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Abstract

We study how government loan programs affect the growth of small businesses by examining a unique policy-based small business lending program in Japan. Combining the loan-level program data with a financial statement database, we find that small business borrowers increase employment and asset levels after receiving the loan and that these effects persist for several years. Differences in debt levels are persistent over time, cash holdings of loan recipients fall in the long run, and the effects on asset levels are larger in magnitude than those on employment. In addition, the effects are larger in magnitude for firms identified as financially constrained. These results suggest that the government loan program is successful in relaxing binding financial constraints for small businesses that participate in the program.

JEL Classification: D04, D25, G21, G31, L25.
Keywords: Policy-based finance, Small and medium-sized enterprises, Firm growth, Financial constraints.

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1 Introduction

Lending to small businesses is typically fraught with risk. Relative to larger businesses, small businesses fail at higher rates and tend to be more informationally opaque, leading to severe asymmetric information.[1] As a result, small businesses, even when they have profitable investment opportunities, often cannot borrow at reasonable rates and, due to binding financial constraints, cannot invest and grow.

A common solution to this problem is government support of lending to small businesses. This support typically takes one of two forms. In the first approach, the government guarantees a loan to a small business by a private lender. By repaying the loan in cases where the small business is unable to do so, the government bears the downside risk while allowing the private lender to profit in cases where the small business is able to repay the loan. The Small Business Administration 7(a) program in the United States and the Enterprise Finance Guarantee program in the United Kingdom are examples of this type of government support. Under the second approach, the government lends directly to small businesses, typically at low interest rates. The Small Business Managerial Improvement Loan (MIL) program in Japan, the focus of this paper, is an example of the second form of government support of small business lending.

While these programs are common worldwide, relatively little is known about their effects on small businesses.[2] In this paper, we study the effect of the second approach, focusing on the MIL program in Japan. To do so, we create a novel data set that combines data on participants in the MIL program with a national credit registry of all businesses in Japan that have outstanding credit or are actively seeking credit.

We use the rich balance sheet and income statement data from the credit registry database to match MIL loan recipients to other observationally similar small businesses that do not participate in the program, using a propensity score matching algorithm. Then, we estimate the effects of the program by comparing the short- and long-term operating performance of MIL loan recipients to that of nonrecipients.

We find significant effects on both employment and capital for loan recipients. In partic-

[1] For more information on the risks associated with lending to small businesses, see Berger and Udell (1995).
[2] A notable exception is Brown and Earle (2017).

ular, in the year of their loans, recipients increase employment by approximately 6% relative to nonrecipients. Moreover, the effect is persistent, as recipients continue to have approximately 5% higher employment three years after the loan. Notably, the effects on levels of capital are larger in magnitude; in the year of the loan, loan recipients increase capital by approximately 16% relative to nonrecipients. Again, the effect is persistent, with capital levels of recipients approximately 13% higher than that of nonrecipients.

The results are broadly consistent with the MIL program relaxing binding financial constraints for loan recipients. We find that the effects are significantly stronger for financially constrained firms, particularly for the effects on capital levels. We identify financially constrained firms in two ways. First, we identify a firm as financially constrained if its total assets are less than the median value for all loan recipients. Alternatively, we identify a firm as financial constrained if its if asset tangibility is below the median for loan recipients. Then, we re-run our main tests for constrained and unconstrained firms separately. Among financially constrained firms, we find that loan recipients increase capital levels by approximately 30 to 50% relative to nonrecipients, depending on the method of identifying constrained firms and the time since the loan. In contrast, among unconstrained firms, capital levels of loan recipients increase by no more than 9% relative to nonrecipients. The difference in the estimated effects on employment across constrained and unconstrained firms is less pronounced. Additionally, we find that debt levels of loan recipients increase by approximately 15% relative to nonrecipients in the year of the loan. This difference in debt levels remains persistent over the next several years, suggesting that nonrecipients are not raising debt from private sources.

A further test is motivated by the evidence that financially constrained firms hold more cash in order to finance investment opportunities when they arise.[3] If the MIL relaxes financial constraints for loan recipients, we would expect that the cash holdings of these firms would fall. This is precisely what we find; while the cash holdings of loan recipients increase relative to those of nonrecipients in the year of the loan, presumably because of the loan proceeds, recipients hold less cash relative to sales than nonrecipients in the years after the loan. In particular, in the year of the loan, loan recipients increase cash holding relative to

[3] See, for example, Opler et al. (1999), Almeida et al. (2004), and Denis and Sibilkov (2009).

sales by 3% when compared with nonrecipients. However, loan recipients' cash holdings are approximately 3% lower than those of nonrecipients in the year after the loan and 6% lower two years after the loan.

Finally, as noted above, the estimated effects of the program on capital levels are more than twice as those on employment. In addition, we find that the capital–labor ratios of loan recipients increase significantly more than those of nonrecipients. In the year of the loan, the capital–labor ratio of loan recipients increases by approximately 16% relative to nonrecipients and remains 14.5% higher three years after the loan. This result is consistent with Garmaise (2008), who shows that financially constrained firms have lower capital–labor ratios. Our evidence suggests that by relaxing binding financial constraints, the MIL program allows loan recipients to increase their capital–labor ratios toward their optimum.

Moreover, we document a positive correlation between the effect on employment and the effect on capital. In particular, by comparing the firm-level residuals of the employment and capital regressions, we find that the loan recipients with larger increases in employment following the loans have larger increases in capital levels as well. In other words, loan recipients increase capital–labor ratios toward their optimum, not by substituting capital for existing labor, but by expanding both inputs.

While these results are all consistent with the MIL program relaxing binding financial constraints, an alternative explanation for our set of results is that investment opportunities vary significantly for loan recipients and nonrecipients. Specifically, it may be the case that loan recipients receive a positive investment shock that improves their expected future operating performance and also leads them to participate in the program. While our propensity score matching approach allows us to compare loan recipients with nonrecipients with similar levels and changes in operating performance in the years before treatment, loan recipients could receive an unobserved shock just prior to participation in the loan program. To rule out this explanation, we exploit the eligibility rules of the program to estimate the program effects using a regression discontinuity approach. We find that the probability of MIL receipt discontinuously drops at the eligibility threshold, which is mirrored by a discontinuous decrease in the employment growth. These findings confirm that the MIL program helps small firms to grow faster and our results are robust to unobserved investment opportunities.

Our paper contributes to the literature analyzing the effects of government support of lending to small businesses. As such government support is a common practice, many studies on such programs have been conducted in various countries, including Cowling (2010) in the United Kingdom, Lelarge, Sraer and Thesmar (2010) and Bach (2014) in France, and Hancock and Wilcox (1998) and Craig, Jackson III and Thomson (2007) in the United States (US). As with our paper, Uesugi, Sakai and Yamashiro (2010) and Wilcox and Yasuda (2019) study government-supported loans in Japan. The former finds that the government loan guarantees are complements to nonguaranteed loans and result in an increase in nonguaranteed loans, whereas the latter finds negative effects on nonguaranteed loans and the ex post performance of loan recipients. Within the literature, the study most closely related to ours is Brown and Earle (2017), which analyzes the effects of loans through the US Small Business Administration on employment growth. Our paper builds on their analysis of the effects of government small business loan programs by exploiting the richer data available for Japanese firms. In particular, unlike Brown and Earle (2017), we have income sheet and balance sheet data, which allows us to estimate program effects on a wide variety of outcomes and therefore gives a fuller understanding of the impacts of these programs.

There is a broader literature on the role of credit provision on small business performance, at both the firm level and in the aggregate. Generally, analyses of firm-level data show sizable impacts of small business lending on firm survival and employment growth.[4] Analyses of aggregated data, on the other hand, find mixed results. For instance, whereas Chen, Hanson and Stein (2017) find that decreases in county-level small business lending following the financial crisis led to a decline in business expansion, employment, and wages, Greenstone, Mas and Nguyen (2020) find little effect of small business loan originations on employment at the county level.

More broadly, this paper contributes to the literature on firm growth under financial constraints. Firm growth and firm dynamics under financial constraints have been extensively studied both theoretically (e.g., Cooley and Quadrini, 2001; Clementi and Hopenhayn, 2006) and empirically (e.g., King and Levine, 1993; Beck, Demirgäç-Kunt and Maksimovic, 2005).

[4] See, for example, Cingano, Manaresi and Sette (2016).

Although most studies are based on aggregate country-level data, there has been recent progress on the empirical side using firm- and loan-level data, such as Banerjee and Duflo (2014) and Brown and Earle (2017), which find additional credit supply contributes to firm growth. Using a richer and more detailed set of variables, this paper contributes to the literature by unraveling the mechanism and channel by which the firms grow, by examining their capital, labor, sales, and debt levels together.

The remainder of the paper is organized as follows. Section 2 discusses the institutional background of small business lending in Japan. Section 3 discusses the data. Section 4 presents the identification strategy employed in this paper and Section 5 discusses our results. Section 6 discusses robustness results and Section 7 concludes.

2 Institutional Background

In this section, we first describe the small businesses in general and their financing situation in Japan. Then, we explain the government-backed small business lending program on which we focus.

2.1 Small Businesses and Their Financing Sources

As in many countries, Japan's small businesses account for a significant proportion of the economy and employment. In 2016, there were approximately 3.6 million companies in Japan, and 85% of them (3 million) were small businesses.[5] Those 3.6 million companies had 47 million employees and generated value-added of 256 trillion Japanese yen (JPY) in total; small businesses accounted for 22% of the total employment, with 10 million employees, and 14% or 36 trillion JPY of the value-added (The Small and Medium Enterprise Agency, 2019). According to the statistics of the Bank of Japan, the total outstanding value of loans and bills discounted by private banks and credit unions on March 31, 2019 was 580 trillion JPY, of which about 70% was for small and medium-sized enterprises (SMEs).[6]

[5] A small business is defined as a company with less than 21 employees (or less than six employees if the company is in the commerce or service sectors).

[6] The Bank of Japan uses a slightly different definition of SMEs to ours; Companies with capital of less than 300 million JPY. The outstanding loan amounts for SMEs include loans from credit unions (*shinkin* banks). See https://www.boj.or.jp/statistics/dl/loan/ldo/index.htm/.

For small companies, the main source of financing is borrowing from banks and credit unions. Table 1 shows the composition of liabilities for all firms in Japan by firm size, based on the *Hojin Kigyo Tokei* (Financial Statements Statistics of Corporations), where the numbers represent the percentage of each item of total liability for each firm size category on March 31, 2015. Borrowing accounts for the largest fraction of liabilities for the smallest category of companies. As company size increases, the fraction of borrowing to total liabilities decreases but borrowing accounts for a significant fraction for all company size categories. Bonds account for a very small fraction of liabilities, except for the largest companies, suggesting that issuing bonds does not substitute for borrowing for small businesses. Overall, Table 1 shows that small businesses rely heavily on borrowing and suggests that financial constraints on borrowing could have a huge impact on the financing and growth of small businesses.

Table 1: Financing Sources for Non-Financial Companies in Japan

This table reports the structure of liability by capital size category of firms, as of the end of March 2014, according to *Hojin Kigyo Tokei (Financial Statements Statistics of Corporations)* published by the Ministry of Finance (MOF) Japan. Based on the random sampling survey by capital size category, MOF estimates the liability structure for the business corporations excluding financial institutions and insurance companies in Japan. Here, “Networth” refers to the sum of capital, reserve, and profit. “Borrowing” is that from financial institutions, firms and/or individuals. “Other” refers to all the liability other than capital, bond and borrowing, including bills receivable, accounts receivable, and bills receivable discounted.

	less than 10M JPY	10M to 50M JPY	50M to 100M JPY	100M to 1B JPY	1B JPY or more	All
Net Worth	33.4%	34.8%	34.5%	26.9%	42.6%	39.0%
Bond	0.6%	0.9%	1.1%	0.5%	6.2%	3.6%
Borrowing	43.6%	36.8%	34.6%	33.7%	25.7%	31.2%
Other	22.4%	27.5%	29.8%	28.9%	25.5%	26.2%

2.2 Small Business Managerial Improvement Loan Program

The largest public financial institution in Japan, the Japan Finance Corporation (JFC), specializes in financing for small businesses. It was founded in 2008 when four public financial institutions specializing in small business finance were consolidated, and is a policy-based financial institution in the sense that it was founded by a special law, the Japan Finance Corporation Act.[7]

One of the main financing programs of the JFC is the focus of this paper, the Small Business MIL program (also known as "Marukei Loans"). MIL, which aims at improving the management of small businesses, has several unique features. First, there is a firm size restriction for applications. Only firms with fewer than 21 employees (or six employees for the commerce and service sectors) are eligible to apply. Second, for this program, the JFC collaborates with regional business associations of small firms, specifically the Chambers of Commerce and Industry and Societies of Commerce and Industry. A small firm applying for MIL needs to participate in a managerial improvement program provided by these regional business associations for at least six months, and the MIL application requires a recommendation letter by the advising association. Third, neither collateral nor credit insurance are required for MIL. Because these are often barriers to small companies accessing loans, MIL aims to contribute to relaxing the financial constraints of small firms by eliminating these requirements. Finally, there are restrictions on loan terms. The upper limit of each loan is 20 million JPY (approximately 185,000 USD) and the term of payment is seven years with a one-year deferment for a working capital loan, and 10 years with a two-year deferment for an investment capital loan. The interest rate is fixed at a low rate and is revised occasionally to reflect financial market conditions and government policy.

