Effects of Public Loan Guarantees on Credit Growth: Evidence from Randomly Assigned Examiners*

Maija Järvenpää† Elias Einiö‡ Aliisa Koivisto§

Mauri Kotamäki¶ Tuomas Takalo||

February 10, 2026

Abstract

We examine the impact of public loan guarantees on firm-level credit growth in Finland using unique guarantee application data linked to a corporate credit register. Exploiting the quasi-random assignment of guarantee applications to examiners, we use examiner leniency as an instrument to identify the effect of guarantee approval. Two key findings emerge. First, the majority of both accepted and rejected applicants increase borrowing after the decision, indicating that applicants are generally creditworthy. Second, IV estimates show no effect of guarantee approval on credit growth, and if anything, suggest a negative impact. These results are consistent with banks acting as gatekeepers: truly credit-constrained firms rarely reach the guarantee provider's screening stage.

Keywords: *Credit constraints; Public loan guarantees; Bank lending; Judge leniency design.*

JEL Classification: *G21; G28; H81.*

*The views expressed in this paper are those of the authors and do not necessarily reflect the views of the Bank of Finland or Finnvera. We thank Christopher Huber, Ari Hyytinen, William Kerr and Ville Voutilainen for useful discussions and comments.

†Aalto University and Bank of Finland: maija.jarvenpaa@bof.fi

‡VATT Institute for Economics Research

§VATT Institute for Economics Research

¶Finnvera and University of Turku

||Bank of Finland and Helsinki GSE

1 Introduction

Public loan guarantees are a widely used policy instrument aimed at alleviating credit constraints, particularly for small and medium-sized enterprises (SMEs), which often face limited access to external finance due to collateral shortages and informational frictions. By sharing credit risk between the state and commercial banks, guarantees are intended to stimulate lending and support investment and growth. The central question, however, is whether such programs genuinely increase credit or merely act as an implicit bank support, substituting existing unguaranteed lending.

The answer to this question is far from trivial. The key challenge is selection into guarantee programs. Guarantees are not randomly assigned but firms—or banks acting on their behalf—apply for them. Therefore, accepted applicants may be, for example, more creditworthy to begin with, implying that they would have received bank funding even without the guarantee. Another limitation is data: existing studies typically rely on credit registers that identify guaranteed loans but not rejected applications, making it impossible to observe the full applicant pool.

We overcome these challenges by using comprehensive application data from Finnvera—a Finnish state-owned specialized financing company—including both accepted and rejected applications, enriched with the Bank of Finland's corporate credit register data, and exploiting the quasi-random assignment of applications to examiners. In particular, we use examiner leniency as an instrumental variable to isolate the causal effect of a positive guarantee decision on firm-level credit growth. This "judge IV" approach is widely applied in judicial studies (Kling (2006) and thereafter) but novel in the study of public credit guarantees. Our design identifies the local average treatment effect for firms whose approval depends on examiner discretion, providing credible causal estimates for marginal applicants.

We document two main findings. First, even firms whose applications are rejected often increase borrowing after the decision. Second, IV estimates indicate no positive effect of approval on loan growth—if anything, effects are negative. These results are consistent with a "gatekeeper" role of banks, where the guarantee provider does not observe truly credit constrained firms as they do not pass the first screening stage by banks.

Our key source of data are all SME financing applications received by Finnvera during 2015–2024. We merge the guarantee application data with loan-level corporate credit registry data of the Bank of Finland. This merged application-loan data, covering years 2019–2024, allows us to examine the development of firm (and firm-bank) level credit and the price of credit around the guarantee decision. A key ad-

vantage for our research question is that our loan-level data covers all bank loans, which allows us to observe whether firms obtained other loans without a public loan guarantee.

We start our empirical analysis by documenting that examiners processing SME financing applications differ in their acceptance propensity. As applications are processed on a first come first served basis, these differences in examiner leniency create exogenous variation in guarantee approval rates for companies. Hence, we use the application examiner leniency as an instrumental variable to study the effect of an approved guarantee decision on firm-level credit growth.

We provide two key empirical findings. First, descriptive evidence shows that the majority of both accepted and rejected applicants increase borrowing after the guarantee decision. This pattern indicates that even the rejected firms are often creditworthy and able to acquire funding through alternative channels.

Second, our instrumental-variable estimates using examiner leniency reveal no positive effect of guarantee approval on firm-level credit growth. Across multiple time horizons, point estimates are negative and statistically insignificant. For firms with prior borrowing relationships, the effect is negative, indicating that guarantees may crowd out other forms of credit. For firms without prior loans, the effect is indistinguishable from zero, implying that guarantees do not materially improve access for new borrowers. These results hold under alternative definitions of the dependent variable and exclusion of crisis-period observations.

To explore the mechanisms underlying our empirical results, we construct a theoretical model of bank lending in the presence of public loan guarantees. In the model, banks prefer lending under loan guarantees but borrowing firms trade off the cost of public loan guarantee with cheaper borrowing interest rates. If public loan guarantees do not reduce borrowing rates enough, the firms borrow less in the presence of loan guarantees. Because the guarantees allow banks to move into more risky borrower segments, aggregate bank lending may increase even if existing bank customers reduce their borrowing.

We contribute to the literature in two main ways. First, we provide new evidence on the lending effects of public credit guarantee programs. Unlike prior studies that generally find positive impacts on lending, our results show no significant increase in credit following guarantee approval. Several factors may explain this difference. Methodologically, our approach identifies local effects for marginal applicants, whereas earlier work often estimates average effects around eligibility thresholds (De Blasio et al., 2018; Gonzalez-Uribe & Wang, 2020; Bachas et al., 2021; Bonfim et al., 2023) or compares credit trajectories of firms with and without guaranteed loans (Altavilla et al., 2025; Jiménez et al., 2024; Cascarino et

al., 2022; Neugebauer & Mateus, 2025; Güler & Samarin, 2023; Martin et al., 2025). The population studied—Finnish SMEs applying for guarantees between 2019 and 2023—may also face relatively mild credit frictions compared to firms in other contexts, limiting the potential impact of guarantees. In addition, program design features, such as guarantee fees and their interaction with bank pricing (Güler & Samarin, 2023), may reduce the net cost advantage of guaranteed loans for borrowers. Finally, institutional characteristics, including the high level of concentration of the Finnish banking sector, may further dampen the effect on lending. By documenting a setting where guarantees do not expand credit, we highlight the importance of market structure and program design in evaluating the effectiveness of public guarantee schemes.

Our second contribution is that we extend the judge-IV methodology to a new domain. Originally developed for and widely used in criminal justice settings (Kling, 2006; Aizer & Doyle Jr, 2015; Bhuller et al., 2020; Huttunen et al., 2022, see e.g.), this design has increasingly been applied to study effects of different firm-level interventions, such as patents (Farre-Mensa et al., 2020), start-up accelerators (González-Uribe & Reyes, 2021) and public subsidies (Hirvonen et al., 2023). To our knowledge, this is the first application of judge-IV to credit guarantee programs.

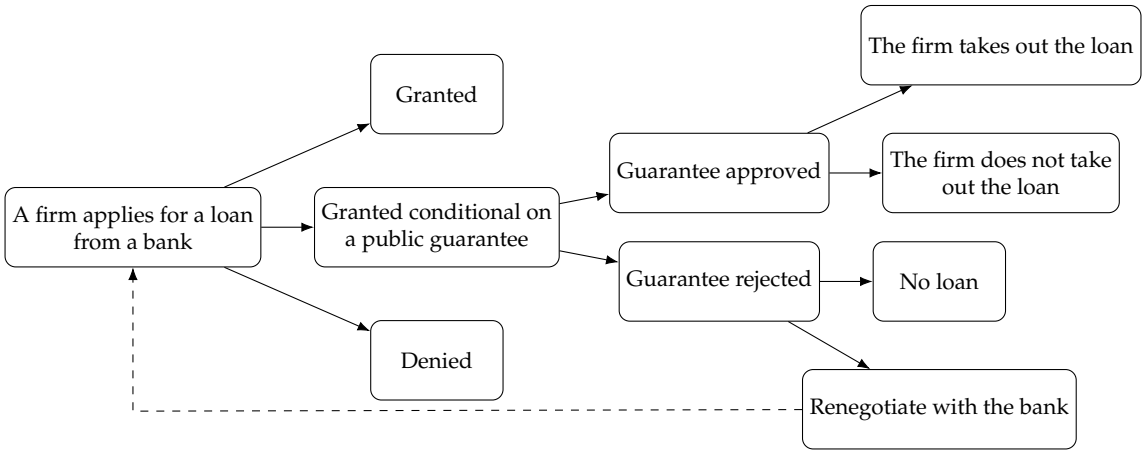
The remainder of the paper proceeds as follows: Section 2 describes the institutional setting; Section 3 details the data; Section 4 explains the empirical strategy; Section 5 presents results; Section 6 presents the theoretical model and tests its additional predictions. Section 7 concludes.

