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Forbearance Lending as a Crisis Management Tool: Evidence from Japan*

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Abstract

Credit market interventions have become a widespread policy tool deployed by governments around the world to support their corporate sectors following shocks like the GFC and the pandemic. Among those policies, forbearance programs allowed firms to temporarily stop making payments on their debt obligations or obtain debt forgiveness; However, the impact of these policies is not fully understood. In particular, forbearance is generally believed to keep unviable firms alive and to contribute to the zombification of the corporate sector. To inform this debate, we examine the effects of Japan's SME Financing Facilitation Act, which encouraged banks to offer forbearance to troubled SMEs. We develop a framework to quantify the aggregate impact of the policy using a difference-in-differences approach combined with back-of-the-envelope counterfactual exercises. Our evaluation indicates that, when coupled with business restructuring plans, forbearance lending can temporarily boost output without contributing to the widespread zombification of the corporate sector. Forbearance is more effective when credit market disruptions impede the reallocation of capital.

Keywords: Forbearance, Zombie Firms, Credit Frictions, Misallocation, Productivity, Credit Market Interventions, Policy Evaluation

JEL classification: E22, E43, E44, E65, G21, O40

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1 Introduction

Interventions in corporate credit markets have featured prominently in crisis responses over the last two decades, from the global financial crisis to the European sovereign debt crisis and the COVID-19 pandemic. Governments worldwide deployed loan guarantees, direct lending schemes, and loan forbearance—a practice whereby banks grant temporary relief to struggling borrowers (e.g., extended repayment periods and reduced interest payments). The academic and policy literature is mostly critical of forbearance for contributing to zombification—a situation where bank lending keeps unviable firms alive, resulting in lower aggregate productivity through credit misallocation and zombie congestion.[1]

Our evaluation of Japan's 2009 SME Financing Facilitation Act, however, suggests that forbearance lending, when coupled with business restructuring plans, can provide temporary relief for struggling firms without contributing to zombification, thereby protecting productive capital. Under the Act, financial institutions were required to make their "best effort" to ease repayment conditions for qualifying SMEs that requested support. The Japanese Financial Services Agency allowed institutions to exclude restructured SME loans from their reported non-performing loans, provided that borrowers and their banks submitted business restructuring plans expected to restore loan performance within five years. The law provides a quasi-experiment that enables us to quantify the aggregate consequences of loan forbearance using a difference-in-differences approach combined with back-of-the-envelope counterfactual exercises, guided by a partial equilibrium search-and-matching model of credit markets incorporating forbearance incentives.

Our first finding is that the Act worked as an interest rate subsidy, depressing average interest rates by about 40.5% over 2010–2018[2]. Treatment effects are larger in the years closer to implementation and fade over time. Second, absent the policy, the aggregate capital stock would have been 1.3% lower on average, at the expense of aggregate capital productivity, which would have been 0.5% higher. Third, the extent of credit reallocation determines whether the policy leads to output gains or losses. In the more plausible scenario of no reallocation, output would have been 2.3% lower on average; under seamless reallocation, output would have been 1.9% higher. Seamless reallocation is unrealistic as frictions impede the process and capital reallocation is pro-cyclical, i.e. depressed during recessions (e.g., Caballero and Hammour, 2005; Eisfeldt and Rampini, 2006). Our counterfactuals therefore provide bounds on output gains and losses. Finally, the Act did not contribute to the creation of zombie firms. On the contrary, greater exposure to the policy improved firm-level performance, suggesting that business restructuring plans allowed troubled SMEs to resurrect. To the extent that banks granted forbearance to viable firms, the Act enabled them to weather temporary difficulties while limiting the negative impact on aggregate productivity. Our results challenge the view that forbearance necessarily

[1]See, e.g., Acharya et al. (2021, 2022, 2023), Andrews and Petroulakis (2019), Barbaro and Tirelli (2021), Faria-e-Castro et al. (2024), Caballero et al. (2008), Adalet McGowan et al. (2018), Banerjee and Hoffmann (2022), 'Alvarez et al. (2023).

[2]The average treatment effect on log interest rates is -0.52 and $\exp(-0.52) - 1 \approx -0.405$.

contributes to zombification: when coupled with business restructuring plans, forbearance can provide relief for otherwise solvent firms facing temporary shocks. A carefully designed intervention can thus serve as a crisis management tool, particularly when credit reallocation is subdued, e.g. in recessions.

We make several contributions to the literature. First, we provide evidence on the impact of loan forbearance using a plausibly exogenous policy shock from the point of view of the lenders. Such evidence is rare as forbearance is typically selective and only applied to a subset of bank–firm relationships with specific characteristics. The SME Financing Facilitation Act is exceptional in that it mandated forbearance for all banks. The exogenous nature of the policy shock is reflected in the following statement by Financial Services Minister Shizuka Kamei:

> "As long as I'm financial services minister, I'm not going to leave small companies in the lurch, unable to get loans. If a bank takes that approach, I'll hit them with a business improvement order." *The Japan Times*, 7 October 2009.

This sets post-GFC forbearance apart from that practised during the Japanese Lost Decade, during which low-capitalised banks were more likely to forbear (e.g. Peek and Rosengren 2005, Caballero et al. 2008). Although there was no penalty imposed on banks that did not follow the "best effort" requirement, almost all requests for loan restructurings were accepted (see Yamori, 2019). The law therefore provides a quasi-experiment which enables us to estimate the plausibly causal impact of forbearance. To our knowledge, we are the first to holistically evaluate the consequences of this policy intervention.

Second, we provide evidence on the aggregate consequences of forbearance. Our evaluation of the SME Financing Facilitation Act builds upon but distinctly departs from existing micro-level evaluations of the policy, most notably Ono and Yasuda (2017), who utilize survey data to contrast firm-level effects of payment deferrals versus debt forgiveness through the lens of debt overhang. Our paper answers a fundamentally different macroeconomic question: does mandated forbearance inevitably lead to zombification, and what are its aggregate real effects? By embedding our empirical estimates into a structural search-and-matching model, we move beyond micro-level outcomes to quantify aggregate counterfactual impacts on total capital, productivity, and output. This complements the broader literature on forbearance lending during Japan's Lost Decade and the European sovereign debt crisis, which consists mostly of firm- or industry-level studies. Studies estimating aggregate impacts include Kwon, Narita and Narita (2015) on Japan's Lost Decade, Tracey (2021) on the European sovereign debt crisis, and Faria-e-Castro, Paul, and Sánchez (2024) on the US. An overall quantitative assessment matters for policy, as answering whether forbearance can serve as a crisis management tool without contributing to zombification requires weighing costs and benefits in aggregate counterfactual exercises.

Third, our methodological contribution is to design a theory-driven empirical strategy that identifies the causal trajectory of the policy over a decade. We leverage a dynamic, continuous difference-in-differences framework combined with recent advances in parallel trends sensitivity analysis (Rambachan and Roth, 2023). This rigorous approach allows us to estimate the effect of the Act in the absence of data on the universe of loan restructurings. We develop a simple search-and-matching model of the credit market where banks have incentives to forbear, which guides our estimation and counterfactual exercises. Using accounting data from Tokyo Shoko Research (TSR) and survey data from the Research Institute of Economy, Trade and Industry (RIETI), we estimate the policy's impact on average interest rates paid by firms, combining the eligibility criteria with RIETI survey data to measure treatment exposure at the firm level. The estimated annual treatment effects on interest rates underpin back-of-the-envelope counterfactual exercises guided by the model, in which we remove the treatment effects and compare the counterfactual population of firms to the observed population in terms of capital stock, capital productivity, and output.

The rest of the paper is structured as follows. Section 2 discusses the literature on zombie lending in Japan and presents the SME Financing Facilitation Act. Section 3 presents our search-and-matching model. Section 4 discusses our data and measurement. Section 5 presents the DiD results for interest rates and explores whether the Act encouraged zombification. Section 6 presents the counterfactuals. Section 7 concludes.

2 Forbearance lending in Japan: Then and now

2.1 Zombie lending and the Lost Decade

Japan is the most prominent case study for forbearance lending, which has often been identified as a key driver behind the elevated number of zombie firms in the Japanese economy. The "Lost Decade" following the financial and banking crisis in the late 1990s and early 2000s produced the first systematic evidence on zombie lending. Peek and Rosengren (2005) find that banks increased loans to financially weaker firms. Troubled banks with reported capital ratios close to the required minimum were more likely to increase loans to their weaker borrowers— evidence of forbearance to hide the extent of non-performing loans. Moreover, if a bank was in the same business group (keiretsu) as the firm, it was more likely to lend to weaker borrowers. Caballero, Hoshi and Kashyap (2008) document a significant increase in zombie firms during 1993–2002, showing that industries with greater prevalence of zombie firms exhibited more depressed job creation and destruction, and lower productivity. Other studies document similar findings.[3] Muto, Sudo and Yoneyama (2023) use a DSGE model to show that a large fraction of the TFP growth decline in the early 1990s was due to the impairment of banks' and firms' balance sheets. Overall, the literature is unequivocal that forbearance lending contributed to the widespread zombification of the Japanese economy during the Lost Decade.

[3]See, e.g., Sekine, Kobayashi and Saita (2003), Ahearne and Shinada (2005), Hamao, Kutsuna and Peek (2012), and Kwon, Narita and Narita (2015).

2.2 The SME Financing Facilitation Act

In November 2009, the Japanese government enacted the SME Financing Facilitation Act[4] to help SMEs that had fallen into unprofitable conditions. Under this law, financial institutions were required to make their "best effort" to ease repayment conditions for qualifying SMEs that asked for support. Not all SMEs were eligible: Article 2 paragraph (2) defines qualifying SMEs based on employee count and stated share capital, with criteria varying by industry, while Article 4 excludes financial institutions and their subsidiaries or parent companies. The eligibility criteria are listed in Supplemental Appendix A. Concurrently, the JFSA relaxed its loan classification rules so that financial institutions could exclude restructured SME loans from their reported non-performing loans provided that firms formulated business improvement plans expected to restore loan performance within five to ten years, or were expected to do so within a one-year grace period. Financial institutions were expected to actively support the formulation of these plans, given that many SMEs lacked the capacity to develop them independently. Business improvement plans typically encompassed cost restructuring measures such as expense reductions and asset sales, revenue-side initiatives including new customer acquisition and product development, and financial restructuring in the form of rescheduled debt repayments and extended maturities (Yamori, 2019). Although there was no penalty for non-compliance with the "best efforts" requirement, almost all restructuring requests were accepted (see Yamori, 2019), reflecting the powerful political incentives to bail out struggling firms.

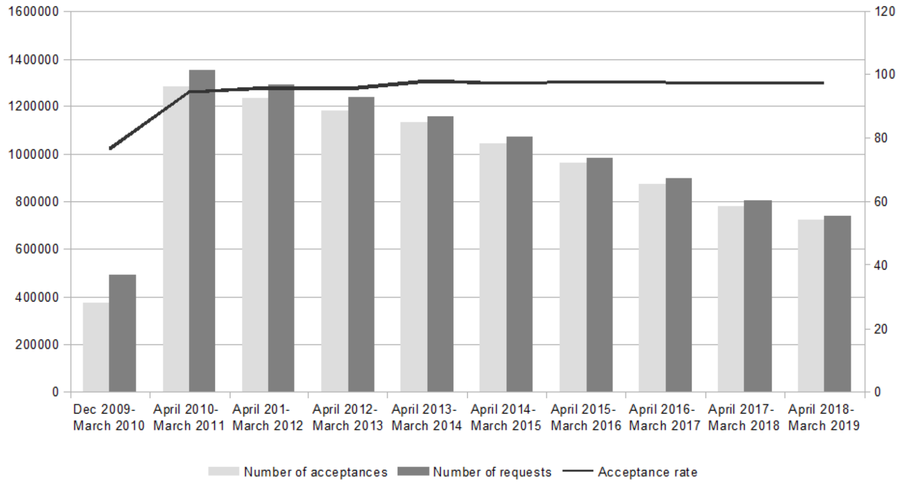
The Act was originally set to expire at the end of March 2011, but it was extended twice before finally expiring at the end of March 2013. However, the practice endured after the Act expired. According to Harada et al. (2015), the JFSA did not reverse the rule allowing banks to classify restructured SME loans as "normal". As a result, troubled SMEs continued to request restructuring, and banks continued to grant nearly all such requests. Figure 1 shows the number of requests by SMEs to change loan conditions and the number that banks accepted. The acceptance rates were extremely high, averaging around 91% between December 2009 and March 2013, and 97% between April 2013 and March 2019, even though the law had formally expired.

There is a concern that the Act might have encouraged banks to roll over loans to zombie firms (see, e.g., Harada et al., 2015; Imai, 2019). However, there are reasons why this time might be different. First, unlike the hidden forbearance of the Lost Decade (Peek and Rosengren, 2005), forbearance under the Act was mandated by law and disclosed by financial institutions. We should therefore not expect it to be driven solely by weak banks (see Supplemental Appendix H). Second, banks could only exclude restructured loans from non-performing loan reports if firms and their lenders jointly developed business improvement plans expected to restore performance, limiting the extent to which forbearance propped up unviable firms and mitigating moral hazard. Yamori (2019) finds that about 60% of firms whose loan conditions were amended ultimately recovered. Finally, the exclusion of financial institutions, and their subsidiaries and parent companies, helps address distortions created

[4]The Act is also referred to as the Debt Moratorium Law.

by keiretsu affiliations during the Lost Decade.

Figure 1: Number of requests by SMEs and acceptance by banks



Notes: Number of requests by SMEs to restructure their loans and number of accepted requests for each fiscal year. Source: Financial Services Agency of Japan.

Overall, the design of the policy means that forbearance under the Act is more likely to constitute "good forbearance" than forbearance during the Lost Decade, i.e. temporary debt relief for struggling but otherwise solvent firms facing a transitory shock. The literature provides a rationale for such interventions. Deeper and longer recessions can cause lasting damage to productive capacity (e.g., Vinci and Licandro, 2021), in part because credit frictions force the exit of productive but constrained firms. Crouzet and Tourre (2021) show that when a downturn is accompanied by financial market disruptions, interventions in corporate credit markets can forestall inefficient liquidations, at the cost of debt overhang and depressed investment in the long run, with short-term benefits quantitatively dominating long-run costs.

3 A structural model of bank forbearance

In this section, we present a tractable model of bank forbearance. The purpose of our model is to provide an analytical framework for our empirical analysis and to serve as a guide for interpreting our results. The tractability of the model allows us to perform back-of-the-envelope counterfactual exercises using difference-in-differences estimation results. The model economy is populated by two types of agents, entrepreneurs and banks. Credit markets are characterized by search frictions. Entrepreneurs produce a homogeneous final good and search for bank credit to expand their capital stock to produce at their optimal scale. Firms differ in terms of their total factor productivity (TFP), z. Firms receive a new TFP draw in every period from a distribution $H(z)$. In addition, we assume that entrepreneurs can be of J observable types, where a type-j firm is characterised by k_{0j}, its capital stock that is not financed by bank loans, but instead is financed by, e.g., equity capital and publicly traded debt. These types are meant to capture time-invariant or slow-moving characteristics, such as size, which are an important determinant of access to bank loans. The J observable types correspond to J credit markets with different credit market tightness.

Banks search for entrepreneurs to whom they can lend profitably. We assume that each firm with observable properties j enters a different credit market to search for an optimal expansion of its capital stock. Hence, all firms in any credit market j have the same properties when entering the market. A bank opening a new credit line in a market must pay a fixed search cost of κ to attempt to find an entrepreneur in the market in every period. Matching frictions characterize each market. Credit market tightness in each market is denoted with θ_{jt}. It is defined as the ratio of vacant credit lines v_{jt} over unmatched entrepreneurs u_{jt} in credit market j $\left(\theta_{jt} = \frac{v_{jt}}{u_{jt}}\right)$. The probability that a bank is matched with an entrepreneur is denoted with q_{jt}. As long as the expected value of a vacant credit line is greater than zero in a market, banks will open additional credit lines. Available credit lines are matched with searching entrepreneurs in a frictional process summarized by a matching function. Once a vacant credit line is matched with an entrepreneur, the lender and the borrower agree on a contract that determines the interest rate on the desired loan amount. Contracting is the result of a Nash bargaining process. Loan conditions are defined intra-period, so a new loan interest rate and loan amount are determined every period. The benefit of a credit line is subject to idiosyncratic shocks to productivity. As long as the idiosyncratic benefit of the bank-entrepreneur match is sufficiently high, the contract is renewed in the next period. If not, both parties agree to end the relationship. The loan creation and destruction decisions generate entrepreneur flows in and out of the credit market. In terms of timing, we assume that credit markets open and matches are formed. Then productivity draws (z_{ijt}) are observed (productivity can therefore be seen as match-specific), interest rates and loan amounts are determined, and finally production ensues.

In addition to search frictions, each credit market is characterised by forbearance incentives. Incentives for forbearance lending are modelled as a termination cost (or severance cost) τ_{ijt} incurred by banks when terminating a lending relationship that is no longer profitable. These abstract forbearance incentives can be interpreted to capture any kind of monetary or reputational cost that the bank incurs when exiting a credit relationship. While this cost could be micro-founded by modelling banks' motives[5], there is no need to do so because forbearance was *mandated* by the SME Financing Facilitation Act. In other words, banks effectively had to offer loan restructurings to struggling SMEs that asked for support.

[5]For example, the cost could be associated with incentives to rationally avoid liquidating non-performing loans, e.g., when realizing loan losses would push the lender's capital below the regulatory threshold.

3.1 Production

Firm i of type j produces output y_{ijt} according to a decreasing returns Cobb-Douglas production function:

$$y_{ijt} = z_{ijt}(k_{ijt}^{\alpha} l_{ijt}^{1-\alpha})^{\gamma} \tag{1}$$

The parameter $\gamma < 1$ describes the degree to which returns per input decrease as firms expand, while the parameter α is the typical exponent capturing returns to factor inputs capital k and labour l. z_{ijt} is the total factor productivity of the firm, l_{ijt} is labour input, and k_{ijt} is the firm's total capital stock. The capital stock is the sum of the firm's initial capital stock (k_{0jt}) and any bank loan that the firm obtains when matched to a lender ($\hat{k}_{ijt}$). Firms pay their workers wages and we assume that labour is fixed. As a result, we define the firm's profits π_{ijt} before interest on bank loans in Equation (2):

$$\pi_{ijt}(z_{ijt}, \hat{k}_{ijt}) = y_{ijt} - w_{ijt} l_{ijt} - r_{0ijt} k_{0jt} \tag{2}$$

$\pi_{ijt}(z_{ijt}, \hat{k}_{ijt})$ corresponds to "profit before interest and taxes" in the profit and loss statements in our financial accounts data set. Upon matching, entrepreneurs seek to expand their capital stock, solving the profit maximisation problem:

$$\max_{\hat{k}_{ijt} \geq 0} \pi_{ijt}(z_{it}, \hat{k}_{ijt}) - \hat{k}_{ijt} r_{ijt}(z_{it}, \hat{k}_{ijt}) \tag{3}$$

$\hat{k}_{ijt} = k_{ijt} - k_{0jt}$ is the optimally chosen size of the credit contract following matching. $\pi_{ijt}(z_{ijt}, \hat{k}_{ijt})$ is the firm's profit before interest payments on bank loans, namely output minus the wage bill and any interest payments on other forms of capital (k_{0jt}).

An unmatched firm of type j has a probability p_{jt} of being matched, and hence receiving the surplus from a new match. p_{jt} depends positively on the tightness θ_{jt} in credit market j. The value function of an unmatched type-j entrepreneur i, V_{ijt}^U, is given by Equation (4):

$$V_{ijt}^U = \pi_{0jt} + p_{jt}\beta \int_{\tilde{z}_{jt+1}}^{\infty} \left(V_{ijt+1}^E(z) - V_{ijt+1}^U\right) h(z)dz + \beta V_{ijt+1}^U \tag{4}$$

$\tilde{z}_{jt+1}$ is the productivity cutoff for new lending relationships[6]. $V_{ijt+1}^E(z)$ is the value function of a matched entrepreneur with productivity z, β is the discount factor, π_{0jt} is the profit of an unmatched type-j firm i which produces only with k_{0jt} (i.e. output minus the wage bill and any interest payments on non-bank capital).

Equation (4) says that the value function of an unmatched entrepreneur is equal to the firm's current outside option value π_{0ijt}, the discounted value function of an unmatched entrepreneur in the next period (βV_{ijt+1}^U), plus the expected surplus of a new match in the next period, i.e. the probability of

[6] We show the determination of this cutoff in the Supplemental Appendix in section E.

a match (p_{jt}) times the expected surplus from the match (integral over z).

The value function of a matched firm is given by Equation (5):

$$V_{ijt}^{E}(z_{it}) = \pi_{ijt}^{*} - \hat{k}_{ijt} r_{ijt} + \beta \int_{\tilde{z}_{jt+1}}^{\infty} \left(V_{ijt+1}^{E}(z) - V_{ijt+1}^{U} \right) h(z) dz + \beta V_{ijt+1}^{U} \tag{5}$$

$\pi_{ijt}^{*} = \pi_{ijt}(z_{it}, \hat{k}_{ijt})$ denotes the optimally chosen firm profit before interest payments on loans, i.e. the firm's profit at the optimally chosen loan amount ($\hat{k}_{ijt}$). $r_{ijt} = r_{ijt}(z_{it}, \hat{k}_{ijt})$ denotes the interest rate paid on the optimally chosen bank loan. Equation (5) states that the value function of a matched entrepreneur is the firm's current profit minus interest payments on bank loans, plus the discounted value of the surplus from remaining in the match in the next period, plus the discounted value function of an unmatched entrepreneur in the next period (since the match could be terminated).

3.2 Banks

Banks are active in all J credit markets that promise positive expected returns. Entry into a credit market continues until expected profits from entering are zero. The entry condition in credit market j is given by Equation (6):

$$\frac{\kappa}{q_{jt}} \leq \int_{\tilde{z}_{jt}}^{\infty} \left(V_{ijt}^{B}(z) + \tau_{ijt} \right) h(z) dz - \tau_{ijt} \tag{6}$$

Equation (6) states that banks will enter credit market j as long as the expected cost is smaller than or equal to the expected benefit of doing so. κ is the cost of posting an open credit line in a credit market in every period. q_{jt} is the probability of matching with an entrepreneur in market j. It is a decreasing function of the tightness of the credit market θ_{jt}. $V_{ijt}^{B}(z)$ is the value for the bank of continuing an existing credit relationship with a type-j firm with productivity z. τ_{ijt} is the cost that the bank incurs when terminating a lending relationship. It is time-varying, relationship-specific and type-dependent. Free entry means that Equation (6) holds with equality for all active credit markets at any time t.

The value of continuing an existing credit relationship for the bank is given by Equation (7):

$$V_{ijt}^{B}(z) = \hat{k}_{ijt} \left(r_{ijt} - \rho_{jt} \right) - \kappa_{jt}^{B} + \beta \int_{\tilde{z}_{jt+1}}^{\infty} \left[V_{ijt+1}^{B} + \tau_{ijt+1} \right] h(z) dz - \beta \tau_{ijt+1} \ . \tag{7}$$

κ_{jt}^{B} is the cost of maintaining a credit line in every period in credit market j and ρ_{jt} is the bank's funding cost for lending to type j. This cost encompasses all other specific characteristics observable to the bank and outside observers and is best described as the interest rate accounting for the riskiness of lending to firms of type j in credit market j. Equation (7) states that the value of continuing an existing credit relationship for the bank is equal to the loan amount multiplied by the interest margin, plus the discounted value of the surplus from continuing the match in the next period, plus the discounted value of the termination cost in the next period.

3.3 Solving for the interest rate

We assume that interest rates (r_{ijt}) result from a Nash bargaining process, in which the surplus from the credit relationship is divided according to the firm's bargaining power, $\eta \in [0, 1]$. The surplus split from new and continued credit relationships with a type-j firm with productivity z_{ijt} is given by:

$$\eta \left(V^B_{ijt}(z_{ijt}) + \tau_{ijt}\right) = (1 - \eta) \left(V^E_{ijt}(z_{ijt}) - V^U_{ijt}\right) \tag{8}$$

The resulting interest rate is:

$$r_{ijt} = \eta\rho_{jt} + (1 - \eta)\frac{(\pi^*_{ijt} - \pi_{0jt})}{\hat{k}_{ijt}} - \eta\frac{\kappa}{\hat{k}_{ijt}}\theta_{jt} + \eta\frac{\kappa^B_{jt}}{\hat{k}_{ijt}} - \eta\frac{(\tau_{ijt} + p_{jt}\tau_{kjt} - \beta\tau_{ijt+1})}{\hat{k}_{ijt}} \tag{9}$$

Equation (9) shows how firm characteristics, search frictions, and forbearance incentives determine interest rates through five distinct channels:

(1) Bank funding cost. The first term, $\eta\rho_{jt}$, captures the bank's cost of funds. The interest rate increases in ρ_{jt}, with the effect magnified when the firm has greater bargaining power (higher η). Intuitively, when a firm has strong bargaining power, it captures a larger share of the match surplus. However, the bank still requires compensation for its funding costs. Since a high-η firm claims more of the surplus, it must correspondingly pay a higher rate to ensure the bank's participation constraint is satisfied.

(2) Loan productivity. The second term, $(1 - \eta)\frac{(\pi^*_{ijt} - \pi_{0jt})}{\hat{k}_{ijt}}$, represents the per-unit extra profit before interest payments that the bank loan enables the firm to generate. This term is weighted by the bank's bargaining power $(1-\eta)$. When the bank has stronger bargaining power, it can appropriate a larger fraction of the surplus created by the loan through higher interest charges. Conversely, firms with high bargaining power (η close to 1) retain most of this productivity gain, paying lower interest rates.

(3) Credit market tightness. The third term, $-\eta\frac{\kappa}{\hat{k}_{ijt}}\theta_{jt}$, captures how credit market competition affects interest rates. Higher credit market tightness (θ_{jt}) strengthens the firm's bargaining position and reduces interest rates. When θ_{jt} is high, there are many banks competing for relatively few borrowers. This increases the firm's probability of matching with a lender, p_{jt}, which in turn raises the value of being unmatched, V^U_{ijt}—the firm's outside option in bargaining. A better outside option allows the firm to negotiate more favorable terms. The per-period cost a bank incurs to post and maintain a vacant credit line while searching for a borrower (κ) amplifies this competitive effect. When search costs are high, banks invest more resources to acquire borrowers, making the presence of many competing banks (high θ_{jt}) especially valuable to firms. Firms with higher bargaining power (η close to 1) benefit more from competitive credit markets. Such firms can better exploit their improved outside options to extract lower interest rates.

(4) Credit line maintenance cost. The fourth term, $\eta \frac{\kappa^B_{jt}}{\hat{k}_{ijt}}$, represents the bank's per-period cost of maintaining an active credit line. Banks must charge higher interest rates to cover these operational expenses. The effect increases with η because firms with greater bargaining power claim a larger share of the surplus, yet the bank's fixed costs remain unchanged. To break even, the bank must therefore extract higher interest payments from high-η borrowers.

(5) Forbearance incentives. The fifth term, $-\eta \frac{(\tau_{ijt}+p_{jt}\tau_{kjt}-\beta\tau_{ijt+1})}{\hat{k}_{ijt}}$, captures forbearance incentives through three mechanisms. First, a higher current per-capital-unit termination cost $\left(\frac{\tau_{ijt}}{\hat{k}_{ijt}}\right)$ reduces interest rates. This cost represents the bank's expense of severing an existing lending relationship, which weakens the bank's threat point in bargaining. Firms with higher bargaining power benefit more from this effect, as they can better exploit the bank's reluctance to terminate. We interpret this as the *bargaining effect* of forbearance. Second, interest rates decrease with the expected per-capital-unit termination cost from a potential new match with another lender, $\frac{\tau_{kjt}}{\hat{k}_{ijt}}$ (where $k \neq i$), weighted by the probability of such a match, p_{jt}. When forbearance incentives are firm-specific rather than match-specific, competition among banks for the firm intensifies—each bank anticipates bearing termination costs if it wins the firm's business. This competitive pressure drives rates down further. Again, high-η firms benefit more. We call this the *competition-forbearance interaction effect.* Third, discounted per-capital-unit termination costs in the next period, $\frac{\beta\tau_{ijt+1}}{\hat{k}_{ijt}}$, *increase* current interest rates. Banks anticipate future termination costs and price them into today's lending terms. When τ_{ijt+1} is high, banks demand higher current rates as compensation for the expected costs of eventually severing the relationship. Overall, current forbearance incentives (τ_{ijt} and $p_{jt}\tau_{kjt}$) reduce interest rates, while anticipated future forbearance costs (τ_{ijt+1}) increase them. The net forbearance effect depends on the relative magnitudes of current versus future termination costs.

Proposition 1. *The mean interest rate a firm pays on its bank loans is driven by the following empirically observable variables:*

a) Higher loan productivity (the extra profit the loan enables) increases interest rates, with the effect stronger when the bank has greater bargaining power.

b) Higher credit market tightness (more vacant credit lines per unmatched firm) reduces interest rates by improving the firm's outside option, with the effect amplified by banks' search costs and stronger when the firm has greater bargaining power.

c) A higher bank funding rate increases interest rates, with the effect stronger when the firm has greater bargaining power. Higher credit line maintenance costs also increase rates through this channel.

d) Higher current per-capital-unit termination costs (τ_{ijt} and $p_{jt}\tau_{kjt}$) reduce interest rates by weakening the bank's threat point and intensifying competition among lenders, with the effect stronger when the firm has greater bargaining power.

e) Higher anticipated future per-capital-unit termination costs (τ_{ijt+1}) increase current interest rates as banks price expected severance costs into lending terms.

f) The competition-forbearance interaction effect (item d, second component) is stronger when credit market tightness is high, assuming that termination costs are firm-specific rather than match-specific.

The main predictions of the model are tested in section 4.8. Overall, the results validate the core predictions of Proposition 1 and provide confidence that the model captures salient features of the Japanese lending market.

3.4 Mapping the SME Financing Facilitation Act to the model

The Act created strong forbearance incentives for Japanese banks by implicitly penalizing relationship termination. Banks were expected to accommodate SME requests as best they could, with the Financial Services Agency monitoring compliance and publicizing banks' response rates. These regulatory pressures effectively created a cost of terminating lending relationships with eligible firms, corresponding to positive values of τ_{ijt} in our model. The net effect of the Act on interest rates is theoretically ambiguous. As shown in Equation (9), current termination costs (τ_{ijt}) and the competition-forbearance interaction ($p_{jt}\tau_{kjt}$) both reduce interest rates, while anticipated future termination costs ($\beta\tau_{ijt+1}$) increase them, so the three channels operate in opposing directions. Although the Act officially expired in March 2013, forbearance practices became institutionalized in bank lending behaviour. As the temporary policy transformed into persistent market practice, expectations of future termination costs likely strengthened, potentially attenuating or even reversing the initial downward pressure on interest rates. We examine empirically how the balance among these channels manifested in actual lending outcomes.

4 Data

We draw upon data provided by Tokyo Shoko Research Ltd. (TSR, hereafter), a private market research and credit reporting agency, which surveys companies operating in all industries of the economy. We construct a rich firm-level panel data set for both listed and unlisted Japanese firms, covering all sectors of the economy for the years 2007–2018. We focus on the non-financial corporate sector. Our data set contains identifying information, including industry classification (Japan Standard Industrial Classification 4-digit), location, and a wide range of data from the firms' income statements and balance sheets. Crucially, the data set contains information on the firms' business affiliations, which allows us to determine whether a firm is eligible to request forbearance under the Act.

4.1 Sampling and representativeness

Although the TSR data set does not cover the universe of firms in Japan, it resembles closely the distribution of the Census data in terms of geographic coverage and firm size (see e.g. Hong et al., 2020). However, we can only work with firm-year observations that have all the data items required for the estimation of our difference-in-differences specifications. Table 1 displays the number of annual observations in the full TSR data set and our sample. The overall number of observations for 2007–2018 is about 609 thousand, or just over 5% of the total in TSR. About 96% of observations are for firms with fewer than 250 employees.

Table 1: Number of observations by year

	Sample			TSR panel		
Year	All	SMEs	Large	All	SMEs	Large
2007	34,988	34,099	889	889,224	873,819	15,405
2008	43,685	42,761	924	902,720	884,830	17,890
2009	67,401	65,167	2,234	930,925	912,979	17,946
2010	60,080	57,915	2,165	955,417	935,247	20,170
2011	58,761	56,607	2,154	967,598	945,224	22,374
2012	57,572	55,471	2,101	988,808	964,741	24,067
2013	58,065	55,925	2,140	997,449	975,163	22,286
2014	55,896	53,769	2,127	977,673	956,737	20,936
2015	53,102	50,936	2,166	960,107	939,825	20,282
2016	48,050	45,908	2,142	966,965	946,376	20,589
2017	47,660	45,529	2,131	963,857	942,396	21,461
2018	23,386	21,560	1,826	974,982	952,836	22,146
Total	608,646	585,647	22,999	11,475,725	11,230,173	245,552
% SMEs		96.22%			97.86%	

Notes: Large firms are defined as having 250 employees or more, and SMEs are defined as having strictly fewer than 250 employees. The TSR numbers exclude Finance and insurance (as we focus on non-financial corporations) and other sectors not covered at all by our sample (real estate and goods rental and leasing; education, learning support; medical, health care and welfare; compound services; and services n.e.c.). Source: TSR.