Table 2 summarizes the number of loan originations and the total loan amount, as well as showing average loan size, the interest rate of MIL loans, and the average interest rate of loans originated by credit unions. The average loan size has increased from around 4 million JPY to 6 million JPY, whereas the number of loans has stayed relatively constant. As a result, the total size of the MIL program has increased by about 50% since 2008. Regarding the interest rate, because the Bank of Japan has had a zero interest rate policy since the late 1990s, the loan rates of private financial institutions, including credit unions for small firms, have remained at a low level, and the loan rate of the MIL program has not been significantly lower that private market rates. However, considering that MIL loans require neither collateral nor

[7] According to the law, the scope of the JFC's activities are approved by the government. The annual report of the JFC states that its basic philosophy is "Following the national policy, provide flexible policy-based financing by utilizing a variety of financing programs and schemes to meet the needs of society, while complementing the activities of private financial institutions." (Japan Finance Corporation 2019)

Table 2: Descriptive Statistics of MIL Program

This table overviews the MIL loans from 2008 to 2018. "Number of Loans Newly Executed" and "Total Loan Amount" are obtained from *Gyomu Tokei (Business Statistics)* issued by Japan Finance Corporation (2019). "Average Loan Size" is calculated by dividing "Total Loan Amount" by "the number of loans newly executed" and both "Total Loan Amount" and "Average Loan Size" are at current price (million JPY). "Average Annual Interest Rate of MIL Loans" and "Average Annual Interest Rate of the Loans by Credit Cooperatives" are obtained from The Small and Medium Enterprise Agency (2018) and measured in percentage. These interested rates for 2017 and 2018 are not published yet.

Fiscal Year	Number of Loans Newly Executed	Total Loan Amount	Average Loan Size	Average Annual Interest Rate of MIL Loans (%)	Average Annual Interest Rate of Loans by Credit Cooperatives (%)
2008	45,948	185,625	4.04	2.00	1.856
2009	42,655	187,244	4.39	1.85	1.850
2010	37,654	147,819	3.93	1.95	1.586
2011	35,159	154,315	4.39	1.85	1.445
2012	40,047	172,228	4.30	1.65	1.459
2013	39,303	198,265	5.04	1.60	1.421
2014	40,083	223,734	5.58	1.35	1.332
2015	43,210	249,566	5.78	1.15	1.339
2016	43,421	257,103	5.92	1.16	1.390
2017	44,060	270,192	6.13	-	-
2018	44,176	279,147	6.32	-	-

credit insurance, the MIL program has been attractive for borrowers.

3 Data

3.1 Sources and Construction of the Data

To examine the effects of the MIL program, we use proprietary data provided by the JFC, which contain the list of loan recipients. In the list, we can observe 52,984 firms that have outstanding balances at the end of the fiscal year 2017, i.e., March 31, 2018. The unique feature of this data set is that it also records information on whether these firms have outstanding balances as at the end of fiscal years 2013, 2014, 2015, and 2016, which enables us to identify when these firms started receiving MIL program loans.[8] Although the list of loan recipients include some variables of interest, such as sales amounts, capital, and location of headquarters, we do not directly observe other variables of interest, such as tangible fixed assets, machinery and equipment, and past information for these firms. Thus, to obtain such

[8] In other words, we can observe whether these firms have outstanding balances as at March 31, 2014, 2015, 2016, and 2017, as well as March 31, 2018.

missing information and construct a control group (nonrecipients), we link the list of loan recipients to the data provided in the Credit Risk Database (CRD), as explained below.

The CRD is one of the most comprehensive financial databases for Japanese SMEs, as it collects financial statement information—balance sheet and income statements—for more than one million firms annually. The data are collected by the CRD Association, which compiles the data from its member organizations, including 51 local credit guarantee corporations, three public financial institutions, 98 private financial institutions, and 15 other institutions, such as credit rating companies. In Japan, local credit guarantee corporations that are public institutions support SMEs by serving as public guarantors, making it easier for them to borrow funds. Thus, any SMEs that have loans from public or private financial institutions must be recorded in this database by their guarantors, along with larger companies that have loans from private financial institutions.

We link the list of loan recipients to the CRD data via five key variables: Prefecture, Sales (in 2017), Japanese SIC, Accounting closing month, and Capital. As a result, we have 33,274 matched firms out of 52,984 firms listed in the data. There are several caveats to note. First, as five matching variables may not be sufficient to uniquely identify a particular firm, there are 2,955 firms with multiple matches in the CRD data, and we drop these firms from our sample. Second, the timing of treatment may not be perfectly identified, as the data do not contain the information on the month in which the firms start borrowing.

3.2 Descriptive Statistics

Table 3 shows the summary statistics of the firm-year level variables. Panels (A) and (B) show the statistics for all firms in the sample and firms receiving the MIL program loans, respectively. Throughout this paper, we refer to the firms receiving the MIL program loans as the treated group and the remaining firms as the control group. As described in the previous section, the timing of the treatment can vary across firms depending on the year in which they receive the MIL program loans. We have about 3.4 million observations for all samples and 76,000 observations for the treated group.

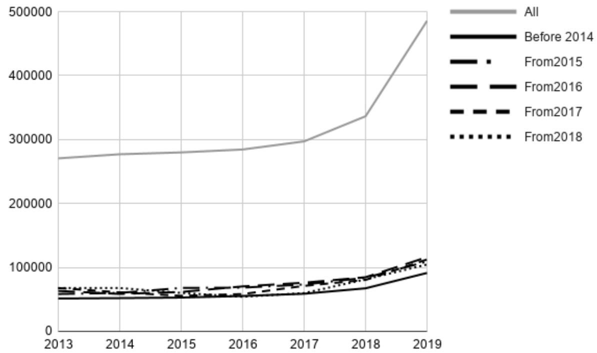
One notable difference between all samples and the treated group is that the latter is much smaller when measured by the number of employees or by the asset size, consistent

Table 3: Summary Statistics for All and Treated Firms

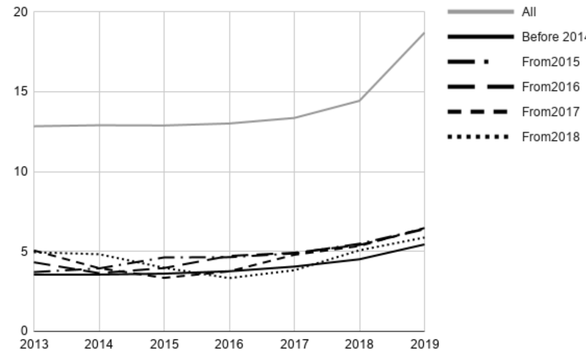
This table reports summary statistics for the following variables used in our study: Number of Employees, measured in number of people; Cash and Deposit, measured in 1,000JPY; Temporal Liquidity, defined as 'Cash and Deposit' divided by 'Sales Amount'; Short Term Loans, measured in 1,000JPY; Short and Long Term Loans, measured in 1,000JPY; Total Assets, measured in 1,000JPY; Tangible Fixed Assets, measured in 1,000JPY; Buildings and Structures, measured in 1,000JPY; and Machinery and Equipment, measured in 1,000JPY. Each column shows the number of observations, means, standard deviations, or 5, 25, 50, 75, and 95 percentiles for each variable.

	N	Mean	S.D.	5%	25%	50%	75%	95%
Panel (A): All Firms (MIL = 0 or 1)								
Number of Employees	3,401,456	14	25.6	0	2	5	14	60
Cash and Deposit	3,369,204	55,959	136,674	500	3,190	11,200	40,700	273,000
Temporal Liquidity	3,272,597	.172	.200	.013	.051	.109	.213	.548
Short Term Loans	3,401,211	30,134	90,396	0	0	2,500	18,600	150,000
Short/Long Term Loans	3,401,262	142,707	312,964	1,080	13,800	40,300	118,000	652,000
Sales Amount	3,401,226	360,534	808,923	9,860	39,800	102,000	288,000	1,630,000
Total Assets	3,401,212	321,921	771,228	5,670	24,700	73,600	242,000	1,520,000
Tangible Fixed Assets	3,401,304	112,147	286,014	0	2,200	14,200	79,100	574,000
Buildings & Structures	2,205,121	60,543	156,027	0	100	5,960	40,800	320,000
Machinery & Equipment	2,156,139	12,797	36,564	0	90	2,120	8,260	60,200
Panel (B): Treated (MIL = 1)								
Number of Employees	76,519	4.47	5.35	0	1	3	6	16
Cash and Deposit	75,761	12,031	18,514	400	1,900	5,320	13,600	48,300
Temporal Liquidity	73,761	.118	.110	.011	.040	.084	.159	.345
Short Term Loans	76,468	9,851	19,555	0	0	2,170	10,500	45,600
Short/Long Term Loans	76,466	48,572	61,584	3,140	13,000	28,400	58,200	168,000
Sales Amount	76,466	104,941	121,145	11,700	32,300	64,100	127,000	351,000
Total Assets	76,468	68,631	92,378	4,990	16,200	36,200	80,600	250,000
Tangible Fixed Assets	76,467	21,947	40,757	0	1,340	6,420	23,200	98,200
Buildings & Structures	27,599	12,268	26,868	0	60	2,370	11,200	58,400
Machinery & Equipment	27,575	5,939	10,659	0	300	2,080	6,470	25,800

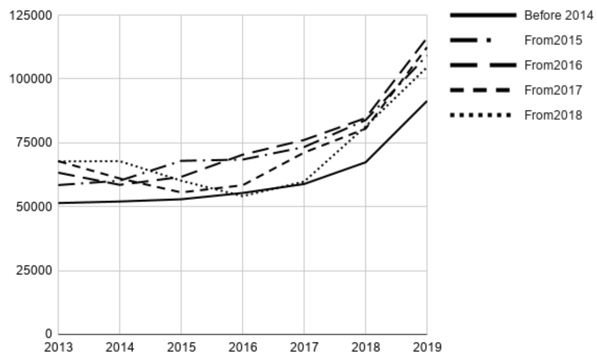
with the objective of the MIL program. Note that about one third of the full sample and two thirds of the treated group do not report "Buildings & Structures" and "Machinery & Equipment." As larger firms tend to report these variables, we have more missing values for the treated group.



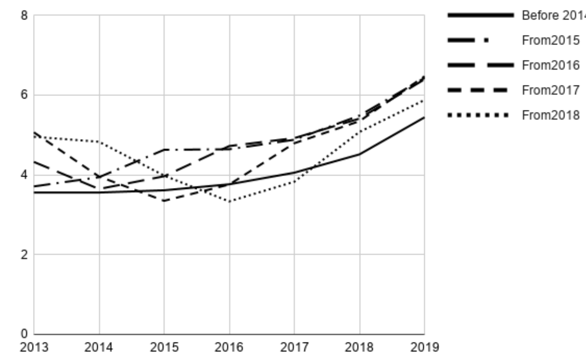
(a1) Average Total Assets over Time



(b1) Average Employment over Time



(a2) Average Total Assets of Treated Firms

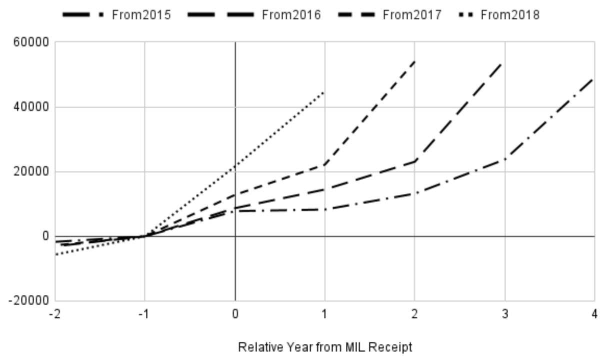


(b2) Average Employment of Treated Firms

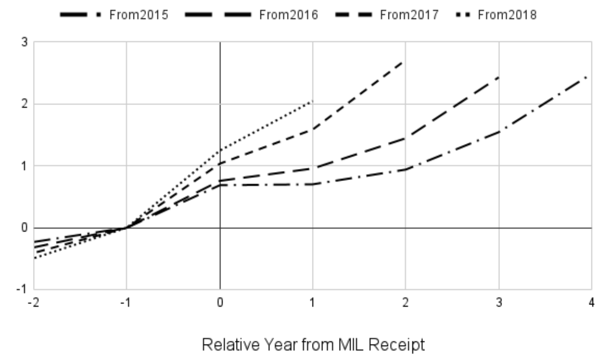
Figure 1: Averages of Total Assets and Employment for Control and Treated Groups over Year

Panels (a1) and (b1) plot the average total assets and average employment, respectively, in 2013–2019, separately for all firms, the firms that received MIL program loans before 2014, and the firms that received MIL program loans in 2015, 2016, 2017, and 2018. Panels (a2) and (b2) plot the average total assets and average employment, respectively, in 2013–2019, separately for the firms that received MIL program loans before 2014 and the firms that received MIL program loans in 2015, 2016, 2017, and 2018. The horizontal axis shows the year in all Panels. The vertical axis shows the total assets (measured in 1,000 JPY) in Panels (a1) and (a2), and the number of employees in Panels (b1) and (b2).