2 Institutional Context

Finnvera is a Finnish state-owned specialized financing company. Its mandate is to complement private financial markets by providing loans, guarantees, and export credit guarantees to promote enterprise growth, internationalization, and contribute to regional development. For SMEs, Finnvera offers several guarantee products aimed at easing access to bank financing. These guarantees typically cover 50–80% of the loan amount and involve costs for the firm in the form of an annual premium of 0.95–1.95% of the guaranteed amount, plus a one-off service fee of 1%.

Figure 1 illustrates the SME guarantee application workflow, from bank submission to Finnvera's decision. Guarantee applications are typically initiated by the firm's bank, which submits the request to Finnvera. The approval process involves an assessment of the applicant's financial standing, risk rating, and compli-

Figure 1: Public loan guarantee application process in Finland



Notes: The figure presents the guarantee application process of the state-owned specialized financing company Finnvera's guarantees from the loan application to the guarantee decision and ultimate loan decision.

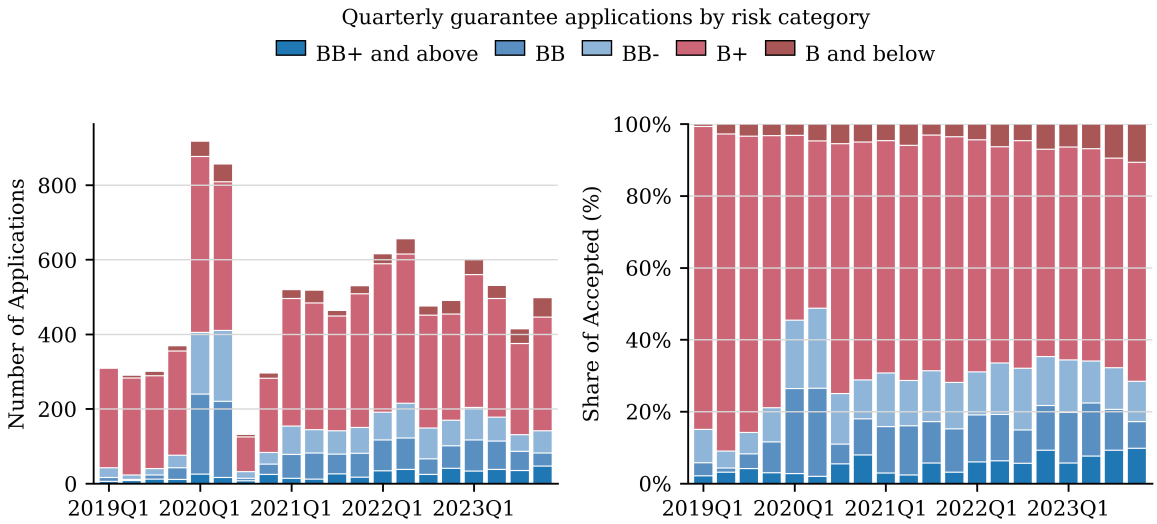
ance with product-specific conditions.

Applications are assigned to examiners within Finnvera's Small Business Financing unit on a first-come-first-served basis. Importantly, this implies that examiners do not specialize for example, in certain industries or product types, which could produce systematic non-random variation in approval propensities (Righi & Simcoe, 2019). Moreover, while some rejections are automatic due to eligibility criteria, most decisions involve examiner discretion, creating heterogeneity in approval outcomes. These institutional features, the quasi-random allocation of applications to examiners and the involvement of examiner discretion, underpin our identification strategy as they create exogeneous variation in approval propensity for applying firms.

The primary determinant of Finnvera's guarantee decision is the applicant firm's ability to repay the loan, which is assessed through a comprehensive analysis that includes, but is not limited to, the firm's credit rating. Figure 2 highlights an important institutional feature: the risk profiles of applicants varies over time. Notably, after the onset of Covid-19, the share of applicants with stronger repayment capacity increased. In addition to this supply-side variation, Finnvera adjusted its approval policy during the pandemic by temporarily lowering risk score thresholds for guarantees. In Section 4, we explain how these time-varying applicant characteristics and policy changes are incorporated into our research design.

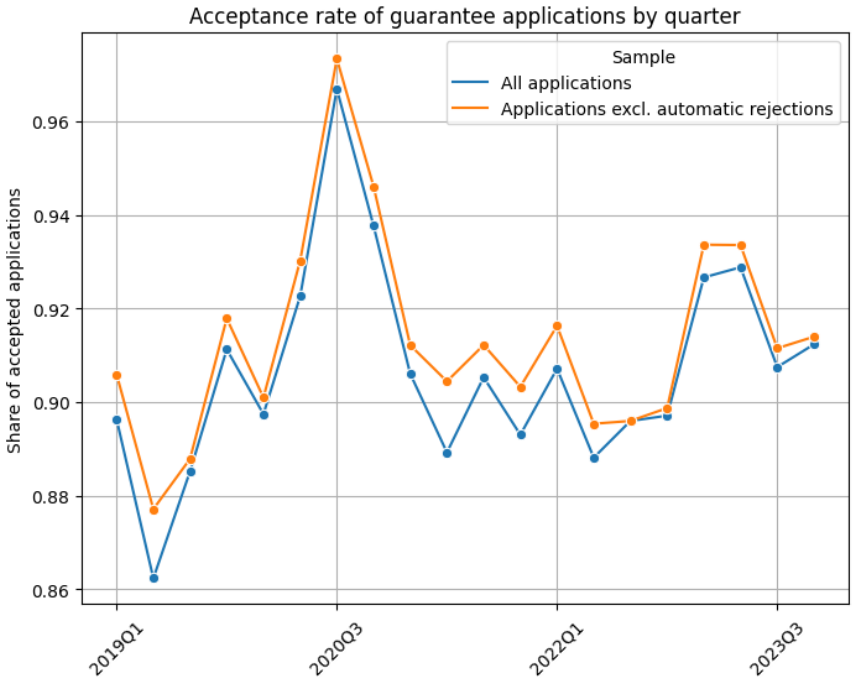
Figure 3 shows the quarterly acceptance rate of Finnvera's guarantee applications. Overall, acceptance rate is relatively high, around 91%. In addition, the temporary relaxed guarantee criteria during the Covid-19 pandemic can be seen as a surge in approvals in 2020Q2–Q4.

Figure 2: Guarantee applications by risk category



Notes: The left hand side figure shows the number of guarantee applications by quarter and risk category bin. The right hand side figure presents the distribution of accepted applications by risk category bin.

Figure 3: Acceptance rate over time.



Notes: The figure shows quarterly acceptance rates for all guarantee applications and for guarantee applications excluding automatic rejections during 2019–2023.

3 Data

Our core data consists of all financing applications handled by Finnvera's Small Business Financing unit between 2015 and 2023. The unit of observation is an application h submitted for firm i, reviewed by examiner r, at date t. Each record includes the application decision date, the examiner identifier, the product type (loan or guarantee), and detailed information on the requested financing—product name, applied amount, and in case of guarantees, associated bank granting the guaranteed loan. In addition, the application data includes data on applicant background characteristics at the time of application, including industry, region, and Finnvera's internal risk score. These observable firm characteristics will serve as controls in our empirical analysis.

To measure firm-level borrowing outcomes, we merge the application data with data from the Bank of Finland's corporate credit registry. Before merging, we aggregate the loan level data to the firm or firm–bank level. The credit registry allows us to trace the evolution of each applicant's outstanding bank credit and pricing around the Finnvera's guarantee decision date. Specifically, we calculate the following variables at firm (and firm-bank) level: (i) total outstanding loans; and (ii) a weighted-average loan margin quarter by quarter before and after the guarantee decision date. Corporate credit registry data is available from 2019 onward. Consequently, our merged sample covers applications during 2019–2023 and therefore covers both the Covid-19 pandemic and periods outside it. This feature allows us to track outcomes both under crisis conditions and in normal times. Table 2 describes the data by approval status of the guarantee applications.

4 Empirical Strategy

Our goal is to estimate the effect of Finnvera's guarantee approval on firm-level borrowing. Let $y_{ih,t+k}$ denote an outcome for firm i associated with application h, measured k periods relative to the decision date t (e.g., the change in total outstanding bank loans). Let D_{ihrt} be an indicator equal to one if the application h of firm i is approved by examiner r at time t. A natural starting point is:

$$y_{ih,t+k} = \beta_0 + \beta_1 D_{ihrt} + \boldsymbol{\gamma}' X_{it} + \alpha_t + \varepsilon_{iht}, \tag{1}$$

where X_{it} collects firm characteristics (e.g., industry, region and Finnvera risk score) and α_t denotes time fixed effects. The coefficient β_1 captures the association between approval and subsequent borrowing at horizon k.

4.1 Examiner leniency as an instrument

Equation (1) is unlikely to identify a causal effect because unobserved firm characteristics may influence both approval and borrowing. For example, more growth-oriented firms could both persuade examiners to approve guarantees and pursue bank financing even absent a guarantee. To overcome this selection problem, we exploit the quasi-random assignment of applications to Finnvera examiners and use examiner leniency as an instrument for approval—a "judge IV" design.