Although our sample only represents a small fraction of the TSR data set, it closely resembles its distribution in terms of geographic coverage, industry coverage, and firm size (See Supplemental Appendix B). To ensure that our counterfactual exercises are representative of the population of Japanese firms, we nevertheless compute sampling weights. We construct sampling cells defined by employment bands (strictly fewer than 250 employees, 250 employees or more), four-digit industries, and prefectures. The weight assigned to each firm is the inverse of its cell's sampling probability, defined as the ratio of total cell employment in the population (full TSR) to total cell employment in the sample.[7]

[7] In robustness checks, we use more granular employment cells. Specifically, we construct sampling cells defined by employment bands (fewer than 4 employees, 5–9, 10–19, 20–29, 30–49, 50–99, 100–299, 300–999,

4.2 Interest rates on debt obligations

The dependent variable in our DiD equations, $\ln(r_{it})$, is the natural logarithm of the firm's average interest rate on all debt obligations, defined as the ratio of interest payments on interest-bearing debt obligations and their outstanding amount. While our focus is on bank loans, interest-bearing debt obligations also encompass bonds and accounts payable. Table 2 presents the sample average share of different types of debt in total debt. Overall, bank debt represents 65.5% of total debt. The share of bank debt decreases slightly with firm size but remains high for all firm sizes. Firms eligible for forbearance under the Act have a slightly higher share of bank debt than non-eligible firms (65.9% vs. 63.6%), consistent with SME classification. Trade credit is the second most important type of debt funding, at 32.8% overall. Bond debt plays a minor role, though it is notably higher for large firms and non-eligible firms. These observations are in line with Japan being a bank-based, rather than a capital market-oriented, economy (see e.g. Antoniou et al., 2008). Furthermore, short-term bank loans account for 94.4% of total bank loans, and short-term debt represents 95% of total debt. The predominance of short-term maturities is especially pronounced among small and eligible firms, where short-term bank loans exceed 95% of total loans. Large and non-eligible firms exhibit somewhat lower short-term shares (around 87–89%). The prevalence of short-term debt implies that interest rates are renegotiated frequently, making them responsive to policy changes such as the Act.[8] In order to isolate the effect of the Act on loan interest rates, the set of control variables in our DiD estimations includes firm i's share of bonds and accounts payable (trade credit) in total debt.

Table 2: Debt composition (% of total)

	Bank loans	Bonds	Trade credit	ST bank loans	ST debt
All firms	65.47	1.78	32.75	94.37	94.97
Small firms	67.41	0.23	32.37	96.76	97.98
Medium-sized firms	64.57	1.32	34.11	94.99	95.77
Large firms	64.68	5.23	30.10	89.24	88.51
Non-eligible firms	63.56	4.95	31.49	86.79	87.43
Eligible firms	65.86	1.14	33.00	95.90	96.50

Notes: Large firms are defined as having 250 employees or more, and SMEs are defined as having strictly fewer than 250 employees. Eligible firms are firms that qualify for support under the SME Financing Facilitation Act. All numbers are in % of total debt, except ST bank loans which are in % of total loans. Source: TSR.

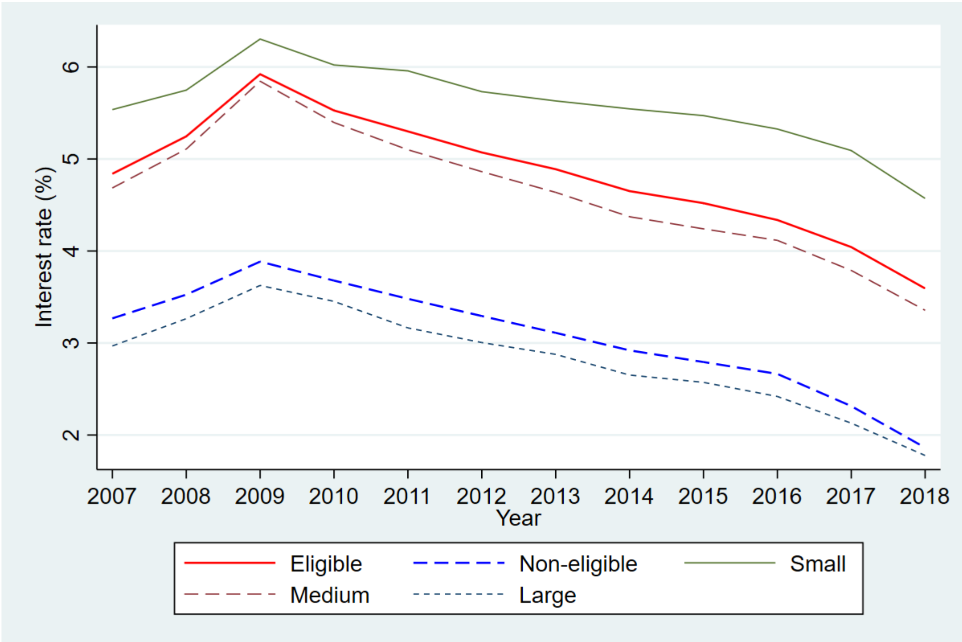
Figure 2 shows the evolution of average interest rates in our sample. The average interest rate paid by firms that are eligible for forbearance under the Act is systematically higher than that paid by non-eligible firms, consistent with the former being SMEs. The interest rates paid by eligible and

1000–1999, more than 2000 employees), four-digit industries, and prefectures. This robustness check is not reported.

[8] Our sample averages are broadly consistent with aggregate statistics. In 2014, the Financial Statements Statistics of Corporations by Industry reported long-term borrowing of 11,164,802 million yen and short-term borrowing of 58,047,176 million yen, implying a short-term share of approximately 84%—close to the ratio observed for large firms in our sample. See `https://www.e-stat.go.jp/dbview?sid=0003386178`.

non-eligible firms display very similar dynamics: an increase through 2009, followed by a decline from 2010 onwards.

Figure 2: Average interest rate on debt obligations (%)



Notes: Average interest rate on debt obligations in the sample. Size categories are those of the Short-Term Economic Survey of Enterprises in Japan of the Bank of Japan. Source: TSR.

4.3 Treatment intensity

4.3.1 Constructing the exposure measure

We build a time-varying measure of the exposure of firm i to treatment under the Act in year t, Treatment Intensity$_{it}$. It is the product of a time-invariant dummy variable for treatment eligibility and a time-varying probability of firm i receiving forbearance of any kind, conditional on eligibility. Since almost all requests for loan restructurings were accepted, this probability of treatment is a good proxy for the probability that firm i applied for support. To identify the treatment effects, we exploit the fact that the lender's decision to offer forbearance conditional on the firm requesting it is plausibly exogenous. The dummy variable for treatment eligibility is equal to one when the firm was considered eligible for loan forbearance under the Act in 2009. The eligibility criteria are laid out in Supplemental Appendix A. The probability of firm i receiving treatment conditional on eligibility is calculated using the results of a Probit estimation on a pooled panel constructed from 3,298 eligible firms surveyed by the Research Institute of Economy, Trade and Industry (RIETI), yielding 15,098 firm-year observations.[9] The survey asked qualifying SMEs whether they had received debt forbearance from

[9] In October 2014, RIETI conducted the "Survey on the Aftermath of the SME Financing Facilitation Act" (Kinyuenkatsukaho Shuryogo ni okeru Kinyu Jittai Chosa). For papers discussing this survey, see Uesugi et al. (2015) (in Japanese) and Ono and Yasuda (2017).

their lenders since December 2009, and crucially, it records the year in which each firm first received forbearance.

While the RIETI survey records the year of treatment for surveyed firms, treatment timing is not observed in the TSR database that constitutes our main estimation sample. We address this challenge by constructing a time-varying treatment intensity measure that accounts for the aggregate pattern of treatment take-up over time, while using only pre-determined firm characteristics to capture cross-sectional variation in exposure. Our approach exploits the treatment timing information available in the RIETI survey to estimate a pooled Probit model on a panel that includes each treated firm observed in the year it first received forbearance, alongside never-treated firms observed in every year from 2009 to 2014. The specification is:

$$Prob(Forbearance_{it} = 1) = \Phi\left(\sum_{c} \gamma_c \cdot X^c_{i,2009} + \alpha_t + D_j\right) \tag{10}$$

The dependent variable $Forbearance_{it}$ is a dummy equal to 1 if firm i reported receiving forbearance in year t. The firm-level control variables $X^c_{i,2009}$ are taken from the TSR database and are fixed at their financial year 2008–2009 values, prior to the introduction of the Act. They include the firm's leverage, credit score, return on assets, the natural logarithm of sales, the number of employees, and the firm's age. D_j are industry fixed effects. α_t are year fixed effects with 2009 as the reference year. In the pooled estimation, treated firms contribute a single observation in their treatment year with $Forbearance_{it} = 1$, while never-treated firms contribute observations in every year from 2009 to 2014 with $Forbearance_{it} = 0$. The year fixed effects α_t capture the aggregate time profile of treatment take-up, since the relative share of treated observations varies across years in proportion to the actual inflow of new treatments.

The results are in Panel A of Table 3. SMEs with higher leverage are more likely to receive forbearance while those with higher credit scores are less likely, consistent with forbearance targeting firms with greater debt overhang and financial distress. The coefficients on ROA, firm age, and the number of employees are not statistically significant, while the natural logarithm of sales enters with a negative and significant coefficient, suggesting that smaller firms (by revenue) were more likely to receive forbearance. The year fixed effects reveal a distinct temporal pattern in treatment take-up. The coefficient on year 2010 is the largest and highly significant, indicating that the inflow of newly treated firms peaked in the first full year of the Act's implementation. The coefficients decline monotonically thereafter, with the 2014 coefficient turning slightly negative, consistent with the Act's expiration in March 2013 and a winding-down of new forbearance arrangements.

Table 3: Probit model of forbearance receipt and temporal stability test

Panel A: Probit model (base specification)			
Firm characteristics		Year fixed effects	
Leverage	0.365***	2010	0.431***
	(0.057)		(0.051)
Credit score	-0.036***	2011	0.133**
	(0.003)		(0.056)
ROA	0.086	2012	0.085
	(0.126)		(0.057)
Ln(sales)	-0.057***	2013	0.045
	(0.016)		(0.058)
Employees	-0.000	2014	-0.046
	(0.000)		(0.061)
Firm age	0.000	Constant	0.898***
	(0.001)		(0.243)
Industry FE		Yes	
Pseudo R^2		0.080	
Observations		15,098	
Panel B: Wald test for temporal stability			
H_0: Firm characteristic × Year coefficients jointly zero			
χ^2 statistic		32.59	
Degrees of freedom		30	
p-value		0.34	

Notes: Panel A reports estimates from the Probit model used to construct the treatment intensity measure (Equation 10). The dependent variable is Forbearance_{it}, a dummy equal to 1 if the firm reported having received forbearance. The sample is a pooled panel constructed from 3,298 eligible firms in the RIETI survey, with treated firms entering in their treatment year and never-treated firms entering in all years from 2009 to 2014. All firm controls are taken from the TSR database and fixed at their financial year 2008–2009 values. The leverage ratio is defined as a firm's debt liabilities divided by its total assets. It is a proxy for debt overhang. The firm's credit score is the TSR score (1–100 points). Return on assets is defined as operating income divided by total assets. The firm's score and ROA are proxies for a firm's net present value. The natural logarithm of gross annual sales and the number of employees are measures of size. It is important to control for firm size, since the transaction costs involved in debt renegotiations may be higher for smaller firms. Finally, firm age is measured as the number of years since a firm was established. Year fixed effects are relative to the reference year 2009. Panel B reports the Wald test from an augmented specification that adds interactions between all six firm controls and year fixed effects (30 additional parameters). The test evaluates the joint null hypothesis that all interaction coefficients equal zero, i.e., that the mapping from 2009 firm characteristics to forbearance receipt is stable across treatment cohorts. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The structure of the Probit model in Equation (10) implies a natural decomposition of treatment intensity into a time-invariant firm-specific component and a time-varying aggregate component. Specifically, for a firm i observed in year t, the treatment intensity is:

$$Treatment\ Intensity_{it} = Eligibility_{i,2009} \times \Phi\left(\hat{\boldsymbol{x}}_i'\hat{\boldsymbol{\gamma}} + \hat{\alpha}_t\right) \tag{11}$$

where $\hat{\boldsymbol{x}}_i'\hat{\boldsymbol{\gamma}} = \sum_c \hat{\gamma}_c \cdot X^c_{i,2009} + \hat{D}_j$ is the time-invariant component of the Probit linear index based on firm i's 2009 characteristics and industry, and $\hat{\alpha}_t$ is a year-specific intercept. For the pre-treatment period ($t \leq 2009$), we set $\hat{\alpha}_t$ equal to the base constant $\hat{\alpha}_0$ (i.e., no year fixed effect), reflecting the baseline probability of forbearance receipt prior to the Act's implementation. For the treatment period (2010–2014), $\hat{\alpha}_t = \hat{\alpha}_0 + \hat{\theta}_t$, where $\hat{\theta}_t$ is the estimated year fixed effect from Table 3, capturing the aggregate time profile of treatment take-up. For years beyond the RIETI estimation sample ($t \geq 2015$), $\hat{\alpha}_t$ is held at its 2014 value.

Despite the time variation introduced by the year fixed effects, the key identifying variation in our difference-in-differences framework continues to come from the cross-sectional differences in treatment intensity driven by pre-determined firm characteristics measured in 2009. The year fixed effects serve only to recalibrate the *level* of treatment propensity in each year, ensuring that the exposure measure reflects the realistic probability of a firm having received treatment by period t given the observed aggregate pattern of take-up. There is substantial heterogeneity in treatment intensity in 2009: mean treatment intensity ranges from 0.15 for small firms to 0.02 for large firms, with the cross-sectional variation driven primarily by the small-firm segment[10].

4.3.2 Validating the exposure measure

A potential concern with our empirical strategy is that we rely in part on a time-invariant measure of treatment intensity constructed from pre-treatment data. This approach is standard when treatment timing and intensity are not directly observed[11]. Callaway et al. (2024) provide the foundational framework for DiD designs with continuous treatments, noting that variation in dose permits evaluation of treatments for which binary DiD is infeasible—as in our setting where treatment is unobserved and varies in intensity across firms. The validity of the approach rests on the assumption that the exposure measure captures stable treatment propensity rather than transitory factors.

Specifically, our treatment intensity measure assumes that the mapping from pre-determined firm characteristics to forbearance receipt is stable over time. While aggregate take-up varies across years (captured by the year fixed effects $\hat{\alpha}_t$), the relative ranking of firms by treatment propensity, governed by the coefficients $\boldsymbol{\gamma}$, should not vary systematically across cohorts. If this assumption fails—for example, if leverage became a stronger predictor of forbearance in later years—the time-invariant firm-specific component would become an increasingly poor characterisation of the cross-sectional distribution of treatment. To test this, we augment the base Probit model from Equation (10) by including interactions between all 2009-fixed firm controls and year dummies:

$$\text{Prob}(\text{Forbearance}_{it} = 1) = \Phi\left(\sum_c \gamma_c \cdot X^c_{i,2009} + \alpha_t + D_j + \sum_t \sum_c \delta_{tc} \cdot (X^c_{i,2009} \times Y_t)\right) \qquad (12)$$

where δ_{tc} represents year-specific *deviations* from the average effects γ_c for each firm characteristic c in year t. The interaction terms $(X^c_{i,2009} \times Y_t)$ capture potential time variation in how pre-determined firm characteristics predict forbearance receipt across treatment cohorts. We use a Wald test to assess

[10] See Table 1 in Supplemental Appendix C for details.

[11] See, e.g., Card & Krueger (1994); Duflo (2001); Finkelstein (2007); Nunn & Qian (2014).

the joint null hypothesis that all interaction coefficients are zero:

$$H_0 : \delta_{tc} = 0 \quad \text{for all } t, c$$

Under the null, the mapping from 2009 firm characteristics to treatment probability is stable across treatment cohorts, with only the aggregate level of take-up (captured by α_t) varying over time. Panel B of Table 3 reports the results of the Wald test. The Wald test statistic is $\chi^2(30) = 32.59$ with a p-value of 0.34. We fail to reject the null hypothesis at all conventional significance levels, indicating no evidence that the effects of 2009 firm characteristics on forbearance receipt vary systematically across treatment cohorts. Inspection of the individual interaction terms (not reported) confirms this: none of the 30 interaction coefficients is individually significant at the 5% level.

4.4 Average product of borrowed capital

We define the average product of borrowed capital as the difference between the profit before interest payments which the firm makes using its entire capital stock (π^*_{ijt}) and its profit before interest payments when it only employs the part of the capital stock that is not financed by banks (π_{i0jt}), divided by the capital financed by banks ($\hat{k}_{ijt}$):

$$APBK_{it} = \frac{(\pi^*_{ijt} - \pi_{0ijt})}{\hat{k}_{ijt}} = \frac{(z_{it}(k^{\alpha}_{it} l^{1-\alpha}_{it})^{\gamma}) - (z_{it}(k^{\alpha}_{it,0} l^{1-\alpha}_{it})^{\gamma})}{\hat{k}_{ijt}} \tag{13}$$

In other words, $APBK_{it}$ measures the average profit (in ¥) before interest payments generated by an extra unit of borrowed capital. Equation (13) assumes that labour input does not adjust in the short run so that the wage bill terms cancel out.[12] In the data, the capital stock not financed by banks ($k_{it,0}$) is calculated as the sum of all assets minus bank loans on the firm's balance sheet. It encompasses shareholder funds and non-bank debt (mainly bonds and trade credit). The parameters of the production function $y = z_i(l^{1-\alpha}k^{\alpha})^{\gamma}$ are estimated. Firm productivity, z, and the parameter α are estimated using the method of Wooldridge (2009). We estimate $\alpha = 0.283$ and $\gamma = 0.963$.[13]

4.5 Credit scores

TSR credit scores are recorded as integers between 0 and 100. They are the sum of sub-scores on four aspects of firm performance: management quality (0–20 pts), growth (0–25 pts), stability (0–45 pts), and transparency and reputation (0–10 pts).[14] The TSR credit scores are positively correlated with actual defaults (Miyakawa, Miyauchi, and Perez, 2017). TSR classifies firms into five groups of

[12] We also estimate our model assuming flexible labour input with similar results in section F.5.

[13] Supplemental Appendix C.2 reports the full production function estimates. Supplemental Appendix C.3 presents some descriptive statistics.

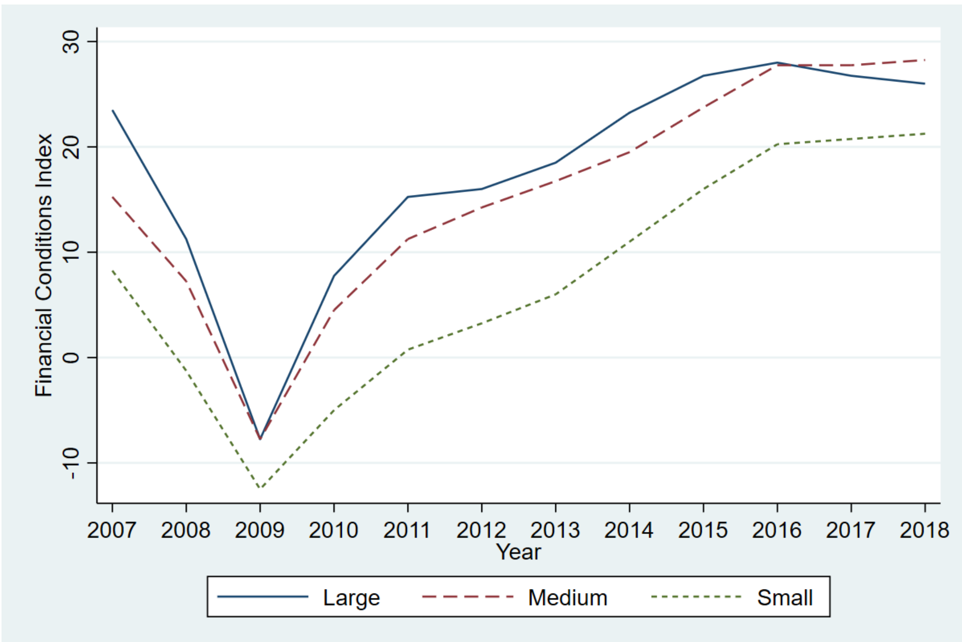
[14] An explanation of TSR credit scores is found at: `http://www.tsrnet.co.jp/guide/knowledge/glossary/ha_05.html` (in Japanese only).

creditworthiness according to their credit scores[15]. The average credit score in our sample is 49.8 over 2007–2018. The scores of eligible firms are systematically lower (at 48.8 on average) than those of non-eligible firms (at 55.2 on average)–consistent with SMEs being riskier borrowers. However, the time series pattern is similar for eligible and non-eligible firms.[16]

4.6 Credit market tightness

We use the Short-Term Economic Survey of Enterprises in Japan (Tankan Survey)[17] to capture time-varying credit market tightness by size class.[18] To capture credit market tightness, we use the indices measuring the lending attitude of banks as reported by private enterprises in the survey. A more positive index means that credit is being offered more easily to firms, which we interpret as higher credit market tightness. Figure 3 presents the indices by size category. There is a sharp deterioration in lending conditions between 2007 and 2009, followed by a sharp recovery until 2016 when the indices stabilise. Small firms face systematically less accommodative lending conditions. Since lending attitudes relaxed markedly after the Act's implementation—likely driven by multiple factors including the Act itself—we control for credit market tightness in our DiD regressions.

Figure 3: Credit market tightness, by size category



Notes: Proxy for credit market tightness by size category. Credit market tightness is defined as the supply of credit lines over the demand for credit lines. Source: Short-Term Economic Survey of Enterprises in Japan.

[15]Scores ≤ 29: "keikai" (caution); 30–49: "ichio keikai" (somewhat caution); 50–64: "tasho chui" (attention); 65–79: "bunan" (safe); 80–100: "keikai fuyo" (no risk).

[16]Supplemental Appendix C.4 presents some descriptive statistics.

[17]See `https://www.boj.or.jp/en/statistics/tk/index.htm`

[18]Size is defined by the firm's amount of capital and divided into three categories. Large enterprises have capital of 1 billion yen or more, medium-sized from 100 million to less than 1 billion, and small from 20 million to less than 100 million. We classify all enterprises with a capital below 100 million yen as small firms.

4.7 Zombie firms

Zombie firms are generally defined as non-viable firms which, in the absence of financial support, would default on their debt obligations. This definition hinges on two criteria: the firm (i) is in financial distress and (ii) receives financial support that keeps it alive (see, e.g., Acharya et al., 2022; Álvarez et al., 2023). In this paper, we construct a zombie dummy variable inspired by the definition of Fukuda and Nakamura (2013) because it allows us to combine the two criteria of subsidized credit and financial distress. Specifically, we combine the subsidized credit criterion of Caballero et al. (2008) (henceforth CHK (2008)) with measures of financial distress (low operating income and high leverage).

Following CHK (2008), we first compare each firm's interest payments to a benchmark level that is calculated assuming the lowest possible rate of interest for a healthy borrower (lower bound interest rate). If a firm's interest rate is below this lower bound, it receives subsidized credit.[19] We classify a firm as a zombie in period t if (i) it receives subsidized credit and its ROA is below the lower bound interest rate in period t[20], or (ii) it does not receive subsidized credit in period t, but its ROA is below the lower bound interest rate in period t and its leverage (total debt over total assets) is above the sample's 90th percentile in period t. Figure 4 displays zombie proportions over time. The proportion of zombies among eligible firms is on average about 17%, while it is about 14% for non-eligible firms. Both groups show a similar pattern: zombie proportions declined through roughly 2014—by about 5.2pp for eligible firms and by about 1.2pp for non-eligible ones—before rising again through 2018. Over the full sample period, the eligible zombie share fell by about 1.3pp, while the non-eligible share increased by about 2.1pp. Firms classified as zombies in 2009 have a markedly higher average treatment intensity than non-zombies—with an average probability of being treated conditional on eligibility (based solely on 2009 characteristics) of around 14% for zombies versus 7% for non-zombies.

For comparison, our estimations also use a definition based on those of Schivardi et al. (2022) and Faria-e-Castro et al. (2024). Both measures are single-tiered in the sense that they only capture financial distress, while ignoring subsidized interest rates.[21] We classify a firm as a zombie under this alternative definition if its ROA is below the lower bound interest rate and leverage exceeds the 90th percentile.[22]

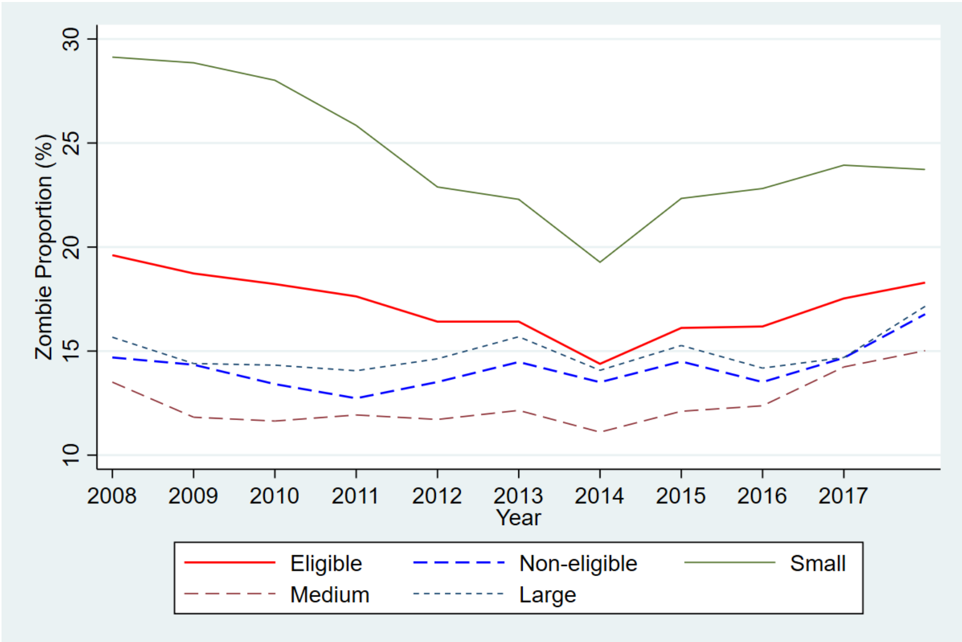
[19] The calculation of this lower bound is described in Supplemental Appendix D.

[20] We use contemporaneous values for ROA, the lower bound interest rate, and leverage in order to avoid losing pre-treatment observations, which is crucial for assessing the parallel trends assumption in our DiD framework.

[21] Schivardi et al. (2022) classify a firm as a zombie in any given year if its return on assets (ROA), defined as the three-year moving average of earnings before interest and taxes (EBIT) over total assets, is below the cost of capital for the safest borrowers, defined as the three-year moving average prime rate, and its leverage exceeds 40%. Faria-e-Castro et al. (2024) classify a firm as a zombie if it has a leverage ratio above the 90th percentile and ROA below the 10th percentile for that year.

[22] As with our two-tiered measure, we use contemporaneous rather than lagged values in order to preserve pre-treatment observations for our DiD analysis.

Figure 4: Proportion of zombie firms over time (%)



Notes: Percentage of zombie firms over time. Zombies are defined as described above, with a two-tiered definition similar to that of FN (2013). Source: TSR.

4.8 Testing the predictions of the model

In this section, we examine whether the conditional correlations implied by our theoretical model are borne out in the data. Proposition 1 characterizes the equilibrium interest rate as a function of loan productivity, credit market tightness, and bank funding costs. While forbearance incentives—the focus of our policy evaluation—are unobservable and are studied through the quasi-natural experiment provided by the Act, the other determinants of interest rates can be tested directly. Table 4 presents estimates from panel regressions of the form:

$$r_{it} = \alpha + \beta_1 \text{APBK}_{it} + \beta_2 \text{Tightness}_{it} + \mathbf{X}'_{it}\gamma + \mathbf{Z}'_{jt-1}\boldsymbol{\delta} + \alpha_i + \delta_{jt} + \varepsilon_{ijt} \quad (14)$$

where r_{it} is the average interest rate (in percent) paid by firm i on its debt obligations in year t, and α_i denotes firm fixed effects. Depending on the specification, we include year fixed effects or bank×year fixed effects (δ_{jt}), firm-level debt structure controls ($\mathbf{X}_{it}$), and lagged bank-level controls ($\mathbf{Z}_{jt-1}$) for the firm's main lender. Standard errors are clustered at the firm level throughout. The results are in Table 4.

Loan productivity. The average product of borrowed capital (APBK), denoted π in the model, captures the productivity of the marginal loan. Proposition 1 predicts that higher loan productivity increases the equilibrium interest rate, as it raises the surplus from the lending relationship that the bank can extract through bargaining. Across all five specifications, the coefficient on APBK is positive and statistically significant at the 1% level, consistent with this prediction.

Credit market tightness. Higher credit market tightness (θ) implies greater competition among lenders, reducing the interest rate the bank can charge. Consistent with this prediction, the coefficient on credit market tightness is negative and statistically significant at the 1% level across all specifications.

Table 4: Testing Proposition 1: Determinants of interest rates

	(1)	(2)	(3)	(4)	(5)
APBK	0.5163***	0.5116***	0.7361***	0.5279***	0.5222***
	(0.0697)	(0.0693)	(0.0928)	(0.0762)	(0.0756)
Credit market tightness	-0.0531***	-0.0515***	-0.0616***	-0.0602***	-0.0591***
	(0.0037)	(0.0037)	(0.0048)	(0.0044)	(0.0044)
Bonds (% of total debt)		-0.0407***			-0.0401***
		(0.0018)			(0.0019)
Trade credit (% of total debt)		0.0015*			0.0004
		(0.0008)			(0.0009)
Lagged NPL ratio			0.0236**		
			(0.0093)		
Lagged capital ratio			0.0036		
			(0.0070)		
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Bank×Year FE	No	No	No	Yes	Yes
Debt structure	No	Yes	No	No	Yes
Bank controls	No	No	Yes	Absorbed	Absorbed
Observations	606,455	606,455	361,615	547,060	547,060
Adj. R^2	0.5975	0.5986	0.6198	0.6154	0.6165

Notes: This table reports estimates from panel regressions of equation (14). The dependent variable is the average interest rate paid by firm i on its debt obligations in year t, expressed in percentage points. APBK (average product of borrowed capital) is the average profit before interest payments generated by an extra unit of borrowed capital, as defined in equation (13), capturing the productivity of the marginal loan (π in the model). Credit market tightness is the Short-Term Economic Survey of Enterprises in Japan's lending attitude index for the firm's size category (θ in the model), where higher values indicate more accommodative lending conditions. Bonds (% of total debt) and Trade credit (% of total debt) are the shares of bonds and trade credit (accounts payable) in total debt, respectively. Lagged NPL ratio and Lagged capital ratio are the one-year lagged ratios of non-performing loans to total loans and of equity capital to total assets, respectively, of the firm's main bank. All specifications include firm fixed effects. Columns (1)–(3) include year fixed effects; Columns (4)–(5) replace these with bank×year fixed effects, which subsume year fixed effects and all time-varying bank-level characteristics. The smaller sample in Column (3) reflects the availability of bank balance sheet data; the reduction in Columns (4)–(5) reflects the requirement of non-missing bank identifiers. Standard errors clustered at the firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Debt structure controls. Columns (2) and (5) add controls for the firm's debt composition: the share of bonds and the share of trade credit in total debt. These controls are important because our dependent variable measures the average interest rate on all debt, not solely bank loans. Firms with greater access to bond markets may face lower rates due to improved outside options, which is confirmed by the negative and significant coefficient on the bond share. The coefficient on the trade credit share is positive and marginally significant in Column (2), but statistically indistinguishable from zero in Column (5) once bank×year fixed effects are included.

Bank funding costs. Proposition 1 predicts that higher bank funding costs are passed through to borrowers. Column (3) tests this by including the lagged NPL ratio and capital ratio of the firm's main lender. The NPL ratio proxies for the bank's cost of funds: banks with higher non-performing loan ratios face greater funding costs and regulatory pressure. Consistent with the model, the coefficient on the lagged NPL ratio is positive and statistically significant at the 5% level. The coefficient on the lagged capital ratio is not statistically significant. Columns (4) and (5) replace year fixed effects with bank×year fixed effects, which absorb all time-varying bank-level factors including funding costs, risk appetite, and lending standards. This demanding specification identifies the effects of APBK and credit market tightness purely from within-bank-year variation. The coefficients of interest remain stable in magnitude and highly significant, indicating that our results are not driven by unobserved bank-level confounders.

Overall, the results in Table 4 validate the core predictions of Proposition 1 and provide confidence that the model captures salient features of the Japanese lending market.

