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Supporting small firms through recessions and recoveries☆

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ABSTRACT

We use variation in the access to a government credit certification program to estimate the financial and real effects of supporting small firms. This program was first implemented during the global financial crisis, but has remained active ever since, allowing us to analyze its effects both during recessions and recoveries. Eligible firms have access to government loan guarantees and a credit quality certification. We estimate real effects using a multidimensional regression discontinuity design. We find that eligible firms borrow more and at lower rates than non-eligible firms, allowing them to increase investment and employment during crises. Industry-level analysis shows reduced productivity heterogeneity in more exposed industries, which is consistent with improved credit allocation. However, when the economy is recovering the effects of the program are less pronounced and centered on the certification component. The cost-per-job in the recovery period is half of the one estimated for the crisis period (5784€ and 11,788€, respectively).

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1. Introduction

Mutual guarantee programs, where governments offer a guarantee on bank loans, are common economic stimulus measures (Bach, 2014; Beck, 2014; Columba et al., 2010; Lelarge et al., 2010; Gonzalez-Uribe and Wang, 2020). Through these programs, governments offer partial guarantees on loans granted by financial institutions to small firms for the purpose of subsidizing the cost of borrowing and alleviating financing frictions, which are known to be larger for firms that are small and more informationally opaque. These programs are often used to respond to financial crises, when the supply of credit is limited (Carpenter and Petersen, 2002; Campello et al., 2010; DeYoung et al., 2015; Brown and Earle, 2017). Government credit guarantee programs were also widely used as a response to the COVID19 pandemic (Core and Marco, 2020; Altavilla et al., 2021; Gourinchas et al., 2021; Minoiu et al., 2021). Despite their popularity among governments and policy-makers, the real effects of these programs through the business cycle and the channels through which they work remain understudied. There are two main reasons for this. First, estimating their causal effects is challenging due to the endogenous selection of firms into these programs. Second, data availability hinders the analysis of their effects, as medium and small firms are mostly private. Despite these challenges, understanding the financial and real effects of support programs to SME through the business cycle is of first-order importance given the resources devoted by governments around the world.

We exploit a stimulus program adopted in Portugal for small and medium enterprises (SME) to study the sensitivity of small firms' investment and employment to the cost of borrowing. Through the *SME-Leader Program*, eligible firms have access to subsidized bank credit through government guarantees and an interest rate cap, and to a public credit rating. The program was created in 2008, at the onset of the global financial crisis, but when the economy began to recover the program remained active. This allows for the evaluation of the effects of support to small firms during crises and recoveries. The richness of the data on the population of Portuguese firms allows for a detailed analysis of financing conditions, including the impact of the program on the price and quantity of credit obtained by firms, and the usage of the borrowed funds.

One more unique feature of the program is that it targets SMEs with sufficiently good credit quality. This means that only firms that meet the required standards, measured by a set of financial ratios are eligible to the program. On the one hand, this feature can reduce the overall cost of the program (because a lower number of defaults is expected), as well as reduce the prevalence of so-called zombie lending practices (Acharya et al., 2022). On the other hand, targeted firms, being of good credit quality, might not necessarily face meaningful constraints when accessing credit markets, especially when the economy is doing better. Eligibility criteria to the program are multiple and change on a yearly basis. This allows for the implementation of a multidimensional regression discontinuity design (MRDD) to estimate its financial and real effects. The multidimensional and time-varying criteria generate exogenous variation in firms' costs of funding, which makes it possible to estimate its impact on the decisions and outcomes of small firms. Because the program certifies eligible firms with one of two ratings, we also exploit variation around the top rating cutoff to estimate the impact of an additional credit rating notch for small firms.

Our analysis covers one decade, from 2008 to 2018, including a period of crisis and the period of expansion that followed. The size of the interest rate subsidy is likely to change with overall economic conditions and the severity of existing financing frictions, as are its real effects. For this reason, we analyse the two periods separately. We define the crisis period in Portugal as that between 2008 and 2013. In 2008, the Portuguese economy suffered the consequences of the failure of Lehman Brothers, which reverberated worldwide. The program was implemented precisely to mitigate the impacts of the ensuing crisis. Less than 1% of Portuguese firms have access to capital markets. Their dependence on bank funding is critical for their activity, which prompted the authorities to swiftly implement this program, aimed at ensuring that creditworthy firms would not lose access to credit. When the economy was beginning to recover, a second and much larger shock hit the Portuguese financial system and, later, the economy. In the spring of 2010, Portuguese banks lost access to wholesale debt market funding, due to investors' concerns associated with the euro area sovereign debt crisis. Banks became largely reliant on European Central Bank funding and the government faced increased difficulties in accessing debt markets, leading up to an international request for financial assistance in the spring of 2011. Portugal successfully exited this assistance program in 2014. We thus define the post-crisis period as 2014–2018. In these years the economy recovered and monetary policy provided ample liquidity. Banks strengthened their capital ratios and cleaned up non-performing loans accumulated through the crisis. Credit supply became less constrained and bank competition increased.

Eligibility for the program is based on financial information reported to Portuguese authorities. If approved, certification is valid for a year. Because firms have discretion and endogenously choose whether to apply for the program, we estimate the intention to treat effect (ITT), i.e., we compare eligible firms' outcomes with those of non-eligible firms. We define a single running variable based on multiple criteria and thresholds following Ferreira et al. (2018), where we first determine the binding criteria for each firm-year and then standardize the distance to threshold across criteria.

We first establish that surpassing the eligibility criteria sharply increases the probability of program take up. Firms that are eligible to participate in the program have access to significantly lower costs of debt financing and increase their bank borrowing. When comparing firms around the cutoff point for the program, we find that eligible firms borrow through loans that are 1.8 percentage points (pp) less expensive than those for non-eligible firms in the year of certification, during the crisis period. We also document that eligible firms increase their borrowing by 8 pp more than non-eligible firms during the crisis period, when aggregate credit growth was decreasing. These effects are

less pronounced during the post-crisis period. Because the program targets firms with low credit risk ex-ante, this is consistent with the size of the subsidy being smaller when economic conditions are better.

