

Loan guarantees, collateral and liquidity: Theory and evidence

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Abstract

We adapt Holmström and Tirole’s (1998) corporate liquidity framework to develop and test a theory of loan guarantees. Guarantees operate through the collateral channel (easing borrowing constraints and expanding project scale) and the liquidity-insurance channel (raising firms’ liquidity

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to scale ratios and shielding them from shocks). Prior evidence finds little support for the collateral channel and largely overlooks liquidity insurance. Using Portuguese data, we show that each euro of a guarantee backing 75% of a four-year loan increases firm liquidity holdings by €0.94 within two years, without discernible effect on scale. The evidence is consistent with moral hazard but not adverse selection.

Keywords: Investment, Liquidity, Collateral, Credit guarantees

JEL Classification Codes: G21, G28, G31

1 Introduction

Credit guarantee schemes, whether provided by private entities or supported by the government, are widely prevalent. Asia has the largest schemes according to Beck, Klapper, and Mendonza (2010) but they are also prominent elsewhere. In the United States, new federal loan guarantees issued in 2019 alone backed loans worth roughly 7% of GDP (Bachas, Kim, and Yannelis, 2021). In Europe, the outstanding stock of firm loan guarantees represented 2.1% of GDP in Italy in 2014, 1.8% in Portugal in 2015, and 0.8% in France in 2015 (Kraemer-Eis, Lang, Torfs, and Gvetadze, 2016).

The standard policy rationale for these schemes is grounded in the conventional theory of credit rationing: loan guarantees reduce financial constraints arising from the informational asymmetries that plague small firms, thus unlocking access to external finance and promoting firm investment (Honohan, 2010). Yet, despite their widespread adoption and economic significance, the real effects of loan guarantees have proved difficult to identify empirically, casting doubt on their effectiveness.¹ The goal of this paper is to solve the apparent paradox/contradiction between the theoretical literature and the empirical findings.

To this end, we develop and test a theory of loan guarantees that reconciles the gap between theoretical predictions and empirical evidence. We argue that the paradox arises from limitations on both the supply and demand sides of existing frameworks, which focus too narrowly on the role of credit in financing upfront investment. On the supply side, credit guarantee schemes provide a composite financial product that bundles two distinct assets. The first is a standard bank loan. The second, often overlooked in the literature, is a contingent claim—a form of insurance that grants access to liquidity in adverse states. Existing theories typically emphasize the loan component while neglecting the embedded liquidity insurance provided by the guarantee, and therefore largely ignore the selection and incentive problems inherent to insurance provision.

On the demand side, the conventional theory of credit rationing emphasizes the collateral channel, whereby guarantees increase pledgeable income and enable firms to raise external finance at the outset of a project. This perspective is incomplete, however, because many projects require additional funding over time. Many high-value activities (particularly those involving large upfront fixed costs, such as exporting or innovation) are highly exposed to future liquidity shocks,

¹See, for example, Uesugi (2008) and Uesugi, Sakai, and Yamashiro (2010) for the case of Japan, Kang, Heshmati and Choi (2008) for the Korean case, Zecchini and Ventura (2009), D’Ignazio and Menon (2020), and Blasio et al. (2018) for Italy, or Rodrigues et al. (2018) for Portugal.

including cost overruns or temporary revenue shortfalls. Because future cash flows are not fully pledgeable as collateral, private markets tend to underprovide insurance against the risk of liquidity shocks. Official loan guarantees can fill this gap by ensuring access to contingent liquidity in episodes of financial stress. When a guarantee is activated, the firm gains access to a pool of funds that allows it to continue operating and complete its project, thereby avoiding inefficient disruptions. In this sense, credit guarantees negotiated *ex ante* provide liquidity insurance that is triggered *ex post*.

Loan guarantees should thus be analyzed within the broader context of corporate liquidity management, supporting firms' precautionary motives rather than merely expanding scale. In the first part of the paper, we extend Holmström and Tirole's (1998) framework of corporate liquidity management to develop a theory of loan guarantees. In our model, firms face two distinct financial constraints—a shortage of collateral and a shortage of insurance—and guarantees operate by relaxing these constraints. Through the collateral channel, guarantees relax borrowing constraints by increasing pledgeable income, thereby allowing firms to undertake larger projects. This channel arises if and only if guarantees embed an explicit or implicit subsidy—we show that the collateral channel is mute if guarantees are not subsidized. Through the liquidity channel, guarantees enhance firms' ability to self-insure by increasing their liquidity-to-scale ratios, thereby reducing the likelihood of inefficient project disruption following adverse shocks. We show that even in the absence of subsidies, loan guarantee schemes can improve social welfare by reallocating liquidity to states in which it is most valuable.

These two channels yield two central, testable implications. First, the liquidity channel predicts that larger guarantees will increase firms' liquidity-to-scale ratios. Importantly, larger guarantees need not translate into greater investment scale at inception, as would be implied by the collateral channel alone. Because liquidity and scale are economic substitutes, resources devoted to precautionary liquidity (such as idle cash holdings, or the upfront fee paid for the guarantee) crowd out investment in productive capacity. Second, viewing guarantees as insurance implies a positive relationship between guarantee size and the probability of activation. This relationship can arise through two distinct mechanisms: adverse selection, whereby firms more exposed to liquidity risk self-select into larger guarantees, and moral hazard, whereby more generous coverage weakens firms' incentives to avoid liquidity shortfalls, increasing activation rates among firms with larger guarantees.

In the second part of the paper, we empirically test these two implications. This exercise raises two central challenges. First, guarantee size is potentially endogenous: the amount of guaranteed credit a firm receives may be correlated with unobserved investment opportunities rendering simple OLS estimates unin-

formative. Second, testing the insurance mechanism requires observing whether guarantees are activated. Activation behavior is informative about both adverse selection and moral hazard, yet such information is rarely available and therefore largely absent from the existing literature. To address both challenges, we exploit a rich proprietary firm-level dataset with a quasi-experimental institutional setting that together support a credible identification strategy.

Our dataset consists of granular firm-level information from *Banco Português de Fomento*, the national institution coordinating Portugal’s credit guarantee scheme. This scheme’s institutional design offers two features crucial for identification. First, guarantees are issued under standardized contracts, ensuring comparability across firms and enabling the construction of well-defined treatment and control groups. Second, we link guaranteed loans to firms’ financial statements, which allows us to control for firm characteristics and track liquidity and investment outcomes for both activating and non-activating firms over the five years following guarantee issuance.

Our quasi-experimental setting combines a regression discontinuity design with a differences-in-differences approach. Regarding the regression discontinuity design, our identification strategy exploits an institutional rule that ties access to loan guarantees to firm certification assigned by an independent government agency. Certification depends on a score variable (constructed from the number of employees, assets, and sales) and a cutoff rule. Firms with scores below the cutoff are eligible for guarantees of up to €18,750, whereas firms above the cutoff can access guarantees of up to €37,500. We use this discontinuity to construct an instrument for guarantee size, comparing firms with different eligibility ceilings. This design provides a plausibly exogenous source of variation in guarantee size: firms with scores located narrowly around the cutoff are similar in observable and unobservable characteristics, including investment opportunities. Identification therefore comes from comparing changes in scale and liquidity among firms with similar pre-treatment scores (i.e. similar characteristics, including similar size) but differential access to guaranteed credit.

Regarding the second component of our quasi-experimental setting, our identification strategy exploits a reform in the Portuguese loan guarantee scheme implemented on 16 January 2012, which increased the coverage rate of guarantees from 50% to 75% of the underlying bank loan. This reform mechanically raised the amount of contingent liquidity (and thus the degree of liquidity insurance) embedded in each guarantee. We study the effects of this reform using a differences-in-differences approach that compares firms before and after 2012. To construct credible treatment and control groups, we leverage the regression discontinuity design described above: treated firms have scores just above the cutoff and therefore access to larger guarantees, while control firms have scores

just below the cutoff and access to smaller guarantees. This combination of a regression discontinuity design and a differences-in-differences approach allows us to isolate the causal impact of an exogenous increase in liquidity insurance on firm outcomes, holding firms' underlying characteristics constant.

We document four main findings by comparing firms eligible for larger guarantees (treated) with otherwise similar firms eligible for smaller guarantees (control), before and after the issuance of a credit guarantee covering 75% of a four-year bank loan. First, treated firms receive guarantees that are, on average, €17,855 larger than those of control firms, close to the maximum eligibility difference of €18,750.

Second, we find no statistically significant effect of guarantees on firm output (our proxy for scale) in the two years following issuance, nor over a five-year horizon. These results are consistent with the existing empirical literature, which generally finds limited effects of credit guarantees on firm growth.

Third, we document a strong liquidity effect. Liquidity, measured as the sum of cash holdings and the contingent payment embedded in the guarantee, increases by €0.94 for every additional euro of guarantee within two years. This effect raises firms' liquidity-to-output ratios in the short run and closely matches the predictions of our theoretical framework. The average difference in liquidity holdings between treated and control firms gradually dissipates and becomes statistically indistinguishable from zero after five years, when the guarantee expires, and liquidity-to-output ratios among treated and control firms converge.

Finally, within the treated group, guarantee size does not differ systematically between firms that eventually activate their guarantees and those that do not, suggesting that the initial allocation of guarantees is not driven by adverse selection. Ex post, firms receiving larger guarantees are more likely to activate them, consistent with the presence of moral hazard or, more broadly, with changes in firms' risk-taking incentives induced by more generous guarantee coverage.

The remainder of the paper is organized as follows. The next section reviews the related literature, and the following section presents the model of corporate liquidity and credit guarantees. Section 4 describes the institutional background of the Portuguese loan guarantee scheme. Section 5 introduces the data and variable construction, and Section 6 outlines the empirical methodology. Section 7 presents the main results, while the following two sections provide extensions and robustness checks. The final section concludes with policy implications.

2 Literature review

Our paper contrasts two distinct views of banking and credit guarantees. In one view, banks primarily act as monitors that substitute for collateral, so that credit rationing arises from insufficient pledgeable income or insufficient monitoring (Holmström and Tirole, 1997); guarantees then operate by relaxing collateral constraints and expanding credit access. In the other view, banks are providers of liquidity insurance, committing to supply funds in adverse states when firms are hit by liquidity shocks (Holmström and Tirole, 1998). The existing literature on credit guarantees largely adopts the first perspective, treating guarantees as a device to offset limited collateral or monitoring/screening capacity. In contrast, we emphasize that guarantees are valuable not only because they mitigate credit rationing *ex ante*, but because they secure access to liquidity *ex post*. From this standpoint, the central inefficiency is not merely a shortage of collateral, but a shortage of insurance against liquidity shortfalls. We assign no monitoring role to official agencies, which typically lack such capacity.

Most theoretical analyses of credit guarantees adopt the collateral-based perspective. For example, Arping, Lóránth, and Morrison (2010) and Wilcox and Yasuda (2013) study guarantees in one-period models in which the guarantee agency absorbs credit risk on behalf of the firm, effectively augmenting collateral and relaxing borrowing constraints. A common implication of these frameworks is that guarantees should raise real outcomes such as investment or output. Empirically, however, such effects have proved difficult to detect. In contrast, we analyze credit guarantees within a two-period liquidity framework in which the guarantor also absorbs liquidity risk. This shift from a static to a dynamic setting moves the focus from *ex ante* credit access to *ex post* liquidity provision and yields predictions that better align with the empirical evidence.

Our analysis also relates to the broader literature on collateral and pledgeability. A substantial empirical literature studies how the liquidation value of assets supports borrowing capacity (Benmelech, Garmaise, and Moskowitz 2005; Benmelech 2008; Benmelech and Bergman 2009, 2011; Gavazza 2010). We differ from this work in that credit guarantees constitute collateral with an explicit and contractually fixed value, rather than an asset whose collateral value must be inferred from liquidation outcomes. Closest in spirit is Cerqueiro, Ongena, and Roszbach (2016), who study the collateral embedded in floating liens. However, floating liens offer limited protection against adverse liquidity shocks and provide little *ex post* flexibility. More generally, while the collateral literature emphasizes borrowing capacity, it largely abstracts from the insurance role of collateral, a role that is central in the case of publicly backed credit guarantees.

A strand of empirical literature evaluates the effects of credit guarantee schemes on firm outcomes. While numerous official reports and academic studies document the operation of these schemes, many do not rely on causal identification. Among studies that do, evidence from a variety of institutional settings—including Japan (Uesugi 2008; Uesugi, Sakai, and Yamashiro 2010), Korea (Kang, Heshmati and Choi 2008), Italy (Zecchini and Ventura 2009; Blasio et al. 2018; D’Ignazio and Menon 2020), Portugal (Rodrigues et al. 2018), and the United States (Bachas, Kim, and Yannelis 2021)—typically finds little or no short-run effect of guarantees on investment. From the perspective of a collateral-based framework, these null results are puzzling. In contrast, they are consistent with a liquidity-insurance view in which guarantees primarily affect firms’ balance-sheet composition rather than balance-sheet scale. A recent contribution by Bonfim, Custódio, and Raposo (2023) examines a Portuguese certification program that also provides access to subsidized loan guarantees. However, because certification and guarantees are bundled, the study cannot disentangle the Holmström and Tirole (1997)-style certification effect from the insurance role of the guarantee. This may explain why the authors detect a positive effect on investment.

Our paper differs from this strand of literature along three dimensions. First, we cleanly identify the effect of credit guarantees in a quasi-experimental setting that isolates guarantees from other policy interventions. Second, we extend the standard credit-rationing framework by explicitly modeling the liquidity-insurance function embedded in loan guarantees, which yields novel, testable predictions about firms’ balance-sheet responses and the activation of guarantees in adverse states. Third, whereas much of the existing work interprets guarantees through their effects on investment or productivity, we show that their primary impact operates through liquidity provision, reshaping firms’ liquidity management rather than expanding investment scale.

3 Liquidity theory

In this section we adapt the standard corporate liquidity management model of Holmström and Tirole (1998) to include credit guarantees, and identify the implications to be tested in the empirical section.

3.1 The model

Two-period models of liquidity naturally accommodate the role of collateral (as for example in the one-period model of Holmström and Tirole 1997) and there-

fore we build a two-period model of credit guarantees that integrates both the conventional collateral and liquidity channels. The model rests on four key ingredients. First, only part of the firm's returns is pledgeable, giving rise to credit rationing. Second, financing and investment decisions are made upfront, before liquidity needs are known. Third, after the initial investment is sunk, the firm experiences a liquidity shock, creating a demand for additional funds. Fourth, the shock is sufficiently severe that the firm must secure financing in advance to avoid costly credit rationing (arising from the premature termination of the project).

The model is an extension of the standard corporate liquidity management model of Holmström and Tirole (1998). The timing of the model is depicted in Figure 1. There are three dates (dates 0, 1, and 2) and a single good. A firm has an insider (the entrepreneur) with a single investment project with a constant return to scale investment technology, who may raise capital from outside investors (the bank) to implement the project. Entrepreneurs and banks are risk neutral, the risk-free rate is zero, and there is perfect competition among banks so that entrepreneurs appropriate all surplus. Accordingly, the bank offers insurance at actuarially fair terms and the market rate equals the risk-free rate.

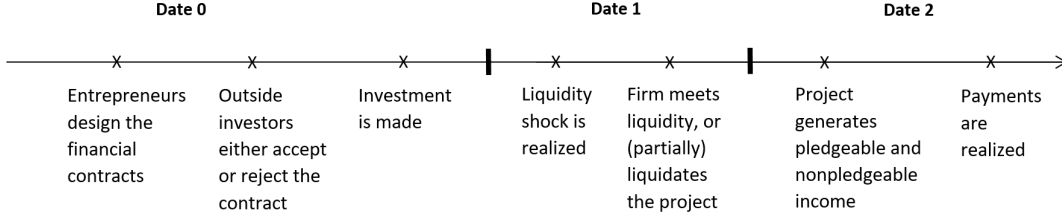


Figure 1: **Timeline** The figure depicts the sequence of events in the model.

At date 0 the entrepreneur chooses the scale of the project $I \in [0, \infty)$, and the final return is obtained at date 2. At date 1 the project is hit by random cost overrun (a “liquidity shock”) proportional to the initial investment, and the entrepreneur must reinvest some extra resources so as to continue with the project. The liquidity shock per unit of the initial investment may be low ρ_L , medium ρ_M , or high ρ_H with probabilities f_L , f_M , and $1 - f_L - f_M$ respectively. The entrepreneur can continue at a smaller scale than I , and the continuation scale is denoted by i with $0 \leq i \leq I$. The continuation scale i is contingent on the shock, and therefore $i \in \{i_L, i_M, i_H\}$ corresponding to the low, medium and high liquidity shocks respectively. The total investment input thus equals $I + i_L\rho_L$, $I + i_M\rho_M$, or $I + i_H\rho_H$. The realization of the shock is publicly observable.

Returns are realized at date 2. The project yields a pledgeable return $\rho_0 i$, which the entrepreneur can credibly commit in advance to pay to outsiders, and

yields a nonpledgeable private return $(\tilde{\rho}_1 - \rho_0) i$ to the entrepreneur. The wedge between pledgeable and nonpledgeable returns may arise for several reasons, notably from the presence of firm-specific capital (assets that are valuable to the borrower but have limited value to the lender). Holmström and Tirole (1997) also show how a simple moral hazard problem with limited liability can generate such a wedge. For simplicity, we abstract from these mechanisms and take the wedge as given. The total return $\tilde{\rho}_1$ is a proxy for unobservable investment opportunities and may take one of three values, $\tilde{\rho}_1 \in \{\rho_1 - \epsilon, \rho_1, \rho_1 + \epsilon\}$ with $\epsilon > 0$ being an arbitrarily small number.

The entrepreneur is protected by limited liability, and has an endowment $A > 0$ at date 0. Entrepreneurs/firms may borrow to make the initial investment in the project, or they may become outside investors and lend their endowment competitively. Throughout we assume the following features of the project.

Inefficient ex ante liquidation First, we assume

$$1 + f_L \rho_L + f_M \rho_M + (1 - f_L - f_M) \rho_H < \frac{1 + f_L \rho_L + f_M \rho_M}{f_L + f_M} < \frac{1 + f_L \rho_L}{f_L} < \rho_1. \quad (1)$$

The ratios represent the expected cost of one unit of the project, adjusted by the probability of completing the project. The expression implies the project has positive expected total net present value (NPV) when terminated if there is a medium or a high cost overrun, and continuing in all states is better than liquidating the project.

Inefficient ex post liquidation Second, we assume

$$0 \leq \rho_L < \rho_M < \rho_H < \rho_1 \quad (2)$$

meaning that it is efficient to continue the project ex post because the initial investment is a sunk cost and $\rho_H < \rho_1$.

Constraints on finance at the start Third,

$$\rho_0 < 1 + f_L \rho_L + f_M \rho_M + (1 - f_L - f_M) \rho_H \quad (3)$$

which implies the project pledgeable income generates insufficient funds to invest initially and carry on with the project in all states—i.e. there is credit rationing at the start of the project. The total present value per unit of investment in the project is the full social surplus per unit of investment ρ_1 , whereas the pledgeable present value per unit of investment in the project is the share ρ_0 that can be appropriated by outside investors. Although the total NPV of the project is positive, the pledgeable part is negative. Entrepreneurs must thus have their own assets A in order to be granted finance, thereby keeping the investment I

finite in the constant returns to scale environment—leverage is limited by the nonpledgeability of the income of the project.

Constraints at the refinancing stage Fourth,

$$\rho_L < \rho_0 < \rho_M \quad (4)$$

so that the low liquidity shock does not require pre-arranged financing, but liquidity must be planned in advance in the medium and high shock states as these shocks are not self-financing—i.e. there is credit rationing at the refinancing stage.