5 Evaluation of the SME Financing Facilitation Act

5.1 Impact on interest rates

5.1.1 Difference-in-differences analysis

According to Equation (9), the determinants of loan interest rates are the average product of borrowed capital, the lender's funding cost, credit market tightness, and forbearance incentives. To capture the latter, we construct a treatment variable $D_t\text{Treatment Intensity}_{it}$ where D_t represents year dummies and $\text{Treatment Intensity}_{it}$ is a time-varying measure of the exposure of firm i to treatment under the Act at time t (see Section 4.3). We estimate the following general specification:

$$\ln(r_{it}) = \alpha + \sum_{t\neq 2009} \beta_t D_t \text{Treatment Intensity}_{it} + \Gamma X_{it} + \sum_{c} \gamma_c \text{Post}_t X_{it}^c + \Lambda Z_{bt} + f_{(\cdot)} + \epsilon_{it} \qquad (15)$$

where $\ln(r_{it})$ is the natural logarithm of firm i's average interest rate on debt obligations at time t, and the sum runs over all years from 2007 to 2018 excluding the baseline year 2009. This log-linear specification implies an underlying multiplicative model for r_{it}. The coefficients β_t represent the percentage difference in interest rates between treated and non-treated firms in each year (see Appendix A). We explicitly choose a multiplicative model because a level specification in r_{it} would constrain the interest rates of all firms to change by the same absolute amount of percentage points each year, which would be inappropriate given the considerable variation in interest rates across firms.[23]

[23]See, e.g., Finkelstein (2007) for a similar multiplicative model.

X_{it} is a vector of control variables, including the firm's average product of borrowed capital and credit market tightness for the size segment to which firm i belongs. It also includes firm i's share of bonds and accounts payable (trade credit) in total debt. These latter two variables are included because r_{it} measures the firm's average interest rate on all debt obligations and we aim to isolate the impact of the Act on loan interest rates. X_{it}^c includes the characteristics of firm i which affect a firm's probability of treatment: leverage, credit score, return on assets, the natural logarithm of sales, the number of employees, the firm's age, and industry dummies (see Section 4.3). The interaction term $Post_t X_{it}^c$ serves two purposes. First, it strengthens our identifying assumption by imposing parallel trends *conditional* on these characteristics rather than unconditionally. This is a more plausible assumption: we require that firms with similar current financial profiles but different pre-treatment exposure would have experienced parallel interest rate trajectories absent the Act. Second, it addresses confounding variables: these characteristics independently determine interest rates through non-treatment channels (e.g., leverage affects credit risk), so omitting them would conflate treatment effects with the mechanical influence of compositional changes in firm characteristics.[24] Z_{bt} denotes bank-level controls, and $f_{(\cdot)}$ represents the fixed effects structure, which varies across specifications as described below. Standard errors are clustered at the firm level throughout. The inclusion of pre-treatment years (2007-2008) allows us to directly assess the parallel trends assumption: if this assumption holds, the β_t coefficients for pre-treatment years should be statistically indistinguishable from zero. We report joint tests of the null hypothesis $\beta_{2007} = \beta_{2008} = 0$ for each specification.

Note that the standalone D_t are absorbed by the year fixed effects. Since Treatment Intensity$_{it}$ is time-varying, it could in principle enter as a standalone control. However, we deliberately omit it for two reasons. First, in the canonical continuous DiD design, the treatment intensity measure is time-invariant and thus absorbed by unit fixed effects. Our measure introduces time variation solely through the common year intercepts $\hat{\alpha}_t$ in the Probit index (Equation 11), which capture aggregate treatment take-up under the Act. This time variation is itself a consequence of the policy, not a confounding factor—the whole point of incorporating $\hat{\alpha}_t$ into our measure is to ensure that estimated treatment effects reflect the actual aggregate pattern of forbearance take-up over time. Including the standalone Treatment Intensity$_{it}$ would therefore be analogous to conditioning on a mediator: the Act caused forbearance take-up to surge in 2010 and decline thereafter, and this pattern is encoded in the $\hat{\alpha}_t$ driving the time variation in Treatment Intensity$_{it}$. Conditioning on the standalone would partial out this policy-induced variation, so that the interaction coefficients β_t would no longer capture the total differential effect of exposure in year t relative to the 2009 baseline, but only the residual year-specific effect *over and above* the aggregate take-up profile—a less interpretable estimand than the total treatment effect we seek for our counterfactual calculations. Second, after absorbing firm fixed effects (which capture the time-invariant component $\hat{\boldsymbol{x}}_i' \hat{\boldsymbol{\gamma}}$) and year fixed effects (which capture the common $\hat{\alpha}_t$ shifts), the only residual variation in the standalone stems from the nonlinearity of $\Phi(\cdot)$, leaving it near-collinear with the existing fixed effects structure.

[24]See for example Tripathy (2020) for a similar approach.

The results are in Table 5. We estimate five specifications that progressively address potential threats to identification. The baseline specification in Column (1) includes firm fixed effects (f_i) and year fixed effects (f_t), with no bank-level controls ($Z_{bt} = \emptyset$). The firm fixed effects control for time-invariant firm characteristics, while year fixed effects absorb aggregate time shocks common to all firms, including the Bank of Japan's policy rates which affect lenders' cost of funds. Column (2) replaces year fixed effects with industry-by-year fixed effects (f_{jt}) at the 4-digit industry level. This is a demanding specification that absorbs all time-varying industry-level shocks, including industry-specific business cycles, sectoral credit conditions, and differential exposure to macroeconomic developments. Since the industry dummies are among the determinants of treatment in X_{it}^c, the industry-by-Post interaction terms in $\sum_c \gamma_c \text{Post}_t X_{it}^c$ are absorbed by the industry-by-year fixed effects while the remaining non-industry firm characteristics continue to enter as Post interactions. The specification of Column (3) augments the baseline by adding the non-performing loan ratio and capital ratio of firm i's main bank b at time t as controls ($Z_{bt} = \{\text{NPL}_{bt}, \text{Capital}_{bt}\}$).[25] These variables control for bank financial health, which affect the cost of funds and banks' willingness to offer forbearance (see Supplemental Appendix H). The specification of Column (4) replaces year fixed effects with bank-by-year fixed effects (f_{bt}) while retaining firm fixed effects. Bank-by-year fixed effects absorb all time-varying bank-level heterogeneity, including changes in bank funding costs, risk appetite, and balance sheet conditions. The identifying variation comes from comparing firms with different treatment intensities that share the same main bank in the same year. Finally, the specification of Column (5) replaces firm fixed effects with bank-by-firm fixed effects (f_{ib}) while retaining year fixed effects. Bank-firm fixed effects control for time-invariant relationship-specific heterogeneity, such as the strength of the lending relationship, or soft information that the bank has accumulated about the firm.

Across all five specifications, we find large, statistically significant negative effects of treatment intensity on interest rates, with the effects gradually diminishing over time. In 2010, immediately following the Act's implementation, the treatment effect ranges from -0.80 to -0.91. Since Treatment Intensity$_{it}$ is a probability bounded between zero and one, a coefficient of -0.91 implies that a treated firm (Treatment Intensity$_{it}$ = 1) has an interest rate approximately 60 percent lower than an otherwise identical untreated firm (Treatment Intensity$_{it}$ = 0) (since $\exp(-0.908) - 1 \approx -0.60$). The effect remains strongly negative through 2016, though it diminishes in magnitude over time: by 2016, the coefficients range from -0.31 to -0.40. By 2017, the treatment effects are no longer statistically significant in most specifications. By 2018 the coefficients turn positive and marginally significant in Columns (1) and (5), with estimates of 0.21 and 0.27 respectively, while the remaining specifications show coefficients close to zero.

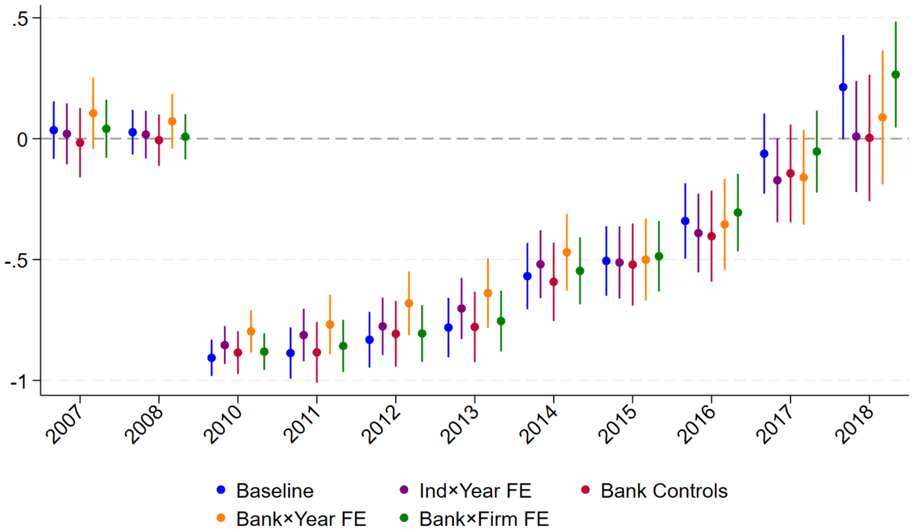
[25]Note that we use the bank-level variables at time t to preserve sample size, but the results are robust to using $t-1$ variables.

Table 5: Effect of the SME Financing Facilitation Act on interest rates

	(1) Baseline	(2) Ind×Year FE	(3) Bank Controls	(4) Bank×Year FE	(5) Bank×Firm FE
Pre-treatment coefficients					
2007 × Treatment Intensity	0.0354	0.0200	-0.0169	0.105	0.0408
	(0.0607)	(0.0643)	(0.0730)	(0.0755)	(0.0613)
2008 × Treatment Intensity	0.0268	0.0170	-0.00635	0.0721	0.00794
	(0.0471)	(0.0502)	(0.0543)	(0.0578)	(0.0479)
Post-treatment coefficients					
2010 × Treatment Intensity	-0.907***	-0.854***	-0.885***	-0.797***	-0.881***
	(0.0384)	(0.0400)	(0.0454)	(0.0448)	(0.0388)
2011 × Treatment Intensity	-0.887***	-0.812***	-0.884***	-0.769***	-0.858***
	(0.0540)	(0.0553)	(0.0640)	(0.0631)	(0.0550)
2012 × Treatment Intensity	-0.832***	-0.776***	-0.807***	-0.681***	-0.806***
	(0.0588)	(0.0606)	(0.0693)	(0.0674)	(0.0599)
2013 × Treatment Intensity	-0.781***	-0.703***	-0.779***	-0.639***	-0.755***
	(0.0627)	(0.0645)	(0.0742)	(0.0735)	(0.0641)
2014 × Treatment Intensity	-0.569***	-0.520***	-0.592***	-0.470***	-0.547***
	(0.0701)	(0.0718)	(0.0826)	(0.0808)	(0.0707)
2015 × Treatment Intensity	-0.506***	-0.512***	-0.521***	-0.500***	-0.487***
	(0.0735)	(0.0761)	(0.0867)	(0.0862)	(0.0742)
2016 × Treatment Intensity	-0.341***	-0.390***	-0.404***	-0.355***	-0.306***
	(0.0796)	(0.0830)	(0.0962)	(0.0961)	(0.0818)
2017 × Treatment Intensity	-0.0620	-0.172*	-0.144	-0.160	-0.0534
	(0.0846)	(0.0886)	(0.103)	(0.0998)	(0.0865)
2018 × Treatment Intensity	0.213*	0.00886	0.00326	0.0882	0.265**
	(0.110)	(0.117)	(0.133)	(0.141)	(0.112)
Firm FE	Yes	Yes	Yes	Yes	No
Year FE	Yes	No	Yes	No	Yes
Ind×Year FE	No	Yes	No	No	No
Bank×Year FE	No	No	No	Yes	No
Bank×Firm FE	No	No	No	No	Yes
Bank Controls	No	No	Yes	No	No
Firm Controls × Post	Yes	Yes	Yes	Yes	Yes
Observations	606,455	605,248	433,734	547,060	593,932
Adj. R^2	0.776	0.776	0.763	0.789	0.786
Pre-trends p-value: $\beta_{2007} = \beta_{2008} = 0$	0.807	0.933	0.974	0.318	0.775

Notes: This table reports the estimated treatment effects for five fixed-effects specifications of Equation (15). Dependent variable: $\ln(r_{it})$. Treatment Intensity constructed from 2009 firm characteristics and year-specific probit intercepts. Baseline year: 2009 (omitted). Standard errors clustered at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 5: Event-study plot



Notes: Event-study coefficients from Table 5 across all five specifications. Each coefficient represents the interaction between Treatment Intensity and a year dummy, with 2009 as the omitted baseline year. The five specifications — Baseline, Ind×Year FE, Bank Controls, Bank×Year FE, and Bank×Firm FE — yield broadly consistent estimates throughout the sample period. Capped spikes represent 95% confidence intervals based on standard errors clustered at the firm level.

This temporal pattern is consistent with the nature of forbearance under the Act. Since most forbearance was granted in the form of payment deferrals (Ono and Yasuda, 2017), the large initial effects reflect the immediate relief from interest payments. As deferred payments come due and forbearance arrangements expire, the measured effect diminishes and eventually dissipates. The positive coefficients in 2018 in some specifications suggest that firms with high treatment exposure subsequently faced interest rates that exceeded those of comparable low-exposure firms, possibly reflecting catch-up payments or updated risk assessments by lenders, but also anticipated future termination costs. The stability of results across specifications is reassuring (see Figure 5). The inclusion of granular industry-by-year fixed effects in Column (2) leaves the estimates largely unchanged relative to the baseline, indicating that the results are not driven by differential industry trends. The joint tests for pre-treatment coefficients ($\beta_{2007} = \beta_{2008} = 0$) yield p-values well above conventional significance levels across all specifications, and the pre-treatment coefficients are individually small and statistically insignificant throughout. The results provide no evidence against the parallel trends assumption in any specification.

Our baseline specification in Equation (15) directly incorporates a test of the parallel trends assumption by estimating treatment effects for pre-treatment years (2007-2008). Beyond this direct test embedded in our main specification, we follow additional approaches to assess the validity of parallel trends. We start with a visual analysis and conclude with the sensitivity test of Rambachan and Roth (2023).

5.1.2 Visual analysis of parallel trends

Panel A of Figure 6 shows mean log interest rates for eligible firms (grey line) and non-eligible firms (black line) over time, normalized to zero in 2009, so that the y-axis shows log point changes relative to the 2009 baseline, interpretable as approximate percentage changes. Prior to 2009, both groups experienced similar increases in interest rates, with the lines tracking each other closely. After 2009, both groups show declining interest rates, though eligible firms appear to experience a smaller decline than non-eligible firms. Panel B shows the approximate percentage change in interest rates for non-eligible firms (grey) and for each exposure tercile among eligible firms, with Tercile 1 representing the lowest exposure. We assign firms to groups using their exposure measure based on 2009 firm characteristics only—that is, the predicted probability from the Probit model excluding the year-specific intercept. In the pre-treatment period, all four lines track each other closely, providing visual support for the parallel trends assumption across the full distribution of treatment intensity. After 2009, a pattern of monotonic divergence emerges: firms with higher exposure experience smaller declines in interest rates.[26]

As a further check, Appendix Figure 8 replicates the visual parallel trends analysis using firm-level interest rate data from Bureau van Dijk's Orbis database, which provides a substantially longer pre-treatment window extending back to 2002. The pre-treatment trajectories of eligible and non-eligible firms track each other over the seven years preceding the Act, especially closely from 2005, reinforcing the evidence for the parallel trends assumption from our main TSR sample.

5.1.3 Sensitivity analysis to violations of parallel trends

Although Table 5 shows no significant treatment effects in the pre-treatment period, the limited length of the pre-treatment window might be a concern. To address this, we implement the sensitivity analysis framework developed by Rambachan and Roth (2023), which allows us to assess the robustness of our treatment effect estimates to potential violations of the parallel trends assumption. We define the *pre-trend* as the year-over-year change in the pre-treatment coefficients:

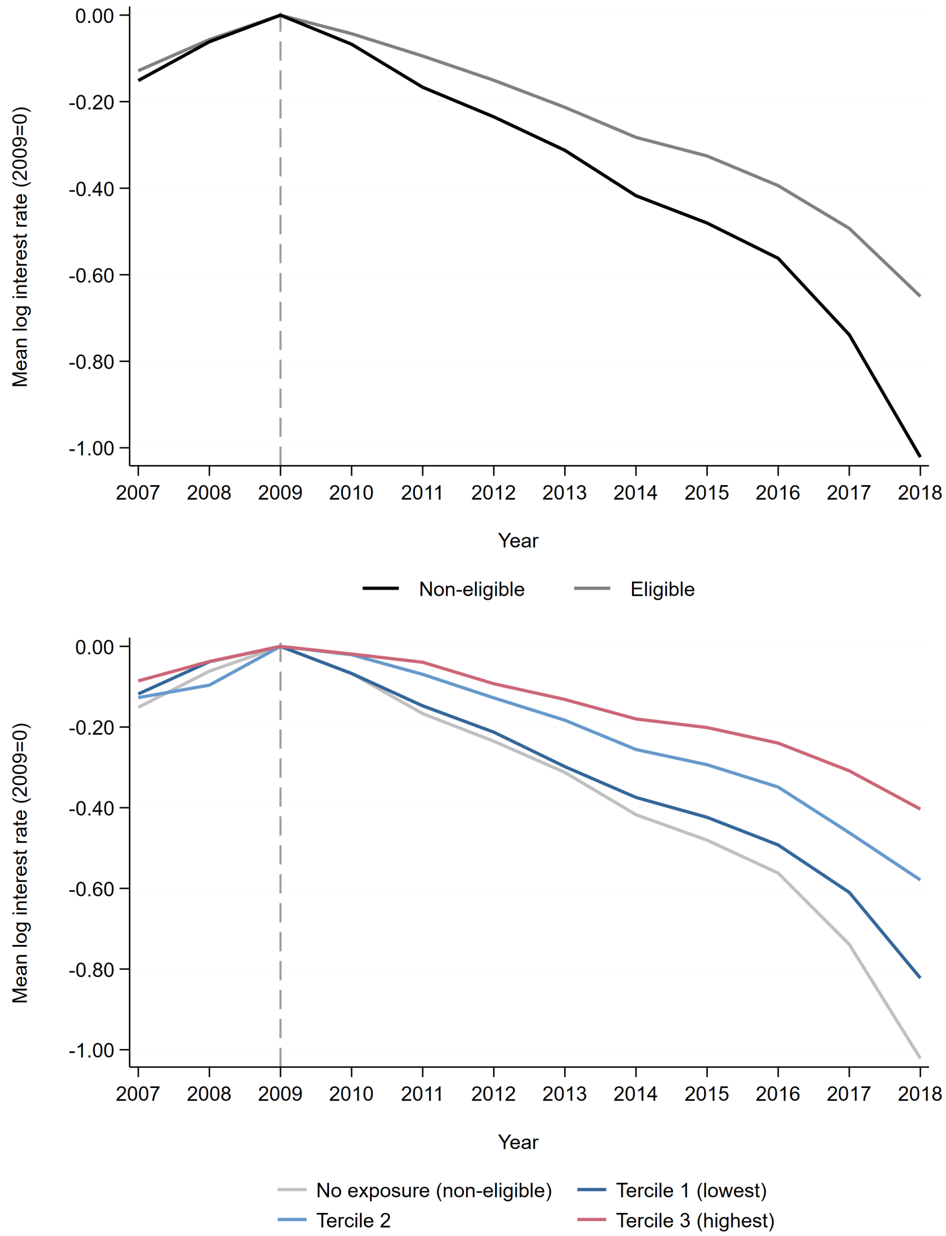
$$\hat{\delta}_{\text{pre}} = \hat{\beta}_{2008} - \hat{\beta}_{2007} \tag{16}$$

The key insight of the Rambachan and Roth (2023) approach is that rather than simply testing whether pre-trends are statistically significant, we can ask a more policy-relevant question: *how large would violations of parallel trends need to be to overturn our conclusions?* The method constructs robust confidence intervals under the assumption that the change in the slope of the differential trend between any two consecutive post-treatment periods is at most $\bar{M}$ times as large as the maximum pre-treatment trend change observed in the data:

$$|\delta_{\tau+1} - \delta_{\tau}| \leq \bar{M} \times |\hat{\delta}_{\text{pre}}|, \quad \forall \tau \in \{\text{post-treatment periods}\} \tag{17}$$

[26]The direction of the raw pattern suggests that confounders work *against* our finding of negative treatment effects post-2009, pushing high-exposure firms' rates upward relative to low-exposure firms.

Figure 6: Mean log interest rates (2009=0), by eligibility and exposure tercile



Notes: The figure plots mean log interest rates by group, normalized to zero in 2009. Values on the y-axis can be interpreted as approximate percentage changes (e.g., -0.05 corresponds to approximately 5% lower than in 2009). The vertical dashed line indicates the introduction of the policy. Panel A compares eligible and non-eligible firms. Panel B divides eligible firms into terciles based on treatment intensity (the predicted probability from the Probit model using 2009 firm characteristics, excluding year-specific intercepts), with non-eligible firms shown as the reference group.

Although we observe only two pre-treatment periods, the Rambachan and Roth (2023) approach does not treat the estimated pre-trend as a precise estimate of the true violation. Instead, it uses the magnitude of the observed pre-trend as a benchmark to ask how robust our results are to progressively larger deviations from parallel trends in the post-treatment period, making the analysis informative even with limited pre-treatment data. The resulting confidence intervals answer the question: would we still reject a zero treatment effect if the parallel trends assumption were violated by up to $\bar{M}$ times the magnitude of the estimated pre-trend in each post-treatment period? When $\bar{M} = 0$, we impose exact parallel trends. As $\bar{M}$ increases, we allow for increasingly large deviations from parallel trends.[27] The key summary statistic is the *breakdown point* $\bar{M}^*$—the smallest value of $\bar{M}$ at which the robust confidence interval first includes zero. A larger breakdown point indicates greater robustness: it means that only very large violations of parallel trends (relative to what we observe in the pre-period) could overturn our findings.

Table 6 presents our pre-trend estimates and sensitivity analysis. Panel A reports the pre-treatment coefficients. The 2007 and 2008 coefficients are both small and statistically insignificant. The joint F-test fails to reject the null hypothesis that both pre-treatment coefficients equal zero ($F = 0.21$, $p = 0.814$). However, what matters most for the parallel trends assumption is not the level of these coefficients but whether they are *changing* over time—that is, the pre-trend. If the pre-treatment coefficients are trending in the same direction as the post-treatment effects, some of the estimated treatment effects could be driven by a continuation of the pre-existing trend rather than by the policy itself. The estimated pre-trend is $\hat{\delta}_{\text{pre}} = \hat{\beta}_{2008} - \hat{\beta}_{2007} = -0.009$ ($p = 0.866$), indicating a negligible and statistically insignificant change in the differential trend between high- and low-exposure firms during the pre-treatment period. The sign of the pre-trend is negative—the same direction as our post-treatment effects—but the pre-trend represents only 1.7% of the average treatment effect in absolute magnitude, making this concern negligible. Panel B reports the treatment effects. The average effect across the nine post-treatment years is $\bar{\beta}_{\text{post}} = -0.520$ ($p < 0.001$), indicating that higher-exposure firms experienced substantially lower interest rates relative to lower-exposure firms following the policy. Panel C presents the sensitivity analysis. The breakdown point is $\bar{M}^* = 9$, meaning that our confidence intervals for the average treatment effect remain bounded away from zero even when we allow post-treatment trend violations up to nine times larger than the observed pre-trend.

[27] The framework considers violations in both directions—it does not assume that any post-treatment deviation continues in the same direction as the pre-trend, but rather evaluates worst-case bounds allowing for deviations of up to $\bar{M} \times |\hat{\delta}_{\text{pre}}|$ per period in either direction. This is reflected in the symmetric widening of the confidence intervals as $\bar{M}$ increases.

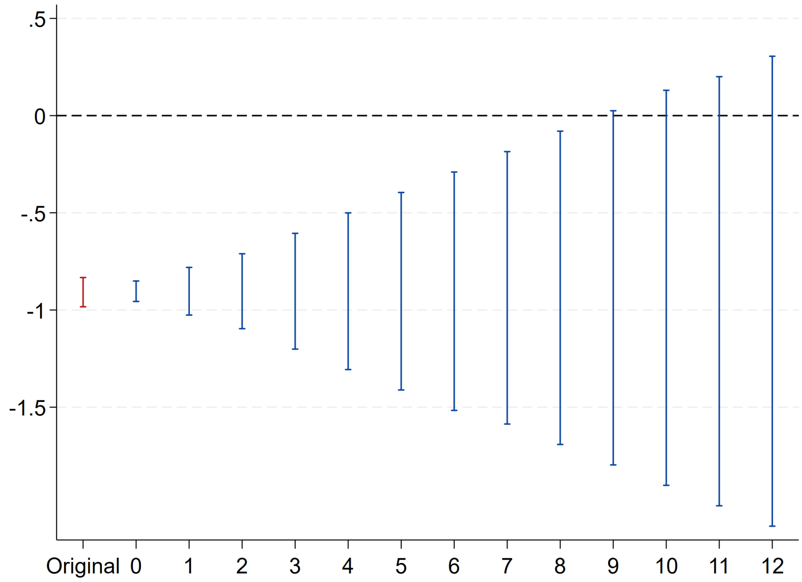
Table 6: Sensitivity Analysis: Pre-trends and robustness

	Coefficient	Std. Error	p-value	Notes
Panel A: Pre-treatment period (2007–2008)				
2007 coefficient (β_{2007})	0.035	0.061	0.565	Relative to 2009
2008 coefficient (β_{2008})	0.026	0.047	0.581	Relative to 2009
Pre-trend ($\hat{\delta}_{\text{pre}}$)	**-0.009**	**0.053**	**0.866**	$= \boldsymbol{\beta_{2008}} - \boldsymbol{\beta_{2007}}$
Joint F-test	$F = 0.21$		0.814	$H_0 : \beta_{2007} = \beta_{2008} = 0$
Panel B: Treatment effects (2010–2018)				
Average effect ($\bar{\beta}_{\text{post}}$)	**-0.520**	**0.059**	**0.000**	**Average across 9 years**
2010 (immediate)	-0.908	0.038	0.000	First year
2014 (mid-period)	-0.570	0.070	0.000	5 years post-treatment
2018 (final year)	0.213	0.110	0.053	9 years post-treatment
Panel C: Rambachan & Roth (2023) Sensitivity Analysis				
Breakdown point ($\bar{M}^*$)	**9**			CI first crosses zero
Pre-trend as % of avg. effect	1.7%			Relative magnitude
Upper CI bound at breakdown $\bar{M}^*$	0.025			

Notes: This table reports pre-treatment coefficients, treatment effects, and a sensitivity analysis following Rambachan and Roth (2023). The dependent variable is $\ln(r_{it})$. Treatment Intensity is the time-varying exposure measure constructed from 2009 firm characteristics and year-specific probit intercepts. All regressions follow the specification of column (1) in Table 5. Standard errors clustered at the firm level.

Figure 7 displays the sensitivity analysis graphically. Each vertical line represents a 95% confidence interval for the average treatment effect $\bar{\beta}_{\text{post}}$ under different assumptions about post-treatment parallel trends violations. The red interval labeled "Original" shows our baseline estimate assuming exact parallel trends. The subsequent blue intervals show robust confidence intervals for increasing values of $\bar{M}$. The upper bound of the confidence interval crosses zero only at $\bar{M}^* = 9$, indicating that our finding of negative treatment effects is highly robust. Overall, the sensitivity analysis substantially mitigates concerns about the limited pre-treatment period: the pre-trend is economically negligible (1.7% of the average treatment effect, $p = 0.866$), the breakdown point is high, and the temporal pattern of treatment effects—large immediate impacts that gradually attenuate—is consistent with a causal interpretation and difficult to reconcile with smooth pre-existing trends.

Figure 7: Sensitivity to violations of parallel trends



Notes: This figure displays robust 95% confidence intervals for the average treatment effect ($\bar{\beta}_{\text{post}}$) over the post-treatment period (2010–2018) under different assumptions about post-treatment violations of parallel trends, following Rambachan & Roth (2023). The horizontal axis shows $\bar{M}$, which bounds the maximum post-treatment trend violation as a multiple of the estimated pre-trend ($|\hat{\delta}_{\text{pre}}|= 0.009$). The red interval labeled "Original" shows the baseline estimate assuming exact parallel trends. Blue intervals show robust confidence intervals for increasing values of $\bar{M}$, widening as we allow for larger potential violations. The dashed horizontal line indicates zero. The breakdown point is $\bar{M}^* = 9$.

5.2 Impact on firm performance

In this section, we examine whether the Act encouraged zombification and affected firm performance more broadly. Using a DiD approach, we examine whether the Act affected: (i) the probability that a firm is classified as a zombie according to the two definitions presented in Section 4.7, (ii) firm exit through bankruptcy[28], (iii) total factor productivity (TFP), and (iv) debt sustainability as measured by the interest coverage ratio (ICR). TFP is estimated following Wooldridge (2009)[29] and ICR is defined as the ratio of EBIT over interest expenses.

If banks granted forbearance to unviable firms, we should observe a positive treatment effect on the probability of being classified as a zombie and a negative treatment effect on TFP. If, on the other hand, banks granted forbearance to illiquid but viable firms which put in place successful restructuring plans, we should observe a negative treatment effect on the probability of being classified as a zombie and a positive treatment effect on TFP. Irrespective of whether forbearance was granted to viable or unviable firms, we expect a negative effect on exit and a positive impact on the interest coverage ratio.

[28]The TSR data set contains information on firm exits. Exits can be categorized into three groups: *tosan* (bankruptcy), *gappei* (merger), and *kaihaikyu* (voluntary exit). The year of exit in TSR is recorded from October (current year) to September (following year). When merging the exit data with our firm-level data set, we define the year of exit as the TSR year of exit minus one. See Supplemental Appendix C.

[29]See Supplemental Appendix C.2.

We estimate the following baseline specification for all outcomes:

$$g(Y_{it}) = \alpha + \sum_{t \neq 2009} \beta_t D_t \text{Treatment Intensity}_{it} + f_{(\cdot)} + \text{Controls}_{it} + \epsilon_{it} \tag{18}$$

where $g(Y_{it})$ is the appropriate transformation of the outcome variable for firm i at time t. For continuous outcomes (TFP and ICR), $g(Y_{it}) = \ln(Y_{it})$, and the coefficients represent percentage differences between treated and non-treated firms in each year (see Appendix A). For binary outcomes (zombie status and exit), $g(Y_{it}) \in \{0, 1\}$ and we estimate linear probability models (LPM). We use LPMs rather than Logit or Probit for our binary outcomes because our specifications require the inclusion of high-dimensional fixed effects.[30] All remaining variables, controls, and the treatment intensity measure are as previously defined. Standard errors are clustered at the firm level throughout. As in our interest rate specifications, we include pre-treatment years (2007–2008) to assess whether parallel trends hold for each outcome, and report joint tests of the null hypothesis $\beta_{2007} = \beta_{2008} = 0$.

For each outcome, we estimate the same five progressively saturated specifications as in Section 5.1.1, with the full equation and all specifications reported in Supplemental Appendix G. For exit, firm fixed effects are excluded from all specifications because exit is a rare, absorbing event. For zombie status, firm fixed effects are included because zombie status varies meaningfully within firms over time. Table 7 reports the baseline specification (firm and year fixed effects, firm controls interacted with Post) for each outcome. The full set of progressively saturated specifications, event-study plots, and parallel trends analyses are presented in Supplemental Appendix G.

5.2.1 Zombie status

The estimated treatment effects on zombie status are significantly negative in every post-treatment year for both definitions (Columns (1)–(2) of Table 7). The treatment effects are increasing in magnitude over time, reaching approximately -0.9 by 2014–2018, consistent with restructuring plans having a gradual effect. A coefficient of -0.9 implies that a treated firm (Treatment Intensity = 1) is approximately 90 percentage points less likely to be classified as a zombie than an otherwise identical untreated firm (Treatment Intensity = 0). These results are robust across all five progressively saturated specifications (Supplemental Appendix G).

The pre-treatment coefficients are significantly negative for both definitions. However, the estimated pre-trend is *positive* ($\hat{\delta}_{\text{pre}} = \beta_{2008} - \beta_{2007} = 0.113$, $p < 0.001$ for the two-tiered definition), meaning that the zombie rate for high-exposure firms was accelerating relative to low-exposure firms before the intervention. Since the post-treatment effects are negative, this pre-trend runs in the *opposite* direction to the treatment effects, rendering our estimates conservative. The Rambachan and Roth (2023) sensitivity analysis yields breakdown points of $\bar{M}^* = 3$ (two-tiered) and $\bar{M}^* = 2.25$ (single-

[30] Nonlinear models suffer from the incidental parameters problem when estimated with firm fixed effects, leading to inconsistent parameter estimates (Greene, 2004).