To construct our instrument for the approval decision D_{ihrt}, we follow a leave-one-out approach standard in the judge IV literature. Using the full applications panel (2015–2023, all applications excluding automatic rejections), we first regress the approval indicator on fully interacted risk score bin - quarter fixed effects $\delta_{s,t}$ (hereafter risk score - time FE) and collecting residuals u_{ihrt}:

$$D_{ihrt} = \delta_{s,t} + u_{ihrt}, \qquad D^*_{ihrt} \equiv u_{ihrt}. \tag{2}$$

Risk score–time fixed effects $\delta_{s,t}$ control for the time-varying applicant risk profile and guarantee policy described in Section 2. More specifically, examiners working at different times face applicants with varying credit risk and guarantee policies with different levels of risk score related stringency. By controlling for these factors, our leniency measure captures residual approval propensity after adjusting for changes in credit risk profile or guarantee policies.[1]

For each examiner r, we define their residual approval propensity Z_{ir} excluding all cases belonging to firm i (jackknife leave-one-firm-out):

$$Z_{ir} \equiv \frac{1}{|\mathcal{H}_r(i)|} \sum_{a \in \mathcal{H}_r(i)} D^*_{i(a)\, a\, r(a)\, t(a)}, \tag{3}$$

where $\mathcal{H}_r(i) = \{\, a \;:\; r(a) = r,\; i(a) \neq i,\; t(a) \in \{0, \ldots, T\}\}$, i.e. the set of applications reviewed by examiner r, excluding those submitted for firm i. Intuitively, Z_{ir} measures how lenient the examiner r is based only on cases of *other firms'*, thus preventing mechanical correlation with D_{ihrt} for firm i.

Next, we estimate the following first- and second-stage equations for the sample of guarantee applications during 2019–2023:

$$\text{First stage:} \quad D_{ihrt} = \alpha_0 + \alpha_1 Z_{ir} + \boldsymbol{\gamma}' X_{it} + \alpha_{s,t} + \eta_{ihrt}, \tag{4}$$

$$\text{Second stage:} \quad y_{ih,t+k} = \beta_0 + \beta_1 \widehat{D}_{ihrt} + \boldsymbol{\gamma}' X_{it} + \alpha_{s,t} + \varepsilon_{iht}. \tag{5}$$

[1] In the raw applications data, there are 15 different risk categories with very few observations in some of them. To ensure that there is a sufficient number of observations in each risk score bin - quarter pair to estimate the fixed effects regression, we aggregate risk categories into three groups.

The instrument Z_{ir} varies across examiner–firm pairs due to the quasi-random assignment of applications to examiners with heterogeneous propensities to approve. We include the same controls X_{it} and time fixed effects $\alpha_{s,t}$ in both stages. As applications are allocated to examiners individually, rather than in batches or shifts, we do not cluster (robust) standard errors as suggested by Abadie et al. (2022) and Chyn et al. (2025).

Our key coefficient of interest, β_1, identifies a Local Average Treatment Effect (LATE): the causal impact of approval on borrowing for *marginal* applications, those that would be approved by relatively lenient examiners but not by stricter ones.

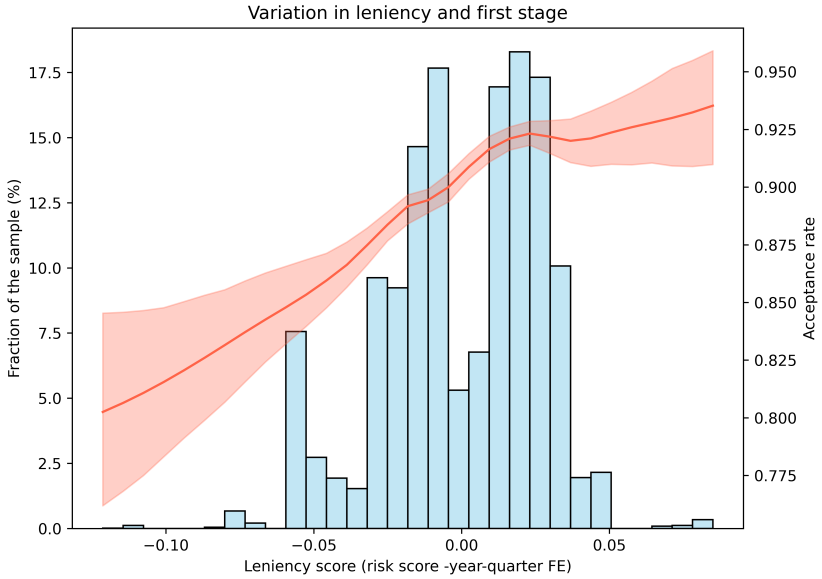
4.2 Validity of the instrument

The key for our identification strategy is that examiner leniency must satisfy the following assumptions: relevance, independence, monotonicity, and exclusion restriction.

Concerning relevance, we report first-stage estimates of α_1 and the corresponding first-stage F-statistics in Table 1. The estimated coefficient for examiner leniency is positive and statistically significant. In addition, the weak instrument F-statistic clearly exceeds the common rule-of-thumb threshold of > 10, indicating sufficient instrument relevance in finite samples. Furthermore, in Figure 4, we visualize the distribution of examiner leniency and estimate its non-parametric relationship with approval probabilities to document substantial cross-examiner variation that predicts decisions.

To establish independence, namely assess whether applications are quasi-randomly assigned to examiners, we study whether the observed characteristics that explain the guarantee decision are unrelated to the examiner leniency. We start with summary statistics. Table 2 compares observable characteristics across accepted and rejected applications. Overall, accepted and rejected applicants look strikingly similar. Accepted applicants are slightly larger in terms of revenue and they tend to be less risky in terms of the credit risk rating. In addition, accepted applicants operate slightly more frequently in the wholesale and retail trade industry. Furthermore, accepted applicants also apply for on average smaller guarantee amounts. Table 3 shows the result of a balance test, where we regress both approval dummy and the leniency measure on observed firm characteristics. As expected, many of these characteristics are strongly correlated with the guarantee decision. In addition, the joint F-statistics is 11.9, indicating that the observed characteristics jointly predict the guarantee approval. On the other hand, the same characteristics do not pre-

Figure 4: Variation in examiner leniency and first stage.



Notes: The histogram shows the distribution of examiner leniency (left y-axis). The solid line shows a local linear regression of the decision dummy on examiner leniency (right y-axis). The shaded area shows 95% confidence interval.

dict examiner leniency with F statistics value of 1.77. All in all, the statistically insignificant relationship between Z_{ir} and X_{it} support independence.

Monotonicity requires that higher examiner leniency weakly increases the probability of approval for every application. In Table 4, we examine first-stage coefficients for examiner leniency across rich subsamples (by industry, region, and prior-loan status). Consistently positive and statistically significant first-stage coefficients across subgroups support monotonicity.

The exclusion restriction posits that examiner leniency affects outcomes only through the approval decision. This is plausible in our context because examiners process applications but do not allocate bank credit directly, set loan terms, or interact with banks or firms beyond the guarantee decision. To bolster this claim, in Section 5.2 we show that Z_{ir} does not predict pre-decision trends in outcomes.

5 Results

5.1 Descriptive evidence on firm borrowing around the guarantee decision

We begin with a descriptive overview of Finnvera's guarantee decisions and applicants' borrowing trajectories. A striking finding from Table 2 is that not only do most accepted applicants increase their borrowing after the guarantee decision, but the majority of rejected applicants also experience growth in outstanding loans following a negative decision. Specifically, 80 % of accepted and 73 % of rejected applicants experience an increase in outstanding loans six months after the guarantee decision. This pattern indicates that firms applying for guarantees—those banks consider creditworthy enough to lend with a guarantee—are not universally credit constrained. Even without the guarantee, many of these firms secure additional financing, implying that guarantees may not be the decisive factor for credit access in this setting.

Next, we examine event-time patterns. Figure 5 plots an event study of firm-level loans outstanding from four quarters before to four quarters after the guarantee decision date, separately for accepted and rejected applications. Again, on average, the outstanding credit rises around the decision for both groups, with no visible difference across groups.

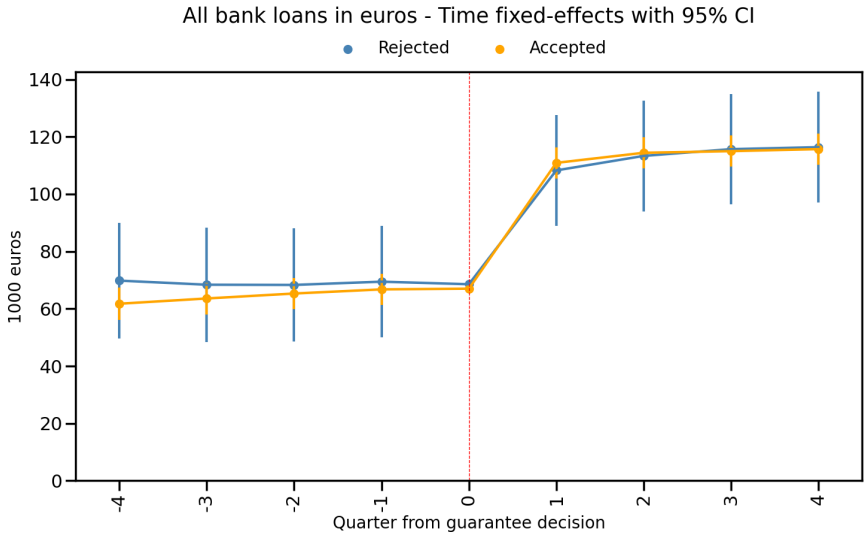
Finally, in Figure 6 we explore the relationship between examiner leniency—our instrumental variable—and firm-level credit growth. A binned scatter plot of $\log(\text{loans}+1)$ or a loan increase dummy six months after the guarantee decision against examiner leniency shows a mild negative association, reinforcing the view that guarantee approval does not systematically drive borrowing outcomes.

