Credit Guarantees and the Price of Credit in Crises: Evidence from Belgium [1]

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Abstract

A central challenge of macroeconomic policy is stimulating demand during crises when risk premia rise and credit markets tighten. This paper shows that the *price* of credit—not only access—is a key margin of adjustment. We exploit Belgium's 2020 Credit Guarantee Scheme, which featured discontinuous variation in the effective borrowing cost for eligible firms around a size cutoff. Using a regression discontinuity design, we estimate a local intensive-margin elasticity of investment with respect to the marginal borrowing rate on newly originated credit affected by eligibility. Lower borrowing costs raise investment, employment, revenues, and firm survival, primarily through substitution away from more expensive market loans. A quantitative model disciplined by the estimated elasticities is used to evaluate alternative guarantee regimes: one-off, unexpected guarantees increase borrower value in the short run, while recurrent and anticipated programs can raise leverage and default risk.

Keywords: Credit guarantees, price frictions, defaults, regression discontinuity design, rollover risk

JEL Codes: E32, G21, H81

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1 Introduction

Stimulating aggregate demand during crises is a central focus of macroeconomic policy debates. Credit guarantee schemes (CGSs), in which a third party—typically the government—partially or fully guarantees bank loans, are among the most widely used crisis interventions in modern credit markets.[1] By shifting default risk away from lenders, guarantee schemes can lower the marginal cost of external finance precisely when risk premia spike and firms face heightened rollover risk (low debt-service capacity).[2] Yet the macro consequences of such programs remain contested: do guarantees meaningfully stabilize investment, or do they mainly reallocate credit while raising leverage and default risk in the medium run? This paper provides new evidence on this trade-off by exploiting quasi-experimental variation in the effective borrowing cost induced by eligibility rules in a large-scale credit guarantee program.

Our contribution is threefold. First, we estimate a policy-relevant elasticity of real activity with respect to the marginal cost of newly originated credit affected by guarantee eligibility, isolating an intensive-margin price effect for firms at the eligibility threshold. We exploit a rule in Belgium's 2020 CGS that featured a discontinuous 25-basis-point wedge in the *all-in* price of guaranteed credit (interest rate plus guarantee fee) at 50 employees: firms below the cutoff faced a 25bp lower guarantee fee than firms above. Because the fee differential is remitted to the government rather than to lenders, this discontinuity shifts borrowers' borrowing costs without mechanically increasing banks' interest income. Using a regression discontinuity design (RDD), we compare firms just above and below the threshold—which are similar on observables and face the same program availability—to estimate the causal effect of lower borrowing costs on firm outcomes. Second, we document how the program reshapes debt composition, separating substitution toward

[1] In the U.S., the Small Business Acts (1953, 1958, 1992), the American Recovery and Reinvestment Act (2009), and the Paycheck Protection Program under the CARES Act (2020) all relied extensively on loan guarantees. In the European Union, all 27 member states implemented CGSs during the COVID-19 crisis, making them the single largest fiscal measure—59% of total interventions (IMF, 2020). Figure A1 in the Appendix summarizes the widespread adoption of these policies: more than 100 countries have implemented a CGS to date.

[2] We proxy rollover pressure using EBITDA/Short-term debt, which we refer to as *debt-service capacity*: lower values indicate greater rollover pressure (difficulty meeting or refinancing near-term obligations).

cheaper guaranteed borrowing from extensive-margin changes in credit access, and we present evidence addressing bank-side selection concerns.

While the RDD delivers clean causal estimates, its main limitation is external validity: it captures short-run effects for firms near the employment threshold under a temporary Belgian program. This makes it difficult to draw broader conclusions about the overall effectiveness of credit guarantees. Our third contribution directly addresses this concern by developing a structural model. To discipline the model, we compare two firms that differ only in the guarantee fee (25 vs. 50 basis points), aligning one-to-one with the RDD estimates of investment and non-guaranteed debt elasticities with respect to borrowing costs. This strategy allows us to use the internally valid microelasticities from the RDD as inputs into a framework that delivers externally valid insights—capturing long-run dynamics, endogenous pricing feedbacks, extensive-margin responses, and the effects of recurrent interventions. In short, the model transforms the RDD's local evidence into the general lessons about policy design and characterize the stabilization–risk trade-off: one-off, unexpected guarantees raise investment in the short run, while recurrent and anticipated guarantees can increase leverage and default risk and shift tail exposure.

Returning to our empirical results, we find that reducing interest rates for guaranteed loans improves firms' real outcomes. Specifically, compared to firms paying higher interest rates, those benefiting from lower rates experienced increases in investment rate, employment growth, and revenue growth by 0.20 percentage points (pp), 0.28 pp, and 0.42 pp, respectively, along with a 0.086 pp decline in firm exit rates. Per the validity of our design, firms near the eligibility cutoff did not differ in all these dimensions in the year before the policy's implementation.

Credit guarantees can affect aggregate outcomes even when total bank credit is limited, because they change the *price schedule* and risk allocation of lending in precisely the states where borrowing constraints bind. In our setting, the policy generates a discrete reduction in the marginal borrowing cost on newly originated guaranteed credit. This matters for two reasons. First, during COVID, risk premia rose and banks tightened underwriting, so the marginal cost of external finance became a first-order determinant of firms' ability to finance working capital and investment. Second, the policy operates on the *marginal* cost of new credit rather than the average cost of the existing debt stock: even a modest change in average interest expenses can correspond to a substantial change in the user cost on the incremental euro of borrowing that finances investment or prevents inefficient liquidation. Accordingly, we interpret our estimates as evidence that the *price of external finance* is an economically relevant margin in crisis times, operating through substitution toward cheaper guaranteed loans and improved rollover/liquidity conditions.

Consistent with a price-based mechanism, firms on both sides of the cutoff accumulate similar amounts of guaranteed debt, but they adjust the composition of their liabilities. Firms facing a 25 bp lower interest-rate schedule on guaranteed loans reduce their non-guaranteed borrowing by 0.26 percentage points, implying substitution away from more expensive market credit toward guaranteed credit. This compositional shift modestly lowers average interest costs—which are mechanically tied to the entire outstanding debt stock—but more importantly it reduces the marginal cost of new external finance and improves short-run financial resilience, as captured by an increase in our debt-service capacity measure by 0.28 percentage points.

We interpret these patterns as evidence that the policy operates primarily by easing crisis-era financing conditions on the marginal unit of new credit and by improving firms' short-run liquidity/rollover position. Although the fee wedge is 25 bps, it applies to newly originated guaranteed borrowing rather than to the average cost of the outstanding debt stock. In crisis states, small changes in the marginal price can have large real effects because financing decisions feature nonlinearities and thresholds (e.g., rollover and coverage constraints). Moreover, the policy induces substitution away from more expensive non-guaranteed borrowing, so the relevant change in the user cost of funds for the marginal financed activity can be larger than the direct 25 bp wedge. A quantitative model disciplined by the estimated elasticities illustrates how a COVID-type shock can amplify these mechanisms.

Our empirical findings suggest that the following are desirable properties of a quantitative model designed to study the CGS. First, the model should generate default in equilibrium. Second, the policy should induce a debt-substitution effect upon its introduction—an empirically central mechanism for the policy's success. Third, a successful model should capture the core co-movements observed during the COVID-19 episode. We develop a quantitative model within this context. A brief overview is as follows: a borrower can access two types of loans from lenders: (i) a non-contingent (standard) long-term unsecured loan and (ii) a credit guarantee loan, which includes provisions ensuring loan repayments to lenders in case of a loan default. Credit guarantee loans are available exclusively during liquidity shocks. Importantly, the pricing of standard loans is endogenously determined. There are two types of aggregate shocks: (i) liquidity shocks, which can act as a credit supply shock by reducing the available pool of funds to borrowers or as a shock that increases a firm's operating expenses, and (ii) income shocks. We allow these shocks to be correlated, consistent with our empirical observations. We calibrate the model using available Belgian macro and micro administrative data.

Our quantitative analysis departs from previous contributions along several key dimensions by solving a production economy in which borrowers have access to two distinct assets. To mirror the empirical approach, we compare two representative firms, consistent with the regression discontinuity design that exploits sharp variation between narrowly defined groups (e.g., firms with 50 vs. 51 employees within a bandwidth). We replicate this empirical discontinuity in the model using a two-type setup. The credit guarantee policy is modeled as a one-time, unanticipated intervention that is not expected to recur. This specification allows the model to capture the local and short-run effects identified in our empirical analysis. To validate the model, we replicate the quasi-experimental variation in borrowing costs—a 25 basis-point reduction in interest rates on guaranteed loans—and compare the model-implied elasticities to the empirical estimates. A 1% decrease in borrowing costs raises investment by 0.038% and reduces non-guaranteed debt by 0.023% in the data, compared to 0.022% and 0.036% in the model, respectively. These effects are of similar magnitude and direction, indicating that the model captures the core empirical elasticities along the price channel and debt substitution margin.

We then extend the model to a setting in which the policy is recurrent and anticipated, allowing firms to incorporate its future availability into their expectations. The model implies that the crisis-era *debt-service/rollover pressure* identified empirically is alleviated when the policy is introduced unexpectedly, but can be exacerbated when the policy becomes recurrent. The intuition is as follows. Under an unanticipated intervention, firms primarily use cheaper guaranteed credit to substitute away from more expensive market borrowing, improving their short-run liquidity position. In contrast, when subsidized credit is expected to recur in downturns, firms borrow more ex ante and self-insure less, because future access to cheaper credit partially substitutes for precautionary balance-sheet adjustments. This increases leverage and default risk.

When credit guarantees become a recurrent fixture, banks expect that in adverse states, the government will bail out borrowers by providing cheap guaranteed loans. This makes default in bad times less costly ex ante: (i) the downside of risky borrowing is partially socialized, and (ii) firms can roll over debt more easily when fundamentals deteriorate. Lenders offering standard loans rationally anticipate a higher future risk of default, as firms become less disciplined in their borrowing behavior and more leveraged on average. They also expect greater endogenous exposure to adverse shocks, as firms take on more risk or borrow more aggressively. Consequently, spreads on standard loans increase to compensate lenders for the higher expected losses.

Why do defaults rise? The intuition is that because credit is easier to obtain—and especially because it is guaranteed in downturns, firms have less incentive to de-leverage in good times or maintain precautionary savings buffers. Over time, average leverage in the economy increases, firms' resilience to negative shocks declines, and default becomes a relatively more attractive option. Recurrent credit guarantees thus reduce the cost of default, leading to a higher long-run frequency of default events.

An interesting question—one that would be overlooked in a model without dynamic investment decisions—is why investment and capital formation nonetheless increase. At first glance, this may appear counterintuitive: if spreads and defaults rise, why do firms invest more? This outcome would indeed reversed in a model with static investment. However, in this dynamic setting, cheaper credit in downturns is equivalent to a lower effective user cost of capital. Firms anticipate that when liquidity dries up, they will still be able to access funds at subsidized rates. This expectation reduces the need for precautionary liquidity buffers and encourages more aggressive investment. Even if spreads on standard loans increase somewhat, the effective borrowing constraint becomes looser. Firms dynamically reoptimize, accepting a higher risk of default because the expected value of higher investment outweighs the increased cost of debt.

The final part of the paper highlights policy trade-offs through a series of counterfactual analyses, including an evaluation of whether the guarantee scheme would have been more effective with long-term debt instead. We find that designing the policy with long-term credit guarantees increases the debt dilution problem, and the policy is better off with short loans. We also consider CGS not only available during crisis times, but also a permanently accessible tool as in Colombia, South Korea, or Chile. In the appendix, we further enrich the model by extending the lender side, offering a framework that can accommodate alternative shock processes—such as a sudden withdrawal of lender funds akin to a bank run, a decline in lender wealth, or an increase in lender risk aversion or impatience. This extension enables researchers to explore how the CGS may function not only as an additional liquidity channel but also as a policy tool that mitigates inefficiencies by restoring financial stability in the presence of such disruptions.

Literature Review: We show that credit guarantees influence firm behavior not merely by expanding access to credit, but by altering its price—a channel emphasized in theory but rarely isolated empirically. While limited access to credit is widely recognized as a key constraint on firm growth, most empirical studies focus on quantity-based frictions—that is, whether firms can borrow—rather than on the cost of borrowing conditional on access. For example, Banerjee and Duflo (2014) show that directed lending programs in India improved firm performance by easing access constraints. Similarly, positive effects of credit guarantees on firm outcomes are documented in Lelarge et al. (2010), Brown and Earle (2017), Gonzalez-Uribe and Wang (2022), Bonfim et al. (2023), and Barrot et al. (2024). In practice, however, government-backed loan programs typically affect both credit supply and borrowing costs, through risk-sharing mechanisms and regulated interest rate caps or guarantee fees. As a result, these studies generally do not isolate changes in loan pricing from changes in credit access induced by such interventions. This stands in contrast to theoretical work such as Holmström and Tirole (1997), which emphasizes how agency frictions can distort the price of credit even when financing is available. In their framework, the cost of external funds depends on the strength of borrower incentives and the intermediary's monitoring capacity. Our setting offers rare empirical evidence of such price-based frictions: conditional on receiving a guaranteed loan, firms face exogenous variation in borrowing costs driven by a pre-policy size cutoff that governs the guarantee fee and, in turn, the interest rate.

We build on and extend this literature by showing that conditional on receiving a guaranteed loan, a 25 basis-point lower interest rate—determined by a regulatory threshold—has large effects on investment, employment, revenues, and firm survival. These effects emerge not from increased credit volumes but through debt substitution: treated firms reduce more expensive, non-guaranteed borrowing, lower their average financing costs, and improve their debt service capacity (as measured by EBITDA over short-term debt). This mechanism highlights a novel, price-related financial friction channel—one that has been theoretically recognized but rarely tested empirically. Studies such as Karlan and Zinman 2008 and Dehejia et al. 2012 demonstrate that small interest rate changes affect credit take-up and investment, but our paper is, to our knowledge, the first to causally identify price effects in the context of guaranteed loans under quasi-experimental conditions.

To date, a number of studies examine the effectiveness of CGSs. A key limitation of this literature is that banks retain discretion over which firms receive guaranteed loans. Güler and Samarin (2023) show that participation in the guarantee program is not mechanical: banks actively choose which loans to include or exclude. In our design, this concern is mitigated if not eliminated: we compare firms that are all subject to credit rationing and find no evidence of selection into treatment based on interest rate differentials around the eligibility threshold. Our identification strategy exploits comparisons between groups of firms that are already effectively "selected into treatment." A key feature of our setting is that the 25/50 basis point guarantee fee was passed on to the government rather than retained by banks, removing incentives for differential lending. Moreover, we show that firms near the threshold are comparable across all key observables. As a result, our context offers a particularly clean environment for causal identification.

Our findings also connect to the broader literature on debt burdens and underinvestment. In corporate finance, Myers (1977) shows how existing leverage can depress investment incentives when marginal returns accrue to debtholders. In macroeconomics, Krugman (1988) emphasizes analogous debt-overhang concerns at the sovereign level. We do not test the canonical claim-dilution mechanism directly. Instead, our evidence highlights a complementary channel through which debt burdens can impede real activity in crises: high near-term repayment obligations and elevated risk premia increase the user cost of external finance and intensify rollover pressure. By lowering borrowing costs on newly originated credit and enabling substitution away from costlier loans, the guarantee scheme improves firms' short-run financial resilience and supports investment and employment. More generally, consistent with Farre-Mensa and Ljungqvist (2016), our results underscore the importance of carefully measuring financial constraints and show that *price-based* constraints can be quantitatively important. In this sense, cheaper crisis credit alleviates *debt-service/rollover pressure*—a form of debt overhang related to near-term repayment capacity rather than the classic Myers (1977) claim-dilution mechanism.

Our paper also contributes to the vast literature on alleviating financial distress. During the COVID-19 pandemic, two of the most widely adopted policy tools were credit guarantee schemes (CGSs) and debt moratoria.[3] Of the two, CGSs received the largest allocation of public resources across EU countries and beyond (IMF (2020)). Following their widespread implementation, a growing literature has evaluated their design and effectiveness. For instance, De Blasio et al. (2018) uses an RDD approach to study credit guarantees in Italy, focusing on credit access and fee structures. In Belgium, Güler and Samarin (2023) examines how guarantee fees affect banks' pricing and issuance decisions for non-guaranteed loans. Our setting offers a rare opportunity to identify this price channel in isolation and to assess its aggregate implications through a structural model.

Our paper complements this work by focusing again on a distinct but overlooked channel: the price at which guaranteed credit is offered. We provide causal evidence that interest rate reductions on guaranteed loans—within a population of firms that all have access—affect firm outcomes through a price-related financial friction channel. In doing so, we contribute to a growing body of research that studies how policy interventions

[3] Önder et al. (2023) and Guler et al. (2024) study the effects of firm and consumer moratoria in Colombia, respectively.

can mitigate financial frictions not just by expanding access, but by improving the terms of borrowing (e.g., Brown et al. 2009, Banerjee and Duflo 2014). We show that lower borrowing costs help firms primarily by enabling debt substitution and reducing average financing costs—mechanisms that directly ease price-related constraints on firm behavior.

Finally, our quantitative analysis extends several recent strands of literature on unsecured debt and default by introducing a dynamic investment decision alongside credit-guaranteed and standard loans. On the consumer side, Chatterjee et al. (2007) develop a quantitative framework that replicates key features of unsecured U.S. consumer borrowing under one-period contracts. On the sovereign borrowing side, our study contributes to the quantitative sovereign default framework à la Eaton and Gersovitz (1981), first applied empirically by Aguiar and Gopinath (2006) and Arellano (2008). Related studies such as Mendoza and Yue (2012) provide a framework with defaultable one-period debt but static business cycle dynamics, while Gordon and Guerron-Quintana (2018) incorporate endogenous capital accumulation alongside long-term debt. In our approach, we provide an additional asset class—credit guarantees—which allows us to study the dynamic, endogenous pricing effects of such policies in a dynamic production environment.

In summary, relative to prior work, our paper is the first to (i) identify a quasi-experimental interest rate variation in a credit guarantee scheme, (ii) demonstrate debt substitution and the corresponding capital accumulation as the primary channel through which credit guarantees affect firm behavior, and (iii) link this mechanism quantitatively to borrower-side value implications under one-time, recurrent and alternative policy regimes. Finally, in appendix, we further enrich the model's lending side with a flexible framework that permits exploration of lender-side mechanisms and shock propagation. Taken together, these contributions highlight the importance of price-based financial frictions in shaping firm behavior, a mechanism that has been theoretically emphasized but empirically and quantitatively underexplored.

2 Institutional Details and Research Design

We begin by describing the characteristics of the 2020 CGS in Belgium. Next, we focus on the policy's eligibility criteria for the interest rates charged to firms and how this feature creates the ideal scenario to isolate the causal impact of better price conditions on guaranteed loans.

2.1 The Belgian Credit Guarantee Scheme

The Belgian government announced the implementation of the CGS on April 1st, 2020. The envelope amount for the guarantee scheme was €50 billion, equivalent to 11.8% nominal GDP in 2020 and accounted for approximately 60% of the nominal fiscal measures put forward by the Belgian government to respond to the COVID-19 pandemic[4]. Figure 1 shows that the Belgian CGS was the fifth biggest compared with other CGSs implemented during the pandemic in the EU zone. The program targeted firms affected by liquidity problems linked to the pandemic: eligible Belgian firms must not have arrears on existing loans and tax and social security contributions by February 1st, 2020, and have less than 30 days in arrears by February 29th, 2020.[5]

Under the first scheme, valid from April 1st to December 31st, 2020, Belgian financial institutions received a fraction of the €50 billion envelope based on their market share to issue new loans to any eligible firm. New loans guaranteed by the Belgian government[6] had a maturity up to a year. Banks were required to issue guaranteed loans to any eligible firm but could "deselect" up to 15% of the total loans to eligible firms.[7] In such cases, an eligible firm still received the loan with a maturity longer than one year without a public guarantee.