Table 7: Effect of the Act on firm performance: baseline specification

	(1) Zombie (two-tiered)	(2) Zombie (single-tiered)	(3) Exit	(4) ln(TFP)	(5) ln(ICR)
Pre-treatment coefficients					
2007 × Treatment Intensity	-0.178***	-0.196***	0.00112	-0.0312	-1.197***
	(0.0314)	(0.0293)	(0.00131)	(0.0515)	(0.216)
2008 × Treatment Intensity	-0.0655**	-0.0472*	0.00950**	-0.106**	-1.060***
	(0.0275)	(0.0256)	(0.00378)	(0.0503)	(0.219)
Post-treatment coefficients					
2010 × Treatment Intensity	-0.482***	-0.431***	0.000571	0.372***	1.354***
	(0.0231)	(0.0215)	(0.00304)	(0.0437)	(0.257)
2011 × Treatment Intensity	-0.639***	-0.606***	-0.00674**	0.576***	1.616***
	(0.0341)	(0.0323)	(0.00323)	(0.0672)	(0.420)
2012 × Treatment Intensity	-0.782***	-0.770***	-0.00861**	0.580***	1.364***
	(0.0359)	(0.0343)	(0.00357)	(0.0718)	(0.462)
2013 × Treatment Intensity	-0.868***	-0.847***	-0.0124***	0.626***	1.561***
	(0.0382)	(0.0365)	(0.00276)	(0.0772)	(0.556)
2014 × Treatment Intensity	-0.929***	-0.981***	-0.00787**	0.531***	0.834
	(0.0433)	(0.0408)	(0.00345)	(0.0852)	(0.522)
2015 × Treatment Intensity	-0.880***	-0.901***	-0.00691*	0.535***	0.736
	(0.0412)	(0.0393)	(0.00376)	(0.0782)	(0.498)
2016 × Treatment Intensity	-0.892***	-0.952***	-0.0132***	0.574***	0.438
	(0.0419)	(0.0403)	(0.00329)	(0.0795)	(0.477)
2017 × Treatment Intensity	-0.883***	-0.904***	-0.0102***	0.573***	0.389
	(0.0423)	(0.0403)	(0.00361)	(0.0784)	(0.445)
2018 × Treatment Intensity	-0.920***	-0.937***	-0.0175***	0.504***	0.217
	(0.0479)	(0.0452)	(0.00257)	(0.0867)	(0.469)
Firm FE	Yes	Yes	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm Controls × Post	Yes	Yes	Yes	Yes	Yes
Observations	606,455	606,455	608,646	606,455	454,698
Adj. R^2	0.403	0.510	0.003	0.368	0.698
Pre-trends p-value	0.000	0.000	0.029	0.104	0.000

Notes: This table reports the baseline specification with firm and year fixed effects for each outcome variable. Columns (1)–(2): linear probability models for zombie status (two-tiered and single-tiered definitions). Column (3): linear probability model for firm exit (bankruptcy); firm fixed effects excluded because exit is a rare, absorbing event. Columns (4)–(5): log TFP (Wooldridge, 2009) and log ICR (EBIT/interest expenses). Baseline year: 2009 (omitted). Standard errors clustered at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Full results with progressively saturated specifications are reported in Supplemental Appendix G.

tiered), indicating robustness to potential violations of parallel trends. A detailed discussion is in Supplemental Appendix G.

5.2.2 Firm exit (bankruptcy)

Column (3) of Table 7 shows that the Act reduced the incidence of bankruptcies among treated firms. The treatment effects are significantly negative in several post-treatment years, with the largest effects appearing from 2013 onward and reaching -0.012 to -0.018 in the later years. Given the very low average bankruptcy rate in Japan (approximately 0.2% in 2018), these are economically substantial effects.

The pre-treatment coefficient for 2008 is positive and significant, indicating that firms with higher treatment intensity were more likely to exit just before the policy intervention—consistent with these firms being in greater financial distress. The pre-trend is positive ($\hat{\delta}_{\text{pre}} = 0.008$, $p = 0.036$), running opposite to the negative treatment effects. However, the pre-trend is large relative to the treatment effect (91.1% of $|\bar{\beta}_{\text{post}}|$), reflecting the small absolute magnitude of exit effects, and the Rambachan and Roth (2023) breakdown point is $\bar{M}^* = 0$. The exit results should therefore be interpreted with appropriate caution, though we note that the dose-response pattern and monotonically growing treatment effects over time are both consistent with a genuine policy effect. Full results and the parallel trends analysis are in Supplemental Appendix G.

5.2.3 Total factor productivity

Column (4) of Table 7 shows that the estimated treatment effects on TFP are significantly positive in all post-treatment years. The coefficient in 2010 is 0.372, implying that a treated firm experienced TFP approximately 45% higher than an otherwise identical untreated firm (since $\exp(0.372) - 1 \approx 0.45$). The treatment effects increase through 2013 (reaching approximately 0.63) and remain large and stable thereafter.

The pre-treatment coefficient for 2008 is negative and significant, indicating that treated firms had lower TFP before the intervention, but the pre-trend ($\hat{\delta}_{\text{pre}} = -0.075$, $p = 0.181$) is negative and insignificant, running opposite to the positive post-treatment effects. The joint F-test does not reject the null ($p = 0.104$). The Rambachan and Roth (2023) breakdown point is $\bar{M}^* = 2$, indicating comfortable robustness. Full results and the parallel trends analysis are in Supplemental Appendix G.

5.2.4 Interest coverage ratio

Column (5) of Table 7 shows that the treatment effects on the ICR are significantly positive in the early post-treatment years (2010–2013), indicating that the Act improved the debt sustainability of treated firms. The coefficient in 2010 is 1.354, implying that treated firms experienced ICRs approximately 287% higher than untreated firms (since $\exp(1.354) - 1 \approx 2.87$). The large magnitude reflects the fact that small changes in interest payments can produce large proportional changes in the interest coverage ratio. The treatment effects decrease over time, consistent with the temporal attenuation of the interest rate effects documented in Table 5: as forbearance arrangements expire and deferred payments come due, the relief to debt-servicing burdens diminishes. By 2014, the coefficients are no longer statistically significant.

The pre-treatment coefficients are large, negative, and highly significant, consistent with treated firms having substantially worse debt sustainability before the intervention. The pre-trend is positive though insignificant ($\hat{\delta}_{\text{pre}} = 0.136$, $p = 0.536$), running in the same direction as the treatment effects. The Rambachan and Roth (2023) breakdown point is $\bar{M}^* = 0.8$, and the ICR results should be interpreted with some caution with respect to the parallel trends assumption. Full results and the parallel trends analysis are in Supplemental Appendix G.

Taken together, the results in this section paint a coherent picture. The SME Financing Facilitation Act reduced the probability of zombie status, reduced firm exit through bankruptcy, improved total factor productivity, and improved debt sustainability—at least in the short to medium run. These findings are consistent with the interpretation that forbearance under the Act was primarily directed at illiquid but viable firms, which successfully implemented restructuring plans.

6 Counterfactual exercises

6.1 Methodology

We use the partial equilibrium model of Section 3 to conduct back-of-the-envelope counterfactual exercises that estimate the aggregate impact of mandated forbearance. These exercises ask: what would have happened to aggregate capital, output, and capital productivity if the SME Financing Facilitation Act had not been enacted? The procedure has four steps: (i) computing firm-level counterfactual capital stocks, (ii) computing firm-level counterfactual output, (iii) aggregating to the economy level, and (iv) considering alternative assumptions about capital reallocation.

Step 1: Firm-level counterfactual capital stocks

We start with the profit maximisation problem for optimal loan size $\hat{k}_{ijt}$ in Equation (3). The first-order condition defines the optimal capital stock as a function of the interest rate:

$$\frac{\partial \pi_{i,j,t}}{\partial \hat{k}_{i,j,t}} = r_{i,j,t} + \hat{k}_{i,j,t} \frac{\partial r_{i,j,t}}{\partial \hat{k}_{i,j,t}} \tag{19}$$

Equation (19) shows that a decrease in the interest rate paid by firms due to forbearance must lead to an increase in the firms' capital stock. Assuming that the firm treats the interest rate as exogenous when choosing its optimal capital stock, $\hat{k}_{i,j,t} \frac{\partial r_{i,j,t}}{\partial \hat{k}_{i,j,t}} = 0$, the firm's marginal product of capital is optimally equated with the interest rate. Substituting the production function (1) into Equation (19) yields the optimal capital stock:

$$k^*_{ijt} = \left(\frac{\alpha\gamma z_{it} l_{ijt}^{(1-\alpha)\gamma}}{r_{i,j,t}} \right)^{\frac{1}{1-\alpha\gamma}} \tag{20}$$

In the baseline specification, we hold labour input constant.[31] Under fixed labour, a change in the interest rate following a change in forbearance incentives affects the firm's capital stock according to Equation (21):

$$\Delta \log(k_{ijt}) = -\frac{1}{1-\alpha\gamma} \Delta \log(r_{i,j,t}) \tag{21}$$

For each firm i in each post-treatment year t, we use Equation (21) to compute the change in capital that would result from removing the interest rate effect estimated in our dynamic DiD model in column (1) of Table 5. The estimated change in the log interest rate is scaled by the firm's treatment intensity, so that firms with greater exposure to the policy experience larger capital adjustments. This yields a firm-specific capital multiplier:

$$m_{it} = \exp\left(\frac{1}{1-\alpha\gamma} \times \text{Treatment Intensity}_{it} \times \widehat{ATE}_t \right)$$

where $\widehat{ATE}_t$ is the estimated annual treatment effect on the log interest rate from column (1) in Table 5. When the policy subsidizes interest rates ($\widehat{ATE}_t < 0$), removing the subsidy raises rates and $m_{it} < 1$, so the counterfactual capital stock falls below the observed one. Conversely, when the policy generates a tax ($\widehat{ATE}_t > 0$, as in later years), removing it lowers rates and $m_{it} > 1$. We truncate m_{it} to the interval $[0.85, 1.15]$ to restrict changes to realistic magnitudes.[32] The firm's counterfactual capital stock is:

$$k_{it}^{CF} = \max\left(m_{it} \times k_{it},\ k_{0it}\right)$$

where k_{0it} is the firm's non-bank-financed capital. This floor ensures that counterfactual capital does not fall below the portion of the capital stock that is not financed through bank lending.

Step 2: Firm-level counterfactual output

Once k_{it}^{CF} has been determined, we compute each firm's counterfactual output by substituting k_{it}^{CF} into the production function (1), using the firm's estimated time-varying TFP ($\hat{z}_{it}$) and its observed labour input:

$$y_{it}^{CF} = \hat{z}_{it}\, l_{it}^{(1-\alpha)\gamma}\, (k_{it}^{CF})^{\alpha\gamma} - w_t l_{it}$$

Firm-level TFP is estimated for each firm-year pair as the Solow residual of the production function: $\hat{z}_{it} = \exp(\ln y_{it} - \alpha\gamma \ln k_{it} - (1-\alpha)\gamma \ln l_{it})$, using the Wooldridge (2009) WRDG-GMM estimates of

[31] Japanese firms are well known for being reluctant to lay off staff during performance declines (see, e.g., Kang and Shivdasani, 1997). We relax this assumption in Supplemental Appendix F.5.

[32] The results are not sensitive to these bounds and remain almost unchanged when we use looser bounds $[0.80, 1.20]$, indicating that very few firm-year observations are affected by the truncation.

α and γ (see Supplemental Appendix C.2[33]). Because TFP is estimated at the firm-year level, the counterfactual preserves each firm's observed productivity trajectory: we only vary the capital stock (and, in the flexible-labour extension, labour), holding the firm's actual productivity path fixed. This gives the net output (value added minus labour costs) each firm would have produced absent the policy, after adjusting its capital stock optimally to the counterfactual interest rate.

Step 3: Aggregation

We aggregate firm-level counterfactuals using sampling weights ω_i that ensure representativeness of the employment and industry distribution in the full TSR data set. The aggregate counterfactual capital stock and output are $K_t^{CF} = \sum_i \omega_i \, k_{it}^{CF}$ and $Y_t^{CF} = \sum_i \omega_i \, y_{it}^{CF}$. We define counterfactual aggregate capital productivity as counterfactual aggregate output per unit of counterfactual capital:

$$APK_t^{CF} = \frac{Y_t^{CF}}{K_t^{CF}} \tag{22}$$

Since y_{it}^{CF} is already net of labour costs (Step 2), APK_t^{CF} measures counterfactual output net of the wage bill per unit of capital. All counterfactual quantities are reported as percentage deviations from their observed equivalents.

Step 4: Reallocation scenarios

We partition firms into *treated* firms, whose capital would decline if the policy were removed ($m_{it} < 1$), and *non-treated* firms ($m_{it} \geq 1$). We consider two scenarios for what happens to the capital that the policy channels toward treated firms.

No reallocation. Each firm retains its counterfactual capital stock k_{it}^{CF} and produces with its own TFP. Aggregate counterfactual output is Y_t^{CF} as computed in Steps 2–3. Capital that would not have been lent to treated firms in the absence of the policy remains idle.

With reallocation. The capital that the policy channels toward treated firms is instead redeployed elsewhere in the economy. Specifically, non-treated firms continue to produce at their observed output levels $Y_t^{\textit{non-treated}} = \sum_{i:m_{it} \geq 1} \omega_i \, y_{it}$. The entire observed capital stock of treated firms, $K_t^{\textit{treated}} = \sum_{i:m_{it}<1} \omega_i \, k_{it}$, is assumed to be seamlessly redeployed by firms that produce at the counterfactual aggregate capital productivity APK_t^{CF}. Counterfactual output under reallocation is therefore:

$$Y_{t,reall}^{CF} = APK_t^{CF} \times K_t^{\textit{treated}} + Y_t^{\textit{non-treated}} \tag{23}$$

Because this scenario assumes instant and costless capital redistribution, it provides an upper bound on the output losses from the policy intervention, while the no-reallocation scenario provides a lower bound. In practice, frictions in firm entry and exit, scaling up production, and acquiring credit would

[33] Supplemental Appendix F.3 shows that the results are robust to alternative production function estimates using the ACF estimator.

limit the extent of reallocation.

Counterfactuals with flexible labour

In the baseline, labour is held fixed. In Supplemental Appendix F.5, we relax this assumption and allow labour to adjust optimally alongside capital. When a firm's capital stock changes, its optimal labour input also changes because the marginal product of labour depends on the capital stock. Combining the first-order conditions for capital and labour yields a larger capital response to interest rate changes than under fixed labour (Equation 34 in Supplemental Appendix F.5), because reducing capital lowers the marginal product of labour, which reduces optimal employment, further depressing output. Counterfactual output is then computed using both adjusted capital and labour in the production function. The aggregation and reallocation steps proceed as in the baseline.

6.2 Results

Table 8 presents the counterfactual results obtained by removing the annual treatment effects in column (1) of Table 5. The percentages show the annual deviations between counterfactual aggregate output, capital stock, and capital productivity and their observed equivalents. The first row of Table 8 shows the percentage deviation between the aggregate counterfactual capital stock and the actual aggregate capital stock. The results point to a substantial effect of removing the interest rate subsidy generated by the Act. The aggregate capital stock would have declined by 2.3% in 2010 if the policy had not been enacted. On average, the policy boosted the aggregate capital stock by about 1.3% during 2010–2018. The gains from the policy fade over time and turn into losses from 2018 onward (Supplemental Appendix F.2 reports the cumulative output effects over the sample period). We interpret these dynamics as resulting from the fact that payment deferrals only generate a temporary subsidy, after which firms return to higher interest rates (possibly higher than pre-treatment). In terms of the model, this indicates a weakening of current forbearance incentives relative to the future. Equation (9) shows that forbearance incentives can generate a tax, as opposed to a subsidy, when the per-capital-unit termination cost in the current period is small relative to the discounted per-capital-unit termination cost in the next period. In other words, a tax can arise if banks have weak forbearance incentives in the present but price in high severance costs in the future.

The second row of Table 8 shows that a higher capital stock came at the expense of lower capital productivity. Capital productivity would have been 0.83% higher in 2010 in the absence of the Act. On average, the Act depressed capital productivity by about 0.5%. The productivity losses decrease over time as the interest rate subsidy and its impact on the capital stock fade. This temporal pattern is driven entirely by the diminishing returns mechanism: because the counterfactual holds each firm's time-varying TFP fixed at its *observed* values and only adjusts the capital stock, the capital productivity dynamics reflect the curvature of the production function ($\gamma < 1$) rather than changes in firm-level efficiency. In the early post-treatment years, the interest rate subsidy boosts capital, and diminishing returns depress capital productivity. As the subsidy fades and the capital adjustment

Table 8: Effect of removing the estimated treatment effects (in %) - fixed labour

Counterfactuals - % change	2010	2011	2012	2013	2014	2015	2016	2017	2018	Average
Capital stock	-2.33 %	-2.27 %	-2.14 %	-1.99 %	-1.50 %	-1.33 %	-0.90 %	-0.17 %	0.53 %	-1.34 %
Capital productivity	0.83 %	0.83 %	0.73 %	0.72 %	0.49 %	0.46 %	0.32 %	0.06 %	-0.15 %	0.48 %
Output, without reallocation	-4.55 %	-4.10 %	-3.59 %	-3.27 %	-2.39 %	-2.05 %	-1.35 %	-0.24 %	0.83 %	-2.30 %
Output, with reallocation	5.54 %	6.10 %	4.04 %	2.51 %	0.01 %	-0.96 %	-0.83 %	1.04 %	0.00 %	1.94 %

reverses, the diminishing-returns drag dissipates. Supplemental Appendix F.4 quantifies the role of decreasing returns by comparing the baseline counterfactual ($\gamma = 0.964$) with a constant-returns-to-scale specification ($\gamma = 1$). Note that this mechanism operates even with homogeneous TFP. TFP heterogeneity across firms adds a cross-sectional composition dimension—the firms whose capital stocks are most affected tend to have different TFP levels—but this is a composition effect, not a within-firm productivity improvement. Separately, our DiD regressions document genuine TFP improvements for treated firms (Table 7 and Supplemental Appendix Table 13), but these are not incorporated into the counterfactual framework, making it a conservative exercise. The third row indicates that the policy intervention protected output, which would have otherwise dropped by about 4.6% in 2010. On average, the policy is estimated to have boosted output by about 2.3%. This assumes no reallocation of capital. Under the alternative scenario with reallocation, where treated firms' capital is redeployed at the counterfactual aggregate capital productivity, the policy is estimated to have decreased output by around 5.5% in 2010 and by about 1.9% on average.

The reallocation results assume instant and costless capital redistribution to firms producing at the counterfactual aggregate capital productivity. Such a redistribution is unlikely given the usual frictions in firm entry, firm exit, scaling up output, and acquiring credit. Furthermore, capital reallocation is procyclical, i.e. depressed during recessions (Caballero and Hammour, 2005; Eisfeldt and Rampini, 2006). While liquidations rise during downturns, they only translate into greater restructuring if followed by increased creation during the recovery. Instead, Caballero and Hammour (2005) show that less financing is available during recessions and their aftermath, which limits creation, so that recoveries tend to occur through lower destruction rather than higher creation. Our reallocation counterfactuals therefore provide an upper-bound estimate of output losses from the policy intervention.

7 Conclusions

There has been scepticism about forbearance lending in academic and policy circles. It acquired a bad reputation as one of the potential drivers behind the post-GFC slowdown in European countries, not least because of the analysis of forbearance lending in Japan during the Lost Decade. While it is true that forbearance lending can contribute to zombification and reduce aggregate productivity, an evaluation of Japan's 2009 SME Financing Facilitation Act suggests that a well-designed credit market intervention based on forbearance lending can help viable firms weather difficult times. We use the Act as a quasi-experimental setting to quantify the aggregate consequences of loan forbearance using a DiD approach combined with back-of-the-envelope counterfactual exercises. We find that the Act worked as an interest rate subsidy, boosting the aggregate capital stock but depressing aggregate productivity. In the more plausible scenario of subdued credit reallocation, the Act is estimated to have boosted output. Importantly, we find that the Act did not contribute to the creation of zombie firms. On the contrary, we find that greater exposure to the policy increased firm-level TFP and reduced the probability that a firm becomes a zombie. This suggests that the business improvement plans that troubled SMEs and their lenders could jointly formulate, enabling banks to classify restructured loans as performing, allowed these firms to resurrect. Exploring how targeted interventions based on forbearance lending could be designed to respond to future shocks is an interesting an interesting avenue for future research and policy design.

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Appendix

A Coefficient interpretation with log dependent variable

Our baseline dynamic DiD specification with continuous treatment intensity is:

$$\ln(r_{it}) = \alpha + \sum_{t \neq 2009} \beta_t D_t \text{Treatment Intensity}_{it} + \Gamma X_{it} + f_i + f_t + \sum_c \gamma_c \text{Post}_t X_{it}^c + \varepsilon_{it} \tag{24}$$

where r_{it} is firm i's average interest rate on debt obligations (in %) at time t, Treatment Intensity$_{it}$ is a continuous measure of treatment exposure for firm i, constructed from baseline year characteristics (2009) and year fixed effects that capture the pattern of aggregate take-up (taking values between 0 and 1), D_t is an indicator variable equal to 1 for year t and 0 otherwise, the sum runs over all years $t \in \{2007, 2008, 2010, \ldots, 2018\}$ excluding the baseline year 2009, X_{it} is a vector of time-varying control variables (product of borrowed capital, credit market tightness, share of bonds and accounts payable in total debt), X_{it}^c is a vector of confounding covariates (firm characteristics that predict treatment exposure: leverage, credit score, ROA, log sales, employees, firm age), Post$_t$ is an indicator equal to 1 for all years $t \geq 2010$ and 0 otherwise, f_i are firm fixed effects, f_t are year fixed effects (absorb common time shocks including standalone D_t), and ε_{it} is the error term. The log-linear specification in Equation (24) implies an underlying multiplicative model for r_{it}:

$$r_{it} = \exp\left(\alpha + \sum_{t \neq 2009} \beta_t D_t \text{Treatment Intensity}_{it} + \Gamma X_{it} + f_i + f_t + \sum_c \gamma_c \text{Post}_t X_{it}^c\right) \times \eta_{it}$$

where $\varepsilon_{it} = \ln \eta_{it}$.

Isolating β_t requires the two differences of the DiD structure: first differencing over time within firms to eliminate firm fixed effects, then differencing across firms to eliminate common time trends. Consider two firms: firm i with Treatment Intensity$_{it} = p$ (where $0 < p \leq 1$) and firm j with Treatment Intensity$_{jt} = 0$ (zero exposure). For post-treatment year $t \geq 2010$, the expected change in the log interest rate from the baseline year (2009) to year t for firm i, holding time-varying controls X and confounding covariates X^c constant over time for simplicity, is:

$$E[\ln r_{it} - \ln r_{i,2009} \mid \text{Treatment Intensity}_{it} = p] = \beta_t p + (f_t - f_{2009}) + \sum_c \gamma_c X_i^c$$

The unobserved firm fixed effect f_i cancels because we difference within the same firm. The confounding covariate term $\sum_c \gamma_c X_i^c$ appears because Post$_t = 1$ in year t but Post$_{2009} = 0$. Note that Treatment Intensity$_{it}$ does not enter the baseline year equation: there is no β term in 2009 (it is absorbed into the omitted reference year), and Post$_{2009} = 0$ sets the confounding interactions to zero. The expected change over the same period for the untreated firm j is:

$$E[\ln r_{jt} - \ln r_{j,2009} \mid \text{Treatment Intensity}_{jt} = 0] = (f_t - f_{2009}) + \sum_c \gamma_c X_j^c$$

Taking the second difference, comparing the trajectory of firm i to that of firm j, while evaluating at the same covariate levels, the common time trend $(f_t - f_{2009})$ and the covariate terms cancel:

$$(E[\ln r_{it} - \ln r_{i,2009} \mid p]) - (E[\ln r_{jt} - \ln r_{j,2009} \mid 0]) = \beta_t p \tag{25}$$

where conditioning on equal covariates is left implicit. This double difference formally isolates $\beta_t p$ as the differential change in expected log interest rates for a firm with treatment intensity p relative to a firm with zero treatment intensity, measured from the 2009 baseline. The result is exact within the model and requires no approximation. Since Treatment Intensity$_{it}$ does not enter the baseline year equation, the 2009 conditional expectations are identical for treated and untreated firms evaluated at the same covariates:

$$E[\ln r_{i,2009} \mid p] = E[\ln r_{j,2009} \mid 0]$$

The baseline terms therefore cancel in Equation (25), so that $\beta_t p$ reduces to the cross-sectional difference in expected log interest rates between a firm with intensity p and an otherwise identical untreated firm in year t:

$$E[\ln r_{it} \mid p] - E[\ln r_{jt} \mid 0] = \beta_t p \tag{26}$$

The dependent variable is log-transformed, so exponentiation gives the exact percentage effect in levels. The percentage difference in the expected interest rate for a firm with intensity p relative to an untreated firm is:

$$\text{Percentage Effect} = (\exp(\beta_t p) - 1) \times 100 \tag{27}$$

For small values of β_t, the standard log-difference approximation $(\ln(a) - \ln(b) \approx (a - b)/b)$ makes $\beta_t \times 100$ approximately the percentage difference in interest rates between a fully treated firm $(p = 1)$ and an untreated firm $(p = 0)$. As an example, suppose we estimate $\beta_t = -0.02$ for some post-treatment year t. Using the exact formula, an otherwise identical firm with full treatment intensity $(p = 1)$ has an interest rate $(\exp(-0.02) - 1) \times 100 \approx -1.98$ percent lower in year t. For a firm with TreatmentIntensity$_{it} = 0.5$, the exact treatment effect is $(\exp(-0.01) - 1) \times 100 \approx -0.995$ percent. Both results align closely with the log approximations of -2 percent and -1 percent respectively.

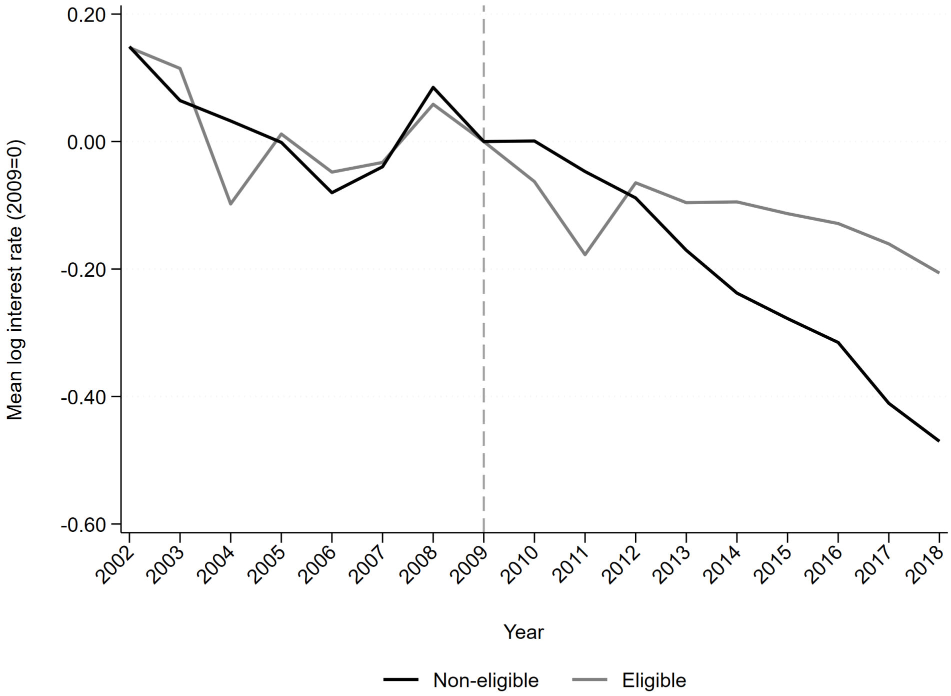
B External validation of parallel trends using Orbis data

Our main analysis relies on the TSR database, which provides coverage from 2007. A potential concern is that two pre-treatment years may be insufficient to assess the parallel trends assumption. To address this, we construct an analogous measure of mean log interest rates using firm-level data from Bureau van Dijk's Orbis database, accessed via Wharton Research Data Services (WRDS), from 2002 onward, thus providing seven pre-treatment years (2002–2008).

Because the composition of firms in the Orbis sample varies substantially across years, we apply an explicit composition-weighting procedure to ensure that year-to-year changes in average log interest rates reflect genuine rate movements rather than shifts in the sample's demographic profile. Specifically, we assign each firm to a cell in a three-dimensional matrix defined by age bin, four-digit industry group, and Tankan size category. We then calculate the share of each cell in the 2009 cross-section and, for all other years, reweight observations so that the aggregate composition of age, industry, and size mechanically reproduces the 2009 distribution. This ensures that the plotted time series are not confounded by differential entry and exit of firm types across years. The plotted series are composition-weighted means of log interest rates, constructed analogously to the TSR figures in the main text.

Figure 8 plots the resulting composition-weighted mean log interest rates for eligible and non-eligible firms, normalized to zero in 2009. Prior to 2009, the two series display some year-to-year volatility but no systematic divergence. Both groups begin at comparable levels in 2002, fluctuate in the early years with some visible crossing of the series around 2003–2004, and then move closely together from approximately 2005 through 2008 before converging at the normalized baseline in 2009. Crucially, none of the short-lived deviations is sustained in one direction; the two groups repeatedly return to similar levels. This pattern—visible over seven pre-treatment years and substantially extending the pre-treatment window available in the TSR data—provides reassurance that the parallel trends assumption underlying our difference-in-differences design is plausible.[34]

Figure 8: Mean log interest rates (2009=0), eligible vs. non-eligible firms—Orbis sample



Notes: The figure plots composition-weighted mean log interest rates for eligible and non-eligible firms using data from Bureau van Dijk's Orbis database, normalized to zero in 2009. Values on the y-axis represent log point changes relative to 2009 and can be interpreted as approximate percentage changes. To account for variation in the composition of the Orbis sample across years, each firm-year observation is reweighted so that the aggregate distribution of firm age, four-digit industry, and Tankan size category in each year matches the 2009 cross-sectional distribution. The vertical dashed line indicates the introduction of the SME Financing Facilitation Act.

[34] As with Figure 6 in the main text, the post-2009 unconditional divergence shows eligible firms experiencing *smaller* declines in interest rates than non-eligible firms. This raw pattern reflects the same confounding variation discussed in the main text: once firm and bank characteristics are controlled for, the regression estimates reveal that higher exposure is associated with proportionally faster rate declines.

Supplemental Appendix for "Forbearance lending as a crisis management tool: Evidence from Japan"

Isabelle Michalski-Roland Yukiko Saito Philip Schnattinger

This is the Supplemental Appendix for "Forbearance lending as a crisis management tool: Evidence from Japan". Section A gives details on the eligibility criteria under the SME Financing Facilitation Act. Section B provides further information on the representativeness of our sample in comparison to the full TSR dataset. Section C provides further descriptive statistics—including the production function estimates used to construct TFP and APBK—to complement section 4 in the paper. Section D describes the determination of the lower bound interest rate used in the construction of the zombie dummy variable in section 4.7 of the paper. Section E derives the loan termination condition of the model of section 3. Section F presents additional details on the counterfactuals, including cumulative output effects and sensitivity to production function parameters. Section G presents the full estimation results for the firm performance regressions of Section 5.2, including progressively saturated specifications, event-study plots, and parallel trends sensitivity analyses following Rambachan and Roth (2023). Finally, Section H examines the correlation between loan forbearance and lender characteristics.