3.2 The second-best contract

Without loss of generality, we focus on the limiting case $\epsilon \rightarrow 0$. A contract between the entrepreneur and outside investors specifies the scale of the project I , the continuation scales i_L , i_M , and i_H , and the final payments to outside investors and the entrepreneur. It is efficient to commit all of the project's pledgeable income $\rho_0 i$ to the outside investors so as to make the project scale as large as possible, and assign only the illiquid portion of the income $(\rho_1 - \rho_0) i$ to the entrepreneur. Holding only the illiquid income is incentive compatible because the return on internal funds A exceeds the risk-free rate. The next two results characterize the second-best.

Lemma The second-best solution solves

$$\begin{aligned} & \max_{\{I, i_L, i_M, i_H\}} f_L (\rho_1 - \rho_L) i_L + f_M (\rho_1 - \rho_M) i_M + (1 - f_L - f_M) (\rho_1 - \rho_H) i_H - I \\ & \text{subject to} \\ & f_L (\rho_0 - \rho_L) i_L + f_M (\rho_0 - \rho_M) i_M + (1 - f_L - f_M) (\rho_0 - \rho_H) i_H \geq I - A \\ & 0 \leq i_L, i_M, i_H \leq I. \end{aligned}$$

The objective function in the maximization problem is the expected social return on the investment. The first constraint is the participation constraint of outside investors—akin to a budget constraint—and relates the bank's outlays to the firm's pledgeable income, requiring that the bank's expected net return be at least equal to its initial investment.

The participation constraint shows the bank provides insurance against liquidity shocks. When the low shock occurs, the bank receives $\rho_0 - \rho_L$ per unit of continued investment. In the remaining states, the bank pays the firm $\rho_M - \rho_0$ or $\rho_H - \rho_0$ per unit of continued investment. The project has negative NPV for the

bank in these two states, and insurance allows the firm to raise fresh funds for an ongoing project with negative NPV. The following result describes the solution to the above maximization problem.

Proposition 1 The second-best solution is $\{I, i_L, i_M, i_H\} = \{I^*, I^*, I^*, I^*\}$ with

$$I^* = \frac{1}{1 - f_L(\rho_0 - \rho_L) - f_M(\rho_0 - \rho_M) - (1 - f_L - f_M)(\rho_0 - \rho_H)} A. \quad (5)$$

Proof Since $\rho_1 - \rho_L$ and $\rho_0 - \rho_L$ are both positive, the entrepreneur and the bank are interested in continuing at full scale when the low shock occurs, and therefore $i_L = I$. The maximization program then amounts to choosing the initial scale I and the continuation scales i_M and i_H in the medium and in the high shock states. Full continuation in the medium and in the high shock states follows from Assumption (1). The budget constraint binds at the optimum, and solving with respect to investment I and replacing I in the objective function we obtain the result. ■

The optimal design reflects a trade-off between liquidity and scale. The firm can either operate at a large scale with a maturity mismatch (holding substantial long-term illiquid assets and little short-term liquidity) or choose a smaller-scale but more resilient balance sheet. Proposition 1 implies that efficiency requires full liquidity insurance, at the cost of reduced scale. This outcome is driven by Assumption (1), which makes continuation socially optimal in all states of nature, including those in which the bank incurs a negative ex post NPV.

3.3 Costless implementation of the second-best solution

We now solve the entrepreneur's problem, assuming that the firm has no alternative use for funds and that the bank can commit to financing negative-NPV projects. The entrepreneur appropriates the nonpledgeable income and the outside investors appropriate the pledgeable income. The next two results characterize the market equilibrium.

Lemma Under costless implementation, the entrepreneur solves

$$\max_{\{I, i_L, i_M, i_H\}} f_L(\rho_1 - \rho_0) i_L + f_M(\rho_1 - \rho_0) i_M + (1 - f_L - f_M)(\rho_1 - \rho_0) i_H - A \quad (6)$$

subject to

$$\begin{aligned} f_L(\rho_0 - \rho_L) i_L + f_M(\rho_0 - \rho_M) i_M + (1 - f_L - f_M)(\rho_0 - \rho_H) i_H &\geq I - A \\ 0 \leq i_L, i_M, i_H &\leq I. \end{aligned}$$

Proof The entrepreneur would rather commit all of the project's pledgeable income to the outside investors so as to make the project scale as large as possible, and keep only the illiquid portion of the income. The entrepreneur only holds illiquid claims because the return on internal funds A exceeds the risk-free rate. ■

The budget constraint binds and full continuation is always rational, and the following result describes the solution to the problem of the entrepreneur.

Proposition 2 Under costless implementation, the entrepreneur sets $\{I, i_L, i_M, i_H\} = \{I^*, I^*, I^*, I^*\}$.

The second-best allocation, which maximizes expected social return, is equivalent to the solution of the entrepreneur's problem of maximizing private rents. This allocation can be implemented in several ways. One natural mechanism is to grant the firm a credit line up to $(\rho_H - \rho_0)I$, which it can readily use at date 1. This facility enables the firm to withstand all shocks. The firm does not draw on the credit line under a low shock, since it is self-financing; it draws partially under a medium shock; and it fully exhausts the credit line under a high shock. The firm pays upfront for the expected use of liquidity at actuarially fair prices, so the credit line provider breaks even in expectation. Ex post, however, any drawdown has negative NPV, and the intermediary is compelled to finance projects with negative NPV.²

3.4 Costly implementation of the second-best solution

The implementation of the second-best contract faces three distinct challenges. The first stems from the entrepreneur's ability to divert excess liquidity toward uses not specified in the contract, either at date 0 or at date 1. For example, when the second-best allocation prescribes continuation following a large adverse shock, entrepreneurs may prefer not to preserve liquidity for reinvestment. Instead, they may allocate these funds to a larger initial investment I , particularly if they anticipate renegotiating loan terms with the bank at date 1. This possibility reflects a standard soft budget constraint problem, which undermines the credibility of the ex ante liquidity arrangement. The second challenge concerns the bank's ability to honor its funding commitment at date 1. When liquidity provision entails negative net present value lending, banks incur substantial regulatory capital costs, which may limit their willingness or capacity to offer fully committed liquidity

²There are other feasible options such as hoarding cash at date 0. Outside investors lend $I + \rho_H I - A$ at the initial date, and make sure that the firm hoards liquid assets such as Treasury bonds at date 0 in the amount $\rho_H I$.

ex ante.³ The third challenge is more fundamental: the private sector may be unable to generate sufficient aggregate liquidity for the efficient functioning of the productive sector as in Holmström and Tirole (1998), Kahn and Wagner (2017) or Jorge and Kahn (2023).

We posit that official credit guarantee schemes are designed precisely to address these frictions by mitigating soft budget constraint problems, alleviating banks' regulatory capital constraints when loans are partially guaranteed by the public sector, or supplying outside liquidity in response to aggregate shocks. For simplicity, we assume that in the absence of official credit guarantees, then $i_M = i_H = 0$. Under this assumption, the second-best allocation is unattainable without guarantees.

In our framework, the government introduces a vertical supply of credit guarantees to correct market imperfections. We model credit guarantees as transfers from a government agency to outside investors (the bank) in states in which firms face liquidity shortfalls. By covering these states, guarantees prevent firms from being prematurely liquidated. We solve the problem of the entrepreneur with costly implementation and with official credit guarantees, and then compare the solution to this problem with the second-best solution.

Define parameter $\bar{G}$ as the value of the official credit guarantee provided by the government agency. Let ℓ represent the upfront fee paid by the firm for an official credit guarantee with value $\bar{G}$ (ℓ is akin to an insurance premium). We assume the firm activates the loan guarantee in the medium and in the high liquidity shock states (these are the states in which the project generates insufficient liquidity), thereby receiving a fixed amount $\bar{G}$ which the firm uses to repay the bank.

The value of the guarantee $\bar{G}$ establishes the limits for the continuation scales i_M and i_H , such that $\rho_M i_M \leq \bar{G}$ and $\rho_H i_H \leq \bar{G}$. The loan guarantee agency expects to pay $(1 - f_L) \bar{G}$ at date 1, and receive the upfront fee ℓ plus the unused funds from the guarantee equal to $(1 - f_L) \bar{G} - f_M \rho_M i_M - f_H \rho_H i_H$. The

³Banks provide liquidity to firms through two main types of credit lines: revocable (uncommitted or conditional) and irrevocable (committed or unconditional) lines of credit. Revocable credit lines are subject to covenants that allow banks to revoke them, meaning access to these facilities is often restricted precisely when firms need liquidity the most; as a result, only irrevocable credit lines effectively provide insurance against liquidity shocks. Consistent with this distinction, the Basel Accords assign higher capital requirements to irrevocable than to revocable credit lines. Loan guarantees are equivalent to irrevocable credit lines in our empirical setting. An irrevocable credit line with a maturity exceeding one year requires banks to hold risk-weighted assets equal to 100% of the contracted limit, regardless of whether the line is being used or not (a revocable line consumes capital only for the amount actually used, with no capital requirement for the undrawn portion; Banco de Portugal 2007). Hence, under the regulatory framework prevailing in 2012, credit provision and liquidity insurance were treated equivalently from a capital perspective.

bank delivers $I + \ell - A$ to the firm at the start of the project, and receives $f_L(\rho_0 - \rho_L)i_L + f_M\rho_0i_M + f_H\rho_0i_H$. We define s as the value of the subsidy implicit in the guarantee. If we assume fair insurance, then $s = 0$ and the net value of the guarantee $f_M\rho_Mi_M + f_H\rho_Hi_H - \ell$ is zero. If $s > 0$, then there will be a subsidy to firms and the insurance provided by the credit guarantee will not be fairly priced. In this context, the entrepreneur solves

$$\max_{\{I, i_L, i_M, i_H\}} f_L(\rho_1 - \rho_0)i_L + f_M(\rho_1 - \rho_0)i_M + (1 - f_L - f_M)(\rho_1 - \rho_0)i_H - A \quad (7)$$

subject to

$$f_L(\rho_0 - \rho_L)i_L + f_M\rho_0i_M + f_H\rho_0i_H \geq I - A + \ell$$

$$f_M\rho_Mi_M + f_H\rho_Hi_H = \ell + s$$

$$\rho_Mi_M \leq \overline{G}$$

$$\rho_Hi_H \leq \overline{G}$$

$$0 \leq i_L, i_M, i_H \leq I.$$

This maximization problem differs from the problem of the entrepreneur/firm without credit guarantees (6) in the following ways:

- The first constraint is the participation constraint of the bank. The bank no longer absorbs the liquidity risk of the firm as in Problem (6).
- The second constraint is the participation constraint for the loan guarantee agency. The subsidy s represents the additional collateral generated by the guarantee, which is the driver of the collateral channel of credit guarantees.
- There are two additional “liquidity constraints” restricting the continuation values i_M and i_H . These constraints are the drivers of the liquidity channel.

Consider the value for investment $I^R = \frac{A + (1 - f_L - f_M)(\rho_0 - \rho_H)\overline{G}/\rho_H}{1 - f_L(\rho_0 - \rho_L) - f_M(\rho_0 - \rho_M)}$ and assume $\rho_M I^R < \rho_H I^*$ for simplicity. The following result describes the solution to the problem of the entrepreneur when it is costly to implement the second-best allocation and guarantees are not subsidized.

Proposition 3 (The liquidity channel) With costly implementation and credit guarantees with value $\overline{G}$ and fair insurance ($s = 0$), then:

- (Non-liquidity-rationed firms) If $\overline{G} \geq \rho_H I^*$, then entrepreneurs choose the second-best solution with $\{I, i_L, i_M, i_H\} = \{I^*, I^*, I^*, I^*\}$.

- (Liquidity channel for liquidity-rationed firms) If $\rho_M I^R \leq \bar{G} < \rho_H I^*$, then entrepreneurs choose $\{I, i_L, i_M, i_H\} = \{I^R, I^R, I^R, I^P\}$ with $I^P = \frac{\bar{G}}{\rho_H - \rho_0} < I^* < I^R$.

Proof With costly implementation and credit guarantees with value $\bar{G}$ and combining the two participation constraints into a single participation constraint, the entrepreneur solves

$$\max_{\{I, i_L, i_M, i_H\}} f_L(\rho_1 - \rho_0) i_L + f_M(\rho_1 - \rho_0) i_M + (1 - f_L - f_M)(\rho_1 - \rho_0) i_H - A \quad (8)$$

subject to

$$f_L(\rho_0 - \rho_L) i_L + f_M(\rho_0 - \rho_M) i_M + (1 - f_L - f_M)(\rho_0 - \rho_H) i_H \geq I - A - s$$

$$\rho_M i_M \leq \bar{G}$$

$$\rho_H i_H \leq \bar{G}$$

$$0 \leq i_L, i_M, i_H \leq I.$$

Under fair insurance, $s = 0$ and the participation constraint is identical to the participation constraint in Problem (6). Hence, the differences between Problem (6) and Problem (8) are the two liquidity constraints. The participation constraint holds with equality because the participation constraint of the bank binds.

We consider two cases. In the first case, $\rho_H I^* \leq \bar{G}$. In this case the value of the guarantee does not restrict the entrepreneur and can achieve the second-best allocation. This is his rational choice. In the second case, $\rho_M I^R \leq \bar{G} < \rho_H I^*$. It is rational for the firm to choose full continuation in the low shock and in the medium shock states because the firm has access to enough liquidity. The second liquidity constraint binds under our assumptions, and this fact allows to replace $i_H = \bar{G}/\rho_H$ in the budget constraint. Solving for investment yields $I = i_L = i_M = I^R > I^*$. To check the inequality, note that $I^* > \bar{G}/\rho_H$, replace I^* for its formula, and reorganize terms to see that $I^* > \bar{G}/\rho_H \Leftrightarrow I^R > I^*$. ■

To see the intuition behind this result, consider the case of Portuguese loan guarantees. This case provides a natural setting to test the effect of guarantees, as the Portuguese official scheme sets a bound on the value of the loan guarantee equal to €18,750 for the control group of firms and equal to €37,500 for the treated group. Hence, $\bar{G}$ may take one of two values depending on the group of firms. We are interested in the marginal effect of the official credit guarantee (i.e. the intensive margin) when $\rho_M I^R \leq 18\,750 < \rho_H I^* \leq 37\,500$.

- Firms with access to large guarantees (treated group, with $\bar{G} = 37\,500 \geq \rho_H I^*$) can cover the medium and the high cost overruns. In this case, the

official guarantee provides full insurance, and firms optimally choose the second-best allocation.

- Firms with access to small guarantees (control group, with $\rho_M I^R \leq \bar{G} = 18\,500 < \rho_H I^*$) can fully cover the medium cost $\rho_M I^R$, *but the guarantee is insufficient to cover the large cost overrun $\rho_H I^R$* . According to Proposition 3, these firms must inefficiently liquidate part of their projects in the high-shock state (since $i_H < I^R$). Proposition 3 further implies that firms with smaller guarantees choose projects of larger scale (since $I^R > I^*$), reflecting the substitution between liquidity and scale, this being a manifestation of the liquidity-scale trade-off property.

Firms face two distinct constraints in our model. First, they face a collateral shortage, reflected in the bank’s participation constraint. Second, firms face a shortage of insurance, captured by the two liquidity constraints. Proposition 3 shows that, under actuarially fair insurance, large loan guarantees alleviate the liquidity constraints *but leave the collateral constraint unaffected*. This result may be written as follows.

Corollary Taking the second-best allocation as a reference, nonsubsidized loan guarantees do not create collateral for initial investment.

Firms with access to large guarantees choose the second-best allocation in the absence of subsidized loan guarantees ($s = 0$), a result that would not hold if guarantees created additional collateral. Liquidity-rationed firms, by contrast, optimally select projects that are too large relative to the second best, not because collateral constraints are relaxed, but as a consequence of the liquidity-scale trade-off.

Subsidized credit guarantees and the collateral channel Credit guarantee schemes, like the Portuguese scheme, often represent an implicit subsidy to firms.⁴ For example, Bachas, Kim, and Yannelis (2021) report that projected U.S. federal credit assistance programs in 2019 embedded an implicit subsidy of \$37.9 billion in loan guarantees.

The conventional view interprets credit guarantees primarily as collateral that mitigates credit rationing. Proposition 3, however, shows that the collateral channel is mute in the absence of subsidies ($s = 0$). It follows that any collateral effect of loan guarantees must be driven by the subsidies they embed. When a guarantee entails a subsidy, then the firm’s pledgeable income increases by

⁴The Portuguese credit guarantee agency subsidizes part of the upfront fee (see Appendix 3).

$s = f_M \rho_M i_M + f_H \rho_H i_H - \ell > 0$, thereby indirectly relaxing the participation constraint of financial intermediaries.⁵ This implication can be formalized as follows.

Corollary Taking the second-best allocation as a reference, loan guarantees create collateral if and only if they are subsidized.

Firms with access to large guarantees would not chose the second-best allocation if guarantees were subsidized ($s > 0$). These firms would increase scale beyond the efficient scale. Hence, implicit or explicit subsidies have important implications for both policy and empirical analysis. On the policy side, subsidies distort firms' scale choices, making the second-best outcome generally unattainable. Models that focus only on subsidies and the conventional collateral channel (i.e. rationing occurs solely at project inception) thus provide incomplete foundations for the design of efficient guarantee schemes.

On the empirical side, the effect of guarantee size on scale is ambiguous. On the one hand, larger guarantees reduce scale through the liquidity-scale trade-off property embedded in Proposition 3 (since $I^R > I^*$). On the other hand, the subsidy $s = f_M \rho_M i_M + f_H \rho_H i_H - \ell > 0$ raises pledgeable income, relaxes borrowing constraints, and encourages firms to expand scale. Because these two forces work in opposite directions, theory yields ambiguous predictions about the relationship between guarantee size and investment scale (thereby explaining the apparent paradox/contradiction between the theoretical literature and the empirical findings described earlier). Yet, despite the ambiguous effects on scale, the model predicts that firms with access to larger guarantees (i.e., non-liquidity-rationed firms) exhibit higher liquidity-to-scale ratios than firms with access to smaller guarantees (i.e., liquidity-rationed firms).

3.5 Empirical implications

The model generates a testable prediction that maps directly into observable firm-level outcomes. Proposition 3 implies that, holding all else constant, a larger guarantee eases liquidity constraints. The ideal experiment would assign guarantees of different size to two identical firms, thereby tightening the liquidity constraint for one firm relative to the other. Our empirical strategy approximates this setup by exploiting exogenous variation in guarantee size as an observable proxy for the severity of liquidity rationing. Against this background, we derive the following testable implication from Proposition 3.

⁵To see this, combine the two participation constraints in Problem (7) into a single participation constraint, as in Problem (8) in the Proof of Proposition 3.

Implication 1 (The liquidity-scale trade-off property) An increase in guarantee size raises the firm’s liquidity-to-scale ratio. This follows from the model’s prediction that, when liquidity is scarce, additional guarantees are directed primarily toward liquidity buffers rather than project scale.

We test this implication using our dataset on Portuguese credit guarantees. To do this, we use two measures of scale: firm output and fixed assets. Yet, there are three reasons why fixed assets may not be the best proxy for project scale. First, investments in fixed assets typically exclude spending on marketing and supply chains (for instance, costs associated with internationalization) which are recorded as operating expenses rather than intangible assets (Altomonte, Morlacco, Sonno, and Favoino, 2025). Second, fixed assets are a poor measure of the economic value of capital when firms replace fully depreciated assets: in such cases, the accounting value of capital rises even though its economic value remains unchanged (Jorge and Zhao, 2025). Finally, firms expand scale through both capital and labor, so equating scale solely with fixed assets provides an incomplete picture. To see this, consider the following production function with constant returns to scale: $Y = \theta K^\alpha L^{1-\alpha}$ with Y, θ, K , and L representing firm output, technology, capital, and labour, respectively, and a constant $\alpha \in (0, 1)$. It follows that $\partial \ln Y / \partial \ln K = \alpha < 1$, because of the diminishing marginal productivity of capital. Hence, one would expect that output grows less than capital, when labor grows less than capital. Since labour is “sticky” in the short run, then output is a more reliable measure of the scale of the project.