We then test whether eligible firms make use of borrowed funds during the crisis period for investment purposes. We find that during the crisis, eligible firms invest more in total assets, including fixed capital and working capital, and increase their employment by more than non-eligible firms. We find that eligible firms invest 1.8 pp more in fixed capital than non-eligible firms in the year of certification. Given the average take up rate of 36% during the crisis period this represents a treatment on the treated (TOT) effect of 5 pp. This effect persists for a year after certification with a similar magnitude, but is not persistent beyond that period. We find weaker effects for working capital: eligible firms invest 1.1 pp more than non-eligible firms, which represents a TOT effect of 3.1 pp. This is a non-persistent effect. The overall impact measured by the change in total assets is consistent with these measured effects: eligible firms increase their total assets by 1.2 pp in the certification year, for a TOT of 3.3 pp. For human capital investment, we find that eligible firms increase their employment by an additional 0.14 employees in the year of certification and by another 0.25 employees in the year after, which represents a TOT effect of 0.38 and 0.69 employees, respectively. Cumulatively, certified firms retain, during the crisis period, approximately 1 additional employee (the median firm in our sample has 19 employees). The real effects of the program persist for at least one year after firms stop being eligible. Financing costs remain lower than they were before firms became eligible, helping them to continue to pursue their growth strategies.

In summary, our results show that a 1 percentage point (pp) decrease in the cost of debt financing is associated with contemporaneous increases of 0.7 pp in total asset growth, 1 pp. in fixed asset growth, and 0.6 pp. in working capital growth. A 1 pp decrease in the cost of debt financing is associated with a 0.22 pp contemporaneous increase in employment growth.

We also find a positive impact of the program on firm growth during the crisis period. Growth in sales is between 0.6 and 0.9 pp higher for eligible firms than non-eligible firms around the threshold in the two years after certification. These firms also increase their exports by significantly more than non-eligible firms around the eligibility threshold. This effect is positive and economically significant. Program-eligible firms export up to 8.6 percentage points more than others firms over the two years after becoming eligible. We do not find these effects in the post-crisis period. These results are consistent with the evidence from related research on international trade. Empirical work on this area finds support for the role of credit constraints on export activity of firms (Minetti and Zhu, 2011; Muûls, 2015; Paravisini et al., 2015), as it is proposed by Melitz (2003) style models with fixed export costs (e.g., Manova, 2013; Chaney, 2016). Overall, our results suggest that this program has a positive impact on firm growth, through domestic and international markets, with real effects in terms of firm investment and employment. Notably, these are mostly observed during the crisis period and are much less salient in the post-crisis period.

To further inform the interpretation of the results, we conducted a survey of certified and non-certified firms for which we obtained 5413 responses. The real effects of the program estimated using administrative data are corroborated by the perception of managers of small firms. Managers confirm that the program allows them to lower their financing costs and boost investment and employment. Furthermore, the survey results allow us to gain insights not available in the financial data. More than one-third of the managers report an increase in the competitive advantage of the firm, 30% mention a positive effect on the firms' ability to innovate, and 19% cite improved relationships with clients.

The *SME-Leader Program* assigns two different credit ratings to firms (*SME Leader* and *SME Excellence*), which allow us to study the role of credit certification as a possible mechanism to reduce financing frictions. Small firms are typically opaque, which makes the process of collecting information and establishing a relationship with creditors long and expensive (Beck and Demirguc-Kunt, 2006). Moreover, unlike large and public firms, these companies cannot benefit from the certification mechanism offered by the main credit rating agencies. When comparing firms with different rating levels to evaluate the value of an additional notch in credit certification, we find no significant effects on financial outcomes, but we find significant results on sales growth. This result suggests that the overall impact of the program is not limited to credit guarantees but is also due to the certification itself, which might be perceived as a positive signal by other stakeholders, including clients. This idea is supported by the evidence collected through the survey, as reputation benefits were considered a very important reason to apply for the program by more than half of the respondents that obtained a certification. Our quantitative results suggest that the effects arising from access to subsidized credit seem to dominate those arising from decreasing information asymmetries regarding firm quality during the crisis, as evidence of a credit rating effect is mostly observed in the post-crisis period.

We then study performance at the firm level, and potential improvements in credit allocation at the industry level. We find a positive intention to treat effect on return on assets but no significant improvement in firm-level productivity during the crisis period. We also find a decrease in loan default outcomes for eligible firms during the crisis period, showing that targeted firms do not engage in risk taking behavior. We then follow the identification strategy in Bertrand et al. (2007) and Sraer and Thesmar (2023), who estimate the contribution of banking deregulation to changes in aggregate total factor productivity (TFP), and exploit variation on treatment exposure at industry level. The estimated coefficient shows a reduction in cross sectional variance for more exposed industries, which is consistent with improved credit allocation.

We discuss whether the program affects real outcomes by loosening financial frictions that mostly affect the quantity of credit available (credit rationing) or simply by subsidizing the marginal cost of borrowing for SMEs. To identify frictions that mostly affect quantities, we focus on firms

that are less likely to face price frictions, i.e., firms that already borrowed at the lowest interest rates. For these firms, the partial credit guarantee should have only a very small price impact (if any). When we restrict the sample to firms in the lowest quartile of interest rates, we do not find significant treatment effects on interest rates (as expected), and we do not find that these firms borrow more during the crisis period. This result is consistent with these firms not facing meaningful quantity constraints. In the spirit of Banerjee and Duflo (2014), we also look at credit substitution. We find some evidence that eligible firms are more likely to substitute more expensive credit for less expensive one through the program. This result is also consistent with these firms not facing severe quantity-related constraints, as otherwise they would likely exhaust all credit sources instead of substituting them. We conclude that the real effects of the program mostly operate through a price channel.

Finally, we estimate the costs and benefits of the program. Our empirical estimates show that the benefits of the program are more visible during the crisis than the recovery. However, we would also expect costs to be smaller, notably if we consider the targeted nature of the program. The costs of the program correspond mostly to the activation of the government guarantee in case of loan default, deducted of the fee that firms must pay to access the credit lines. When we consider these costs as a proportion of the jobs created and the investment generated by SME Leader firms, we find that during the crisis years, the cost of each job created was 11,788 EUR. During the recovery, the cost per job was down to 5784 EUR. The cost per EUR invested was 0.08 EUR in the crisis period and 0.05 EUR in the recovery. These numbers confirm that even though the benefits of the program were less salient during the recovery, its targeted nature also significantly reduced the (absolute and relative) costs. Using the aggregate TFP effects estimated according to Sraer and Thesmar (2023), we find that for each euro spent in the program there was an increase in aggregate TFP of 66 euros, during the crisis period.