A Eligibility under the SME Financing Facilitation Act

Under the SME Financing Facilitation Act, financial institutions providing loans to SMEs which were experiencing, or were expected to experience, difficulties repaying their debts, were obliged to alter the loan conditions and alleviate the payment burden of the borrowers to the best of their ability. Struggling SMEs were required to submit an application to their respective financial institutions, but as described in section 2.2 most applications were accepted. The eligibility dummy was created in accordance with the SME Financing Facilitation Act, specifically Article 2, paragraph (2) items (i) through (ix) and Article 4. To begin, we identify firms that qualify as SMEs as laid out in Article 2, paragraph (2). The eligibility criteria are based on the firm's number of employees and stated share capital. We then exclude firms identified by Article 4 of the SME Financing Facilitation Act, which specifies certain types of SMEs that are not eligible for financing under the Act. Article 2 Paragraph 2 of the SME Financing Facilitation Act defines qualifying SMEs as follows:

- Companies with stated capital of 300 million yen or less, engaged in general business activities, excluding financial services and other exclusions designated by the Cabinet Order. For companies operating primarily a retail or services business, the stated capital threshold is 50 million yen or less. For companies operating primarily a wholesale business, the stated capital threshold is 100 million yen or less.[35]

[35] Japanese SME legislation groups retail and wholesale industries together as the "commerce" sector, which is different from the "services" industry. In terms of the Japan Standard Industrial Classification, the "services" industries refer to the following: Division G – Information and Communications, 38 Broadcasting, 39 Information services, 411 Video picture information production and distribution, 412 Sound information production, 415 Commercial art and graphic design, 416 Services incidental to video picture information, sound information, character information production and distribution, Division K - Real Estate and Goods Rental and

- Companies with fewer than 300 regular employees, engaged in general business activities, excluding financial services and other exclusions designated by the Cabinet Order. For companies operating primarily a retail business, the maximum number of employees is 50. For companies operating primarily a wholesale or services business, the maximum number of employees is 100.
- SME cooperatives, agricultural cooperatives, federations of agricultural cooperatives, fishery cooperatives, forest owners' cooperatives, forestry production associations, federation of forestry cooperatives, consumer cooperatives and federations of consumer cooperatives that are engaged in general business activities, or that have at least two-thirds of their members engaged in general business activities, excluding financial services and other exclusions designated by the Cabinet Order.
- Cooperative partnerships engaged in general business activities.
- Corporations that operate a medical business as their principal business, and whose number of regular employees is not more than 300.
- Commercial and industrial partnerships, federations of commercial and industrial partnerships that are engaged in general business activities or whose members are engaged in general business activities.
- Shopping district promotion cooperatives and federations of shopping district promotion cooperatives that are engaged in general business activities or whose members are engaged in general business activities.
- Environmental health industry associations, minor environmental health industry cooperatives, and federations of environmental health industry associations that are engaged in general business activities or whose members are engaged in general business activities, and that have at least two-thirds of their direct and indirect members reporting a stated capital of 50 million yen or less or a number of regular employees of maximum 50. For associations whose members operate primarily a retail business, the stated capital threshold is 100 million yen. For associations whose members operate primarily a wholesale or services business, the employee threshold is 100.
- Sake brewers' associations, federations of Sake Brewers' Associations and Japan Sake Brewers' Association that have at least two-thirds of their direct and indirect members reporting a stated capital of 300 million yen or less or a number of regular employees of maximum 300.
- Liquor merchants' associations, federations of liquor merchants' associations and All Japan Liquor Merchants' Association that have at least two-thirds of their direct and indirect members reporting a stated capital of 50 million yen or less or a number of regular employees of maximum 50. For the liquor wholesale industry, the stated capital threshold is 100 million yen and the employee threshold is 100.
- Coastal shipping associations or federations of coastal shipping associations that have at least two-thirds of their direct and indirect members reporting a stated capital of 300 million yen or less or a number of regular employees of maximum 300.

Leasing, 693 Automobile parking, 70 Goods rental and leasing, Division L - Scientific research, professional and technical services, Division M - Accommodations, Eating and Drinking Services, 75 Accommodations, Division N - Living-related and personal services and amusement services, Excluding 791 Travel Agency, Division O - Education, learning support Division P - Medical, health care and welfare, Division Q -Compound Services, Division R -Services, N.E.C.

- Other firms designed by the Cabinet Order.

Article 4 excludes the following firms due to their affiliations to large firms or financial institutions:

- Financial institutions.
- Subsidiaries or parent companies of financial institutions.
- Large firms, i.e. firms with stated capital of more than 500 million yen or liabilities of more than 200 million yen.
- Subsidiaries of large firms and firms designated by the Cabinet Order as having a special relationship with a large firm.

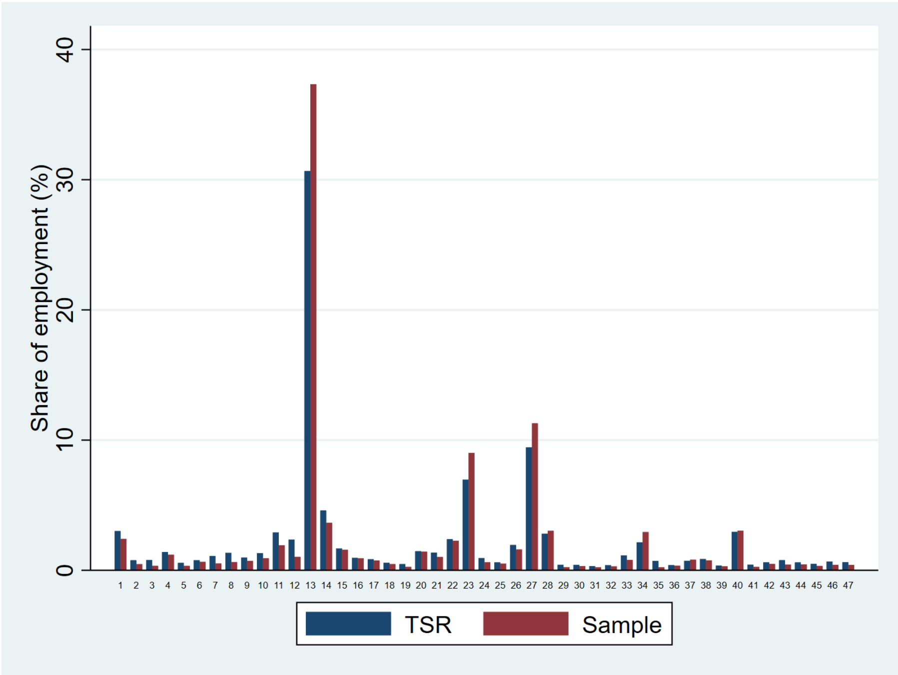
B Representativeness of the TSR sample

Although the TSR data set does not cover the universe of firms in Japan, it resembles closely the distribution of the Census data in terms of geographic coverage and firm size (see e.g. Hong et al., 2020). It can therefore be used to study aggregate economic developments in Japan. However, we can only work with firm-year observations that have all the data items required for the estimation of our difference-in-differences specifications. In this section, we compare our TSR sample to the *full* TSR data set in terms of geographical coverage, and size and industry distributions. The full TSR numbers exclude Finance and insurance (as we focus on non-financial corporations) and other sectors not covered at all by our sample. The excluded sectors are real estate and goods rental and leasing; education, learning support; medical, health care and welfare; compound services; and services n.e.c.

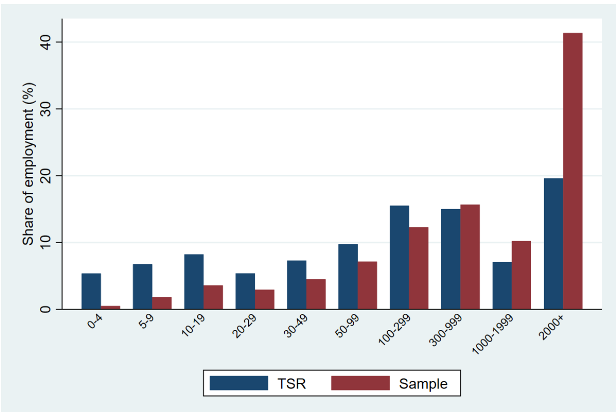
Figure 1 displays these comparisons for 2016; the patterns are similar in other sample years. Panel (a) shows the fraction of firms in each of the 47 prefectures in the TSR data set and in our sample. The two distributions track each other closely across nearly all prefectures. Tokyo (prefecture 13) dominates both distributions, accounting for roughly 30% of employment in the full TSR data and around 37% in the sample. Beyond Tokyo, the next-largest prefectures (Osaka (27), Aichi (23), and Kanagawa (14)) also feature prominently in both distributions, and the remaining prefectures show only minor discrepancies between the two series. Panel (b) compares the distribution of firms by size. The overall shape of the two distributions is similar, with employment shares rising across size categories. The largest gaps are found at the tails: micro-enterprises with fewer than 10 employees are under-represented in the sample (for instance, the 0–4 category accounts for roughly 5% of TSR employment but under 1% in our sample), while very large enterprises with more than 2,000 employees are over-represented (approximately 20% in TSR versus over 40% in the sample). In the intermediate size categories, from 50 to 999 employees, the two distributions align closely. Panel (c) shows the firm industry distribution. Manufacturing (sector E) is the largest sector in both distributions, though it receives somewhat greater weight in the sample (around 47%) than in the full TSR data (around 33%). Wholesale and retail trade (sector I) is the second-largest sector, with a slightly higher share in TSR than in the sample. The remaining sectors display broadly similar shares across the two distributions. Overall, while our estimation sample is tilted toward larger firms and manufacturing, it preserves the broad sectoral and geographic structure of the full TSR data set.

Figure 1: Representativeness of the sample: TSR versus sample distributions

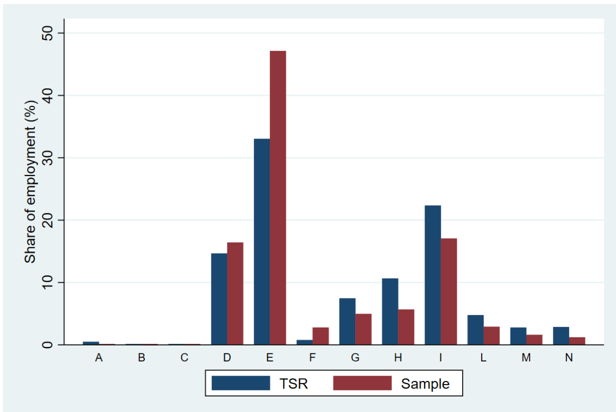
(a) Geographical distribution



(b) Size distribution



(c) Industry distribution



Notes: All panels show distributions in 2016; patterns are similar in other sample years. Panel (a): The 47 prefectures are: 1. Hokkaidō, 2. Aomori, 3. Iwate, 4. Miyagi, 5. Akita, 6. Yamagata, 7. Fukushima, 8. Ibaraki, 9. Tochigi, 10. Gunma, 11. Saitama, 12. Chiba, 13. Tōkyō, 14. Kanagawa, 15. Niigata, 16. Toyama, 17. Ishikawa, 18. Fukui, 19. Yamanashi, 20. Nagano, 21. Gifu, 22. Shizuoka, 23. Aichi, 24. Mie, 25. Shiga, 26. Kyōto, 27. Ōsaka, 28. Hyōgo, 29. Nara, 30. Wakayama, 31. Tottori, 32. Shimane, 33. Okayama, 34. Hiroshima, 35. Yamaguchi, 36. Tokushima, 37. Kagawa, 38. Ehime, 39. Kōchi, 40. Fukuoka, 41. Saga, 42. Nagasaki, 43. Kumamoto, 44. Ōita, 45. Miyazaki, 46. Kagoshima, 47. Okinawa. Panel (b): The size bands are: 0–4, 5–9, 10–19, 20–29, 30–49, 50–99, 100–299, 300–999, 1000–1999, and 2000+ employees. Panel (c): The industry divisions follow the Industrial Classification used in the 2016 Economic Census for Business Activity (see `https://www.stat.go.jp/english/data/e-census/2016/industry.html`): A. Agriculture and forestry, B. Fisheries, C. Mining and quarrying of stone and gravel, D. Construction, E. Manufacturing, F. Electricity, gas, heat supply and water, G. Information and communications, H. Transport and postal services, I. Wholesale and retail trade, L. Scientific research, professional and technical services, M. Accommodation, eating and drinking services, N. Living-related and personal services and amusement services.

C Supplemental descriptive statistics

This section provides further descriptive statistics to complement Section 4 in the paper.

C.1 Treatment intensity

Table 1 summarizes the distribution of treatment intensity across firms in 2009 (solely based on 2009 firm characteristics). Across all firms (Panel A), mean treatment intensity is 0.09, but this masks substantial heterogeneity by size: small firms have the highest average exposure (0.15), followed by medium-sized firms (0.07), while large firms have near-zero exposure (0.02), with more than half registering zero treatment intensity. Conditioning on eligible firms (Panel B) shifts the distribution upward, particularly for large firms, whose mean exposure rises from 0.02 to 0.04 as the non-eligible large firms with zero exposure are dropped. The eligible-firm sample retains nearly all small firms (22,965 of 23,066) but only about a third of large firms (4,229 of 11,477), consistent with the eligibility criteria targeting SMEs. Within the eligible population, the interquartile range remains narrow for medium-sized and large firms relative to small firms, indicating that much of the cross-sectional variation in treatment intensity is driven by the small-firm segment.

Table 1: Treatment intensity in 2009

	Obs	Mean	Std. Dev.	P25	Median	P75
Panel A: All firms						
All firms	67,401	0.09	0.10	0.04	0.07	0.10
Small firms	23,066	0.15	0.13	0.08	0.11	0.17
Medium-sized firms	32,858	0.07	0.04	0.04	0.06	0.08
Large firms	11,477	0.02	0.02	0.00	0.00	0.03
Panel B: Eligible firms only						
All firms	57,538	0.10	0.10	0.05	0.08	0.11
Small firms	22,965	0.15	0.13	0.08	0.11	0.17
Medium-sized firms	30,344	0.07	0.04	0.05	0.07	0.09
Large firms	4,229	0.04	0.02	0.02	0.04	0.05

Notes: Panel A reports summary statistics for the treatment intensity measure across all firms in the 2009 cross-section (ignoring the year fixed effects). Panel B restricts the sample to firms eligible for forbearance under the SME Financing Facilitation Act. Size classes follow the Tankan classification.

C.2 Production function estimation

We estimate the Cobb-Douglas production function $y_{it} = z_{it}(l_{it}^{1-\alpha} k_{it}^{\alpha})^{\gamma}$ to recover firm-level TFP and the capital elasticity used in the construction of the average product of borrowed capital (APBK) and in the counterfactual exercises. Output is measured as value added (current profit plus the wage bill plus net interest payments). Labour input is the total wage bill (comprising manufacturing labour costs, salaries, bonuses, welfare, and retirement contributions). Capital is proxied by total balance sheet assets. Materials are used as the proxy variable to address the simultaneity of input choices and measured as the cost of sales net of manufacturing labour costs. This exclusion avoids double-counting since manufacturing labour costs are already part of the labour input. All variables are deflated using industry-level deflators from the JIP Database 2023 (RIETI and Hitotsubashi University, 2023; 2015 base year): a value-added deflator for output, labour, and materials, and a capital-stock deflator for assets. The deflators are mapped from 2-digit Japan Standard Industrial Classification (JSIC) codes to JIP sectors using a weighted average based on nominal value added. The estimation sample excludes finance and insurance industries (JSIC divisions 62–71 and 81+), requires positive values for output, capital, labour, and materials, and retains only firms observed in at least two consecutive years.

Our baseline specification (Column (1) of Table 2) uses the Wooldridge (2009) WRDG-GMM estimator, which embeds the first-stage proxy equation and the second-stage production function in a single GMM system, improving efficiency relative to two-step approaches. A key advantage of the WRDG framework is that it avoids the sequential estimation uncertainty that accumulates in conventional two-step proxy-variable methods, yielding more precise coefficient estimates and straightforward inference. As a robustness check, Column (2) replaces the WRDG estimator with the Ackerberg, Caves, and Frazer (2015) (ACF) estimator, which addresses the potential collinearity between labour and the control function that can arise in the first stage of proxy-variable methods. Because the ACF correction re-estimates the labour coefficient in the second stage, it provides a useful diagnostic for whether the WRDG labour share is distorted by this functional dependence problem. Standard errors for the ACF specification are obtained via bootstrap ($R = 20$ replications). In both specifications, the coefficient estimates are normalised so that the labour and capital shares sum to unity: $\alpha = \hat{\beta}_K/(\hat{\beta}_L + \hat{\beta}_K)$ and $1 - \alpha = \hat{\beta}_L/(\hat{\beta}_L + \hat{\beta}_K)$. Returns to scale ($\gamma$) are estimated in a separate step by regressing log output on the composite input index $\alpha \ln k_{it} + (1 - \alpha) \ln l_{it}$, controlling for year fixed effects.

Table 2 shows that the estimates are robust across estimators. The labour share $(1 - \alpha)$ is 0.72 under WRDG and 0.83 under ACF, while the capital share (α) is 0.28 and 0.17, respectively. The somewhat larger labour share under ACF is consistent with the correction for first-stage collinearity reallocating explanatory power from capital to labour, a pattern commonly reported in the literature. Returns to scale are close to unity in both cases ($\gamma \approx$ 0.96–0.97), consistent with mild decreasing returns at the firm level. The capital elasticity $\alpha\gamma$, which governs the average product of borrowed capital (APBK) and the counterfactual capital multiplier, is 0.27 under WRDG and 0.17 under ACF. Although the point estimates of α differ across columns, the close agreement in γ and the qualitative similarity of $\alpha\gamma$ indicate that the production function parameters are not sensitive to the choice of estimation method. Our baseline counterfactual uses the Column (1) estimates ($\alpha = 0.281$, $\gamma = 0.964$). Supplemental Appendix F.3 confirms that the counterfactual results are robust to using the ACF parameters instead.

Firm-level TFP is recovered as a time-varying residual for each firm-year observation:

$$\hat{z}_{it} = \exp(\ln y_{it} - \alpha\gamma \ln k_{it} - (1 - \alpha)\gamma \ln l_{it}).$$

This time-varying TFP enters the counterfactual exercises of Section 6, where it ensures that each firm's observed productivity trajectory is preserved while only the capital stock (and, in the flexible-labour extension, labour) is adjusted. By conditioning on realised TFP, the counterfactuals isolate the pure capital-reallocation channel, holding firm-level efficiency fixed at its historically observed path.

Table 2: Production function estimates

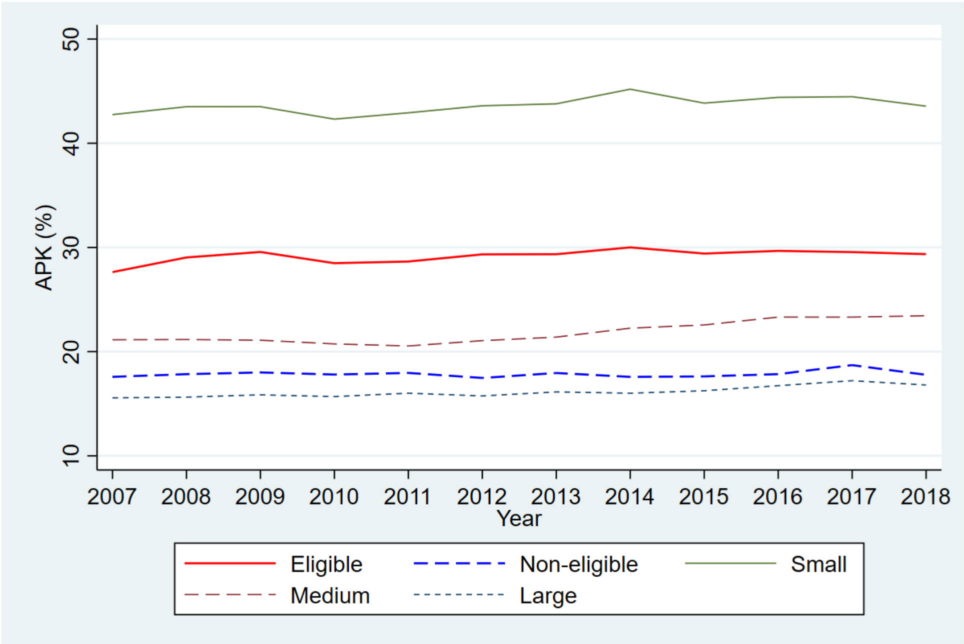
	(1)	(2)
Production function parameters		
$1-\alpha$ (Labour share)	0.719	0.829
	(0.001)	(0.066)
α (Capital share)	0.281	0.171
	(0.002)	(0.032)
Returns to scale		
γ (Returns to scale)	0.964	0.969
	(0.000)	(0.000)
$\alpha\gamma$	0.271	0.166
Specification		
Method	WRDG	ACF
Year FE in RTS	Yes	Yes

Notes: Column (1) uses the Wooldridge (2009) WRDG-GMM estimator; column (2) uses the Ackerberg, Caves, and Frazer (2015) correction with bootstrapped standard errors ($R = 20$). Coefficients are normalised so that $\alpha + (1-\alpha) = 1$. γ denotes returns to scale, estimated via OLS regression of log output on the composite input index with year fixed effects. Standard errors in parentheses. The intermediate input proxy excludes manufacturing labour costs to avoid overlap with the labour input. All variables deflated using JIP 2023 industry-level value added and capital stock deflators (2015 base year).

C.3 Average product of borrowed capital

Figure 2 below shows the sample mean average product of borrowed capital ($APBK$, defined in Equation (13) in the paper) for different types of firms. The $APBK$ is systematically highest for small firms (with one extra ¥ of bank credit generating an extra 44 ¥ in profit before interest, on average) and lowest for large firms (with one extra unit of bank credit generating an extra 16 ¥ in profit before interest, on average). To the extent that marginal and average costs move together, this is consistent with the fact that small firms face higher marginal costs of borrowed capital, i.e. higher loan interest rates. The $APBK$ is very stable across time for all types of firms.

Figure 2: Average product of borrowed capital (in ¥)



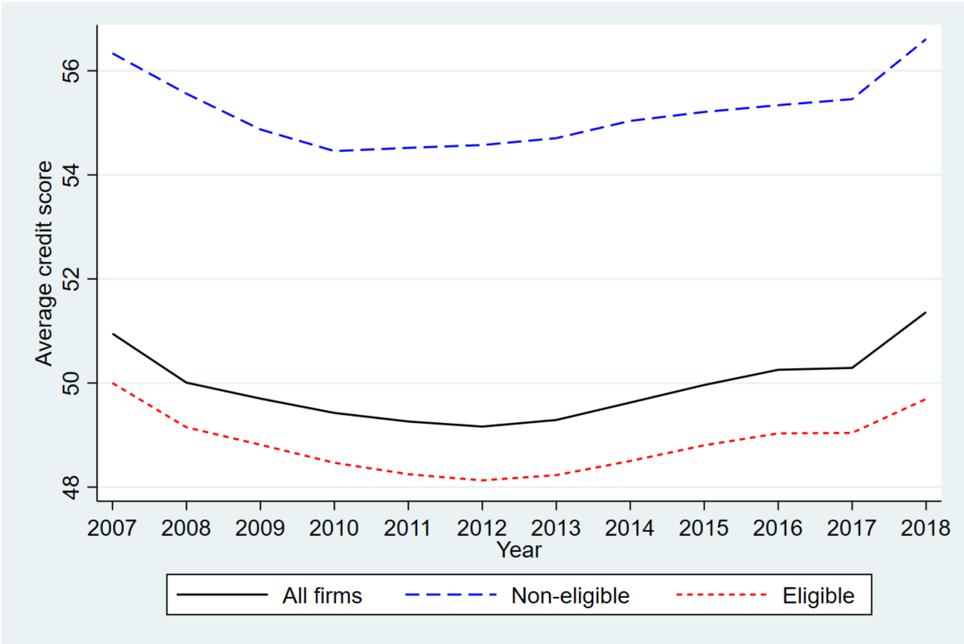
Note: Average product of borrowed capital in the sample, as defined in Equation (13) in the paper, assuming fixed labor input. Size categories are those of the Tankan Survey of the Bank of Japan. Source: TSR.

C.4 Credit scores

TSR classifies firms into five groups of creditworthiness according to their credit scores. Scores less than or equal to 29 are classified as "keikai" (caution). Scores between 30 and 49 are classified as "ichio keikai" (somewhat caution). Scores between 50 and 64 are categorized as "tasho chui" (attention). Scores between 65 and 79 are "bunan" (safe), and those between 80 and 100 are considered to be "keikai fuyo" (no risk). The average credit score in our sample is 49.8 over 2007-2018, on the fence between "ichio keikai" and "tasho chui" (Figure 3). The credit scores of eligible firms are systematically lower (at 48.8 on average) than those of non-eligible firms (at 55.2 on average) - consistent with SMEs being riskier borrowers. However, the time series pattern is similar for eligible and non-eligible firms. Credit scores deteriorate slightly from a pre-crisis peak in 2007 to a low in 2012, recovering by 2018.

Table 3 shows the percentage of firms in each creditworthiness category in the sample for the whole sample period, and the years 2007, 2012, and 2018. There are very few firms in the worst (keikai) and best (keikai fuyo) creditworthiness categories. The deterioration in the average credit score between 2007 and 2012 apparent in Figure 3 is driven by a transfer of firms from "tasho chui" to "ichio keikai". This is reversed between 2012 and 2018.

Figure 3: Average credit score by eligibility status



Notes: Average credit score in the sample. Source: TSR.

Table 3: Distribution of firms by creditworthiness category (%)

	2007–2018	2007	2012	2018
keikai	0.07	0.07	0.09	0.02
ichio keikai	50.60	41.48	55.08	41.73
tasho chui	46.30	54.81	42.14	52.79
bunan	3.00	3.61	2.68	5.41
keikai fuyo	0.02	0.03	0.01	0.04

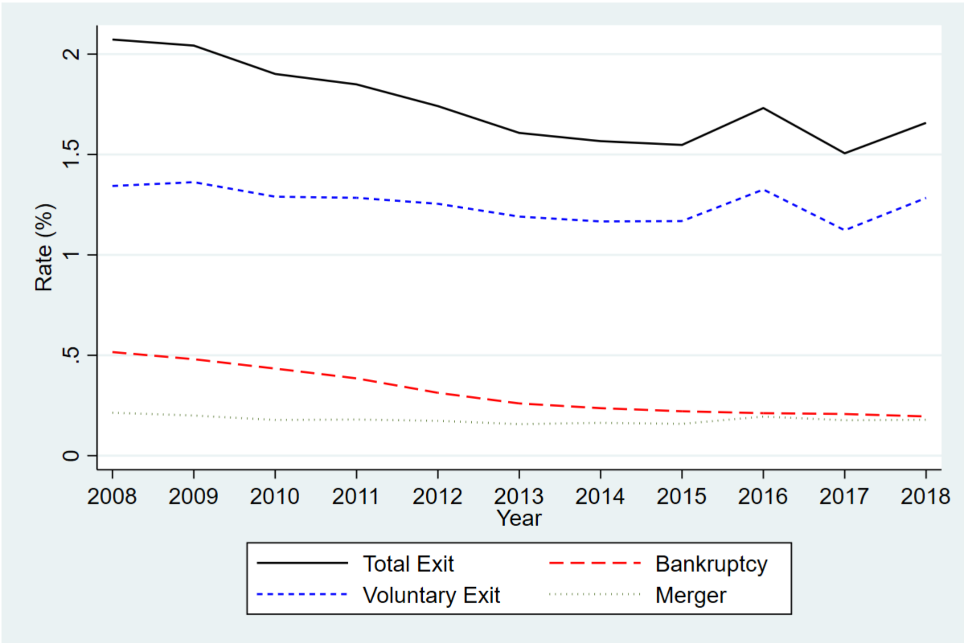
Notes: Share of firms in each of the five creditworthiness categories in our sample. Source: TSR.

C.5 Exit

The TSR data set contains information on firm exits. For the firms that exited, TSR provides information about the reasons for exit. Exits can be categorized into three groups: *tosan* (bankruptcy), *gappei* (merger), and *kaihaikyu* (voluntary exit). TSR distinguishes between three different types of voluntary exits: *kyugyo* (temporary suspension of business), *haigyo* (business closure), and *kaisan* (dissolution of company). Figure 4 shows the exit rates over time for all types of exit and for each type separately. The year of exit in TSR is recorded from October (current year) to September (following year). When merging the exit data with our firm-level data set, we define the year of exit as the TSR year of exit minus one. In other words, we define the exit rate in a given year as the number of firms that exited between October of the previous year and September of the current year as a percentage of the total number of firms in the previous year. Figure 4 shows that exit rates are very low according to the TSR data, a finding which is well-documented (see, e.g., Hong et al., 2020). In addition, exit rates have declined substantially over our sample period. The overall exit rate declined from 2.07%

in 2008 to 1.66% in 2018. The decline is mainly driven by a drop in the rate of bankruptcies, from 0.52% in 2008 to 0.2% in 2018.

Figure 4: Exit rates by exit category



Notes: Exit rates (in %) over time, by exit category. Source: TSR.

D Measurement of lower bound interest rate

The guiding principle to construct a lower bound interest rate ($R^*_{i,t}$) is to select interest rates that are extremely advantageous for the borrower, so that the lower bound is in fact less than most firms would pay in the absence of forbearance. The lower bound $R^*_{i,t}$ is constructed following the methodology of Caballero, Hoshi and Kashyap (2008):

$$R^*_{i,t} = rs_{t-1}BS_{i,t-1} + \left(\frac{1}{5}\sum_{j=1}^{5} rl_{t-j}\right) BL_{i,t-1} + rb_{t-1} \times Bonds_{i,t-1} \tag{28}$$

Where $BS_{i,t}$, $BL_{i,t}$ and $Bonds_{i,t}$ are short-term bank loans (maturity of less than one year), long-term bank loans (more than one year) and total bonds outstanding from firm i in year t; and rs_t, rl_t and rb_t are interest rates paid on short-term bank loans, long-term bank loans and corporate bonds, respectively.

We measure the lower bound interest rate on short-term loans (rs_t) with the Bank of Japan's short-term prime rate. With regards to long-term loans, we follow a similar approach and use the Bank of Japan's long-term prime rate[36]. However, TSR only reports the stock of long-term bank loans outstanding, without information on the exact maturity of the loans. Therefore, we follow Caballero,

[36]Short-term and long-term prime rates are obtained from the Bank of Japan database: https://www.boj.or.jp/en/statistics/dl/loan/prime/prime.htm/ The most frequent short-term prime rate is used. For every year, we take the minimum of the available short and long-term prime rates.

Hoshi and Kashyap (2008) and assume that each firm's long-term loans have an average maturity of 2.5 years and with one-fifth of them having been originated in each year for five years[37] This implies that the lower bound interest rate on long-term loans is an equally weighted average of the last five years of the long-term prime rates. Thus, we calculate the minimum required interest payment on long-term loans by multiplying the outstanding long-term loans of all maturities with the five-year average of the long-term prime rates. In our sample, short-term bank loans account for around 50% of total debt and long-term bank loans (due in more than 1 year) account for around 3.3% of total debt.

For bonds, we adopt an extremely conservative approach that assumes the minimum required interest rate is zero for the entire sample period. However, this assumption does not affect our results. First, bonds account on average for less than 1.8% of total debt in our sample. Therefore, the lower bound interest rate calculations are very insensitive to the assumption we make on the minimum required interest rate on bonds. Moreover, the assumption pushes down the lower bound interest rate $R^*_{i,t}$, and therefore would lead us to identify fewer zombie firms. Some papers in the literature (Caballero, Hoshi and Kashyap, 2008) have also assumed that bond financing uses only convertible bonds, which, by their nature, have lower yields. By assuming such low required interest rates on bonds, the approach reduces the risk of misclassifying creditworthy companies as zombies.

E Loan termination condition

Substituting Equation (9) in Equation (7), we can find the cutoff productivity values at which loan relationships are terminated. There will be one cutoff value for each market j. The credit line termination condition for market j is given by Equation (29),

$$(1-\eta)(\pi_{ijt}(z_s)^* - \pi_{0jt}(z_s)) - (1-\eta)\rho_{jt} - \eta\kappa\theta_{jt} + \eta\kappa^B_{jt} - \eta[\tau_{ijt} + p_{jt}\tau_{kjt} - \beta\tau_{ijt+1}] + \beta\frac{\kappa}{q_{jt+1}} - \beta\tau_{ijt+1} = -\tau_{ijt}. \tag{29}$$

The productivity cutoff $\tilde{z}_j$ is found by equating the value of a continuing relationship for the bank at the negotiated interest rate equal to zero.[38] Termination costs reduce the productivity cut-off. In other words, loan relationships that should be terminated are kept alive. We can rewrite this as Equation (30):

$$\pi_{ijt}(z_s)^* - \pi_{0jt}(z_s) = \hat{k}_{ijt}\rho_t + \eta\kappa^B_{jt} - \tau_{ijt} + \beta\tau_{ijt+1} - \frac{1}{1-\eta}\left[\eta\kappa\theta_{jt} + \eta p_{jt}\tau_{kjt} + \beta\frac{\kappa}{q_{jt+1}}\right] \tag{30}$$

Equation (30) shows that the cutoff productivity value is reached when the period surplus of production equals the outside value of the match for the bank minus the continuation value of the match. When the capital stock of the credit line $\hat{k}_{ijt}$ is freely adjustable, the cost of maintaining a credit line $\eta\kappa^B_{jt}$ vanishes. In this case, the cutoff value cannot be reached, as the match can be maintained costlessly by reducing $\hat{k}_{ijt}$ to zero.

Proposition 2. *When the capital of the credit line $\hat{k}_{ijt}$ is freely adjustable, maintaining a credit line is costless for the bank, and forbearance incentives are weakly positive and non-increasing over time, credit lines will not be severed for any productivity realisation z_{ijt}.*

[37] Five years corresponds to the average maturity of bank loans according to Smith (2003).

[38] Equivalently, we could set the value of a continuing match for the firm equal to zero, or the total surplus (firm + bank) equal to zero.

Proof: When $\hat{k}_{ijt}$ is freely adjustable and forbearance incentives are stable then for any productivity realisation $z_{ijt} \in \mathbb{R}$, $\hat{k}_{ijt} = 0$ is an option. In this case $\pi_{ijt}(z_s)^* - \pi_{0jt}(z_s) = 0$. Hence, Equation (30) becomes $0 > -\tau_{ij}(1-\beta) - \frac{1}{1-\eta}\left[\eta\kappa\theta_{jt} + \eta p_{jt}\tau_{kjt} + \beta\frac{\kappa}{q_{jt+1}}\right]$. This is automatically satisfied when $\tau_{ij} > 0$.

