credit guarantees. Restricting our sample in this way is effective in controlling for the distorting influence of government guarantees on borrowers' decisions with regard to the use of collateral or personal guarantees. However, eliminating all borrowers with loans that are subject to government guarantees, even when these loans make up only a miniscule fraction of loan portfolios, comes at a cost, namely that the sample is limited to larger and less risky SMEs.

Hence, as a robustness check of our results, we conduct similar estimations in which the potentially distorting role of government guarantees is controlled in a less stringent manner. That is, we exclude from the sample only those borrowers whose *entire* loans supplied by their main bank are covered by government credit guarantees. Extending loans to firms whose entire loans are covered by government guarantees is riskless for banks. In this case, banks have no incentive to require borrowers to pledge collateral or personal guarantees. In contrast, banks do have incentives to require collateral or personal guarantees when not all of a borrower's loans are covered by government guarantees.

The results of these estimations, shown in Tables 5 (the propensity score estimation) and 6 (the treatment effect estimation), are qualitatively not very different from the base case results for the sample consisting only of firms with no government guarantees at all.[13] In the first-step probit estimation to obtain propensity scores, we find that the overall fitness of the equations improves as indicated by the increase in Pseudo R-squared. The level of significance of most explanatory variables is qualitatively the same as in the base case, except for some firm performance variables in the collateral equation. That is, in contrast with the result in the base case, the positive coefficient on $ICOVER\_SM_t$ becomes insignificant. Moreover, the coefficient on ROA_t is positive and significant, implying that more profitable firms tend to pledge collateral more often. The added explanatory variable $GOVGUAR_t$, which is the binary dummy variable representing whether a borrower partially uses government credit guarantees for loans supplied by the main bank, has a positive and significant coefficient both in the collateral and the personal guarantees equation: borrowers using government guarantees tend to pledge collateral and personal guarantees more often than borrowers that do not rely on such guarantees. Turning to the second-step estimation to obtain the treatment effect, we observe that the effect is qualitatively the same but not as statistically significant as in the base case estimations. With respect to the treatment effects for collateral, all the coefficients for the ex-post firm performance variables have the same signs as in the baseline estimation except for ΔCAP_{t+1}^j, whose coefficient is reversed but is statistically insignificant. The profitability of

[13] The summary statistics for the sample consisting of firms that partially use government guarantees are presented in the Appendix Table.

collateralized borrowers improves and their probability of financial distress declines when compared with non-collateralized borrowers, but the size of the coefficient becomes smaller and their statistical significance decreases. With respect to Hypotheses 2 and 3, there is no strong supportive evidence: the signs of ΔDOC_t^j , ΔRES_t^j , $\Delta FIXED_t^j$ are consistent with the monitoring and availability hypotheses, but their effects are not statistically significant. Regarding personal guarantees, we find some evidence that the probability of being in financial distress is higher for those firms that pledge personal guarantees: the coefficients on $\Delta p_{t+1}^j(ICOVER\_SM = 1)$ and $\Delta p_{t+1}^j(ROA\_NG = 1)$ are positive and significant. In contrast with the positive treatment effects for collateral, we cannot find evidence that personal guarantees mitigate moral hazard on the part of borrowers and improve their ex-post performance.

(Insert Tables 5 and 6)

As another robustness check of the results obtained in the base case, we estimate the treatment effects for collateral and personal guarantees using different matching algorithms: 10-nearest matching, Kernel matching, and radius matching. For our robustness check using different matching algorithms, we use the sample consisting only of firms that used no government credit guarantees at all. Kernel matching is a nonparametric estimation technique that uses the weighted averages of nearly all observations from the non-treated observations to construct the control group. Radius matching chooses all observations that lie within an arbitrarily determined "radius (propensity score range)" as the control group.