Our paper contributes to the literature on credit constraints faced by small firms. Credit-constrained firms are limited in their ability to grow (Banerjee and Duflo, 2014; Beck and Demirguc-Kunt, 2006). Often small firms have limited access to capital markets, and thus their most important source of external finance is typically bank loans (Ferrando et al., 2015). During the global financial crisis, banks were forced to adjust their portfolios in response to negative shocks, implying that SMEs' access to credit became severely constrained (Blattner et al., 2021; Carbo-Valverde et al., 2016; DeYoung et al., 2015). Demirgüç-Kunt et al., 2020 show that small firms around the world were more severely affected by these constraints during the global financial crisis, especially in countries with weaker information-sharing mechanisms. This was not a unique feature of this crisis, as small firms are generally more exposed to cyclical fluctuations (Crouzet and Mehrotra, 2020) and credit crunches (Gorton and He, 2008; Dinlersoz et al., 2018). We contribute to this literature by showing that a targeted program designed to support SMEs had a positive impact on their investment and growth during a profound economic and financial crisis. Importantly, we also show that the program targeted firms that were unlikely to face severe frictions affecting the quantity of credit, and that increased borrowing and investment was mostly due to subsidized interest rates.

Our paper also contributes to the literature examining how government interventions can address financial frictions faced by small firms. These frictions can arise from vulnerability to information problems, as well as from the market power of banks (Carbo-Valverde et al., 2009; Ryan et al., 2014). Government and national financial structures affect credit availability mainly through lending technologies (Berger and Udell, 2006; Behr et al., 2013; Kahn and Wagner, 2021), so several measures have been developed to improve small firms' access to finance through bank loans at different levels. Gonzalez-Uribe and Wang (2020) examine a loan guarantee program implemented in the UK, during the Great Recession, and find positive effects on performance, survival and job retention. For the U.S., there is abundant evidence that Small Business Administration loans were helpful in eliminating constraints in credit supply (Bachas et al., 2021), creating jobs (Brown and Earle, 2017), and promoting economic growth (Denes et al., 2021). There is also evidence on the broadly positive effects of government guarantee programs in France (Lelarge et al., 2010; Bach, 2014; Barrot et al., 2019), Italy (Columba et al., 2010; Bartoli et al., 2013; D'Acunto et al., 2018; D'Ignazio and Menon, 2020), and Chile (Mullins et al., 2018). The unique features of the Portuguese program, namely its targeted nature, the interest rate cap, as well as the richness of the data allow for a clean identification of financing conditions, including the impact of the program on the price and quantity of credit obtained by firms, as well as the usage of the borrowed funds. We show that real effects of the Portuguese program mostly operate through a lower cost of debt capital channel. This finding is complementary to Gonzalez-Uribe and Paravisini (2017) showing that the Seed Enterprise Investment Scheme in the UK program, which reduced the cost of equity capital, had a positive impact on firm investment.

Finally, our paper contributes to the understanding of how effective is support to small firms through the business cycle. In a recent paper, Crouzet and Tourre (2020) estimate a structural model of investment, financing and default to examine the trade-offs of credit support programs during crises and in their aftermath. They show that supporting firms' access to credit during crises may be helpful to avoid their liquidation, but that this support can create debt overhang problems during recoveries, slowing investment and growth. Our paper contributes to the literature on government interventions in credit markets by estimating the impact of access to subsidized bank credit on firm growth and performance, as well as documenting the real economic effects in terms of investment and employment. The program design allows for a more precise estimate of the real effects, as selection, manipulation, and anticipation effects that often hinder identification are addressed by exploiting the features of the program. The program also differs from most government interventions because it targets small firms with

low credit risk as defined by the government. By focusing on a subset of firms, the fiscal costs of the program are necessarily smaller. Our results show that a targeted program can improve the outcomes for the treated firms. Given that the program has been operational for over a decade, we can evaluate its effects both during a crisis and in the subsequent recovery period. The effects arising from improved access to credit prevail mainly during the financial crisis, as suggested by Crouzet and Tourre (2020), but the costs are also much smaller during recoveries.

Our results have relevant policy implications. Small and medium firms represent a large fraction of the European economy: according to the "Annual Report on European SMEs" by the European Union (EU), in 2016, they represented almost all (98%) non-financial enterprises and two-thirds (66%) of total EU employment, and accounted for almost three-fifths (57%) of the value added generated by the non-financial sector. Because of their importance in the economy, these firms are given particular attention by policy-makers, who recognize the challenges associated with small firms' access to credit. Our research design helps us to understand how relevant financial and informational frictions are in hampering firms' access to credit and growth, leading to different outcomes in terms of investment in physical and human capital. This allows policy-makers to understand the potential impacts of enacting policies to alleviate financial and informational constraints on small firms, notably for the best performers. This can be particularly relevant during financial crises or other economic distress events such as the recent COVID-19 pandemic. While in the first wave of the pandemic, governments around the world hurriedly offered indiscriminate support to small firms to offset the impacts of lockdowns and demand shocks, as it became clearer that the pandemic would have lasting and uncertain effects, a consensus emerged that support should be targeted (Bartik et al., 2020), to avoid the proliferation of zombie firms and unmanageable public finances imbalances, and to promote the efficient reallocation of resources in the economy. This paper offers evidence that supporting targeted small firms during a financial crisis has positive and lasting effects on investment and growth. However, these effects become more muted when the economy is recovering. That said, reputational benefits arising from certification of credit quality are important in boosting firm performance in good times. From a cost-benefit perspective, a targeted program allows for a reasonable balance through crises and recoveries. During crises the benefits are more meaningful, but the costs are also higher, as loan guarantees will lead to fiscal losses more often. During recoveries, the targeted firms show very small default probabilities. The benefits may not be as large, but the fiscal costs are negligible. We show that the cost-per-job in the recovery period is half of the one estimated for the crisis period (5,784 € and 11,788 €, respectively). Furthermore, having a permanent program can be helpful in triggering a fast reaction in case of an unexpected shock, as it happened at the onset of the pandemic.

The paper proceeds as follows. In Section 2, we describe the institutional setting, the program and the data. In Section 3, we explain the empirical strategy, and in Section 4 we present the results. Section 5 presents robustness tests and extensions, and Section 6 discusses the implications of the main results, making use of survey evidence whenever possible. Finally, Section 7 concludes the paper.