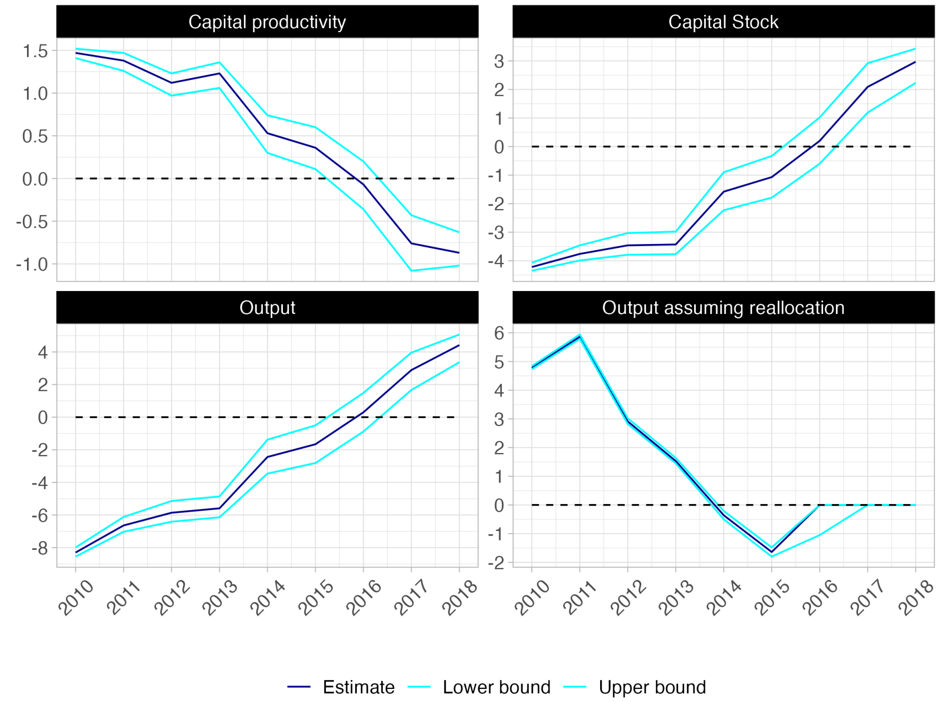
Therefore, exit from credit lines can only happen either when τ_{ij} is increasing strongly in the next period when capital is not freely adjustable for the firm, or κ^B_{jt} is sufficiently large. In this case, the match surplus may become negative as a low productivity realisation may result in the average marginal value created by the loan producing negative profit. We assume that the cost of maintaining a credit line is sufficiently large that the bank wants to sever credit lines below a certain productivity level, though sluggish adjustment in the size of the lent capital stock could deliver a similar result.

F Counterfactuals

F.1 Plotting the counterfactuals

Figure 5 plots the counterfactuals from Table 8 in the paper for a better illustration of the time series.

Figure 5: Effect of removing forbearance incentives (in %) - fixed labour



Notes: The lower and upper bounds of the counterfactuals are constructed using two standard deviations of the estimated effects on the interest rate and otherwise making the same assumptions as for the baseline estimate.

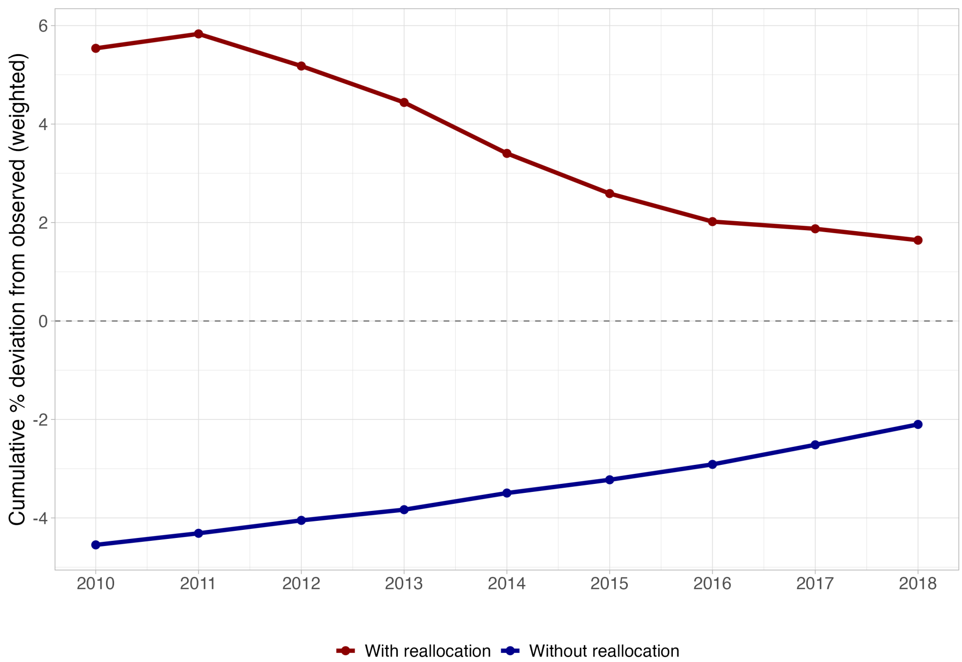
F.2 Cumulative counterfactual output effects

Table 8 in the paper reports annual percentage deviations between counterfactual and observed output. To complement these year-by-year comparisons, we construct a cumulative deviation measure defined as the ratio of cumulated level differences to cumulated observed output:

$$\text{Cumulative deviation}_t = \frac{\sum_{s=2010}^{t}(Y_s^{CF} - Y_s^{Obs})}{\sum_{s=2010}^{t} Y_s^{Obs}} \times 100$$

This measure captures the fraction of total output gained or lost due to the policy over the entire period up to year t. Figure 6 plots the resulting series for both the no-reallocation and reallocation scenarios. In the absence of reallocation, the cumulative output deviation reaches -2.1% by 2018, indicating that the policy cumulatively raised total output by approximately 2.1% of cumulated observed output over nine years. The deviation is largest in the early years (around -4.5% when only 2010 output enters the denominator) and attenuates as smaller annual effects are progressively added to the cumulation. Under the reallocation scenario, the cumulative deviation is $+1.6\%$ by 2018, implying that had capital been seamlessly redeployed, aggregate output over the entire period would have been cumulatively higher by 1.6% in the absence of the policy.

Figure 6: Cumulative counterfactual output effects



Notes: The figure plots the cumulative percentage deviation of counterfactual output from observed output, computed as $\sum_{s=2010}^{t}(Y_s^{CF} - Y_s^{Obs})/\sum_{s=2010}^{t} Y_s^{Obs} \times 100$. The blue line shows the scenario without capital reallocation; the red line shows the scenario with reallocation. Aggregates are employment-weighted.

F.3 Sensitivity to production function parameters

Table 4 reports how the average counterfactual deviations change when we replace the baseline Wooldridge (2009) WRDG-GMM production function parameters with the ACF estimates (Column (2) of Table 2). Both specifications use the same difference-in-differences treatment effects; only the production function parameters (α, γ) differ. The direct channels are robust to this choice: the average capital deviation is -1.19% under ACF versus -1.34% under WRDG, and the average output deviation without reallocation is -2.23% versus -2.30%. The output counterfactual with reallocation, however, is more sensitive to the estimator: it averages 4.49% under ACF compared with 1.94% under WRDG. This difference is driven by the substantially lower capital share under ACF

($\alpha = 0.171$ versus 0.281). A lower capital share implies higher aggregate output per unit of capital (Y/K), so each unit of freed capital that is reallocated to more productive firms generates a larger output gain. Because the reallocation counterfactual multiplies the freed capital by this aggregate capital productivity, the reallocation effect is amplified under ACF even though the amount of freed capital is similar. The direct output effects—which do not depend on the reallocation assumption—remain robust across estimators.

Table 4: Counterfactual sensitivity to production function parameters

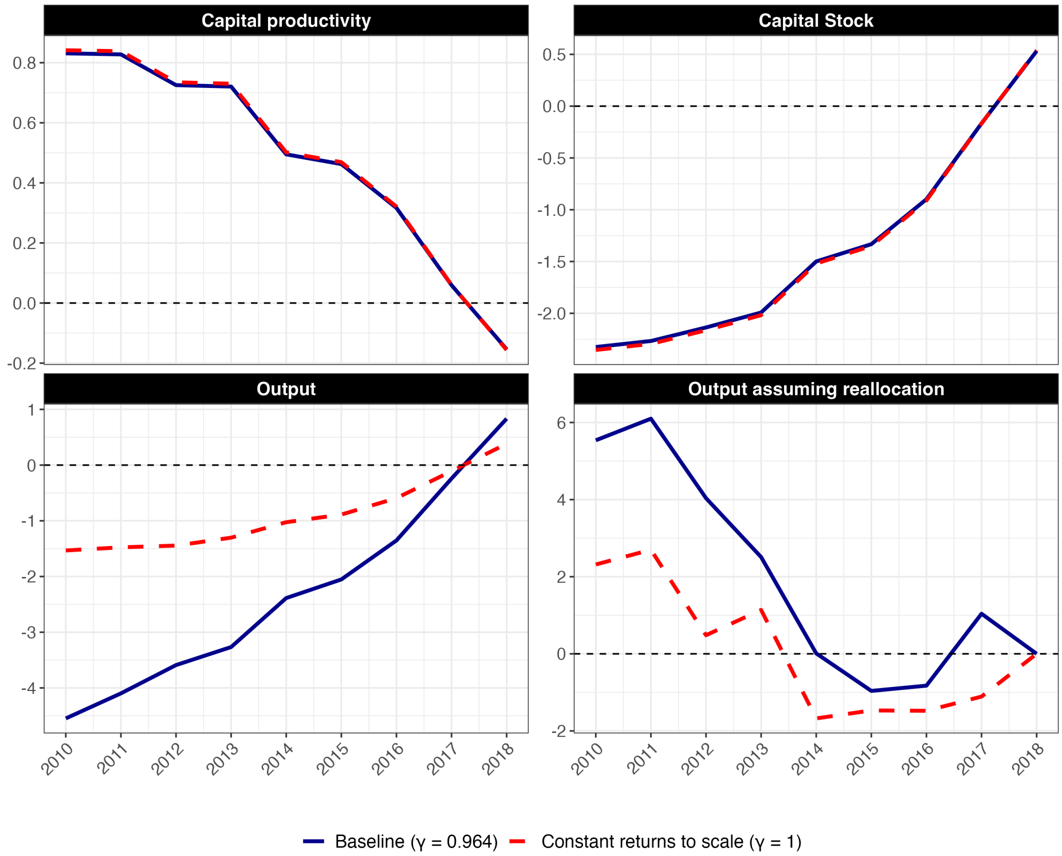
	Parameters			Avg. % Deviation (2010–18)		
Specification	α	γ	$\alpha\gamma$	Capital	Output	Output (reall.)
ACF	0.171	0.969	0.166	-1.19	-2.23	4.49
WRDG	0.281	0.964	0.271	-1.34	-2.30	1.94

Notes: Each row uses the same difference-in-differences treatment effects but different production function estimates to compute the counterfactual capital stock, output, and output with capital reallocation. Columns show the average percent deviation of the counterfactual from observed employment-weighted aggregates over 2010–2018.

F.4 Constant vs. decreasing returns to scale

Our baseline counterfactuals use the estimated returns to scale parameter $\gamma = 0.964$, implying mild decreasing returns. To assess the sensitivity of our results to this assumption, Figure 7 compares the baseline counterfactual ($\gamma = 0.964$, labelled "DRS") with an alternative specification that imposes constant returns to scale ($\gamma = 1$, labelled "CRS"), holding all other parameters fixed. The top row shows that neither the capital productivity response (top left) nor the capital stock response (top right) is materially affected by the returns to scale assumption: the CRS and DRS paths are nearly indistinguishable in both panels, though the capital stock response is marginally larger under CRS as expected from the larger capital multiplier $1/(1-\alpha\gamma)$ when $\gamma = 1$. The main differences emerge in the aggregate output counterfactuals (bottom row). Without reallocation (bottom left), the output effects are substantially larger under DRS than under CRS, and a similar pattern holds with reallocation (bottom right). The mechanism behind this amplification is an operating leverage effect: under DRS, the production function $z(k^{\alpha}l^{1-\alpha})^{\gamma}$ generates lower gross output for given inputs, but labour costs are subtracted in full, leaving a thinner net output base. A given capital stock adjustment therefore represents a proportionally larger percentage change in net output under DRS than under CRS. While the returns to scale assumption is thus inconsequential for the capital and capital productivity channels, it matters quantitatively for the aggregate output counterfactuals.

Figure 7: Counterfactuals under constant vs. decreasing returns to scale



Notes: Each panel compares the baseline counterfactual with decreasing returns to scale (DRS, $\gamma = 0.964$, solid blue) to an alternative with constant returns to scale (CRS, $\gamma = 1$, dashed red). Both specifications use the same WRDG-GMM capital share ($\alpha = 0.281$), treatment effects, and firm-level time-varying TFP. Employment-weighted aggregates.

F.5 DiD and counterfactuals with flexible labour input

Table 5 and Figure 8 below mirror Table 5 and Figure 5 in the paper and present the results of our DiD estimation of the treatment effects of the Act on interest rates, assuming flexible labour input. The only difference is that we are now allowing labour input to vary in the definition of the $APBK$:

$$APBK_{it} = \frac{(\pi_{ijt} - \pi_{i0jt})}{\hat{k}_{ijt}} = \frac{(z_{ijt}(k_{ijt}^{\alpha} l_{ijt}^{1-\alpha})^{\gamma} - wl_{ijt}) - (z_{ijt}(k_{i,0}^{\alpha} l_{ijt,0}^{1-\alpha})^{\gamma} - wl_{ijt,0})}{\hat{k}_{ijt}} \tag{31}$$

where l_{ijt} is the observed labour input when the firm uses both bank-funded capital and alternative resources and $l_{ijt,0}$ is the optimal labour input when the firm only uses non-bank-funded capital ($k_{ijt,0}$).

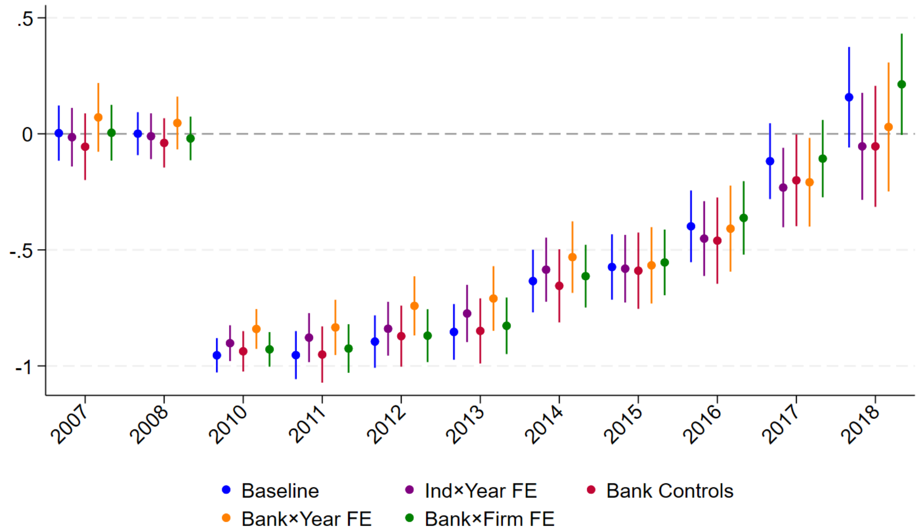
As expected, the results of the DiD estimation do not change much when we allow for flexible labour because the only variable that is affected by this assumption is $APBK_{it}$. The annual treatment effects reported in Table 5 below are of a similar magnitude to those in Table 5 in the paper and follow the same pattern. The pre-treatment coefficients for 2007 and 2008 are small and individually insignificant across all five specifications, supporting the parallel trends assumption.

Table 5: Effect of the Act on interest rates (flexible labour)

	(1) Baseline	(2) Ind×Year FE	(3) Bank Controls	(4) Bank×Year FE	(5) Bank×Firm FE
Pre-treatment coefficients					
2007 × Treatment Intensity	0.00305	-0.0145	-0.0554	0.0709	0.00478
	(0.0607)	(0.0644)	(0.0733)	(0.0756)	(0.0613)
2008 × Treatment Intensity	0.000833	-0.0102	-0.0390	0.0468	-0.0199
	(0.0472)	(0.0503)	(0.0542)	(0.0581)	(0.0480)
Post-treatment coefficients					
2010 × Treatment Intensity	-0.954***	-0.902***	-0.937***	-0.841***	-0.929***
	(0.0377)	(0.0394)	(0.0444)	(0.0438)	(0.0380)
2011 × Treatment Intensity	-0.953***	-0.878***	-0.951***	-0.834***	-0.925***
	(0.0528)	(0.0539)	(0.0618)	(0.0608)	(0.0533)
2012 × Treatment Intensity	-0.895***	-0.840***	-0.872***	-0.741***	-0.870***
	(0.0576)	(0.0592)	(0.0671)	(0.0650)	(0.0582)
2013 × Treatment Intensity	-0.854***	-0.774***	-0.849***	-0.710***	-0.827***
	(0.0613)	(0.0629)	(0.0716)	(0.0711)	(0.0621)
2014 × Treatment Intensity	-0.634***	-0.585***	-0.655***	-0.531***	-0.613***
	(0.0688)	(0.0705)	(0.0804)	(0.0787)	(0.0690)
2015 × Treatment Intensity	-0.574***	-0.581***	-0.590***	-0.567***	-0.554***
	(0.0719)	(0.0744)	(0.0839)	(0.0839)	(0.0722)
2016 × Treatment Intensity	-0.398***	-0.451***	-0.460***	-0.409***	-0.362***
	(0.0788)	(0.0822)	(0.0949)	(0.0946)	(0.0808)
2017 × Treatment Intensity	-0.118	-0.231***	-0.200**	-0.208**	-0.107
	(0.0834)	(0.0873)	(0.101)	(0.0975)	(0.0850)
2018 × Treatment Intensity	0.158	-0.0539	-0.0539	0.0296	0.214*
	(0.110)	(0.118)	(0.133)	(0.142)	(0.111)
Firm FE	Yes	Yes	Yes	Yes	No
Year FE	Yes	No	Yes	No	Yes
Industry×Year FE	No	Yes	No	No	No
Bank×Year FE	No	No	No	Yes	No
Bank×Firm FE	No	No	No	No	Yes
Bank Controls	No	No	Yes	No	No
Firm Controls × Post	Yes	Yes	Yes	Yes	Yes
Observations	606,455	605,248	433,734	547,060	593,932
Adj. R^2	0.776	0.776	0.762	0.788	0.786
Pre-trends p-value: $\beta_{2007} = \beta_{2008} = 0$	0.999	0.970	0.701	0.603	0.855

Notes: This table reports the estimated treatment effects for five fixed-effects specifications of Equation (15) in the paper, assuming flexible labour input. Dependent variable: $\ln(r_{it})$. Treatment Intensity constructed from 2009 firm characteristics and year-specific probit intercepts. Baseline year: 2009 (omitted). Standard errors clustered at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 8: Event-study plot



Notes: This figure plots the event-study coefficients from Table 5 across all five specifications. Each coefficient represents the interaction between Treatment Intensity and a year dummy, with 2009 as the omitted baseline year. The pre-treatment coefficients for 2007 and 2008 are small and statistically indistinguishable from zero across all specifications, supporting the parallel trends assumption. Following the introduction of the SME Financing Facilitation Act in 2009, firms with higher treatment intensity experience a sharp and persistent decline in $\ln(r_{it})$, with effects largest in 2010–2011 and gradually attenuating toward zero by 2017–2018. The five specifications — Baseline, Industry×Year FE, Bank Controls, Bank×Year FE, and Bank×Firm FE — yield broadly consistent estimates throughout the sample period. Capped spikes represent 95% confidence intervals based on standard errors clustered at the firm level.

The counterfactuals, by contrast, are affected by the assumption of flexible labour. Under flexible labour, firms choose their optimal capital stock given the optimal adjustment in labour input. Without loss of generality, we normalise wages to 1. Optimal labour input is where the marginal product of labour equals the marginal cost, i.e.:

$$l_{ijt} = \left[(1-\alpha)\gamma z_{ijt} k_{ijt}^{\alpha\gamma}\right]^{\frac{1}{1-\gamma(1-\alpha)}}. \tag{32}$$

Optimal capital input is then given by:

$$k_{ijt} = \left(\frac{\alpha\gamma z_{ijt} l_{ijt}^{\gamma(1-\alpha)}}{r_{ijt}}\right)^{\frac{1}{1-\alpha\gamma}}. \tag{33}$$

Combining these two equations, a change in the interest rate affects the firm's capital stock according to Equation (34):

$$\Delta \log(k_{ijt}) = -\left(1 - \frac{\alpha\gamma}{1-\gamma(1-\alpha)}\right)^{-1} \Delta \log(r_{i,j,t}) \tag{34}$$

The multiplier $\left(1 - \frac{\alpha\gamma}{1-\gamma(1-\alpha)}\right)^{-1}$ exceeds $\frac{1}{1-\alpha\gamma}$ in Equation (21) because reducing capital lowers the marginal product of labour, which reduces optimal employment, further depressing output. The change in optimal labour input is:

$$\Delta \log(l_{ijt}) = \frac{\alpha\gamma}{1-\gamma(1-\alpha)} \Delta \log(k_{i,j,t}) \tag{35}$$

The counterfactual procedure follows the same four steps as in Section 6. In Step 1, we use Equation (34) instead of Equation (21) in the paper to compute firm-level counterfactual capital stocks, with the same truncation of the capital multiplier to the interval $[0.85, 1.15]$. In Step 2, we compute counterfactual labour using Equation (35) and then substitute both adjusted capital and labour inputs into the production function to obtain counterfactual output. Steps 3 and 4 (aggregation and reallocation) proceed as in the baseline.

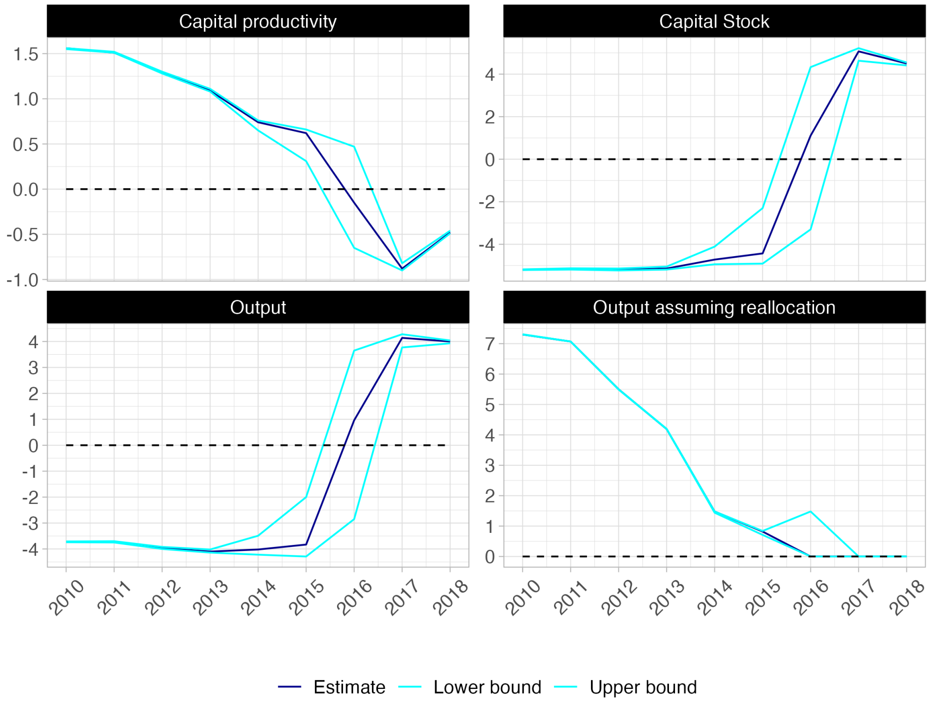
Table 6 presents the results. They are qualitatively similar to those of Table 8 in the paper, but the magnitudes are affected by adjustments in labour input. The aggregate capital stock would have declined by 5.0% (vs. 2.3%) in 2010 and by about 2.8% (vs. 1.3%) on average if the policy had not been enacted. On average, the Act depressed capital productivity by about 0.7% (vs. 0.5%). Without reallocation, output would have declined by 3.6% (vs. 4.6%) in 2010 and 2.2% (vs. 2.3%) on average. With reallocation, the policy is estimated to have decreased output by around 7.1% (vs. 5.5%) in 2010 and by about 3.0% (vs. 1.9%) on average.

Table 6: Effect of removing the estimated treatment effects (in %) - flexible labour

Counterfactuals - % Change	**2010**	**2011**	**2012**	**2013**	**2014**	**2015**	**2016**	**2017**	**2018**	**Average**
Capital stock	-5.01 %	-5.02 %	-5.00 %	-4.93 %	-4.55 %	-4.28 %	-2.76 %	1.86 %	4.13 %	-2.84 %
Capital productivity	1.53 %	1.52 %	1.30 %	1.12 %	0.74 %	0.59 %	0.31 %	-0.27 %	-0.30 %	0.73 %
Output, without reallocation	-3.55 %	-3.58 %	-3.76 %	-3.87 %	-3.84 %	-3.72 %	-2.45 %	1.58 %	3.82 %	-2.15 %
Output, with reallocation	7.09 %	7.07 %	5.59 %	4.31 %	1.82 %	0.59 %	0.54 %	0.00 %	0.00 %	3.00 %

Figure 9 plots the counterfactuals from Table 6 for a better illustration of the time series of counterfactuals with flexible labour.

Figure 9: Effect of removing the forbearance incentives (in %) - flexible labour



Notes: The lower and upper bounds of the counterfactuals are constructed using two standard deviations of the estimated effects on the interest rate and otherwise making the same assumptions as for the baseline estimate.

G Full estimation results for firm performance

This section presents the full estimation results for the firm performance regressions reported in Section 5.2 of the paper. Table 7 in the paper reports the baseline specification with firm and year fixed effects for each outcome. Here we present the full set of progressively saturated specifications, event-study plots, and parallel trends sensitivity analyses for each outcome variable. The full estimating equation is:

$$g(Y_{it}) = \alpha + \sum_{t \neq 2009} \beta_t D_t \text{Treatment Intensity}_{it} + \sum_{c} \gamma_c \text{Post}_t X^c_{it} + \Lambda Z_{bt} + f_{(\cdot)} + \epsilon_{it} \quad (36)$$

where $g(Y_{it})$ is the appropriate transformation of the outcome variable, X^c_{it} are firm-level controls interacted with a post-treatment indicator, Z_{bt} are bank controls, and $f_{(\cdot)}$ denotes progressively saturated fixed effects as described in Section 5.1.1. Five specifications are estimated for each outcome: (1) baseline with firm and year FE; (2) industry×year FE; (3) bank controls; (4) bank×year FE; (5) bank×firm FE. For exit, firm fixed effects are excluded from all specifications because exit is a rare, absorbing event, and only four specifications are reported.

For each outcome, we report the pre-treatment coefficients β_{2007} and β_{2008} and the estimated pre-trend $\hat{\delta}_{\text{pre}} = \beta_{2008} - \beta_{2007}$. We assess the sensitivity of our treatment effect estimates to potential violations of parallel trends using the methodology of Rambachan and Roth (2023). The breakdown point $\bar{M}^*$ indicates the maximum post-treatment violation of parallel trends (as a multiple of the estimated pre-trend) at which the confidence interval for the average treatment effect remains bounded away from zero.

G.1 Zombie status (two-tiered definition)

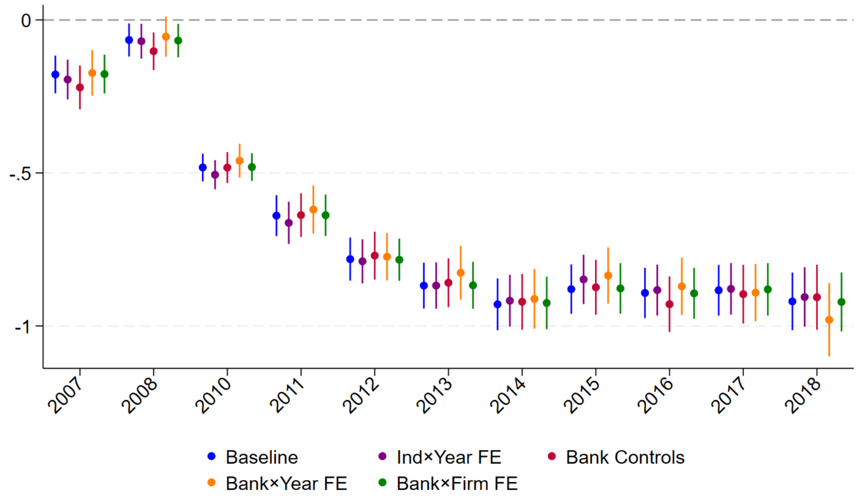
Table 7 presents the full LPM results for the two-tiered zombie definition across all five specifications. The estimated treatment effects are significantly negative in every post-treatment year across all specifications. The results are remarkably stable (see Figure 10): the point estimates vary little across specifications, indicating that the findings are not driven by omitted industry-level shocks, bank characteristics, or time-invariant bank-firm match effects.

Table 7: Effect of the Act on zombie status (two-tiered definition)

	(1) Baseline	(2) Ind×Year FE	(3) Bank Controls	(4) Bank×Year FE	(5) Bank×Firm FE
Pre-treatment coefficients					
2007 × Treatment Intensity	-0.178***	-0.195***	-0.220***	-0.173***	-0.177***
	(0.0314)	(0.0331)	(0.0367)	(0.0381)	(0.0323)
2008 × Treatment Intensity	-0.0655**	-0.0693**	-0.102***	-0.0546	-0.0674**
	(0.0275)	(0.0290)	(0.0313)	(0.0335)	(0.0279)
Post-treatment coefficients					
2010 × Treatment Intensity	-0.482***	-0.506***	-0.482***	-0.460***	-0.481***
	(0.0231)	(0.0242)	(0.0258)	(0.0282)	(0.0231)
2011 × Treatment Intensity	-0.639***	-0.663***	-0.638***	-0.619***	-0.638***
	(0.0341)	(0.0351)	(0.0365)	(0.0402)	(0.0345)
2012 × Treatment Intensity	-0.782***	-0.789***	-0.770***	-0.774***	-0.784***
	(0.0359)	(0.0367)	(0.0399)	(0.0397)	(0.0351)
2013 × Treatment Intensity	-0.868***	-0.868***	-0.859***	-0.826***	-0.867***
	(0.0382)	(0.0386)	(0.0407)	(0.0449)	(0.0391)
2014 × Treatment Intensity	-0.929***	-0.917***	-0.921***	-0.911***	-0.925***
	(0.0433)	(0.0432)	(0.0466)	(0.0498)	(0.0438)
2015 × Treatment Intensity	-0.880***	-0.848***	-0.873***	-0.835***	-0.877***
	(0.0412)	(0.0411)	(0.0457)	(0.0468)	(0.0421)
2016 × Treatment Intensity	-0.892***	-0.883***	-0.929***	-0.870***	-0.893***
	(0.0419)	(0.0425)	(0.0463)	(0.0478)	(0.0425)
2017 × Treatment Intensity	-0.883***	-0.879***	-0.896***	-0.891***	-0.880***
	(0.0423)	(0.0431)	(0.0489)	(0.0478)	(0.0438)
2018 × Treatment Intensity	-0.920***	-0.905***	-0.906***	-0.980***	-0.921***
	(0.0479)	(0.0495)	(0.0542)	(0.0611)	(0.0492)
Firm FE	Yes	Yes	Yes	Yes	No
Year FE	Yes	No	Yes	No	Yes
Industry×Year FE	No	Yes	No	No	No
Bank×Year FE	No	No	No	Yes	No
Bank×Firm FE	No	No	No	No	Yes
Bank Controls	No	No	Yes	No	No
Firm Controls × Post	Yes	Yes	Yes	Yes	Yes
Observations	606,455	605,248	433,734	547,060	593,932
Adj. R^2	0.403	0.403	0.403	0.402	0.409
Pre-trends p-value: $\beta_{2007} = \beta_{2008} = 0$	0.000	0.000	0.000	0.000	0.000

Notes: Dependent variable: zombie firm indicator (two-tiered definition). Linear probability model estimated by OLS. Treatment Intensity constructed from 2009 firm characteristics with time-varying intercepts estimated from the RIETI survey. Baseline year: 2009 (omitted). Standard errors clustered at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 10: Event-study plot: zombie status (two-tiered definition)

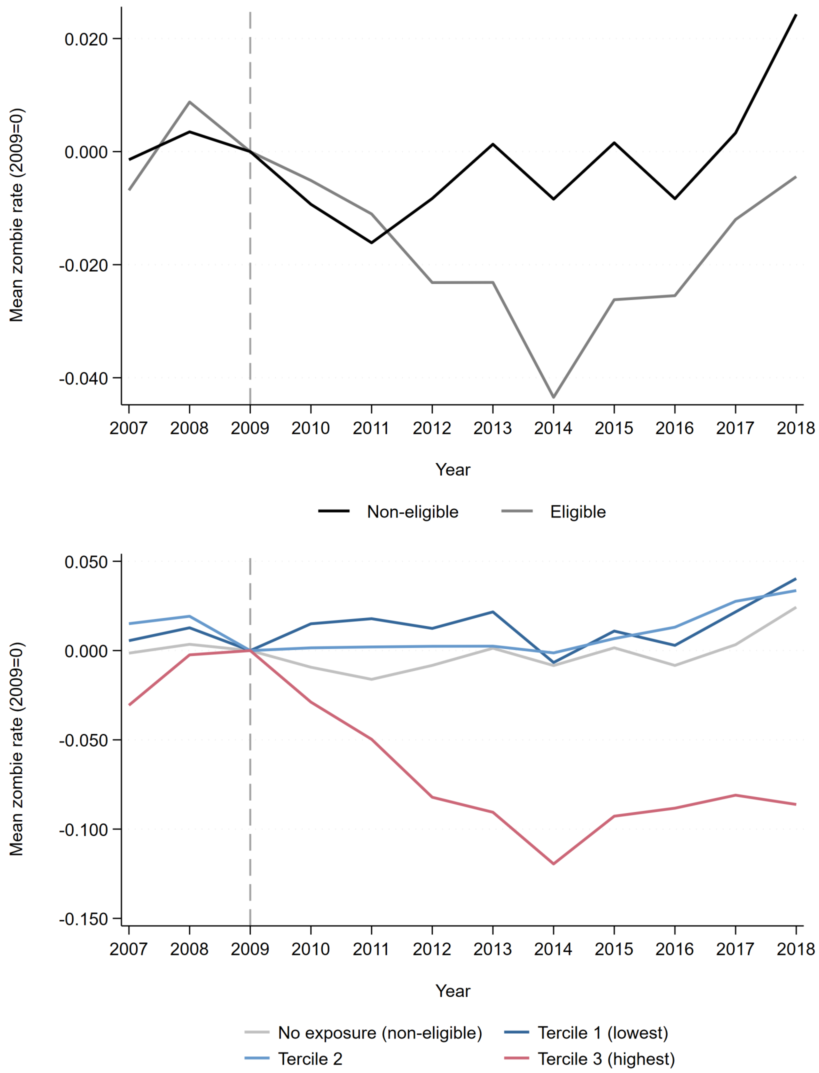


Notes: Event-study coefficients from Table 7. Each coefficient represents the interaction between Treatment Intensity and a year dummy, with 2009 as the omitted baseline year. The dependent variable is the zombie firm indicator based on the two-tiered definition (subsidized credit and financial distress). Linear probability model. Capped spikes represent 95% confidence intervals based on standard errors clustered at the firm level.