A natural way to examine the effect of the MIL program is to track the average of the variables of interest for the treated and control groups over time. For example, Figure 1 shows how the total assets, measured in 1,000s of JPY, and the number of employees evolve over time. Panels (a1) and (b1) of Figure 1 plot the average total assets and the average number of employees of the control group, firms treated before 2015, firms treated in 2015, firms treated in 2016, firms treated in 2017, and firms treated in 2018. From the figure, it is clear that the



(a) Average Total Assets of Treated Firms Normalized by the Year Before Treatment



(b) Average Employment of Treated Firms Normalized by the Year Before Treatment

Figure 2: Averages of Total Assets and Employment for Control and Treated Groups Normalized by the Year Before Treatment

Panels (a) and (b) plot the average total assets and average number of employees, respectively, relative to the year before the treatment, separately for the firms that received MIL program loans in 2015, 2016, 2017, and 2018. The horizontal axis shows the relative year from the receipt of the loan. The vertical axis shows the total assets (measured in 1,000 JPY) in Panel (a) and the number of employees in Panel (b).

treated group firms are very different from the control group firms in firm size. As a result, the effect of the MIL program is not particularly evident from the figure. For a valid comparison, it is essential to compare firms with similar characteristics. Thus, Panels (a2) and (b2) of Figure 1 plot the same variables as in Panels (a1) and (b1) but exclude the control group, which allows us to compare treated firms with other firms with similar characteristics. In these two panels, both the total assets and the number of employees increase in the year in which the firms received the treatment. For example, in Panel (b2), the firms that received MIL program loans in 2015 (denoted by the dash-dotted line) did indeed increase their employment in 2015; the firms that received the MIL program loans in 2016 (denoted by the dashed line) increased their employment in 2016, and so on. These observations suggest that the MIL has a positive effect on both variables. In addition, note that the control group firms and firms treated before 2015 follow a similar trend, which captures macroeconomic factors or year-specific factors that affect all firms.

To see the effect of the treatment more clearly, we shift the plots in Panels (a2) and (b2), normalizing the average in the year before the treatment to zero in Figure 2. Now, the horizontal axis in both Panels (a) and (b) shows the year relative to the treatment year and the vertical axis shows the average relative to the average of the year before the treatment. For all cohorts, both the total assets and the number of employees increase in the treatment year, and continue to increase over time. Before the treatment, the average is around zero in both pretreatment years, years –1 and –2, for all cohorts. At the same time, all cohorts exhibit small but increasing pre-trends. In addition, the year-specific growth rates are notably different. Overall, Figures 1 and 2 suggest that the MIL program may have a positive effect on firm growth, both in terms of the asset size and the number of employees. However, the figures indicate that controlling for firm characteristics, year-specific conditions, and preexisting trends and conditions is important in making any causal inference.

4 Empirical Strategy

The statistics in Section 3 suggest that controlling for firm characteristics is essential to evaluate the effect of the MIL program. Moreover, the MIL program is not randomly allocated, in the sense that only firms with credit needs would apply for the program. Therefore, controlling for the potential credit needs is essential. To this end, we adopt a difference-in-differences model with multiple time periods proposed by Callaway and Sant'Anna (2021), an extension of Abadie (2005), that considers a difference-in-differences model with the propensity scores as the weights for observations.

This estimation method has several advantages in our setting. First, it explicitly controls for firm characteristics. As presented in Section 3, there is significant heterogeneity between firms. The model proposed by Callaway and Sant'Anna (2021) controls for firm heterogeneity through the propensity score. Second, the model allows for selection due to firms' credit needs based on observables. Firms participate in the MIL program only if they need credit and face credit constraints. Including observables that capture firms' credit needs, such as the growth rate of sales, assets and/or employment, in the propensity score calculation allows us to control for selection due to firms' credit needs. This is essential because we expect that the standard "parallel trends assumption" to hold only after conditioning on firms' credit needs. Our estimation strategy allows for the possibility that the parallel trend assumption does not hold unconditionally but holds after controlling on observed covariates.

Third, it allows for heterogeneous treatment effects depending on firm characteristics. As the difference-in-differences model does not impose linearity, our estimation model allows for potential heterogeneous effects of the MIL program. Fourth, it allows for variation in treatment timing. As discussed in Goodman-Bacon (2021), when there is variation in treatment timing, estimation based on a standard difference-in-differences model results in a weighted average of different treatment effects between different cohorts. The model proposed by Callaway and Sant'Anna (2021) gives us estimates that have an intuitive interpretation. A natural alternative to our estimation strategy is to use a standard difference-in-differences model. We discuss the results under the alternative models in detail in Appendix A. The results are qualitatively similar, but we cannot eliminate statistically significant pre-trends under the standard difference-in-differences model.[9]

Furthermore, we are not only interested in the causal effect of the MIL program, but also in the evolution of its effects over time. For example, if all firms have access to alternative financing sources other than the MIL program, then we would expect the effect to vanish over time as the firms' credit needs would be satisfied eventually. On the other hand, if firms are financially constrained and cannot find alternative financing sources easily, we would expect the effect to be persistent. To examine how the effect evolves over time, we adopt an event study design framework, estimating a series of treatment effects around the treatment year. This framework is a very common approach when evaluating the treatment effect, e.g., Deshpande and Li (2019), and allows us to see whether any pre-trend exists.

Formally, the average treatment effect on the treated (ATT) from τ years from the treatment for the firms who receive MIL program loans in year t is identified as:

$$\mathrm{ATT}(t,\tau) = E\left[\left(\frac{G_{it}}{E[G_{it}]} - \frac{\frac{p_t(X_{i,t-1})C_{it}}{1-p_t(X_{i,t-1})}}{E\left[\frac{p_t(X_{i,t-1})C_{it}}{1-p_t(X_{i,t-1})}\right]}\right)(y_{i,t+\tau} - y_{i,t-1})\right], \tag{1}$$

where G_{it} is one if firm i receives a MIL program loan in year t and zero otherwise, C_{it} is one if firm i never receives a MIL program loan and zero otherwise, $p_t(X_{i,t-1})$ is the probability

[9] One important conceptual difference between the method proposed by Callaway and Sant'Anna (2021) and a standard two-way fixed-effect model is that the former always compares treated firms to never-treated firms, whereas the latter estimates the weighted average of the effect among different combinations of differently treated cohort and control groups. See Goodman-Bacon (2021) for a detailed discussion.

that firm i with covariates $X_{i,t-1}$ receives a MIL program loan in year t conditional on $G_{it} = 1$ or $C_{it} = 1$, and y_{iu} is the outcome variable of firm i in year u. We define ATT τ years from the treatment as the weighted average of $\text{ATT}(t, \tau)$ as:

$$\text{ATT}(\tau) = \sum_t w_t \text{ATT}(t, \tau),$$

where w_t denotes the weight, which is the number of firms treated in year t divided by the total number of treated firms.

We estimate $\text{ATT}(t, \tau)$ by replacing the expectation with the empirical average, and $p_t(X_{i,t-1})$, the propensity score, by estimating a logit model. For $X_{i,t-1}$, we use a dummy variable indicating whether the total number of employees is less than 21, years of operation, region and industry fixed effects, and one-year to three-year lagged values, values in years $t-1$, $t-2$, and $t-3$, of the logarithm of sales, the number of employees, cash deposits, total assets, tangible assets, and short- and long-term loans. The lagged values of the variables are meant to capture firms' credit needs. For example, by including one-year and two-year lagged values of sales, we can control for the level of sales and the growth rate of sales in the year before the treatment. We present the estimation results for the propensity score in Appendix B. The standard error is estimated by bootstrap with 200 replications.

Let us note that our estimation strategy requires that randomness in the application to the MIL program exists even after controlling for the potential credit needs. As described in Section 2, the application to the MIL program requires participation in a managerial improvement program and a recommendation letter by regional business associations. Firms that have better relationships with their local business associations have easier access to such resources, which creates one source of randomness in the application. We discuss an alternative identification and estimation strategy in Section 6.

5 Results

In this section, we discuss our main results concerning the effects of the loan program on firm outcomes. First, we examine the effect on employment and capital levels, and then we examine the mechanisms driving our results.

Table 4: ATT Estimates of the MIL Effect on Employment and Assets

This table reports ATT estimates of $t-3$ to $t+4$ years from the treatment where the dependent variables are $\log$(Number of Employees), $\log$(Tangible Fixed Assets), and $\log$(Building & Structure), listed in each column. Propensity scores used in the estimation of ATT are obtained by a series of logit regressions by year, regressing the indicator variable of MIL loan receipt on a dummy variable indicating whether the total number of employees is less than 21, years of operation, one-year to thee-year lagged values of $\log$(Sales), $\log$(Employees), $\log$(Cash & Deposit), $\log$(Total Assets), $\log$(Short & Long Term Loans), and $\log$(Tangible Fixed Assets), and region- and industry-fixed effects as covariates, and the results are given in Table B1. With the estimated propensity scores, for each dependent variable, we then estimate ATT for each cohort, depending on the year of MIL receipt, and each year from the treatment, $t-3,\ldots,t+4$, and take the weighted average over the cohorts to normalize the effects by the years from loan receipt. Standard errors in parentheses are estimated by bootstrap with 200 replications and significance levels are denoted by <0.1 (*), <0.05 (**), and <0.01 (***).

Years since Loan Receipt	log(Num of Employees)	log(Tangible Fixed Assets)	log(Building & Structure)	log(Machinery & Equipment)
-3	-0.004	0.002	0.016	-0.073***
	(0.008)	(0.019)	(0.017)	(0.022)
-2	0.007	0.016	0.021	0.014
	(0.006)	(0.014)	(0.013)	(0.017)
0	0.058***	0.158***	0.042***	0.109***
	(0.006)	(0.016)	(0.014)	(0.018)
1	0.064***	0.157***	0.036*	0.134***
	(0.007)	(0.02)	(0.019)	(0.024)
2	0.052***	0.146***	0.065**	0.141***
	(0.01)	(0.029)	(0.027)	(0.03)
3	0.05***	0.130***	0.065	0.162***
	(0.015)	(0.036)	(0.041)	(0.046)
4	0.065**	0.086	-0.046	0.113
	(0.028)	(0.067)	(0.064)	(0.08)
N	2,921,666	2,921,666	2,033,082	1,982,589

5.1 Effects on Employment and Capital

To understand how firms use the loan proceeds, in Table 4, we present the ATT for employment and measures of tangible assets, listed in each column. In the table, each row shows the ATT for years since loan receipt, ranging from year $t-3$ to $t+4$, relative to the year before the treatment.

First, in column 1, we find that in each of the two years prior to the loan, there is no significant difference in employment levels between treated and control firms. Moreover, as shown in Figure 3, the trend in the estimate is flat in the years prior to the loan. However, in the year of the loan receipt, employment at treated firms increases significantly relative to employment at control firms. The estimate implies that the increase in employment is

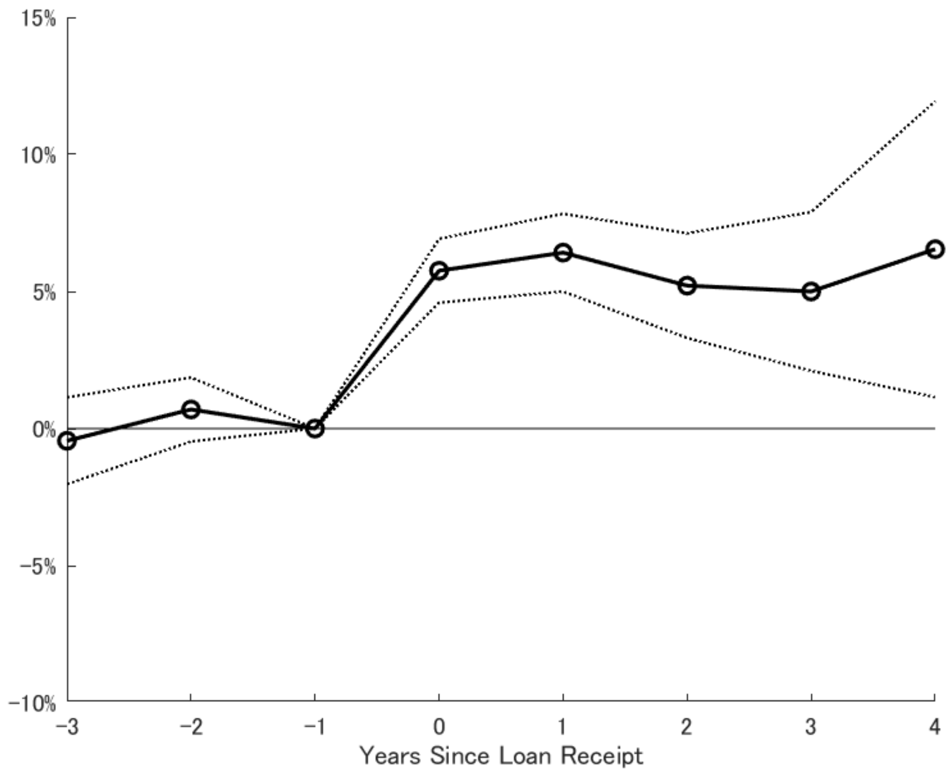


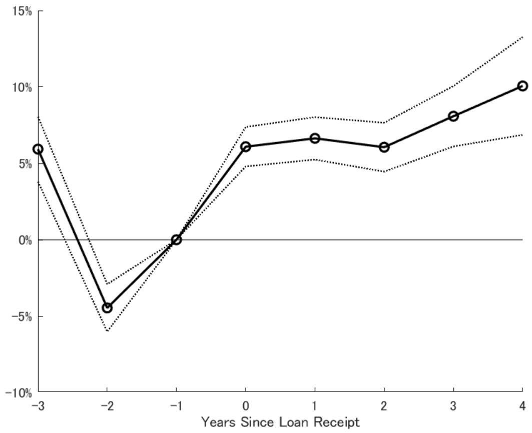
Figure 3: ATT Estimates of the MIL Program's Effect on Employment over Time

This figure shows ATT estimates of $t-3$ to $t+4$ years from the treatment where the dependent variable is a logarithm of firms' employment. The dotted lines are the bounds of the 95% confidence interval, based on standard errors obtained by bootstrap with 200 replications.