2. Institutions and data

2.1. The SME-leader program

The *SME-Leader Program* offers SMEs a credit certification (rating) issued by a governmental agency (IAPMEI). The program was introduced in 2008 with the stated objective of ensuring that the best performing SMEs had access to financing during the global financial crisis. To achieve this goal, the program was designed to work through two channels. The first is the credit certification mechanism, which mimics the credit ratings by international rating agencies, but focuses on SMEs rather than on large companies.

The second channel more explicitly addresses the lending dimension. Once in the program, firms have access to credit lines with partial guarantees provided by mutual guarantee societies funded and secured by the Portuguese government. This allows firms to borrow at lower rates and in a more streamlined and standardized process for credit approval. The terms and conditions applied vary across credit lines and change throughout the sample period. For illustration purposes, the maximum spread that banks could place on credit lines granted to *SME-Leader firms* in 2015 ranged between 2.7 and 3 p.p. above the 6-month Euribor (banks can charge lower spreads). For reference, the average spread for new loans under 1 million euros was 3.8 p.p. in the same period. Firms also had to pay a commission for access to the mutual guarantee, which was 0.65% for the most expensive credit lines. The maximum government guarantee and loan maturity also varies across credit lines. In 2015, the maximum guarantee was between 50% and 70%, while the maximum loan maturity allowed was 10 years. The program includes other stated benefits such as access to training and partnerships with service providers through IAPMEI.

Although the program was originally designed to mitigate constraints in access to credit during the global financial crisis, the perceived success of the program lead to its continuation. In 2016, the program was recognised at the European Enterprise Promotion Awards (EEPA), where it won the "Improving the Business Environment" award. The EEPA reward initiatives that promote entrepreneurship and business growth. According to IAPMEI, "this award reflects the strong impact that the *SME Leader* program had on companies awarded the statute, in terms of company financing, recognition of SMEs and in improving the flow of information". In 2022, the program is still active. However, the conditions under which firms can access the program changed materially over the years. To be eligible for *SME Leader* status, a firm has to satisfy a set of criteria based on its most recent financial and operational performance. To be eligible in a given year, a firm must satisfy the criteria with respect to the previous year's financial statements.

The eligibility criteria, set by the governmental agency, have changed every year since the creation of the program. Over time, the set of criteria included the following financial variables and ratios: total assets, number of employees, total sales, net income, EBITDA, net income/assets, net income/equity, equity/assets, EBITDA/assets, EBITDA/sales, debt/EBITDA, sales growth and EBITDA growth. The program criteria for each year in our sample are reported in Table B1 in the Appendix.[1] Overall, the criteria have become more demanding over time. There are no other programs or incentives using the same eligibility thresholds.

In addition to the financial criteria, firms must meet a set of more general qualifying criteria that are the same every year. These include being officially classified as SME by IAPMEI (this is solely based on firm size measured by the number of employees, revenue and assets, according to EU recommendation 2003/361), have three consecutive years of complete financial statements, and have no conflicting situations (e.g., late payments) with the Portuguese tax authorities, IAPMEI or Social Security.[2]

To obtain certification, firms must apply through a Portuguese commercial bank that sponsors its application. There is no application fee. The bank has to assess whether the firm meets the eligibility criteria, performs credit screening, and submits the application to the government agency. The bank also negotiates the interest rate and other commercial fees with the firm and maintains its monitoring function. The program might imply a smaller margin on these loans for banks, but the sponsor bank benefits from regulatory capital savings, given the partial government guarantees attached to these credit lines. The cap on the guarantee is intended to align incentives such that the bank performs sufficient monitoring of the loan (Chemla and Hennessy, 2014).

The certification is valid for one year. To remain in the program, the firm must comply with the set of criteria defined for that year and submit an application through the sponsor bank. Firms apply to the program through just one of the sponsor banks but can re-apply in the following year with a different bank. Unlike credit rating agencies, IAPMEI does not screen the firms, it simply establishes the criteria for eligibility. The typical annual timeline of the program is as follows. Firms submit their annual financial reports from the previous fiscal year to the relevant authorities around April; eligibility criteria based on previous fiscal year financial statements are announced, and firms apply to the program during the summer; the list of certified firms is publicly announced by IAPMEI during the fall; and firms benefit from their certified status until September of the following year (Figure B1 in the Appendix).

For a reference, in 2020, 9955 firms were certified as *SME Leaders* (1,398 more than in the previous edition). These firms account for more than 40 million euros in turnover and more than 325 thousand jobs. Most firms belong to the retail (34.4%), manufacturing (24.4%), food and accommodation (10.8%), and construction (10.8%) sectors. The majority are small firms (71.9%). Medium firms account for 22.2% of the total, and micro firms represent 6%.

A unique feature of the program is its two-tier credit certification (rating). While most eligible firms receive the *SME-Leader* certification, a smaller fraction are classified as *SME-Excellence* firms. To benefit from the top rating (*SME-Excellence*), firms have to meet a more demanding set of criteria, which also changes on an annual basis. The formal financial benefits of being in the program are identical for *SME-Leader* and *SME-Excellence* firms. As such, the additional benefits from being an *SME-Excellence* firm are expected to derive primarily from the certification effect. By being part of the program, the firms can publicize this certification on their websites and other communication platforms (a few examples are shown in Figure C1).

Table A1 reports the number of eligible firms for each of the two certification categories and the number of *SME-Leader* and *SME-Excellence* firms in a given year. The program started in 2008 only with one level of certification, *SME-Leader*. The top rating certification (*SME-Excellence*) was added in the following year. The number of certified companies increased until 2012 possibly due to increased awareness about the program, and has been stable since then. The number of eligible, non-certified firms decreased over time as the program criteria became tighter and, possibly, awareness increased. The average take-up rate, measured as the number of certified firms as a percentage of the number of eligible firms, is 41.5%, being 30% during the crisis period and 63.4% after the crisis (post 2013). This take-up rate is very similar to that of the Paycheck Protection Program (PPP) in 2020 (41.8%, according to Cororaton and Rosen, 2021). Figure 1 shows the number of firms entering the program for the first time, which decreases over time and is consistent with firms being certified for more than one year during the sample period. In Section 6, we discuss the selection of firms into the program and present survey evidence on the costs and benefits of the program as perceived by firm managers.