The results using these alternative matching algorithms are displayed in Tables 7 and 8, presenting the treatment effect estimations for collateral and personal guarantees, respectively. Although there are a few cases in which the sign or the statistical significance of coefficients is different from the 5-nearest matching estimation in Section 5.2 (Tables 3 and 4), the results are qualitatively the same. Collateralized borrowers experience a greater improvement in ex-post performance than non-collateralized borrowers, and this treatment effect is weaker for personal guarantees.[14]

(Insert Tables 7 and 8)

5.4 Discussion

On balance, the results of the above estimations indicate that the treatment effect of providing collateral is that it lowers the riskiness and increases the profitability of firms that pledge collateral. On the other hand, these effects are tenuous in the case of personal guarantees. Given these results, our next task is to consider how these improvements are achieved. Our main hypothesis (Hypothesis 1) is that the effects are brought about by the reduction of moral hazard on the part of borrowers. The alternative hypotheses are the following: the monitoring hypothesis, according to which collateral or personal guarantees induce banks to exert greater monitoring effort, which in turn results in borrowers exercising greater discipline in reducing their riskiness (Hypothesis 2); and the availability hypothesis, according to which borrowers obtain better access to funds and thus improve their quality by investing in projects with positive net present values (Hypothesis 3). In contrast with Hypothesis 1, these hypotheses focus on how lenders' actions affect borrowers' performance.

In the propensity score estimation, pledging collateral is positively associated with observed borrower riskiness. And in the treatment effect estimation, borrowers with collateral see an increase in their profitability and a reduction in their riskiness as represented by the

[14] One notable exception is the treatment effect for personal guarantees in the estimation using radius matching. Here, we observe that the probabilities of default and negative net worth become smaller for the treatment group (borrowers that pledge personal guarantees). These findings are consistent with Hypothesis 1. However, because we do not obtain similar results from the other matching procedures, we decided not to attach much importance to this result.

probability of falling into financial distress. These results are consistent with the predictions of Hypothesis 1.[15]

The variables we employed to examine Hypotheses 2 and 3 are ΔDOC_t^j, ΔRES_t^j, and $\Delta FIXED_t^j$. The signs of these variables are inconsistent with the hypotheses in some cases, and the coefficients are consistent but insignificant in other cases. Thus, we cannot find supporting evidence for these hypotheses.

Therefore, we may infer that collateralized borrowers improve their performance by their own managerial effort or by refraining from asset substitution. Table 9 reinforces this conjecture. Here, we decompose the improvements in ΔROA_{t+1}^j (treatment effects in Tables 3 and 7) into three factors: the increase in gross sales, the reduction in expenses, and the reduction in total assets.[16] If the greater improvements in the profitability of collateralized borrowers (treatment effects) were driven by an increase in the availability of credit to finance new investment projects (Hypothesis 3), then it is likely that, as a result of the new investments, we would observe an increase in gross sales that would contribute to an improvement in ROA. Table 9 shows, however, that the main reason for the improvement in collateralized borrowers' ROA is a reduction in expenses, while the contribution of gross sales to the improvement in collateralized firms' profitability was actually negative. In other word, collateralized borrowers seem to increase their profitability through cost-cutting restructuring. This evidence is

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[15] Admittedly, even though the empirical evidence of the propensity score and treatment effect estimations supports Hypothesis 1, we cannot reject the signaling hypothesis which was briefly referred to in footnote 5. It is still possible that borrowers who *appear* risky use collateral to signal that they are actually riskless and reveal that they are indeed riskless through their ex-post performance.

[16] To be precise, the first term of the following equation corresponds to the increase in gross sales, while the second corresponds to the reduction in expenses and the third corresponds to the reduction in total assets. The final cross-factor term is negligible.

$$\Delta ROA = \frac{\Delta S}{A_t} - \frac{\Delta C}{A_t} + \left(S_t - C_t\right) \cdot \Delta\left(\frac{1}{A}\right) + \left(\Delta S - \Delta C\right) \cdot \Delta\left(\frac{1}{A}\right)$$

where *S*, *C*, and *A* represent gross sales, expenses, and total assets, respectively.

contradictory to the availability hypothesis, but is compatible with the theory that collateralized borrowers improve their performance through greater managerial effort or by refraining from asset substitution.[17]

6. Conclusion

Focusing on Japanese SMEs, which tend to be dependent on bank loans, we examined the effectiveness of collateral and personal guarantees in improving the ex-post performance of firms, such as their profitability and the probability of falling into financial distress. The examination was based on a unique panel data set of more than 500 Japanese SMEs covering the years 2001-2005.