We use several measures of liquidity. In particular, and following Sufi (2009, page 1059), we also use the sum of cash plus the contingent payment implicit in credit guarantees as this sum represents the total liquidity available to the firm. A loan guarantee is a contingent asset that gives access to a pool of cash in the case of activation, thereby representing a substitute for cash.

Insurance, adverse selection, and moral hazard Our approach views loan guarantees as a form of liquidity insurance. This perspective naturally raises the standard concerns associated with asymmetric information in insurance markets, namely adverse selection and moral hazard. In the empirical insurance literature, these issues are typically studied through the joint behavior of insurance coverage and accident risk. In our setting, coverage corresponds to the size of the guarantee, while accident risk maps naturally into the probability of guarantee activation. A central prediction of models with asymmetric information is a positive correlation between coverage and accident frequency.

The empirical literature on car and health insurance exploits this insight extensively. Chiappori and Salanié (2000) and Chiappori, Jullien, Salanié, and Salanié (2006) use the property that the insurer offers a menu of contracts to

argue that there is a positive correlation between coverage and frequency of accidents.⁶ This correlation also holds in a dynamic setting (Abbring, Chiappori, Heckman, Pinquet 2003, Abbring, Chiappori, Pinquet 2003).

Loan guarantee schemes, however, differ in important ways from the insurance environments typically studied in the literature. First, official guarantee agencies do not offer a menu of contracts designed to induce self-selection or to mitigate incentive problems. In our setting, coverage levels (€18,750 versus €37,500) are determined by administrative certification criteria rather than by firms' risk characteristics, and the unit price of insurance does not increase with coverage. Second, guarantee contracts are effectively static. Portuguese guarantee agencies deliberately avoid repeated interactions with firms that activate their guarantees, in contrast to standard insurance markets (such as car insurance) where long-term relationships and experience rating play a central role. As a result, the contractual environment we study features simple, one-shot insurance arrangements with limited scope for dynamic incentive provision.

In this institutional environment, the standard model of asymmetric information predicts a positive correlation between guarantee size (coverage) and the probability of activation (accidents). This prediction can arise through two distinct channels. First, under adverse selection, riskier firms should sort into larger guarantees, generating a positive correlation between coverage and activation. However, because guarantees are subsidized and firms can generally apply for the maximum coverage for which they are eligible, this selection channel may be muted or entirely absent in the data. Second, under moral hazard, more generous guarantees weaken firms' incentives to avoid liquidity shortfalls, leading to higher activation rates among firms with larger guarantees. We exploit these predictions by examining the relationship between guarantee size and activation as a test for the presence and nature of asymmetric information in the Portuguese loan guarantee scheme. We conclude by stating the testable implication that will be examined later.

Implication 2 (The correlation between guarantee size and frequency of activation property) The frequency of activation is increasing in the size of the guarantee, as larger guarantees exacerbate adverse selection and moral hazard in insurance provision.

⁶Jorge and Kahn (2023) show that an analogous contract menu property arises in a corporate liquidity management setting closely related to ours.

4 Institutional background

In this section, we describe the institutional setting of the Portuguese credit guarantee system and the specific guarantee programme analyzed in this paper. We outline how credit guarantees operate in practice and detail the rules governing eligibility. Because access to Portuguese credit guarantees requires official certification as a small or medium-sized enterprise (SME), we also describe the SME certification process, with emphasis on micro and small firms, which constitute the sample used in our analysis.

Credit guarantees in Portugal The current configuration of the Portuguese credit guarantee system dates to 2003. Its operations are carried out jointly by specialized intermediaries (the *Sociedades de Garantia Mútua*, SGM hereafter) and by commercial banks. Appendix 1 presents the institutional structure of the system; here we describe the mechanics of a typical guaranteed lending transaction.

A credit guarantee transaction involves three parties: the borrowing firm, the bank, and the SGM. To obtain a guaranteed loan, the firm submits a project to its bank, which conducts an initial screening and, if the project is deemed eligible, submits an application to the SGM on behalf of the firm. The bank does not transmit a credit score or other proprietary credit assessment to the SGM. The SGM then evaluates the application and makes the guarantee decision. Lacking privileged information about the firm, the SGM’s assessment primarily verifies eligibility criteria. Consistent with this limited role, the rejection rate is very low (1.1% in our sample) likely reflecting banks’ anticipation of SGM approval standards.

Conditional on approval, the bank extends a four-year installment loan with constant quarterly repayments at an interest rate set by the SGM. The firm pays a guarantee premium and is required to purchase shares in the SGM, which function as a deductible and may be resold upon expiration of the guarantee. The guarantees analyzed in this paper cover 75% of the loan principal. In the event of default, the SGM pays the bank 75% of the outstanding loan balance (it is a guarantee on first demand), appropriates the firm’s SGM shares, and acquires 75% of the loan claim. Anecdotal evidence suggests that SGMs do not actively pursue debt collection after guarantee activation. Banks, however, may renegotiate the remaining exposure with the firm and are required to report the non-guaranteed share of the loan as overdue credit to the Portuguese credit register.

SME certification Applying for a loan guarantee requires official SME certification issued by *IAPMEI – Agência para a Competitividade e Inovação, I.P.*

Year dimension	Number of employees	Annual sales		Total assets
Micro firm	< 10	≤ €2 million	or	≤ €2 million
Small firm	< 50	≤ €10 million	or	≤ €10 million

Table 1: Thresholds for computing year dimension. Source: European Commission (2019)

(IAPMEI), a public agency that operates independently of the SGM.⁷ The certification process, documented in Cruz, Serrano, and Gonçalves (2014), consists of two steps. First, the firm’s *year dimension* is determined using annual activity data, combining the number of employees (mandatory) with either sales or assets, according to the thresholds reported in Table 1. We replicate this procedure using a score variable (constructed from the number of employees, sales, and assets) together with a cutoff rule. Firms with scores below the cutoff receive micro certification, whereas firms above receive small certification. Details on the construction of the score variable are available in Appendix 4. Second, firms are assigned either micro or small certification based on their *year dimensions* over the previous three years. A firm retains its certification status if it attains the same *year dimension* for two consecutive years. As a result, certification is typically determined more than one year before a firm applies for a loan guarantee. This timing reflects the fact that certification is based on the firm’s penultimate *year dimension* rather than on contemporaneous firm characteristics. Appendix 2 provides additional administrative details on the SME certification process.

To ensure a consistent application of these rules, we restrict our analysis to firms with at least three years of available data and, in our main specification, to firms whose year dimension remained unchanged for at least two consecutive years. Although this restriction could raise concerns about selection bias, firms with “unstable” *year dimensions* account for only 3.5% of the sample. Moreover, our results are robust to including these firms, indicating that this restriction does not drive our findings (see Section 8.6).

Program *Linha de Crédito PME Crescimento* Our analysis focuses on the *Linha de Crédito PME Crescimento* program, a public loan guarantee scheme introduced on 16 January 2012 (SPGM, 2012). We study the special regime applicable to firms with micro or small certification, under which the two groups are treated identically except for the maximum guarantee amount. Firms with micro certification are eligible for guarantees of up to €18,750, while

⁷SME certification attests firm size according to the European Commission’s definition (Recommendation 2003/361/EC), which was incorporated into Portuguese law in 2007 and became fully effective by 2012. The Commission’s SME definition was gradually adopted across Europe starting in 2003, followed by a monitoring and adjustment period, and entered into full force in 2012. For a detailed timeline, see European Commission (2019, p. 32).

Firm certification	Value of the guarantee	Value of the bank loan
Micro firm	€18 750	€25 000
Small firm	€37 500	€50 000

Table 2: Maximum value do the guarantee and maximum value of the bank loan in euros.

firms with small certification may obtain guarantees of up to €37,500 (see Table 2). Appendix 3 provides a detailed description of the terms, conditions, and covenants governing guarantees issued under this regime, which we refer to as “SGM guarantees.”

External validity Although our empirical analysis focuses on SGM guarantees in Portugal, we believe our findings generalize to a broad class of public credit guarantee schemes.⁸ Cross-country surveys document substantial commonality in institutional design. Beck, Klapper, and Mendoza (2010) examine 76 credit guarantee organizations across 20 high-income countries, 25 middle-income countries, and India; OECD (2013) reviews more than 29 public schemes across OECD economies; and Chatzouz, Gereben, Lang, and Torfs (2017) survey 18 organizations in 13 European countries. Despite heterogeneity in contractual details, these studies highlight a shared set of core features, including similar coverage ratios, maturities, fee structures, and eligibility criteria. In particular, eligibility is almost universally based on firm size (typically measured by employment) and programs are explicitly targeted at SMEs. Beck, Klapper, and Mendoza (2010) report a median coverage ratio of 80%, close to the 75% coverage ratio in our setting. Taken together, this evidence suggests that the Portuguese scheme provides a representative environment in which to test the liquidity theory of credit guarantees. Accordingly, we view our theoretical implications as extending naturally to public guarantee programs in other institutional contexts.

5 Data and variables

5.1 Data

We use a unique proprietary dataset supplied by *Banco Português de Fomento* with all credit guarantee applications to *Linha de Crédito PME Crescimento* from 16 January 2012 until 14 July 2013. The total number of applications available in the dataset is 41,897. We only use applications from micro and small firms,

⁸Loan guarantees are widespread: Green (2003) documents more than 2,000 schemes operating in nearly 100 countries, and virtually all OECD members maintain some form of such program.

which represent approximately 72.1% of the total number of available guarantees. We have information on whether the guarantee is approved or denied, as well as the stated reason for rejection (the rejection rate is 1.1%). Importantly, these reasons are seldom related to the typical screening criteria used by banks, such as creditworthiness or project risk, and are more often of a bureaucratic nature, including incomplete documentation or failure to meet formal eligibility requirements. For originated guarantees, we have information on the characteristics of the guarantee, such as value of the guarantee, value of the bank loan backed by the guarantee, loan rate, fees, maturity, grace period, date of application and date of approval of guarantee. Moreover, we know if the guarantee has been activated and the recovery value after activation. All characteristics of guarantees are stable during the period of analysis. We also know if the firm obtained other guarantees before or after the 16 January 2012 - 14 July 2013 period, as well as their total value at origination and whether they have been activated or not.

We complement this information with two micro-level databases available at the *BPLIM - Banco de Portugal Microdata Research Laboratory* (BPLIM). The first database is the Central Balance Sheet - Harmonized Panel (CBHP) and contains firm-level annual data from 2006 to 2019 (BPLIM, 2021). This dataset is mostly based on information reported through *Informação Empresarial Simplificada* (IES), whereby firms provide detailed information about the firm such as number of employees, age, industry and information on annual balance sheet and profit and loss accounts. The CBHP covers the universe of non-financial corporations operating in Portugal, but does not include information on individual people (and therefore our sample does not include data on firms that are sole proprietorships or on independent workers; guarantees issued to these economic agents represent 1.75% of guarantees in Portugal according to SPGM 2014). The second database is the Harmonized Central Credit Register (HCRC) which includes information on all credit accounts larger than 50 for Portuguese firms with monthly frequency, since 2009 until 2019 (BPLIM, 2023).

After merging the dataset supplied by *Banco Português de Fomento* with CBHP, we obtain observations on 22,655 guarantees, of which 5,324 do not belong to the special regime applicable to firms with micro or small certification in program *Linha de Crédito PME Crescimento*. We remove guarantees that back loans issued by banks that do not obtain a match on HCRC and on the dataset supplied by *Banco Português de Fomento*, thus removing 947 guarantees (these are likely to be guarantees on loans from banks that merged during the 2012-2016 period). After merging the three datasets, we obtain a sample of 16,180 guarantees and 14,931 firms. We exclude the second guarantee whenever the firm obtains one guarantee in 2012 and one guarantee in 2013, thus obtaining the same number of firms as the number of guarantees. We drop 104 observations on

guarantees that do not fulfill the legal specifications detailed in Appendix 3.

The final sample is an unbalanced panel of 13,758 firms with observations from 2007 until 2016. All firms obtained at least one credit guarantee in 2012 or in 2013. Following standard procedures, we winsorise the top and bottom 1 percentiles (for all variables except the value of the credit guarantee) in each calendar year so as to minimize the effects of outliers.

5.2 Variables

In Appendix 4 we detail the construction of the various variables that we use in the analysis throughout the paper. Here, we provide a brief explanation for some of the variables. We measure firm output as the value added by the firm which, broadly speaking, measures the value of total sales minus the value of intermediate consumption, thus representing the income available for the contributions of labor and capital to the production process. The variable “cash” is defined as the sum of cash holdings and bank deposits. We construct two broader liquidity measures. The first, “liquidity”, augments cash with the contingent payment embedded in the first SGM guarantee. The second, “liquidity (all guarantees)”, extends this measure further by adding the total contingent payment embedded in all outstanding official guarantees received by the firm.

We do not observe the certification status of each firm issued by IAPMEI. We thus reconstruct firm certification following the steps described in Section 4. Since we may erroneously assign the classification of micro to some firms that IAPMEI deems as small, we decided to drop observations from firms that obtain guarantees larger than €18,750 and we have classified as micro.⁹

Table 3 lists key variables used in this study and presents descriptive statistics for each variable based on the entire sample of 13,286 firms for the year before receiving the guarantee (except for the variable “value of guarantee”). The average Portuguese firm is relatively small, hiring less than eight employees on average. Firms hold substantial amounts of cash, around 10% of their total assets, with an average amount around one third of their fixed assets. Portuguese firms rely heavily on debt, with total liabilities representing around 70% of their assets. Firm output represents about one fourth of sales on average.

⁹SME certification requires firms to classify themselves as autonomous, partner, or linked, based on ownership and control relationships. For partner or linked firms, size is determined by consolidating activity across affiliated enterprises (see Appendix 2). As IES (and thus CBHP) does not report this information, our data may misclassify some firms deemed small by IAPMEI as micro firms.

	Mean	P25	Median	P75	Std Dev
Total assets (€)	523 450	153 050	319 308	646 606	633 383
Fixed Assets (€)	153 684	17 061	57 006	169 460	295 245
Number of employees	7.76	3	5	10	7.21
Firm output (€)	157 329	50 598	100 751	205 817	169 201
Sales (€)	605 880	170 200	353 839	743 808	755 450
Exports (€)	43 085	0	0	704	200 517
Cash (€)	46 377	6 697	20 323	51 966	82 347
Value of guarantee (€)	21 562	18 750	18 750	18 750	8 088
Previous guarantees (€)	245	0	0	0	3 139
Total liabilities (€)	362 864	93 632	210 075	445 134	468 389
Current liabilities (€)	251 196	52 791	134 278	312 976	349 294
Subsidies (€)	2 444	0	0	0	32 029

Table 3: **Summary statistics** This table reports summary statistics for the variables used in the analysis for the entire sample of 13,286 firms. This sample accounts for all SME firms. P25 and P75 are the 25th and 75th percentiles, respectively. The reference period is the year before the credit guarantee was granted, except for the value of the guarantee, and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

The variable “previous guarantees” measures the total value of the contingent payments embedded in all outstanding official guarantees received by the firm, and suggests that firms in our sample received few guarantees before 2012. The variable “value of guarantee” measures the value of the SGM guarantee, and suggests that firms with micro certification are likely to represent more than 75% of our sample of 13,286 firms.

The differences between mean and median in Table 3 suggest skewed distributions for most variables, and therefore recommend distinguishing between average values and the values for the average firm. We conduct the analysis in changes, and we perform the analysis in terms of conditional expectations (following Angrist and Pischke, 2009). A key concern in our analysis is that the value of credit guarantees is correlated with subsidies, and Table 3 suggests caution. Our results would be biased if subsidies targeted small firms.

6 Methodology

We study the effects of credit guarantees by comparing outcomes across two groups of firms: a treated group with access to guarantees of €37,500 and a control group with access to guarantees of €18,750. Accordingly, we construct a binary treatment indicator $Treated_i$ which equals one for firms with access to large guarantees and zero for firms with access to small guarantees.

We begin by presenting the empirical specification and then discuss the identification strategy, which exploits the assignment mechanism of the Portuguese credit guarantee program, and the procedures used to ensure comparability between treated and control firms.

6.1 Empirical specification

Our baseline specification is an instrumental variables (IV) regression, in which we use certification as an instrument for the actual value of the guarantee received. Specifically, we implement a two-stage least squares (2SLS) procedure on a pooled panel, in which the first-stage equation is

$$G_i = \alpha_1 + \beta \times Treated_i + \gamma_1 X_i + \epsilon_{1,i} \quad (9)$$

where i indexes firms/guarantees, G_i denotes the value of the guarantee received by firm i , $Treated_i$ is the treatment variable, and X_i is a vector of firm-level control variables. Control variables include number of employees, total assets, sales, fixed assets, cash, leverage, exports, and industry fixed effects (industry dummies), and are measured the year before the firm applies for the guarantee.¹⁰

In the second stage, we regress the change in the outcome variable over time on the instrumented value of the guarantee. To account for differences in initial levels across firms, we estimate regressions using changes in firm-level outcome variables rather than levels

$$y_{i,t} - y_{i,0} = \alpha_2 + \delta \widehat{G}_i + \gamma_2 X_i + \epsilon_{2,i} \quad (10)$$

where y_{it} is the value of the outcome variable in year $t \in \{2, 5\}$, and $y_{i,0}$ is the pre-treatment baseline (the guarantee is issued during year 1). The covariance between $\epsilon_{1,i}$ and $\epsilon_{2,i}$ may be different from zero, and $E[\epsilon_{2,i}|X_i, Treated_i] = E[\epsilon_{2,i}|X_i] = 0$. The coefficient δ captures the causal effect of the guarantee on the outcome of interest, conditional on observables. We report standard errors that are robust to heteroskedasticity. If firm i exits the market at date τ , then we either set the dependent variable $y_{i,t}$ equal to zero for $t \geq \tau$ if the firm activates the guarantee, or we set the dependent variable $y_{i,t}$ equal to the last available value for $t \geq \tau$ if the firm does not activate the guarantee.¹¹ We avoid “log-like” transformations to avoid measurement problems (McConnell 2014, Ciani and Fisher

¹⁰We build industry dummies based on the two-digit level of the CAE industry classification. CAE stands for classification of economic activities in Portugal, and it is similar in function to NAICS and SIC. We use the third revision, made in 2007, in which the two-digit level classification contains 89 industries.

¹¹We also considered the following approach: If firm i exits the market at date τ , then we set the dependent variable y_{it} equal to zero for $t \geq \tau$. The results from the two approaches are similar.

2019, and Chen and Roth 2024). Recall from Section 3.4, we are interested in the marginal effect of the loan guarantee.

We complement our analysis by applying a dynamic version of the differences-in-differences estimator to exploit the rich time-series variation in the data. For a credit guarantee issued during year 1 and for variables with available data since year -3 , we estimate by OLS regression models of the form

$$y_{i,t} - y_{i,0} = \sum_{k=-3, k \neq 0}^5 \delta_k \times Treated_i \times Year_t + \eta_t + \varepsilon_{i,t} \quad (11)$$

where $t \in \{-3, \dots, 5\}$ indexes year, $Year_t$ are year dummies, and η_t are year fixed effects. The reference year is 0, we estimate the δ_k 's and the η_t 's, and the coefficients of interest are the δ_k 's. Standard errors are heteroscedasticity-consistent and clustered at the firm level, following Bertrand, Duflo, and Mullainathan (2004). A key assumption for the differences-in-differences specification is that treated and control groups would have behaved similarly before the guarantees were awarded—aka parallel trends. With Specification (11) it is possible to test for parallel trends by comparing the values of the coefficients δ_k before year 1. By way of preview, the liquidity-to-output ratios of both groups behave similarly before treatment, thereby supporting our identification strategy.