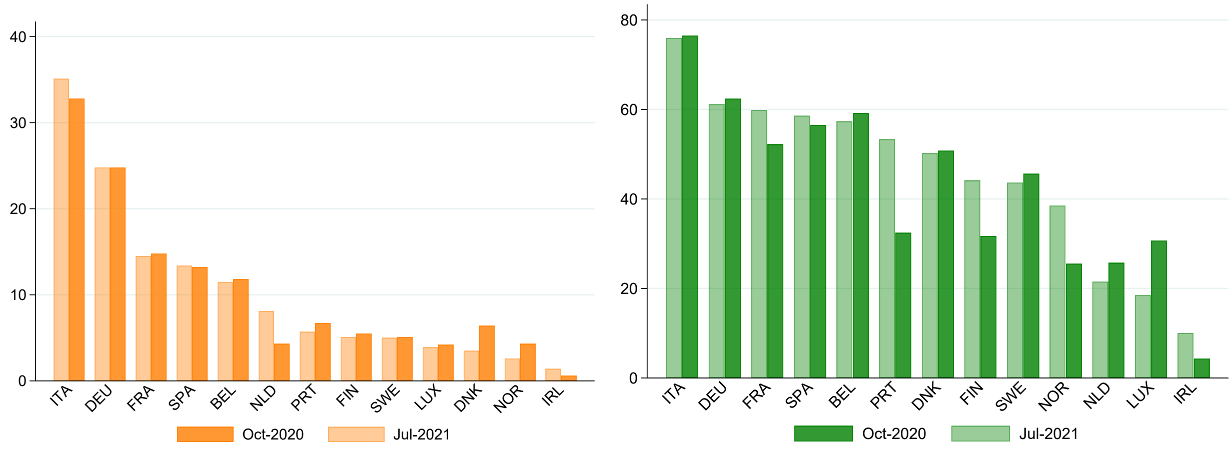
[4] In terms of the budget destined for debt alleviation measures in Belgium during the pandemic the CGS captured more than 90% of the nominal amount

[5] With the COVID-19 pandemic, the European Commission relaxed the restrictions on state aid allowing EU economies to implement CGS under the *Temporary Framework*, which, among other things, defined eligibility requirements based on the definition of "*undertakings in difficulty*" (Anderson et al. 2021)

[6] In the event of default, the Belgian government would cover 50% and 80% of the losses on guaranteed loans after the bank's reference portfolio losses were 3%-5% and more than 5%, respectively.

[7] Banks that "deselected" more than 15% of their total eligible loans had to refund a higher fee for any credit guarantee: (i) 27 bp for SMEs and (ii) 54 bp for large corporations.

Figure 1: Credit Guarantees in the EU: 2020-2021



(a) Percentage of nominal GDP (b) Percentage of Fiscal Measures due COVID-19

Source: Fiscal Monitor Database of Country Fiscal Measures in Response to the COVID-19 Pandemic.

Certain types of credit are explicitly excluded from the guarantee program. These include: (1) leasing agreements, (2) factoring arrangements, (3) consumer and mortgage loans, (4) refinancing of existing credit, (5) renewed borrowing under credit issued before April 1st, 2020, (6) credit restricted by contract for activities outside Belgium, and (7) credit that would generally qualify as guaranteed credit but has been specifically designated as outside the guarantee scheme at the time it is issued. The maximum loan amount was determined by the highest among (i) the firm's liquidity needs[8], (ii) twice the last wage bill reported by the firm, and (iii) 25% of the firm's turnover reported in the previous financial report.Most importantly, the interest rate, which included a fee for the guarantee that lenders were required to remit to the government, was capped differently for certain firms receiving guaranteed loans. See NBB, 2020 for more details on the Belgian CGS.

2.2 Loan Price Conditions on Guaranteed Loans

Now, we describe the circumstances generating a differential interest rate on new loans guaranteed by the Belgian government in 2020. We show that eligibility for better

[8]Liquidity needs were set to 12 months for small and medium enterprises and 18 months for large enterprises.

price conditions on guaranteed loans was linked to a size category defined by three pre-determined dimensions: employment, turnover, and total assets.

Eligible firms receiving guaranteed loans were charged a differential interest rate directly linked to their size category. Specifically, the loan pricing varied due to the differential guarantee fee: (i) 25 bp for Small and Medium Enterprises (SME) and (ii) 50 bp for large enterprises. This meant that the interest rate on guarantee loans was capped at 1.50% for SMEs and 1.75% for large enterprises.

The size category a firm receives in a given year is based on comparing the last two yearly balance sheet reports with three thresholds: 50 full-time employees, a turnover of €9 million, and €4.5 million in total assets. Any firm surpassing no more than one threshold is classified as an SME, while it is categorized as large if it is above two or more thresholds. It is worth noting that employment is the most relevant dimension in determining a firm's size category. Particularly, about 98% of firms during 2018-2019 are classified as SMEs or large corporations due to the employment being above and below the 50-employee threshold.

Overall, this implies that for firms receiving guarantee loans in 2020, the interest rate deterministically increases once pre-determined employment, assets, or turnover surpasses more than one of the cutoffs defining the size category for firms.

Credit guarantees lower the interest rate charged by banks. After adding the guarantee fee, the total cost is cheaper than standard loans. In Belgium, the average interest rate plus fee for new loans with guarantees was on average 11bp lower than deselected new loans. In general, in less subsidized schemes where the government only guarantees a small portion, the total cost can be roughly equal, but still attractive because it unlocks credit that might otherwise be rationed.

2.3 Identification

Next, we argue how the discontinuity in eligibility to receive a different guaranteed loan pricing can be exploited as an exogenous source of variation to estimate the effect of lowering the interest rate on credit guarantees. Then, formulate the empirical strategy characterizing our RD setup.

As explained previously, the differential fee imposed by the Belgian government to provide guarantees on new loans under the CGSs in 2020 generates a unique variation in borrowing costs: the interest rate on guaranteed loans reduces deterministically by 25 bp. for firms classified as SMEs relative to large corporations. Importantly, the fee was paid to the government, not banks. This is important to ensure no endogenous incentives for differential lending.

The first step in defining our empirical strategy is to single out firms receiving credit guarantees in 2020. This step is crucial as the discontinuity in the interest rate is only relevant for firms that obtain guaranteed loans. To identify firms participating in the CGSs, we employ administrative balance-sheet data on statements for amounts payable for 2020; in that year, firms were required to report in detail the outstanding amount on all items in their guaranteed debt portfolio. Using this information, we define firms participating in the Belgian CGSs if they report having a positive outstanding balance on total debts guaranteed by Belgian public authorities at the end of 2020.

Our second step is to simplify the multidimensional size classification underlying the credit guarantee scheme in order to implement a transparent regression discontinuity design. As described in Section 2.2, the guarantee-fee category (and the corresponding interest-rate schedule on guaranteed loans) is based on a "2-of-3" size rule using employment, total assets, and turnover, with firms classified as large if at least two thresholds are exceeded.

In the data, employment alone provides a tight proxy for this classification among firms with guaranteed borrowing in 2020. Specifically, within the sample of guaranteed borrowers, the vast majority of firms lie far from the region where turnover/assets could overturn the employment-based classification: nearly 98% have employment levels that align with their program size category in the administrative records. We therefore restrict attention to firms that, as of end-2018, are clearly below or clearly above the 50-employee threshold (SMEs with fewer than 50 employees and large firms with more than 50 employees), and we implement the RD using the employment cutoff as the running variable.

This restriction preserves the relevant variation in the regulated borrowing-cost wedge on guaranteed loans while limiting classification ambiguity near the cutoff. Appendix G further shows that our findings are robust in a "pivotal" subsample where employment is the marginal determinant under the full 2-of-3 rule (i.e., cases in which crossing 50 employees mechanically changes whether the firm satisfies the 2-of-3 criterion).

We can employ a *sharp* RDD setup based on the sample selection restrictions described before. For any firm "i" receiving a publicly guaranteed loan in 2020, let $FTE_i = 50 - fte_i$, be our running variable defined as the difference between the employment threshold and the number of employees (fte_i) for that firm at the end of 2018. Moreover, let D_i be the treatment indicator for receiving a lower interest rate (i.e., "treatment") on a guaranteed loan in 2020, with $D_i = 1$ a firm is treated and zero if the firm belongs to the control group. Given the regulatory conditions of the Belgian CGSs, we know that D_i is entirely defined by the running variable FTE_i. Therefore:

$$D_i = \mathbf{1}\left\{FTE_i \geq 0\right\} \tag{1}$$

To obtain our *sharp*-RD estimates, we employ a local non-parametric linear regression approach (Calonico et al., 2014). In particular, for an outcome variable $Y_{i,t}$ of firm "i" at the end of year "t" we estimate:

$$\arg\min_{\boldsymbol{\beta}_{Y_t}} \sum_{i}^{I} \Big(Y_{i,t} - \beta_{0,Y} - \beta_{1,Y_t} D_i + \beta_{2,Y_t} FTE_i - \beta_{3,Y_t} FTE_{ij} \times D_i\Big)^2 K\left(\frac{FTE_i}{h}\right) \tag{2}$$

In the non-parametric approach described in equation (2), we first estimate the optimal employment bandwidth "h" to determine the firm variation arbitrarily close to the cutoff we employ. Then, restricting our sample to firms within the optimal bandwidth, we estimate $\boldsymbol{\beta}_{Y_t}$ by minimizing the quadratic sum of residuals weighted by our triangular kernel $K(\cdot)$ giving more importance to firms closer to the cutoff.

The baseline specification in our RD design controls for industry fixed effects[9] to absorb any industry unobservable confounders affecting some industries differently than others

[9]We use two-digit industry codes (NACE-BEL 2008-Rev. 2) in most regressions. For outcomes with smaller sample sizes, we use one-digit industry codes (NACE Rev. 2 main sections) to preserve statistical power.

(e.g., 2020 COVID pandemics). Additionally, our specification also controls for a dummy taking the value of one if firms report having guaranteed loans with private banks in 2020. For economic performance outcomes we additionally control for the difference between firms' total assets in 2018 and the asset threshold (€4.5 million).[10]

The coefficient of interest capturing the sharp-RD estimator is β_{1,Y_t}. Notice that this coefficient is computed on a year-by-year basis using cross-section variation of firms: (i) one year before (i.e., $t = T - 1$), (ii) during the year (i.e., $t = T$), and (ii) up to three years after (i.e., $t = T + 1, T + 2, T + 3$) the policy was implemented. Equation (3) defines the RD estimator for the contemporaneous effect of lowering the interest rate on outcome Y.

$$\beta_{1,Y_T} = \lim_{x \downarrow 0} \mathbb{E}[Y_{i,T} | FTE_i = x] - \lim_{x \uparrow 0} \mathbb{E}[Y_{i,T} | FTE_i = x] \tag{3}$$

Intuitively, this expression captures the mean difference in Y across firms receiving credit guarantees at the end of 2020 but differently treated in terms of the interest rate: some firms treated with a lower interest rate due to being marginally below the employment threshold in 2018, and firms charged a higher interest rate because had slightly more than 50 employees in 2018.

Our RD design identifies the local treatment effect of receiving a lower interest rate for firms exactly at the employment cutoff (i.e., continuity condition). In our case, this condition required that firms within an arbitrarily small bandwidth of the employment threshold are similar in all observable and unobservable characteristics, then any difference in Y_i during the year of the policy should be explained by the fact that some received a guaranteed loan with relatively lower interest rate.

However, as discussed in Section 2.1, banks retain discretion over whether loans are granted under the credit guarantee scheme or deselected. This feature of the policy has been shown to generate strategic behavior on the bank side. In particular, Güler and Samarin (2023) show that banks' use of the guarantee scheme is not mechanical: banks actively decide which loans to include or exclude from the program, and this discretion

[10] Including this control improves the precision and statistical significance of the RD estimates for these outcomes, though our main results remain qualitatively unchanged when it is excluded.

is related to borrower characteristics and loan terms, with some firms receiving non-guaranteed credit under alternative conditions.

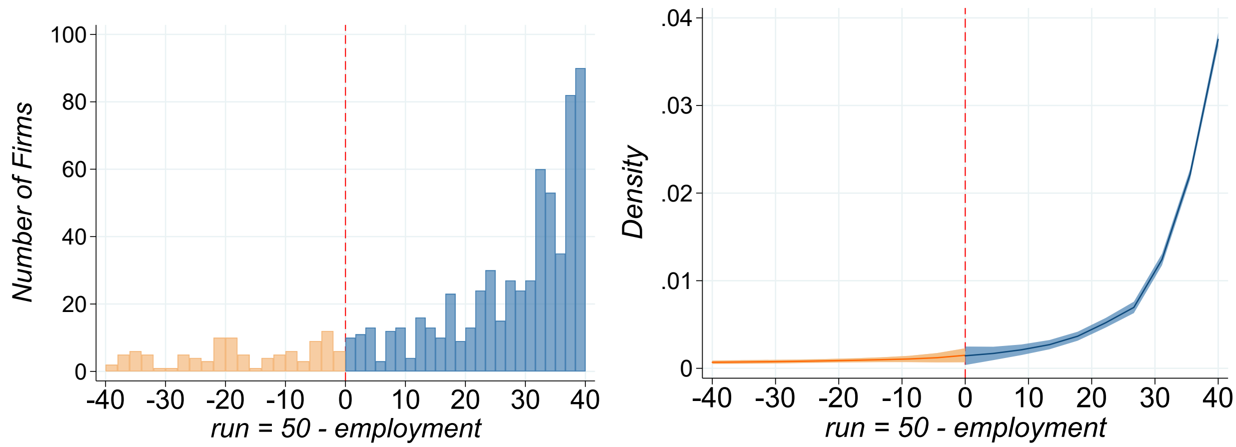
Our identification strategy relies on differences in the regulated interest rate *conditional on guaranteed borrowing*, which are mechanically determined by firms' pre-policy size classification. To assess whether banks sort firms into guaranteed borrowing discontinuously at the SME threshold, we estimate a sharp regression discontinuity design using the *universe of Belgium firms*, with the probability of holding publicly guaranteed debt in 2020 as the outcome variable. As shown in Figure B2 and Table B1 (Appendix B), we find no statistically or economically significant discontinuity at the 50-employee cutoff: within a narrow bandwidth around the threshold, the probability of receiving a guaranteed loan differs by only 0.07 percentage points relative to a mean take-up rate of 5.6%. This evidence indicates that selection into guaranteed borrowing is smooth around the cutoff, supporting the interpretation that our RD estimates isolate price variation among guaranteed borrowers rather than bank-driven selection.

3 Data

We use firm-level balance sheet information from *Bel-first*. This data source provides comprehensive annual information on balance sheet items for the universe of companies in Belgium from 2015 to 2024. We employ the unconsolidated balance-sheet reports. In particular, we focus on the statements of amounts payable for 2020 to be able to identify firms receiving credit guarantees in that year. Additionally, we gathered information from the assets, income, and social balance statements from 2018 to 2023 to measure real and financial outcome variables.

The total number of firms reporting positive publicly guaranteed debt in 2020 in the *Bel-first* data is 3,461, implying an economy-wide take-up rate of approximately 1.17% relative to the universe of firms reporting balance-sheet information in that year. Firms in our sample capture 99.7% of all Belgian companies reporting positive balances of guaranteed debt in 2020, based on the official National Bank of Belgium statistics.[11] For our RD setup, our sample includes 2,853 firms, with 2,504 firms treated (i.e., less than or equal to 50 employees) and 349 in the control group (i.e., more than 50 employees).[12]

Figure 2: Treatment Distribution Along the Employment Cutoff



(a) Treatment Distribution

(b) McCrary's Test

Panel (a) shows the histogram of firms with a guaranteed loan in 2020 along the running variable. The running variable represents 2018 employment re-centered around zero using the cutoff of 50 employees. All firms to the right (blue) of the cutoff that reported less than 50 employees in 2018 are treated with a lower interest rate, while firms to the left (orange) of the cutoff with more than 50 employees in 2018 get a higher interest rate. Panel (b) shows the point estimates (line) and confidence intervals (shaded) for the density to evaluate the bunching of observations around the employment cutoff (McCrary, 2008). The p-value (0.87) does not reject the null, indicating a lack of manipulation of the running variable.

Figure 2 plots the distribution of firms in our sample along the running variable. Panel (a) presents a histogram of the frequency of firms receiving a publicly guaranteed loan in 2020 within a small bin of our running variable. The x-axis represents the distance of a firm's employment level in 2018 from the threshold (i.e., 50 employees). Then, conditional on obtaining a guaranteed loan in 2020, any firm to the right (orange colored) of zero receives a lower interest rate, while a firm to the left of zero (blue colored) gets a higher

[11]According to the aggregate annual accounts of enterprises, NPI, and foundations reported by the NBB, 3,468 enterprises reported positive amounts of payable amounts guaranteed by Belgian public authorities in 2020. Although 50 billion euros were allocated, only 26 billion euros were used during the Belgian CGS, and our final sample covers 75% of guaranteed debt allocations (see NBB.stat).

[12]The higher take-up rate observed locally around the 50-employee threshold—approximately 5.6% within the optimal regression discontinuity bandwidth—reflects the fact that firms close to the SME cutoff are, by construction, larger and more likely to rely on bank credit than the average firm in the economy.

interest rate on its credit guarantee. Notice that the number of firms increases as we move along our running variable from -40 to +40. This only reflects the importance of firms with less than 50 employees in Belgium and is consistent with the case of other advanced economies: in 2017, SMEs captured 70% of total employment.

In Appendix C, Table C2, we report the summary statistics for firms in our sample at the end of 2020. The average firm in our sample holds €3.1 million in publicly credit guarantees, which represents 26% of their total liabilities. In terms of assets, the average firm in our sample holds €15.7 million in total assets, out of which 43% of can be used as collateral (i.e., tangible fixed assets) while only 16% are fully liquid (i.e., cash and equivalents). The latter is consistent with 38% of firms in our sample holding privately guaranteed credit.

4 Main Results

In this section, we report the main results. We begin by describing the RD estimates on firm's economic performance. Next, we show evidence of the mechanism explaining how lower borrowing costs from credit guarantees impact firms' performance. Finally, we present the evidence supporting the identification strategy in our RD setup.