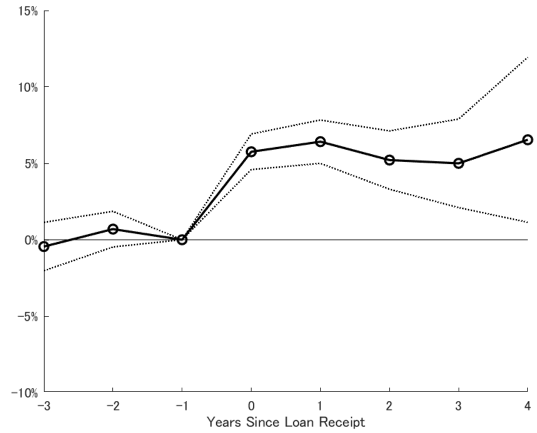
approximately 5.8%. The estimated effect for the years following the loan remains positive, significant, and similar in magnitude to the initial effect. Thus, it appears that firms use part of the loan proceeds to immediately increase employment and then maintain this higher level of employment for several years.

Note that these results confirm that our empirical strategy has an advantage over this alternative approach. To make a comparison between the approaches, we demonstrate using two panels in Figure 4: Panel (b) is the same figure as in Figure 3, whereas Panel (a) depicts the same effects obtained by a standard difference-in-differences model, regressing the same outcome variable on the variables used in the propensity score calculation, implying that we use exactly the same set of information.[10] Although our approach does not show

[10]For more detailed explanations, see Appendix A.



(a) DID with Covariates



(b) DID with Propensity Score Matching

Figure 4: Comparison of the MIL Program's Effects on Employment for Two Different Approaches

Both panels show ATT estimates of $t-3$ to $t+4$ years from the treatment where the dependent variable is a logarithm of the number of employees. In Panel (a), we use a standard difference-in-differences estimator by regressing a logarithm of the number of employees on the indicator variables of τ ($\tau = -3, -2, \ldots, 4$) years after treatment, firm and year fixed effects, and the same variables that are used for calculation of propensity scores as control variables, including a dummy variable indicating whether the total number of employees is less than 21, years of operation, one-year to thee-year lagged values of $\log$(Sales), $\log$(Employees), $\log$(Cash & Deposit), $\log$(Total Assets), $\log$(Short- & Long-Term Loans), $\log$(Tangible Fixed Assets), and region and industry fixed effects. Panel (b) is the same as Figure 3. Panels (a) and (b) plot the average total assets and average number of employees. The dotted lines are the bounds of the 95% confidence interval, based on standard errors, clustered at the firm level in Panel (a) or obtained by bootstrap with 200 replications in Panel (b).

any pre-trend in the years prior to the loan, the estimates from the alternative approach in Panel (a) exhibit a statistically and economically significant pre-trend, suggesting that the treated firms have certain symptoms or characteristics before receiving MIL program loans and that the estimates could suffer from this selection issue. This observation enables us to conclude that our approach successfully controls this selection issue of participation in the MIL program through the propensity scores.

Returning to Table 4, the estimates for the effect on tangible fixed assets, demonstrated in column 2, is similar to the results we found for employment. Again, the estimates for the years before the loan receipt are small and insignificant and, as shown in Figure 5, show no meaningful pre-trend. However, in the year of the loan receipt, tangible fixed assets increase at treated firms relative to control firms; the estimate is positive and statistically significant, and implies an increase of about 15.8%. Unlike the effect on employment, however, the esti-

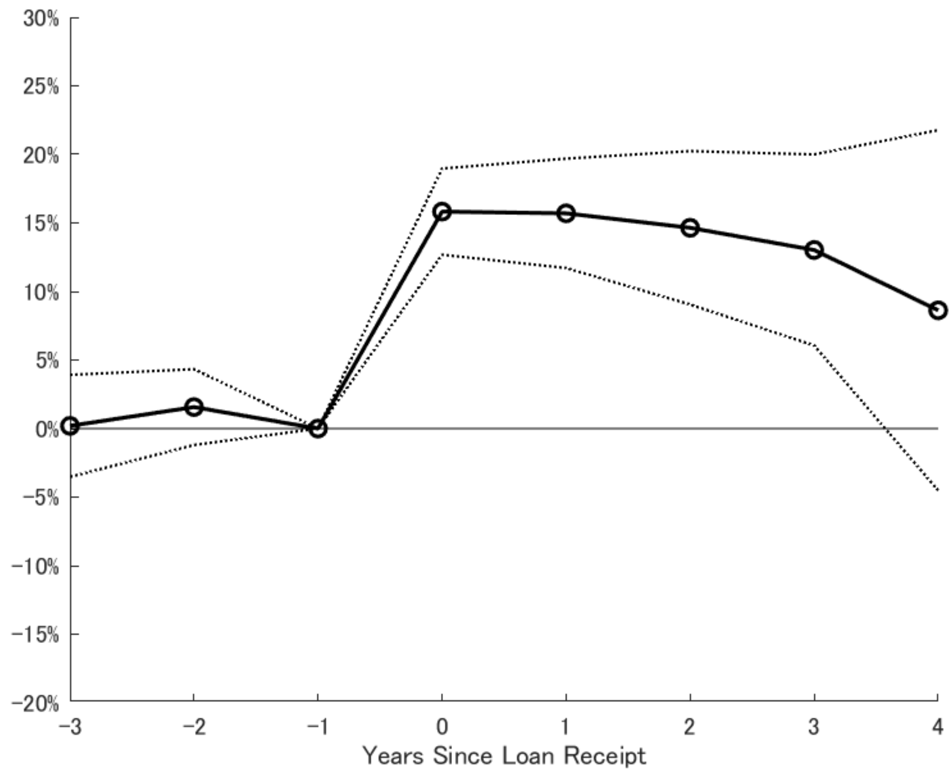


Figure 5: ATT Estimates of the MIL Program's Effect on Tangible Fixed Assets Over Year

This figure shows ATT estimates of $t-3$ to $t+4$ years from the treatment where the dependent variable is a logarithm of firms' tangible fixed assets. The dotted lines are the bounds of the 95% confidence interval, based on standard errors obtained by bootstrap with 200 replications.

mates show that the effect on tangible fixed assets attenuates somewhat over time. Treated firms have approximately 13% higher levels of tangible fixed assets three years after the loan and 8.6% higher levels four years after the loan. While the latter estimate is not statistically significant, this is likely to be the result of low power, as there are relatively few observations four years after the loan.

Columns 3 and 4 of Table 4 separate tangible fixed assets into buildings and structures and machinery and equipment. Treated firms increase both types of tangible fixed assets in the year of the loan, although the effect on machinery and equipment is much larger in magnitude. Buildings and structures increase by approximately 4.2% at treated firms relative to control firms in the year of the loan, while machinery and equipment increase by approximately 10.9%. In the years following the loan, there is a slight upward trend in the estimates;

two years after the loan, the value of buildings and structures at treated firms are 6.5% higher than at control firms, while the value of machinery and equipment is approximately 14.1% higher. In the third year after the loan, the estimated effect on buildings and structures is still 6.5%, although not significant, while the estimate for machinery and equipment is 16.2%, or almost 50% higher than the initial effect.

5.2 The Role of Financial Constraints

Our estimates suggest that the firms use the proceeds to increase employment as well as their tangible assets and that these increases seem to be fairly long lasting. Next, we study a potential mechanism, namely, binding financial constraints at treated firms, behind these large and persistent effects.

To do so, we re-estimate the effects on employment and tangible assets for financially constrained and unconstrained firms separately. We identify financially constrained firms based on two separate measures in the year prior to the loan; the first measure is total assets while the second measure is asset tangibility, defined as the ratio of tangible fixed assets to total assets. Then, we classify firms as financially constrained if these measures are below their respective median values among treated firms. The results are presented in Table 5, where Panel (A) uses total assets to measure financial constraints and Panel (B) uses asset tangibility to identify financially constrained firms.

As shown in columns 1 and 2 of Panel (A), the effects on employment are much stronger for constrained firms than for unconstrained firms. Among constrained firms, employment at treated firms increases in the year of the loan by approximately 6.9% relative to the control firms. For constrained firms, employment is only 3.5% higher at treated firms. Moreover, while the effect remains positive and generally significant for constrained firms, the estimate for the unconstrained firm sample is close to zero and insignificant for years $t+1$ through $t+4$. Thus, it appears that the loan allows financially constrained firms to permanently increase employment, whereas there is no long-term effect on employment at unconstrained firms.

Similarly, the treatment effect on tangible assets is significantly larger for constrained firms, as shown in columns 3 and 4 of Panel (A). While both constrained and unconstrained firms experience a significant increase in tangible assets in year t, the magnitude of the effect is much larger for constrained firms; whereas the estimates imply that unconstrained firms experience an increase of approximately 8.7%, tangible assets at constrained firms increase by approximately 58.4%. Moreover, while the effect at constrained firms is fairly persistent—in year $t + 3$, treated firms have approximately 54.9% higher levels of tangible assets—the effect at unconstrained firms declines over time and, in year $t + 3$, is approximately half of the initial effect and no longer statistically significant.

Table 5: ATT Estimates of the MIL Effect on Employment and Assets by Two Financial Constraints Measures

This table reports ATT estimates of $t-3$ to $t+4$ years from the treatment and the dependent variables are $\log$(Employment) and $\log$(Tangible Fixed Assets), listed in each column, by two measures of financial constraint, Totals Assets and Asset Tangibility. The firms are divided into two groups, financially constrained or unconstrained, whether the total assets or asset tangibility, defined as the ratio of tangible fixed asset to total assets, are below the median values among treated firms. Propensity scores used in the estimation of ATT are obtained by a series of logit regressions by year, regressing the indicator variable of MIL loan receipt on a dummy variable indicating whether the total number of employees is less than 21, years of operation, one-year to thee-year lagged values of $\log$(Sales), $\log$(Employees), $\log$(Cash & Deposit), $\log$(Total Assets), $\log$(Short & Long Term Loans), and $\log$(Tangible Fixed Assets), and region- and industry-fixed effects as covariates, and the results are given in Table B1. With the estimated propensity scores, for each dependent variable, we then estimate ATT for each cohort, depending on the year of MIL receipt, and each year from the treatment, $t-3,\ldots,t+4$, and take the weighted average over the cohorts to normalize the effects by the years from loan receipt. Standard errors in parentheses are estimated by bootstrap with 200 replications and significance levels are denoted by <0.1 (*), <0.05 (**), and <0.01 (***).