2.2. Data

The government agency responsible for the program makes publicly available the list of firms that are certified in each year, as well as the criteria to be certified as *SME-Leader* and *SME-Excellence* firms. We collect data on certified firms and program criteria between 2008 and 2018 from IAPMEI. This allows us to determine whether a firm is certified as an *SME-Leader* or *SME-Excellence* firm in a given year.

We merge these data with detailed accounting data on the firms, using their unique fiscal identification number. The Portuguese Central Balance Sheet database covers all non-financial firms operating in Portugal. The data are sourced from *Informação Empresarial Simplificada* (IES), a joint project of the Ministry of Finance, Ministry of Justice, Statistics Portugal and Banco de Portugal. The aim of this

[1] Firms in the tourism sector are subject to a different set of criteria. The program is managed by a different institution, Tourism Portugal. We exclude firms in this sector from our analysis.

[2] According to EU recommendation 2003/361, for a firm to be classified as an SME it must have fewer than 250 employees and less than 50 million euros in turnover (or less than 43 million euros in total assets).

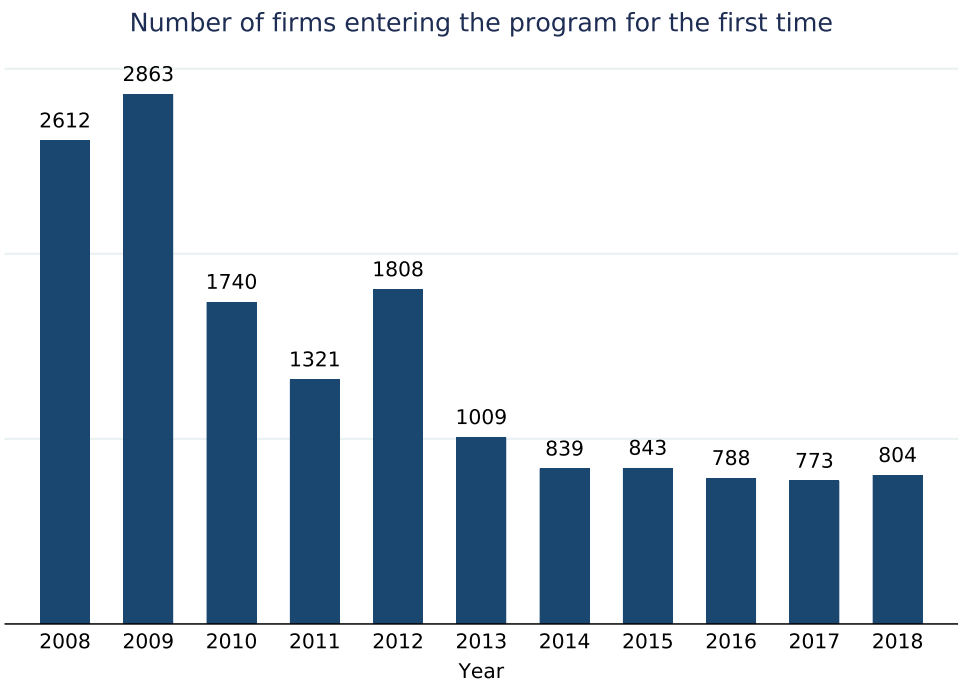

Fig. 1. This figure shows the number of firms certified as Leader or Excellence for the first time in each year.

project is to integrate most of the information that all Portuguese firms have to report for legal, fiscal and statistical purposes. This is the information used in the program to confirm whether a firm meets the eligibility criteria. Banco de Portugal revises the data for economic and statistical analysis purposes (this revised version of the data is the Central Balance Sheet database). We collect this data from 2007 to 2018.

These data, together with detailed criteria data, allow to identify all firms that are eligible for the program. The granular and detailed information in the financial dataset also allows us to measure firm outcomes, including investment, employment and sales growth.

We merge these data with the Central Credit Register dataset, owned and managed by Banco de Portugal. This includes monthly information on all loans outstanding in Portugal, granted by resident credit institutions. The reporting threshold is among the smallest in the world (50 euros). This virtually universal coverage is key for the analysis of SME financing. Most credit registers worldwide typically have higher reporting thresholds, sometimes excluding smaller firms from the analysis. This dataset has information on the total outstanding bank loans of each firm and on the status of each loan (for instance, whether it has become overdue or was renegotiated). There is also information on unused credit lines, loan products, maturity and collateral.

Finally, we collect data on interest rates using a database on loan flows, available at Banco de Portugal. For each new loan originated, banks report the interest rate, maturity, existence of collateral and the loan amount. This dataset is available only since mid-2012, which implies that it cannot be used to fully assess the effects of the program in the entire period.

3. Empirical strategy

We exploit eligibility for the "*SME-Leader Program*" as a source of variation in the cost of debt for SMEs. We exploit the variation around the different criteria thresholds of the program to define a counterfactual for changes in debt, investment and employment in the absence of the subsidy and credit certification. While we observe the firms that are certified and those that are not in a given year, we do not have information on applications and therefore cannot explicitly account for selection into the program. We thus estimate an intention-to-treat (ITT) effect, i.e., we compare eligible firms with non-eligible firms around different cutoff points, defined by the multiple eligibility criteria.

We also exploit the eligibility for *SME-Excellence* as a source of variation in the credit certification level. We exploit the discontinuity between excellence-eligible and leader-eligible firms to estimate the effect of obtaining the top certification level.

3.1. Sample and summary statistics

Our main sample comprises 229,778 firm-years and 55,041 unique firms from the period 2007–2018 for which eligibility data are available. Table 1 reports summary statistics for all firms in our sample, including criteria (Panel A) and outcome variables (Panels B and C). Non-SME firms are excluded from the sample. We also exclude financial firms, not-for-profit and state-owned firms, as well as firms in the tourism sector because this sector has its own stimulus program. The median firm in our sample has 19 employees, sales of 1.29 million euros, assets of 1.28 million euros and is 18 years old.

Table 1
Summary statistics.