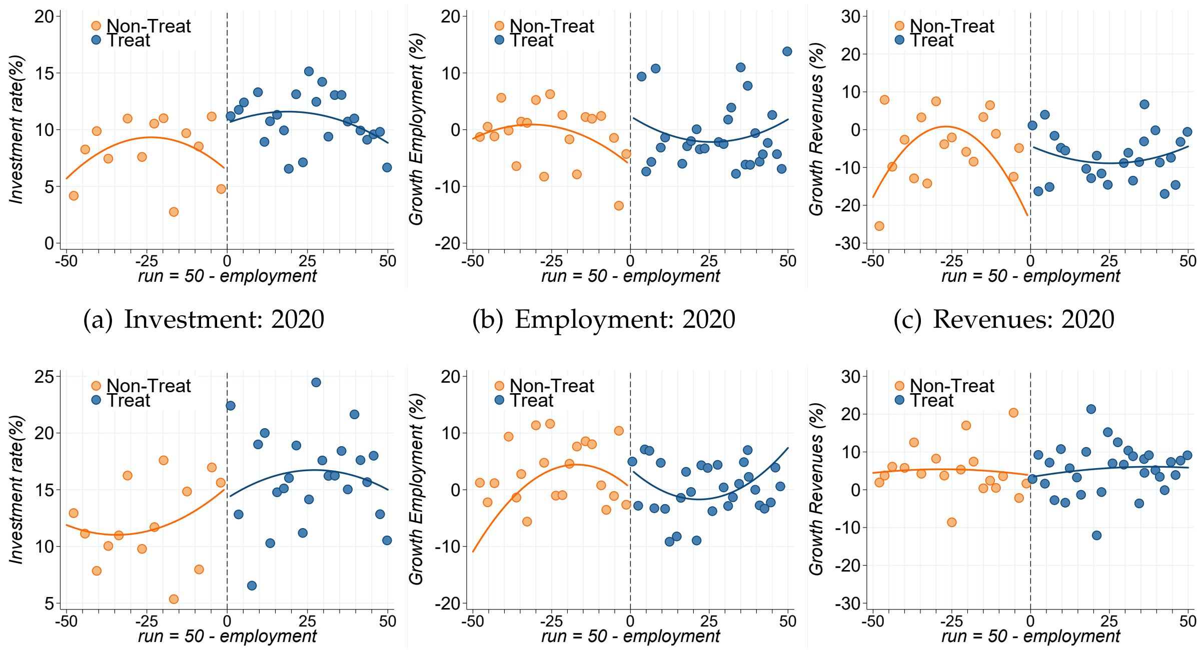
4.1 Firm Real Outcomes

Our variables of interest are investment rate, employment growth, and revenue growth. We define the investment rate as the ratio of tangible fixed assets acquisition relative to the previous year's total fixed assets. We measure employment using the number of full-time equivalent employees entered in the staff register.Finally, we proxy revenues using gross value added, which is consistently reported across firms in our sample.[13] For employment and revenue, we compute growth rates using a symmetric year-over-year definition, which averages the current and previous year values in the denominator. This approach

[13] Data on operating revenues and turnover is largely unavailable because Belgian SMEs are not required to report this information in their annual financial statements.

reduces sensitivity to scale and outliers and handles zero values more consistently than log-difference measures.

Figure 3: Firm-level outcomes



(a) Investment: 2020 (b) Employment: 2020 (c) Revenues: 2020

(d) Investment: 2019 (e) Employment: 2019 (f) Revenues: 2019

The figure examines the impact of the policy on the year before and during the year of the guarantee scheme implementation. We employ balance sheet data for firms receiving a guaranteed loan in 2020. All variables are expressed as percentage changes. Panel (a), (b), and (c) show the investment rate and growth rate of employment and revenue for 2020, while panel (c), (d), and (e) show the same variables for 2019. Each dot represents the mean of the outcome within a bin of the running variable. The solid lines are quadratic fits using dots on each side of the cutoff. The number of bins and specific location are determined using a quantile-spaced mimicking variance approach (see Cattaneo et al., 2019).

Figure 3 visually depicts our main findings for investment, employment, and revenue growth. Each dot represents the average outcome value within a bin of the running variable, and the lines show quadratic fits estimated separately on either side of the employment threshold. Panel (a) shows a visible upward jump in investment in 2020 for treated firms. While the jumps in employment (Panel b) and revenue growth (Panel c) are less visually pronounced, the corresponding local linear point estimates—based on observations close to the threshold—indicate statistically significant effects.[14] In contrast, the second row shows no evidence of discontinuities in any of the three outcomes one year before the Belgian CGS was implemented, reinforcing the credibility of our identification strategy.

Table 1 presents the RD estimates for firm performance outcomes. Panel (A) reports the results for investment, while Panels (B) and (C) cover employment and revenue growth, respectively. In each panel, the first row reports the estimated treatment effect β_{1,Y_t} (as defined in equation (2)) for several years: a pre-policy year (Column 1), the policy year 2020 (Column 2), and up to three years after the program ended (Columns 3–5). To ensure comparability, we use 2019 as the pre-policy year for employment growth, since treatment is assigned based on 2018 employment levels. This avoids mechanically inducing differences in growth rates—specifically, a downward bias for firms just above the 50-employee cutoff. For other outcomes, such as investment and revenue, we use 2018 as the pre-treatment baseline year, which allows us to test whether firms were ex-ante similar based on the same reference year used to define the running variable.

Consistent with our previous visual evidence, we find that firms receiving credit guarantees at a lower interest rate performed better during the year the policy was implemented. Specifically, firms borrowing credit guarantees at a 25 bp. lower interest rate increase investment, employment growth, and revenue growth by 0.20 pp., 0.28 pp., and 0.42 pp., respectively. These effects are not driven by pre-existing differences: in the pre-policy period, point estimates for investment (0.05 pp.), employment growth (0.02 pp.), and revenue growth (0.03 pp.) are small and statistically insignificant.

[14]This difference arises because the figure displays smoothed trends over the full range of the running variable, while the RD estimates rely on a narrower, optimally selected bandwidth around the cutoff where identification is strongest.

Table 1: RD benchmark results: Firm-Level Outcomes

	Pre-Policy	Year: 2020	Post-Policy		
	T-1	T	T+1	T+2	T+3
	(1)	(2)	(3)	(4)	(5)
(A) Investment Rate					
Sharp-RD	0.05	0.20**	0.10	0.22	0.05
	(0.08)	(0.08)	(0.08)	(0.19)	(0.29)
Observations	2,385	2,508	2,482	2,448	1,754
Bandwidth (in # emp)	8.7	10.7	10.2	13.8	9.7
(B) ΔEmployment					
Sharp-RD	0.02	0.28***	-0.21***	0.08***	-0.07
	(0.03)	(0.04)	(0.07)	(0.03)	(0.04)
Observations	1,765	1,764	1,738	1,677	1,372
Bandwidth (in # emp)	10.4	7.3	8.5	5.7	13.6
(C) ΔRevenues					
Sharp-RD	0.03	0.42***	-0.56***	-0.09	-0.02
	(0.08)	(0.08)	(0.12)	(0.08)	(0.03)
Observations	2,526	2,631	2,615	2,576	2,526
Bandwidth (in # emp)	8.9	8.7	6.8	6.4	7.5
(D) ΔTotal Assets					
Sharp-RD	-0.02	0.19***	-0.017	-0.036	-0.001
	(0.04)	(0.07)	(0.02)	(0.07)	(0.03)
Observations	2,345	2,410	2,402	2,357	2,303
Bandwidth (in # emp)	13.4	6.0	5.1	14.7	5.3
(E) ΔEquity					
Sharp-RD	-0.004	-0.011	-0.04***	-0.041*	-0.032**
	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)
Observations	2,345	2,410	2,402	2,357	2,303
Bandwidth (in # emp)	28.2	11.2	9.5	15.0	10.9
(F) ΔLoan Debt					
Sharp-RD	0.030	0.307***	-0.311*	0.124	0.070
	(0.05)	(0.11)	(0.18)	(0.17)	(0.18)
Observations	2,235	2,372	2,339	2,251	2,148
Bandwidth (in # emp)	6.5	11.8	9.1	12.4	14.1

Authors' calculations. The table reports RD estimates of the effect of lower interest rates on credit guarantees on firm-level outcomes using Belgian balance-sheet data (2017–2023). Investment rate is the acquisition of tangible fixed assets relative to total fixed assets in the previous year. **Δ**Employment and **Δ**Revenues are symmetric year-over-year growth rates for number of full-time equivalent employees and gross value added, respectively. **Δ**Total assets and **Δ**Equity are annual changes relative to total assets. **Δ**Loan debt is the annual growth in financial debts payable within and after one year. Columns show estimates for the pre-policy year (T-1), the policy year (T), and up to three years after ($T+1$, $T+2$, $T+3$). All regressions control for two-digit industry fixed effects (NACE-BEL 2008-Rev. 2), an indicator for guaranteed loans from private banks, and the distance to the €4.5 million asset threshold. Estimates correspond to β_{1,Y_t} from equation (2). Robust bias-corrected standard errors in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Following the policy year, the dynamic effects vary across outcomes. For investment, we observe positive but statistically insignificant effects one and two years after the program (+0.10 pp. and +0.22 pp.), with the impact dissipating by year three. Employment and revenue growth both exhibit a similar pattern: a significant decline in the first post-policy year (-0.21 pp. and -0.56 pp., respectively), suggesting possible catch-up dynamics among control firms as the temporary advantage of lower borrowing costs wore off. Employment growth shows a modest rebound in the second year (+0.08 pp.) before returning to baseline levels by year three, while revenue growth returns to baseline already in the second year. These dynamic patterns are consistent with the short maturity structure of the guaranteed loans and suggest that the primary effects of lower interest rates were concentrated in the year of implementation.

The dynamic response of investment, employment, and revenues following the end of the policy aligns with the policy's structure: the benefit of lower interest rates on credit guarantees persists until the guaranteed loans mature 12 months later. First, treated firms experienced a 0.11 pp. and 0.20 pp. higher investment rate one and two years after the policy was implemented, though these estimates are statistically insignificant. Ultimately, the effect on investment dissipates three years after the credit guarantee scheme (CGS) ends. Employment growth is 0.21 pp. lower in the first year and 0.07 pp higher in the second year, which can be interpreted as employment catching up—initially for firms in the control group and later for those in the treatment group. Nonetheless, the difference in employment growth returns to pre-policy levels three years after the policy ends. We observe a similar pattern for revenue growth, except for a short-lived 0.32 pp. increase for treated firms in the year following the policy's conclusion.

Panels (D), (E), and (F) in Table 1 present the dynamic effects on the growth rates of total assets, equity, and loan debt, respectively. First, we find no significant pre-policy differences across treatment and control groups, indicating balanced trends prior to the program. In the year of the policy, treated firms increase their total asset growth by 0.19 pp.—a statistically significant effect that aligns with the contemporaneous rise in investment observed in Panel (A). This effect dissipates after one year, with small and insignificant differences thereafter.

In contrast, equity growth is consistently lower for treated firms throughout the post-policy period, with statistically significant negative effects in years one through three (-0.04 pp., -0.041 pp., and -0.032 pp., respectively). This pattern may reflect increased external financing reliance among treated firms: the interest rate reduction allows firms to expand investment with less need for internal funding. Rather than reflecting deteriorating fundamentals, the decline in equity growth is consistent with firms taking advantage of improved credit conditions to re-leverage.

Panel (F) shows the response of loan debt growth, measured as the combined change in financial debts payable within and after one year. This measure captures debt issued by financial institutions—primarily bank loans—and offers a more targeted proxy for borrowing activity than total liabilities. We find that treated firms increase their loan debt by 0.31 pp. during the policy year, a statistically significant effect, followed by a moderate decline in the first post-policy year (-0.31 pp.). This pattern is suggestive of partial debt substitution, a mechanism we explore in more detail in the next section: although firms reduce more expensive non-guaranteed debt, they do not do so one-for-one, and thus overall loan issuance rises. The subsequent decline in debt growth is consistent with firms reverting to their pre-policy borrowing behavior once the interest rate discount expired. Firms no longer benefit from the lower cost of guaranteed loans, reducing the incentive to take on additional debt. Some may even repay or avoid rolling over maturing guaranteed loans, leading to a net decline in debt growth. While we cannot fully rule out a direct subsidy effect—as borrowing became cheaper—it is important to note that the response is not purely mechanical: the increase in debt issuance alongside higher investment and asset accumulation suggests that financial frictions were binding and eased through the policy. This supports the interpretation of credit guarantees as alleviating constraints rather than merely subsidizing firm activity.

As we show in Appendix F, the contemporaneous effects are qualitatively similar when estimated using a difference-in-differences specification applied to samples restricted to the average RD bandwidth used for each outcome in Table 1, reinforcing the robustness of the main findings. These difference-in-differences estimates are intended as a complementary descriptive exercise rather than as a standalone identification strategy and are inherently underpowered given the narrow bandwidths and limited time-series variation, particularly for lumpy and noisy firm-level outcomes. Consistent with this interpretation, employment, an outcome that adjusts more smoothly and is measured with less noise, exhibits statistically significant responses, while other outcomes display similar directional patterns but are estimated imprecisely.[15] Importantly, our main conclusions rely on the regression-discontinuity design, which does not depend on parallel trends assumptions and yields precise year-by-year estimates of the policy effect.

4.2 Exploring the Mechanism

Table 2 presents RD estimates for the contemporaneous effects of lower interest rates on firms' debt structure, borrowing costs, and default risk. Specifically, we examine how firms adjust their guaranteed debt issuance, non-guaranteed liabilities, average interest costs, debt service capacity, and exit probabilities in response to the interest rate discount. Guaranteed debt accumulation is defined as the percentage change in debt guaranteed by Belgian public authorities. Debt substitution measures the percentage change in non-guaranteed debt (i.e., total liabilities minus publicly guaranteed debt) relative to total liabilities. Average interest is calculated as the ratio of financial charges to the lagged value of total liabilities plus equity. We scale interest expenses by total financing (debt plus equity) rather than by debt alone to obtain a measure of the average cost of external funds that is comparable across firms with different leverage levels. This normalization avoids mechanically inflating the measured interest rate for firms with low outstanding debt and captures changes in borrowing costs that operate through the pricing of new credit rather than through changes in leverage.[16] Debt service capacity is proxied by the annual change in the EBITDA-to-short-term debt ratio. Exit probabilities are captured using two indicators: (i) whether the firm stops reporting financial data after 2023, and (ii) whether it enters liquidation, bankruptcy, or dissolution between 2020 and 2023.[17]

[15] The one exception is asset growth, where the DID estimate is negative—possibly reflecting measurement timing differences or greater sensitivity to sample composition in that outcome.

[16] Our results are qualitatively unchanged when interest expenses are scaled by financial debt only.

[17] These events are registered and reported by the Crossroads Bank for Enterprises (CBE) managed by the Ministry of Economy (FPS).

Table 2: Debt Substitution, Financial Burden, Debt Service, and Default Risk

	Guarant. Debt Accum.	Debt Subst.	Average Interest	ΔDebt Serv. Capacity	Exit Probability	
					Last Availab. Year	Legal Situation
	(1)	(2)	(3)	(4)	(5)	(6)
Sharp-RD	-0.076	-0.262*	-0.015***	0.283***	-0.033*	-0.086**
	(0.11)	(0.14)	(0.00)	(0.08)	(0.02)	(0.04)
Obs.	2,852	1,590	2,596	2,633	2,642	2,642
BW (in # emp)	13.2	13.0	8.7	8.7	7.1	7.3

Authors' calculations. The table reports RD estimates of the contemporaneous effect of lower interest rates on credit guarantees on firm-level debt structure, financing costs, and exit risk using Belgian balance-sheet data (2020). Guaranteed Debt Accumulation is the percentage change in debt guaranteed by Belgian public authorities. Debt Substitution is the percentage change in non-guaranteed debt (i.e., total liabilities minus publicly guaranteed debt) relative to total liabilities. Average interest costs are computed as the ratio of financial charges to lagged total liabilities plus equity. ΔDebt Service Capacity is the annual change in the EBITDA-to-short-term debt ratio. Exit probabilities are defined as indicator variables for firms (i) with no balance sheet data after 2023 (Last Available Year), or (ii) undergoing liquidation, bankruptcy, dissolution, or absorption between 2020–2023 (Legal Situation). All regressions control for industry fixed effects and an indicator for guaranteed loans from private banks. We use two-digit industry codes (NACE-BEL 2008-Rev. 2) for average interest and debt service capacity, and one-digit industry codes (NACE Rev. 2 main sections) for the other outcomes. Estimates correspond to β_{1,Y_t} from equation (2) for the policy year (2020). Robust bias-corrected standard errors in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels.

Column (1) shows that firms receiving credit guarantees at a lower interest rate accumulate 0.076 pp. less in publicly guaranteed debt than the control group—an estimate that is small and not statistically significant, effectively indistinguishable from zero. Instead, column (2) provides stronger evidence of a substitution effect: treated firms reduce their non-guaranteed debt by 0.262 pp. more than those paying higher interest rates—a statistically significant effect. Importantly, this pattern does not reflect refinancing of existing loans, which is prohibited by the program's design. Rather, it captures balance-sheet substitution at the margin: firms obtain new guaranteed loans at lower interest rates and respond by reducing reliance on other, more expensive forms of non-guaranteed borrowing. This partial substitution implies that firms are replacing relatively expensive non-guaranteed debt with cheaper guaranteed credit. The implied elasticity shows that for each 1% decrease in borrowing costs for guaranteed debt, non-guaranteed debt reduces by about 0.023%.[18]

[18] We compute the elasticity as $\varepsilon_S = (\Delta S/\bar{S})/(\Delta r/\bar{r})$, where $\Delta S = -0.00262$ is the RDD estimate for debt substitution (i.e., change in non-guaranteed debt to total liabilities), $\bar{S} = 0.73$ is the ratio of average non-guaranteed debt to the average total liabilities in 2020, $\Delta r = -0.0025$ is the change in guaranteed borrowing costs (25 bp.), and $\bar{r} = 0.016$ is the average interest costs in 2020 (i.e., proxy for average interest rate). This yields $\varepsilon_S \approx 0.023$.

Column (3) shows that average interest costs decline by 0.015 pp for treated firms, consistent with substitution away from more expensive non-guaranteed borrowing. Because this measure averages over the entire outstanding debt stock, the implied change in average borrowing costs is mechanically small. The relevant margin for investment, however, is the *marginal* cost of newly originated credit, which the program shifts discretely via the guarantee-fee wedge and by reallocating borrowing toward cheaper funding sources.

Finally, columns (5) and (6) show that lower interest rates reduce exit probabilities. Treated firms are 3.3 pp. less likely to stop reporting data after 2022, and 8.6 pp. less likely to experience a legal exit event—both statistically significant reductions. These findings further support the idea that access to cheaper credit improves firm viability and reduces default risk.

Table D3 explores the dynamics of debt substitution, average interest costs, and debt service capacity in the years before, during, and after the implementation of the CGS. Column (1) confirms that there were no significant differences across treatment and control groups before the policy was implemented, supporting the validity of the design. Column (2) reproduces the coefficients for these variables reported previously. Column (3) shows that the effects on financial outcomes persist one year after the policy: non-guaranteed debt remains lower (-0.193 pp.), average interest costs stay suppressed (-0.015 pp.), and debt service capacity remains elevated (+0.186 pp.). While the real effects on investment and employment observed in Table 1 are concentrated in the policy year, the persistence in financial adjustments likely reflects gradual restructuring of liabilities and delayed balance sheet responses, even as firms' real activity normalizes.

By the second year after the policy (Column 4), substitution begins to reverse—non-guaranteed debt increases by 0.25 pp., suggesting that firms in the treatment group may have returned to non-guaranteed borrowing as their guaranteed loans matured. Meanwhile, average interest costs and debt service capacity return close to baseline. By year three (Column 5), we observe no statistically significant differences across any of the three outcomes, indicating that the effects of the interest rate reduction were temporary and largely concentrated in the year of policy implementation and its immediate aftermath.