6.2 The exclusion restriction

The exclusion restriction underlying our IV strategy requires that SME certification is not correlated with firms' investment opportunities. We support this assumption using several institutional and design features. First, we restrict the analysis to firms that both applied for and received a loan guarantee, ensuring that all firms in the sample revealed demand for guaranteed credit. This restriction avoids the common but flawed comparison between firms with and without guarantees, where the latter are likely to lack attractive investment opportunities.

Second, certification is determined by a score (constructed from the number of employees, total assets, and sales) and a deterministic cutoff rule. This rule generates discrete variation in the maximum guarantee amount: firms with scores below the cutoff are eligible for guarantees of up to €18,750, whereas firms above the cutoff may obtain guarantees of up to €37,500. We use this discontinuity to construct an instrument for guarantee size. Firms with scores lying in a narrow neighborhood around the cutoff are comparable in observable and unobservable characteristics, including investment opportunities, but differ in their maximum eligible guarantee. Identification therefore comes from comparing changes in scale

and liquidity among firms with similar pre-treatment scores (i.e. similar characteristics, including similar size) but differential access to guaranteed credit. We assume that firms with scores just above or below the cutoff are virtually indistinguishable in terms of investment opportunities and, consistent with this assumption, placebo tests based on alternative cutoffs reveal no systematic differences in outcomes.

Finally, strategic manipulation of certification is unlikely for three reasons. First, the economic incentives to manipulate the score are weak. The cost of altering certification (approximately €33,700, corresponding to the minimum-wage cost of hiring an additional worker for four years) exceeds the maximum increase in the guarantee cap (€18,750) associated with a change in certification status.¹² Second, certification is predetermined and remains fixed for at least one year prior to the application for a loan guarantee. Certification is therefore assigned before the investment opportunities that materialize in the year preceding the application can arise. Because firms cannot retroactively adjust their certification status, the instrument is plausibly independent of investment decisions at the time of application. Third, institutional features further limit firms' ability to act strategically. SME certification was introduced only in 2009, eligibility criteria for the guarantees analyzed in this paper were announced shortly before implementation. Moreover, guarantee coverage rates prior to 2012 were only 50% of the loan amount. Together, these features substantially reduce both the scope and the expected payoff from manipulating certification status.¹³

6.3 Rationing of high investment opportunities

Theory predicts differences in outcome variables between treated and control firms only when the control group is constrained in its demand for liquidity. In our setting, firms with micro certification can access guarantees up to €18,750, whereas firms with small certification can obtain guarantees up to €37,500. Table 4 reports firm characteristics for treated and control firms in a window around the certification cutoff.

The evidence in Table 4 suggests that most micro-certified firms request the maximum admissible guarantee of €18,750. Nonetheless, a non-negligible subset of firms in our dataset receives guarantees below this threshold. These firms are

¹²Firms must also satisfy additional conditions to qualify for SGM guarantees, including the commitment to maintain employment throughout the duration of the guarantee (see Appendix 2).

¹³The aggregate demand for new guarantees was broadly stable in 2011 and 2012 (SPGM, 2013), suggesting that firms did not strategically postpone applications to benefit from more favorable conditions.

	Mean Treated	Mean Control	Test of differences t-statistic	Test of differences p-value
Total assets (€)	808 105	529 107	-10.23	0.00
Fixed Assets (€)	241 217	141 344	-7.58	0.00
Number of employees	10.89	7.96	-36.93	0.00
Firm output (€)	234 215	158 482	-15.03	0.00
Sales (€)	926 078	630 812	-8.10	0.00
Exports (€)	82 589	28 244	-3.82	0.00
Cash (€)	65 540	48 887	-3.47	0.00
Value of guarantee (€)	28 252	18 291	-29.90	0.00
Previous guarantees (€)	248	165	-0.72	0.47
Total liabilities (€)	560 583	358 964	-9.76	0.00
Current liabilities (€)	380 805	257 968	-7.59	0.00
Subsidies (€)	2 576	5 264	1.07	0.29

Table 4: **Summary statistics for the treated and control groups for all firms** This table reports means for some variables used in the analysis, and includes all firms with score around the cutoff (approximately 8-11 employees). The treated group contains 849 firms and the control group includes 736 firms. The reference period is the year before the credit guarantee was granted, except for the value of the guarantee, and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

unlikely to be liquidity-rationed at €18,750 and therefore fall outside the scope of the model’s predictions, which apply to firms with sufficiently large investment opportunities to demand guarantees above the €18,750 cap.

Accordingly, we exclude from the baseline sample all firms that obtain guarantees below €18,750, removing 1,462 firms out of a total of 13,286 firms . Moreover, our empirical design compares micro-certified firms that are constrained at the €18,750 ceiling with small-certified firms whose investment opportunities require guarantees above that level. We therefore also exclude 1,096 small-certified firms that received exactly €18,750 (again out of 13,286 firms), since these firms would not have been rationed had they held micro certification. A potential concern is that this classification may misallocate some firms and thereby introduce measurement error. To address this issue, the robustness section reassigns these 1,096 firms to the micro-certified group, and the results remain qualitatively unchanged. Table 5 compares the treated and the control groups for the resulting sample of firms with scores close to the certification cutoff and with high investment opportunities, that is, firms for whom the liquidity constraint at €18,750 is economically relevant.

	Mean Treated	Mean Control	Test of differences t-statistic	Test of differences p-value
Total assets (€)	876 125	546 803	-10.85	0.00
Fixed Assets (€)	262 800	144 488	-8.01	0.00
Number of employees	11.14	8.00	-36.25	0.00
Firm output (€)	258 123	160 506	-16.46	0.00
Sales (€)	1 043 755	646 803	-9.17	0.00
Exports (€)	82 434	28 164	-4.27	0.00
Cash (€)	68 788	48 642	-4.04	0.00
Value of guarantee (€)	36 640	18 750	-115.31	0.00
Previous guarantees (€)	386	142	-1.22	0.22
Total liabilities (€)	607 801	370 490	-10.36	0.00
Current liabilities (€)	416 520	262 694	-5.87	0.00
Subsidies (€)	1 215	1 239	0.07	0.95

Table 5: **Summary statistics for the treated and control groups for firms with high investment opportunities** This table reports means for some variables used in the analysis, and includes firms with score around the cutoff (approximately 8-11 employees). We exclude from the sample micro firms that obtained guarantees lower than €18750 euros and small firms that obtained guarantees lower or equal to €18750 euros. The treated group contains 437 firms and the control group includes 686 firms. The reference period is the year before the credit guarantee was granted, except for the value of the guarantee, and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

6.4 Treated and control groups: descriptive statistics

We perform matching to ensure that the control group provides a credible counterfactual. Selecting “high-quality” matches is particularly important in our dynamic difference-in-differences framework, where outcome variables are expressed in changes and subsume the effect of control variables.

To construct comparable groups, we sort firms into bins based on characteristics measured on the year prior to treatment. We consider six dimensions: average three-year growth in employees, total assets, sales, and fixed assets; leverage; export status. For each growth variable, we form two bins: positive versus negative growth. For leverage, we define four bins using the 25th, 50th, and 75th percentiles. Finally, we create two bins distinguishing exporters from non-exporters.

We then implement coarsened exact matching (CEM) based on these bins. CEM temporarily coarsens the data and performs exact matching within the defined strata. We apply k-to-k matching as proposed by Blackwell, Iacus, King, and Porro (2009), ensuring that each treated firm is matched to a control firm within the same bin structure.

After this matching process, we dropped observations on treated firms that do

	Mean Treated	Mean Control	Test of differences p-value
Total assets (€)	894 182	567 850	0.00
Fixed Assets (€)	262 602	144 485	0.00
Number of employees	10.57	8.24	0.00
Firm output (€)	243 891	162 999	0.00
Sales (€)	1 078 733	661 644	0.00
Exports (€)	80 926	38 851	0.00
Cash (€)	70 497	50 079	0.00
Value of guarantee (€)	36 617	18 750	0.00
Previous guarantees (€)	273	168	0.00
Total liabilities (€)	619 070	380 320	0.00
Current liabilities (€)	422 352	269 382	0.00
Subsidies (€)	821	838	0.00

Table 6: **Summary statistics for the treated and control groups for firms with high investment opportunities (with matching)** This table reports means for some variables used in the analysis, and includes firms with score around the cutoff (approximately 8-11 employees). We exclude from the sample micro firms that obtained guarantees lower than €18 750 and small firms that obtained guarantees lower or equal to €18 750. The treated group contains 317 firms and the control group includes 317 firms. We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The reference period is the year before the credit guarantee was granted, except for the value of the guarantee, and the sources are *Banco Português de Fomento* and *Banco de Portugal*. We do not report the t-statistic for for the test of differences because the numbers are large.

not obtain a good match in the control group, thereby obtaining a final sample of 317 treated firms. Table 6 provides sample means of the main variables for the treated and the control groups, as well as difference-in-means tests. The results are fairly comparable with the results without matching reported in Table 5.

Despite restricting attention to a narrow window around the score cutoff and applying matching procedures, Table 6 reveals differences in observable characteristics between treated and control firms. These imbalances arise because one component of the score variable, the number of employees, takes discrete values. As a result, firms located just below and just above the cutoff may differ mechanically in size. Although discreteness does not pose methodological problems for regression discontinuity designs (Lee and Card, 2008), it raises the concern that firms on either side of the threshold may differ in size. In particular, the number of employees is the most binding criterion for SME certification, and a firm with 10 workers is approximately 10 percent larger than a firm with 9 workers, implying a non-negligible difference in size at the cutoff.

Concerns about these differences between treated and control firms are mitigated for two reasons. First, the sample used in Table 6 approximately defines

the control group as firms with 8 or 9 employees and the treated group as firms with 10 or 11 employees. This four-employee window implies larger differences in size than the narrower 9-to-10 employee comparison at the certification cutoff. Nevertheless, the estimated treatment effects are very similar across the two samples. We therefore retain the wider window, as it yields more precise estimates with smaller standard errors.

Second, although treated and control firms differ in variables measured in levels, they are highly comparable in financial ratios and, most importantly, in their liquidity-to-scale ratios, which are the primary outcomes of interest in our analysis. Consistent with our theoretical framework, differences in scale or level variables are not informative for identifying the liquidity channel emphasized in this paper.

A remaining concern is that differences in firm size between treated and control firms may be correlated with differences in investment opportunities. In our setting, however, this concern is unlikely to be quantitatively important for four reasons. First, documented correlations between firm size and productivity typically arise over large differences in scale—for example, between small and large firms. The existing literature does not identify systematic productivity differences across firms that differ by one to three employees, as in our sample. Second, placebo tests based on alternative certification cutoffs yield no systematic differences in outcomes, suggesting that firms on either side of the threshold do not differ meaningfully in underlying productivity or investment opportunities. Third, our empirical specifications are estimated in changes, akin to a first-differencing estimator, which absorbs all time-invariant sources of heterogeneity in investment opportunities across firms, whether observed or unobserved. Consequently, any size-related differences in investment opportunities that are constant over time are differenced out. The validity of the pre-treatment parallel trends assumption further supports the conclusion that differences in firm size are unlikely to account for our findings. Fourth, our regressions include controls for multiple measures of firm size and performance, further mitigating concerns that residual differences in scale drive our results.

7 Empirical results

In this section, we test the two empirical implications derived in Section 3.5. The first concerns the allocation of resources between scale and liquidity in order to assess the relative importance of the conventional collateral channel relative to the liquidity insurance channel. The second implication concerns the relationship

between guarantee size and guarantee activation.

Our empirical analysis proceeds in three steps. We first present quantitative estimates from the first- and second-stage regressions of our 2SLS approach. We then complement these estimates with graphical evidence based on a differences-in-differences approach. The results obtained from these two approaches are broadly consistent, supporting the robustness of our findings. Finally, we report the results on the frequency of guarantee activation.

7.1 Instrumental variables (2SLS)

7.1.1 First-stage regression

We adopt an instrumental variables approach, and a natural concern is that our “constructed” certification instrument may not replicate the official certification process described in Section 4, and thus might be weakly correlated with guarantee values.

Table 7 reports the coefficients from Specification (9), where the value of the guarantee is regressed on certification and a set of firm-level controls measured prior to treatment. The estimates differ across samples, with each column reporting results for a distinct subsample: all firms, firms that did not activate their guarantees, and firms that did.

The results indicate a sharp discontinuity in the value of the guarantee at the certification threshold. Certification strongly predicts guarantee size, while the coefficients on most controls are close to zero, with the exception of leverage. The adjusted R-squared values confirm that certification accounts for nearly all variation in guarantee size, a convenient feature for our IV design. These findings provide strong support for the view that our constructed certification instrument accurately replicates the official certification process, thereby helping to validate our identification strategy.

Insurance and adverse selection We examine whether adverse selection affects the demand for loan guarantees (we analyze moral hazard below). We present two pieces of evidence suggesting that adverse selection is not a first-order concern in our setting.

First, Columns (2) and (3) of Table 7 report results for the subsamples of firms that do not and do activate their guarantees ex post. Although only 46 firms ultimately trigger the guarantee, we find no economically meaningful differences

	(1) Value of guarantee (all firms)	(2) Value of guarantee (firms did not activate)	(3) Value of guarantee (firms activated)
$Treated_i$	17 854.9*** (285.84)	18 047.09*** (330.01)	17 859.89*** (2756.87)
Employees	-25.02 (109.57)	-91.86 (110.00)	-34.80 (981.60)
Total Assets	-0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)
Sales	0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)
Fixed Assets	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Cash	0.00 (0.00)	0.00 (0.00)	0.01 (0.02)
Leverage	898.56 (568.257)	1 251.40 (568.72)**	-2 649.76 (5165.91)
Exports	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Industry fixed effects	Yes	Yes	Yes
Adjusted R-squared	0.933	0.938	0.837
Observations	634	588	46

Table 7: **First-stage regressions** This table reports estimates of coefficient β associated with the variable $Treated_i$ in Specification (9). The regressor $Treated_i$ is an indicator variable that equals 1 if and only if the firm has access to a large guarantee. Robust standard errors are in parentheses and *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Column (1) reports results for all firms, column (2) reports results for the subsample of firms that did not activate their guarantees, and column (3) reports results for the subsample of firms that activated their guarantees. All columns report results for firms with score around the cutoff (approximately 8-11 employees). In column (1), we perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sources are *Banco Português de Fomento* and *Banco de Portugal*.

in ex ante guarantee demand between activating and non-activating firms. In particular, firms that later activate the guarantee do not request systematically larger guarantees at origination. The absence of differential demand across these groups suggests that firms with higher ex ante risk are not disproportionately selecting into larger coverage.

Second, as documented in Section 6.3, some firms request guarantees lower than €18,750. This pattern indicates that firms do not mechanically choose the highest available coverage. One plausible interpretation is that banks screen borrowers and restrict guarantee size for riskier firms, thereby attenuating potential adverse selection at the application stage.

Taken together, these findings point to little evidence of adverse selection in the demand for loan guarantees. Coverage choices appear broadly similar across firms that do and do not experience adverse outcomes, suggesting that higher guarantee limits do not, by themselves, attract a disproportionate share of ex ante “bad risks.”

That said, our data do not allow us to directly test for strategic selection by banks, as we lack bank-level identifiers. In principle, banks could steer riskier borrowers toward guaranteed credit while retaining safer firms on non-guaranteed loans. While this channel cannot be ruled out, the available evidence does not strongly support it as a dominant force in our setting.

7.1.2 Second-stage regression

We regress the outcome variables on the predicted value of the guarantee from the first-stage, together with the same set of exogenous controls, in order to obtain consistent estimates of the causal effect of guarantees. Tables 8 and 9 report the coefficients from Specification (10). Table 8 focuses on short-term effects within a two-year horizon, while Table 9 reports longer-term effects over a five-year horizon. Recall that guarantees expire after four years.

In the short run, the results in Table 8 broadly confirm earlier findings in the empirical literature. The estimates of the coefficients on firm output and fixed assets are statistically insignificant. By contrast, the effects on liquidity are both economically and statistically significant. A one-euro increase in the guarantee raises our main liquidity measure (cash plus the contingent payment embedded in the SGM guarantee) by €0.94. The effect is likely to be less than one because firms partially amortize their loans within two years.

Consistent with these results, the average difference between the liquidity-to-

	(1)	(2)	(3)	(4)
	Firm output	Fixed assets	Cash	Liquidity
Value of guarantee	-0.402 (0.577)	-0.237 (0.710)	0.314 (0.448)	0.941** (0.451)
Employees	2 750.523 (3 391.677)	681.637 (4 175.682)	-580.073 (2 634.557)	-528.507 (2 651.948)
Total Assets	-0.016* (0.009)	0.024** (0.011)	0.002 (0.007)	0.001 (0.007)
Sales	0.004 (0.005)	0.010 (0.006)	0.003 (0.004)	0.003 (0.004)
Fixed assets	0.022 (0.017)	-0.091*** (0.021)	-0.006 (0.013)	-0.005 (0.013)
Cash	0.016 (0.039)	0.019 (0.048)	-0.158*** (0.030)	-0.150*** (0.031)
Leverage	-30 074.965* (17 479.948)	-14 000.101 (21 520.534)	-15 333.124 (13 577.922)	-16 392.064 (13 667.548)
Exports	0.004 (0.014)	0.008 (0.017)	0.021* (0.011)	0.022** (0.011)
Industry fixed effects	Yes	Yes	Yes	Yes
Observations	634	634	634	634

Table 8: **Second-stage regressions (two years)** This table reports estimates of coefficients δ associated with the instrumented value of the guarantee $\hat{G}_i$ in Specification (10) for $t = 2$ and for a range of outcome variables. Robust standard errors are in parentheses and *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. All columns report results for firms with score around the cutoff (approximately 8-11 employees). We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sources are *Banco Português de Fomento* and *Banco de Portugal*.

output ratios of the treated and of the control groups increases in year 2 ; the estimate of this difference has a p-value equal to 7.7% (results not shown). This finding is in line with Implication 1, and supports the liquidity channel at the expense of the conventional collateral channel. However, the liquidity-to-fixed-assets ratio is not statistically different from zero (results not shown) but we have argued that fixed assets are not a good proxy for scale. Taken together, the evidence offers partial support for Implication 1: larger guarantees shift capital allocation towards liquidity, raising the firm's liquidity-to-scale ratio.

Turning to the long run, Table 9 shows no statistically significant effects of guarantees at a five-year horizon. While both scale and liquidity measures increase somewhat for treated firms relative to controls, the estimates lack precision. From the perspective of the liquidity channel, this pattern is natural: the liquidity effects dissipate once the guarantee expires. Indeed, because SGM guarantees have four-year maturity, the liquidity measure converges to the value of

	(1)	(2)	(3)	(4)
	Firm output	Fixed assets	Cash	Liquidity
Value of guarantee	-0.715 (1.017)	-0.792 (1.636)	-0.728 (0.748)	-0.728 (0.748)
Employees	6 309.340 (5 985.931)	13 476.603 (9 624.216)	9 007.351** (4 401.301)	9 007.351** (4 401.301)
Total Assets	-0.044*** (0.015)	0.025 (0.025)	-0.010 (0.011)	-0.010 (0.011)
Sales	0.002 (0.009)	0.017 (0.015)	0.007 (0.007)	0.007 (0.007)
Fixed assets	0.049 (0.030)	-0.115** (0.048)	0.023 (0.022)	0.023 (0.022)
Cash	0.127* (0.069)	0.389*** (0.111)	-0.249*** (0.051)	-0.249*** (0.051)
Leverage	-19 153.031 (30 850.154)	-24 667.483 (49 601.067)	-56 608.619** (22 683.323)	-56 608.619** (22 683.323)
Exports	0.061** (0.025)	-0.010 (0.040)	0.035* (0.018)	0.035* (0.018)
Industry fixed effects	Yes	Yes	Yes	Yes
Observations	634	634	634	634

Table 9: **Second-stage regressions (five years)** This table reports estimates of coefficients δ associated with the instrumented value of the guarantee $\widehat{G}_i$ in Specification (10) for $t = 5$ and for a range of outcome variables. Robust standard errors are in parentheses and *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. All columns report results for firms with score around the cutoff (approximately 8-11 employees). We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sources are *Banco Português de Fomento* and *Banco de Portugal*.

cash once the guarantee ends, explaining why the estimates in Columns (3) and (4) are identical.