Employing the matching estimation approach, we found that riskier firms were more likely to pledge collateral, and their profitability tended to increase and their riskiness tended to decline one year later. Most of the estimation results are consistent with the moral hazard hypothesis, according to which risky firms increase their efforts or refrain from asset substitution once they have pledged collateral, and as a result end up with improvements in their ex-post performance. The monitoring and availability hypotheses may offer alternative explanations, but we do not find sufficient supportive evidence in our empirical analysis. These results become tenuous for personal guarantees.

As far as we know, this is the first empirical analysis on collateralized borrowers' ex-post performance which controls for the possible selection bias in the provision of collateral. There are various possible extensions to this analysis. One is to widen the time window for observing borrowers' ex-post performance. Currently, we only have a one-year window for

[17] Although we do not find supportive evidence for the monitoring hypothesis (Hypothesis 2) in the matching estimations, the results in Table 9 may be consistent with the monitoring hypothesis in that the cost-cutting restructuring of collateralized borrowers may be the result of pressure from their main bank.

analysis due to data availability constraints. However, as more data become available over time, we may be able to extend the window to several years. Another possible extension would be to construct the data set in a different manner. In this paper, we limited our sample to borrowers that did not pledge collateral in year *t-1* and identified the treatment effect by contrasting borrowers pledging collateral in year *t* with those that did not. As an alternative, we could choose borrowers that pledged collateral in year *t-1* and identify the effect of collateral by contrasting borrowers ceasing to pledge collateral in year *t* with those continuing to pledge collateral. This would allow us to address the very interesting question of whether newly pledging collateral and ceasing to pledge collateral have symmetric treatment effects. Tackling these issues may reinforce this paper's findings, as well as further expand our understanding of how the provision of collateral attenuates moral hazard and how ceasing to pledge collateral exacerbates it.

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Table 1: Summary Statistics

	Mean					
	COLL(t-1) =0			*GUAR(t-1)* =0		
	All	*COLL(t)* =1	*COLL(t)* =0	All	*GUAR(t)* =1	*GUAR(t)* =0
GUAR(t)	0.440	0.617	0.393			
	(0.497)	(0.488)	(0.489)			
GUAR(t-1)	0.431	0.557	0.397			
	(0.496)	(0.499)	(0.490)			
COLL(t)				0.584	0.704	0.547
				(0.493)	(0.458)	(0.498)
COLL(t-1)				0.597	0.693	0.567
				(0.491)	(0.463)	(0.496)
ROA	0.025	0.023	0.026	0.024	0.022	0.025
	(0.047)	(0.044)	(0.048)	(0.046)	(0.048)	(0.046)
ICOVER	30.778	18.212	34.155	22.286	13.980	24.819
	(111.942)	(70.012)	(120.595)	(87.208)	(62.552)	(93.340)
CAP	0.339	0.373	0.330	0.343	0.380	0.331
	(0.249)	(0.247)	(0.249)	(0.221)	(0.222)	(0.220)
ROA_NG	0.175	0.226	0.161	0.187	0.218	0.177
	(0.380)	(0.420)	(0.368)	(0.390)	(0.414)	(0.382)
ICOVER_SM	0.221	0.313	0.196	0.248	0.330	0.223
	(0.415)	(0.466)	(0.398)	(0.432)	(0.471)	(0.417)
CAP_NG	0.031	0.035	0.030	0.014	0.017	0.014
	(0.174)	(0.184)	(0.172)	(0.119)	(0.129)	(0.116)
LnEMP	3.851	3.871	3.845	4.316	3.880	4.448
	(1.048)	(1.026)	(1.055)	(1.060)	(1.042)	(1.030)
LnLIAB	12.887	12.672	12.945	13.529	12.784	13.757
	(1.911)	(1.828)	(1.930)	(1.771)	(1.876)	(1.674)
LONG	0.363	0.426	0.346	0.400	0.479	0.376
	(0.366)	(0.347)	(0.369)	(0.330)	(0.357)	(0.318)
LAND	0.085	0.114	0.077	0.117	0.131	0.112
	(0.105)	(0.111)	(0.102)	(0.119)	(0.119)	(0.119)
CASH	0.181	0.180	0.182	0.153	0.196	0.140
	(0.151)	(0.137)	(0.154)	(0.131)	(0.150)	(0.122)
RATE	0.019	0.020	0.019	0.020	0.023	0.019
	(0.020)	(0.016)	(0.021)	(0.017)	(0.019)	(0.016)
OWNER	0.508	0.609	0.481	0.441	0.721	0.356
	(0.500)	(0.490)	(0.500)	(0.497)	(0.450)	(0.479)
BANKS	4.033	3.504	4.175	4.658	3.911	4.886
	(3.670)	(2.647)	(3.890)	(3.698)	(2.876)	(3.888)
LnDURATION	3.068	3.236	3.023	3.287	3.287	3.288
	(0.816)	(0.747)	(0.828)	(0.703)	(0.722)	(0.697)
Number of observations	543	115	428	766	179	587