4.3 Supporting Evidence on Identification

To close our empirical analysis, we explore the appropriateness of our research design in isolating the effect of lower interest rates on credit guarantees. Specifically, we provide evidence supporting our identification strategy regarding the continuity assumption in our RD setup. This is also important to address potential selection concerns: the policy requires banks to participate in the CGS and to lend to firms that meet specified criteria, but it also provides leeway by allowing banks to choose which firms to lend to.

The most important element in our RDD identification strategy is that firms with lower (treatment) and higher (control) interest rates on their credit guarantees in 2020 are almost identical except for receiving treatment. We begin by testing how suitable this assumption is for our RD setup. In particular, we present evidence of potential jumps in the distribution of firms and other firm-level pre-determined observable characteristics along our running variable.

A first concern is that the announcement of the Belgian government about the conditions imposed on the interest rate for credit guarantees could induce firms to "manipulate" their employment levels to reduce the cost of credit guarantees (i.e., around the cutoff). Figure 2, Panel (b) evaluates manipulation or self-selection by checking for evidence on bunching of observations around the employment cutoff. A simple inspection of this figure shows no discernible jump in the estimated densities (continuous lines) when we move to the right side of the employment cutoff. More formally, we follow McCrary, 2008 and evaluate the null of continuity of the treatment distribution around the cutoff: the resulting p-value of 0.87 eliminates any concern of firms misreporting their employment levels in 2018.

In Panel A of Figure 4, we evaluate arbitrary cutoff points different from the one that triggers a discount in the interest rate. Finding significant effects on placebo cutoffs could indicate systematic differences among firms on each side of the cutoff or a concurrent policy, potentially contaminating our results. We evaluate placebo cutoffs for up to ± 30 employees lower and higher than the actual cutoff $FTE_i = 0$. None of our baseline results on firm performance and debt portfolio are statistically significant on the placebo cutoffs.

Additionally, Panel B of Figure 4 presents the "donut-hole" test, where we further check for evidence of manipulation that the McCrary test might have potentially missed. We estimate the contemporaneous coefficient (β_{1,Y_T}) for all the firm-level outcomes in our analysis, but we exclude observations in the immediate neighborhood to test for "bunching" of observations around the employment cutoff. Most of our results are similar when excluding firms with 1, 2, and 3 employees above or below the cutoff.

On the other hand, if our continuity assumption holds, there should not be any observable difference in pre-determined characteristics when moving from the left side to the right side of the employment threshold. We have already shown that this was the case for the main outcomes of interest. Next, we expand this analysis to other firm-level observable characteristics.

Table E4 (Appendix E) presents our formal results using firm-level variables related to assets, debt, labor costs, and profitability during 2018-2019. The second column provides the sharp-RD point estimates, and the third and fourth columns report the p-values and 95% confidence intervals. Our results provide evidence of equally balanced distributions across the running variable before the CGS was enacted: firms on either side of the cutoff are not statistically different in terms of pre-determined levels of assets (i.e., total, fixed, tangible fixed, and cash), leverage, short-term and long-term debt share, wage bill, earnings, and profits.

Finally, we conduct an additional robustness exercise that directly addresses the multidimensional nature of the size classification underlying the credit guarantee scheme. Under the program rules, firm size is determined by a "2-of-3" criterion based on employment, turnover, and total assets. While our baseline regression discontinuity design exploits variation around the 50-employee threshold, employment need not be pivotal for all firms under the multidimensional rule.

To ensure that our results are not driven by cases in which employment is irrelevant for size classification, we restrict the sample to firms for which employment is the marginal determinant of size status. Specifically, we retain firms that exceed exactly one of the other two thresholds (turnover or assets). Under the 2-of-3 rule, these are the firms for which crossing the 50-employee cutoff mechanically changes whether the firm satisfies the size criterion and, consequently, the interest rate applied to guaranteed loans. In this pivotal subsample, size classification coincides with the employment threshold by construction.

We then re-estimate the sharp RD in this pivotal subsample. For transparency, we use the same bandwidths as in the baseline specifications so that any differences reflect the sample restriction rather than bandwidth choice; Appendix G shows that the results are also robust to re-selecting bandwidths within the restricted sample. Tables G6 and G7 in Appendix G show that the main findings remain qualitatively similar in this restricted sample, confirming that our results are not driven by the multidimensional nature of the size definition.

Comparing the contemporaneous RD estimates for real outcomes in the pivotal (marginal) subsample (Table G6) with the baseline results for the policy year reported in column (2) of Table 1 shows a high degree of consistency across specifications. The estimated effects on the investment rate, employment growth, revenue growth, and loan-debt growth remain positive, statistically significant, and similar in magnitude.

Two balance-sheet outcomes differ in the pivotal subsample: total asset growth changes sign and equity growth becomes significantly negative. We interpret these patterns cautiously. They suggest that, for firms for which employment is pivotal under the 2-of-3 rule, the primary adjustment to cheaper credit operates through financing composition rather than broad balance-sheet expansion. In particular, the negative equity response is consistent with reduced reliance on equity financing (or lower retained earnings) when the marginal cost of debt falls.[19]

A similar comparison for financial outcomes yields the same conclusion. As shown in Table G7, the pivotal-subsample estimates closely mirror the baseline results in Table 2: we detect no discontinuity in guaranteed debt accumulation, but a strong and statistically significant reduction in non-guaranteed borrowing. This substitution away from costlier market loans is accompanied by improvements in credit conditions and firm survival: interest-cost measures fall and exit probabilities decline, with effects that are comparable—and in some cases larger—in the pivotal subsample. Overall, these results confirm that the

[19]Balance-sheet items may also be more sensitive to accounting conventions, timing, and composition effects; accordingly, we emphasize financing flows and debt composition outcomes as our core mechanism evidence.

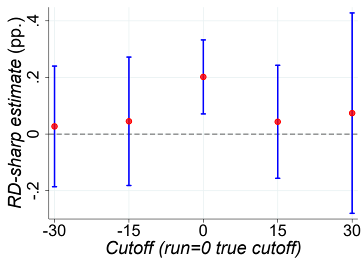
price-based mechanism operates similarly in the baseline and pivotal samples, and that our main findings are not driven by the multidimensional nature of the size definition.

These robustness checks reinforce the credibility of our empirical strategy: we find no evidence of manipulation around the cutoff, no discontinuities in pre-determined firm characteristics, no signs of confounding policy shocks or sorting, and no changes when restricting the sample to firms for which employment is the marginal determinant of size classification. Together, they support the interpretation that our RDD isolates a clean price-based treatment effect.

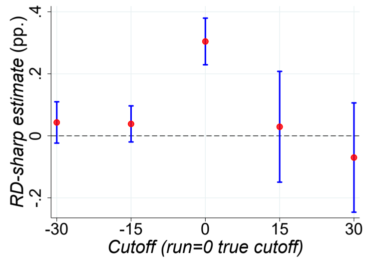
Taken together, these results show that modest reductions in borrowing costs significantly improve firm outcomes, primarily through debt substitution and reduced financing costs. Importantly, these effects persist precisely among firms for which the policy-induced price variation is mechanically tied to the employment threshold, reinforcing the interpretation that our findings are driven by exogenous changes in borrowing costs rather than classification artifacts. To assess whether these empirical patterns can be rationalized in a structural model—and to evaluate the external validity, long-run effects, extensive margin, and firm's welfare implications of the policy—we now turn to a quantitative model. We calibrate the model to replicate the quasi-experimental interest rate variation and confirm that the model closely matches the elasticities of investment and debt substitution observed in the data.

Figure 4: Additional Robustness Tests

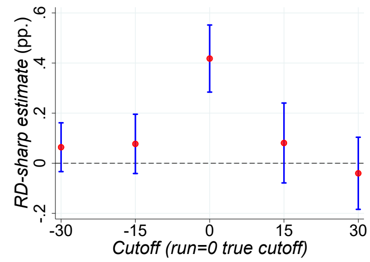
Panel A: Alternative Cutoffs



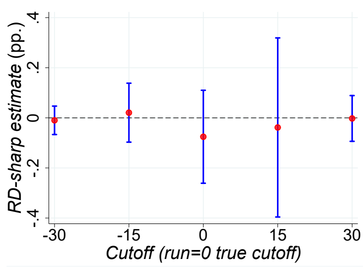
(a) Investment



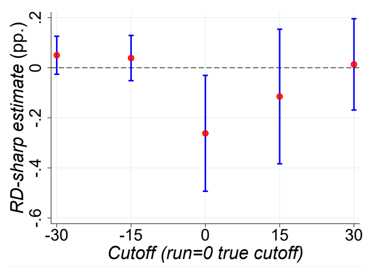
(b) Employment



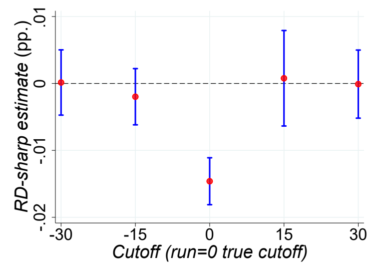
(c) Revenues



(d) Guarant. Debt Accum.

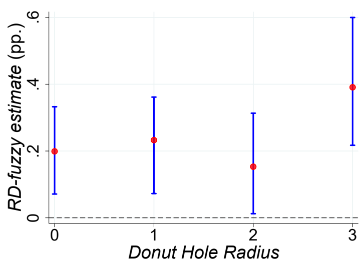


(e) Debt Substitution

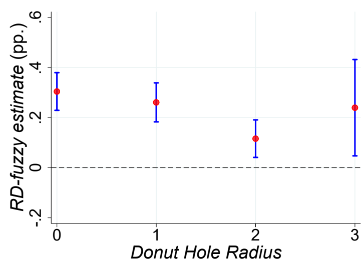


(f) Average Interest

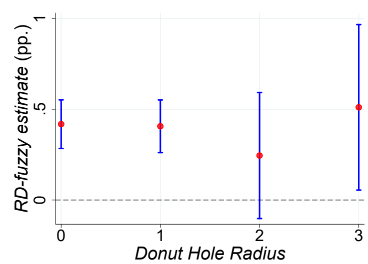
Panel B: Donut-Hole Sensitivity Test



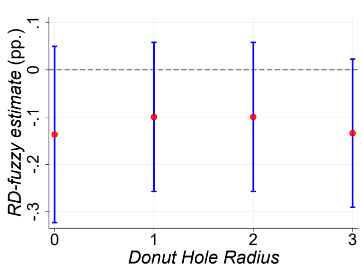
(g) Investment



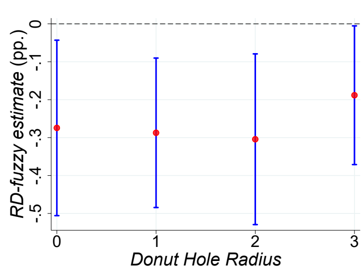
(h) Employment



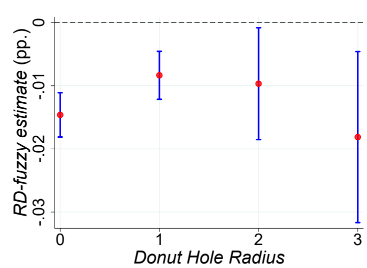
(i) Revenues



(j) Guarant. Debt Accum.



(k) Debt Substitution



(l) Average Interest

Figure shows the test for alternative placebo cutoffs (Panel A) and Donut-hole sensitivity test (Panel B) on the contemporaneous estimated effect of lower interest rates on credit guarantees for firm's investment rate (Figures (a) and (g)), employment growth (Figures (b) and (h)), revenues growth (Figures (c) and (i)), guaranteed debt accumulation (Figures (d) and (j)), debt substitution (Figures (e) and (k)), and average interest costs (Figures (f) and (l)). Panel A shows RD estimates under alternative placebo cutoffs. Panel B shows the RD estimates, excluding firms with 1, 2, and 3 equivalent full-time employees above/below the cutoff.

5 A Quantitative Model of Credit Guarantees

Why a quantitative model? Our empirical analysis demonstrates that firms benefit from policy relief in the short run. However, as noted in the introduction, while RDDs have become increasingly influential for establishing causal effects, they are inherently limited to identifying local average treatment effects. Although this approach ensures high internal validity, it offers limited external validity and does not illuminate the long-term, extensive margin, or firm's welfare implications of the policy. To address these limitations, we develop a quantitative model that aligns with our empirical findings and enables us to conduct a range of counterfactual policy experiments.

Modeling strategy. We extend a standard quantitative default framework by introducing a production economy and a second loan instrument backed by government guarantees. Three design features are central to our approach:

1. **Two-type firms.** The main empirical variation arises between two narrowly defined groups—firms just below and just above the 50-employee threshold—within a tight local bandwidth. To reflect this, we model two representative firms that differ only in the interest rate applied to guaranteed loans (25 basis points). While the RDD uses a sample of many firms around the cutoff, the identifying variation is highly localized. Abstracting from additional heterogeneity allows us to closely match the empirical design and isolate the core mechanism driving observed differences in firm behavior. This modeling choice facilitates a direct mapping from reduced-form elasticities to structural mechanisms without introducing distributional assumptions over firm types. We use a two-type abstraction to match the RD local comparison.[20]

2. **Dynamic production economy** We allow capital to be an endogenous choice variable. This feature, in contrast to models with static investment, is essential for capturing firm responses to credit guarantees, highlighting dynamic misallocation under antic-

[20]Introducing heterogeneity would require solving for the full distribution of firms, which makes it infeasible to retain aggregate shocks, ex-ante policy analysis and equilibrium default. Adding simplified heterogeneity is left to future work.

ipated versus unanticipated policies, and quantifying firm's welfare and long-run effects.

3. **Portfolio choice and investment.** Firms endogenously allocate borrowing between (i) conventional long-term loans and (ii) government-guaranteed loans while simultaneously choosing investment levels. Endogenous loan pricing plays a central role: when firms anticipate that credit guarantees will recur, they optimally reallocate their debt portfolios in expectation of future support, and invest more aggressively.

4. **Lender block.** We begin by modeling lenders as risk-neutral to preserve tractability. In Appendix J, we enrich this block by explicitly incorporating lender preferences, building on Epstein and Zin (1989), with overlapping generations model. This more general framework allows credit terms to endogenously respond to aggregate conditions, thereby enabling a wider range of policy experiments. For instance, access to guaranteed loans is limited to periods characterized by firm-level liquidity shocks, which we model as temporary surges in operating expenses—a key friction in the environment. Under this richer structure, one can also study credit supply shocks, such as declines in lenders' wealth that restrict external financing, or shifts in lenders' risk aversion and patience. For brevity, we relegate the details of the lender block to appendix.

5.1 The model

This section introduces a dynamic model where a representative borrower/firm can access standard long-term loans and credit guarantees and undertakes an investment decision. The model assumes that the borrower cannot commit to future decisions regarding default, borrowing and investment. Our model echoes the logic of Holmström and Tirole (1997) by featuring endogenous loan pricing shaped by borrower incentives and default risk. Firms face higher borrowing costs when leverage increases, not because of credit rationing, but because lenders rationally anticipate greater repayment risk—thereby embedding agency frictions directly into the pricing kernel.

Each period follows a structured sequence of events. First, the firm observes its productivity shocks and global conditions. Once these shocks are realized, the borrower decides on default, investment and borrowing, subject to constraints shaped by its default choice. The firm's operating income comes from capital k and inelastic labor supply l to produce output y using the Cobb–Douglas function $y = Ak^{\alpha^k} l^{1-\alpha^k}$. Since we compare two representative firms that have employment levels of 50 and 51 to mimic the empirical design, we take labor supply as unity so that the production function becomes $y = Ak^{\alpha^k}$. Productivity process follows an AR(1) process

$$\log(A_t) = (1-\rho)\,\mu + \rho \log(A_{t-1}) + \varepsilon_t, \tag{4}$$

with $|\rho| < 1$, and $\varepsilon_t \sim N\left(0, \sigma_\varepsilon^2\right)$.

Following Hatchondo et al. (2025), a borrower's expenditures denoted as g_t may take a low or a high value: $g_t \in \{g_L, g_H\}$, and it evolves according to a Markov process. During normal times, g_t equals g_L, while g_t becomes g_H during a liquidity shock, with $g_H > g_L$. Specifically, a liquidity shock g initiates with probability $\pi_{LH}(A) \in [0,1]$ and concludes with probability $\pi_{HL} \in [0,1]$. To account for the correlation between negative conditions in international capital markets and low domestic aggregate income (Calvo et al., 2004; Calvo et al., 2006), we model π_{LH} as a decreasing function of A, expressed as $\pi_{LH}(A) = \min\left\{\pi_0 exp^{-\pi_1 \log(A) - 0.5\pi_1^2\sigma_\varepsilon^2}, 1\right\}$. In calibration that we pin π_0, π_1 to match the observed frequency/duration of liquidity events.

In the case of anticipated shocks, access to credit guarantees is granted automatically during a liquidity shock and expires when the shock is over.

Borrower objective. We interpret the representative borrower as an owner-managed firm (entrepreneur) that values net payouts. Let c_t denote net payouts (dividends/owner consumption) after investment and debt service. Preferences are

$$\mathbb{E}_t \sum_{j=t}^{\infty} \beta^{j-t} u(c_j),$$

with $u(\cdot)$ increasing and concave. This formulation provides a parsimonious way to capture precautionary motives and risk sensitivity on the borrower side while keeping the model disciplined by the estimated elasticities.

Asset spaces: Two asset classes exist in the model. The first one is the standard long-term loans. As Hatchondo and Martinez (2009), we assume that a standard loan issued in period t promises an infinite stream of coupons that decrease at a constant rate δ. In particular, a loan issued in period t promises to pay $\delta(1-\delta)^{j-1}$ units of the tradable good in period $t+j$, for all $j \geq 1$. Hence, standard loan dynamics can be represented as follows:

$$L_{t+1} = (1-\delta)L_t + i_{L,t},$$

where δL_t are the payments due in period t, and $i_{L,t}$ is the new number of standard loans an individual lender provides in period t.

Loans under credit guarantees, L_C, are one-period as in the implemented CGS program. The access to these loans is triggered when the borrower faces a liquidity shock $g = g_H$, and when the shock is over, the access expires.

If the borrower has not defaulted and access to credit guarantees is triggered, then the budget constraint reads:

$$c = y - i - \frac{\Theta}{2}(k'-k)^2 - g_H - \kappa L - L_C + q(L', L'_C, k', A, g)\left[L' - L(1-\delta)\right] + \frac{L'_C}{1+r_C},$$

where q denotes the price of long-term standard loans, and r_C denotes the interest rate on credit guarantees, $i = k' - (1-\delta_k)k$ is investment, and $\frac{\Theta}{2}(k'-k)^2$ is the adjustment cost of capital. Note that the firm expenditures g become g_H as the borrower can only access these credit guarantees when liquidity shock hits. Next, $\kappa = \frac{r+\delta}{1+r}$ denote coupon payments of standard loans.