7.2 Differences-in-differences approach

We complement the baseline analysis with a dynamic differences-in-differences specification. This approach parallels the reduced form of the instrumental variables approach, as we regress the outcome variables directly on the instrument certification. By way of preview, the results on the dynamic differences-in-differences approach broadly confirm the results of the 2SLS approach, but provide additional insight into the evolution of the variables.

The figures in this section plot the estimated coefficients δ_k from Specification (11), which trace the evolution of scale and liquidity relative to the year preceding

the guarantee (normalized to zero). We report point estimates together with 90% and 95% confidence intervals to assess both the magnitude and the precision of the effects over time.

7.2.1 Scale

Panel (a) of Figure 2 plots the estimated coefficients δ_k for firm output. The evolution of output prior to the guarantee is broadly parallel across treated and control firms, lending support to the parallel-trends assumption. The estimates suggest that guarantees have little impact on output after the guarantee is granted.

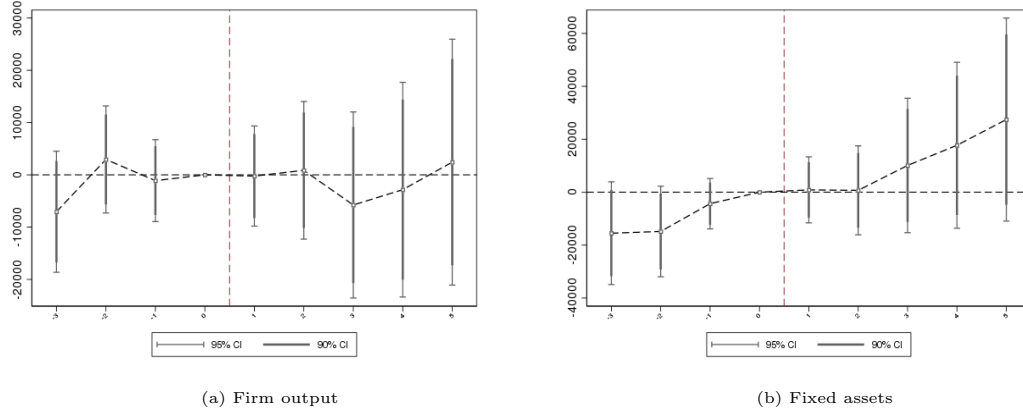


Figure 2: **Scale variables** This figure plots the point estimates and the 90% and 95% confidence intervals for the coefficients δ_k associated with the variable $Treated_i$ in Specification (11) for firm output and fixed assets. The horizontal axis displays time in years around the origination date of the guarantee (represented by the vertical dashed line) with year 0 representing the year prior to origination. The units on the vertical axis measure differences in euros between the treated and the control groups. The treated group contains firms with access to guarantees equal to €37,500 and the control group contains firms with access to guarantees equal to €18,750. All panels report estimates for firms with score around the cutoff (approximately 8-11 employees). We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sample period is 2008 to 2017 and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

Panel (b) of Figure 2 present analogous estimates for fixed assets. Here the pre-trend differences between treated and control firms are more pronounced, complicating causal interpretation. While the gap between treated and control firms widens after the guarantee is granted, this increase may simply reflect a continuation of pre-existing trends rather than a causal effect.

Composition effects As in much of the existing literature, identifying scale effects is challenging. Importantly, however, the absence of a short-run increase in output does not imply that loan guarantees are economically inconsequential. Rather than expanding the scale of activity, guarantees may primarily affect the composition of firms' activities.

By delivering support when the firm is under financial stress, loan guarantees can relax downside risk and thereby enable firms to undertake riskier, higher-value projects that involve substantial sunk costs. Such projects, most notably innovation or internationalization, require large upfront investments and expose firms to cash-flow risk well before revenues materialize. Exporting, for example, entails fixed costs related to market entry, product adaptation, distribution networks, and regulatory compliance, while subjecting firms to heightened liquidity risk during the ramp-up phase (Melitz, 2003).

Consistent with this mechanism, Custódio, Hansman, and Mendes (2024), using an empirical design closely related to Bonfim, Custódio, and Raposo (2023), document a positive effect of loan guarantees on firms’ export activity. Our results align with this view: guarantees appear to matter less as “credit for expansion” and more as “insurance that renders otherwise difficult projects viable”. In this sense, guarantees reshape firms’ project choice rather than mechanically increasing output in the short run.

7.2.2 Liquidity

Panel (a) of Figure 3 plots the estimated coefficients δ_k for cash holdings. While trends are not perfectly parallel prior to the guarantee, the average difference in cash holdings between treated and control firms do not seem to decrease (on average) following guarantee issuance. This pattern provides little evidence of substitutability between loan guarantees and cash. A natural explanation is that activating a guarantee is costly for firms, making guarantees an imperfect substitute for internal liquidity. These costs resemble a deductible: guarantee activation may limit the firm’s future access to guarantees and can trigger additional bank-imposed measures, since 25% of the underlying loan becomes delinquent for the originating bank. As a result, firms may optimally continue to hold precautionary cash even after obtaining a guarantee, rather than replacing cash with guaranteed credit.

Cash is a central component of all our liquidity measures, and the presence of differential pre-trends prior to guarantee issuance complicates the interpretation of other liquidity outcomes. Panel (b) of Figure 3 reports the corresponding estimates for overall liquidity. Although parallel pre-trends cannot be firmly established, the estimates display a sharp and economically meaningful increase in liquidity for treated firms (when compared with control firms) immediately following guarantee issuance. This short-run response is consistent with a liquidity effect of guarantees, but, given the pre-trend patterns, we interpret the magnitude of the estimated effect with caution.

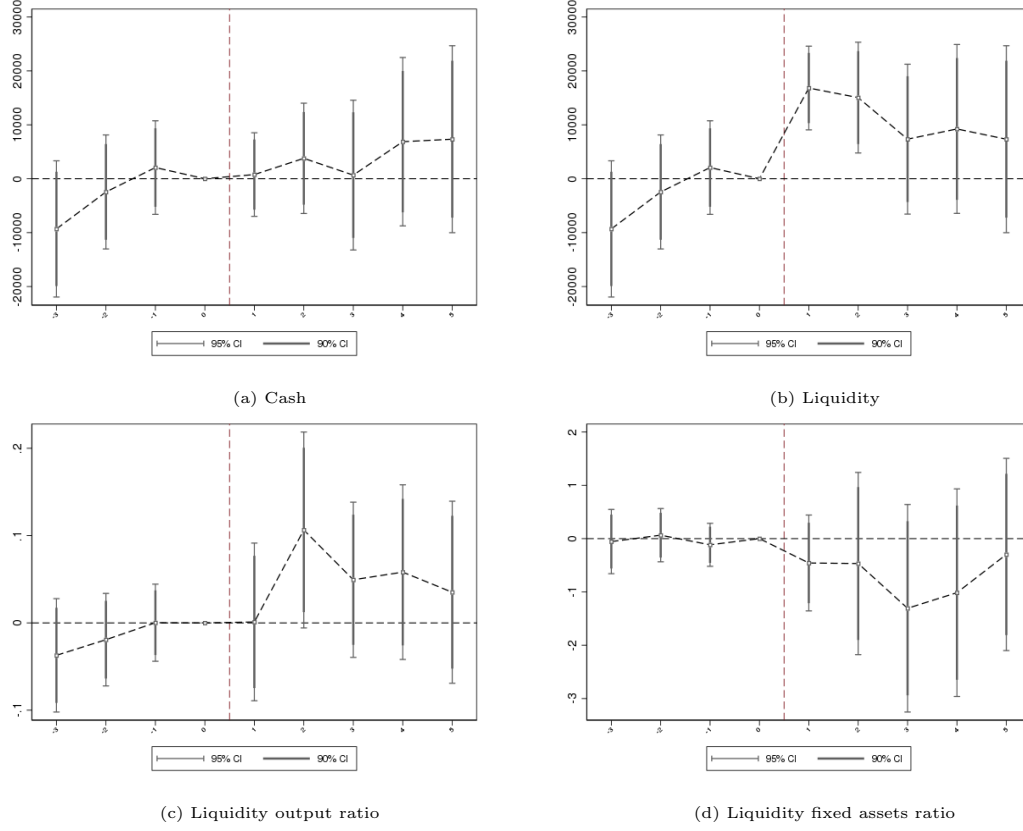


Figure 3: **Liquidity variables** This figure plots the point estimates and the 90% and 95% confidence intervals for the coefficients δ_k associated with the variable $Treated_i$ in Specification (11) for several dependent variables. The horizontal axis displays time in years around the origination date of the guarantee (represented by the vertical dashed line) with year 0 representing the year prior to origination. The units on the vertical axis measure differences in euros between the treated and the control groups. The treated group contains firms with access to guarantees equal to €37,500 and the control group contains firms with access to guarantees equal to €18,750. All panels report estimates for firms with score around the cutoff (approximately 8-11 employees). We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and on leverage and exporter status. The sample period is 2008 to 2017 and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

Having examined liquidity and scale separately, we next consider their ratio, which provides a direct test of Implication 1. Panel (c) of Figure 3 plots the coefficients for the liquidity-to-output ratio. In this case, the parallel-trends assumption cannot be rejected. The estimates reveal a sizable and persistent increase in the liquidity-to-output ratio for treated firms relative to controls in the two years following guarantee issuance. Quantitatively, the difference in ratios rises to 0.106 by year two, before gradually converging toward the cash benchmark as the guarantee approaches maturity. This dynamic pattern is consistent with Implication 1: larger guarantees increase firms’ liquidity relative to scale.

By contrast, the evidence is weaker when fixed assets are used as the measure of scale. Panel (d) of Figure 3 plots the evolution of the liquidity-to-fixed-assets ratio and shows a relative decline for treated firms following guarantee issuance. This pattern stands in contrast to the results based on output and likely reflects the limitations of fixed assets as a proxy for firm scale in our setting. Fixed assets may be measured with substantial noise, may not capture changes in intangible capital, and may not be a good proxy for scale, as discussed earlier.

7.3 Insurance and moral hazard

We do not observe exogenous ex post liquidity shocks directly. Instead, in our setting we observe the endogenous activation of loan guarantees, which is potentially subject to moral hazard. As discussed above, the insurance-based framework predicts a positive relationship between the size of coverage and the probability of claims: larger guarantees should be associated with higher activation rates. On the one hand, our earlier results provide little evidence of adverse selection in guarantee demand. On the other hand, conditional on selection, larger guarantees may alter firms’ incentives by encouraging greater risk-taking, implying higher activation rates through moral hazard. We now test this implication empirically.

Table 10 reports results on guarantee activation and firm exit. Two important measurement issues warrant emphasis. First, guarantee activation is a relatively rare event in our baseline sample, which focuses on firms with 8–11 employees. To improve statistical power, we therefore also present results for an expanded sample that includes firms with 5–14 employees. Second, we do not directly observe firm default. Instead, we use a conservative proxy that classifies a firm as having defaulted only if it both activates a guarantee and subsequently exits the market. For completeness, we also report exit rates for firms that never activate their guarantees. Exits among these firms should not be interpreted as defaults; these exits are more likely to reflect mergers, acquisitions, or voluntary closures

Year of activation	Wide score ($\approx$ firms with 5-14 employees)		Score around the cutoff ($\approx$ firms with 8-11 employees)	
	Control firms	Treated firm	Control firms	Treated firm
$t = 1$	38	31	4	10
$t = 2$	60	17	5	7
$t = 3$	47	22	5	5
$t = 4$	24	11	4	6
Activation (total)	169	81	18	28
Activation and missing	99	52	9	18
No. activation (total)	2 688	880	299	289
No. activation and missing	81	15	11	5
Share of activations (%)	5.92	8.43	5.68	8.83

Table 10: **Number of firms activating their credit guarantees** This table reports the number of activations for each year after the guarantee is granted among treated and control firms for two different samples. The columns with wide score report firms with score in a large interval (approximately 5-14 employees). The columns with score around the cutoff report results for firms with approximately 8-11 employees. We also report the number of firms that exited the market. Firms that activated their guarantees and exited the market are likely to have defaulted. The sources are *Banco Português de Fomento* and *Banco de Portugal*.

rather than financial distress.

With these caveats in mind, the evidence indicates that treated firms activate their guarantees more frequently than control firms. In the expanded sample, the activation rate is 8.43% for treated firms, compared with 5.92% for controls; in the baseline sample, the corresponding figures are 8.83% and 5.68%. Given that treated and control firms are balanced along observable and nonobservable characteristics, these differences are unlikely to be driven by systematically different exposure to exogenous cost overruns. Instead, they suggest that treated firms make greater use of the insurance embedded in the guarantee program. This finding is consistent with the evidence reported by Cowan, Drexler, and Yañez (2015) for a loan guarantee scheme in Chile.

At the same time, a nontrivial fraction of firms that activate their guarantees subsequently exit the market and therefore likely experience default under our proxy. For these firms, the guarantee does not appear to prevent failure ex post. By contrast, exit rates are substantially lower among firms that never activate their guarantees, consistent with the interpretation that most such exits reflect strategic decisions, such as mergers, acquisitions, or voluntary closures, rather than distress-driven default.

In sum, treated firms in our sample activate guarantees more frequently than control firms, but this increased use of insurance does not translate into lower de-

fault rates *ex post*. While these results are based on relatively small samples and should be interpreted with caution, they closely mirror the patterns documented by Cowan, Drexler, and Yañez (2015), lending external validity to our findings.

Taken together, these results on guarantee activation and firm exit/default are consistent with the presence of moral hazard, or more broadly with changes in firms' risk-taking incentives induced by guarantee coverage. The higher activation rates among treated firms indicate that larger guarantees are associated with greater realized risk. Importantly, this pattern does not necessarily imply inefficient or socially undesirable behavior. It may reflect efficient risk-taking (such as entry into export markets or investment in innovation) where a higher probability of failure is an inherent feature of value-enhancing activities. Alternatively, it may reflect reduced effort or weaker repayment discipline at the firm level, or attenuated monitoring incentives at the bank level when a larger share of losses is insured.

From a policy perspective, an insurance-based view of loan guarantees implies that some increase in activations is an expected and, to some extent, desirable outcome. The relevant policy question is therefore not whether activations rise, but whether the induced risk-taking is productive and aligned with policy objectives, and whether program design and monitoring mechanisms appropriately balance risk-sharing with incentive provision. For completeness, Appendix 5 plots the evolution of some outcome variables for firms activating their guarantees and for firms not activating their guarantees.

8 Robustness

We estimate regressions in changes, akin to a first-differencing estimator, which eliminates all time-invariant firm-specific characteristics, including persistent unobserved investment opportunities. Our identifying assumption is that SME certification is not correlated with unobserved within-firm changes in investment opportunities after certification is assigned.

We conduct several additional tests to address concerns that our results may be driven by confounding factors. Specifically, we show that (i) our main results are robust to alternative measures of liquidity; (ii) SME certification is not correlated with omitted confounders, such as government subsidies; (iii) our main results continue to hold when we control by industry-by-time fixed effects; (iv) placebo tests based on nonexistent certification cutoffs in the score variable yield no effects; and (v) similar results obtain across alternative samples.

8.1 Other measures of liquidity

In this section, we assess the evolution of two other measures of liquidity. First, we examine a broader measure of liquidity that includes all official guarantees received by firms. Second, we look into working capital as an alternative measure of liquidity.

Liquidity (all guarantees) Firms in our sample are free to apply for multiple guarantees over time. First, some firms may have obtained guarantees under earlier programs. Second, firms can apply for general guarantees that are not specifically targeted at micro or small firms, although these typically offer less favorable terms (such as lower coverage ratios; see Section 4). Third, firms may reapply in subsequent years for guarantees explicitly designed for micro and small firms. The conditions of SGM guarantees remained stable throughout 2012–2013 and the program was extended until 2017 with only minor adjustments. Importantly, the administrative thresholds of €18,750 and €37,500 were unchanged, while the guarantee coverage rate was reduced slightly from 75% to 70% after 21 March 2014.

To assess whether access to other guarantees affects our results, we estimate the second-stage regressions from Section 7.1.2 using a broader measure of liquidity that aggregates all guarantees obtained from SGMs over the relevant horizon. Table 11 reports estimates from Specification (10) over a two-year horizon (Column 1) and a five-year horizon (Column 2).

Column (1) shows that this broader liquidity measure increases by €1.088 per euro of guaranteed credit at the two-year horizon. This estimate helps address a potential concern regarding substitution across guarantee programs. In particular, firms with access to smaller SGM guarantees could, in principle, compensate by applying for additional guarantees under the general regime (albeit at less favorable terms) thereby offsetting the discrete difference in guarantee size that underpins our identification strategy. Column (1) suggests that this is not the case: the corresponding increase in the narrow liquidity measure amounts to €0.941 (reported in Column 4 of Table 8) which is close to the €1.088 estimate obtained using the broad measure. Column (2) suggests that the liquidity effects dissipate at the five-year horizon.

We next estimate the dynamic differences-in-differences Specification (11). Panel (a) of Figure 4 plots the estimated coefficients δ_k for liquidity when all guarantees are included. Three main insights emerge. First, although guarantee schemes were already in place prior to 2012, a comparison of treated and control firms in Table 6 together with the evidence in Panel (a) indicates that take-up among firms in our sample increases markedly only from 2012 onward. Earlier

	(1)	(2)
	Liquidity two years (all guarantees)	Liquidity five years (all guarantees)
Value of guarantee	1.088** (0.480)	-0.092 (0.885)
Employees	-869.389 (2 825.255)	6 579.383 (5 206.483)
Total Assets	0.002 (0.007)	-0.010 (0.013)
Sales	0.010** (0.004)	0.011 (0.008)
Fixed assets	-0.005 (0.014)	0.027 (0.026)
Cash	-0.142*** (0.033)	-0.254 *** (0.060)
Leverage	-17 165.347 (14 560.733)	-50 834.310 (26 833.055)
Exports	0.023** (0.012)	0.058*** (0.022)
Industry fixed effects	Yes	Yes
Observations	634	634

Table 11: **Second-stage regressions for liquidity including all guarantees** This table reports estimates of coefficients δ associated with the instrumented value of the guarantee $\hat{G}_i$ in Specification (10) for $t = 2$ and for $t = 5$. Robust standard errors are in parentheses and *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. All columns report results for firms with score around the cutoff (approximately 8-11 employees). We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sources are *Banco Português de Fomento* and *Banco de Portugal*.

guarantee programs therefore play a negligible role in our analysis. Second, comparing narrow liquidity in Panel (b) of Figure 3 with broad liquidity in Panel (a) of Figure 4 in year 1 shows no differential application behavior between treated and control firms for guarantees not targeted at micro and small firms. This finding reinforces the interpretation that the divergence in liquidity holdings between treated and control firms is driven by SGM guarantees, rather than by other guarantees. Third, by year 2, the difference between treated and control firms amounts to €5,661. This difference is substantially smaller than the treatment-induced difference of €17,854.9 reported in Table 7, lending further credibility to our causal estimates at this horizon (overall, these results confirm the second-stage findings comparing narrow liquidity and broad liquidity measures at the two-year horizon reported above). Differences between narrow liquidity in Panel (b) of Figure 3 and broad liquidity in Panel (a) of Figure 4 become more pronounced at longer horizons, coinciding with the expiration of initial SGM guarantees and

the reapplication of some firms for new guarantees.