	Mean	Std. Dev.	p25	p50	p75	p99	Obs.
Panel A: Criteria							
Assets (EUR)	4,836,760.95	56,910,086.44	535,932.97	1,275,276.88	3,186,986.25	40,685,064.00	314,148
Employees	30.92	32.56	13.00	19.00	34.00	181.00	314,148
Sales (EUR)	2,713,561.48	3,329,354.16	568,951.34	1,285,786.19	3,251,883.50	12,368,278.00	314,148
Net income (EUR)	100,148.06	4,140,377.86	537.38	13,827.35	73,379.99	2,084,028.88	314,148
EBITDA (EUR)	314,919.40	4,246,352.82	21,734.94	81,718.81	244,062.60	3,853,076.50	314,148
Net income-to-assets	0.01	0.08	0.00	0.01	0.05	0.17	314,148
Net income-to-equity	0.09	0.27	0.01	0.06	0.17	0.83	314,148
Equity-to-assets	0.33	0.26	0.17	0.32	0.51	0.79	314,148
EBITDA-to-assets	0.08	0.10	0.03	0.07	0.13	0.29	314,148
EBITDA-to-sales	0.07	0.10	0.03	0.06	0.12	0.28	314,148
Debt-to-EBITDA	3.05	4.77	0.33	1.96	5.06	15.17	278,658
Sales growth	0.03	0.24	−0.11	0.01	0.13	0.84	314,148
EBITDA growth	−0.11	1.08	−0.47	−0.07	0.29	2.54	314,144
Firm age	19.77	11.65	10.00	18.00	27.00	45.00	314,148
Distance to threshold (Leader)	−0.35	0.76	−0.57	−0.08	0.01	0.75	314,148
Distance to threshold (Excellence)	−0.82	0.82	−1.13	−0.59	−0.27	0.20	279,489
Eligible (0/1)	0.38	0.49	0.00	0.00	1.00	1.00	314,148
Panel B: Debt and equity							
Interest rate (new loans)	0.08	0.06	0.04	0.06	0.09	0.24	131,250
Loan maturity	4.64	1.40	3.73	4.51	5.44	7.36	101,974
Probability of default	0.04	0.07	0.00	0.01	0.04	0.36	288,375
Collateral (0/1)	0.80	0.40	1.00	1.00	1.00	1.00	131,250
Bank loans (EUR)	744,886.25	1,117,679.83	71,305.18	260,886.89	829,822.81	4,177,265.50	278,662
Δ Bank loans (EUR)	27,917.26	235,817.12	−51,768.11	−1,278.66	67,153.36	682,159.81	265,020
Issued capital (EUR)	816,256.72	1,213,043.82	89,553.87	286,342.72	900,630.38	4,495,807.50	314,148
Δ Issued capital (EUR)	9,140.34	29,153.32	0.00	0.00	0.00	120,001.41	314,148
Panel C: Other firm variables							
Δ Total assets (EUR)	66,913.84	413,649.21	−82,110.24	11,740.47	154,724.14	1,207,098.00	314,148
Fixed assets (EUR)	677,615.39	980,881.83	66,170.61	250,897.38	791,123.00	3,612,822.75	314,148
Δ Fixed assets (EUR)	3,163.87	112,342.54	−35,197.62	−7,047.73	15,037.27	332,044.22	314,148
Working capital (EUR)	1,178,338.66	1,525,526.84	211,604.21	542,378.25	1,406,523.75	5,692,936.50	314,148
Δ Working capital (EUR)	38,264.15	289,009.51	−58,614.89	13,309.93	113,089.30	790,215.50	314,148
Δ Employees	0.20	3.73	−1.00	0.00	2.00	9.00	314,148
Wage(EUR per worker)	17,609.59	11,363.23	11,444.12	15,103.22	20,430.17	57,390.84	314,146
Δ Wage (EUR per worker)	256.70	2,109.08	−854.06	237.54	1,365.85	4,797.97	314,146
Δ Sales (EUR)	37,834.96	528,988.52	−141,064.44	5,334.27	172,440.11	1,423,912.00	314,148
Exports (EUR)	635,625.76	2668,229.29	0.00	0.00	168,376.16	11,410,318.00	314,148
Δ Exports (EUR)	13,802.99	148,627.95	0.00	0.00	2,000.00	452,253.00	314,148
Default	0.16	0.36	0.00	0.00	0.00	1.00	298,535
MRPK	1.54	26.37	0.72	1.13	1.73	7.06	314,148

This table shows the summary statistics for the full sample of firms from 2007 to 2018. EBITDA is defined as earnings before interest, taxes, depreciation, and amortization. The distance to threshold is computed according to the methodology described in Section 3.2. Firm age is reported in years. Eligible takes the value 1 if the firm meets the eligibility criteria for Leader in a given a year. Wage corresponds to the average wage per worker. Interest rate on new loans, loan maturity and collateral are available from 2012 on-wards. The probability of default is obtained from Banco de Portugal's internal credit risk model. MRPK is the marginal revenue product of capital, which we measure as the ratio of sales to the book value of assets.

Table A2 in the appendix shows the summary statistics for *SME-Leader* and *SME-Excellence* eligible, non-eligible and certified firms. Overall, Leader and Excellence certified firms are larger and better performing. This is consistent with the notion that firms become eligible for the program based on accounting performance, credit quality and size.

3.2. Methodology

We use a multidimensional regression discontinuity design (MRDD) to estimate differences in debt, investment and employment between eligible and non-eligible firms. Therefore, the analysis is restricted to a set of firms that lie around the eligibility threshold. In other words, we compare firms that are eligible for the *SME-Leader Program* but only meet the criteria by a small margin with firms that are not eligible for certification by a small margin. The firms 'just below the threshold' are used as the counterfactual for firms that are 'just above the threshold'. Analogously, to estimate the top rating effect, we compare firms that are eligible for the SME-Excellence certification by only a small margin with the firms that were not eligible for the top rating by a small margin. In a one-dimensional regression discontinuity design, the sample bandwidth definition and distance to threshold are determined by a single criterion. In a multidimensional design, there are multiple criteria and multiple thresholds. Therefore, we need to define a single running variable and threshold. We define the distance to threshold of a given firm in a given year using the criterion that is the most binding.[3] We follow

[3] As an example, to be eligible for the program in a given year, firms must have positive net income and an equity-to-assets ratio greater than or equal to 25%. For a firm that has positive net income that is very close

Fig. 2. This figure shows the first order polynomial fit of regressing the treatment variable (certification as *SME-Leader* or *SME-Excellence*) on the distance to threshold.

the approach of Ferreira et al. (2018) to define the binding distance to threshold across criteria. We first calculate the distance to threshold for all criteria and standardize these distances. Second, we define the binding criterion as that with the greatest distance to threshold. Last, we aggregate the standardized distances to threshold across criteria to define the running variable (standardized distance to threshold). Figure 2 shows that the probability of being treated (certified as *SME-Leader* in this case) significantly increases at zero for our running variable. The econometrics literature on regression discontinuity design provides detailed guidance on the choice of the optimal bandwidth (Imbens and Kalyanaraman, 2012); the choice of the local polynomial order to include in the regression (Pei et al., 2020); and the inclusion of covariates (Frölich and Huber, 2019). We follow Calonico et al. (2014) regarding the choice of the optimal bandwidth.