If a liquidity shock does not hit the borrower, access to the credit guarantees is not triggered, then the consumption is given by

$$c = y - i - \frac{\Theta}{2}(k'-k)^2 - g_L - \kappa L - L_C + q(L', L'_C, k', A, g)\left[L' - L(1-\delta)\right].$$

5.2 Recursive Formulation

Let $s \equiv (A, g)$ denote the vector of exogenous states. Let V denote the value function of a representative agent that is not currently in default. The function V satisfies the following functional equation:

$$V(L, L_C, k, s) = \max \left\{ V^R(L, L_C, k, s), V^D(L, L_C, k, s) \right\}, \tag{5}$$

where the value of repaying is given by

$$V^R(L, L_C, k, s) = \max_{L' \geq 0, L'_C \geq 0, k' \geq 0, c \geq 0} \left\{ u\left(c\right) + \beta \mathbb{E}_{(A'|A, g'|(A', g)} V(L', L'_C, k', s') \right\}, \tag{6}$$

subject to

$$c = Ak^{\alpha^k} - i - \frac{\Theta}{2}(k' - k)^2 - g - \kappa L - L_C + \mathcal{I}(g)\frac{L'_C}{1 + r_C} + q(L', L'_C, k', s) \left[L' - L(1 - \delta) \right],$$
$$k'(L, L_C, k, s) = i - (1 - \delta_k)k,$$
$$q(L', L'_C, k', s) \geq \underline{q} \; \forall \; L' > L(1 - \delta),$$

where $\mathcal{I}(g)$ is an indicator function. $\mathcal{I}(g)$ becomes unity when a liquidity shock hits the representative agent and thus is allowed to borrow under the CGS and equals zero otherwise. Namely,

$$\mathcal{I}(g) = \begin{cases} 1 & \text{if } g = g_H, \\ 0 & \text{otherwise.} \end{cases}$$

The value of defaulting is given by:

$$V^D(L, L_C, k, s) = \max_{k' \geq 0, c \geq 0} u\left(c\right) + \beta \mathbb{E}_{s'|s} \left[(1 - \psi) V^D(L, L_C, k', s') \psi V(\alpha L, \alpha_C L_C, k', s') \right], \tag{7}$$

subject to

$$c = (1 - \phi(y))Ak^{\alpha^k} - i - \frac{\Theta}{2}(k' - k)^2 - g,$$
$$k' = i - (1 - \delta_k)k.$$

We assume lenders are risk-neutral and relax this assumption in Appendix J by providing overlapping generations model of risk-averse lenders. The price of standard loans is given by

$$q'(L', L'_C, k', s) = \frac{1}{1+r}\mathbb{E}_{s'|s}\Bigg[\left(1 - d'\left(L', L'_C, k', s'\right)\right)\left[\kappa + (1-\delta)q(L'', L''_C, k'', s')\right] + d'\left(L', L'_C, k', s'\right) q_d(L', L'_C, k', s')\Bigg], \tag{8}$$

and the defaulted loan can be traded to the collection agency at a price

$$q_D(L', L'_C, k', s) = \frac{1}{1+r}\mathbb{E}_{s'|s}\Bigg[\alpha\psi\ \Big((1-d')\left[\kappa + (1-\delta)q\left(\hat{L}\left(\alpha L, \alpha_C L'_C, k', s'\right), \hat{L_C}\left(\alpha L', \alpha_C L_C, k', s'\right), k'', s'\right)\right. + d' q_d(\alpha L', \alpha_C L'_C, k', s')\Big) + (1-\psi) q_d\left(L', L'_C, k', s'\right)\Bigg], \tag{9}$$

where r is the risk-free rate denoting the opportunity cost of lending, $d' = \hat{d}\left(\alpha L', \alpha_C L'_C, k', s'\right)$ denotes the next-period equilibrium default decision, $L'' = \hat{L}\left(L', L'_C, k', s'\right)$ denotes the next-period equilibrium standard loan decision, $L''_C = \hat{L}_C\left(L', L'_C, k', s'\right)$ denotes the next-period equilibrium guaranteed loan decision, and $k'' = \hat{k}\left(L', L'_C, k', s'\right)$ denotes the next-period equilibrium capital decision.

5.3 Recursive Equilibrium

A *Markov Perfect Equilibrium* is characterized by

1. rules for default $\hat{d}$, standard loan borrowing $\hat{L}$, credit guarantee borrowing $\hat{L}_C$, and capital formation $\hat{k}$
2. and loan price function q for standard loans, respectively,

such that:

i. given the loan price function q, the policy functions $\hat{d}$, $\hat{L}$, $\hat{L}_C$, and $\hat{k}$ solve the Bellman equations (5), (6), and (7).

ii. given policy rules $\left\{\hat{d}, \hat{L}, \hat{L}_C, \hat{k}\right\}$, the loan price function q and q_D satisfy conditions (8, 9).

5.4 Calibration for the economy without credit guarantees

We resort to the administrative Belgian data for most parameters and estimate it ourselves. For the remaining parameters, we use conventional estimates reported in the literature.

Table 3: Parameter values

	Parameter	Value	Target
Risk aversion borrower	γ	2	Standard RBC value
Risk-free rate	r	4%	Standard RBC value
Discount factor	β	0.92	
Probability of reentry after default	ψ	1/3	Data
Recovery rate	α	0.50	Data
Recovery rate	α_C	0.50	
Standard loan duration	δ	0.2	Average duration 4 years
Credit guarantee duration	δ_C	1	Average duration 1 year
Operating expenditure	$g = g_L$	0.20	Data
Credit guarantee cap	$\overline{b_C}$	0.10	Data
Capital share of income	α^k	0.33	Data
		Calibrated	
Income autocorrelation coefficient	ρ_ϵ	0.792	Estimated
Standard deviation of innovations	σ_ϵ	2.3%	Estimated
Mean log endowment	μ	$(-1/2)\sigma_\epsilon^2$	Normalization
Income cost of defaulting	d_0	-1.21	Spread and debt-to-income ratio
Income cost of defaulting	d_1	1.321	Spread and debt-to-income ratio
Probability of entering liq. shock	π_0	0.23	3 high-financing needs episodes every twenty years
Probability of entering liq. shock	π_1	33	4% lower average income
Liq. oper. income shock	g_H	0.35	15% annual operating income loss

We first calibrate the benchmark model without credit guarantees ($L_C = 0$) to reflect key characteristics of the Belgian economy.

The utility function assumes a constant coefficient of relative risk aversion, given by:

$$u(c) = \frac{c^{1-\gamma} - 1}{1-\gamma}, \text{ with } \gamma \neq 1.$$

Borrowers have the standard RBC risk aversion parameter, $\gamma = 2$. The income cost of defaulting is defined as $\phi(y) = d_0 y + d_1 y^2$. This quadratic income cost structure allows us to match the average levels of standard loans and the interest spread observed in the data.

Table 3 presents the benchmark values assigned to all model parameters. Each period in the model represents one year. The risk-free interest rate is set to 4 percent, and the discount factor β is set to 0.92, both standard values in quantitative studies on defaults.

The spread data that firms pay became available after 2010, and the balance sheet information of a firm became available in our data after 2015. Thus, to estimate the firm's income process, we use Belgian GDP data. The parameters governing the endowment process are chosen to replicate the behavior of logged and quadratically detrended GDP in Belgium during this period.

In our data, operating expenditure, g_L, is 20 percent of average income. Thus, we set it to 0.20. Next, we set $\delta = 0.2$, which, alongside the spread, yields an average debt duration of 4.1 years in the simulations—consistent with the average duration of standard loans in Belgium.[21]

We model the liquidity shock as one that increases the firm's gross financing needs, such as in the case of a natural disaster. This type of shock can primarily be domestic or global driven, similar to the framework used in Hatchondo et al. (2025).

Formally, we assume that borrower expenditures g shock follows a Markov process with probabilities set as $\pi_0 = 0.38$, $\pi_1 = 0.38$, and $\pi_{HL} = 0.8$. The low expenditure level, $g_L = 0.20$, is consistent with the baseline calibration, while the high expenditure level, $g_H = 0.35$, represents a 15 percentage point increase in operating expenses relative to average operating income.

To calibrate the model, we adjust two parameters for the cost of default, two parameters for the likelihood of entering a high-financing-need period, and the risk premium (d_0, d_1, π_0, π_1, and π_{HL}). These are calibrated to match five key moments: an average spread of 1.8 percent, a standard loan-to-operating-income ratio of 33.7 percent, three high-financing needs episodes per 20 years, a 4 percent trend-income reduction during these episodes (Calvo et al., 2006) with a 1.0 percentage point increase in spreads during a liquidity shock. Again, the targets for debt and spread levels are based on data from Belgium.

[21]For the average debt duration and apply the Macaulay definition of duration, which, given the coupon structure in this paper, is given by $D = \frac{1+r^*}{\delta+r^*}$, where r^* denotes the constant per-period yield delivered by the loan.

In our model, consistent with CGS implementation, the duration of credit guarantees is set to be 1 year, $\delta_C = 1$.

We cannot observe the percentage of defaulted commercial debt recovered in our administrative data because it requires data from a collection agency. Instead, we used S&P Global Ratings (2023) to find the percentage of a defaulted loan that can be recovered and set the recovery rate α to be 0.5. We also do not have data on how much defaulted guaranteed debt is recovered from borrowers. We assume that $\alpha_C = \alpha$ as a starting point.[22] Details of computation are relegated to Appendix H.

6 Quantitative Results

This section presents our results. We first calibrate our baseline model to Belgium's aggregate moments and then introduce the CGS, both as a one-time policy as well as a recurrent policy. Using our model, we match the short-run empirical estimates obtained in Section 4 to demonstrate that our one-time intervention model closely aligns with our estimates. Having established the consistency of our model with its empirical counterpart, we then use it to study alternative policy amendments.

Table 4 compares the data moments from administrative data with the one obtained from the model. The model features plausible moments and matches both the debt statistics and the business cycle moments reasonably well. Briefly, the model generates a loan-to-operating-income ratio of 33.7 percent, corresponding to the mean loan-to-income ratio of all Belgian firms in our administrative data. The model can also match the mean credit spreads. It is not immediately possible to compute the bankruptcy rate in our data, we reported the non-performing loans (NPL) ratio, which is around 4.9 percent in the data. Even though not targeted, the model does a good job of matching the NPL ratio.

[22] Different values of α_C do not have qualitative effects on our results.

Table 4: Main Results: Long-run moments in different model economies

	(1) Data	(2) Baseline (No cred guarantee)	(3) Credit guar. One-time	(4) Credit guar. Recurrent
Mean standard loan to opr. income (%)	33.7	33.1	33.1	36.0
Mean credit guarantees to opr. (%)	*n.a.*	*n.a.*	one-time	0.3
Mean investment to opr. (%)	14	16	16	16
Excess investment volatility (σ_i/σ_y)	0.4	0.4	0.4	0.4
Mean spread (r_s) (%)	1.8	1.8	1.8	2.3
Spread volatility ($\sigma(r_s)$)	0.6	0.9	0.9	1.3
$\rho(r_s, y)$	-0.5	-0.8	-0.8	-0.8
Default rate	5.0	1.5	1.5	6.3
Average duration of debt (in yrs)	4.1	4.1	4.1	4.1
Spread rise during liq. shocks	1.0	1.3	1.3	3.3

The first column reports data moments, the second column reports the results of the baseline model without credit guarantees, and the third column presents the results with credit guarantees with 10% cap and one-time unanticipated intervention, while the last column shows the results with credit guarantees with recurrent intervention. The standard deviation of a variable is denoted by σ, and the coefficient correlation between variables is denoted by ρ. Consumption and income are reported by natural logs.

6.1 Validation: Empirical vs. Model-Implied Elasticities

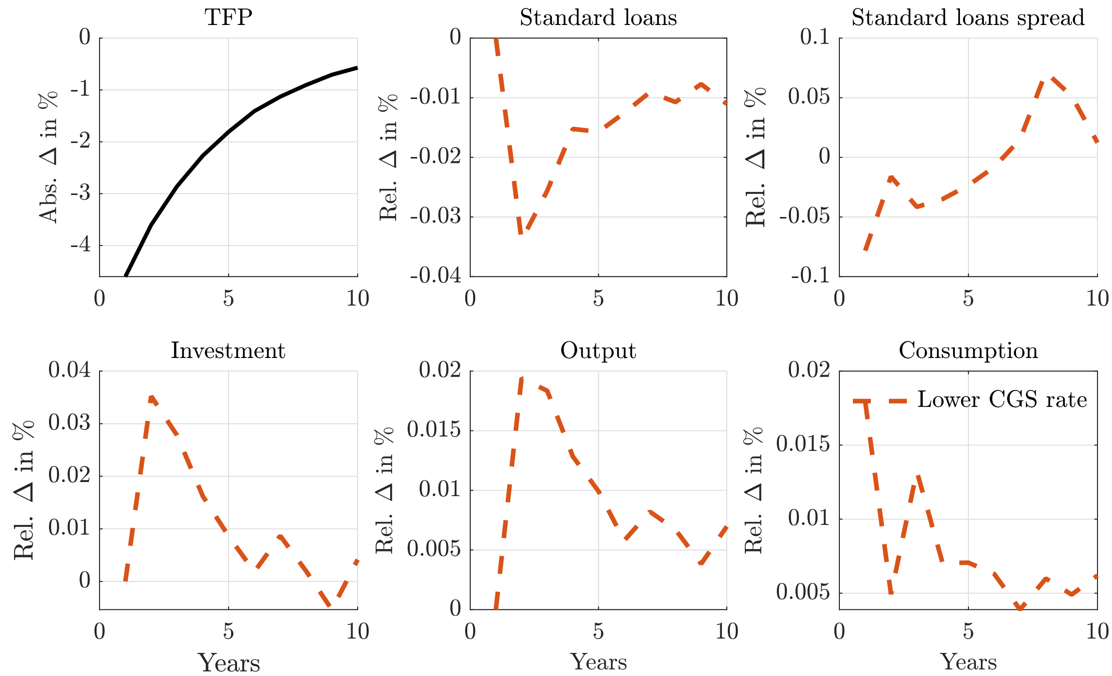
To closely align our quantitative model with the empirical findings outlined in Section 4, we compare two identical firms that differ only in the interest rate applied to their guaranteed loans: one faces a rate that is 25 basis points higher. Recall that our RDD strategy in the empirical section compares firms within the vicinity of the 50-employee threshold, confirming that these firms are otherwise identical in all observable characteristics.[23]

By examining how the model predicts outcomes during the COVID-19 crisis, we seek to establish a stronger link between the model's assumptions and the real-world estimates we have derived. Results from this analysis are provided in Figure 5. This represents an economy in which the policy is designed as a one-off intervention that agents did not anticipate. The upper-left chart shows the economy subject to a two-standard-deviation productivity shock at time zero, which then evolves according to equation (4), with an initial state corresponding to a liquidity shock. The plots depict the relative deviations of key variables from their simulated counterparts in an economy that pays 25 basis points lower interest on CGS loans.

Our model predictions align with our empirical estimates. In particular, Figure 5 shows that the economy receiving a lower CGS rate deleverages its standard loans more by using

[23] Assessing whether our quantitative model aligns with empirical estimates is a complex endeavor, and we must exercise caution when drawing comparisons. There are inherent challenges in bridging micro-estimates with a macro model (see Önder et al. (2024)).

Figure 5: Impulse response functions of introducing credit guarantees for firms that pay 25 bp less



Effects of introducing credit guarantees along with key variables of interest aligning to our empirical findings: standard loans, output, and investment changes

the proceeds of the guaranteed loans. This leads to a lower interest rate on new standard loans, due to a reduced probability of default. In terms of real effects, the that is subject to lower rate undertakes more investment which also leads to higher production and consumption. This also highlights that firms mitigate their exposure to the rollover risk by reducing indebtedness, which we will further examine in the upcoming sections.

6.1.1 Computation of Elasticity Measures

Table 5: Empirical and Model-Implied Elasticities

Outcome Variable	**Empirical Elasticity**	**Model-Implied Elasticity**
Investment ($\partial I/\partial r$)	-0.038	-0.022
Non-guaranteed debt ($\partial b/\partial r$)	0.023	0.036

To facilitate comparison between our empirical estimates and the predictions of the quantitative model, we compute an elasticity of investment with respect to borrowing costs in both settings. This elasticity provides a scale-free measure of responsiveness that allows us to assess the model's ability to replicate the observed empirical sensitivity.

Elasticity: estimand and interpretation. We report an *intensive-margin* response of investment to the *marginal cost of new borrowing* affected by the CGS. Formally, we summarize the RDD effect as an arc elasticity:

$$\varepsilon_I = \frac{\frac{\Delta I}{\bar{I}}}{\frac{\Delta r}{\bar{r}}}, \tag{10}$$

where ΔI is the RDD-induced change in the investment rate, $\bar{I}$ is the baseline investment rate, Δr is the RDD-induced change in the (effective) borrowing cost on new loans, and $\bar{r}$ is the baseline borrowing cost for that same margin. This object measures the percent change in investment relative to the percent change in the borrowing cost *for firms around the eligibility threshold*. It is not an extensive-margin effect on take-up, nor an elasticity to an economy-wide shift in market rates.

Because the intervention is a fixed change in borrowing costs (25 bps), the arc elasticity in (10) depends mechanically on the baseline level $\bar{r}$. To facilitate interpretation and comparability across settings with different baseline interest-rate levels, we also report the corresponding semi-elasticity of investment with respect to the borrowing cost in levels:

$$\tilde{\varepsilon}_I = \frac{\Delta I / \bar{I}}{\Delta r}, \tag{11}$$

where Δr is expressed in decimal units (so that a 100 bps change corresponds to $\Delta r = 0.01$).[24]

Empirical elasticity calculation. From the RDD, we estimate that a reduction in the borrowing cost by 25 basis points increases the investment rate by 0.20 percentage points.

[24]Equivalently, one can report $\frac{\Delta I}{\Delta r}$ in percentage points of investment per 100 bps, which does not depend on $\bar{r}$.

Specifically, $\Delta I = +0.0020$, $\bar{I} = 0.333$, $\Delta r = -0.0025$, and $\bar{r} = 0.016$. We compute:

$$\frac{\Delta I}{\bar{I}} = \frac{0.0020}{0.333} = 0.0060 \quad (0.60\%), \qquad \frac{\Delta r}{\bar{r}} = \frac{-0.0025}{0.016} = -0.1563 \quad (-15.6\%).$$

Thus,

$$\varepsilon_I = \frac{0.0060}{-0.1563} = -0.038.$$

In absolute value, this implies that a 1% *relative* decrease in borrowing costs (e.g., from r to $0.99r$) increases the investment rate by about 0.038% relative to baseline.

For interpretability in levels, the semi-elasticity is:

$$\tilde{\varepsilon}_I = \frac{0.0060}{-0.0025} = -2.40,$$

so that a 100 bps decline in borrowing costs ($\Delta r = -0.01$) increases investment by approximately $2.40 \times 0.01 = 0.024$, i.e., *2.4% relative to baseline*. Equivalently, $\Delta I = 0.0020$ for 25 bps implies an increase of 0.008 (0.8 percentage points) in the investment rate per 100 bps.

Model elasticity calculation. In the quantitative model, we conduct a comparable experiment by simulating two economies that differ only in the borrowing cost applied to guaranteed loans. The baseline borrowing cost is $\bar{r}_{\text{model}} = 0.063$ and we impose the same reduction $\Delta r_{\text{model}} = -0.0025$. The model implies that cumulative investment increases by 0.10% relative to baseline, i.e., $\Delta I / \bar{I} = 0.0010$. We compute:

$$\frac{\Delta r_{\text{model}}}{\bar{r}_{\text{model}}} = \frac{-0.0025}{0.063} = -0.0397 \quad (-4.0\%), \qquad \varepsilon_I^{\text{model}} = \frac{0.0010}{-0.0397} = -0.025.$$

The corresponding semi-elasticity is

$$\tilde{\varepsilon}_I^{\text{model}} = \frac{0.0010}{-0.0025} = -0.40,$$

so that a 100 bps decline in borrowing costs raises investment by about 0.40% relative to baseline in the model.