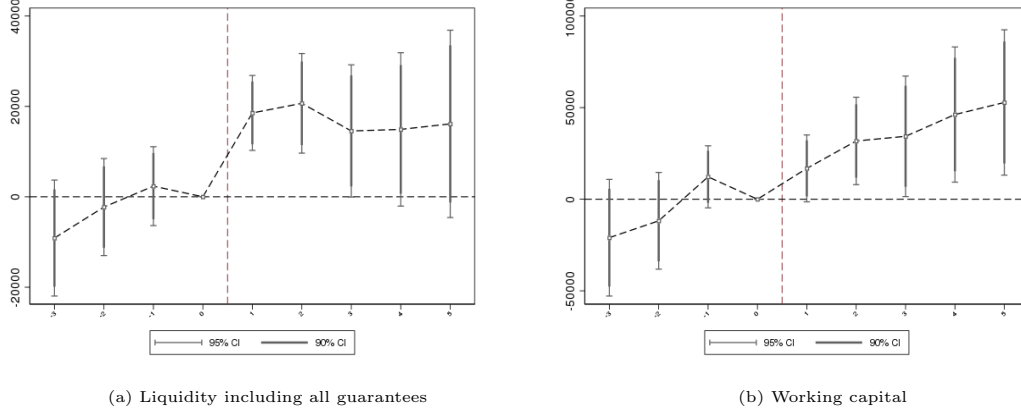


Figure 4: **Other liquidity variables** This figure plots the point estimates and the 90% and 95% confidence intervals for the coefficients δ_k associated with the variable $Treated_i$ in Specification (11) for liquidity (all guarantees) and working capital. The horizontal axis displays time in years around the origination date of the guarantee (represented by the vertical dashed line) with year 0 representing the year prior to origination. The units on the vertical axis measure differences in euros between the treated and the control groups. The treated group contains firms with access to guarantees equal to €37,500 and the control group contains firms with access to guarantees equal to €18,750. All panels report estimates for firms with score around the cutoff (approximately 8-11 employees). We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sample period is 2008 to 2017 and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

Given these results, a refined interpretation of our findings emerges. Because the treatment induces a sharp discontinuity in the value of the guarantee at the certification threshold both for SGM guarantees and for the aggregate measure of all guarantees at the issuance date, it is natural to interpret our estimates as capturing the causal effect of SGM guarantees on broader liquidity (i.e. taking into account the broader guarantee system). In the refined interpretation, the estimated effect encompasses not only the initial SGM guarantee but also the option available to firms to reapply for additional guarantees in the aftermath of issuance (under essentially similar contractual conditions as the initial SGM guarantees). Since program take-up among our sample firms began in earnest only in 2012, our estimates can be viewed as reflecting the cumulative impact of the guarantee scheme over the 2012–2017 period. Of course, the decision to obtain additional guarantees is endogenous, as firms facing more favorable investment opportunities are also more likely to reapply. Consequently, subsequent guarantees compound with investment opportunities, implying that the estimated treatment effect reflects both the initial exogenous difference in the value of the guarantee and the endogenous applications in the aftermath of issuance. In this sense, our estimates capture the aggregate liquidity provision through the guarantee system rather than the partial effect of a single guarantee. In this vein, Bonfim, Custódio, and Raposo (2023) provide complementary evidence from a setting characterized by greater temporal variation in program conditions and

covenant structures, analyzing multiple guarantees over a longer horizon and framing their results as a broader policy evaluation of a specific credit guarantee program.

Working capital For completeness, Panel (b) of Figure 4 plots the evolution of working capital. As in earlier panels, differences in pre-trends between treated and control firms complicate causal interpretation. Although the gap between the two groups widens following guarantee issuance, this pattern may largely reflect the continuation of pre-existing trends rather than a causal effect of the guarantee. Reassuringly, when we construct broader liquidity measures that combine working capital with the outstanding value of guarantees, our main results on liquidity levels and liquidity-to-scale ratios remain qualitatively unchanged (results not shown).

8.2 Other public policies

One may conceive the possibility that other public policies distinguish firms with small certification from firms with micro certification, and the differences between the treated and the control group may arise from these policies and not from credit guarantees. This is unlikely to be the case for three main reasons. First, the general legislation on the main incentive programs in Portugal during 2012 and 2013 (QREN and COMPETE) do not seem to distinguish between micro and small firms. Recall that the official certification was implemented after many of these programs had been designed.

Second, a number of specific studies on Portuguese public policies do not find any relevant distinction between micro firms and small firms during 2012 and 2013: Santos, Cincera and Serrano (2019) analyze the Portuguese Innovation Incentive System; Martins (2021) studies an European Social Fund grant; Gabriel, Santos and Tavares (2022) explore the spatial discontinuity in access to European funds in Portugal; Alexandre (2021) makes a broad evaluation of incentives to firms; Alexandre, Chaves and Portela (2022) evaluate incentives by the European Regional Development Fund. None of these studies documents statistically significant differences between micro and small firms.

Finally, we have a measure of the subsidies received by each firm. Column (1) in Table 12 shows the results of regressing the value of subsidies on the year before applying for the guarantee on certification status, and suggests concerns about this source of confounding are not serious. Moreover, Figure 5 depicts the evolution of subsidies by presenting estimates of coefficients δ_k obtained from

	(1)	(2)
	Subsidies	Value of guarantee (all firms)
$Treated_i$	-48.22 (634.76)	17 937.43*** (202.02)
Control variables	Yes	Yes
Adjusted R-squared	0.298	0.941
Observations	634	740

Table 12: **Subsidies, and reallocating small firms** Column (1) in this table reports the estimates of the coefficient associated with the variable $Treated_i$ in the regression of the value of subsidies on the variables $Treated_i$. The regressor $Treated_i$ is an indicator variable that equals 1 if and only if the firm receives a large guarantee. Column (2) reports estimates of coefficient β associated with the variable $Treated_i$ in Specification (9). The column reports results for 1,096 small firms with guarantees of €18,750 reclassified as having micro certification. All regressions include the number of employees, total assets, sales, fixed assets, cash, leverage, exports, and industry fixed effects as controls. Robust standard errors are in parentheses and *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. All columns report results for firms with score around the cutoff (approximately 8-11 employees). We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sources are *Banco Português de Fomento* and *Banco de Portugal*.

Specification (11) plotting the point estimates (with the point estimate for the year before the guarantee was granted normalized to zero) and the 90% and 95% confidence intervals. The figure strongly suggests subsidies are not a post-intervention confounder. Anticipation effects may exist (recall certification is determined more than a year before firms apply for a credit guarantee) but their economic magnitude is limited.

8.3 Industry-specific dynamics

A potential concern is that industry-specific dynamics generate time-varying shocks to investment opportunities (as suggested by Altomonte, Morlacco, Sonno, and Favoino, 2025). To address this issue, we augment Specification (11) with industry-by-time fixed effects, which absorb shocks common to firms within the same industry at a given point in time. As shown in Figure 6, the resulting estimates closely mirror those obtained without these controls. We therefore conclude that our main results are not driven by industry-specific dynamics and that the causal interpretation remains intact.

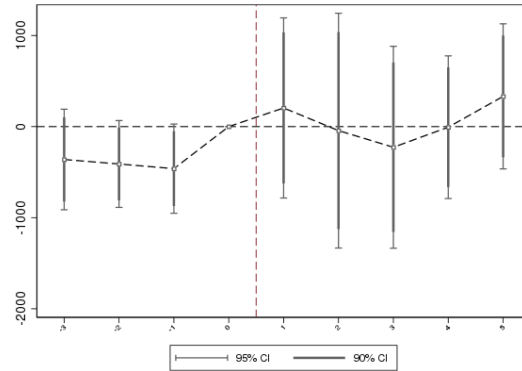


Figure 5: **Subsidies** This figure plots the point estimates and the 90% and 95% confidence intervals for the coefficients δ_k associated with the variable $Treated_i$ in Specification (11) for the dependent variable subsidies. The horizontal axis displays time in years around the origination date of the guarantee (represented by the vertical dashed line) with year 0 representing the year prior to origination. The units on the vertical axis measure differences in euros between the treated and the control groups. The treated group contains firms with access to guarantees equal to €37,500 and the control group contains firms with access to guarantees equal to €18,750. The figure reports estimates for firms with score around the cutoff (approximately 8-11 employees). We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sample period is 2008 to 2017 and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

8.4 Placebo certification cutoffs

We perform a series of placebo tests to address concerns that our findings might be driven by confounding factors. Specifically, we repeat the first-stage regression using alternative cutoffs of the score variable. We consider two cases. In the first case, we compare firms with 5–6 employees (placebo control group) to those with 7–8 employees (placebo treated group) with cutoff separating the groups equal to 7 employees. In the second case, we compare firms with 11–12 employees to those with 13–14 employees, with a cutoff equal to 13 employees. In these two cases, we do not exclude firms that obtain guarantees lower or equal to €18,750 (i.e. firms with “low investment opportunities”). This is because there would be insufficient variation in the value of the guarantees in the first case, since almost all firms would obtain a value of guarantees equal to €18,750. The results of the two cases, reported in columns (1) and (2) of Table 13 respectively, show no effects of comparable magnitude to those observed at the true cutoff.

8.5 Reallocating small firms

We explained in Subsection 6.3 that we dropped observations on 1,096 firms with small certification that obtained guarantees equal to €18,750 as these firms would have not been rationed if they had been firms with micro certification.

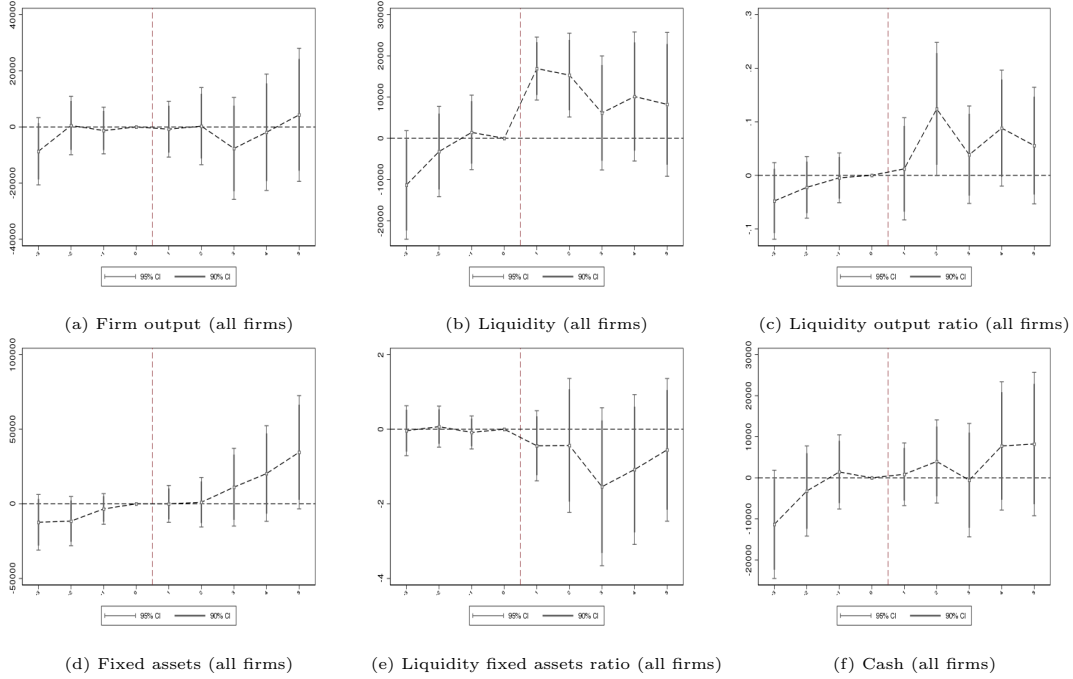


Figure 6: Industry-specific dynamics This figure plots the point estimates and the 90% and 95% confidence intervals for the coefficients δ_k associated with the variable $Treated_i$ in Specification (11) augmented with industry by time fixed effects, for several dependent variables. The horizontal axis displays time in years around the origination date of the guarantee (represented by the vertical dashed line) with year 0 representing the year prior to origination. The units on the vertical axis measure differences in euros between the treated and the control groups. The treated group contains firms with access to guarantees equal to €37,500 and the control group contains firms with access to guarantees equal to €18,750. All panels report estimates for firms with score around the cutoff (approximately 8-11 employees). We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sample period is 2008 to 2017 and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

	(1)	(2)
	Value of guarantee (5-8 employees)	Value of guarantee (11-14 employees)
$Treated_i$	-4 280.17*** (268.45)	-333.97 (682.67)
Control variables	Yes	Yes
Adjusted R-squared	0.324	0.019
Observations	2454	558

Table 13: **Placebo tests** This table reports estimates of the coefficient β associated with the variable $Treated_i$ in Specification (9). The regressor $Treated_i$ is an indicator variable that equals 1 if and only if the firm has access to a large guarantee. Robust standard errors are in parentheses and *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Column (1) reports results for firms with score around a placebo cutoff, which includes firms with approximately 5-8 employees; column (2) reports results for firms with score around a placebo cutoff, which includes firms with approximately 11-14 employees. All regressions include the number of employees, total assets, sales, fixed assets, cash, leverage, exports, and industry fixed effects as controls. We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sources are *Banco Português de Fomento* and *Banco de Portugal*.

One potential concern is that some firms in the control group with micro certification are in fact not rationed (and therefore, we are treating differently the treated and the control groups). If so, they should behave similarly to treated firms. This implies that the presence of such firms in the control group biases our estimates against confirming Implication 1, rather than in its favor.

Another potential concern is firm misclassification. We reconstruct certification status using the rules described in Section 4, based on the number of employees reported in IES. However, discrepancies between employee counts in IES and those in *Relatório Único* (see Appendix 2) may account for the misclassification of the 1,096 small firms excluded from our baseline sample. To address this concern, we reclassify these firms as firms with micro certification and replicate the validation exercise of the assignment mechanism. Specifically, we re-estimate the first-stage regressions in Table 7 to measure the effect of certification on guarantee values. Column (2) in Table 12 shows only minor differences relative to Table 7, indicating that selection bias is unlikely to be a major concern.

8.6 All firms

So far our analysis has been performed with firms with a score around the cutoff (i.e. firms with approximately 8-11 employees) or with firms that have their certification predetermined. Figure 7 depicts the evolution of the liquidity-scale ratio without imposing these constraints. The figure presents estimates of coefficients

δ_k obtained from Specification (11) plotting the point estimates (with the point estimate for the year before the guarantee was granted normalized to zero) and the 90% and 95% confidence intervals.

Regarding the analysis including firms without predetermined certification, Panel (a) plots the point estimates for the coefficients δ_k for the liquidity-output ratio, and Panel (b) presents the estimates for the liquidity fixed-assets ratio. Both panels consider firms with score around the cutoff. The evolution of the liquidity ratios is similar to the evolution of the same ratios in Panels (c) and (d) in Figure 3, thereby suggesting that our main analysis is largely unaffected.

Regarding the analysis including firms with a wider interval for the score (and no matching), we consider an interval which broadly includes firms with 5-14 employees, Panel (c) plots the point estimates for the coefficients δ_k for the liquidity-output ratio, Panel (d) presents the estimates for the liquidity fixed-assets ratio. The results are similar to Panels (c) and (d) in Figure 3, thereby suggesting that our analysis extends to a larger sample.

9 Other mechanisms

This section explores additional mechanisms that may shape the effects of loan guarantees and help interpret our baseline results. We examine three dimensions in turn. First, we assess financial additionality by studying the response of bank lending. Second, we investigate whether pre-treatment leverage mediates firms' responses to guarantees. Third, we exploit heterogeneity in demand conditions (proxied by export status) to assess how investment opportunities interact with guaranteed credit. Together, these analyses provide complementary evidence on the channels through which guarantees operate. We consider additional channels in Appendix 5.

9.1 Financial additionality

The evaluation of loan guarantee schemes typically differentiates between financial additionality and economic additionality (Cusmano, 2013). Financial additionality captures whether guarantees expand bank credit or affect firms' financing conditions, whereas economic additionality assesses whether these changes translate into real economic outcomes.

Having already assessed economic additionality through outcomes such as

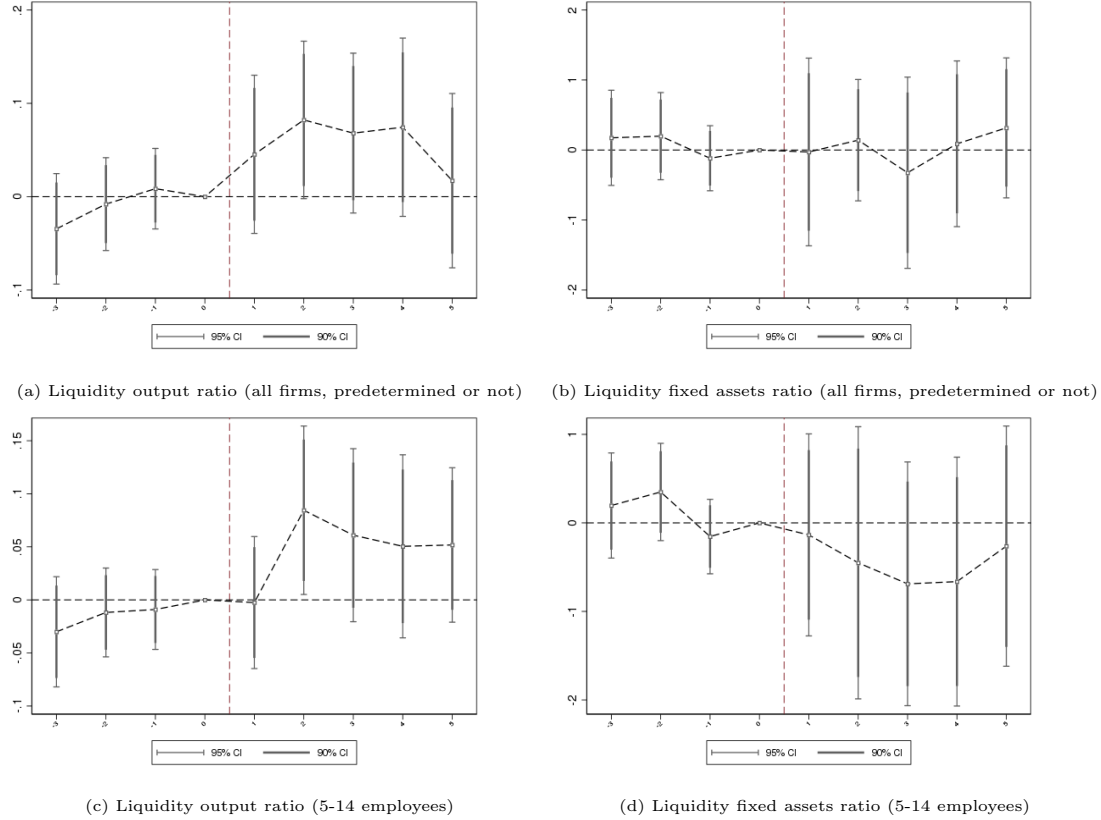


Figure 7: Liquidity ratios: robustness including firms without predetermined certification and considering wide score This figure plots the point estimates and the 90% and 95% confidence intervals for the coefficients δ_k associated with the variable $Treated_i$ in Specification (11) for the liquidity output ratio and for the liquidity fixed assets ratio, for firms with score around the cutoff (approximately 8-11 employees) with or without predetermined certification, and for firms with wide score (approximately 5-14 employees) with predetermined certification. The horizontal axis displays time in years around the origination date of the guarantee (represented by the vertical dashed line) with year 0 representing the year prior to origination. The units on the vertical axis measure differences in euros between the treated and the control groups. The treated group contains firms with access to guarantees equal to €37,500 and the control group contains firms with access to guarantees equal to €18,750. In the sample with score around the cutoff, we perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sample period is 2008 to 2017 and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

output and investment, this section focuses on financial additionality, measured through the evolution of bank lending. In practice, the implementation of loan guarantee schemes is frequently delegated to financial intermediaries, reflecting banks' comparative advantage in screening and monitoring borrowers relative to public guarantee agencies. A growing literature shows that banks' involvement can materially affect both the allocation and effectiveness of guarantees. For example, Ono, Uesugi, and Yasuda (2013) study credit guarantees in Japan during the 2008–2009 financial crisis and document an expansion in credit supply to participating firms. However, when guarantees were extended to firms with existing bank relationships, this increase was partly offset by a contraction in non-guaranteed lending, indicating a substitution effect. Moreover, firms receiving guaranteed loans from their relationship bank experienced a larger deterioration in ex post performance than firms receiving non-guaranteed loans. These findings raise concerns about strategic behavior by banks in the administration of guarantee programs. Consistent with this interpretation, Cascarino, Gallo, Palazzo, and Sette (2022) and Altavilla, Ellul, and Pagano (2022) document evidence of strategic allocation by Italian banks during the COVID-19 pandemic.