Formally, we estimate the following model:

$$y_{it} = \beta v_{it} + \sum_{p=1}^{P} [\gamma_{p0} + \gamma_{p1} v_{it}] D^p + \epsilon_{it} \quad (1)$$

where y_{it} is a firm outcome (e.g., the interest rate on new loans), v_{it} is an indicator variable that takes value 1 if a firm is eligible as *SME-Leader* in year t (i.e., $v_{it} = 1 \; if \; D_{it} \geq 0$) and $\sum_{p=1}^{P} [\gamma_{p0} + \gamma_{p1} v_{it}]^p$ is a polynomial of order P of the distance to threshold. The coefficients γ_{p0} and γ_{p1} can differ on the left- and right-hand sides of the threshold. In the estimation, we follow Calonico et al. (2017).

The coefficient of interest is β, which measures the average difference in the outcome variable y_{it} between eligible and ineligible firms as determined by the program criteria in year t. A positive coefficient indicates that the average of the outcome variable for eligible firms is larger than for non-eligible firms. Because there is only partial take-up of the program, β is an ITT estimate. The treatment-on-the-treated (TOT) estimate is obtained by scaling up the ITT by the take-up rate.

to zero and an equity-to-assets ratio of 100%, the most binding criterion is net income.

An underlying assumption in the regression discontinuity design is that firms' assignment around the eligibility threshold is as good as random and that y_{it} would be a smooth function around threshold absent treatment (*local continuity assumption*). This implies that firms do not manipulate their financial statements to meet the program criteria. The design of the program arguably makes it difficult to manipulate eligibility for the following reasons: 1) the program is subject to multiple criteria, and these change on a yearly basis; 2) the eligibility criteria for a given year are always based on the financial statements of the previous year and only announced after the date on which firms have to file their financial reports with the authorities; 3) all SMEs must have a certified accountant who files and signs the financial reports; and 4) there are penalties for late filing of financial reports, and firms must pay fees to file for restatement. In Portugal all firms, irrespective of size, must submit detailed financial statements (balance sheet, income statement and cash flow statement) to the authorities.

A second implication of this identifying assumption is that program thresholds are not standard restrictions to participate in other programs or subsidies. This is indeed the case, which reduces the concern of sorting around the cutoff points. Although the local continuity assumption cannot be formally tested, we study the distribution of eligibility criteria around each of the cutoff points using McCrary tests (McCrary, 2008). Figure 3 shows the distribution of criteria around cutoff points for the year before their introduction into the program.[4] Overall, we do dot find significant discontinuities around the relevant thresholds except for *Net Income*. Earnings discontinuity has been extensively documented in the accounting literature (see, for instance Burgstahler and Dichev, 1997; Beaver and Nelson, 2007, who show that discontinuity in earnings can be observed in the absence of discretion). This observed discontinuity in net income is thus plausibly unrelated to the program. Nevertheless, because firms above and below the cutoff for *Net Income* may systematically differ in other observable and unobservable characteristics, in Section 5 we conduct robustness tests where we exclude *Net Income* as a criterion, as well as other criteria with the lowest p-values in the density tests.

Other potential bias in our estimates might arise from the choice of bandwidth and polynomial order. We discuss this possibility in greater detail in Section 5 and perform robustness tests where we choose alternative bandwidths and polynomial orders.

4. Results

We use the *SME-Leader* program to estimate the sensitivity of investment and employment to the cost of debt financing. The first step in our empirical evaluation is to examine changes on the cost of debt financing to check whether firms have access to less expensive bank financing due to the government guarantees and credit

[4] We also present p-values of Cattaneo et al. (2018) density tests for discontinuity around the thresholds in Table A3 in the appendix.