Overall, this descriptive evidence suggests that the guarantee is not the sole driver of firm-level credit growth. If firms can obtain loans without guarantees, the marginal effect of approval may be limited. To address this question directly, we move beyond descriptive patterns to our IV estimation.

5.2 The effect of guarantees on credit growth

Table 5 presents our baseline results, namely IV and OLS estimates for log(loans + 1) at applicant firm level six months after the guarantee decision. OLS regressions indicate a positive and statistically significant association between guarantee approval and subsequent borrowing. In contrast, IV estimates show a negative, yet statistically insignificant effect. The contrast between OLS and IV estimate implies that for applicants at the margin of approval, the positive guarantee deci-

Figure 5: Event study loans outstanding around the guarantee decision, accepted vs. rejected



The figure shows event study estimates for firm-level loans outstanding four quarter before and four quarters after the guarantee decision for accepted and rejected applications. The sample includes all guarantee applications during 2019–2023.

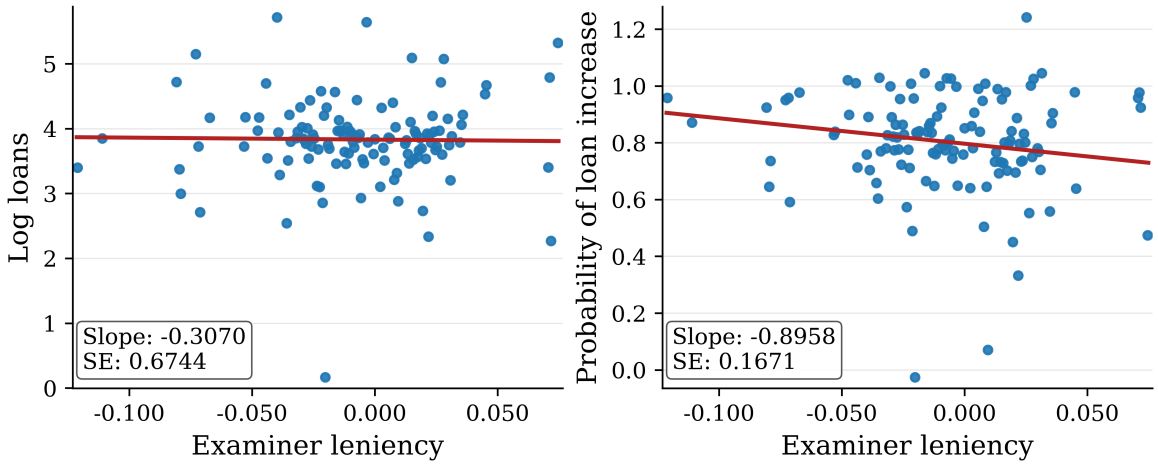
sion does not increase firm-level borrowing either because a) accepted applicants would have experienced growth in credit even without the guarantee (selection) or b) the effect of guarantee approval depends on the population studied (all applicants vs. marginal applicants).

To examine the evolution of IV estimates across different time horizons, Figure 7 plots IV estimates for log(loans + 1) from twelve months before to twelve months after the decision date. Pre-decision coefficients are positive yet statistically insignificant, providing support for the exclusion restriction. Post-decision estimates turn slightly negative, though remain statistically insignificant across all horizons. This confirms that our IV results are not driven by lags in loan withdrawals after the guarantee decision.

Next, we examine whether the effects of guarantees differ between the extensive and intensive margin. Table 6 reports IV and OLS estimates for firms without outstanding loans (columns 1–4, extensive margin) and firms with existing bank loans at the time of the guarantee decision (columns 5–8, intensive margin). The first finding is that the positive OLS results in Table 5 are driven by extensive margin effects: firms without prior lending relationships show a strong association between guarantees and credit growth. In contrast, firms with existing loans exhibit no such relationship. However, IV estimates are negative for both groups. Overall, these findings suggest that at the margin of approval, guarantees do not materially improve access to bank finance—even for new borrowers.

As a robustness checks, we repeat our analysis with alternative dependent vari-

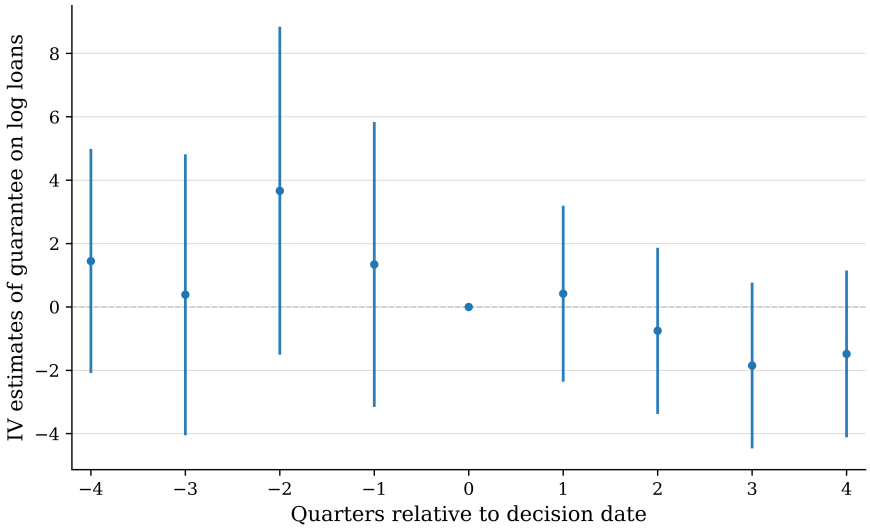
Figure 6: Binned scatter plot of loans and examiner leniency



Notes: These figures show binned scatterplots of firm-level credit growth versus examiner leniency. In the left hand side graph, credit growth is measured as the natural logarithm of loans outstanding + 1 six months after the guarantee decision. In the right hand side graph, the y-axis variable is a dummy that equals 1 if loans outstanding have increased six months after the guarantee decision. To construct the plots, we first regress the y-axis variable on risk score - time fixed effects and collect the residuals. We then add the mean y-axis variable value to each residual. The solid line shows the OLS estimate by using the underlying microdata. The sample includes all guarantee applications received during 2019–2023.

ables: a dummy that equals 1 if outstanding loans of the firm increase within 6 months after the guarantee decision (Table 7) and $log(loans + 1)$ at firm-bank level (Table 8), and by excluding March–December 2020, namely the onset of the Covid-19 with heightened acceptance rate (Table 9). In all of these cases, our results remain qualitatively unchanged.

Figure 7: IV estimates over time.



Notes: The figure shows IV estimates and their 95 % confidence intervals of the effect of the positive guarantee decision on firm-level credit growth measured as $log(loans + 1)$ four quarters before and four quarters after the guarantee decision. For pre-decision quarters, the sample includes firms with existing loans. For post-decision quarters the sample includes all guarantee applications during 2019–2023.

6 A Model of Public Loan Guarantees

Overall, our empirical results suggests that public loan guarantees have no effect on bank lending. To explore the mechanism underlying such result might arise, we construct a theoretical model of bank lending in the presence of public loan guarantees.

6.1 Assumptions

Consider the interaction between a positive measure of *firms* with access to identical investment projects and a competitive *bank* with access to liquid funds. Both the firms and the bank are risk-neutral, have market power, and there is no time preference. If a firm *invests* $k \in [0, \infty)$ units, its project pays $\pi(k)$ verifiable units with probability $p \in (0, 1)$ and zero otherwise. The conditional return function $\pi : [0, \infty) \to [0, \infty)$ satisfies the standard properties: $\pi'(k) > 0, \pi''(k) < 0, \pi(0) = 0,$ and $\lim_{k \to 0} \pi'(k) = \infty$. Here, and in what follows, we use the Lagrange notation for derivatives whenever no confusion may arise. For simplicity, we assume that the firms have no liquid funds to be invested in their project. Therefore, the firms must tap into the bank for financing their entire investment.