Panel (A): Total Assets as a Measure of Financial Constraints				
Years Since	log(Employment)		log(Tangible Fixed Assets)	
Loan Receipt	Constrained	Unconstrained	Constrained	Unconstrained
-3	0.048**	-0.001	0.13	0.003
	(0.021)	(0.01)	(0.089)	(0.022)
-2	-0.021	0.002	-0.052	0.004
	(0.014)	(0.007)	(0.067)	(0.02)
0	0.069***	0.035***	0.584***	0.087***
	(0.022)	(0.009)	(0.084)	(0.015)
1	0.108***	0.015	0.606***	0.088***
	(0.027)	(0.01)	(0.111)	(0.022)
2	0.053*	-0.007	0.57***	0.059**
	(0.032)	(0.012)	(0.16)	(0.029)
3	0.056	-0.006	0.549**	0.048
	(0.04)	(0.02)	(0.21)	(0.042)
4	0.183*	0.006	0.344	0.029
	(0.096)	(0.034)	(0.367)	(0.062)
N	719,339	2,188,316	719,339	2,188,316
Panel (B): Asset Tangibility as a Measure of Financial Constraints				
Years Since	log(Employment)		log(Tangible Fixed Assets)	
Loan Receipt	Constrained	Unconstrained	Constrained	Unconstrained
-3	0.005	0.002	0.063	-0.051**
	(0.013)	(0.016)	(0.054)	(0.026)
-2	-0.007	-0.008	0.011	-0.035*
	(0.009)	(0.013)	(0.043)	(0.019)
0	0.034***	0.049***	0.455***	-0.003
	(0.01)	(0.01)	(0.043)	(0.015)
1	0.009	0.051***	0.426***	0.000
	(0.015)	(0.013)	(0.066)	(0.016)
2	0.01	-0.001	0.347***	0.014
	(0.02)	(0.017)	(0.074)	(0.026)
3	-0.014	0.017	0.281**	0.033
	(0.03)	(0.024)	(0.116)	(0.031)
4	0.04	0.018	0.182	0.035
	(0.04)	(0.044)	(0.179)	(0.047)
N	1,291,682	1,629,757	1,291,682	1,629,757

When asset tangibility is used to identify financially constrained firms, as shown in Panel (B) of Table 5, we no longer find that the effects on employment are stronger for financially constrained firms. Rather, columns 1 and 2 show that the initial effect is larger in magnitude for unconstrained firms; whereas treated constrained firms increase employment by 3.4% in year t, treated unconstrained firms increase employment by approximately 4.9%. However, we find no long-term effects on employment for either population, as the estimates in years $t+2$ and $t+3$ are no longer statistically significant.

However, as shown in columns 3 and 4 of Panel (B), there are large, permanent effects on tangible assets for financially constrained firms and no effects for financially unconstrained firms. In particular, among constrained firms, treated firms increase tangible assets by 45.5% in year t relative to control firms. This estimate remains large and significant in later years, with the estimate for year $t+3$ implying that treated firms have approximately 28.1% higher levels of tangible assets than control firms. Among unconstrained firms, however, the estimates across all years are close to zero and statistically insignificant.

Taken together, the results of Table 5 suggest that the government loans play an important role in relaxing financial constraints among small firms. While there is some weak evidence that relaxing this constraint allows firms to increase employment, the evidence is much stronger in relation to the effect on tangible assets. Regardless of how financially constrained firms are identified, the treatment effect on tangible asset levels is significantly larger for constrained firms. These results suggest that not only are financial constraints binding for the treated firms, the constraints are particularly relevant for tangible assets.

If the MIL loan program does relax binding financial constraints, the literature on the effects of financial constraints suggests several additional tests. First, if financial constraints are binding and are relaxed by the government loans, control firms should be unable to borrow from private lenders. As a result, treated firms should have persistently higher levels of debt following loan receipt. To confirm this, we look at the patterns in long-term debt over time in Column 1 of Table 6.

In the years before the loan, the estimate is statistically significant at the 10% level, but small in magnitude. In the year of the loan, the estimate is large, implying a 15% increase in outstanding debt, and highly significant, consistent with the borrowing firms receiving the

Table 6: ATT Estimates of the MIL Effect on Cash, Debt, and Capital-Labor Ratio

This table reports ATT estimates of $t-3$ to $t+4$ years from the treatment where the dependent variables are $\log$(Cash & Deposit), $\log$(Temporal Liquidity), $\log$(Short & Long Term Loans), and $\log$(Tangible Fixed Assets/Number of Employees) (hereinafter $\log$(K over L)), listed in each column. Propensity scores used in the estimation of ATT are obtained by a series of logit regressions by year, regressing the indicator variable of MIL loan receipt on a dummy variable indicating whether the total number of employees is less than 21, years of operation, one-year to thee-year lagged values of $\log$(Sales), $\log$(Employees), $\log$(Cash & Deposit), $\log$(Total Assets), $\log$(Short & Long Term Loans), and $\log$(Tangible Fixed Assets), and region- and industry-fixed effects as covariates, and the results are given in Table B1. With the estimated propensity scores, for each dependent variable, we then estimate ATT for each cohort, depending on the year of MIL receipt, and each year from the treatment, $t-3,\ldots,t+4$, and take the weighted average over the cohorts to normalize the effects by the years from loan receipt. Standard errors in parentheses are estimated by bootstrap with 200 replications and significance levels are denoted by <0.1 (*), <0.05 (**), and <0.01 (***).

Years Since Loan Receipt	log(Cash & Deposit)	log(Temporal Liquidity)	log(Short & Long Term Loans)	log(K over L)
-3	0.009	0.012	0.021*	- 0.005
	(0.009)	(0.009)	(0.012)	(0.030)
-2	0.005	0.009	0.019*	-0.008
	(0.008)	(0.008)	(0.01)	(0.022)
0	0.036***	0.028***	0.150***	0.165***
	(0.009)	(0.009)	(0.008)	(0.022)
1	-0.007	-0.028***	0.162***	0.166***
	(0.010)	(0.01)	(0.01)	(0.031)
2	-0.032**	-0.061***	0.150***	0.168***
	(0.013)	(0.013)	(0.013)	(0.042)
3	0.003	-0.029	0.172***	0.145***
	(0.019)	(0.018)	(0.016)	(0.053)
4	-0.001	-0.033	0.171***	0.068
	(0.035)	(0.033)	(0.025)	(0.077)
N	2,921,666	2,917,819	2,921,666	2,921,666

government loans while the control firms did not receive loans from any source. In the three years following the loans, outstanding debt for the treated firms remains significantly higher relative to those of the control firms. After three years, treated firms have approximately 17.2% more outstanding debt than control firms. In other words, it appears that, due to the government loans, debt at the treated firms increases substantially but, in subsequent years, neither group of firms raises significantly different levels of debt.

In addition to the patterns in debt levels, the relaxation of financial constraints has implications for the cash holdings of treated firms. In particular, if firms are truly financially constrained and unable to raise external financing, they must have sufficient cash holdings to finance investment opportunities as they arise. Consistent with this intuition, previous studies, such as Opler et al. (1999), have found that financially constrained firms have greater cash holdings than do unconstrained firms. Therefore, if the government loan program does relax financial constraints among treated firms, we would expect that their cash holdings would decline over time.

Therefore, we next estimate ATT using logarithms of cash & deposits and temporal liquidity (defined as cash& deposits divided by sales) as dependent variables. The estimation results are presented in Columns 2 and 3 of Table 6. First, as we would naturally expect, both cash & deposits and temporal liquidity increase in the year of loan receipt, as treated firms use some, but not all, of the loan proceeds. For both measures, the estimates imply that cash holdings increase by approximately 3% relative to control firms. In the medium run, however, compared with the control firms, the treated firms have significantly lower cash holdings; two years after the loan receipt, total cash & deposits are about 3% lower and temporal liquidity is about 6% lower. In the longer term, while the cash & deposit holdings of treated firms converge with those of control firms, we do continue to find large but insignificant differences in temporal liquidity. Therefore, treated firms do appear to hold less cash than control firms, consistent with a lessened need to hoard cash to finance subsequent investment.

Additionally, Garmaise (2008) shows that financially constrained firms have lower capital–labor ratios than do unconstrained firms. At constrained firms, informed employees are relatively less expensive than capital, yielding lower capital–labor ratios. If financially constrained firms do have suboptimal capital–labor ratios and the government loan program does relax financial constraints, we would expect capital–labor ratios to rise at treated firms following loan receipt. As shown in Column 4 of Table 6, we find that this is the case. In the year of the loan, the capital–labor ratio of loan recipients increases by approximately 16% relative to nonrecipients.[11] This effect is persistent and capital–labor ratios at treated firms remain 14.5% higher relative to control firms three years after the loan. Therefore, it does appear that loan receipt allows treated firms to adjust their capital–labor ratios towards their optimum.

Interestingly, we find that this capital–labor adjustment occurs not through strict substitution of capital for labor but through expansion of both factors at different ratios. To un-

[11] Throughout this paper, we measure capital–labor ratios by dividing tangible assets by the number of employees.

derstand the process through which firms adjust, we examine the correlation between the residuals from the employment and capital regressions. If firms increase only one input, or if they substitute capital for labor, firms that increase capital would not increase labor or even decrease labor relative to ATT, which creates a negative correlation between the capital and labor residuals. On the other hand, if firms scale up and increase capital and labor simultaneously, firms increasing capital more would increase labor more relative to ATT, which creates a positive correlation.

Formally, we first define the residual by:

$$\text{Res}_{it}^{y} = y_{i,t} - y_{i,t-1} - \text{ATT}^{y}(t,0) \times I\{i \text{ treated in year } t\},$$

where $\text{ATT}^{y}(t,0)$ is ATT for variable y, and $I\{\}$ is an indicator function. Then, we regress Res_{it}^{Asset} on $\text{Res}_{it}^{Employee}$ by estimating the following equation:

$$\text{Res}_{it}^{Asset} = \beta^{res}\text{Res}_{it}^{Employee} + FE_t + FE_{ind} + \text{Control}_{it} + e_{it},$$

where FE_t is a year fixed effect, FE_{ind} is an industry fixed effect, Control_{it} is other control variables, and e_{it} is an error term. Other control variables include a dummy variable indicating whether the total number of employees is less than 21, years of operation, one-year lagged values of $\log$(Sales), $\log$(Employees), $\log$(Cash & Deposit), $\log$(Total Assets), $\log$(Short- & Long-Term Loans), $\log$(Tangible Fixed Assets), and region fixed effects.

Table 7 summarizes the estimation results. The first three columns show the estimation results using only treated firms in the treatment years, whereas the last three columns show the estimation results using all firms, and observations from all years with β^{res} estimated separately for treated and control. firms. First, when we look at the first three columns, the results show that the residual from the employee regression is positively correlated with the residual from the asset regression, and the correlation is statistically significant for tangible fixed asset and building and structure. The positive correlation implies that firms increase labor and capital at the same time and the loan enables firms to expand rather than creates capital–labor substitution.

Table 7: Residual Regression

This table reports the OLS estimation results using residuals from the estimated ATT. For each variable of interest, we define the residual as $\text{Res}_{it}^{y} = y_{i,t} - y_{i,t-1} - \text{ATT}^{y}(t,0) \times I\{i \text{ treated in year } t\}$, where y_{it} is either the $\log$(Tangible Fixed Assets) or $\log$(Number of Employees). Then we estimate the following relationship using OLS; $\text{Res}_{it}^{Asset} = \beta^{res}\text{Res}_{it}^{Employee} + FE_t + FE_{ind} + \text{Control}_{it} + e_{it}$, where FE_t is a year fixed effect, FE_{ind} is an industry fixed effect, Control_{it} is other control variables, and e_{it} is an i.i.d. error term. Other controls include a dummy variable indicating whether the total number of employees is less than 21, years of operation, one-year lagged values of $\log$(Sales), $\log$(Employees), $\log$(Cash & Deposit), $\log$(Total Assets), $\log$(Short & Long Term Loans), and $\log$(Tangible Fixed Assets), and region-fixed effects. The first three columns show results from observation from treated firms in the treated years, and the next three colums show results from all observations. Standard errors in parentheses and significance levels are denoted by <0.1 (*), <0.05 (**), and <0.01 (***).

	Treated Firms Only			All Firms		
	log(Tangible Fixed Assets)	log(Building & Structure)	log(Machinery & Equipment)	log(Tangible Fixed Assets)	log(Building & Structure)	log(Machinery & Equipment)
Employment Residual (Treated)	0.205*** (0.038)	0.272*** (0.075)	0.134 (0.102)	0.171*** (0.022)	0.261*** (0.066)	0.091 (0.088)
Employment Residual (Control)	- -	- -	- -	0.126*** (0.002)	0.179*** (0.003)	0.145*** (0.004)
Other Controls	X	X	X	X	X	X
N	7,760	3,260	3,240	2,683,359	1,887,834	1,844,798
Adj. R^2	0.213	0.030	0.037	0.087	0.020	0.015

6 Robustness

The endogeneity of the MIL program treatment is a natural concern. The results presented in Section 5 may be driven by some unobserved factors correlated with participation in the MIL program. For example, while we match treated firms to control firms on the basis of multiple years of data, treated firms may have received an unobserved investment shock that accounts for the results discussed above. In this section, we adopt the regression discontinuity design (RDD) to address this concern and to show the robustness of our results. As explained in Section 2, eligibility for the MIL program is based on the number of employees: i.e., firms with more than 20 employees (or more than five employees in the commerce or service sectors) are not eligible for the MIL program loans. We utilize this institutional feature and examine the growth rate of employment and tangible assets around the thresholds. Here, there are two data issues. First, we cannot observe the number of employees precisely at the time that firms apply for the MIL program. The data show the number of employees in the financial statement that firms submit to financial institutions, which may not exactly correspond to the definition of the number of employees used in the MIL application. In addition, the timing of the measurement may be different. The data are provided as of the firm's closing month, whereas they can apply to the MIL program at any time throughout the year. In fact, in the data, we observe firms with more than 20 employees receiving MIL program loans, which suggests that measurement errors exist for the reasons above. Second, the data do not identify whether a firm is subject to the five-employee threshold or the 20-employee threshold. The data contain the industry code but, in almost all industry categories, we observe firms with five or fewer employees, between 6 to 20 employees, and more than 20 employees receiving MIL program loans. In practice, eligibility for the MIL program is based on finer industry definitions, which we do not observe in our data. Nonetheless, we believe that RDD could be a useful research design.