Note: Standard deviations are in parentheses.

Table 2: Propensity Score Estimation

	Dependent Variable: *COLL*		Dependent Variable: *GUAR*	
LnEMP	0.108		-0.161	**
	(0.087)		(0.073)	
LnLIAB	-0.068		-0.140	**
	(0.065)		(0.059)	
CAP	-0.274		-0.640	*
	(0.425)		(0.358)	
LONG	0.445	**	0.115	
	(0.193)		(0.184)	
LAND	1.507	**	0.384	
	(0.691)		(0.541)	
ROA	3.248		2.111	
	(1.990)		(1.757)	
ICOVER	-0.001		-0.001	
	(0.001)		(0.001)	
CASH	-0.720		0.503	
	(0.556)		(0.498)	
RATE	0.070		-3.108	
	(3.778)		(3.986)	
OWNER	0.009		0.751	***
	(0.168)		(0.131)	
ROA_NG	-0.185		-0.165	
	(0.332)		(0.253)	
ICOVER_SM	0.788	***	0.339	
	(0.297)		(0.224)	
CAP_NG	0.015		0.084	
	(0.428)		(0.503)	
BANKS	-0.003		0.010	
	(0.027)		(0.021)	
LnDURATION	0.186	**	-0.032	
	(0.092)		(0.090)	
GUAR(t)	0.472	***		
	(0.169)			
GUAR(t-1)	-0.064			
	(0.169)			
COLL(t)			0.346	**
			(0.165)	
COLL(t-1)			0.231	
			(0.167)	
Constant	0.863		1.487	*
	(1.396)		(0.898)	
Year Dummies	Yes		Yes	
Industry Dummies	Yes		Yes	
Dummies for Main Bank Type	Yes		Yes	
Number of observations	543		766	
Pseudo R-sq	0.146		0.221	
Log likelihood	-239.573		-324.436	

Note: ***, **, * indicate a significance level of 1, 5, and 10%, respectively.

Table 3: Treatment Effect Estimation for Collateral

	Period	5-Nearest Matching Treatment	Control	Difference in Difference	
ROA	*t*	0.022	0.023		
	t+1	0.032	0.021	0.012	**
CAP	*t*	0.380	0.373		
	t+1	0.396	0.370	0.019	**
ICOVER	*t*	10.857	20.267		
	t+1	24.943	27.787	6.567	
RATE	*t*	0.018	0.024		
	t+1	0.020	0.018	0.007	***
p(DEFAULT)	*t*	0.000	0.000		
	t+1	0.000	0.006	-0.006	*
p(CAP_NG)	*t*	0.021	0.042		
	t+1	0.000	0.051	-0.029	*
p(ICOVER_SM)	*t*	0.299	0.280		
	t+1	0.138	0.290	-0.170	***
p(ROA_NG)	*t*	0.221	0.213		
	t+1	0.116	0.227	-0.120	**
DOC	*t-1*	1.569	1.502		
	t	1.585	1.575	-0.058	
RES	*t-1*	2.479	2.442		
	t	2.479	2.383	0.058	
FIXED	*t-1*	0.269	0.300		
	t	0.280	0.309	0.002	

Note: ***, **, * indicate a significance level of 1, 5, and 10%, respectively.