A *government agency* may *guarantee* (part of) firms' borrowing from the bank. We allow for the loan guarantee to affect the market for corporate credit via three

channels. First, a *default channel* subsidizes the bank: If a firm has a guarantee, the agency will reimburse a fraction $\gamma \in [0,1]$ of the loan amount to the firm's bank in the case of the firm's default. Thus, γ is the coverage of the loan guarantee. Second, a *Basel III channel* also subsidizes the bank: Basel III risk-weighted capital regulation implies that the bank may be able hold less capital against government-backed lending. For example, if a bank is using the internal-ratings based approach to calculate its capital requirements, it may invoke the high reliability to government guarantees to reduce its capital requirement. A bank using the standardized approach has less freedom in this respect. We assume that the bank can raise funds flexibly at rate $c \in (0,\infty)$ when the firm's borrowing is unguaranteed and that the guarantee reduces this rate by fraction s. Third, a *guarantee fee channel* directly increases firms' borrowing cost: To obtain a guarantee, a borrowing firm must pay a credit-guarantee fee f per guaranteed amount to the agency. We assume s and f to be weakly increasing functions of γ, with $s : [0,1] \to [0,1]$ and $f : [0,1] \to [0,\infty)$.

We do not model the government agency granting guarantees and its objective explicitly. Instead, we solve the model for any given γ and conduct comparative static analysis on it. In practice, the agency chooses between some $\gamma \in (0,1]$ (accepting a guarantee application) and $\gamma = 0$ (rejecting a guarantee application). For brevity, we focus on marginal changes in γ, and equate a marginal increase in γ with the agency's granting of a public loan guarantee.

We consider both firms with a *credit history* and firms without one. If a loan applicant has no credit history, the bank must incur a firm-specific *monitoring cost* after granting the loan. We model this monitoring cost as a random variable M whose distribution across firms is described by a cumulative distribution function $\Phi(m) = \Pr(M \leq m)$, with density $\phi(m) = \Phi'(m)$. We provide one financing friction foundation for M in Section 6.4, but M may be thought of reflecting information-based financing frictions faced by new borrowers quite broadly: E.g., we may thing that the bank faces a firm-specific fixed cost of initiating a loan to a new borrower, arising from administrative costs such as origination, underwriting, documentation, regulatory or due diligence costs.

If a firm has a credit history, the fixed cost is normalized to zero – administrative, information acquisition, and monitoring costs associated with lending to a familiar borrower are plausibly smaller than with a new lending relationship. Thus, these familiar borrowers are not assumed to suffer from standard information-based financing frictions. Nonetheless, even these borrowers may underinvest because the bank has market power and can charge a lending rate above the market rate. We next study whether public loan guarantees alleviate or amplify these financing frictions faced by new and old borrowers .

6.2 Firm's Investment and Borrowing

A firm's problem, irrespective of its credit history, is given by

$$\max_{k\in[0,\infty)} \Pi^F(k, r, \gamma) = p[\pi(k) - (r + f(\gamma))k] \tag{6}$$

in which r is the *lending interest rate* charged by the bank, so $r + f(\gamma)$ constitutes the firm's total marginal cost of credit. With probability p the firm's investment pays its return $\pi(k)$ in which case the firm pays back its loan with interest to the bank. We consider a standard debt contract whereby the bank has seniority and can seize the project's cashflow if the firm defaults. Thus, in equilibrium, the firm honors the contract if it has cashflow to do so. Moreover, as the firm has no liquid funds of its own, the firm pays the credit-guarantee fee $f(\gamma)$ to the agency only if it has cashflow to do so. The unique solution to the firm's problem of equation (6) is given by

$$k^F(r, \gamma) \equiv (\pi')^{-1}(r + f(\gamma)). \tag{7}$$

From equation (6) we can establish that the firm's borrowing and investment at the intensive margin is inversely related to the firm's marginal cost of credit $r + f(\gamma)$. Next, we characterize the relation between r and γ in equilibrium.

6.3 Effects of Loan Guarantee at the Intensive Margin

Consider the bank at the *intensive margin*, i.e., when the bank faces a firm with a credit history or a firm with no credit history after the bank has sunk the fixed cost of initiating a loan. The bank's profit function can be written as

$$\Pi^B\big(k^F(r, \gamma), r, \gamma\big) = \big[r(p + \gamma(1 - p)) - c(1 - s(\gamma))\big]k^F(r, \gamma) \tag{8}$$

in which $p + \gamma(1 - p)$ is the effective project success probability from the bank's perspective and $c(1 - s(\gamma))$ is the bank's marginal cost of funds. Thus, the term in the square-brackets is the bank's expected interest rate margin which we assume to be positive. The equilibrium lending rate is then given by

$$r^*(\gamma) \equiv \arg\max_{r\in[0,\infty)} \Pi^B(k^F(r, \gamma), r, \gamma). \tag{9}$$

Let us denote by $\varepsilon(r) \equiv (r/k^F)\partial k^F/\partial r < 0$, by $k^*(\gamma) \equiv k^F(r^*(\gamma), \gamma)$, and by $R^*(\gamma) \equiv r^*(\gamma) + f(\gamma)$, the interest rate elasticity of the firm's borrowing, the equilibrium borrowing of the firm, and the firm's equilibrium marginal cost of credit. We have the following result:

Proposition 1. *(i) Public loan guarantees increase (decrease) bank lending at the intensive margin if and only if they decrease (increase) the firms' marginal cost of credit, i.e.,*

$$k^{*\prime}(\gamma) \gtreqless 0 \quad \textit{iff} \quad R^{*\prime}(\gamma) = f'(\gamma) + \frac{dr^*}{d\gamma} \lesseqgtr 0.$$

(ii) The sign of the effect of public loan guarantees on the firms' marginal cost of credit, i.e., the sign of $R^{*\prime}(\gamma)$, is given by the sign of

$$(1-p)[1+\varepsilon(r^*(\gamma))] + \frac{\varepsilon(r^*(\gamma))}{r^*(\gamma)}\big[cs'(\gamma) - (p+\gamma(1-p))f'(\gamma)\big].$$

Proof. In Appendix A.

Part (i) of Proposition 1 shows that, at the intensive margin, the effect of public loan guarantees on bank lending is inversely related to the their effect on the firms' marginal cost of credit. The effect of public loan guarantees on the equilibrium marginal cost of credit can in turn be decomposed into the direct effect via the credit guarantee fee $f(\gamma)$ and the indirect effect via the lending interest rate $r^*(\gamma)$. Because the direct effect is positive, a necessary condition for the negative effect of public loan guarantees on the marginal cost of credit is that the guarantees reduce the bank's lending interest rate.

The effect of loan guarantees on lending interest rates is generally ambiguous. As a result, the effect of loan guarantees on the marginal cost of credit and bank lending at the intensive margin is also ambiguous. As shown by part (ii) of Proposition 1, the effect of the loan guarantees on the cost of credit depends on a complex mix of the three channels whereby loan guarantees affect the market for corporate credit: First, in the absence of the Basel III and guarantee fee channels ($s'(\cdot) \approx f'(\cdot) \approx 0$), loan guarantees reduce the firm's cost of credit only if the firm's borrowing is sufficiently sensitive to the changes in the interest rate ($\varepsilon(r^*) > -1$). Intuitively, if the firm's borrowing is unresponsive to changes in the lending rate, loan guarantees prompt the bank to charge a *higher* lending rate, because the bank will get its lending rate reimbursed in the case of default. Second, if the Basel III channel reduces the bank's cost of funds ($s'(\cdot) > 0$), the bank passes some of this reduction to borrowing firms – the rate of this pass trough depends on the interest rate elasticity. Thus, the Basel III channel tends to reduce the firm's cost of credit. Third, the guarantee fee channel increases the firms' cost of credit ($f'(\cdot) > 0$), but the bank may respond to this increase by reducing its lending rate. Thus, the magnitude of this positive effect of the guarantee fee channel on the cost of credit depends on the interest rate elasticity.

More generally, we may write the key equilibrium variables as $r^*(\gamma, c, p)$, $R^*(\gamma, c, p)$,

and $k^*(\gamma, c, p)$, and use equation (8) and part (i) of Proposition 1 to establish comparative statics with respect to c and p. It is straightforward to show that the bank's equilibrium lending rate r^* and, thereby, the firms' equilibrium marginal cost of credit R^* increase with the bank's cost of funds c. Thus, bank lending at the intensive margin k^* decreases with c. This effect reflects the bank lending channel of monetary policy transmission, because the Central Bank interest rate may be thought to directly affect c. Moreover, r^*, and, thereby, R^* decrease (increase) with the project success probability p if $\varepsilon(r^*) < (>) - 1$ and vice versa for k^*. Thus, the bank charges a more creditworthy firm with a lower (higher) interest rate and increases (decreases) its lending to that firm, if the firm's borrowing is sufficiently (in)sensitive to the changes in the interest rate. The explanation for this effect is analogous to one of the default channel of the public loan guarantees.