Table 14 examines financial additionality by analyzing the evolution of bank lending over a two-year horizon following guarantee issuance. Due to data limitations, we observe bank loan balances only from one year prior to the guarantee, which prevents us from tracing their dynamic evolution in the same way as in previous figures. Column (1) reports estimates from the first-stage Specification (9), included for completeness, while Columns (2)–(4) present results from second-stage Specification (10).

Column (2) reports the average difference in lending between treated and control firms for loans extended by the bank issuing the guarantee. For each additional euro of guaranteed credit, treated firms receive €0.49 more in loans from the guaranteeing bank. This estimate, however, is not statistically significant. This finding is consistent with the possibility that banks partially offset guaranteed lending by reducing the supply of non-guaranteed credit to the same firms, suggesting substitution within the lending portfolio.

Column (3) reports the estimated difference in lending from banks that did not issue the guaranteed loan. Although the coefficient is not statistically significant, the point estimate indicates an increase in credit from other banks. This pattern is consistent with firms obtaining the guaranteed loan from a bank with which they have a weaker pre-existing relationship, while continuing to access (or slightly expand) credit from lenders with established relationships.

Column (4) reports the estimated effect on total bank lending. Although the coefficient is not statistically significant, the point estimate implies an average

	(1)	(2)	(3)	(4)
	Value of guarantee	Loans of bank with guarantee	Loans of banks without guarantee	All bank loans
$Treated_i$	17 768*** (0.360)			
Value of guarantee		0.490 (0.449)	0.346 (1.021)	0.836 (1.194)
Employees	-0.013 (0.019)	-831.907 (2630.305)	-3044.703 (5980.622)	-3886.61 (6994.183)
Total Assets	-0.000 (0.000)	-0.007 (0.007)	-0.005 (0.015)	-0.012 (0.017)
Sales	0.000 (0.000)	-0.000 (0.004)	0.018** (0.009)	0.017* (0.010)
Fixed assets	-0.000 (0.000)	-0.007 (0.013)	0.071** (0.029)	0.064* (0.034)
Cash	0.000 (0.000)	-0.022 (0.031)	-0.034 (0.071)	-0.056 (0.083)
Leverage	1.100* (0.624)	-15504.754 (13508.769)	25057.873 (30715.384)	9553.119 (35920.849)
Exports	0.000 (0.000)	0.047*** (0.011)	0.156*** (0.024)	0.203*** (0.029)
Industry fixed effects	Yes	Yes	Yes	Yes
First stage	Yes	No	No	No
Second stage	No	Yes	Yes	Yes
Adjusted R-squared	0.927			
Observations	582	582	582	582

Table 14: **Financial additionality (two years)** Column (1) in this table reports the first-stage estimate of the coefficient β associated with the variable $Treated_i$ in Specification (9). The regressor $Treated_i$ is an indicator variable that equals 1 if and only if the firm has access to a large guarantee. The remaining columns report the second-stage estimates of coefficients δ associated with the instrumented value of the guarantee $\hat{G}_i$ in Specification (10) for $t = 2$ and for a range of outcome variables. Robust standard errors are in parentheses and *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. All columns report results for firms with score around the cutoff (approximately 8-11 employees). We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The sources are *Banco Português de Fomento* and *Banco de Portugal*.

increase of €0.836 in total loans over the two years following guarantee issuance. Extending the horizon to five years yields a smaller point estimate of €0.65 for total bank lending, which likewise remains statistically insignificant (results not reported).

9.2 Leverage

We now assess how pre-treatment leverage mediates the impact of guarantees on treated and control firms. When firms have outstanding debt, part of their future cash flows has already been pledged to “old” creditors when they apply for a “new” loan. This reduces the disposable pledgeable income available to new investors, making additional financing difficult unless contracts can be renegotiated.

Average leverage, measured as the ratio of debt to total assets, is nearly identical across the two groups in our sample (47.23% for treated firms versus 47.44% for controls). Figure 8 compares treated and control groups for a subsample of firms with leverage below the median (low leverage) and for a subsample of firms with leverage above the median (high leverage). The figure suggests that leverage plays a limited role in influencing the effects of guarantees. An exception may be observed in the evolution of fixed assets among low-leverage firms, but we have argued fixed assets may not be a reliable proxy for scale. Overall, these findings suggest that our analysis is robust to heterogeneity in pre-treatment leverage.

9.3 Demand shocks

The macroeconomic environment during our sample period provides a natural source of heterogeneity in firms’ investment opportunities. Portugal, a small open economy, experienced a recession between 2011 and 2013, marked by a sharp contraction in domestic demand. Over the same period, exports (accounting for roughly one-third of GDP in 2012) expanded at an average annual rate of 8%. This divergence enables us to contrast exporters, who were partly insulated from the domestic demand shock, with non-exporters, who were more directly exposed. To account for this heterogeneity in demand conditions, we construct treated and control groups separately for exporters and non-exporters, thereby exploiting variation in both foreign and domestic market conditions to identify heterogeneous investment opportunities.

Figure 2 in Section 7.2.1 plots the estimated coefficients δ_k for firm output and fixed assets using the full sample. Figure 9 then disaggregates the analysis by

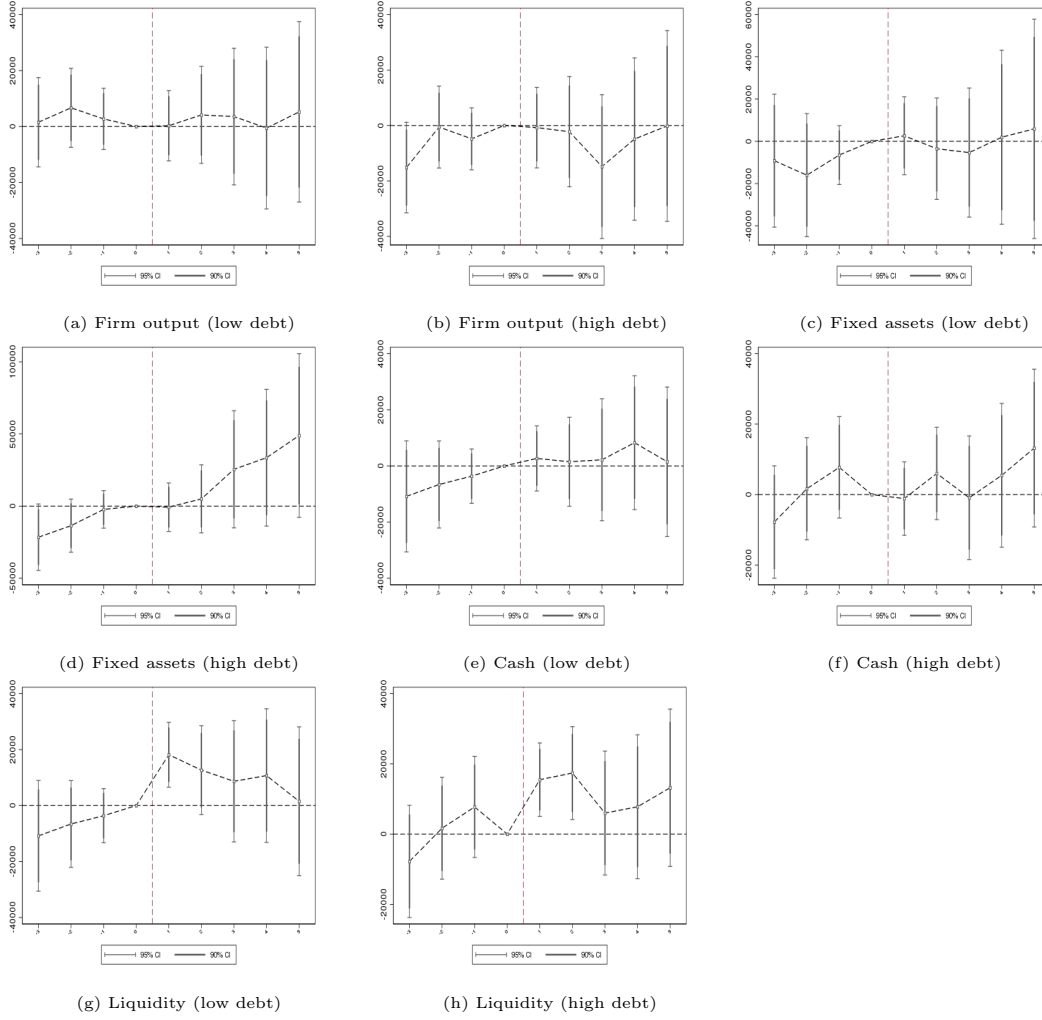


Figure 8: The effects of leverage This figure plots the point estimates and the 90% and 95% confidence intervals for the coefficients δ_k associated with the variable $Treated_i$ in Specification (11) for several dependent variables, for the subsample of firms with high debt, and for the subsample of firms with low debt. High and low debt are defined as above or below the median of the leverage ratio in the year prior to origination. The horizontal axis displays time in years around the origination date of the guarantee (represented by the vertical dashed line) with year 0 representing the year prior to origination. The units on the vertical axis measure differences in euros between the treated and the control groups. The treated group contains firms with access to guarantees equal to €37,500 and the control group contains firms with access to guarantees equal to €18,750. All panels report estimates for firms with score around the cutoff (approximately 8-11 employees). We perform k-to-k matching on the three-year average of annual growth rates of the number of employees, total assets, sales, and fixed assets, and on leverage and exporter status. The subsample of high debt firms has 160 firms in the treated group and 157 firms in the control group, and the subsample of low debt has 157 firms in the treated group and 160 firms in the control group. The sample period is 2008 to 2017 and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

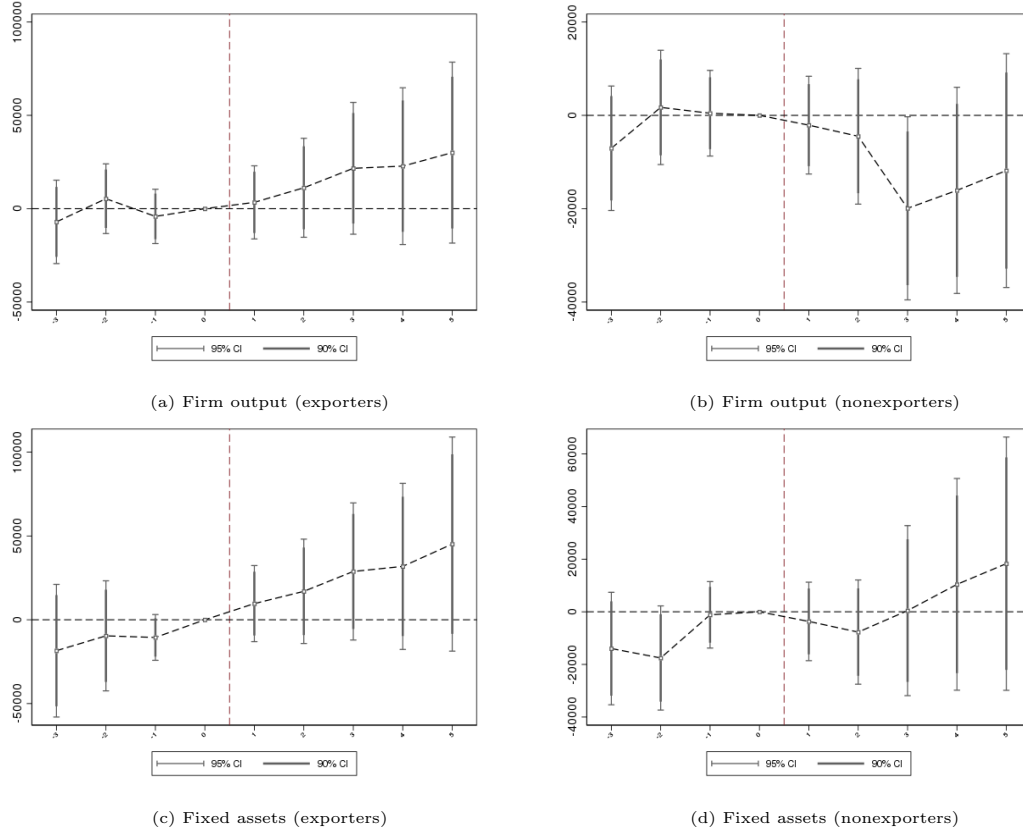


Figure 9: **Scale variables (exporters versus nonexporters)** This figure plots the point estimates and the 90% and 95% confidence intervals for the coefficients δ_k associated with the variable $Treated_i$ in Specification (11) for firm output and fixed assets, for the subsample of exporting firms, and for the subsample of non-exporting firms. The horizontal axis displays time in years around the origination date of the guarantee (represented by the vertical dashed line) with year 0 representing the year prior to origination. The units on the vertical axis measure differences in euros between the treated and the control groups. The treated group contains firms with access to guarantees equal to €37,500 and the control group contains firms with access to guarantees equal to €18,750. All panels report estimates for firms with score around the cutoff (approximately 8-11 employees). The subsample of exporters has 108 firms in the treated group and 108 firms in the control group, and the subsample of non-exporters has 209 firms in the treated group and 209 firms in the control group. The sample period is 2008 to 2017 and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

export status: Panel (a) shows the evolution of output for exporting firms, while Panel (b) reports the corresponding estimates for non-exporters. In both subsamples, output trends prior to guarantee issuance are broadly parallel across treated and control firms, lending support to the parallel-trends assumption within each group.

The full-sample estimates in Figure 2 indicate little average effect of guarantees on output. Once the sample is disaggregated by export status, however, a clear asymmetry emerges. Among exporters, the output gap between treated and control firms widens over time (Panel a of Figure 9), whereas among non-exporters the gap narrows (Panel b of Figure 9). This divergence suggests that

firm-specific demand conditions (such as ex post investment opportunities) mediate the real effects of guaranteed credit. These findings underscore the importance of accounting for heterogeneity in investment opportunities when assessing scale effects.

Panels (c) and (d) of Figure 9 report the corresponding estimates for fixed assets for exporters and non-exporters. In contrast to output, pre-trend differences between treated and control firms are more pronounced in both exporter and non-exporter subsamples. As a result, the fixed-assets estimates should be viewed as descriptive rather than causal.

Figure 3 in Section 7.2.2 reports the estimated coefficients δ_k for liquidity variables in the full sample. Figure 10 then disaggregates the analysis by export status: Panel (a) shows the evolution of cash holdings for exporting firms, while Panel (b) reports the corresponding estimates for non-exporters. Pre-trend dynamics differ across the two groups, complicating causal interpretation and calling for caution when attributing post-treatment differences in cash holdings to the guarantee.

Panels (c) and (d) of Figure 10 report the corresponding estimates for liquidity for exporters and non-exporters. Although parallel pre-trends cannot be established, both panels display a sharp and economically meaningful increase in liquidity for treated firms immediately following guarantee issuance. This short-run response is consistent with a liquidity effect of guarantees.

Panels (e) and (f) of Figure 10 plot the coefficients for the liquidity-to-output ratio for exporters and non-exporters, respectively. In both subsamples, the parallel-trends assumption cannot be decisively rejected. The estimates reveal a pronounced increase in the liquidity-to-output ratio of treated firms relative to controls during the two years following guarantee issuance. This pattern is consistent with Implication 1: larger guarantees raise firms' liquidity relative to scale. Importantly, the robustness of this result across exporter and non-exporter subsamples indicates that unobserved heterogeneity in firm-specific demand conditions is unlikely to drive the findings. Taken together, these results underscore the liquidity-to-scale ratio as an informative, internally consistent, and theoretically grounded metric for assessing the impact of loan guarantees.

10 Conclusion

Credit guarantee schemes are widespread. The prevailing view in the literature is that government-backed guarantees alleviate credit rationing arising from infor-

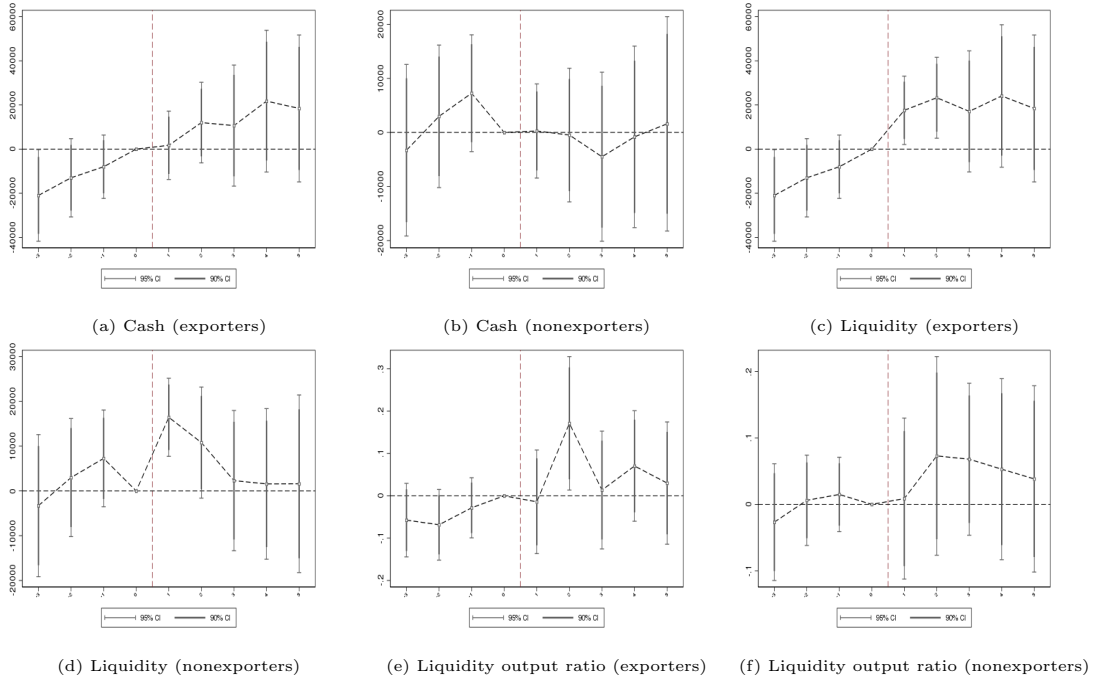


Figure 10: Liquidity variables (exporters versus nonexporters) This figure plots the point estimates and the 90% and 95% confidence intervals for the coefficients δ_k associated with the variable $Treated_i$ in Specification (11) for several dependent variables, for the subsample of exporting firms, and for the subsample of non-exporting firms. The horizontal axis displays time in years around the origination date of the guarantee (represented by the vertical dashed line) with year 0 representing the year prior to origination. The units on the vertical axis measure differences in euros between the treated and the control groups. The treated unit contains firms with access to guarantees equal to €37,500 and the control group contains firms with access to guarantees equal to €18,750. All panels report estimates for firms with score around the cutoff (approximately 8-11 employees). The subsample of exporters has 108 firms in the treated group and 108 firms in the control group, and the subsample of non-exporters has 209 firms in the treated group and 209 firms in the control group. The sample period is 2008 to 2017 and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

mational frictions, with the principal policy goal of stimulating investment. According to this conventional framework, the collateral channel is the main mechanism through which guarantees affect real outcomes. This paper highlights a complementary mechanism: credit guarantees also operate through the liquidity-insurance channel, providing firms with protection against liquidity shocks, such as cost overruns, thereby helping firms overcome information problems that hinder the functioning of insurance markets.