Table 4: Treatment Effect Estimation for Personal Guarantees

	Period	5-Nearest Matching			
		Treatment	Control	Difference in Difference	
ROA	*t*	0.021	0.029		
	t+1	0.024	0.028	0.004	
CAP	*t*	0.378	0.411		
	t+1	0.393	0.425	0.001	
ICOVER	*t*	15.438	21.110		
	t+1	16.687	33.787	-11.428	*
RATE	*t*	0.021	0.022		
	t+1	0.020	0.022	0.000	
p(DEFAULT)	*t*	0.000	0.000		
	t+1	0.000	0.000	0.000	
p(CAP_NG)	*t*	0.012	0.006		
	t+1	0.006	0.006	-0.006	
p(ICOVER_SM)	*t*	0.317	0.266		
	t+1	0.273	0.221	0.001	
p(ROA_NG)	*t*	0.210	0.178		
	t+1	0.222	0.214	-0.023	
DOC	*t-1*	1.664	1.595		
	t	1.794	1.682	0.043	
RES	*t-1*	2.435	2.426		
	t	2.377	2.501	-0.133	*
FIXED	*t-1*	0.308	0.281		
	t	0.311	0.288	-0.004	

Note: ***, **, * indicate a significance level of 1, 5, and 10%, respectively.

Table 5: Propensity Score Estimation (Users of Partial Government Guarantee)

	Dependent Variable: *COLL*	Dependent Variable: GUAR
LnEMP	0.097	-0.142 **
	(0.077)	(0.067)
LnLIAB	-0.032	-0.105 **
	(0.058)	(0.053)
CAP	-0.256	-0.674 **
	(0.374)	(0.325)
LONG	0.393 **	0.131
	(0.174)	(0.169)
LAND	1.274 **	0.750
	(0.599)	(0.485)
ROA	3.739 **	1.421
	(1.667)	(1.605)
ICOVER	-0.001	-0.001
	(0.001)	(0.001)
CASH	-0.349	0.507
	(0.459)	(0.452)
RATE	-2.598	0.620
	(3.118)	(3.147)
OWNER	0.024	0.703 ***
	(0.143)	(0.122)
ROA_NG	0.252	-0.210
	(0.262)	(0.219)
ICOVER_SM	0.374	0.312
	(0.231)	(0.192)
CAP_NG	0.144	0.263
	(0.366)	(0.430)
BANKS	-0.010	-0.002
	(0.023)	(0.019)
LnDURATION	0.232 ***	0.028
	(0.079)	(0.084)
GUAR(t)	0.427 ***	
	(0.148)	
GUAR(t-1)	-0.081	
	(0.145)	
COLL(t)		0.387 ***
		(0.147)
COLL(t-1)		0.093
		(0.144)
GOVGUAR(t)	1.106 ***	0.939 ***
	(0.193)	(0.190)
GOVGUAR(t-1)	0.350 *	0.143
	(0.207)	(0.208)
Constant	-1.840	0.129
	(0.768)	(0.734)
Year Dummies	Yes	Yes
Industry Dummies	Yes	Yes
Dummies for Main Bank Type	Yes	Yes
Number of observations	701	928
Pseudo R-sq	0.262	0.323
Log likelihood	-317.625	-399.284

Note: ***, **, * indicate a significance level of 1, 5, and 10%, respectively.