6.4 Effects of Loan Guarantee at the Extensive Margin

Consider next the bank's problem at the *extensive margin*, i.e., when the bank faces a *de novo* loan applicant with no credit history. In this case, the bank needs to incur the fixed cost M of initiating the loan, drawn from the cumulative distribution function $\Phi(m)$. Let $\Pi^{l*}(\gamma) \equiv \Pi^l\left(k^*(\gamma), r^*(\gamma), \gamma\right)$, $l \in \{F, B\}$, denote the firm's and bank's equilibrium profits. Then, the bank is willing to lend for the new loan applicant if and only if $\Pi^{B*}(\gamma) - m \geq 0$. Thus, new loan applicants with

$$m \leq \overline{m}(\gamma) = \Pi^{B*}(\gamma) \tag{10}$$

can borrow from the bank, and firms with $m > \overline{m}(\gamma)$ are shut out from the market of corporate credit.

One interpretation of the underlying financial friction follows from Holmström & Tirole (1997): M is a firm-specific monitoring cost that the bank must incur upon granting a loan, and its realization m is observed at least by the bank before the lending decision. Specifically, assume that a firm can choose between two projects. The good project is the one described in Section 6.1. The bad project fails with probability one but yields private benefits larger than $\Pi^{F*}(\gamma)$ to the firm's decision maker. The project choice and private benefits are non-verifiable to third parties. The bank's monitoring eliminates the bad project from the firm's choice set. Then, in equilibrium, the bank provides lending with monitoring for firms with $m \leq \overline{m}(\gamma)$ and rejects loan applications from firms with $m > \overline{m}(\gamma)$.

The aggregate bank lending to new loan applicants is given by

$$L(\gamma) \equiv \Phi(\overline{m}(\gamma))k^*(\gamma), \tag{11}$$

in which $\Phi(\overline{m}(\gamma))$ denotes the proportion of new loan applicants that obtain a loan and $k^*(\gamma)$ is the equilibrium lending per firm. From equations (10) and (11) we get the following result:

Proposition 2. *(i) Public loan guarantees improve (weaken) the access of new firms to bank lending if and only if they increase (reduce) the profitability of bank lending, i.e.,*

$$\overline{m}'(\gamma) \gtreqless 0 \quad \textit{iff} \quad \Pi^{B*\prime}(\gamma) \lesseqgtr 0.$$

(ii) The sign of the effect of public loan guarantees on the profitability of bank lending, i.e., the sign of $\Pi^{B*\prime}(\gamma)$, is given by the sign of

$$(1-p)(r^*(\gamma) - \gamma f'(\gamma)) + cs'(\gamma). \tag{12}$$

(iii) The effect of public loan guarantees on aggregate bank lending to firms with no credit history is given by the sign of

$$L'(\gamma) = k^*(\gamma)\phi(\overline{m}(\gamma))\overline{m}'(\gamma) + \Phi(\overline{m}(\gamma))k^{*\prime}(\gamma). \tag{13}$$

Proof. In Appendix A.

Part (i) of Proposition 2 suggests that if public loan guarantees improve the profitability of bank lending, banks can afford to expand their lending to more opaque firms requiring more monitoring or other scrutiny. Part (ii) characterizes the circumstances where that profitability increase happens: Equation (12) shows that the default and Basel III channels (captured by the terms $(1-p)r^*(\gamma)$ and $cs'(\gamma)$) improve profitability by subsidizing the bank, but the guarantee fee channel (captured by the term $(1-p)\gamma f'(\gamma)$) reduces profitability by reducing new firms' demand for credit. Nonetheless, part (i) suggest a revealed preference argument: Whenever we observe bank-backed applications for loan guarantees by new firms, equation (12) should be positive and, accordingly, loan guarantees should improve those firms' access to credit.

Part (iii) characterizes the effect of loan guarantees on aggregate bank lending to new firms. The sign of the first term on the right-hand side of equation (13) captures the effects on bank lending to *marginal* new firms, i.e., on those new firms whose access to bank lending is affected by public loan guarantees. The sign of this term follows from parts (i) and (ii) and, as argued above, may be assumed to be positive whenever we observe guarantee applications of new firms. The second term on the right-hand side of equation (13) captures the effects on *intramarginal* new firms, i.e., on those new firms that would have obtained loans in the absence of public loan guarantees. The sign of the second term follows from Proposition 1:

It is non-negative if and only if public loan guarantees do not increase firms' cost of credit.

Thus, assuming that public loan guarantees improve the profitability of bank lending to new firms, a sufficient condition for public loan guarantees to increase aggregate bank lending to new firms is that they do not increase the firms' marginal cost of credit and, by implication, do not decrease bank lending to firms with a credit history. If the second term characterizing the sign of $L'(\gamma)$ is strictly negative, however, the effect of public loan guarantees on aggregate bank lending to new loan applicants is ambiguous. As a result, even if public loan guarantees improve access of new firms to bank lending, they do not necessarily increase bank lending to new firms, because each intramarginal new firm may borrow less in the presence of public loan guarantees.

6.5 Mechanism: The cost of credit

To understand why guarantees fail to expand borrowing, we examine their effect on loan pricing. Specifically, if banks do not fully account for the reduced credit risk provided by the guarantee, the overall cost of credit (interest rate plus guarantee fees) may be higher with a guarantee than without one. In such cases, firms would have little incentive to increase borrowing following a positive guarantee decision.

Table 10 reports IV and OLS estimates of the positive guarantee decision for weighted average loan margins (interest rate spread) for firms that have increased borrowing six months after the guarantee decision. While OLS estimates are modest—around –13 basis points—IV estimates indicate a clear negative effect: the guarantee decision reduces the weighted average loan margin by 1.94–2.24 percentage points.

To assess the impact on the total cost of credit, this reduction must be compared to the costs associated with guarantees. Finnvera guarantees involve an annual premium of 0.95%–1.95% of the guaranteed amount, depending on product type and applicant risk profile, plus a one-off service fee of 1% (temporarily lowered to 0.1% during the pandemic). In total, guarantee costs amount to roughly 2%–3% of the guaranteed amount, which—given a coverage ratio of 50%–80%—corresponds to 1%–2.4% of the borrowed amount. Thus, margin reductions broadly offset guarantee costs but do not significantly lower the total cost of credit. Consequently, both accepted and rejected applicants experience similar changes in total borrowing costs after the guarantee decision, which helps explain the absence of differential credit growth between the two groups.

7 Conclusion

This paper offers new evidence on the lending effects of public loan guarantees using an examiner leniency design and comprehensive guarantee application data. In our setting, guarantee approval does not increase firm-level borrowing; if anything, it reduces credit for applicants at the margin of approval. As both accepted and rejected applicants increase their borrowing after the guarantee decision, banks may act as gatekeepers, filtering out truly credit-constrained firms before applications reach the guarantee provider.

Prior research on Finnvera guarantees documents positive relationship between guarantees and real outcomes such as sales growth, employment, and investment (Ylhäinen, 2013; Kotamäki, 2025). Taken together our findings and existing literature, it seems that guarantees may operate through channels other than immediate credit growth—such as improving the stability of banking relationships, facilitating refinancing, or extending loan maturities. Understanding these alternative mechanisms is an important avenue for future research, particularly in light of policy debates on the role of public guarantee programs in supporting small and medium-sized enterprises.

References

Abadie, A., Athey, S., Imbens, G. W., & Wooldridge, J. M. (2022, 10). When should you adjust standard errors for clustering?*. *The Quarterly Journal of Economics*, *138*(1), 1-35.

Aizer, A., & Doyle Jr, J. J. (2015). Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges. *The Quarterly Journal of Economics*, *130*(2), 759–803.

Altavilla, C., Ellul, A., Pagano, M., Polo, A., & Vlassopoulos, T. (2025). Loan guarantees, bank lending and credit risk reallocation. *Journal of Financial Economics*, *172*, 104137.

Bachas, N., Kim, O. S., & Yannelis, C. (2021). Loan guarantees and credit supply. *Journal of Financial Economics*, *139*(3), 872–894.

Bhuller, M., Dahl, G. B., Løken, K. V., & Mogstad, M. (2020). Incarceration, recidivism, and employment. *Journal of Political Economy*, *128*(4), 1269–1324.

Bonfim, D., Custódio, C., & Raposo, C. (2023). Supporting small firms through recessions and recoveries. *Journal of Financial Economics*, *147*(3), 658–688.

Cascarino, G., Gallo, R., Palazzo, F., & Sette, E. (2022). Public guarantees and credit additionality during the covid-19 pandemic. *Bank of Italy Temi di Discussione (Working Paper)*, *1369*.

Chyn, E., Frandsen, B., & Leslie, E. (2025). Examiner and judge designs in economics: a practitioner's guide. *Journal of Economic Literature*, *63*(2), 401–439.

De Blasio, G., De Mitri, S., D'Ignazio, A., Russo, P. F., & Stoppani, L. (2018). Public guarantees to sme borrowing. a rdd evaluation. *Journal of Banking & Finance*, *96*, 73–86.

Farre-Mensa, J., Hegde, D., & Ljungqvist, A. (2020). What is a patent worth? evidence from the us patent "lottery". *The Journal of Finance*, *75*(2), 639–682.

González-Uribe, J., & Reyes, S. (2021). Identifying and boosting "gazelles": Evidence from business accelerators. *Journal of Financial Economics*, *139*(1), 260–287.