Interpretation. These results show that the elasticity implied by the model is of similar order of magnitude to the empirical elasticity (–0.022 vs. –0.038), suggesting that the quantitative framework captures the responsiveness of investment to borrowing costs reasonably well. Remaining differences may reflect simplifications in the model environment or heterogeneity in firm responses not captured by the representative agent formulation. We do the same computation for non-guaranteed debt and results are summarized in Table 5.

Having established that the model provides a suitable laboratory for studying credit guarantee schemes, we proceed in the following sections to examine the model dynamics under an ex-ante recurrent policy design, analyze the extensive margin of firm behavior, and evaluate the trade-offs associated with a range of policy counterfactuals.

6.2 Recurrent vs. One-Time Responses

We evaluate two policy interventions. Our baseline intervention, aligned with the empirical section, is an unanticipated, one-time policy that applies uniformly to all agents in the economy and is not expected to recur. However, one can also consider a design where the CGS is automatically triggered in response to liquidity shocks, allowing agents to form expectations accordingly.

Figure 6 plots the policy functions for these two interventions. The left panel shows the equilibrium borrowing policies for standard loans in economies with and without credit guarantees during a liquidity shock, with credit guarantees fixed at zero ($L_C = 0$). Standard loan holdings are set at the ergodic level of the baseline economy ($L = 33.1\%$) for both scenarios. The dashed red line represents the baseline economy, while the solid blue lines depict the recurrent (anticipated) CGS regime. The right panel plots the equilibrium price functions as a function of income, holding debt constant at the ergodic level ($L = 37.1\%$), and the bottom panel illustrates the capital formation policy function.

A key insight is that when the policy is implemented as a recurring, anticipated intervention, overall borrower indebtedness increases relative to the baseline. This arises because firms, when making financing decisions, expect the CGS to be activated whenever liquidity shocks occur. Anticipation of future support leads to higher total borrowing (combining standard loans and credit guarantees). Consequently, the problem of debt dilution intensifies: lenders rationally foresee future increases in borrowing and offer lower loan prices today for a given debt level. Even if the immediate default risk is low, the long-term nature of debt contracts means lenders account for the cumulative risk, pricing accordingly within the Markov equilibrium.

The CGS economy also exhibits important precautionary motives for using guarantees. Although credit guarantees are always cheaper, borrowers primarily rely on them during periods of financial stress when obtaining new standard loans becomes more costly. Notably, firms hold higher capital stocks in the recurrent policy regime. We provide further intuition for these dynamics in Figure 7, which plots the impulse response functions of the model economies.

6.2.1 Impulse Response Functions

The dynamic response of investment and borrowing across debt types under different policy regimes is a central result of the model. Figure 7 shows that capital and investment increase more in the recurrent economy, while consumption also rises in the long run. The figure also illustrates the extensive margin of policy interventions, in contrast to Figure 5, which focuses on the intensive margin by comparing firms that both have access to credit guarantees. In the impulse responses, the dashed yellow lines depict the unanticipated, one-time intervention, the solid blue lines show the recurrent policy that begins unexpectedly but is subsequently anticipated, and the dashed-dotted red line presents the benchmark case without intervention.

We feed the same firm productivity shocks as in Figure 5: an initial liquidity shock combined with a two-standard-deviation productivity shock that follows an AR(1) process according to equation (4). The remaining panels report endogenous outcomes. In the short run, the model generates dynamics consistent with the empirical mechanism: absent the CGS, the marginal cost of standard borrowing rises sharply, whereas the guarantee lowers borrowing costs on newly originated credit and allows firms to substitute away from costlier standard loans, improving their liquidity/rollover position (debt-service capacity). In the long run, however, standard-loan balances remain higher in the recurrent-and-anticipated economy, and both spreads and default rates stay persistently elevated. This reflects an ex-ante incentive effect: when subsidized credit is expected to recur in downturns, firms borrow more and self-insure less, raising leverage and default risk in bad states. By contrast, under a one-time unanticipated intervention, firms delever after the shock and spreads and default probabilities eventually fall slightly below the benchmark.

An important question—one that would be obscured in a model without dynamic investment decisions—is why investment and capital accumulation nonetheless increase, as clearly illustrated in Figure 6, even if the impulse responses understate the magnitude due to scaling. At first glance, this pattern may appear counterintuitive: if spreads and defaults rise, why do firms invest more? In a model with static investment, the opposite would occur, with higher borrowing costs dampening capital formation. However, in this setting, cheaper credit in downturns effectively lowers the user cost of capital over the planning horizon. Firms anticipate that when liquidity deteriorates, they will continue to have access to subsidized credit. This expectation reduces the need for precautionary liquidity buffers and encourages more aggressive investment. Even if spreads on standard loans rise somewhat, the effective borrowing constraint becomes looser. As a result, firms dynamically reoptimize, accepting a higher probability of default because the expected gains from higher investment outweigh the incremental borrowing costs.

Initial consumption increases under both the one-time and recurrent interventions, reflecting the cheaper availability of credit guarantees. Perhaps surprisingly, consumption also rises in the long run under recurrent policies. This contrasts with a standard endowment economy without investment, where higher borrowing costs would eventually depress consumption. Here, the expansion of investment raises output sufficiently that, despite higher interest payments on standard loans, firms maintain a higher long-run consumption path.

6.2.2 Borrower value implications

We summarize the borrower-side valuation effects of alternative CGS regimes using the model's value function $V(L, L_C, k, s)$. Importantly, this object measures the value of the borrowing firm (or its owner) under the equilibrium pricing of standard loans and the availability rules for credit guarantees; it is not a measure of aggregate welfare.

Across regimes, the one-time (unexpected) CGS increases borrower value around the intervention by lowering the effective cost of funds in stressed states and facilitating debt substitution toward cheaper guaranteed borrowing. In contrast, recurrent (anticipated) CGSs alter ex-ante incentives: the expectation of subsidized credit in downturns weakens deleveraging incentives and encourages more aggressive borrowing and investment choices. This raises long-run leverage and default risk, which ultimately reduces borrower value despite higher capital accumulation and output.[25]

6.3 Counterfactual analyses

In this section, we perform two counterfactual analyses. First, we investigate how the results would change if the credit guarantee loan were designed as a long-term loan with a one-time intervention. In this case, we set the duration of credit guarantees to four years, the duration of an average standard loan. In the second analysis, we examine the implications of implementing the CGS as an intervention that is always available, such as in the case of Colombia, South Korea, or Chile. Our benchmark case is the economy with liquidity shocks, represented by the second column of Table 4.

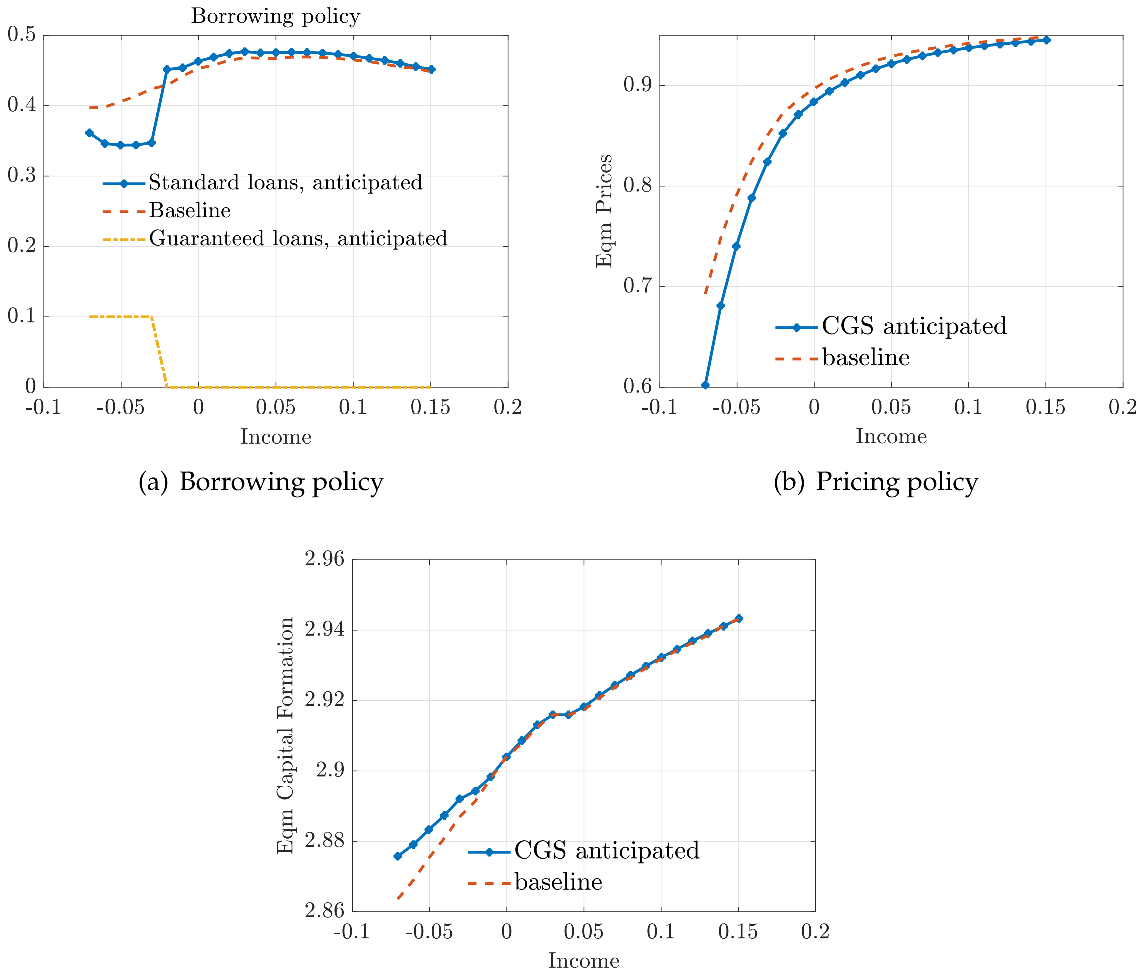
Figure 8 presents the IRFs from these analyses. The following observations stand out. Interestingly, when the CGS is designed as a long-term credit guarantee, the policy exacerbates the debt dilution problem at the time of its introduction. Even though it is a one-time intervention, its long-term nature prevents the borrower from deleveraging as much as in the one-period case (see Figure 7). As a result, the mitigation of the rollover problem is muted. This is also visible in the spreads chart. Notice that the deleveraging in

[25] **Payout-equivalent changes in borrower value.** For comparability across states, we report payout-equivalent changes in borrower value. Formally, η is defined as the constant proportional change in the baseline payout stream that makes the borrower indifferent between the baseline and CGS economies:

$$\mathbb{E}_t \sum_{\tau=t}^{\infty} \beta^{\tau-t} u\Big((1+\eta)\, c_\tau^{\text{baseline}} \mid \cdot\Big) = \mathbb{E}_t \sum_{\tau=t}^{\infty} \beta^{\tau-t} u\Big(c_\tau^{\text{CGS}} \mid \cdot\Big).$$

We emphasize that this is a borrower-side valuation metric rather than social welfare.

Figure 6: Borrowing into standard loans and guaranteed loans



(a) Borrowing policy

(b) Pricing policy

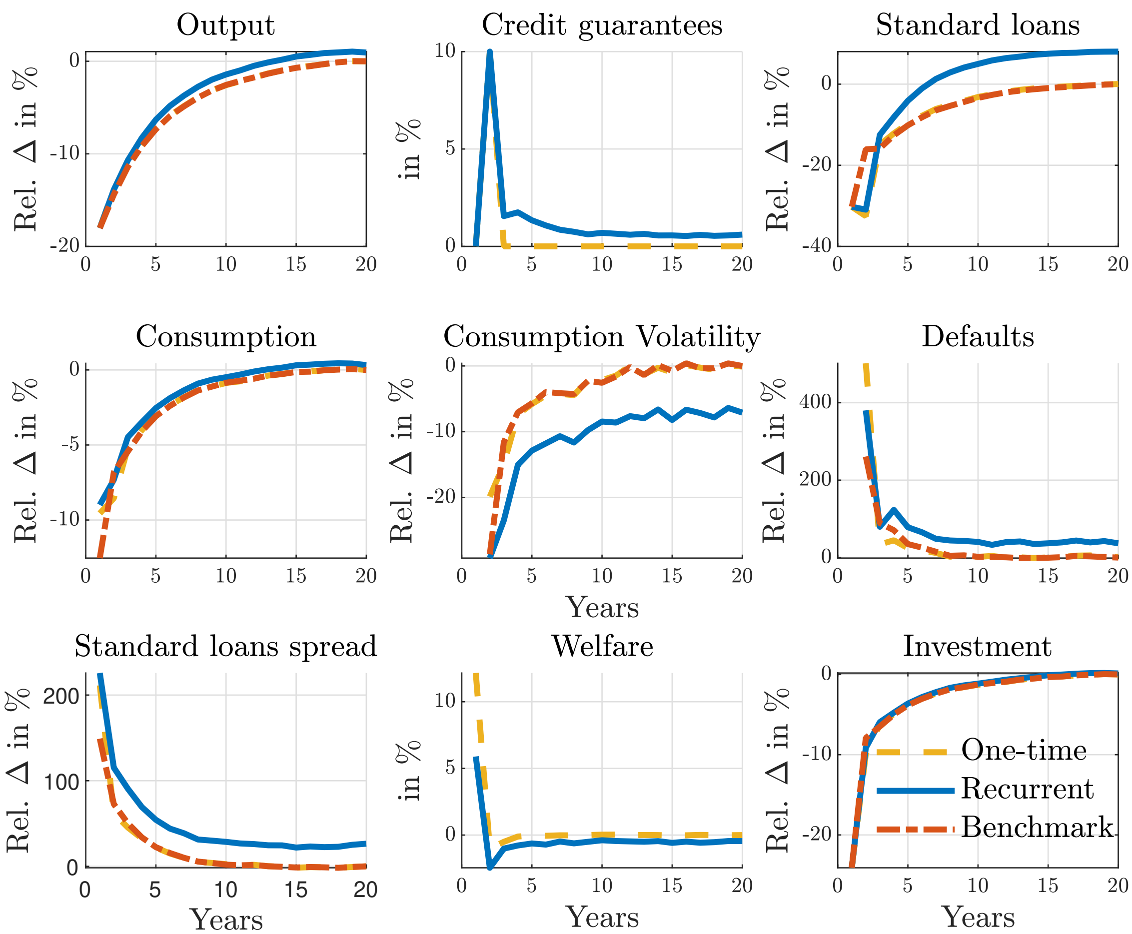
(c) Capital formation policy

Panel (a) illustrates the equilibrium borrowing policy for economies with credit guarantees compared to those without guarantees (baseline). The initial states correspond to the ergodic debt level observed in the baseline economy, 33% of standard loans with zero initial credit guarantees, where firms face a liquidity shock that allows them access to guaranteed loans, panel (b) depicts the corresponding prices in both economies, and panel (c) displays the equilibrium capital choices in both economies.

standard loans immediately after the intervention is insufficient to bring down spreads in standard loans, which remain elevated for a few periods.

Interesting observations stand out in the counterfactual where CGS is permanently available. Even though the CGS is always available, the borrower conserves its usage. Notice that when credit guarantees are unexpectedly introduced, their usage initially hits the available limit, but the borrower does not maintain usage at that limit. Even though credit guarantees provide a cheaper source of funding, the borrower saves them for worse

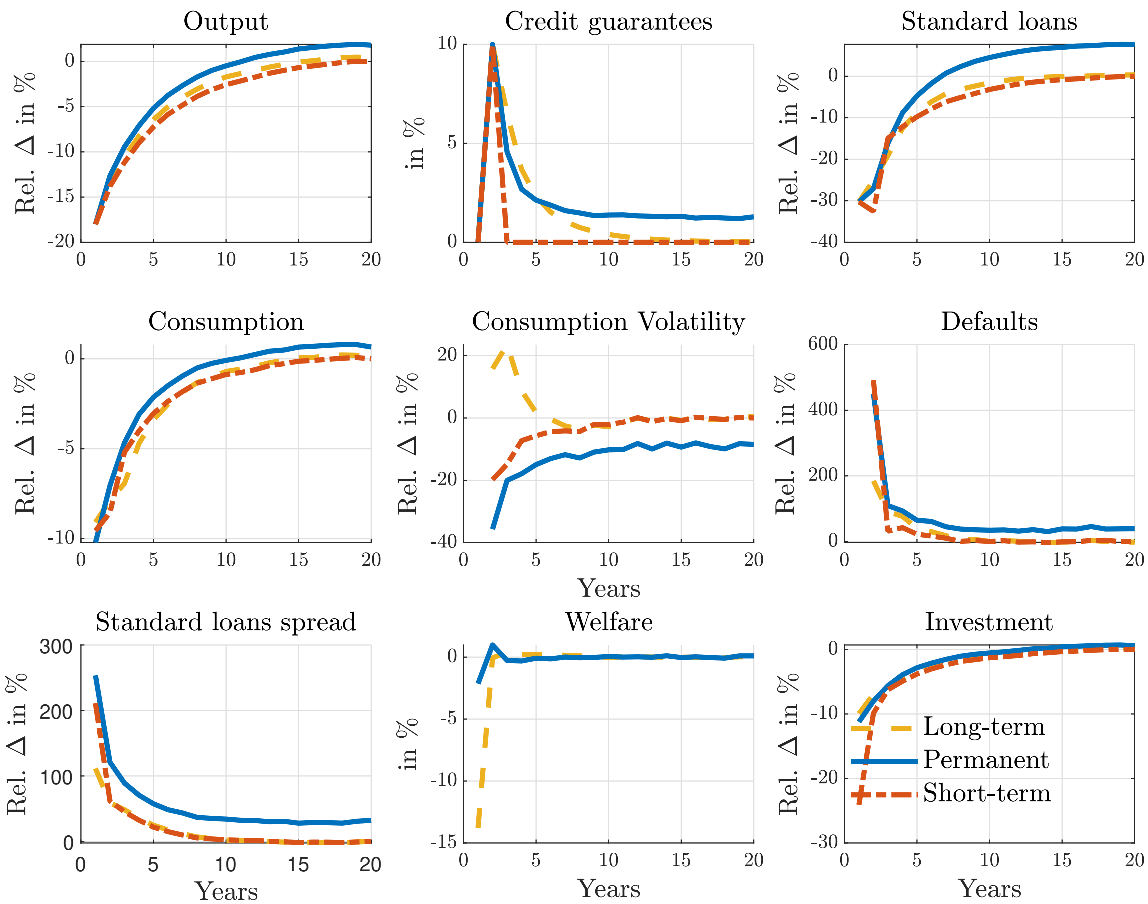
Figure 7: IRFs: Recurrent vs. One-time



Effects of introducing credit guarantees on model's variables with one-time and recurrent interventions.

times when new borrowing becomes excessively expensive. Upon first accessing credit guarantees, the borrower, despite deleveraging with standard loans, maintains total loans (comprising both standard loans and credit guarantees) at a level higher than the baseline. This leads to a higher spread in the long run. Similar to the recurrent case in Figure 7, the impatient borrower front-loads its consumption profile. In the long run, interest rate payments take their toll, and the borrower ends up with lower consumption and, thus, lower borrower-side value.

Figure 8: IRFs: Counterfactuals: Long-term credit guarantees, Permanent Credit guarantees



Effects of introducing credit guarantees on model's endogenous variables. The permanent economy introduces always-available credit guarantees. The economy with "Long-term" introduces credit guarantees as a long-term, one-time intervention while the economy with "Short-term" introduces credit guarantees as a short-term, one-time intervention.