Table 6: Treatment Effect Estimation (Users of Partial Government Guarantee)

		COLL			*GUAR*		
	Period	Treatment	Control	Difference in Difference	Treatment	Control	Difference in Difference
ROA	*t*	0.019	0.020		0.018	0.025	
	t+1	0.027	0.019	0.008 *	0.020	0.025	0.001
CAP	*t*	0.334	0.328		0.339	0.359	
	t+1	0.340	0.342	-0.008	0.350	0.377	-0.006
ICOVER	*t*	7.161	12.938		10.314	12.652	
	t+1	15.064	16.131	4.710	11.790	19.364	-5.236
RATE	*t*	0.021	0.019		0.022	0.023	
	t+1	0.022	0.018	0.003 **	0.021	0.024	-0.002 **
p(DEFAULT)	*t*	0.000	0.000		0.000	0.000	
	t+1	0.000	0.001	-0.001	0.004	0.001	0.003
p(CAP_NG)	*t*	0.025	0.038		0.020	0.007	
	t+1	0.012	0.041	-0.015 *	0.012	0.006	-0.007
p(ICOVER_SM)	*t*	0.346	0.316		0.356	0.351	
	t+1	0.203	0.241	-0.068	0.312	0.215	0.092 ***
p(ROA_NG)	*t*	0.263	0.223		0.228	0.234	
	t+1	0.138	0.194	-0.096 ***	0.248	0.190	0.064 **
DOC	*t-1*	1.625	1.520		1.764	1.619	
	t	1.725	1.498	0.122	1.913	1.774	-0.006
RES	*t-1*	2.432	2.375		2.372	2.369	
	t	2.469	2.353	0.059	2.339	2.418	-0.083
FIXED	*t-1*	0.231	0.277		0.297	0.278	
	t	0.241	0.281	0.006	0.302	0.283	0.000

Note 1: ***, **, * indicate a significance level of 1, 5, and 10%, respectively.
Note 2: The matching algorithm is 5-nearest matching.

Table 7: Treatment Effect Estimations for Collateral (Different Matching Algorithms)

		10-Nearest Matching			Kernel Matching			Radius Matching		
	Period	Treatment	Control	Difference in Difference	Treatment	Control	Difference in Difference	Treatment	Control	Difference in Difference
ROA	*t*	0.022	0.025		0.022	0.024		0.022	0.026	
	t+1	0.032	0.024	0.011 **	0.032	0.024	0.011 **	0.032	0.028	0.008 *
CAP	*t*	0.380	0.370		0.380	0.368		0.380	0.338	
	t+1	0.396	0.368	0.018 **	0.396	0.367	0.017 **	0.396	0.345	0.008
ICOVER	*t*	10.857	20.902		10.857	19.171		10.857	28.898	
	t+1	24.943	33.222	1.766	24.943	29.767	3.490	24.943	39.346	3.637
RATE	*t*	0.018	0.022		0.018	0.022		0.018	0.018	
	t+1	0.020	0.018	0.006 ***	0.020	0.017	0.006 ***	0.020	0.017	0.003 **
p(DEFAULT)	*t*	0.000	0.000		0.000	0.000		0.000	0.000	
	t+1	0.000	0.007	-0.007 ***	0.000	0.003	-0.003 ***	0.000	0.001	-0.001 ***
p(CAP_NG)	*t*	0.021	0.038		0.021	0.039		0.021	0.022	
	t+1	0.000	0.047	-0.031 **	0.000	0.046	-0.028 *	0.000	0.033	-0.033 **
p(ICOVER_SM)	*t*	0.299	0.284		0.299	0.290		0.299	0.194	
	t+1	0.138	0.274	-0.151 ***	0.138	0.277	-0.148 ***	0.138	0.219	-0.185 ***
p(ROA_NG)	*t*	0.221	0.213		0.221	0.207		0.221	0.156	
	t+1	0.116	0.233	-0.125 ***	0.116	0.222	-0.120 ***	0.116	0.175	-0.125 ***
DOC	*t-1*	1.569	1.542		1.569	1.513		1.569	1.615	
	t	1.585	1.591	-0.034	1.585	1.582	-0.053	1.585	1.712	-0.082
RES	*t-1*	2.479	2.419		2.479	2.471		2.479	2.394	
	t	2.479	2.390	0.029	2.479	2.433	0.039	2.479	2.374	0.020
FIX	*t-1*	0.269	0.295		0.269	0.287		0.269	0.269	
	t	0.280	0.303	0.003	0.280	0.295	0.003	0.280	0.272	0.007

Note: ***, **, * indicate a significance level of 1, 5, and 10%, respectively.