Parallel trends analysis

Figure 11 plots mean zombie rates, normalized to zero in 2009. In the top panel, both eligible and non-eligible firms move in broadly similar directions before 2009, rising slightly from 2007 to 2008 before declining to their 2009 levels, though the eligible group exhibits somewhat larger fluctuations. After 2009, eligible firms experience a sharp decline in zombie rates relative to non-eligible firms. The gap between the two groups widens steadily through 2014, at which point it begins to partially narrow as eligible firms' zombie rates recover somewhat. Non-eligible firms' zombie rate, by contrast, remains relatively stable throughout the sample period, drifting only modestly upward toward the end of the panel. In the bottom panel, the tercile decomposition reveals clear heterogeneity in pre-treatment dynamics. Before 2009, Tercile 3 (highest-exposure) firms were on an upward trajectory, with their zombie rates *rising* toward the 2009 baseline. Terciles 1 and 2 and the non-eligible group, by contrast, remain largely flat throughout the pre-treatment period. Importantly, this pre-treatment upward trend for the highest-exposure firms runs in the *opposite* direction to the large negative post-treatment effects observed for this group, implying that any bias from differential pre-trends would work against finding our result and rendering our estimates conservative. After 2009, the dose-response pattern is striking: Tercile 3 firms show a dramatic and sustained reduction in zombie rates, reaching a trough around 2014 before stabilizing at a persistently lower level. Terciles 1 and 2, meanwhile, display only modest movements.

Figure 11: Mean zombie rate, two-tiered definition (2009=0), by eligibility and exposure tercile



Notes: This figure plots mean zombie rates, normalized to zero in 2009, using our two-tiered definition (subsidized credit and financial distress). The top panel compares eligible and non-eligible firms. The bottom panel disaggregates eligible firms into terciles of treatment intensity (based on the time-invariant 2009 measure) and includes non-eligible firms (no exposure) as a reference group. The dashed vertical line marks the onset of the SME Financing Facilitation Act (2009). Source: TSR.

Panel A of Table 8 reports the pre-treatment coefficients. Both $\beta_{2007} = -0.178$ and $\beta_{2008} = -0.066$ are negative and significant, indicating that in 2007 and 2008, firms with higher treatment intensity had *lower* zombie rates relative to their own 2009 level than did firms with lower treatment intensity. Put differently, high-exposure firms were increasingly likely to be classified as zombies as the financial crisis deepened. The joint F-test strongly rejects the null that both pre-treatment coefficients are zero ($F = 16.17$, $p < 0.001$).

The estimated pre-trend is $\hat{\delta}_{\text{pre}} = \beta_{2008} - \beta_{2007} = 0.113$ ($p < 0.001$), which is positive and significant: the coefficients were *increasing* over time in the pre-treatment period. This means that the zombie rate for high-exposure firms was *accelerating* relative to low-exposure firms. Since our post-treatment effects are negative (the Act *reduced* zombie rates for high-exposure firms), this positive pre-trend runs in the *opposite* direction to the treatment effects. Any continuation of this accelerating zombification post-2009 would bias us toward finding *smaller* (or positive) treatment effects, not the large negative effects we estimate.

Panel B reports the average post-treatment effect of -0.808 ($p < 0.001$), which is roughly seven times larger in absolute value than the pre-trend. The effect is large and immediate ($\beta_{2010} = -0.482$, $p < 0.001$), grows through 2014 ($\beta_{2014} = -0.929$), and remains stable thereafter ($\beta_{2018} = -0.919$). Panel C presents the sensitivity analysis. The breakdown point is $\bar{M}^* = 3$: the confidence interval for the average treatment effect remains bounded away from zero even when allowing for post-treatment trend deviations of more than three times the estimated pre-trend in each period. At $\bar{M} = 3$, the upper bound of the confidence interval is 0.009—just barely above zero, confirming that our results are robust.

Table 8: Sensitivity analysis: Pre-trends and robustness (Zombie dummy, two-tiered definition)

	Coefficient	Std. Error	p-value	Notes
Panel A: Pre-treatment period (2007–2008)				
2007 coefficient (β_{2007})	-0.1784	0.0314	0.000	Relative to 2009
2008 coefficient (β_{2008})	-0.0657	0.0275	0.017	Relative to 2009
Pre-trend ($\hat{\delta}_{\text{pre}}$)	**0.1127**	**0.0305**	**0.000**	$= \boldsymbol{\beta_{2008} - \beta_{2007}}$
Joint F-test	$F = 16.17$		0.000	$H_0 : \beta_{2007} = \beta_{2008} = 0$
Panel B: Treatment effects (2010–2018)				
Average effect ($\bar{\beta}_{\text{post}}$)	**-0.8080**	**0.0331**	**0.000**	**Average across 9 years**
2010 (immediate)	-0.4818	0.0231	0.000	First year
2014 (mid-period)	-0.9290	0.0433	0.000	5 years post-treatment
2018 (final year)	-0.9193	0.0479	0.000	9 years post-treatment
Panel C: Rambachan & Roth (2023) Sensitivity Analysis				
Breakdown point ($\bar{M}^*$)	**3**			CI crosses zero at $\bar{M}^*$
Pre-trend as % of avg effect	13.9%			Relative magnitude
Upper CI bound at $\bar{M} = 3$	0.009			Sign at max violation

Notes: This table reports pre-treatment coefficients, treatment effects, and sensitivity analysis following Rambachan and Roth (2023). The dependent variable is the two-tiered zombie indicator (subsidized credit and financial distress). All regressions include firm and year fixed effects. Standard errors are clustered at the firm level. The breakdown point $\bar{M}^*$ indicates the value at which confidence intervals for the average treatment effect first include zero when allowing for post-treatment violations of parallel trends up to $\bar{M}$ times the estimated pre-trend.

G.2 Zombie status (single-tiered definition)

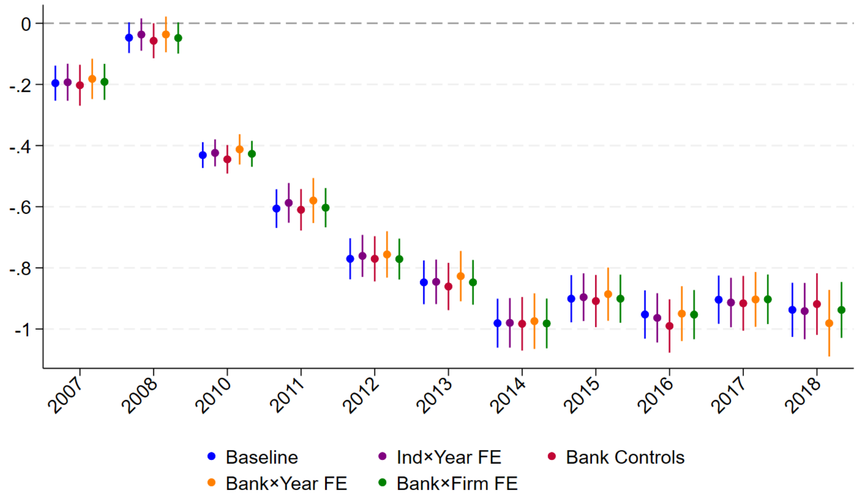
Table 9 presents the full LPM results for the single-tiered zombie definition. The results closely mirror those for the two-tiered definition, with significantly negative treatment effects in every post-treatment year across all specifications.

Table 9: Effect of the Act on zombie status (single-tiered definition)

	(1) Baseline	(2) Ind×Year FE	(3) Bank Controls	(4) Bank×Year FE	(5) Bank×Firm FE
Pre-treatment coefficients					
2007 × Treatment Intensity	-0.196***	-0.193***	-0.203***	-0.182***	-0.192***
	(0.0293)	(0.0309)	(0.0342)	(0.0336)	(0.0300)
2008 × Treatment Intensity	-0.0472*	-0.0369	-0.0573**	-0.0365	-0.0482*
	(0.0256)	(0.0270)	(0.0292)	(0.0298)	(0.0260)
Post-treatment coefficients					
2010 × Treatment Intensity	-0.431***	-0.424***	-0.445***	-0.412***	-0.427***
	(0.0215)	(0.0225)	(0.0238)	(0.0253)	(0.0217)
2011 × Treatment Intensity	-0.606***	-0.587***	-0.610***	-0.580***	-0.603***
	(0.0323)	(0.0331)	(0.0346)	(0.0375)	(0.0327)
2012 × Treatment Intensity	-0.770***	-0.761***	-0.770***	-0.756***	-0.771***
	(0.0343)	(0.0350)	(0.0377)	(0.0387)	(0.0341)
2013 × Treatment Intensity	-0.847***	-0.846***	-0.861***	-0.827***	-0.847***
	(0.0365)	(0.0371)	(0.0395)	(0.0420)	(0.0373)
2014 × Treatment Intensity	-0.981***	-0.980***	-0.983***	-0.974***	-0.982***
	(0.0408)	(0.0414)	(0.0446)	(0.0464)	(0.0416)
2015 × Treatment Intensity	-0.901***	-0.896***	-0.908***	-0.886***	-0.901***
	(0.0393)	(0.0398)	(0.0435)	(0.0444)	(0.0401)
2016 × Treatment Intensity	-0.952***	-0.963***	-0.990***	-0.950***	-0.953***
	(0.0403)	(0.0411)	(0.0445)	(0.0457)	(0.0410)
2017 × Treatment Intensity	-0.904***	-0.913***	-0.916***	-0.903***	-0.903***
	(0.0403)	(0.0413)	(0.0457)	(0.0457)	(0.0414)
2018 × Treatment Intensity	-0.937***	-0.941***	-0.918***	-0.981***	-0.938***
	(0.0452)	(0.0470)	(0.0514)	(0.0556)	(0.0467)
Firm FE	Yes	Yes	Yes	Yes	No
Year FE	Yes	No	Yes	No	Yes
Industry×Year FE	No	Yes	No	No	No
Bank×Year FE	No	No	No	Yes	No
Bank×Firm FE	No	No	No	No	Yes
Bank Controls	No	No	Yes	No	No
Firm Controls × Post	Yes	Yes	Yes	Yes	Yes
Observations	606,455	605,248	433,734	547,060	593,932
Adj. R^2	0.510	0.505	0.506	0.515	0.513
Pre-trends p-value: $\beta_{2007} = \beta_{2008} = 0$	0.000	0.000	0.000	0.000	0.000

Notes: Dependent variable: zombie firm indicator (single-tiered definition). Linear probability model estimated by OLS. Treatment Intensity constructed from 2009 firm characteristics with time-varying intercepts estimated from the RIETI survey. Baseline year: 2009 (omitted). Standard errors clustered at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 12: Event-study plot: zombie status (single-tiered definition)

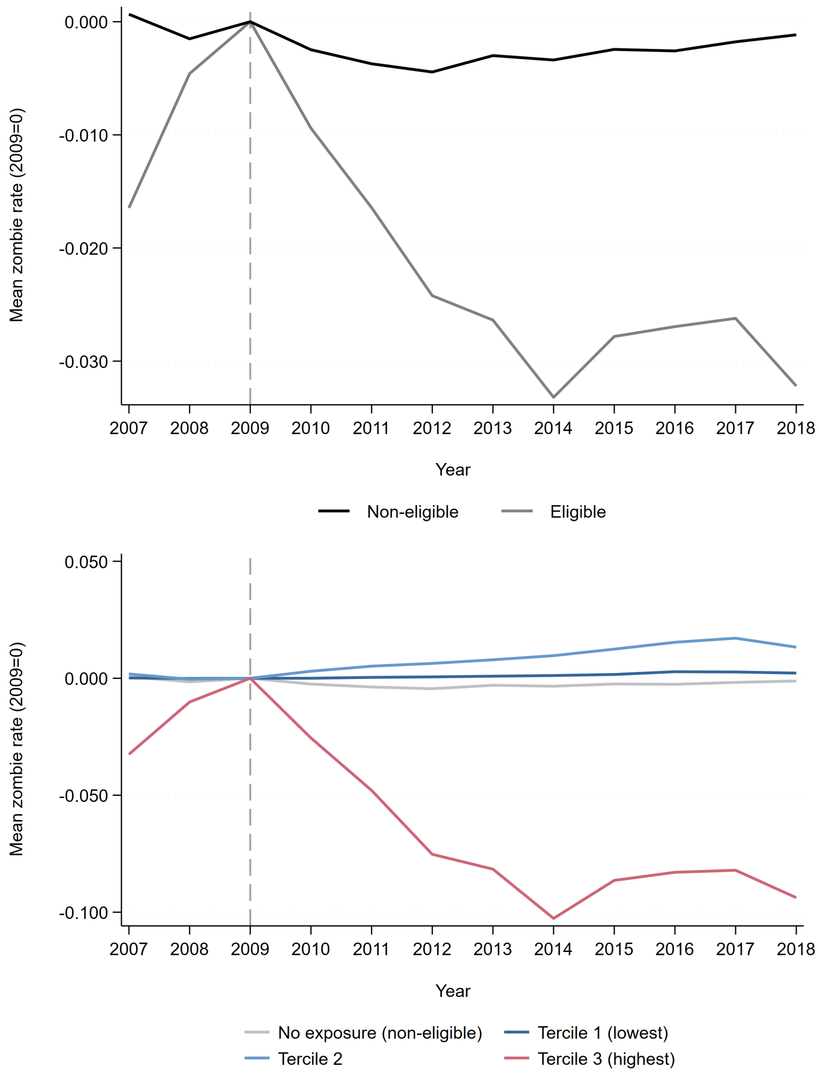


Notes: Event-study coefficients from Table 9. Each coefficient represents the interaction between Treatment Intensity and a year dummy, with 2009 as the omitted baseline year. The dependent variable is the zombie firm indicator based on the single-tiered definition (financial distress only). Linear probability model. Capped spikes represent 95% confidence intervals based on standard errors clustered at the firm level.

Parallel trends analysis

Figure 13 plots mean zombie rates under the single-tiered definition, normalized to zero in 2009. In the top panel, the non-eligible group is essentially flat before 2009, hovering close to zero throughout the pre-treatment period. The eligible group, by contrast, drifts upward toward its 2009 level, suggesting a differential pre-trend in which eligible firms were increasingly classified as zombies during the crisis years. After 2009, eligible firms show a substantial and sustained decline in zombie rates, with the gap between the two groups widening through 2014 before stabilizing at a persistently lower level. Non-eligible firms, meanwhile, remain essentially flat throughout the entire sample period, exhibiting only a slight dip in the years immediately following 2009. In the bottom panel, the tercile decomposition again reveals important heterogeneity in pre-treatment dynamics. Before 2009, Tercile 3 (highest-exposure) firms' zombie rates were rising toward their 2009 level, indicating that these firms were increasingly becoming zombies during the crisis. Terciles 1 and 2 and the non-eligible group, by contrast, remain flat near zero throughout the pre-treatment period. As with the two-tiered measure, this pre-treatment upward trend for the highest-exposure firms runs counter to the large negative post-treatment effects, implying that any bias from differential pre-trends would work against finding our result and rendering our estimates conservative. After 2009, Tercile 3 firms again exhibit the largest decline, with zombie rates falling sharply through 2014 before partially recovering. Terciles 1 and 2 show only minimal movement, remaining close to zero throughout the post-treatment period.

Figure 13: Mean zombie rate, single-tiered definition (2009=0), by eligibility and exposure tercile



Notes: This figure plots mean zombie rates for the single-tiered definition (financial distress), normalized to zero in 2009. The top panel compares eligible and non-eligible firms. The bottom panel disaggregates eligible firms into terciles of treatment intensity (based on the time-invariant 2009 measure) and includes non-eligible firms (no exposure) as a reference group. The dashed vertical line marks the onset of the SME Financing Facilitation Act (2009). Source: TSR.

Table 10 confirms the similarity of the results with the two-tiered zombie definition. Panel A reports $\beta_{2007} = -0.196$ and $\beta_{2008} = -0.047$, both negative, with the joint F-test strongly rejecting the null ($F = 23.35$, $p < 0.001$). The pre-trend is $\hat{\delta}_{\text{pre}} = 0.149$ ($p < 0.001$), positive and significant, indicating that the zombie rate differential between high- and low-exposure firms was accelerating in the pre-treatment period: high-exposure firms were becoming zombies at an increasing rate relative to low-exposure firms. As with the two-tiered measure, this pre-trend runs in the *opposite* direction to the negative post-treatment effects and therefore works against our findings.

Panel B reports the average post-treatment effect of -0.814 ($p < 0.001$), roughly five times larger in absolute value than the pre-trend. The dynamic pattern is similar: an immediate effect ($\beta_{2010} = -0.431$), peaking around 2014 ($\beta_{2014} = -0.981$), and remaining large through 2018 ($\beta_{2018} = -0.937$). Panel

C shows a breakdown point of $\bar{M}^* = 2.25$, with the upper CI bound at 0.012 at that point. This is slightly lower than for the two-tiered measure, reflecting the somewhat larger pre-trend relative to the treatment effect (18.3% versus 13.9%), but nonetheless represents a high degree of robustness.

Table 10: Sensitivity analysis: Pre-trends and robustness (Zombie dummy, single-tiered definition)

	Coefficient	Std. Error	p-value	Notes
Panel A: Pre-treatment period (2007–2008)				
2007 coefficient (β_{2007})	-0.1958	0.0293	0.000	Relative to 2009
2008 coefficient (β_{2008})	-0.0471	0.0256	0.066	Relative to 2009
Pre-trend ($\hat{\delta}_{\text{pre}}$)	**0.1486**	**0.0286**	**0.000**	$= \boldsymbol{\beta_{2008} - \beta_{2007}}$
Joint F-test	$F = 23.35$		0.000	$H_0 : \beta_{2007} = \beta_{2008} = 0$
Panel B: Treatment effects (2010–2018)				
Average effect ($\bar{\beta}_{\text{post}}$)	**-0.8143**	**0.0325**	**0.000**	**Average across 9 years**
2010 (immediate)	-0.4311	0.0215	0.000	First year
2014 (mid-period)	-0.9807	0.0408	0.000	5 years post-treatment
2018 (final year)	-0.9370	0.0452	0.000	9 years post-treatment
Panel C: Rambachan & Roth (2023) Sensitivity Analysis				
Breakdown point ($\bar{M}^*$)	**2.25**			CI crosses zero at $\bar{M}^*$
Pre-trend as % of avg effect	18.3%			Relative magnitude
Upper CI bound at $\bar{M} = 2.25$	0.012			Sign at max violation

Notes: This table reports pre-treatment coefficients, treatment effects, and sensitivity analysis following Rambachan and Roth (2023). The dependent variable is the single-tiered zombie indicator. All regressions include firm and year fixed effects. Standard errors are clustered at the firm level. The breakdown point $\bar{M}^*$ indicates the value at which confidence intervals for the average treatment effect first include zero when allowing for post-treatment violations of parallel trends up to $\bar{M}$ times the estimated pre-trend.

G.3 Firm exit (bankruptcy)

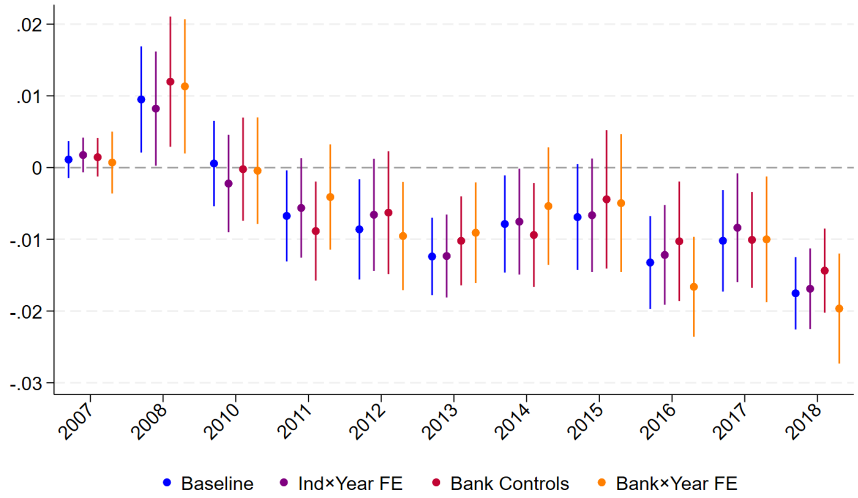
Table 11 presents the full LPM results for firm exit through bankruptcy across all four specifications (firm fixed effects are excluded throughout, as exit is a rare, absorbing event). The pre-treatment coefficients reveal a significant positive coefficient for 2008 across all specifications, indicating that higher-exposure firms experienced elevated bankruptcy rates during the financial crisis relative to the 2009 baseline. This is consistent with the crisis disproportionately affecting the types of firms that would later benefit most from the Act. After 2009, the estimates turn consistently negative, with the effects growing in magnitude over time and reaching their largest values in 2018. The pattern is broadly stable across all four specifications, though the inclusion of $Bank \times Year$ fixed effects in Column (4) attenuates some of the mid-period coefficients. The joint pre-trends test rejects equality of the 2007 and 2008 coefficients to zero at conventional levels in most specifications, driven primarily by the 2008 crisis-year coefficient. As discussed in the main text, this pre-treatment pattern runs in the opposite direction to the post-treatment effects, implying that any resulting bias would work against our findings.

Table 11: Effect of the Act on firm exit through bankruptcy

	(1) Baseline	(2) Ind×Year FE	(3) Bank Controls	(4) Bank×Year FE
Pre-treatment coefficients				
2007 × Treatment Intensity	0.00112	0.00175	0.00145	0.000708
	(0.00131)	(0.00124)	(0.00137)	(0.00220)
2008 × Treatment Intensity	0.00950**	0.00822**	0.0120***	0.0113**
	(0.00378)	(0.00406)	(0.00463)	(0.00478)
Post-treatment coefficients				
2010 × Treatment Intensity	0.000571	-0.00222	-0.000223	-0.000436
	(0.00304)	(0.00347)	(0.00367)	(0.00379)
2011 × Treatment Intensity	-0.00674**	-0.00563	-0.00885**	-0.00411
	(0.00323)	(0.00354)	(0.00352)	(0.00375)
2012 × Treatment Intensity	-0.00861**	-0.00658*	-0.00628	-0.00954**
	(0.00357)	(0.00399)	(0.00437)	(0.00385)
2013 × Treatment Intensity	-0.0124***	-0.0123***	-0.0102***	-0.00908**
	(0.00276)	(0.00295)	(0.00316)	(0.00358)
2014 × Treatment Intensity	-0.00787**	-0.00754**	-0.00940**	-0.00537
	(0.00345)	(0.00376)	(0.00368)	(0.00418)
2015 × Treatment Intensity	-0.00691*	-0.00665*	-0.00443	-0.00496
	(0.00376)	(0.00404)	(0.00493)	(0.00490)
2016 × Treatment Intensity	-0.0132***	-0.0122***	-0.0103**	-0.0166***
	(0.00329)	(0.00354)	(0.00424)	(0.00355)
2017 × Treatment Intensity	-0.0102***	-0.00838**	-0.0101***	-0.0100**
	(0.00361)	(0.00387)	(0.00341)	(0.00447)
2018 × Treatment Intensity	-0.0175***	-0.0169***	-0.0144***	-0.0197***
	(0.00257)	(0.00287)	(0.00299)	(0.00391)
Year FE	Yes	No	Yes	No
Industry×Year FE	No	Yes	No	No
Bank×Year FE	No	No	No	Yes
Bank Controls	No	No	Yes	No
Firm Controls × Post	Yes	Yes	Yes	Yes
Observations	608,646	607,501	437,887	553,856
Adj. R^2	0.003	-0.000	0.003	0.016
Pre-trends p-value: $\beta_{2007} = \beta_{2008} = 0$	0.029	0.047	0.020	0.057

Notes: Dependent variable: firm exit indicator (bankruptcy). Linear probability model estimated by OLS. Firm fixed effects are excluded because exit is a rare, absorbing event; including them would restrict identification to the small subset of firms that eventually exit. Treatment Intensity constructed from 2009 firm characteristics with time-varying intercepts estimated from the RIETI survey. Baseline year: 2009 (omitted). Standard errors clustered at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 14: Event-study plot: firm exit (bankruptcy)

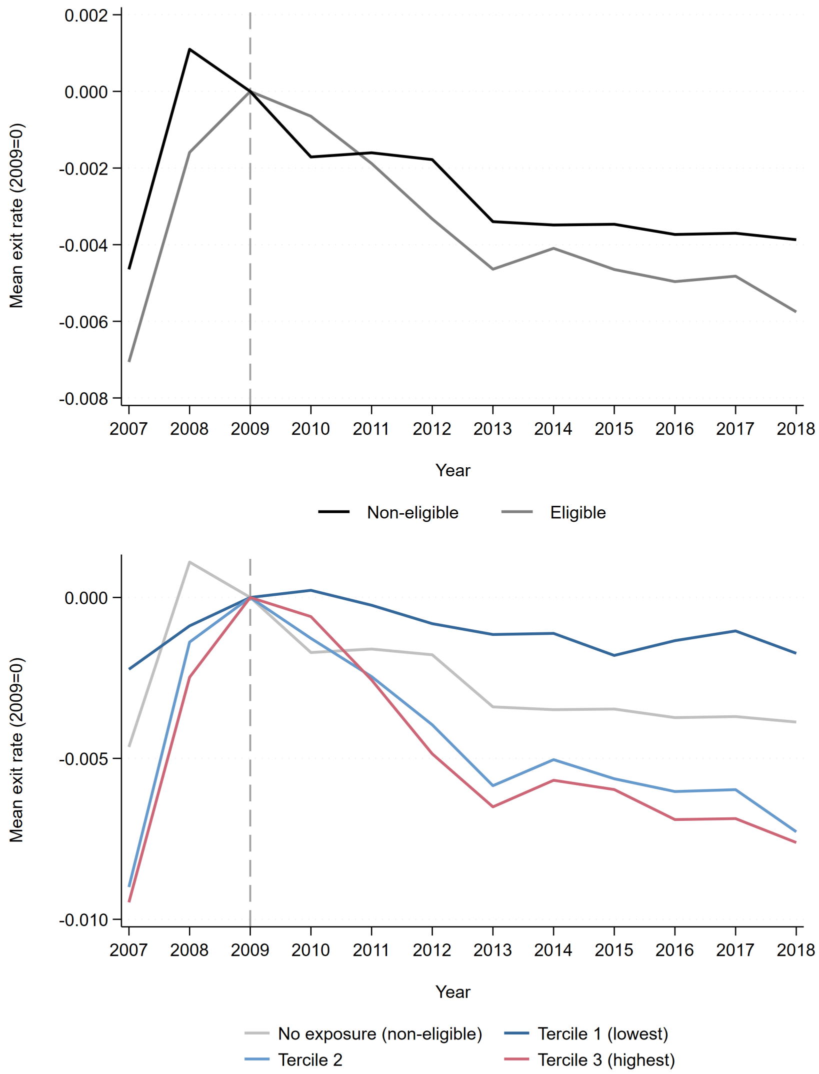


Notes: Event-study coefficients from Table 11. Each coefficient represents the interaction between Treatment Intensity and a year dummy, with 2009 as the omitted baseline year. The dependent variable is the firm exit indicator (bankruptcy). Linear probability model estimated without firm fixed effects. Capped spikes represent 95% confidence intervals based on standard errors clustered at the firm level.

Parallel trends analysis

Figure 15 plots mean exit rates, normalized to zero in 2009. In the top panel, both eligible and non-eligible firms show rising exit rates from 2007 to 2009, though from different starting points: the eligible group starts lower and rises more steeply, while the non-eligible group peaks slightly above its 2009 level in 2008 before settling back, suggesting differential pre-treatment trajectories. In the bottom panel, all groups (including non-eligible firms) exhibit rising exit rates from 2007 to 2009, reflecting the deepening financial crisis. The highest-exposure firms (Tercile 3) start from the lowest point in 2007 and rise most steeply, while Terciles 1 and 2 start closer to zero. As with the zombie outcomes, this pre-treatment upward trend for high-exposure firms runs counter to the negative post-treatment effects. After 2009, both panels show declining exit rates for all groups, but with a clear dose-response pattern: Tercile 3 firms show the largest decline in exit rates, followed by Tercile 2, with Tercile 1 experiencing the smallest decline. The gap between exposure groups widens through 2018.

Figure 15: Mean exit rate (2009=0), by eligibility and exposure tercile



Notes: This figure plots mean exit/bankruptcy rates, normalized to zero in 2009. The top panel compares eligible and non-eligible firms. The bottom panel disaggregates eligible firms into terciles of treatment intensity (based on the time-invariant 2009 measure) and includes non-eligible firms (no exposure) as a reference group. The dashed vertical line marks the onset of the SME Financing Facilitation Act (2009). Source: TSR.

Panel A of Table 12 reports $\beta_{2007} = 0.001$ (insignificant) and $\beta_{2008} = 0.010$ ($p = 0.012$). Both pre-treatment coefficients are *positive*: in 2007 and 2008, high-exposure firms had *higher* exit rates relative to their own 2009 level than did low-exposure firms. The pre-trend is $\hat{\delta}_{\text{pre}} = \beta_{2008} - \beta_{2007} = 0.008$ ($p = 0.036$), positive and significant: the exit rate differential between high- and low-exposure firms was *increasing* from 2007 to 2008. The joint F-test rejects the null ($F = 3.53$, $p = 0.029$). Since our post-treatment effects are *negative* (the Act reduced exit rates for high-exposure firms), the positive pre-trend runs in the *opposite* direction to the treatment effects. Economically, this is precisely what one would expect: the Act was introduced because vulnerable, high-exposure firms were increasingly failing during the financial crisis. It is plausible that absent the Act, this upward exit trend for high-exposure firms would have *continued*, which would imply that our estimates are conservative (i.e., the true treatment effect is larger in absolute value than what we estimate).

However, unlike the zombie outcomes, the pre-trend here is large relative to the average post-treatment effect: at 91.1% of $|\bar{\beta}_{\text{post}}|$, the pre-trend is nearly as large as the treatment effect itself. This reflects the small absolute magnitude of the exit effects, which is a consequence of exit being a rare event. Panel B reports the average post-treatment effect of -0.009 ($p < 0.001$). The dynamic pattern shows no immediate effect in 2010 ($\beta_{2010} = 0.001$, $p = 0.848$), with the effect emerging gradually ($\beta_{2014} = -0.008$, $p = 0.023$) and strengthening through the end of the sample ($\beta_{2018} = -0.018$, $p < 0.001$).