To implement the RDD, we first construct a subset of the sample by using propensity score matching. We take this approach because (1) the fraction of treated firms is very small compared with the set of all firms (only 76,000 treated observations from over 3.4 million total observations, as shown in Table 2), and (2) firms may be different in dimensions other than employment, which we aim to control by propensity score matching. Formally, we first calculate the propensity score using the same specification as in Section 5, but we exclude employment-related variables from the regression. For each year t, we calculate the propensity score of receiving MIL program loans based on the variables in year $t-1$, $t-2$, and $t-3$. For each firm treated in year t, we select five observations from never-treated firms that have the closest propensity score from the sample in the same year t. The treated firms and the selected never-treated firms form a set of year t treated and control firms. Then, we construct the sample for the RDD by appending these data sets for all years.

The object is to examine whether discontinuities in the receipt of MIL program loans are mirrored by discontinuities in employment and asset growth. For this purpose, we formulate our research design as follows. Let Y_it denote firm i's growth rate of employment or assets in year t and let $x_{i,t-1}$ denote firm i's number of employees in year $t-1$. Then, I_{it}^{MIL} is an indicator that takes a value of one if firm i receives a MIL program loan in year t, and C denotes the threshold of the MIL program eligibility. Here, the growth rate is defined as $\log Y_{it} - \log Y_{i,t-1}$. The probability of receiving a MIL program loan does not jump from one to zero because not all eligible firms receive MLE and measurement error exists, as explained above. Therefore, we adopt a fuzzy RDD where the treatment status is determined by:

$$I_{it}^{MIL} = \alpha + \beta 1\{x_{it} \leq C\} + g(x_{it} - C) + \varepsilon_{it},$$

and the outcome is determined by:

$$Y_{it} = \gamma + \delta I_{it}^{MIL} + f(x_{it} - C) + \epsilon_{it},$$

where $1\{\cdot\}$ is an indicator function that takes a value of one if the statement inside the bracket is true, $f(\cdot)$ and $g(\cdot)$ are unknown smooth functions, and ε_{it} and ϵ_{it} are independent error terms.

In this section, we focus on the threshold of five employees because of the sample size. Around the five-employee threshold, there are about 900 observations (538 treated firms with five employees and 334 treated firms with six employees), whereas there are only 60 observations around the 20-employee threshold (43 treated firms with 20 employees and 18 treated firms with 21 employees).

Figure 6 presents the results graphically, and Table 8 presents the estimated coefficients and z values. The coefficients are estimated by fitting a local first-order polynomial estimate using an Epanechnikov kernel based on the bias-correction methodology in Calonico et al. (2014a) and Calonico et al. (2014b). Panel (a) of Figure 6 and the right columns in Table 8 present the result of the first stage: i.e., the probability of receiving an MIL program loan, Panel (b) of Figure 6 and the first row in Table 8 present the result for employment growth, and Panel (c) of Figure 6 and the second row of Table 8 present the results for asset growth. The results show that there exists a discrete jump in the probability of treatment at the threshold, which confirms the validity of our RDD. We also find that the MIL program has a positive effect on employment growth, which confirms that our results in Section 5 are not driven by unobserved heterogeneity. The results are robust to log-transformation and the choice of bandwidth. We also find a statistically significant effect for employment growth when we use the difference in level as the left-hand side variable instead of the growth rate, and the results are qualitatively and quantitatively the same with different choices of bandwidth.

Table 8: RDD Estimates of the MIL Effect on Employment and Assets

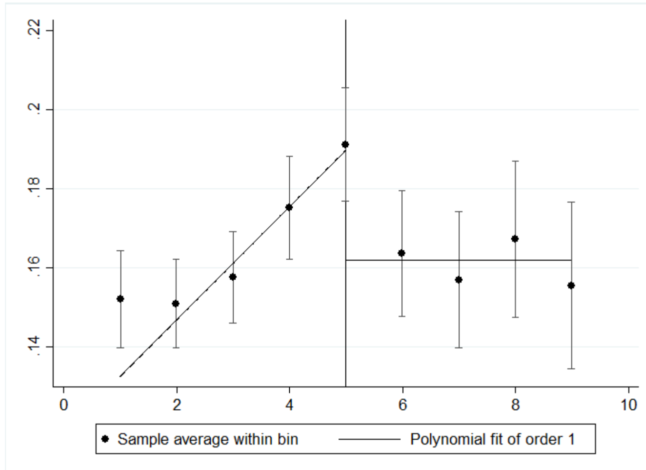
This table presents regression discontinuity (RD) results for the growth rate of the number of employees and the tangible fixed assets, defined as the first difference of the logarithm of the number of employees and the fixed tangible, respectively. The first two columns show the estimated bias-corrected regression discontinuity treatment coefficient and its z-value, the second two columns show the estimated first-stage coefficient and its z-value, and the last two columns show the main bandwidth used to construct the RD point estimator and the bias bandwidth used to construct the bias-correction estimator. The coefficients are estimated by fitting a local first-order polynomial estimate using a epanechnikov kernel using the bias-correction methodology in Calonico et al. (2014a) and Calonico et al. (2014b). The point of discontinuity is at the number of employees to be five in the year before the loan receipt. Significance levels are denoted by <0.1 (*), <0.05 (**), and <0.01 (***).

	Second Stage		First Stage		Bandwidth		
	Estimates (δ)	z-value	Estimates(β)	z-value	main	bias	N
log(Employment)	0.962**	2.060	-0.018**	-2.391	5.176	9.742	37,447
log(Tangible Fixed Assets)	0.712	0.532	-0.017**	-2.547	6.158	9.705	37,437

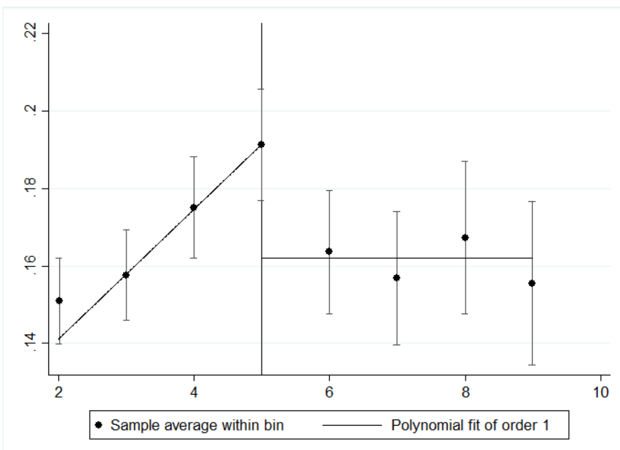
However, we do not find any significant effects in the tangible fixed asset growth. One reason for this is that the running variable is not truly continuous. In the data, the asset growth rate is positively correlated with the number of employees. When we compare firms with five and six employees, it is true that the probability of receiving MIL program loans is lower for six-employee firms and the asset growth rate is higher for treated firms than for control firms for both five- and six-employee firms. However, the asset growth rate is higher for both treated and untreated firms. As a result, the average asset growth rate is higher for six-employee firms because the low probability of getting treated is offset by the natural high asset growth of six-employee firms. If the employment took a continuous value, we could compare firms with five employees and 5.1 employees, in which case we might be able to detect significant effects. However, the number of employees changes only discretely.[12] Our results in Section 5 are based on a comparison between treated firms and control firms after controlling for observable characteristics, which allows us to quantify the treatment effect even with the presence of such differences in the natural growth rate of capital.

Overall, we believe the RDD results are broadly consistent with our main results.

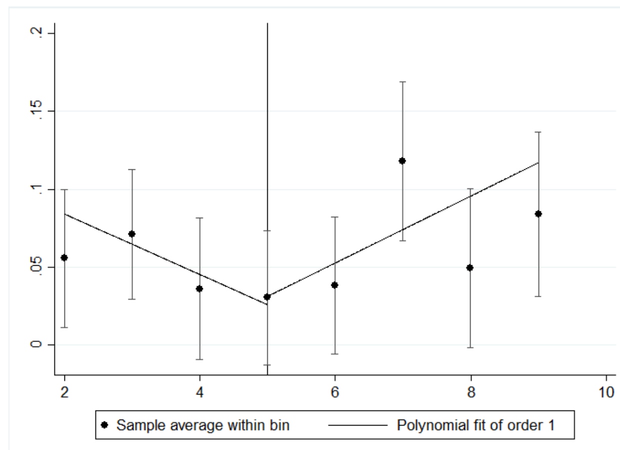
[12]The change in the industry composition of treated firms around the threshold may be another reason that we do not find statistically significant results for the tangible fixed asset growth. As the five-employee threshold applies to relatively less capital-intensive industries, treated firms above the threshold may be more capital intensive, which may further lead to higher capital growth after treatment. However, we do not find any significant difference in the capital-to-labor ratios of treated firms around the threshold.



(a) Probability of Treatment



(b) Employment Growth



(c) Asset Growth

Figure 6: Regression Discontinuity Design

This figure displays the sample average for each value of the number of employees in the year before the treatment and the fitted regression curve together with the 95% confidence interval for the probability of treatment in Panel (a), the growth rate of employment in Panel (b), and the growth rate of tangible fixed assets in Panel (c). In all panels, the horizontal axis shows the number of employees in the year before the treatment, and the point of discontinuity is set at five. The fitted regression line is based on the first-order polynomial estimates using an Epanechnikov kernel and the bias-correction method in Calonico, Cattaneo and Titiunik (2014a) and Calonico, Cattaneo and Titiunik (2014b).

7 Conclusion

We study the impact of a government loan program in Japan on small business performance. We find that borrowers increase both employment and capital levels immediately following receipt of the loans and that these differences are persistent for several years. Our analysis suggests that these results arise because the government loan program relaxes binding financial constraints for treated firms. Using proxies for financial constraints, we find that the results are significantly stronger among financially constrained firms than among unconstrained firms. Smaller firms and firms with lower asset tangibility tend to have larger increases in employment and capital, with the differences particularly large for capital. In addition, treated firms experience an immediate and persistent increase in outstanding debt levels, while they decrease cash holdings in the medium term. Further, the effects on capital are larger in magnitude than those on labor. These results are all consistent with the interpretation that the government loan program relaxes financial constraints for treated firms, allowing them to expand, and to move closer to their optimal capital-to-labor ratio.

While these results suggest that the loan program helps small businesses, the question of whether these benefits outweigh the costs of the program are unclear. According to the Small Enterprise Agency, the central government and prefectural governments spend approximately 100 billion JPY per year on the program. In addition to these direct costs, there may be substantial indirect costs, such as the effects of the program on the competitors of the loan recipients. Even if benefits of this program outweigh its costs, it is unclear whether the optimal form of government support for small business credit is direct loans to small businesses, as in the MIL program, or guarantees for private loans to small businesses, as in the case of the Small Business Administration in the US. These questions are left for future research.

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Appendix A Alternative Estimation Strategy

One natural alternative to the estimation model presented in Section 4 is to use a difference-in-differences model. The most naive specification would be the following:

$$y_{it} = \alpha_i + \alpha_t + \sum_{\tau=-3}^{4} \beta_\tau I\{\tau \text{ year after Treatment}\} + \varepsilon_{it}, \tag{2}$$

where α_i and α_t are individual and year fixed effects. A regression model with both individual fixed effects and time fixed effects as in Equation (2) is commonly called a two-way fixed-effect model. A two-way fixed-effect model is easy to implement and often used when there is a variation in treatment timing. See Goodman-Bacon (2021) for a detailed discussion of the model. As in the standard difference-in-differences model, one crucial assumption required for meaningful estimates is the parallel trend assumption. However, as argued in Section 4, the decision to participate in the MIL program would not be random, and firms with higher credit demand would be more likely to participate in the program. As a result, we would expect a statistically significant pre-trend to exist when we estimate Equation (2).

One straightforward way to address this concern is to include covariates in the estimation equation, i.e., modify the equation to the following:

$$y_{it} = \alpha_i + \alpha_t + \sum_{\tau=-3}^{4} \beta_\tau I\{\tau \text{ year after Treatment}\} + X_{i,t-1}\gamma + \varepsilon_{it}, \tag{3}$$

where $X_{i,t-1}$ is the covariates that capture firms' credit needs, which includes the same variables used in the first stage regression of our main analysis. In this section, we present the estimation results based on Equations (2) and (3) to show that a naive difference-in-differences model is not appropriate in our setting and that a difference-in-differences model with covariates produces qualitatively and quantitatively similar results to those in the main text. In the estimation of Equation (3), we use the same variables as in the estimation of the propensity score for $X_{i,t-1}$.