Table 8: Treatment Effect Estimations for Personal Guarantees (Different Matching Algorithms)

		10-Nearest Matching			Kernel Matching			Radius Matching		
	Period	Treatment	Control	Difference in Difference	Treatment	Control	Difference in Difference	Treatment	Control	Difference in Difference
ROA	*t*	0.021	0.026		0.021	0.026		0.021	0.024	
	t+1	0.024	0.026	0.003	0.024	0.026	0.003	0.024	0.029	-0.002
CAP	*t*	0.378	0.411		0.378	0.393		0.378	0.339	
	t+1	0.393	0.425	0.001	0.393	0.412	-0.004	0.393	0.348	0.006
ICOVER	*t*	15.438	21.310		15.438	19.858		15.438	22.446	
	t+1	16.687	28.062	-5.503	16.687	30.550	-9.443	16.687	27.173	-3.477
RATE	*t*	0.021	0.022		0.021	0.022		0.021	0.018	
	t+1	0.020	0.021	0.000	0.020	0.022	-0.001	0.020	0.018	-0.001
p(DEFAULT)	*t*	0.000	0.000		0.000	0.000		0.000	0.000	
	t+1	0.000	0.000	0.000	0.000	0.000	-0.0004 **	0.000	0.002	-0.002 ***
p(CAP_NG)	*t*	0.012	0.009		0.012	0.016		0.012	0.006	
	t+1	0.006	0.010	-0.007	0.006	0.009	0.001	0.006	0.014	-0.014 **
p(ICOVER_SM)	*t*	0.317	0.268		0.317	0.280		0.317	0.237	
	t+1	0.273	0.227	-0.002	0.273	0.231	0.006	0.273	0.181	0.012
p(ROA_NG)	*t*	0.210	0.199		0.210	0.188		0.210	0.183	
	t+1	0.222	0.214	-0.002	0.222	0.208	-0.008	0.222	0.157	0.039
DOC	*t-1*	1.664	1.600		1.664	1.591		1.664	1.746	
	t	1.794	1.666	0.064	1.794	1.665	0.056	1.794	1.900	-0.024
RES	*t-1*	2.435	2.435		2.435	2.470		2.435	2.390	
	t	2.377	2.557	-0.180 **	2.377	2.520	-0.107	2.377	2.406	-0.073
FIX	*t-1*	0.308	0.292		0.309	0.285		0.308	0.312	
	t	0.311	0.297	-0.002	0.312	0.293	-0.004	0.311	0.318	-0.003

Note: ***, **, * indicate a significance level of 1, 5, and 10%, respectively.

Table 9: Decomposition of Treatment Effects with respect to ROA

	Treatment effect w.r.t. ROA	Contributions to the treatment effect			
		Increase in gross sales	Reduction in expenses	Reduction in total assets	Cross-factor
5-nearest matching	0.012	-0.005	0.017	0.001	-0.001
10-nearest matching	0.011	-0.025	0.036	0.001	-0.001
Kernel matching	0.011	-0.024	0.034	0.000	0.000
Radius matching	0.008	-0.029	0.037	-0.001	0.001

Note: The figures for treatment effects of collateralized borrowers with respect to ROA are taken from Tables 3 and 7. Decomposition of the treatment effects is as follows:

$$\Delta ROA = \frac{\Delta S}{A_t} - \frac{\Delta C}{A_t} + \left(S_t - C_t\right) \cdot \Delta\left(\frac{1}{A}\right) + \left(\Delta S - \Delta C\right) \cdot \Delta\left(\frac{1}{A}\right)$$

where *S*, *C*, and *A* represent gross sales, expenses, and total assets, respectively. The first term corresponds to the increase in gross sales, while the second corresponds to the reduction in expenses and the third corresponds to the reduction in total assets. The final term represents the cross-factor term.