We develop a model that integrates these two channels and test its predictions using data from the Portuguese public credit guarantee scheme. Both theory and evidence point to a causal effect of guarantees on firms' liquidity holdings, but no systematic effect on scale outcomes such as firm output. The ambiguous impact on scale reflects the substitutability between liquidity and scale.

This substitutability has important implications for behavior and efficiency. First, when uncertainty is high and financial markets are incomplete, it may be rational for firms to prioritize liquidity holdings over scale expansion. Firms may optimally use guarantees to strengthen liquidity buffers rather than to finance additional investment, thereby reconciling theoretical predictions with the empirical finding that guarantees seldom raise project scale. Second, such behavior need not imply inefficiency. By providing contingent liquidity in adverse states, guarantees reduce the risk of premature liquidation of otherwise viable projects. Their ex post activation thus enhances welfare by stabilizing production capacity in times of financial distress.

The model also delivers clear policy implications. First, it clarifies the distinction between the liquidity and collateral channels by showing that a fair-insurance loan guarantee does not operate through collateral provision. Absent subsidies, guarantees do not increase pledgeable income ex ante and therefore do not relax borrowing constraints in the standard collateral-based sense. Their economic value instead derives from the contingent reallocation of liquidity to adverse states of the world. Second, for this reason, a non-subsidized guarantee scheme can improve social welfare by directing liquidity to states in which it is most valuable. Third, the model makes explicit that any collateral effect of loan guarantees arises exclusively from the presence of implicit or explicit subsidies embedded in official guarantee schemes, rather than from the guarantee contract *per se*.

These insights suggest that the conventional evaluation of credit guarantee schemes, based primarily on their impact on investment or firm scale, is incomplete. The limited response of contemporaneous investment does not imply policy failure. Rather, guarantees should also be assessed by their ability to enhance firms' resilience and sustain productive activity.

Finally, our findings on adverse selection and moral hazard have implications for the design of loan guarantee schemes. In particular, they underscore the importance of contract features that preserve the insurance nature of guarantees while limiting distortions. Effective schemes should rely on risk-based pricing, focus on activities characterized by high fixed costs and high expected value, and maintain meaningful bank exposure to ensure continued screening and monitoring incentives. In addition, effective safeguards against risk shifting into guaranteed loans within the banking system, combined with streamlined and credible follow-up procedures, are essential to contain moral hazard while preserving timely access to liquidity.

A second set of implications concerns the communication and evaluation of guarantee programs. If guarantees primarily operate as liquidity insurance, their success should not be judged solely by contemporaneous investment or output responses. Instead, program evaluation should emphasize firm resilience, survival, and the preservation of productive capacity in adverse states, as well as compositional outcomes such as innovation and internationalization. Clearly articulating the insurance logic of guarantees (to policymakers, financial institutions, firms, and the general public) is therefore central to aligning expectations with the economic mechanisms at work.

Appendix 1 The current configuration of the Portuguese credit guarantee system is structured in two layers. At the top, the *SPGM – Sociedade de Investimento, S. A.* (SPGM) coordinates the whole system and is 99% owned by the government. It has been superseded by *Banco Português de Fomento* in 2020, the source of our data, and functions as the holding *de facto* of the whole system and manages the counter-guarantee fund—the *Fundo de Contragarantia Mútuo* (FCGM). At the lower layer, there are four mutual guarantee companies, the *Sociedades de Garantia Mútua* (SGM), whose capital is mostly held by private financial institutions and the beneficiary firms (see Table 15).

	Public institutions	Banks	Capital held by firms in the form of mutualism
Norgarante	14.62	3.03	82.35
Lisgarante	20.36	10.81	68.83
Garval	19.8	28.86	51.34
Agrogarante	25.26	14.91	59.83

Table 15: Shares of capital in the SGM in 2012 (%). Sources: Norgarante (2013), Lisgarante (2013), Garval (2013) and Agrogarante (2013).

Agrogarante deals with agriculture and the other SGMs operate at the re-

Bank	25
FCGM (State)	60
SGM	15

Table 16: Shares of responsibility in the case of loan default (%) with the bank retaining skin in the game.

gional level, assisting mostly SMEs. The credit guarantee scheme is a mitigated mutualism scheme with strong government support. The FCGM is backed by the government through several specialized funds (with specific purposes such as providing credit guarantees, financing innovation, or supporting start-ups) and which are legal entities independent from the FCGM. *Finova* is the fund allocated to the official credit guarantees during 2012 and 2013, and counter-guarantees 80% of each operation carried out by the SGM. The remaining 20% share is the responsibility of each individual SGM, and aims at preserving incentives as the SGM keep some skin in the game in every operation. The guarantees analyzed in this paper cover 75% of the bank loan, thus yielding the responsibilities depicted in Table 16 in the case of loan default.

Appendix 2 SME Certification

The SME certification process consists of two steps.

1. The first step involves determining the firm’s *year dimension*. This classification is based on annual activity data, combining the number of employees (mandatory) with either total sales or total assets. Firms must designate themselves as autonomous, partner, or linked, depending on ownership stakes and control relationships. For firms with partner or linked enterprises, size is calculated by combining their own activity data with that of those partner or linked enterprises. The classification follows the thresholds outlined in Table 1, and exceeding only one of the two financial thresholds does not alter the firm’s *year dimension*.
2. The second step consists of determining whether the firm qualifies for certification as “micro” or as “small”. Certification is determined by the last three *year dimensions*. A firm retains its status if it obtains the same *year dimension* for two consecutive years.

Application for SME certification The official SME certificate issued by IAPMEI is based on information filed in two official reports:

- Simplified Corporate Information (IES, *Informação Empresarial Simplificada*) filed with the tax authority by July 15 (extended to July 31 in 2012).
- Employment Report (*Relatório Único*) submitted to the Ministry of Employment by April 15.

IAPMEI can verify the accuracy of reported data and ensures compliance. SME certification expires 20 business days after the IES deadline and can be renewed online immediately. The official certification is required for SGM credit guarantees, with approval for micro and small firms taking no more than 15 business days. If full-year data is unavailable, firms may use good-faith estimates for certification.

Good-faith estimates introduce a layer of complexity in the certification process, as firms have two options available if they apply for SME certification in the first part of the year, before filing IES. They may have their certification issued with older information reported in IES, or they may provide good-faith estimates regarding their last accounting year and have their certification issued with more recent information.

This information together with the rules described in Section 4 may be used as “instructions” to build tables that unambiguously assign certification status to each and every firm. IAPMEI developed a tool to simplify the process of certification, which is called “Certification of SME online”. The official size assigned by the certification service depends on the joint evolution of the number of employees, yearly total sales, and total assets. Given the deadlines for the issuance of SME certificates by IAPMEI, we consider three periods for applications:

- 26 January 2012 - 31 August 2012: firms could obtain their SME certification based on information for 2008, 2009 and 2010, or on information for 2009, 2010 and 2011. For the sake of simplicity, we assign the certification that ensures the most favorable treatment (i.e. small if feasible).
- 1 September 2012 – 31 December 2012: firms obtained their SME certification based on information for 2009, 2010 and 2011.
- 1 January 2013 – 13 June 2013: firms could obtain their SME certification based on information for 2009, 2010 and 2011, or on information for 2010, 2011 and 2012. We assign the certification that ensures the most favorable treatment.

We replicate the SME certification process with the information reported in IES, which is reported in CBHP. Our replication has two main limitations.

- IAPMEI uses data on the number of employees reported in *Relatório Único*, whereas we use data available in IES. Anecdotal evidence suggests that most firms report the same number of employees to IES and to *Relatório Único*. Our CBHP data on annual turnover and annual balance sheet total is the same data used by IAPMEI.
- IES (and therefore CBHP) has no information on whether the firm is autonomous, partner, or linked. Our replication thus assigns the certification of micro to some firms that IAPMEI deems small, and we drop applications from firms that obtain guarantees larger than €18,750 and we classified as micro.

For all practical purposes, Tables 17 and 18 implement the rules described in Section 4 and identify the cases with predetermined certification in our sample. Table 17 considers firms applying for SME certification in the period 26 January 2012 - 31 August 2012 or in the period 1 January 2013 – 13 June 2013, and Table 18 considers firms applying for SME certification in the period 1 September 2012 – 31 December 2012.

Appendix 3 *Linha de Crédito PME Crescimento*: Terms, conditions and covenants

Our analysis focuses on two specific programs with identical features: (1) *Linha de Crédito PME Crescimento*, launched on 16 January 2012 and replaced by *Linha de Crédito PME Crescimento 2013* on 15 February 2013; and (2) *Linha de Crédito PME Crescimento 2013*, which began on 23 January 2013 and was succeeded by a similar program on 14 June 2013. For simplicity, we refer to both as *Linha de Crédito PME Crescimento*.

We focus on these two programs because they share similar institutional characteristics and represent a substantial share of total Portuguese credit guarantee issuance. For example, *Linha de Crédito PME Crescimento* accounted for 84.67% of the total value of credit guarantees issued by *Norgarante* (one of the SGM) in 2013. The remaining share was distributed across nine smaller, targeted programs aimed at specific groups or sectors, including university students, vocational training, and the cork industry.

Firms must fulfill a number of conditions to apply for the *Linha de Crédito PME Crescimento* (SPGM, 2012) such as (i) no previously reported incidents with the banking system, the tax authorities or the social security, (ii) positive equity in the last accounting report, and positive net income in two of the last

		Year-dimension				
Application	Jan-Aug 2012	2008	2009	2010	2011	
Application	Jan-Jun 2013	2009	2010	2011	2012	
Case		$t - 4$	$t - 3$	$t - 2$	$t - 1$	PCD
1		M	M	M	S	M
2		M	M	S	M	
3		M	S	M	M	
4		S	M	M	M	M
5		M	M	S	S	
6		M	S	M	S	
7		S	M	M	S	M
8		M	M	M	M	M
9		S	S	S	S	S
10		S	S	S	M	S
11		S	S	M	S	S
12		S	M	S	S	S
13		M	S	S	S	S
14		S	S	M	M	S
15		S	M	S	M	S
16		M	S	S	M	M

Table 17: **Predetermined certified dimension (first part of the year)** The table identifies the cases in which the *certified dimension* of the firm is prodetermined and applies to applications for credit guarantees in the period between January and August 2012 or in the period between January and June 2013. The last column identifies the *predetermined certified dimension* (PCD) of the firm.

		Year-dimension			
Application	Set-Dec 2012	2009	2010	2011	
Case		$t - 3$	$t - 2$	$t - 1$	PCD
1		M	M	S	M
2		M	S	M	
3		S	M	M	
4		M	M	M	M
5		M	S	S	
6		S	M	S	
7		S	S	S	S
8		S	S	M	S

Table 18: **Predetermined certified dimension (second part of the year)** The table identifies the cases in which the *certified dimension* of the firm is predetermined and applies to applications for credit guarantees in the period between September and December 2012. The last column identifies the *predetermined certified dimension* (PCD) of the firm.

four accounting reports, (iii) commitment to maintain employment during the lifespan of the guarantee.

These requirements suggest that most firms applying for SGM guarantees are financially healthy. SGM guarantees impose little restrictions on the type of operations backed by the guarantee. The official documents mention explicitly new investment in tangible or intangible assets, or strengthening the working capital among the eligible operations. Purchasing financial assets, land, or existing buildings are not eligible operations.

This study focuses on the special regime for micro and small firms, whereby firms can benefit from favorable conditions with the following standardized provisions:

- Installment loan with constant quarterly loan repayments made during an amortization period equal to four years, with a grace period equal to six months. The duration of the loan is approximately two years.
- The maximum limit of the credit guarantee equals 75% of the bank loan, this being the key difference to the general regime (which has a limit equal to 50%). The bank bears responsibility for the 25% of the loan that is not guaranteed, and may ask for additional guarantees to the firm, as for example real collateral. These additional guarantees are also constituted in favor of the SGM *pari passu*, i.e. the bank must repay to the SGM part of the proceeds collected from the borrower.
- The spread over the 3-months-Euribor of the bank loan equaled 5% (the spread was larger or equal to 5% in the general regime). According to Banco de Portugal (2012), the average interest rate on new bank loans granted in Portugal equaled 7.6% in January 2012 (with the 3-months Euribor equal to 1.125%) and equaled 6.8% in October 2012 (with the 3-months Euribor equal to 0.197%). The quarterly fee for the use of the SGM guarantee was fully subsidized.
- Each firm had to buy shares of the SGM equal to 2% of the amount of the credit guarantee, which could be resold later when the guarantee expired (if and only if the guarantee was not activated). These transactions were done at nominal prices, and did not entail capital losses. This is an upfront fee.
- The provisions for micro firms and small firms differed in only one respect: the maximum amount of the credit guarantee. Micro firms were eligible for guarantees up to €18,750, while small firms could receive up to €37,500

(see Table 2). Under the special regime, each firm could obtain at most one guarantee in 2012 and one in 2013. Although firms could apply for additional guarantees under other regimes, these did not offer more favorable conditions.

Appendix 4 Variable definitions Codes in parentheses refer to the code in the dataset *Central Balance Sheet—Harmonized Panel Data* (CBHP) provided by *Banco de Portugal*.

VBP = Turnover (D001) + Variation in production (D006) + Capitalized production (D007) + Supplementary income (DL043) + Operating subsidies (D005)

CI = Costs of goods sold and material consumed (D025) + Supplies and external services (D026) + Indirect taxes (DL047)

Firm output = $\max[\text{VBP} - \text{CI}, 0]$

Sales = Turnover (D001)

Exports = Sales EU market (MG004) + Services EU market (MG005) + Sales Extra-EU market (MG007) + Services Extra-EU market (MG008)

Long-term bank credit (B085) are bank loans with maturity larger than one year.

Total liabilities (B080)

Current liabilities (B089)

Number of employees (E001)

Total assets (B001)

Subsidies = Operational subsidies (D005)

Leverage = Total liabilities/Total assets

Score = $\max \left\{ E001 \times 5, \frac{\min\{D001, B001\}}{40000} \right\} - 50$. We normalize the score variable to have a cutoff of zero for comparative purposes. The “score is around the cutoff” means score $\in [-10, 10)$ which means $E001 \in [8, 12)$. “Wide score” means score $\in [-25, 25)$ which approximately means $E001 \in [5, 15)$.

Appendix 5 Evolution of firms activating their guarantees We compare the evolution of outcomes for treated and control firms, separately for the subsample of firms that did not activate their guarantees and the subsample that did. While these subsample comparisons cannot be interpreted causally due to the “bad control problem”, they shed light on the mechanisms underlying the liquidity channel and illustrate how outcomes evolve when firms face cost overruns.

Figure 11 plots the coefficients δ_k from Specification (11), normalized to zero in the year prior to the guarantee, with 90% and 95% confidence intervals. Because a large share of activating firms subsequently exit the market (and we do not observe their post-exit outcomes) we adopt an agnostic approach and exclude these firms from the activating subsample in Figure 11. Inference is limited by the small number of activating firms: the activating subsample includes only 19 firms (9 control, 10 treated), resulting in wide confidence intervals and low statistical power.

Panels (a) and (b) provide no clear evidence on output dynamics: the average differences between treated and control firms increases for firms in our sample when they activate their guarantees, but the results are not statistically significant. Panels (c) and (d) for fixed assets show a divergent behavior between firms that activate and firms that do not activate their guarantees. We note, however, that fixed assets have proven difficult to interpret throughout our analysis. For cash, estimates are again noisy and imprecise. By contrast, Panels (g) and (h) show more straightforward liquidity patterns, with similar dynamics for treated and control activating firms.

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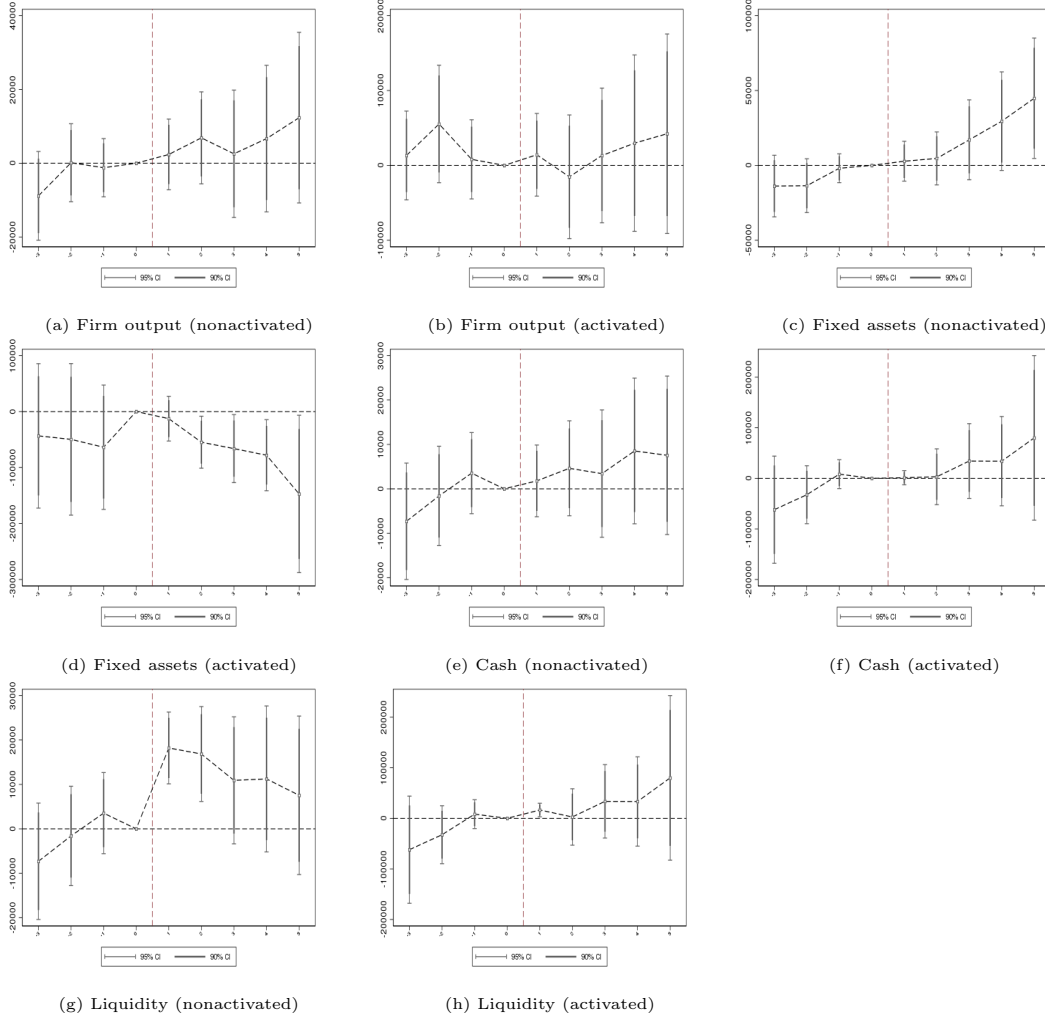


Figure 11: **Guarantee activation and insurance** This figure plots the point estimates and the 90% and 95% confidence intervals for the coefficients δ_k associated with the variable $Treated_i$ in Specification (11) for several dependent variables, for the subsample of firms that did not activate their guarantees and for the subsample of firms that activated their guarantees. The horizontal axis displays time in years around the origination date of the guarantee (represented by the vertical dashed line) with year 0 representing the year prior to origination. The units on the vertical axis measure differences in euros between the treated and the control groups. The treated group contains firms certified as small and the control group contains firms certified as micro. All figures report estimates for firms with score around the cutoff (approximately 8-11 employees). The subsample of firms that did not activate guarantees has 299 firms in the treated group and 289 firms in the control group, and the subsample of firms that activated guarantees has 10 firms in the treated group and 9 firms in the control group. The sample period is 2008 to 2017 and the sources are *Banco Português de Fomento* and *Banco de Portugal*.

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