Tables A1 and A2 show the estimated coefficients for Equation (2) and Equation (3), respectively. In the estimation, we restrict our sample to firms with less than 21 employees. As we expect, the estimated coefficients in Table A1 exhibit strong pre-trends, whereas many of those pre-trends are eliminated in the estimated coefficients in Table A2. Figures A7 and A8 present the estimated coefficients for a subset of dependent variables, the logarithm of total assets, tangible fixed assets, the number of employees, sales, short- and long-term loans, and cash and deposits, based on Equations (2) and (3), respectively.

For all dependent variables, the estimated coefficients in Figure A7 exhibit an increasing pre-trend just before the treated firms receive the MIL program loans, which suggests that firms require credit when those variables are increasing; i.e., when they are increasing assets and employment, borrowing more, and experiencing increasing sales. Compared with the estimates in Figure A7, including covariates eliminates some of the pre-trends. We no longer see the increasing pre-trend for total assets, sales, and cash and deposits, suggesting that the covariates do in fact help control for the firms' credit needs. In terms of the magnitude, the estimated coefficients are similar to the estimated coefficients based on Equation (1). However, an increasing pre-trend for some of the variables remains.

Table A1: Plain Difference-in-Differences Estimates of the MIL Effect

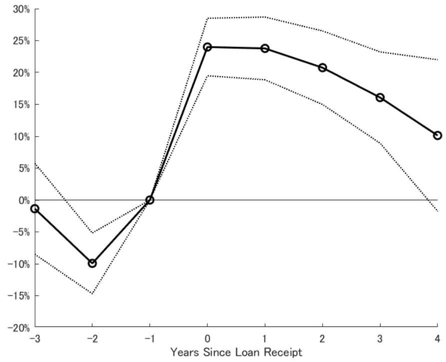
This table reports difference-in-differences estimates of $t-3$ to $t+4$ years from the treatment where the dependent variables are $\log$(Short Term Loans), $\log$(Short & Long Term Loans), $\log$(Cash & Deposit), $\log$(Temporal Liquidity), $\log$(Sales), $\log$(Number of Employees), $\log$(Total Assets), $\log$(Tangible Fixed Assets), $\log$(Building & Structure), and $\log$(Machinery & Equipment), listed in each column. For each dependent variable, we estimate the difference-in-differences model by regressing each outcome variable on the indicator variables of τ ($\tau = -3, -2, \ldots, 4$) years after treatment, year-fixed effect, and firm-fixed effect. Standard errors in parentheses are clustered at firm-level and significance levels are denoted by <0.1 (*), <0.05 (**), and <0.01 (***).

Years Since Loan Receipt	log(Num of Employees)	log(Total Assets)	log(Tangible Fixed Assets)	log(Building & Structure)	log(Machinery Equipment)	log(Sales)	log(Cash & Deposit)	log(Temporal Liquidity)	log(Short Term Loans)	log(Short & Long Term Loans)
-3	0.028**	-0.043***	-0.014	-0.045	0.023	-0.058***	-0.022	0.024	-0.02	-0.065**
	(0.012)	(0.009)	(0.036)	(0.06)	(0.07)	(0.011)	(0.017)	(0.017)	(0.059)	(0.027)
-2	-0.042***	-0.031***	-0.099***	-0.08**	0.025	-0.049***	-0.031***	0.009	-0.049	-0.098***
	(0.007)	(0.005)	(0.024)	(0.039)	(0.051)	(0.007)	(0.011)	(0.011)	(0.038)	(0.019)
0	0.077***	0.073***	0.24***	0.115***	0.157***	-0.005	0.094***	0.094***	0.054	0.342***
	(0.006)	(0.005)	(0.023)	(0.032)	(0.042)	(0.006)	(0.011)	(0.011)	(0.037)	(0.016)
1	0.08***	0.06***	0.238***	0.136***	0.164***	0	0.029**	0.02*	0.108**	0.304***
	(0.007)	(0.005)	(0.025)	(0.034)	(0.046)	(0.006)	(0.012)	(0.011)	(0.041)	(0.017)
2	0.073***	0.043***	0.208***	0.188***	0.149***	-0.011	0.011	0.003	0.164***	0.253***
	(0.008)	(0.006)	(0.029)	(0.042)	(0.053)	(0.008)	(0.014)	(0.014)	(0.048)	(0.018)
3	0.085***	0.024***	0.161***	0.162***	0.129*	-0.028***	0.01	0.014	0.183***	0.216***
	(0.011)	(0.008)	(0.037)	(0.051)	(0.066)	(0.009)	(0.018)	(0.017)	(0.064)	(0.02)
4	0.11***	-0.008	0.101*	0.087	-0.027	-0.057***	-0.003	0.022	0.327***	0.166***
	(0.018)	(0.014)	(0.061)	(0.081)	(0.103)	(0.014)	(0.03)	(0.029)	(0.109)	(0.025)
N	3,661,269	3,654,017	3,650,021	2,175,582	2,153,655	3,657,591	3,614,854	3,597,817	3,651,618	3,650,074
R^2	0.8491	0.9483	0.8703	0.9101	0.8053	0.8739	0.8556	0.7359	0.768	0.799

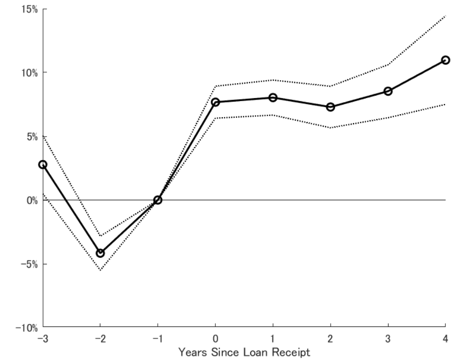
Table A2: Alternative Difference-in-Differences Estimates of the MIL Effect

This table reports difference-in-differences estimates of $t-3$ to $t+4$ years from the treatment where the dependent variables are $\log$(Short Term Loans), $\log$(Short & Long Term Loans), $\log$(Cash & Deposit), $\log$(Temporal Liquidity), $\log$(Sales), $\log$(Number of Employees), $\log$(Total Assets), $\log$(Tangible Fixed Assets), $\log$(Building & Structure), and $\log$(Machinery & Equipment), listed in each column. For each dependent variable, we estimate the difference-in-differences model by regressing each outcome variable on the indicator variables of τ ($\tau = -3, -2, \ldots, 4$) years after treatment, firm- and year-fixed effect, and the same variables that are used for calculation of propensity scores as control variables, including a dummy variable indicating whether the total number of employees is less than 21, years of operation, one-year to thee-year lagged values of $\log$(Sales), $\log$(Employees), $\log$(Cash & Deposit), $\log$(Total Assets), $\log$(Short & Long Term Loans), and $\log$(Tangible Fixed Assets), and region- and industry-fixed effects. Standard errors in parentheses are clustered at firm-level and significance levels are denoted by <0.1 (*), <0.05 (**), and <0.01 (***).

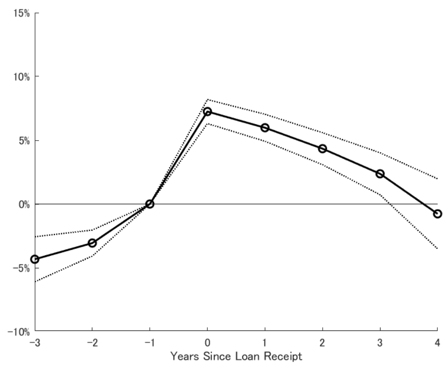
Years Since Loan Receipt	log(Num of Employees)	log(Total Assets)	log(Tangible Fixed Assets)	log(Building & Structure)	log(Machinery Equipment)	log(Sales)	log(Cash & Deposit)	log(Temporal Liquidity)	log(Short Term Loans)	log(Short & Long Term Loans)
-3	0.059***	0.003	0.084***	-0.029	0.103	0	0.042**	0.036*	-0.063	-0.037
	(0.011)	(0.008)	(0.029)	(0.052)	(0.074)	(0.012)	(0.02)	(0.019)	(0.073)	(0.024)
-2	-0.045***	-0.008	-0.072***	-0.098**	0.052	-0.001	0.004	0.001	-0.103*	-0.084***
	(0.008)	(0.006)	(0.021)	(0.038)	(0.054)	(0.008)	(0.014)	(0.014)	(0.053)	(0.018)
0	0.061***	0.052***	0.161***	0.131***	0.135***	0	0.075***	0.074***	0.025	0.184***
	(0.007)	(0.005)	(0.018)	(0.031)	(0.045)	(0.007)	(0.012)	(0.012)	(0.044)	(0.015)
1	0.066***	0.047***	0.125***	0.126***	0.141***	0.015*	0.014	0.002	0.11**	0.191***
	(0.007)	(0.005)	(0.019)	(0.033)	(0.047)	(0.008)	(0.013)	(0.013)	(0.048)	(0.016)
2	0.061***	0.033***	0.105***	0.139***	0.092*	0.022**	-0.01	-0.027*	0.153***	0.187***
	(0.008)	(0.006)	(0.022)	(0.038)	(0.054)	(0.009)	(0.015)	(0.015)	(0.055)	(0.018)
3	0.081***	0.039***	0.113***	0.135***	0.132**	0.03**	0.025	0.004	0.229***	0.236***
	(0.01)	(0.007)	(0.027)	(0.046)	(0.065)	(0.011)	(0.018)	(0.018)	(0.068)	(0.023)
4	0.101***	0.023*	0.096**	0.089	-0.017	0.036**	0.008	-0.017	0.434***	0.251***
	(0.016)	(0.012)	(0.044)	(0.069)	(0.098)	(0.017)	(0.03)	(0.029)	(0.109)	(0.037)
N	2,248,054	2,248,054	2,247,231	1,540,886	1,524,570	2,247,769	2,234,653	2,231,191	2,244,896	2,247,098
R^2	0.849	0.969	0.906	0.937	0.825	0.929	0.878	0.766	0.781	0.857



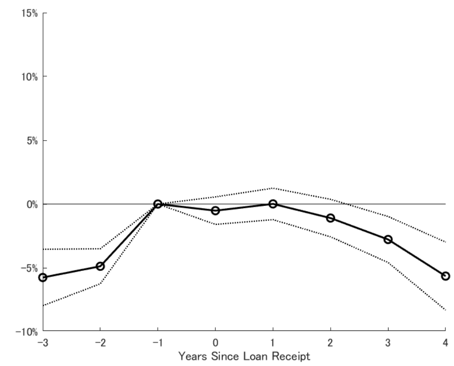
(a) Tangible Fixed Assets



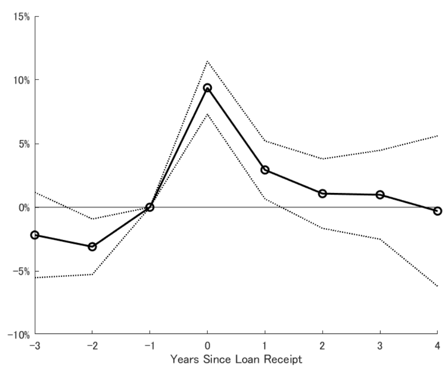
(b) Number of Employees



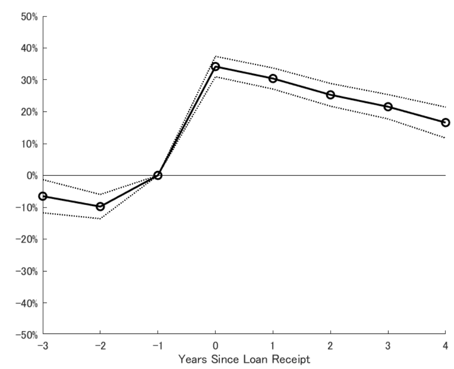
(c) Total Assets



(d) Sales



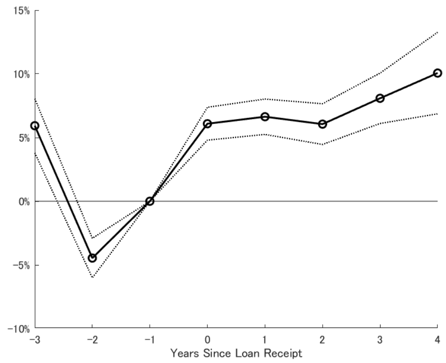
(e) Cash and Deposits



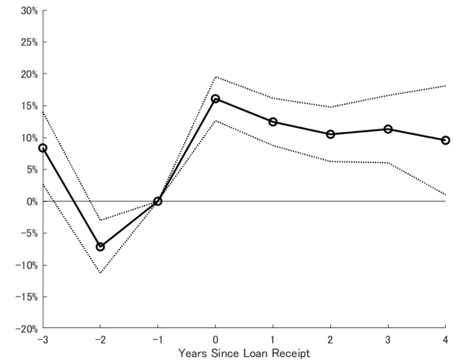
(f) Short- and Long-Term Loans

Figure A7: Plain Difference-in-Differences Estimates of the MIL Program Effect Over the Year

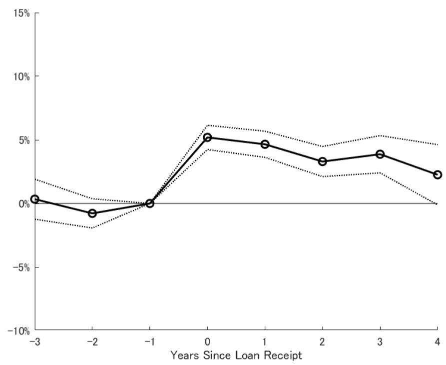
This figure shows the difference-in-differences estimates of $t-3$ to $t+4$ years from the treatment where the dependent variables are described in each panel name and the independent variables are the indicator variables of τ ($\tau = -3, -2, \ldots, 4$) years after treatment and firm and year fixed effects. The dotted lines are the bounds of the 95% confidence interval, based on standard errors clustered at the firm level.