Appendix Table: Summary Statistics (Users of Partial Government Guarantee)

	Mean					
	COLL(t-1) =0			*GUAR(t-1)* =0		
	All	*COLL(t)* =1	*COLL(t)* =0	All	*GUAR(t)* =1	*GUAR(t)* =0
GUAR(t)	0.536	0.742	0.447			
	(0.499)	(0.439)	(0.498)			
GUAR(t-1)	0.512	0.662	0.447			
	(0.500)	(0.474)	(0.498)			
COLL(t)				0.546	0.768	0.459
				(0.498)	(0.422)	(0.499)
COLL(t-1)				0.546	0.717	0.480
				(0.498)	(0.451)	(0.500)
GOVGUAR(t)	0.190	0.427	0.086	0.130	0.374	0.032
	(0.392)	(0.496)	(0.281)	(0.337)	(0.485)	(0.175)
GOVGUAR(t-1)	0.168	0.352	0.088	0.100	0.269	0.032
	(0.374)	(0.479)	(0.284)	(0.301)	(0.444)	(0.177)
ROA	0.022	0.018	0.024	0.020	0.016	0.022
	(0.050)	(0.053)	(0.049)	(0.048)	(0.047)	(0.049)
ICOVER	24.177	10.034	30.351	22.951	12.836	27.916
	(99.586)	(53.131)	(113.580)	(100.549)	(98.476)	(101.245)
CAP	0.321	0.308	0.326	0.337	0.330	0.339
	(0.241)	(0.228)	(0.247)	(0.237)	(0.236)	(0.237)
ROA_NG	0.205	0.272	0.176	0.199	0.241	0.181
	(0.404)	(0.446)	(0.381)	(0.400)	(0.428)	(0.385)
ICOVER_SM	0.271	0.380	0.223	0.277	0.382	0.226
	(0.445)	(0.487)	(0.417)	(0.448)	(0.486)	(0.419)
CAP_NG	0.034	0.042	0.031	0.018	0.026	0.015
	(0.182)	(0.202)	(0.173)	(0.134)	(0.159)	(0.121)
LnEMP	3.692	3.545	3.756	4.119	3.712	4.298
	(1.066)	(1.051)	(1.067)	(1.097)	(1.057)	(1.067)
LnLIAB	12.705	12.512	12.789	13.436	12.775	13.736
	(1.842)	(1.620)	(1.927)	(1.964)	(1.761)	(1.979)
LONG	0.398	0.469	0.366	0.434	0.504	0.402
	(0.356)	(0.324)	(0.365)	(0.329)	(0.332)	(0.323)
LAND	0.083	0.099	0.076	0.109	0.128	0.100
	(0.107)	(0.116)	(0.103)	(0.116)	(0.118)	(0.115)
CASH	0.190	0.199	0.186	0.160	0.198	0.144
	(0.149)	(0.143)	(0.151)	(0.137)	(0.143)	(0.131)
RATE	0.021	0.023	0.020	0.020	0.024	0.018
	(0.022)	(0.015)	(0.024)	(0.020)	(0.021)	(0.019)
OWNER	0.578	0.709	0.520	0.453	0.754	0.336
	(0.494)	(0.455)	(0.500)	(0.498)	(0.431)	(0.473)
BANKS	3.997	3.751	4.105	5.300	4.193	5.727
	(3.643)	(3.279)	(3.789)	(5.911)	(3.964)	(6.459)
LnDURATION	3.005	3.086	2.970	3.279	3.279	3.280
	(0.845)	(0.816)	(0.856)	(0.691)	(0.729)	(0.675)
Number of observations	701	213	488	928	308	620

Note: Standard deviations are in parentheses.