7 Conclusions

This paper studies the effects of credit guarantee schemes (CGSs))—the largest and most widely adopted fiscal stimulus in the European Union- on firm behavior, focusing on the price at which guaranteed credit is offered rather than on credit access. Using a unique institutional feature of Belgium's 2020 CGS—a 25 basis-point discontinuity in guarantee fees based on a regulatory employment threshold—we provide quasi-experimental evidence that small differences in loan pricing lead to meaningful differences in firm outcomes. Firms that faced lower borrowing costs under the CGS increased investment, employment, and revenue growth, and experienced lower exit rates, relative to otherwise identical firms just above the threshold.

Our regression discontinuity design identifies these effects cleanly, and we provide extensive robustness checks to rule out alternative explanations. We then develop a quantitative model to interpret and generalize these findings. The model incorporates dynamic investment, two forms of debt (standard and guaranteed), and endogenous loan pricing. It is disciplined by key empirical elasticities and captures the core mechanism of debt substitution. We use this framework to study the endogenous price and borrower-side value implications of alternative policy designs, including recurrent CGSs and guarantees tied to long-term debt.

Our results deliver three takeaways. First, we provide clean evidence that the *price* of credit—not only access—is a first-order margin of adjustment during crises, using a discontinuity in the all-in cost of guaranteed borrowing. Second, we show that the program primarily operates through substitution away from costlier non-guaranteed debt and improvements in firms' short-run financial resilience. Third, a quantitative model disciplined by these micro elasticities highlights a stabilization–risk trade-off: one-off guarantees support activity in the short run, whereas recurrent and anticipated guarantees can raise leverage and default risk and shift tail exposure to the public sector.

This paper examines how lowering the *effective price of guaranteed credit* affects firm outcomes. Empirically, we exploit a discontinuity in the guarantee-fee schedule in Belgium's 2020 Credit Guarantee Scheme (CGS). Under the program rules, guaranteed loans were available to eligible firms, but those with more than 50 employees faced a guarantee fee that was 25 basis points higher than that for firms below the cutoff (50 vs. 25 bps). Because this fee is remitted to the government rather than to lenders, it creates quasi-experimental variation in borrowers' all-in borrowing costs on guaranteed credit that is not mechanically tied to bank interest income. Using a regression discontinuity design, we compare firms just above and below the 50-employee threshold to estimate the causal effect of lower borrowing costs on investment, employment, revenues, and distress outcomes.

Our primary finding is that firms facing a lower interest-rate schedule on guaranteed loans increase investment, employment, and revenues, and are less likely to exit. The evidence points to a price-based mechanism operating through the composition of borrowing. Firms substitute away from more expensive non-guaranteed credit toward cheaper financing, which improves their short-run liquidity and debt-service position (rollover pressure). By lowering the marginal cost of new borrowing in a stressed environment, the scheme supports additional investment and helps firms avoid inefficient contraction and exit.

We further show that our RD setup is robust and provides the ideal context for isolating the causal effect of improved pricing conditions on guaranteed loans. In particular, we demonstrate that firms near the eligibility threshold are comparable in all key dimensions, including access to additional credit, with the only distinction being that some receive more favorable loan pricing conditions.

We then complement our empirical estimates with a quantitative model of credit guarantees that addresses several valid questions that have not been investigated with our empirical design. We show that while a one-time, unexpected implementation of the policy was effective, an ex-ante implementation would result in losses, as it leads to higher indebtedness, a greater probability of default, and lower long-run borrower-side value. Ultimately, the design of CGSs—particularly their predictability and recurrence—critically shapes their effectiveness and borrower-side value consequences.

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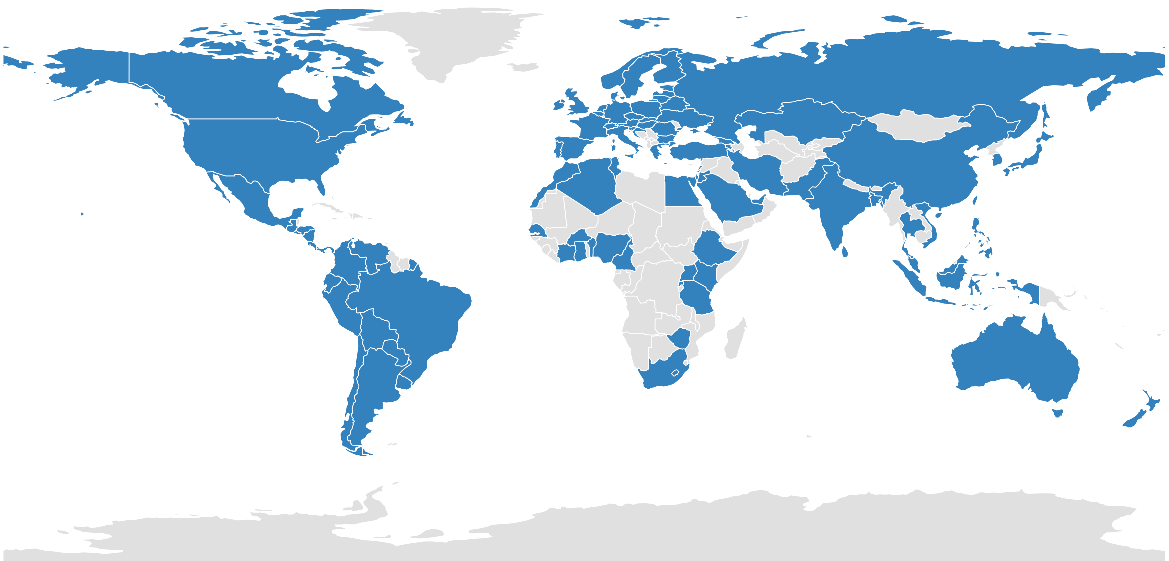
Appendix A A common policy: CGS

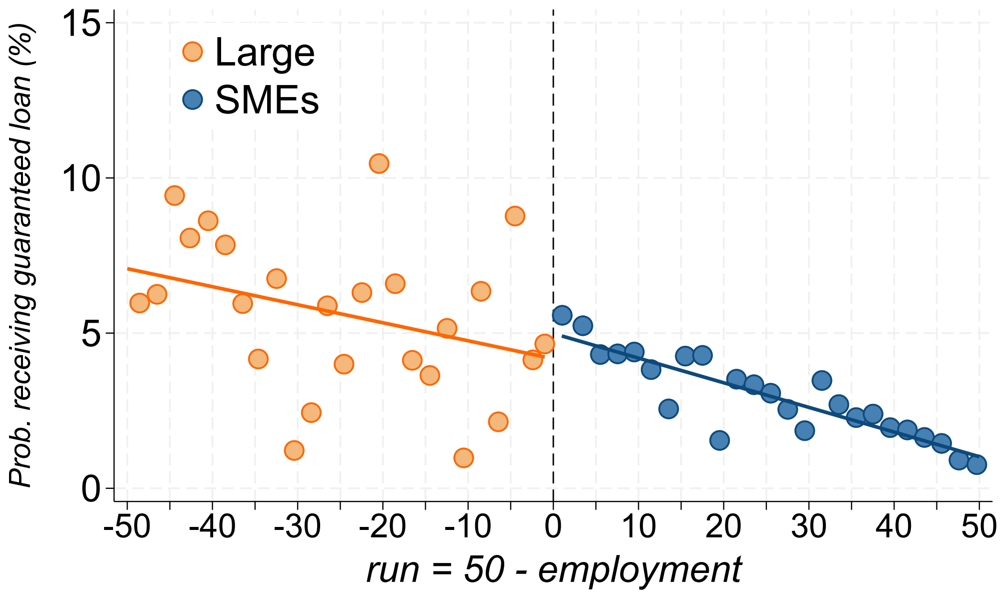
Figure A1: CGS implementers so far



The figure plots the countries that have implemented A CGS. Over 100 countries worldwide have CGS programs, startinf from 1930s.

Appendix B Identification Checks and Selection into Guaranteed Borrowing

Figure B2: Probability of Receiving a Guaranteed Loan in 2020



The figure plots the probability of receiving a publicly guaranteed loan in 2020 against the running variable defined as 50 minus employment in 2018. Each dot represents the share of firms receiving guaranteed loans within bins of the running variable, computed using the universe of firms in *Bel-first*. Blue dots correspond to SMEs (employment ≤ 50), and orange dots correspond to large firms (employment > 50). Solid lines show local linear fits estimated separately on each side of the cutoff. We choose twenty five bins on each side of the cutoff.

Table B1: Regression Discontinuity Estimates: Take-Up of Guaranteed Loans in 2020

	RD Estimate (pp)	Std. Error	Observations
Probability of CGS take-up	0.066	1.777	228,758
Mean take-up (within BW)		5.6%	
Bandwidth (in # emp)		16.7	

Authors' calculations. The table reports sharp regression discontinuity estimates of the probability of receiving a publicly guaranteed loan in 2020 using the universe of firms in *Bel-first*. The running variable is employment in 2018 normalized around the 50-employee cutoff. Estimates are obtained using local linear regression with MSE-optimal bandwidth selection. Robust Bias-corrected standard errors in parentheses, *, **, ***, indicate significance at the 10% 5% and 1% level, respectively.

Appendix C Summary Statistics

Table C2: Summary Statistics: 2020

	Mean	S.D	P^{25}	P^{50}	P^{75}	N_{obs}
Total liabilities (mill. €′)	11.9	64.8	0.4	1.1	3.8	2,852
Pub. guarant. debt (mill. €′)	3.1	12.1	0.1	0.3	1.0	2,853
Non guarant. debt (mill. €′)	8.8	68.4	0.2	0.6	2.3	2,851
Loan debt (mill. €′)	5.1	16.9	0.2	0.5	2.0	2,632
Leverage	0.6	0.2	0.4	0.6	0.8	2,632
Short-term loans (%)	50.4	28.7	25.2	47.9	75.5	2,632
Long-term loans (%)	45.0	28.5	20.1	46.0	70.0	2,632
Priv. guarant. debt (%)	37.6	29.5	11.1	30.8	63.0	2,632
Total assets (mill. €′)	15.7	51.2	0.8	2.1	7.0	2,632
Fixed assets (mill. €′)	8.6	28.7	0.3	0.9	3.6	2,605
Tangible fixed assets (%)	43.3	29.0	16.4	43.4	67.4	2,513
Cash and equiv. (%)	15.6	16.4	3.1	10.0	23.5	2,630
Acquis. tang. fixed assets (mill. €′)	1.0	9.9	0.0	0.0	0.2	2,682
Investment rate (%)	33.1	92.2	1.0	5.8	24.3	2,693
ΔEmployment (%)	-0.8	52.9	-8.0	0.0	6.2	2,058
ΔRevenues (%)	-4.6	60.9	-22.8	0.8	14.3	2,837
Average interest (%)	1.6	2.5	0.6	1.2	1.9	2,686
Guaranteed debt accumulation (%)	14.4	29.5	-2.6	5.1	27.9	2,852
Debt substitution (%)	-4.0	43.8	-8.4	-0.7	7.6	1,590
ΔDebt Service (%)	0.2	43.8	-14.8	-0.2	15.2	2,840
Exit (Last Available Year)	0.04	0.2	0	0	0	2,642
Exit (Legal Situation)	0.05	0.2	0	0	0	2,642

Authors' calculations. The table presents summary statistics for firms in our selected sample in 2020, using firm-level balance sheet data for Belgian firms participating in the 2020 CGS. Total liabilities comprise short-term debt (debts payable within one year), long-term debt (debts payable after one year), and provisions and deferred payments. Total debt, public guaranteed debt, non-guaranteed debt, total assets, fixed assets, and acquisition of tangible fixed assets are expressed in millions of euros. Loan debt is defined as the sum of financial debts payable within and after one year. Short-term debt, long-term debt, and private guaranteed debt are expressed as percentages relative to total liabilities. Tangible fixed assets and cash at hand are expressed as percentages relative to total assets. Leverage is defined as the ratio of total liabilities to total assets. Investment rate is calculated as the acquisition of tangible fixed assets divided by total fixed assets in the previous year. **Δ**Employment and **Δ**Revenues are symmetric year-over-year growth rates for the number of full-time equivalent employees and gross value added, respectively. Guaranteed Debt Accumulation is the percentage change in debt guaranteed by Belgian public authorities. Debt Substitution is the percentage change in non-guaranteed debt (i.e., total liabilities minus publicly guaranteed debt) relative to total liabilities. Average interest costs are computed as the ratio of financial charges to lagged total liabilities plus equity. ΔDebt Service Capacity is the annual change in the EBITDA-to-short-term debt ratio. Exit probabilities are defined as indicator variables for firms that (i) have no balance sheet data after 2023 (Last Available Year), or (ii) underwent liquidation, bankruptcy, dissolution, or absorption between 2020 and 2023 (Legal Situation).

Appendix D Debt Substitution, Interest Costs, and Debt Service Capacity Dynamics

Table D3: Debt Substitution, Financial Burden, and Debt Service: Dynamics

	Pre-Policy	Year: 2020	Post-Policy		
	T-1	T	T+1	T+2	T+3
	(1)	(2)	(3)	(4)	(5)
(A) Debt Substitution					
Sharp-RD	-0.035	-0.262*	-0.193*	0.250***	0.045
	(0.09)	(0.14)	(0.11)	(0.07)	(0.05)
Observations	994	1,590	1,700	1,260	968
Bandwidth (in # emp)	18.9	13.0	16.4	9.5	17.7
(B) Average Interest					
Sharp-RD	-0.002	-0.015***	-0.015***	0.007	-0.001
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Observations	2,518	2,596	2,608	2,572	2,515
Bandwidth (in # emp)	9.8	8.7	7.1	14.3	11.9
(C) ΔDebt Service Capacity					
Sharp-RD	-0.056	0.283***	0.186***	0.011	-0.086
	(0.08)	(0.08)	(0.06)	(0.05)	(0.07)
Observations	2,525	2,633	2,616	2,578	2,525
Bandwidth (in # emp)	12.2	8.7	14.5	14.7	9.6

Authors' calculations. The table reports RD estimates of the effect of lower interest rates on credit guarantees on firm-level debt structure and financing costs using Belgian balance-sheet data (2017–2023). Debt Substitution is the percentage change in non-guaranteed debt (i.e., total liabilities minus publicly guaranteed debt) relative to total liabilities. Average interest costs are computed as the ratio of financial charges to lagged total liabilities plus equity. ΔDebt Service Capacity is the annual change in the EBITDA-to-short-term debt ratio. Columns show estimates for the pre-policy year (T-1), the policy year (T), and up to three years after ($T+1$, $T+2$, $T+3$). All regressions control for industry fixed effects and an indicator for guaranteed loans from private banks. We use two-digit industry codes (NACE-BEL 2008-Rev. 2) for average interest and debt service capacity, and one-digit industry codes (NACE Rev. 2 main sections) for debt substitution. Estimates in the first row of each panel correspond to the RD estimate for β_{1,Y_t} in equation (2). Robust Bias-corrected standard errors in parentheses, *, **, ***, indicate significance at the 10% 5% and 1% level, respectively.

Appendix E Pre-existing differences

Table E4: Testing for pre-policy differences in firms' observable characteristics

Variable	Mean		RD Estimator	Robust Inference		BW (in # emp.)	Obs.
	Treatment	Control		p-value	95% Conf. Int.		
Total assets	20.90	21.38	0.484	0.70	[-1.60, 2.57]	12.63	7,848
Fixed assets total	18.10	17.38	-0.722	0.54	[-2.66, 1.22]	10.16	7,761
Tangible fixed assets	16.26	15.08	-1.184	0.32	[-3.15, 0.78]	9.40	7,492
Cash at hand	2.44	2.99	0.553	0.17	[-0.11, 1.22]	12.81	7,842
Total liabilities	13.96	12.24	-1.721	0.22	[-4.04, 0.60]	6.80	7,320
Leverage	0.71	0.71	0.003	0.92	[-0.05, 0.06]	8.12	7,320
Short-term debt (%)	0.57	0.62	0.049	0.11	[-0.00,0.10]	13.63	7,318
Long-term debt (%)	0.41	0.39	-0.024	0.45	[-0.08,0.03]	11.27	7,320
Wage Bill	2.95	2.80	-0.154	0.30	[-0.40, 0.09]	21.55	7,751
EBITDA	1.07	1.21	0.138	0.66	[-0.38, 0.66]	14.14	7,847
Profit rate	0.08	0.09	0.018	0.23	[-0.01, 0.04]	17.64	5,293

Authors' calculations. The table reports RD estimates (rows) for pre-determined firm characteristics across the employment threshold using Belgian balance-sheet data (2017–2019). Total assets, fixed assets, tangible fixed assets, cash at hand, wage bills, and EBITDA are expressed in millions of euros. Leverage is defined as the ratio of total liabilities to total assets. Short-term and long-term debt shares are calculated as the ratio of debt payable within and after one year, respectively, to total liabilities. Profit rate is the ratio of net profits to total assets. All regressions control for year and one-digit industry fixed effects (NACE Rev. 2 main sections). Robust bias-corrected standard errors are used to compute confidence intervals and p-values.

Appendix F DID Estimates

Let $D_i = \mathbf{1}\{FTE_i \geq 0\}$ and $D_t = \mathbf{1}\{\text{year} = t\}$ for $t = T-2, T-1, T+2, T+3$. We employ a difference-in-difference (DID) regression model using only observations within the average bandwidth of our running for each firm level outcome.

$$Y_{i,t} = \lambda_{k,t} + \delta D_i + \alpha_{T,Y} D_i \times D_T + \sum_{\tau=T-2}^{T-1} \alpha_{\tau,Y} D_i \times D_\tau + \sum_{\tau'=T+1}^{T+3} \alpha_{\tau',Y} D_i \times D_{\tau'} + \epsilon_{ik,t} \quad \text{(F1)}$$

The estimates for the contemporaneous effect of being treated by a lower interest rate on credit guarantees (i.e., $\alpha_{T,Y}$) is presented in F5.

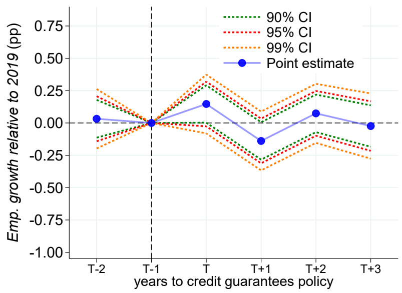
Table F5: DID results: Firm level outcomes

	ΔEmployment	**Investment rate**	**ΔRevenues**
DID	0.147*	0.082	0.098
	(0.088)	(0.116)	(0.091)
Bandwidth (in # emp)	8.8	10.0	8.4
Observations	377	572	443
$\bar{R}^2$	50.5	53.4	23.1
	ΔAssets	**ΔEquity**	**ΔLoan Debt**
DID	-0.039	-0.032	0.147
	(0.064)	(0.022)	(0.174)
Bandwidth (in # emp)	8.7	21.0	11.0
Observations	327	996	419
$\bar{R}^2$	37.5	21.7	40.0

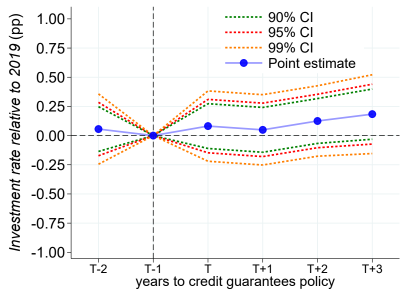
Authors' calculations. The table reports DID estimates of the effect of lower interest rates on credit guarantees on firm-level outcomes, using Belgian balance-sheet data (2017–2023). The sample includes firms within the average bandwidth of the running variable used for each cross-sectional RD estimate in Table 1. Investment rate is defined as the acquisition of tangible fixed assets relative to total fixed assets in the previous year. **Δ**Employment and **Δ**Revenues are symmetric year-over-year growth rates for the number of full-time equivalent employees and gross value added, respectively. **Δ**Total assets and **Δ**Equity are annual changes relative to total assets. **Δ**Loan debt is the annual growth in financial debts payable within and after one year. All regressions include time-varying fixed effects, defined as interactions between year and (i) two-digit industry codes (NACE-BEL 2008-Rev. 2), (ii) the distance to the €4.5 million asset threshold, and (iii) an indicator for guaranteed loans from private banks. Estimates correspond to the DID coefficient $\alpha_{T,Y}$ in equation (F1). Standard errors in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

The full dynamic estimates (i.e., $\alpha_{T-2,Y}$, ..., $\alpha_{T+3,Y}$) are depicted in Figure F3.