Gonzalez-Uribe, J., & Wang, S. (2020). The effects of small-firm loan guarantees in the uk: Insights for the covid-19 pandemic crisis. *Workin paper*.

Güler, O., & Samarin, I. (2023). Who gets publicly guaranteed loans? the effect of guarantee feeson loan allocation and pricing. *Working Paper*.

Hirvonen, J., Kässi, O., & Ropponen, O. (2023). Jobs, workers, and firms: Dissecting the labour market effects of finland's covid-19 subsidy program. *ETLA Working Papers 111*.

Holmström, B., & Tirole, J. (1997). Financial intermediation, loanable funds, and the real sector. *Quarterly Journal of Economics*, *112*, 663-982.

Huttunen, K., Kaila, M., Macdonald, D., & Nix, E. (2022). Punishing financial crimes: The impact of prison sentences on defendants and their colleagues. *American Economic Journal: Economic Policy (Forthcoming)*.

Jiménez, G., Laeven, L., Martinez-Miera, D., & Peydró, J.-L. (2024). Public guarantees, private banks' incentives, and corporate outcomes: evidence from the covid-19 crisis. *ECB Working Paper Series No 2913*.

Kling, J. R. (2006). Incarceration length, employment, and earnings. *American Economic Review*, *96*(3), 863–876.

Kotamäki, M. (2025). Purpose matters: Impact of public loan guarantees on business growth. *Economics Letters*, *254*, 112455.

Martin, A., Mayordomo, S., & Vanasco, V. (2025). *Banks vs. firms: who benefits from credit guarantees?*

Neugebauer, K., & Mateus, M. (2025). Stayin' alive? government support measures in portugal during the covid-19 pandemic. *Portuguese Economic Journal*, *24*, 335-372.

Righi, C., & Simcoe, T. (2019). Patent examiner specialization. *Research Policy*, *48*(1), 137-148.

Ylhäinen, I. (2013). Essays on the economics of small business finance. *Doctoral dissertation*.

A Proofs of the Theoretical Model

Proof of Proposition 1. (i) Differentiating $k^*(\gamma) \equiv k^F(r^*(\gamma), \gamma)$ using equation (7) gives

$$k^{*\prime}(\gamma) = \frac{\partial k^F}{\partial r}\frac{dr^*}{d\gamma} + \frac{\partial k^F}{\partial \gamma} = \frac{1}{\pi''(k)}\left(\frac{dr^*}{d\gamma} + f'(\gamma)\right).$$

Because $\pi''(k) < 0$, the sign of $k^{*\prime}(\gamma)$ is inversely related to the sign of

$$R^{*\prime}(\gamma) = f'(\gamma) + \frac{dr^*}{d\gamma}. \tag{A1}$$

(ii) Because $r^*(\gamma)$ solves $\partial \Pi^B(k^F(r,\gamma), r, \gamma)/\partial r = 0$, totally differentiating this first-order condition gives

$$\frac{dr^*}{d\gamma} = -\frac{\left.\frac{\partial^2 \Pi^B}{\partial r \partial \gamma}\right|_{r=r*}}{\left.\frac{\partial^2 \Pi^B}{\partial r^2}\right|_{r=r^*}}. \tag{A2}$$

Taking derivatives in equation (8) yields

$$\begin{aligned}\left.\frac{\partial^2 \Pi^B}{\partial r \partial \gamma}\right|_{r=r^*} = (1-p)k^*(\gamma)(1+\varepsilon(r^*)) + \big[(p+\gamma(1-p))f'(\gamma) + cs'(\gamma)\big]\frac{\partial k^F}{\partial r} \\ + \big[r^*(p+\gamma(1-p)) - c(1-s(\gamma))\big]f'(\gamma)\frac{\partial^2 k^F}{\partial r^2},\end{aligned} \tag{A3}$$

in which we have used $\partial k^F/\partial \gamma = f'(\gamma)\partial k^F/\partial r$ and $\partial^2 k^F/(\partial r \partial \gamma) = f'(\gamma)\partial^2 k^F/\partial r^2$ from equation (7), and

$$\left.\frac{\partial^2 \Pi^B}{\partial r^2}\right|_{r=r^*} = 2(p+\gamma(1-p))\frac{\partial k^F}{\partial r} + \big[r^*(p+\gamma(1-p)) - c(1-s(\gamma))\big]\frac{\partial^2 k^F}{\partial r^2}. \tag{A4}$$

Because $r^*(\gamma) \equiv \arg\max_{r\in[0,\infty)} \Pi^B(k^F(\cdot,\gamma),\cdot,\gamma)$, we assume that $\partial^2 \Pi^B/\partial r^2 \,|_{r=r^*} < 0$. For this second-order condition, equation (A4) implies as a sufficient condition, e.g., $\partial^2 k^F/\partial r^2 \leq 0$.

Substitution of equations (A2)–(A4) for $dr^*/d\gamma$ in equation (A1) and simplifying gives

$$R^{*\prime}(\gamma) = \frac{k^*(\gamma)\left\{\frac{\varepsilon(r^*)}{r^*}\big[(p+\gamma(1-p))f'(\gamma) - cs'(\gamma)\big] - (1-p)\big[1+\varepsilon(r^*\big]\right\}}{2(p+\gamma(1-p))\frac{\partial k^F}{\partial r} + \big[r^*(p+\gamma(1-p)) - c(1-s(\gamma))\big]\frac{\partial^2 k^F}{\partial r^2}}. \tag{A5}$$

The denominator of equation (A5) is equation A4 and is hence strictly negative. As a result, the sign of $R^{*\prime}(\gamma)$ is given by the opposite sign of the term in the curly brackets in the numerator of equation (A5). ■

Proof of Proposition 2. (i) Part (i) directly follows from differentiation of equation (10). (ii) Differentiating equation (10) using equation (8) and the Envelope Theorem gives after some tedious algebra

$$\Pi^{B*\prime}(\gamma) = [(1-p)(r^*(\gamma) - \gamma f'(\gamma)) + cs'(\gamma)]k^*(\gamma),$$

which shows how the sign of $\Pi^{B*\prime}(\gamma)$ is given by the sign of the term in the square brackets on the right-hand side.

(ii) Differentiating equation (11) gives the equation for $L'(\gamma)$. ■.

B Tables

Table 1: First stage estimates.

	Decision	
	(1)	(2)
Leniency	0.54***	0.53***
	(0.13)	(0.13)
Weak instrument F statistic	16.98	16.41
Controls	No	Yes
Observations	9777	9777
Firms	7773	7773
Banks	12	12
Examiners	44	44

Notes: Table presents first-stage results. The dependent variable is an indicator equal to 1 (0) when the guarantee application is accepted (rejected). The independent variable is the leave-one-out measure of examiner leniency. Column 1 reports results without and Column 2 with additional controls: industry dummies and a dummy equal to 1 if the firm is in the Capital region. Both specifications include risk score–time fixed effects. The sample includes all guarantee applications during 2019–2023. *p<0.1; **p<0.05; ***p<0.01

Table 2: Summary statistics.

	Accepted applications		Rejected applications	
	Count	Mean	Count	Mean
Loans outstanding	8,929	65.670	848	66.130
Applied guarantee amount	8,929	44.510	848	54.160
Change in outstanding loans	8,929	47.300	848	46.420
Loan increase dummy	8,929	0.800	848	0.730
New loan contract	8,929	0.590	848	0.400
Loan margin of existing loans	4,539	2.030	391	2.090
Loan margin six months after the decision	8,142	2.470	729	2.570
Manufacturing	8,929	0.110	848	0.130
Construction	8,929	0.140	848	0.110
Trade	8,929	0.210	848	0.170
Service	8,929	0.520	848	0.550
High risk	8,929	0.680	848	0.760
Capital district	8,929	0.250	848	0.240
Number of employees	8,854	4.240	847	4.470
Revenue	5,677	0.810	440	0.730

Notes: Table presents summary statistics separately for accepted and rejected guarantee applications. Loans outstanding is the applicant firm's outstanding loans at the time of the guarantee decision in EURth. Applied guarantee amount is shown in EURth. Change in outstanding loans is the change in outstanding loans six months after the guarantee decision in EURth. Loan increase dummy is a dummy that equals 1 if the applicant firm experiences an increase in loans outstanding six months after the guarantee decision. New loan contract is a dummy that equals 1 if the applicant firm obtains a new loan contract within six months after the guarantee decision. Loan margin of existing loans in percentages is calculated for firms with existing loans. Loan margin six months after the decision is calculated firms with loans outstanding six months after the guarantee decision. Manufacturing, construction,trade and services are industry dummies. High risk is a dummy if the internal risk category of the applicant firm is B or below. Capital region is a dummy that equals 1 if the applicant firm is located in the Helsinki metropolitan area. Revenue is shown in EURmn. The sample includes all guarantee applications during 2019–2023.

Table 3: Balance check.