Panel C reports a breakdown point of $\bar{M}^* = 0$: even under the assumption of no post-treatment violation of parallel trends, the robust confidence interval barely excludes zero, and any positive allowance for trend violations causes it to include zero. This low breakdown point is a mechanical consequence of the large pre-trend relative to the small treatment effect. The Rambachan and Roth (2023) framework is agnostic about the *direction* of potential post-treatment violations, allowing for deviations in either direction up to $\bar{M} \times |\hat{\delta}_{\text{pre}}|$ per period. In the exit case, even though the economically plausible counterfactual is that the upward pre-trend would have continued (implying our estimates are conservative), the framework must also allow for the possibility that the trend reversed. This quickly generates bounds wide enough to include zero given the small treatment effect. The exit results should be interpreted with appropriate caution, though we note that the dose-response pattern visible in Figure 15 and the monotonically growing treatment effect over time are both consistent with a genuine policy effect.

Table 12: Sensitivity analysis: Pre-trends and robustness (Exit)

	Coefficient	Std. Error	p-value	Notes
Panel A: Pre-treatment period (2007–2008)				
2007 coefficient (β_{2007})	0.0011	0.0013	0.392	Relative to 2009
2008 coefficient (β_{2008})	0.0095	0.0038	0.012	Relative to 2009
Pre-trend ($\hat{\delta}_{\text{pre}}$)	**0.0084**	**0.0040**	**0.036**	$\boldsymbol{= \beta_{2008} - \beta_{2007}}$
Joint F-test	$F = 3.53$		0.029	$H_0 : \beta_{2007} = \beta_{2008} = 0$
Panel B: Treatment effects (2010–2018)				
Average effect ($\bar{\beta}_{\text{post}}$)	**-0.0092**	**0.0018**	**0.000**	**Average across 9 years**
2010 (immediate)	0.0006	0.0030	0.848	First year
2014 (mid-period)	-0.0079	0.0035	0.023	5 years post-treatment
2018 (final year)	-0.0175	0.0026	0.000	9 years post-treatment
Panel C: Rambachan & Roth (2023) Sensitivity Analysis				
Breakdown point ($\bar{M}^*$)	**0**			CI crosses zero at $\bar{M}^*$
Pre-trend as % of avg effect	91.1%			Relative magnitude
Upper CI bound at $\bar{M} = 3$	0.006			Sign at max violation

Notes: This table reports pre-treatment coefficients, treatment effects, and sensitivity analysis following Rambachan and Roth (2023). The dependent variable is the exit/bankruptcy indicator. All regressions include year fixed effects. Standard errors are clustered at the firm level. The breakdown point $\bar{M}^*$ indicates the value at which confidence intervals for the average treatment effect first include zero when allowing for post-treatment violations of parallel trends up to $\bar{M}$ times the estimated pre-trend.

G.4 Total factor productivity

Table 13 presents the full results for log TFP across all five specifications.

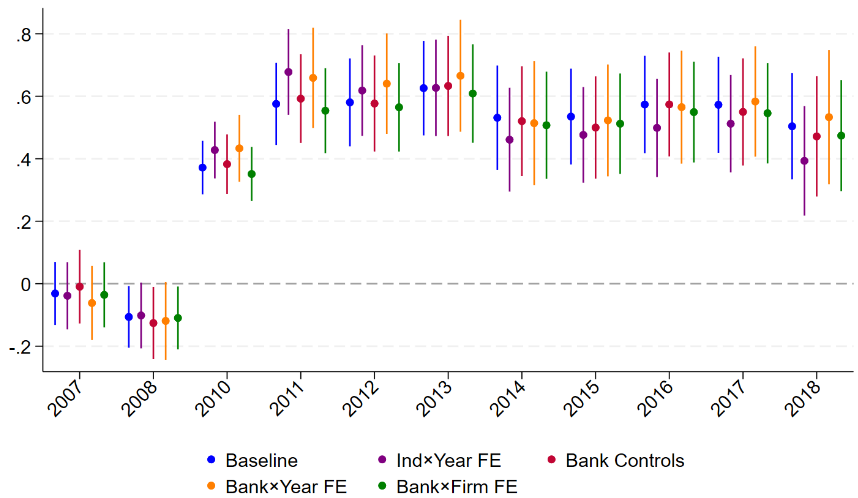
Table 13: Effect of the Act on total factor productivity

	(1) Baseline	(2) Ind×Year FE	(3) Bank Controls	(4) Bank×Year FE	(5) Bank×Firm FE
Pre-treatment coefficients					
2007 × Treatment Intensity	-0.0312	-0.0386	-0.00959	-0.0618	-0.0357
	(0.0515)	(0.0549)	(0.0600)	(0.0605)	(0.0532)
2008 × Treatment Intensity	-0.106**	-0.102*	-0.126**	-0.119*	-0.110**
	(0.0503)	(0.0537)	(0.0590)	(0.0635)	(0.0513)
Post-treatment coefficients					
2010 × Treatment Intensity	0.372***	0.428***	0.383***	0.433***	0.351***
	(0.0437)	(0.0463)	(0.0485)	(0.0547)	(0.0442)
2011 × Treatment Intensity	0.576***	0.678***	0.592***	0.659***	0.554***
	(0.0672)	(0.0699)	(0.0724)	(0.0818)	(0.0693)
2012 × Treatment Intensity	0.580***	0.618***	0.577***	0.640***	0.565***
	(0.0718)	(0.0740)	(0.0784)	(0.0820)	(0.0723)
2013 × Treatment Intensity	0.626***	0.627***	0.633***	0.666***	0.609***
	(0.0772)	(0.0787)	(0.0818)	(0.0914)	(0.0804)
2014 × Treatment Intensity	0.531***	0.461***	0.520***	0.514***	0.507***
	(0.0852)	(0.0848)	(0.0897)	(0.101)	(0.0875)
2015 × Treatment Intensity	0.535***	0.476***	0.500***	0.523***	0.512***
	(0.0782)	(0.0780)	(0.0835)	(0.0914)	(0.0820)
2016 × Treatment Intensity	0.574***	0.499***	0.574***	0.565***	0.549***
	(0.0795)	(0.0803)	(0.0848)	(0.0923)	(0.0822)
2017 × Treatment Intensity	0.573***	0.512***	0.550***	0.583***	0.546***
	(0.0784)	(0.0795)	(0.0875)	(0.0900)	(0.0820)
2018 × Treatment Intensity	0.504***	0.393***	0.471***	0.533***	0.474***
	(0.0867)	(0.0894)	(0.0982)	(0.110)	(0.0906)
Firm FE	Yes	Yes	Yes	Yes	No
Year FE	Yes	No	Yes	No	Yes
Industry×Year FE	No	Yes	No	No	No
Bank×Year FE	No	No	No	Yes	No
Bank×Firm FE	No	No	No	No	Yes
Bank Controls	No	No	Yes	No	No
Firm Controls × Post	Yes	Yes	Yes	Yes	Yes
Observations	606,455	605,248	433,734	547,060	593,932
Adj. R^2	0.368	0.371	0.358	0.371	0.373
Pre-trends p-value: $\beta_{2007} = \beta_{2008} = 0$	0.104	0.167	0.079	0.165	0.100

Notes: Dependent variable: ln(TFP), where TFP is estimated following Wooldridge (2009). Treatment Intensity constructed from 2009 firm characteristics with time-varying intercepts estimated from the RIETI survey. Baseline year: 2009 (omitted). Standard errors clustered at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The pre-treatment coefficients for 2007 are small and statistically insignificant across all columns. The 2008 coefficient is negative and significant at conventional levels in most specifications, consistent with higher-exposure firms experiencing a sharper productivity decline during the financial crisis. This pre-treatment pattern runs in the opposite direction to the positive post-treatment effects, implying that any bias from differential pre-trends would work against our findings. The joint pre-trends tests fail to reject equality of the pre-treatment coefficients to zero in most specifications. After 2009, the estimated treatment effects are significantly positive in all post-treatment years across all specifications, with the effects rising sharply in 2010 and 2011 before plateauing. This pattern is remarkably stable across all five specifications.

Figure 16: Event-study plot: ln(**TFP**)

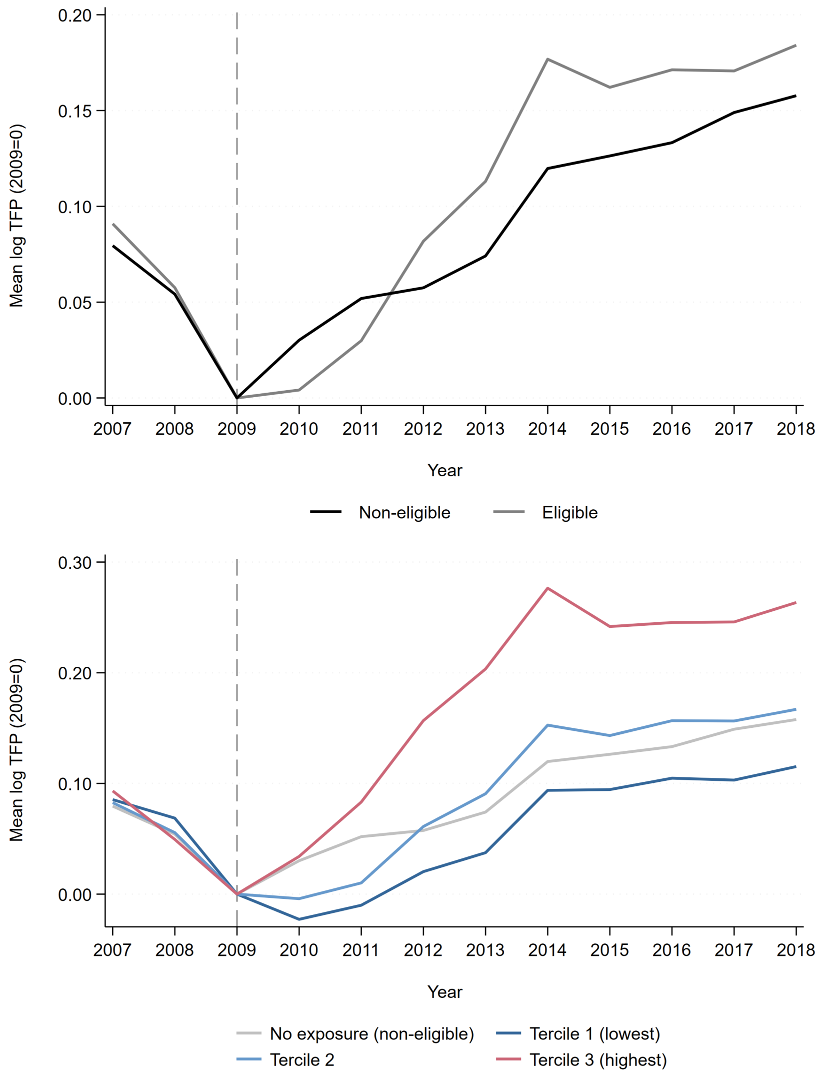


Notes: Event-study coefficients from Table 13. Each coefficient represents the interaction between Treatment Intensity and a year dummy, with 2009 as the omitted baseline year. The dependent variable is ln(TFP), where TFP is estimated following Wooldridge (2009). Capped spikes represent 95% confidence intervals based on standard errors clustered at the firm level.

Parallel trends analysis

Figure 17 plots mean log TFP, normalized to zero in 2009. In the top panel, both eligible and non-eligible firms experience declining TFP between 2007 and 2009, consistent with the common recessionary shock of the global financial crisis. The pre-treatment trajectories are broadly similar, with both groups falling by roughly 0.07 to 0.09 log points over the two pre-treatment years, providing support for the parallel trends assumption. After 2009, both groups recover, but eligible firms exhibit notably faster proportional TFP growth. The two series begin to diverge visibly around 2012, and the gap continues to widen through 2014 before stabilizing, with eligible firms maintaining a persistent productivity advantage over non-eligible firms through the end of the sample period. The bottom panel disaggregates eligible firms by treatment intensity tercile. Before 2009, all four groups follow broadly similar downward trajectories, declining from comparable levels in 2007 toward their common 2009 baseline. After 2009, a clear dose-response pattern emerges: Tercile 3 (highest-exposure) firms experience by far the strongest TFP growth, pulling sharply away from the other groups beginning around 2011 and reaching approximately 0.28 log points above the baseline by 2014. Tercile 2 firms exhibit intermediate growth, while Tercile 1 firms recover more slowly and remain closer to the non-eligible group throughout the post-treatment period. This monotonic ordering of post-treatment TFP trajectories by treatment intensity is consistent with the Act facilitating productivity recovery among the most exposed firms.

Figure 17: Mean log TFP (2009=0), by eligibility and exposure tercile



Notes: This figure plots mean log TFP, normalized to zero in 2009. The top panel compares eligible and non-eligible firms. The bottom panel disaggregates eligible firms into terciles of treatment intensity and includes non-eligible firms (no exposure) as a reference group. The dashed vertical line marks the onset of the SME Financing Facilitation Act (2009). Source: TSR.

Table 14 presents the sensitivity analysis. Panel A reports the pre-treatment coefficients. Both $\beta_{2007} = -0.031$ and $\beta_{2008} = -0.106$ are negative, indicating that in the pre-treatment years, firms with higher treatment intensity had *lower* TFP relative to their own 2009 level than did firms with lower treatment intensity. The estimated pre-trend is $\hat{\delta}_{\text{pre}} = \beta_{2008} - \beta_{2007} = -0.075$ ($p = 0.181$): TFP for high-exposure firms was *declining* relative to low-exposure firms before the intervention. Since our post-treatment effects are positive, this pre-trend runs in the *opposite* direction and therefore works against our findings. Any continuation of this pre-existing dynamic post-treatment would bias us toward finding *smaller* (or negative) treatment effects, not larger ones. Note also that the joint F-test does not reject the null that both pre-treatment coefficients are zero ($F = 2.26$, $p = 0.105$) and $\hat{\delta}_{\text{pre}}$ is insignificant.

Panel B reports the average post-treatment effect of 0.541 ($p < 0.001$), highly significant and roughly seven times larger in absolute value than the pre-trend. Panel C presents the Rambachan and Roth (2023) sensitivity analysis. The breakdown point is $\bar{M}^* = 2$, meaning that the confidence interval for the average treatment effect remains bounded away from zero unless the post-treatment trend deviation exceeds twice the magnitude of the estimated pre-trend. Given that the pre-trend is small, runs in the opposite direction to the treatment effects, and is statistically insignificant, this represents a comfortable margin of robustness.

Table 14: Sensitivity analysis: Pre-trends and robustness (TFP)

	Coefficient	Std. Error	p-value	Notes
Panel A: Pre-treatment period (2007–2008)				
2007 coefficient (β_{2007})	-0.031	0.051	0.551	Relative to 2009
2008 coefficient (β_{2008})	-0.106	0.050	0.035	Relative to 2009
Pre-trend ($\hat{\delta}_{\text{pre}}$)	**-0.075**	**0.056**	**0.181**	$= \boldsymbol{\beta_{2008} - \beta_{2007}}$
Joint F-test	$F = 2.26$		0.105	$H_0 : \beta_{2007} = \beta_{2008} = 0$
Panel B: Treatment effects (2010–2018)				
Average effect ($\bar{\beta}_{\text{post}}$)	**0.541**	**0.066**	**0.000**	**Average across 9 years**
2010 (immediate)	0.372	0.044	0.000	First year
2014 (mid-period)	0.532	0.085	0.000	5 years post-treatment
2018 (final year)	0.504	0.087	0.000	9 years post-treatment
Panel C: Rambachan & Roth (2023) Sensitivity Analysis				
Breakdown point ($\bar{M}^*$)	**2**			CI crosses zero at $\bar{M}^*$
Pre-trend as % of avg effect	13.9%			Relative magnitude
Upper CI bound at $\bar{M} = 2$	0.754			Sign at max violation

Notes: This table reports pre-treatment coefficients, treatment effects, and sensitivity analysis following Rambachan and Roth (2023). The dependent variable is ln(TFP). All regressions include firm and year fixed effects. Standard errors are clustered at the firm level. The breakdown point $\bar{M}^*$ indicates the value at which confidence intervals for the average treatment effect first include zero when allowing for post-treatment violations of parallel trends up to $\bar{M}$ times the estimated pre-trend.

G.5 Interest coverage ratio

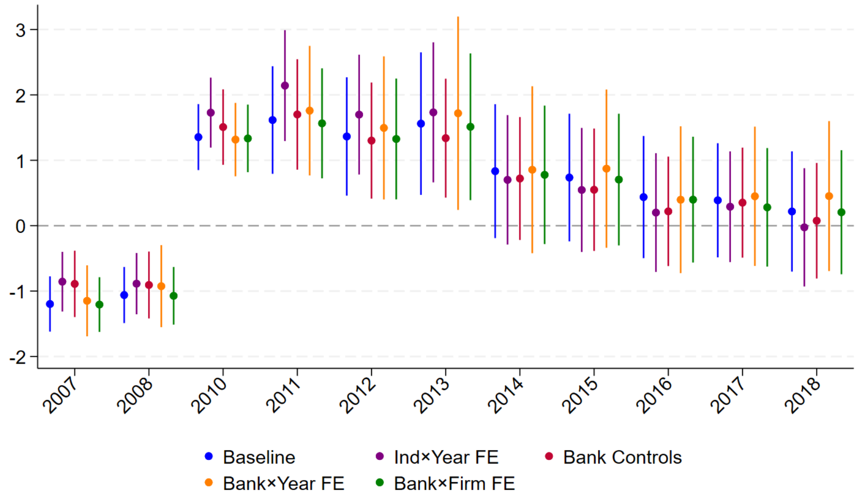
Table 15 presents the full results for the log interest coverage ratio across all five specifications. The pre-treatment coefficients for both 2007 and 2008 are large, negative, and highly significant across all columns, indicating that higher-exposure firms had substantially lower interest coverage ratios relative to the 2009 baseline during the crisis years. The joint pre-trends tests reject equality of the pre-treatment coefficients to zero in all specifications ($p < 0.001$). As with the previous outcomes, however, this pre-treatment pattern runs in the opposite direction to the positive post-treatment effects, implying that any resulting bias would work against our findings. After 2009, the treatment effects are large, positive, and highly significant in the early post-treatment years, with the coefficients peaking around 2011 to 2013. From 2014 onward, the coefficients remain positive but lose statistical significance, suggesting that the Act's effect on interest coverage ratios was concentrated in the initial years of the policy. This temporal pattern is consistent across all five specifications. The fading of the treatment effect over time is consistent with the interest rate channel: as forbearance lending reduced borrowing costs for treated firms, their interest coverage ratios improved mechanically; as the Act's effects on interest rates attenuated in later years, so too did the ICR differential.

Table 15: Effect of the Act on interest coverage ratios

	(1) Baseline	(2) Ind×Year FE	(3) Bank Controls	(4) Bank×Year FE	(5) Bank×Firm FE
Pre-treatment coefficients					
2007 × Treatment Intensity	-1.197***	-0.856***	-0.890***	-1.149***	-1.206***
	(0.216)	(0.233)	(0.259)	(0.277)	(0.213)
2008 × Treatment Intensity	-1.060***	-0.886***	-0.907***	-0.925***	-1.072***
	(0.219)	(0.239)	(0.261)	(0.320)	(0.225)
Post-treatment coefficients					
2010 × Treatment Intensity	1.354***	1.729***	1.508***	1.317***	1.334***
	(0.257)	(0.273)	(0.294)	(0.286)	(0.264)
2011 × Treatment Intensity	1.616***	2.142***	1.701***	1.759***	1.565***
	(0.420)	(0.433)	(0.431)	(0.505)	(0.429)
2012 × Treatment Intensity	1.364***	1.698***	1.301***	1.495***	1.326***
	(0.462)	(0.467)	(0.453)	(0.558)	(0.471)
2013 × Treatment Intensity	1.561***	1.733***	1.339***	1.719**	1.513***
	(0.556)	(0.546)	(0.464)	(0.754)	(0.572)
2014 × Treatment Intensity	0.834	0.701	0.721	0.855	0.778
	(0.522)	(0.505)	(0.479)	(0.651)	(0.540)
2015 × Treatment Intensity	0.736	0.547	0.549	0.872	0.705
	(0.498)	(0.484)	(0.477)	(0.617)	(0.514)
2016 × Treatment Intensity	0.438	0.201	0.219	0.397	0.399
	(0.477)	(0.463)	(0.427)	(0.573)	(0.491)
2017 × Treatment Intensity	0.389	0.290	0.353	0.450	0.281
	(0.445)	(0.432)	(0.429)	(0.543)	(0.462)
2018 × Treatment Intensity	0.217	-0.0250	0.0757	0.453	0.206
	(0.469)	(0.461)	(0.451)	(0.585)	(0.484)
Firm FE	Yes	Yes	Yes	Yes	No
Year FE	Yes	No	Yes	No	Yes
Industry×Year FE	No	Yes	No	No	No
Bank×Year FE	No	No	No	Yes	No
Bank×Firm FE	No	No	No	No	Yes
Bank Controls	No	No	Yes	No	No
Firm Controls × Post	Yes	Yes	Yes	Yes	Yes
Observations	454,698	453,352	320,465	390,561	442,303
Adj. R^2	0.698	0.700	0.683	0.711	0.707
Pre-trends p-value: $\beta_{2007} = \beta_{2008} = 0$	0.000	0.000	0.000	0.000	0.000

Notes: Dependent variable: ln(ICR), where ICR = EBIT/interest expenses. Treatment Intensity constructed from 2009 firm characteristics with time-varying intercepts estimated from the RIETI survey. Baseline year: 2009 (omitted). Standard errors clustered at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 18: Event-study plot: ln(**ICR**)

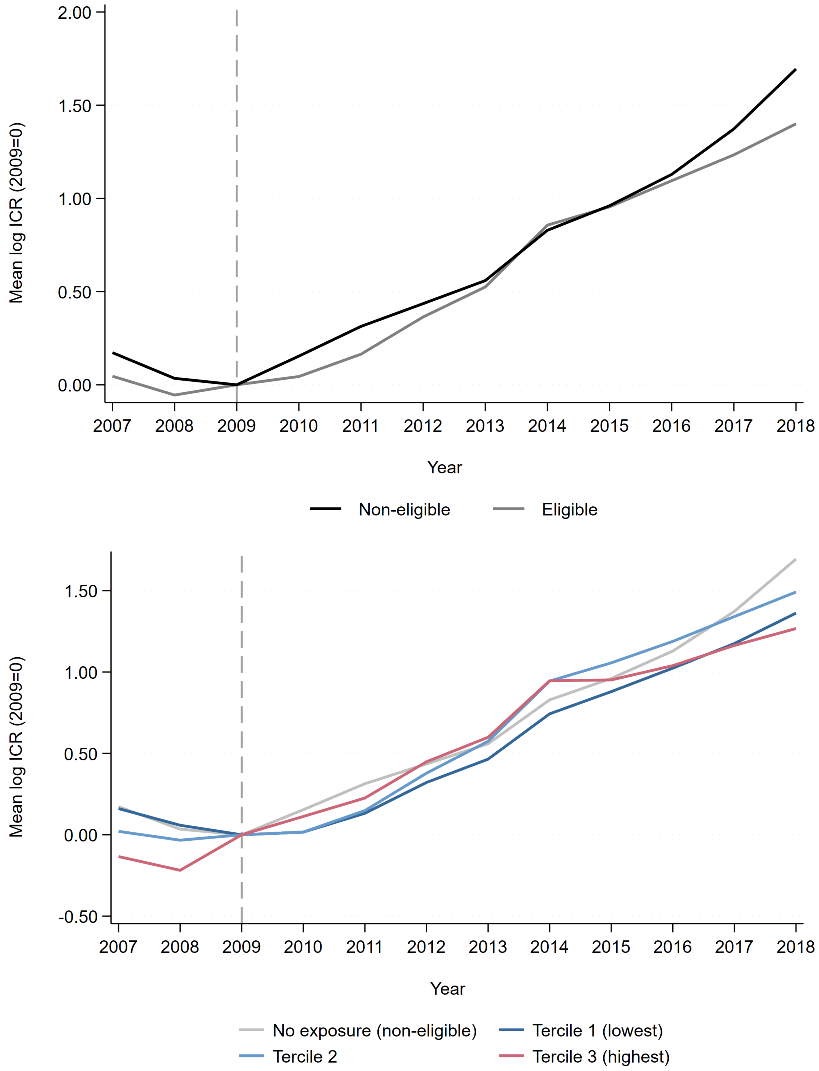


Notes: Event-study coefficients from Table 15. Each coefficient represents the interaction between Treatment Intensity and a year dummy, with 2009 as the omitted baseline year. The dependent variable is ln(ICR), where ICR = EBIT/interest expenses. Capped spikes represent 95% confidence intervals based on standard errors clustered at the firm level.

Parallel trends analysis

Figure 19 plots mean log ICRs, normalized to zero in 2009. In the top panel, eligible and non-eligible firms follow broadly similar pre-2009 trajectories, both declining toward the normalized baseline, though the non-eligible group starts from a somewhat higher level in 2007, consistent with better initial debt-servicing capacity among firms less exposed to the Act. After 2009, both groups experience a steady and pronounced recovery in interest coverage ratios, rising together through 2018. The bottom panel provides a more granular view by disaggregating eligible firms into treatment intensity terciles. Before 2009, the groups do not follow perfectly parallel paths: the highest-exposure firms (Tercile 3) display a noticeable dip in 2008 before converging to the common baseline, while the lowest-exposure firms (Tercile 1) and the no-exposure group start above and trend downward. These pre-2009 differences are modest in magnitude but suggest some heterogeneity in pre-treatment ICR dynamics across exposure groups. After 2009, all four groups recover in tandem, following strikingly similar upward trajectories through 2018. The dose-response pattern that is so prominent for zombie status, exit, and TFP is largely absent here, consistent with the regression evidence that the Act's effect on interest coverage ratios dissipated in the later years of the sample as treated firms' ICR levels converged toward those of untreated firms.

Figure 19: Mean log ICR (2009=0), by eligibility and exposure tercile



Notes: This figure plots mean log ICR (interest coverage ratio), normalized to zero in 2009. The top panel compares eligible and non-eligible firms. The bottom panel disaggregates eligible firms into terciles of treatment intensity and includes non-eligible firms (no exposure) as a reference group. The dashed vertical line marks the onset of the SME Financing Facilitation Act (2009). Source: TSR.

Table 16 (Panel A) reports the pre-treatment coefficients. Both $\beta_{2007} = -1.195$ and $\beta_{2008} = -1.059$ are large, negative, and highly significant, indicating that in 2007 and 2008, high-exposure firms had substantially *lower* ICR levels relative to their own 2009 level than did low-exposure firms. The joint F-test strongly rejects the null that both are zero ($F = 18.36$, $p < 0.001$). The pre-trend is $\hat{\delta}_{\text{pre}} = \beta_{2008} - \beta_{2007} = 0.136$ ($p = 0.536$). This positive value indicates that ICRs for high-exposure firms were *improving* relative to low-exposure firms in the pre-treatment period. Since our post-treatment effects on ICR are also positive, this pre-trend runs in the *same* direction as the treatment effects. If this favourable pre-existing dynamic continued post-treatment, it could account for part of the estimated treatment effects. This is a legitimate concern, though we note that the pre-trend is not statistically significant and is small relative to the average post-treatment effect.

Panel B reports the average post-treatment effect of 0.949 ($p = 0.031$), though the effect is concentrated in the early post-treatment years (2010 coefficient: 1.359, $p < 0.001$) and fades over time (2018 coefficient: 0.222, $p = 0.637$). Panel C shows that the breakdown point is $\bar{M}^* = 0.8$. This lower breakdown point reflects both the direction of the pre-trend (which does not work against us) and the wider confidence intervals associated with the fading treatment effect. The ICR results should therefore be interpreted with some caution with respect to the parallel trends assumption.

Table 16: Sensitivity analysis: Pre-trends and robustness (Interest Coverage Ratio)

	Coefficient	Std. Error	p-value	Notes
Panel A: Pre-treatment period (2007–2008)				
2007 coefficient (β_{2007})	-1.195	0.216	0.000	Relative to 2009
2008 coefficient (β_{2008})	-1.059	0.219	0.000	Relative to 2009
Pre-trend ($\hat{\delta}_{\text{pre}}$)	**0.136**	**0.220**	**0.536**	$= \boldsymbol{\beta_{2008} - \beta_{2007}}$
Joint F-test	$F = 18.36$		0.000	$H_0 : \beta_{2007} = \beta_{2008} = 0$
Panel B: Treatment effects (2010–2018)				
Average effect ($\bar{\beta}_{\text{post}}$)	**0.949**	**0.440**	**0.031**	**Average across 9 years**
2010 (immediate)	1.359	0.257	0.000	First year
2014 (mid-period)	0.837	0.522	0.109	5 years post-treatment
2018 (final year)	0.222	0.469	0.637	9 years post-treatment
Panel C: Rambachan & Roth (2023) Sensitivity Analysis				
Breakdown point ($\bar{M}^*$)		**0.8**		CI crosses zero at $\bar{M}^*$
Pre-trend as % of avg effect		14.3%		Relative magnitude
Upper CI bound at $\bar{M} = 0.8$		2.659		Sign at max violation

Notes: This table reports pre-treatment coefficients, treatment effects, and sensitivity analysis following Rambachan and Roth (2023). The dependent variable is ln(ICR), where ICR = EBIT/interest expenses. All regressions include firm and year fixed effects. Standard errors are clustered at the firm level. The breakdown point $\bar{M}^*$ indicates the value at which confidence intervals for the average treatment effect first include zero when allowing for post-treatment violations of parallel trends up to $\bar{M}$ times the estimated pre-trend.

H Loan forbearance and lender characteristics

In this section, we explore the correlation between bank health and forbearance incentives in cross-sectional analysis. We measure bank health with the capital ratio and the non-performing loans (NPL) ratio as defined in Ogura and Uchida (2014) and Ogura (2018). We expect banks with a higher NPL ratio to have stronger incentives to forbear because the regulator allowed financial institutions to exclude the restructured SME loans from their reported NPLs under the condition that they came up with business restructuring plans.

We proxy forbearance incentives at the bank level with the average treatment intensity of each bank's borrowers by taking the arithmetic average of its borrowers' treatment intensity in 2009. Each TSR borrower can be linked to up to 10 lenders. The average number of banks reported by firms is under 3.5. Therefore, we limit ourselves to three lenders per firm. In other words, a bank's borrowers are firms that report the bank as either the first, second, or third lender. We regress the average treatment intensity of the bank's borrowers in 2009 on the bank's capital ratio and non-performing loans ratio in 2008. Standard errors are clustered at the bank level. The results are in Table 17.

Table 17: Bank characteristics and treatment intensity (cross-section 2009)

	(1) NPL ratio only	(2) Capital ratio only	(3) Both
Lagged NPL ratio	0.00206***		0.00207***
	(0.000146)		(0.000146)
Lagged capital ratio		0.000344***	0.000369***
		(0.000109)	(0.000109)
Constant	0.0710***	0.0785***	0.0668***
	(0.000882)	(0.00128)	(0.00155)
Observations	13406	13406	13406
R-squared	0.0156	0.000693	0.0164

Notes: Standard errors clustered at bank level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is the average treatment intensity of bank borrowers in 2009. NPL and capital ratios are lagged (2008 values).

Both the NPL ratio and the capital ratio are significantly correlated with the average treatment intensity of a bank's borrowers at the 1% level, and these results are robust to including both predictors jointly (Column 3). The coefficient on the lagged NPL ratio in Column 3 is 0.00207, indicating that a 1 percentage point increase in the NPL ratio is associated with an increase of approximately 0.2 percentage points in the average treatment intensity of a bank's borrowers. The positive coefficient on the capital ratio indicates that better-capitalised banks were also more likely to have borrowers with higher treatment intensity. Together, these results suggest that healthier banks with higher NPL ratios were more likely to offer forbearance. Since both bank health indicators are significantly correlated with forbearance incentives, this motivates controlling for bank characteristics of the main lender in our difference-in-differences specifications.

This analysis cannot shed any substantial light on whether weak banks had more incentives to forbear because of reverse causation between the health of a bank's borrowers (and hence their treatment intensity) and the bank's own health. However, the finding that forbearance is associated with higher capitalisation stands in contrast to the narrative that especially low-capitalised banks had an incentive to lend to zombies during the Lost Decade and more recently in Europe (e.g., Peek and Rosengren, 2005; Caballero et al., 2008; Watanabe, 2010; Bruche and Llobet, 2014; Schivardi et al., 2022; Acharya et al., 2020). Our sample suggests that better-capitalised banks were more likely to forbear, probably because they had the balance-sheet capacity to do so. The positive correlation between the NPL ratio and forbearance, on the other hand, indicates that banks might have had strong incentives to design successful restructuring plans for their borrowers in receipt of forbearance. Indeed, this would have allowed them to exclude the restructured SME loans from their reported non-performing loans under the rules set by the Japanese Financial Services Agency.

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