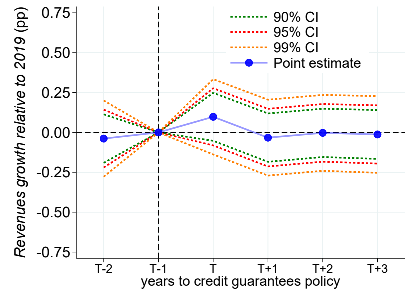
Figure F3: DID Dynamic Estimates: Firm Level Outcomes



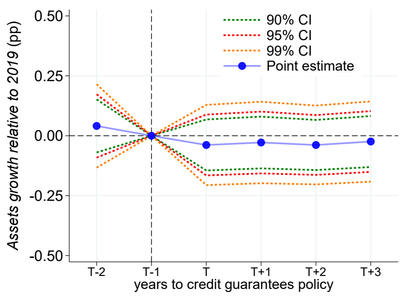
(a) Employment



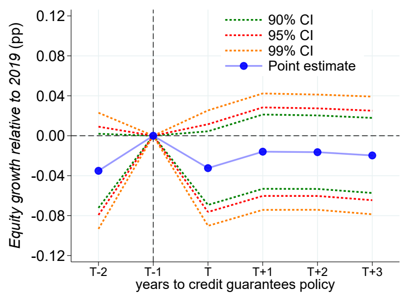
(b) Investment Rate



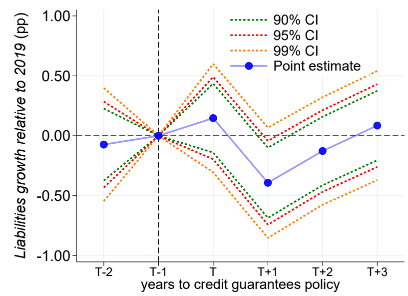
(c) Operating Revenues



(d) Assets



(e) Equity



(f) Liabilities

The figure plots the point estimates and confidence intervals for the lead (α_{T-2},α_{T-2}), contemporaneous (β), and lag (α_{T+1},α_{T+2},α_{T+3}) coefficients from equation (F1).Panel (a), (b), and (c) show the dynamic estimates for employment growth, investment rate, and revenue growth. Panel (d), Panel (e), and Panel (f) show the results for total assets, equity, and loan debt growth.

Appendix G Robustness: Marginal Employment Classification

Table G6: Firm-Level Outcomes: Marginal Employment Subsample

	Investment Rate	ΔEmployment	ΔRevenues	ΔAssets	ΔEquity	ΔLoan Debt
Sharp-RD	0.169***	0.239**	0.627***	-0.076***	-0.047***	0.415***
	(0.03)	(0.10)	(0.13)	(0.01)	(0.01)	(0.05)
Obs.	547	436	570	540	540	537
BW (in # emp)	10.7	7.3	8.7	6.0	11.2	11.8

Authors' calculations. The table reports RD estimates of the contemporaneous effect of lower interest rates on credit guarantees on firm-level outcomes using a restricted sample of firms for which employment is the marginal determinant of size classification under the official multidimensional rule. Investment rate is the acquisition of tangible fixed assets relative to total fixed assets in the previous year. ΔEmployment and ΔRevenues are symmetric year-over-year growth rates for number of full-time equivalent employees and gross value added, respectively. ΔTotal assets and ΔEquity are annual changes relative to total assets. ΔLoan debt is the annual growth in financial debts payable within and after one year. All regressions control for two-digit industry fixed effects (NACE-BEL 2008-Rev. 2), an indicator for guaranteed loans from private banks, and the distance to the €4.5 million asset threshold. Estimates correspond to β_{1,Y_t} from equation (2) for the policy year (2020). Robust bias-corrected standard errors are reported in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels.

Table G7: Financial Outcomes: Marginal Employment Subsample

	Guarant. Debt Accum.	Debt Subst.	Average Interest	ΔDebt Serv. Capacity	Exit Probability	
					Last Availab. Year	Legal Situation
	(1)	(2)	(3)	(4)	(5)	(6)
Sharp-RD	-0.026	-0.351*	-0.010***	0.461***	-0.094***	-0.176***
	(0.11)	(0.20)	(0.00)	(0.09)	(0.03)	(0.06)
Obs.	618	420	565	570	601	601
BW (in # emp)	13.2	13.1	8.7	8.7	7.1	7.3

Authors' calculations. The table reports RD estimates of the contemporaneous effect of lower interest rates on credit guarantees on firm-level debt structure, financing costs, and exit risk using the marginal employment subsample described in Appendix Appendix G. Guaranteed debt accumulation is the percentage change in debt guaranteed by Belgian public authorities. Debt substitution is the percentage change in non-guaranteed debt (i.e., total liabilities minus publicly guaranteed debt) relative to total liabilities. Average interest costs are computed as the ratio of financial charges to lagged total liabilities plus equity. ΔDebt service capacity is the annual change in the EBITDA-to-short-term debt ratio. Exit probabilities are defined as indicator variables for firms (i) with no balance sheet data after 2023 (Last Available Year), or (ii) undergoing liquidation, bankruptcy, dissolution, or absorption between 2020–2023 (Legal Situation). All regressions control for industry fixed effects and an indicator for guaranteed loans from private banks. Two-digit industry codes (NACE-BEL 2008-Rev. 2) are used for average interest and debt service capacity, and one-digit industry codes (NACE Rev. 2 main sections) for the other outcomes. Estimates correspond to β_{1,Y_t} from equation (2) for the policy year (2020). Robust bias-corrected standard errors are reported in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels.

Appendix H Computation

The solution to the model is achieved by iteratively updating the value functions, V^R and V^D, along with the price function, q. To address the potential issue of multiplicity highlighted in Krusell and Smith (2003), we first solve for the equilibrium in a finite-horizon economy. Starting from an initial guess for the terminal value, we iterate backward until the differences between the value and price functions in consecutive periods fall below 10^{-5}. The resulting objects are then used as the starting point for computing the equilibrium in the infinite-horizon version of the model.

The numerical implementation employs a grid with 40 points for standard loans, 40 points for credit guarantees, 40 points for capital, and 31 points for income. Expectations are computed using 300 Gauss-Legendre quadrature nodes and finer grids of standard loans, credit guarantees, and capital (100 points in each dimension).[26]

To solve the model with a one-time policy intervention, we introduce an additional state variable, p, which governs the probability of accessing credit guarantees. Under the one-time intervention scenario, this probability is set to 10^{-6} upon the realization of a liquidity shock, effectively rendering it negligible. This assumption enables us to compute policy functions under a single-intervention framework. In contrast, in the recurrent policy regime, the probability of accessing credit guarantees conditional on a liquidity shock is set to one, introducing the flexibility to incorporate uncertainty about policy recurrence.

Appendix I Tables Investment Ratios

Variable	Mean	SD	$\frac{\text{Mean Investment}}{\text{Mean Variable}}$	$\frac{\text{SD Investment}}{\text{SD Variable}}$
Investment (mill. €′)	0.184	5.653	1	1
Operating Income (mill. €′)	5.276	10.492	0.035	0.539
Assets (mill. €′)	3.095	27.513	0.059	0.205
Investment/Operating Income	0.138	0.408		
Investment/Assets	0.060	0.118		

[26]We use tools developed in Önder (2023) for the portfolio problem. In particular, we rely on bi-dimensional optimizers, which have been shown to outperform taste-shock methods in portfolio choice applications. We also implement the divide-and-conquer algorithm proposed by Gordon and Qiu (2018) to accelerate convergence.

Appendix J Risk-Averse Lenders

Lenders. We enrich the model's lending side with a flexible framework that permits exploration of lender-side mechanisms and shock propagation. A unit mass of identical, risk-averse, competitive lenders arrives in overlapping generations and lives for two periods. Each lender enters with state-dependent wealth $w_i(g)$, where $g \in \{g_H, g_L\}$ captures global financial conditions. This allows, for instance, COVID-19 to be treated as a credit supply shock: $w_i(g_H) < w_i(g_L)$. In the analysis below, we kept it constant.

Lenders invest their wealth in risky sovereign loans L, and—when available—government-backed credit guarantees L_C.

In the second period, lenders may face (i) inclusion (borrowers continue borrowing), or (ii) exclusion (sovereign default). In the inclusion state, lenders can trade both newly issued and outstanding loans. In exclusion, only pre-existing lenders trade claims in secondary (collection) markets.

Lenders' Problem. Lenders have Epstein-Zin recursive preferences (Epstein and Zin, 1989), with CRRA period utility $U(C)$, relative risk aversion Γ, and inverse intertemporal elasticity of substitution ϱ. Each lender maximizes:

$$\max_{C,C',\ell>0} \left[(1-\beta^{\ell}(g))U(C)^{1-\varrho} + \beta^{\ell}(g)\left(\mathbb{E}_{s'|s}\left\{U(C')^{1-\Gamma}\right\}\right)^{\frac{1-\varrho}{1-\Gamma}} \right]^{\frac{1}{1-\varrho}} \tag{J2}$$

subject to:

$$C = w_i(g) - q(L', L'_C, k', s)L' - \mathcal{I}(g)\frac{L'_C}{1+r_C} \tag{J3}$$

$$C' = w_i(g') + \mathcal{R}(L', L'_C, k', s')L' + \mathcal{I}(g)L'_C \tag{J4}$$

The indicator function $\mathcal{I}(g) \in \{0,1\}$ activates the credit guarantee program. Returns on sovereign loans are:

$$\begin{aligned} \mathcal{R}(L', L'_C, k', s') = {} & (1-d(L', L'_C, k', s'))\left[\kappa + (1-\delta)q(L'', L''_C, k'', s')\right] \\ & + d(L', L'_C, k', s') \cdot q_D(L', L'_C, k', s') \end{aligned} \tag{J5}$$

where $d(\cdot) \in \{0,1\}$ is the sovereign default indicator, κ is the coupon, and δ is the maturity rate, as before.

Pricing Kernel. The stochastic discount factor (SDF) under Epstein-Zin preferences is:

$$M(s,s') = \underbrace{\beta^{\ell}(g')\left(\frac{C'}{C}\right)^{-\varrho}}_{\text{Intertemporal substitution}} \underbrace{\left(\frac{U(C')}{\mathbb{E}_{s'|s}\left\{U(C')^{1-\Gamma}\right\}^{1/(1-\Gamma)}}\right)^{\frac{\varrho-\Gamma}{1-\varrho}}}_{\text{Risk adjustment}} \tag{J6}$$

When $\varrho = \Gamma$, this collapses to standard CRRA utility. The no-arbitrage condition implies:

$$q(L', L'_C, k', s) = \mathbb{E}_{s'|s} \left\{ M(s, s') \cdot \mathcal{R}(L', L'_C, k', s') \right\} \tag{J7}$$

where $q(\cdot)$ denotes the price of a one-period bond issued in state s and repaid (or not) in s'.

Lenders' problem when the borrower has bad credit standing. Lenders that arrive in a state where the firm is in a bad credit standing face a similar problem but with a different set of risky returns. In particular:

$$\max_{C, C', \ell > 0} \left[(1 - \beta^{\ell}(g)) U(C_d)^{1-\varrho} + \beta^{\ell}(g) \left(\mathbb{E}_{s'|s} \left\{ U(C'_d)^{1-\Gamma} \right\} \right)^{\frac{1-\varrho}{1-\Gamma}} \right]^{\frac{1}{1-\varrho}} \tag{J8}$$

subject to:

$$C_d = w_i(g) - q_D(L, L_C, s) L, \tag{J9}$$

$$C'_d = w_i(g') + \mathcal{R}_d(L', L'_C, s') L, \tag{J10}$$

$$\begin{aligned} \mathcal{R}_d(L', L'_C, k', s') = \alpha\psi \ & \Big((1 - d') \left[\kappa + (1 - \delta) q \left(\hat{L} \left(\alpha L', \alpha L'_C, k', s' \right), \hat{L_C} \left(\alpha L', \alpha L'_C, k', s' \right), s' \right) \right] \\ & + d' q_d (\alpha L', \alpha L'_C, k', s') \Big) + (1 - \psi) q_d \left(L', L'_C, k', s' \right), \end{aligned} \tag{J11}$$

J.1 Quantitative Results

Recalibration The model is recalibrated with the following parameters: default cost parameters $d_0 = -0.81$, $d_1 = 0.985$, lenders discount factor $\beta^{\ell} = 1$, wealth $w = 50$, the inverse of the elasticity of intertemporal substitution (EIS) $\varrho = 0.5$, and the coefficient of relative risk aversion of lenders $\Gamma = 2$.

Results We only modify the lender block of the model, keeping all other elements—including the Bellman equations and shock processes—identical to the baseline. The results in this section can therefore be interpreted as a robustness exercise for the assumption of risk neutrality on the lender side. Table 8 summarizes the long-run outcomes, while Figure 4 presents the impulse responses. A key finding is that the results remain strikingly similar to those in the main text. In particular, when the CGS is implemented as a recurrent policy, it raises the long-run default probability. Consistent with earlier insights, an anticipated and recurring credit guarantee scheme leads to higher borrower indebtedness relative to the baseline. This arises because firms internalize the expectation that the CGS will be activated in response to future liquidity shocks, leading them to borrow more today—both through standard loans and credit guarantees. Anticipated future support thus exacerbates

Table 8: Main Results: Long-run moments in different model economies with averse lenders

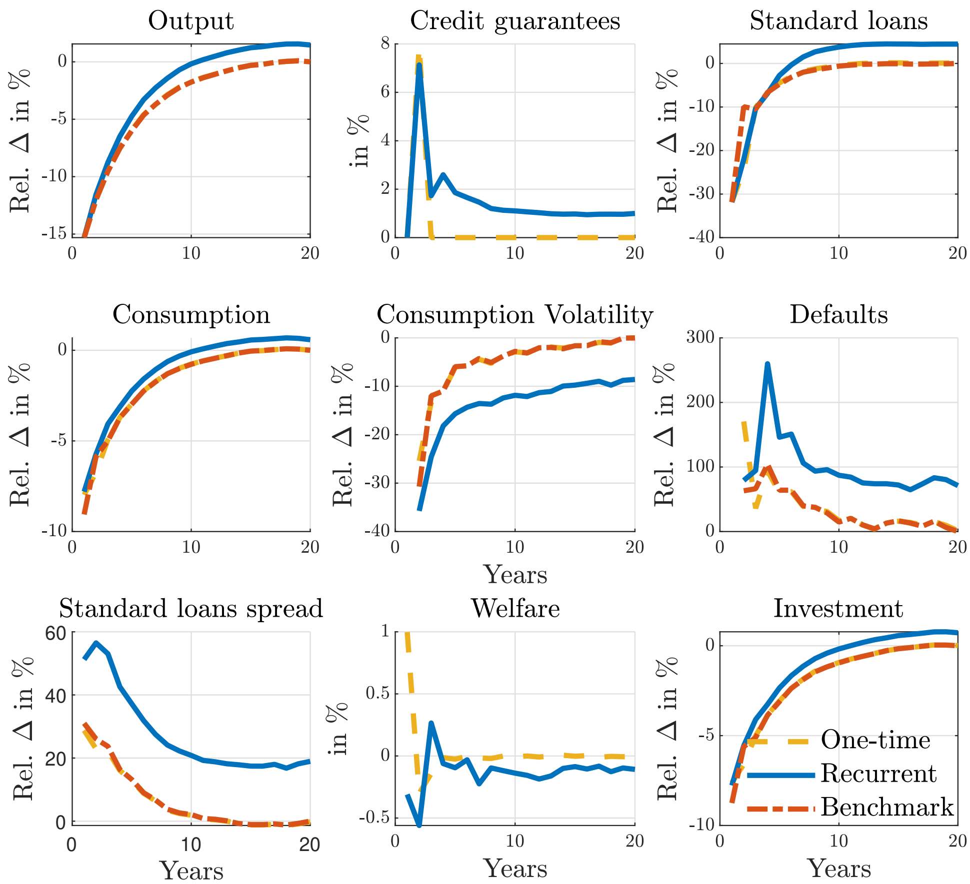
	(1) Data	(2) Baseline (No cred guarantee)	(3) Credit guar. One-time	(4) Credit guar. Recurrent
Mean standard loan to opr. income (%)	33.7	34.4	34.4	35.9
Mean credit guarantees to opr. (%)	*n.a.*	*n.a.*	one-time	0.7
Mean investment to opr. (%)	14	16	16	16
Excess investment volatility (σ_i/σ_y)	0.4	0.4	0.4	0.4
Mean spread (r_s) (%)	1.8	1.8	1.8	2.1
Spread volatility $(\sigma(r_s))$	0.6	0.6	0.6	0.9
$\rho(r_s, y)$	-0.5	-0.7	-0.7	-0.8
Default rate	5.0	1.6	1.6	3.2
Average duration of debt (in yrs)	4.1	4.1	4.1	4.0
Spread rise during liq. shocks	1.0	1.1	1.1	1.5

The first column reports data moments, the second column reports the results of the baseline model without credit guarantees, and the third column presents the results with credit guarantees with 10% cap and one-time unanticipated intervention, while the last column shows the results with credit guarantees with recurrent intervention. The standard deviation of a variable is denoted by σ, and the coefficient correlation between variables is denoted by ρ. Consumption and income are reported by natural logs.

debt dilution: lenders rationally expect higher future borrowing and reduce the price of loans for any given debt level.

One notable departure from the main text is in the borrower-side value implications of the recurrent policy. Under risk-averse lenders, the recurrent CGS generates value losses, in contrast to the gains observed under risk-neutral lenders. Although investment and output are higher in this scenario, the policy induces deadweight losses through elevated default rates, which ultimately dominate the benefits.

Figure 4: IRFs: Recurrent vs. One-time



Notes: Effects of introducing credit guarantees on model's variables with one-time and recurrent interventions with risk-averse lenders.

J.2 Further discussion on lenders' block

In our analysis, we allow discount factors, wealth, and risk aversion to be state-dependent parameters, but we keep them constant throughout for tractability. This setup nonetheless provides a flexible environment within which various types of shocks and their implications can be explored. For example, one could study policies aimed at restoring market confidence. In the event of a bank-run-type shock, the supply of loanable funds may decline, such that $w_i(g_L) < w_i(g_H)$. However, if a credit guarantee scheme is activated, one might assume that the pool of loanable funds is restored, implying $w_i(g_H) = w_i(g_L)$. We leave a systematic exploration of these extensions to future research.