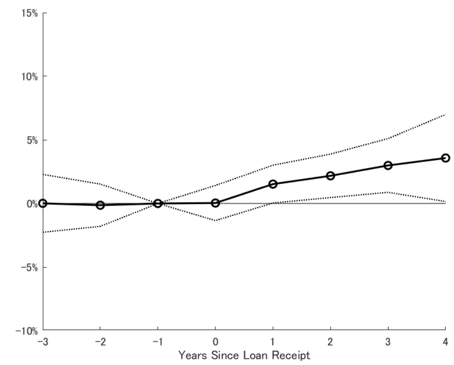
(a) Number of Employees



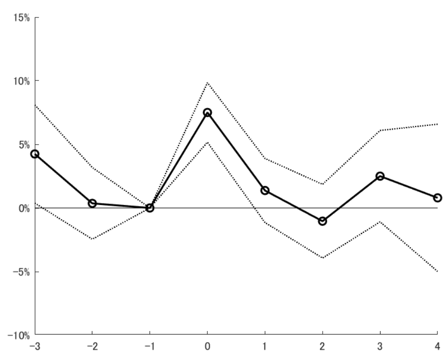
(b) Tangible Fixed Assets



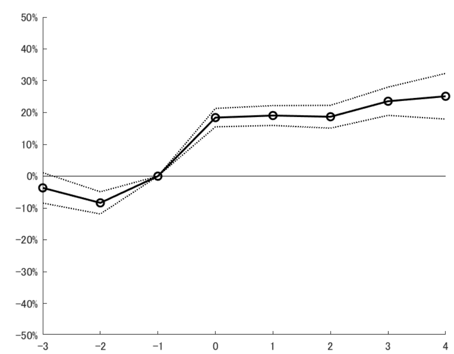
(c) Total Assets



(d) Sales



(e) Cash and Deposit



(f) Short- and Long-Term Loans

Figure A8: Alternative Difference-in-Differences Estimates of the MIL Program Effect Over the Year

This figure shows difference-in-differences estimates of $t-3$ to $t+4$ years from the treatment where the dependent variables are described in each panel name and the independent variables are the indicator variables of τ ($\tau = -3, -2, \ldots, 4$) years after treatment and firm and year fixed effects and control variables, including a dummy variable indicating whether the total number of employees is less than 21, years of operation, one-year to thee-year lagged values of $\log$(Sales), $\log$(Employees), $\log$(Cash & Deposit), $\log$(Total Assets), $\log$(Short- & Long-Term Loans), and $\log$(Tangible Fixed Assets), and region and industry fixed effects. The dotted lines are the bounds of the 95% confidence interval, based on standard errors clustered at the firm level.

Appendix B Estimation Results for the Propensity Score

Tables B1 and B2 below present the propensity score estimation results. Table B1 presents the results without including a constraint measure, whereas Table B2 presents the results including asset tangibility as one of the measures of financial constraints. Due to limited space in Table B1 and Table B2, we give table description for them below.

Description for Table B1 This table reports a series of probit estimates, where the dependent variables are indicator variables of whether the firms receive the MIL program loans in year t, $t = 2016, \ldots, 2019$ and the independent variables include a dummy variable indicating whether the total number of employees is less than 21, years of operation, one-year to thee-year lagged values of $\log$(Sales), $\log$(Employees), $\log$(Cash & Deposit), $\log$(Total Assets), $\log$(Short- & Long-Term Loans), and $\log$(Tangible Fixed Assets), and region and industry fixed effects. Note that due to the data collection process, the number of observations for 2019 is slightly smaller than the numbers for other years. Standard errors are shown in parentheses and the symbols *, **, and *** denote significance levels of $<$0.1, $<$0.05, and $<$0.01, respectively.

Description for Table B2 This table reports a series of probit estimates where the dependent variables are indicator variables of whether the firms receive the MIL program loans in year t, $t = 2016, \ldots, 2019$ and the independent variables include asset tangibility, defined as tangible fixed assets divided by total assets, a dummy variable indicating whether the total number of employees is less than 21, years of operation, one-year to thee-year lagged values of $\log$(Sales), $\log$(Employees), $\log$(Cash & Deposit), $\log$(Total Assets), $\log$(Short- & Long-Term Loans), and $\log$(Tangible Fixed Assets), and region and industry fixed effects. Note that due to the data collection process, the number of observations for 2019 is slightly smaller than the numbers for other years. Standard errors are shown in parentheses and the symbols *, **, and *** denote significance levels of $<$0.1, $<$0.05, and $<$0.01, respectively.

Table B1: Estimation Results on First Stage Propensity Score Regression by Year

Due to limited space, please see Appendix B for the table description.

	2016	2017	2018	2019
log(Years of Operation$_i$)	-0.012	-0.056*	-0.012	-0.113*
	(0.035)	(0.033)	(0.038)	(0.058)
$1_{\{Employees_{i,t-1} \leq 20\}}$	1.722***	1.920***	1.796***	1.894***
	(0.205)	(0.201)	(0.215)	(0.301)
log(Sales$_{i,t-1}$)	0.150**	0.144**	0.061	0.174
	(0.074)	(0.072)	(0.08)	(0.127)
log(Employees$_{i,t-1}$)	-0.047	0.019	-0.037	-0.106
	(0.053)	(0.051)	(0.058)	(0.097)
log(Cash Deposits$_{i,t-1}$)	-0.030	-0.095***	-0.028	-0.129**
	(0.034)	(0.033)	(0.038)	(0.056)
log(Total Asset$_{i,t-1}$)	-0.061	-0.015	0.029	-0.029
	(0.092)	(0.089)	(0.099)	(0.147)
log(Short Term and Long Term Loans$_{i,t-1}$)	0.188***	0.100***	0.073**	0.107**
	(0.037)	(0.033)	(0.035)	(0.051)
log(Tangible Asset$_{i,t-1}$)	0.039*	0.021	0.060***	0.002
	(0.023)	(0.021)	(0.023)	(0.034)
log(Sales$_{i,t-2}$)	0.151*	0.105	0.189**	0.210
	(0.084)	(0.083)	(0.096)	(0.144)
log(Employees$_{i,t-2}$)	-0.442***	-0.484***	-0.460***	-0.368***
	(0.058)	(0.055)	(0.064)	(0.109)
log(Cash Deposits$_{i,t-2}$)	-0.037	0.010	-0.103***	-0.073
	(0.037)	(0.037)	(0.041)	(0.061)
log(Total Asset$_{i,t-2}$)	-0.006	-0.093	0.006	0.093
	(0.111)	(0.108)	(0.121)	(0.176)
log(Short Term and Long Term Loans$_{i,t-2}$)	-0.070**	-0.022	-0.036	-0.076
	(0.033)	(0.034)	(0.036)	(0.052)
log(Tangible Asset$_{i,t-2}$)	-0.101***	-0.106***	-0.151***	-0.088**
	(0.028)	(0.026)	(0.028)	(0.042)
log(Sales$_{i,t-3}$)	-0.103*	-0.034	-0.011	-0.060
	(0.056)	(0.064)	(0.073)	(0.098)
log(Employees$_{i,t-3}$)	0.057	0.100*	0.133**	0.124
	(0.054)	(0.052)	(0.06)	(0.101)
log(Cash Deposits$_{i,t-3}$)	0.010	0.051	0.018	-0.005
	(0.034)	(0.034)	(0.038)	(0.056)
log(Total Asset$_{i,t-3}$)	-0.243***	-0.205**	-0.300***	-0.355**
	(0.092)	(0.09)	(0.101)	(0.146)
log(Short Term and Long Term Loans$_{i,t-3}$)	0.025	0.023	0.023	0.050
	(0.03)	(0.028)	(0.031)	(0.047)
log(Tangible Asset$_{i,t-3}$)	0.069***	0.083***	0.111***	0.095**
	(0.025)	(0.023)	(0.026)	(0.038)
Δ log(Employees$_{t-3}$)	-0.071	-0.101**	-0.219***	-0.069
	(0.05)	(0.049)	(0.057)	(0.095)
Fixed-effects				
Region	X	X	X	X
Industry	X	X	X	X
N	638,947	618,771	572,567	309,440
Pseudo R^2	0.051	0.051	0.051	0.064

Table B2: Estimation Results on First Stage Propensity Score Regression with a Financial Constraint Measure

Due to limited space, please see Appendix B for the table description.

	2016	2017	2018	2019
Asset Tangibility$_{i,t-1}$	0.291*	0.431***	0.426***	0.264
	(0.151)	(0.145)	(0.163)	(0.245)
log(Years of Operation$_i$)	-0.01	-0.054	-0.01	-0.112*
	(0.035)	(0.033)	(0.038)	(0.058)
$1_{\{Employees_{i,t-1} \leq 20\}}$	1.727***	1.926***	1.802***	1.899***
	(0.205)	(0.201)	(0.215)	(0.301)
log(Sales$_{i,t-1}$)	0.164**	0.162**	0.079	0.187
	(0.075)	(0.073)	(0.082)	(0.129)
log(Employees$_{i,t-1}$)	-0.046	0.019	-0.037	-0.105
	(0.053)	(0.051)	(0.058)	(0.097)
log(Cash Deposits$_{i,t-1}$)	-0.02	-0.08**	-0.012	-0.12**
	(0.035)	(0.034)	(0.038)	(0.056)
log(Total Asset$_{i,t-1}$)	-0.067	-0.026	0.016	-0.037
	(0.092)	(0.089)	(0.099)	(0.147)
log(Short Term and Long Term Loans$_{i,t-1}$)	0.183***	0.095***	0.069**	0.105**
	(0.037)	(0.033)	(0.034)	(0.051)
log(Tangible Asset$_{i,t-1}$)	0.021	-0.003	0.035	-0.012
	(0.024)	(0.022)	(0.025)	(0.036)
log(Sales$_{i,t-2}$)	0.157*	0.113	0.199**	0.216
	(0.085)	(0.084)	(0.097)	(0.144)
log(Employees$_{i,t-2}$)	-0.442***	-0.483***	-0.459***	-0.369***
	(0.058)	(0.055)	(0.064)	(0.109)
log(Cash Deposits$_{i,t-2}$)	-0.035	0.013	-0.101**	-0.071
	(0.037)	(0.037)	(0.041)	(0.061)
log(Total Asset$_{i,t-2}$)	-0.014	-0.106	-0.005	0.085
	(0.111)	(0.107)	(0.121)	(0.175)
log(Short Term and Long Term Loans$_{i,t-2}$)	-0.069**	-0.021	-0.035	-0.076
	(0.033)	(0.034)	(0.036)	(0.052)
log(Tangible Asset$_{i,t-2}$)	-0.101***	-0.106***	-0.15***	-0.088**
	(0.028)	(0.026)	(0.028)	(0.042)
log(Sales$_{i,t-3}$)	-0.101*	-0.028	-0.005	-0.057
	(0.057)	(0.065)	(0.074)	(0.098)
log(Employees$_{i,t-3}$)	0.057	0.100*	0.134**	0.125
	(0.054)	(0.052)	(0.06)	(0.101)
log(Cash Deposits$_{i,t-3}$)	0.011	0.054	0.02	-0.004
	(0.034)	(0.034)	(0.038)	(0.056)
log(Total Asset$_{i,t-3}$)	-0.238***	-0.200**	-0.294***	-0.351**
	(0.092)	(0.089)	(0.101)	(0.146)
log(Short Term and Long Term Loans$_{i,t-3}$)	0.026	0.025	0.024	0.05
	(0.03)	(0.028)	(0.031)	(0.047)
log(Tangible Asset$_{i,t-3}$)	0.065**	0.077***	0.105***	0.092**
	(0.025)	(0.023)	(0.026)	(0.038)
Δ log(Employees$_{t-3}$)	-0.071	-0.101**	-0.22***	-0.069
	(0.05)	(0.049)	(0.057)	(0.095)
Fixed-Effects				
Region	X	X	X	X
Industry	X	X	X	X
N	638,947	618,771	572,567	309,440
Pseudo R^2	0.051	0.052	0.051	0.064