	Decision (1)	Leniency (2)
Intercept	0.972***	-0.0079
	(0.007)	(0.0120)
Prior loan	0.004	0.0011**
	(0.006)	(0.0005)
Trade	0.020***	-0.0000
	(0.007)	(0.0006)
Capital district	0.007	0.0001
	(0.006)	(0.0005)
Employer	0.017**	0.0005
	(0.007)	(0.0005)
Small application	0.041***	-0.0006
	(0.006)	(0.0005)
Observations	9777	9777
Firms	7773	7773
Banks	12	12
Examiners	44	44
F-test	F = 11.9***	F = 1.77
Mean (dependent variable)	0.913	-0.0007

Notes: Table shows results for regressing the guarantee decision dummy and examiner leniency on the set of observed firm and application level characteristics. Prior loan is a dummy that equals 1 if the applicant firm has loans outstanding at the time of the guarantee decision. Trade is a dummy that equals 1 if the applicant firm operates in wholesale or retail trade industry. Capital district is a dummy that equals 1 if the firm is located in the Helsinki metropolitan area. Employer is a dummy that equals 1 if the number of employees is greater than one. Small application is a dummy if the applied guarantee amount is at most EUR 25 000. Both specifications include risk score–time fixed effects. The sample includes all guarantee applications during 2019–2023. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 4: First-stage estimates by subsample

	Capital region	Outside Capital region	Construction and manufacturing	Trade and services	Prior loans	No prior loans
	(1)	(2)	(3)	(4)	(5)	(6)
Leniency	0.87***	0.43***	0.64**	0.56***	0.46***	0.60***
	(0.27)	(0.15)	(0.27)	(0.15)	(0.17)	(0.20)
Controls	No	No	No	No	No	No
Observations	2444	7333	2414	7158	4930	4847
Firms	1931	5842	1876	5738	3901	4365
Banks	11	12	12	11	12	11
Examiners	44	44	44	44	44	44

Notes: Table shows first-stage results for different subsamples. The dependent variable is an indicator equal to 1 (0) when the guarantee application is accepted (rejected). The independent variable is the leave-one-out measure of examiner leniency. All specifications include risk score–time fixed effects. The samples include all guarantee applications during 2019–2023 for the given subpopulation of firms.*p<0.1; **p<0.05; ***p<0.01

Table 5: IV and OLS estimates of the effect of the positive guarantee decision on firm-level credit growth.

	IV		OLS	
	(1)	(2)	(3)	(4)
Intercept	3.80***	4.54***	3.00***	3.65***
	(1.38)	(1.32)	(0.43)	(0.42)
Decision (0/1)	−0.63	−0.76	0.17***	0.18***
	(1.33)	(1.34)	(0.06)	(0.06)
Controls	No	Yes	No	Yes
Observations	9777	9777	9777	9777
Firms	7773	7773	7773	7773
Banks	12	12	12	12
Examiners	44	44	44	44

Notes: Table presents IV and OLS estimates of the effect of the positive guarantee decision on firm-level credit growth, measured by the natural logarithm of loans outstanding + 1 six months after the guarantee decision. Columns 1 and 3 report results without and Columns 2 and 4 with additional controls: industry dummies and a dummy equal to 1 if the firm is in the Capital region. All specifications include risk score–time fixed effects. The sample includes all guarantee applications during 2019–2023. *p<0.1; **p<0.05; ***p<0.01

Table 6: IV and OLS estimates of the effect of the positive guarantee decision on firm-level credit growth: extensive vs. intensive margin

	Extensive margin: firms without prior loans				Intensive margin: firms with prior loans			
	IV		OLS		IV		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	4.80***	5.14***	2.88***	3.52***	5.72***	6.22***	3.20***	3.71***
	(1.75)	(1.65)	(0.54)	(0.57)	(1.89)	(1.79)	(0.51)	(0.45)
Decision (0/1)	−1.64	−1.46	0.28***	0.29***	−2.51	−2.62	0.01	0.02
	(1.67)	(1.67)	(0.09)	(0.09)	(1.86)	(1.87)	(0.07)	(0.07)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4847	4847	4847	4847	4930	4930	4930	4930
Firms	4365	4365	4365	4365	3901	3901	3901	3901
Banks	11	11	11	11	12	12	12	12
Examiners	44	44	44	44	44	44	44	44

Notes: Table presents IV and OLS estimates of the effect of the positive guarantee decision on firm-level credit growth, measured by the natural logarithm of loans outstanding + 1 six months after the guarantee decision for firms without and with loans outstanding at the time of the guarantee decision. Columns 1–4 show results for firms without and Columns 5–8 for firms with outstanding loans at the time of the guarantee decision. Columns 1, 3, 5 and 7 report results without and Columns 2, 4, 6 and 8 with additional controls: industry dummies and a dummy equal to 1 if the firm is in the Capital region. All specifications include risk score–time fixed effects. The sample includes all guarantee applications during 2019–2023. *p<0.1; **p<0.05; ***p<0.01

Table 7: IV and OLS estimates of the effect of the positive guarantee decision on firm-level credit growth: dummy dependent variable.

	IV		OLS	
	(1)	(2)	(3)	(4)
Intercept	2.65***	2.54***	0.73***	0.74***
	(0.35)	(0.34)	(0.11)	(0.11)
Decision (0/1)	−1.84***	−1.82***	0.09***	0.09***
	(0.33)	(0.34)	(0.02)	(0.02)
Controls	No	Yes	No	Yes
Observations	9777	9777	9777	9777
Firms	7773	7773	7773	7773
Banks	12	12	12	12
Examiners	44	44	44	44

Notes: Table presents IV and OLS estimates of the effect of the positive guarantee decision on firm-level credit growth, measured by the dummy that equals 1 if outstanding loans of the firm have increased six months after the guarantee decision. Columns 1 and 3 report results without and Columns 2 and 4 with additional controls: industry dummies and a dummy equal to 1 if the firm is in the Capital region. All specifications include risk score–time fixed effects. The sample includes all guarantee applications during 2019–2023. *p<0.1; **p<0.05; ***p<0.01

Table 8: IV and OLS estimates of the effect of the positive guarantee decision on firm-bank-level credit growth.

	IV		OLS	
	(1)	(2)	(3)	(4)
Intercept	4.53***	4.95***	2.76***	3.28***
	(1.52)	(1.46)	(0.44)	(0.45)
Decision (0/1)	−1.36	−1.35	0.40***	0.42***
	(1.47)	(1.48)	(0.07)	(0.07)
Controls	No	Yes	No	Yes
Observations	9737	9737	9737	9737
Firms	7751	7751	7751	7751
Banks	9	9	9	9
Examiners	44	44	44	44

Notes: Table presents IV and OLS estimates of the effect of the positive guarantee decision on firm-bank-level credit growth, measured by the natural logarithm of loans outstanding (from the bank granting the guaranteed loan) + 1 six months after the guarantee decision. Columns 1 and 3 report results without and Columns 2 and 4 with additional controls: industry dummies and a dummy equal to 1 if the firm is in the Capital region. All specifications include risk score–time fixed effects. The sample includes all guarantee applications during 2019–2023. *p<0.1; **p<0.05; ***p<0.01

Table 9: IV and OLS estimates of the effect of the positive guarantee decision on firm-level credit growth: excluding March–December 2020.

	IV		OLS	
	(1)	(2)	(3)	(4)
Intercept	4.95***	5.62***	2.94***	3.52***
	(1.33)	(1.29)	(0.44)	(0.43)
Decision (0/1)	−1.78	−1.96	0.23***	0.25***
	(1.26)	(1.28)	(0.07)	(0.07)
Controls	No	Yes	No	Yes
Observations	7996	7996	7996	7996
Firms	6431	6431	6431	6431
Banks	12	12	12	12
Examiners	44	44	44	44

Notes: Table presents IV and OLS estimates of the effect of the positive guarantee decision on firm-level credit growth, measured by the natural logarithm of loans outstanding + 1 six months after the guarantee decision. Columns 1 and 3 report results without and Columns 2 and 4 with additional controls: industry dummies and a dummy equal to 1 if the firm is in the Capital region. All specifications include risk score–time fixed effects. The sample includes all guarantee applications during 2019–2023, excluding March–December 2020. *p<0.1; **p<0.05; ***p<0.01

Table 10: IV and OLS estimates of the effect of the positive guarantee decision on the weighted average loan margin.

	IV		OLS	
	(1)	(2)	(3)	(4)
Intercept	4.50***	3.78***	2.40***	2.08***
	(1.21)	(1.14)	(0.32)	(0.33)
Decision (0/1)	−2.24*	−1.94*	−0.13***	−0.13***
	(1.16)	(1.15)	(0.05)	(0.05)
Controls	No	Yes	No	Yes
Observations	7795	7795	7795	7795
Firms	6477	6477	6477	6477
Banks	12	12	12	12
Examiners	44	44	44	44

Notes: Table presents IV and OLS estimates of the effect of the positive guarantee decision on the weighted average loan margin in percentages. Columns 1 and 3 report results without and Columns 2 and 4 with additional controls: industry dummies and a dummy equal to 1 if the firm is in the Capital region. All specifications include risk score–time fixed effects. The sample includes all guarantee applications during 2019–2023. *p<0.1; **p<0.05; ***p<0.01