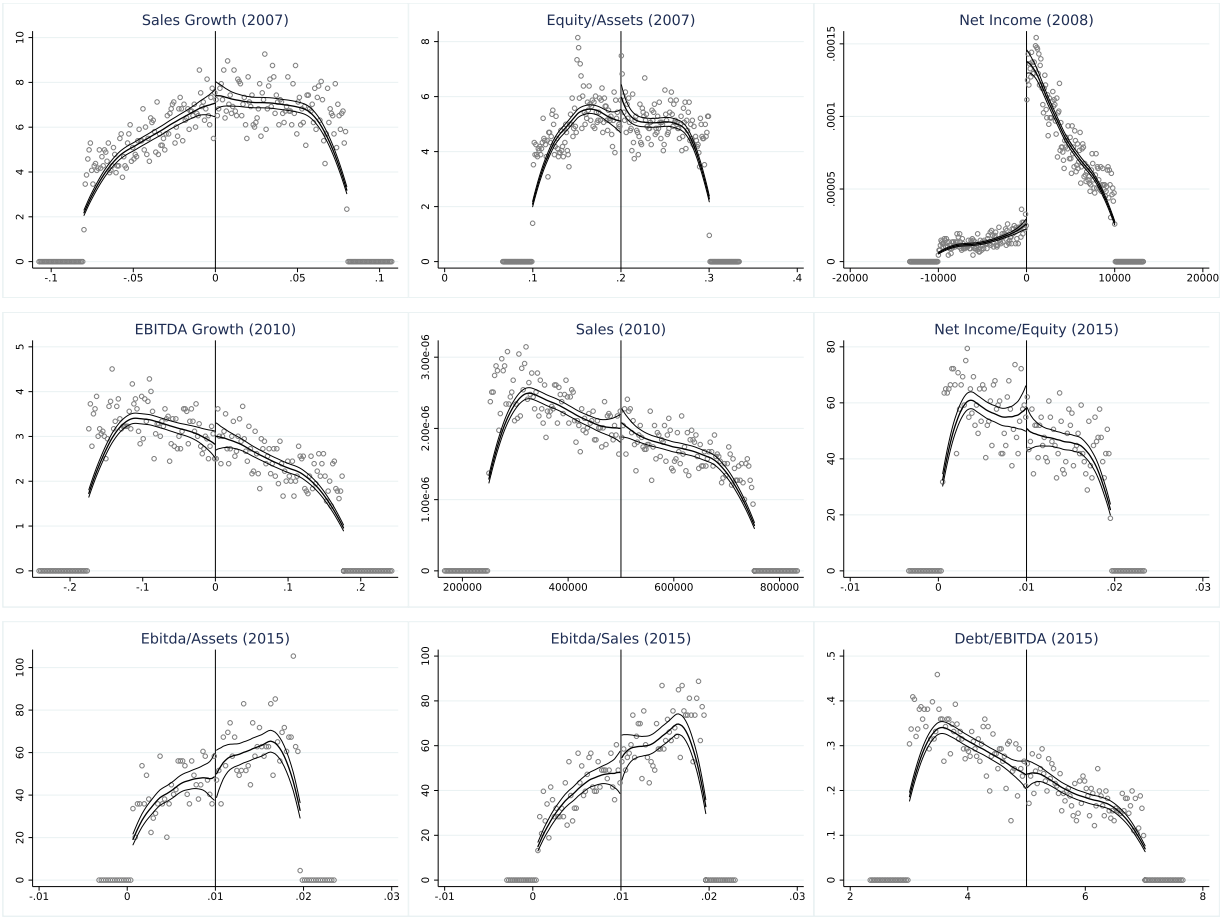

Fig. 3. McCrary plots - density tests around the thresholds. This figure shows the McCrary plots for the density test around the thresholds of eligibility criteria, on the year before the criteria is first introduced.

certification that are offered by the program. Second, we consider financial responses: changes in debt and equity financing. Third, we analyze investment and employment effects, and sales growth. We perform this analysis during the crisis period and the post-crisis period and across sub-samples of firms expected to face different levels of financing frictions.

4.1. Cost of debt financing and financial responses

Firms that are eligible to participate in the program have significantly lower costs of debt financing during the crisis period. Table 2 shows the results from estimating Eq. (1) using loan flow data. Columns (1) and (2) show the impact of the program on the cost of new loans. The cost of debt for eligible firms is 1.8 pp lower than for non-eligible firms during the crisis period (Panel A, column 1). Figure 4 shows that this decrease corresponds to a drop from approximately 11 pp for non eligible firms to 9 pp for eligible firms, around the eligibility threshold. As the loan flow data are available since 2012 and the average take-up rate for the 2012–2013 period is 60%, the TOT effect is up to 3 pp. These effects are persistent and of similar magnitude one year post-certification, but not during the post-crisis period (Panel B). Table A4 shows that the estimated effect on the interest rate is similar when loan-level covariates are included in the estimation.

The decrease in interest rates is perhaps not surprising, as certified firms are offered subsidized loans through the government guarantee. However, other factors could lead to a different result. For instance, it could also be that 1) unconstrained firms simply substitute existing loans for loans at the new subsidized rate without increasing their borrowing; 2) firms expand their borrowing beyond the credit offered through the program at a higher cost; 3) banks participating in the program capture the subsidy and lend at market rate; or 4) banks lend through the program to constrained firms at market rates, and these firms increase their borrowing. As such, the overall impact of the program on the cost of financing for firms is not trivial ex-ante. Actually, in the recovery period there seems to exist a marginally positive effect on interest rates.

In terms of the maturity of new loans, we find that eligible firms have shorter loan maturity. The estimated ITT coefficients are −0.213 for the year of eligibility and −0.124 the year after (columns (3) and (4)). This corresponds to a contemporaneous decrease in debt maturity of approximately 4 months. This effect is persistent over time and is also visible in the recovery period. Though the program could arguably lead to better loan terms and conditions,

Table 2
Financing conditions and default probability.

	Interest rate (new loans)		Loan maturity		Collateral (new loans)		Probability of default	
	T	T + 1	T	T + 1	T	T + 1	T	T + 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 2012–2013								
Eligible	−0.018***	−0.016***	−0.213***	−0.124**	0.070***	0.084***	−0.003***	−0.004***
	[0.002]	[0.002]	[0.060]	[0.045]	[0.014]	[0.010]	[0.001]	[0.001]
Obs.	17,662	26,200	13,117	21,659	17,326	30,271	95,604	92,967
Bandwidth	0.182	0.119	0.149	0.152	0.169	0.220	0.176	0.218
Panel B: 2014–2018								
Eligible	0.003*	0.000	−0.207***	−0.096*	0.018*	0.025*	−0.001	−0.002*
	[0.001]	[0.001]	[0.043]	[0.043]	[0.009]	[0.010]	[0.001]	[0.001]
Obs.	42,966	31,858	32,097	26,173	44,271	33,303	46,936	35,268
Bandwidth	0.151	0.135	0.112	0.146	0.172	0.165	0.123	0.133

This table shows the intention to treat estimates for the impact of firm certification as Leader/Excellence on the interest rate on new loans (columns (1) and (2)), loan maturity (columns (3) and (4)), the collateral on new loans (columns (5) and (6)) and the probability of default (columns (7) and (8)). Panel A reports results for the period 2012–2013 (except in columns (7) and (8), where the period is 2008–2013) and Panel B reports results for the period 2014–2018. Columns (1), (3), (5) and (7) show estimates where the dependent variable is observed at the year of award and columns (2), (4), (6) and (8) one year after the award. All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

we should note that the certification is valid only for one year. Banks may thus become less willing to offer loans with longer maturities, that go beyond the duration of the certification.

Columns (5) and (6) report the results for collateral on new loans. We find that the use of collateral in bank loans is greater for eligible firms than non-eligible firms around the eligibility threshold. This is expected because all the loans granted through the program have an associated government guarantee, which makes them classified by the bank and in the data as having collateral.

Columns (7) and (8) show the impact on the probability of default. These probabilities are estimated in an internal credit risk model managed by the Banco de Portugal. We find that eligible firms show a significantly lower probability of default than in the counterfactual during the crisis (Panel A). The coefficient is −0.003 in the year of eligibility and −0.004 one year after. The TOT is thus between 0.005 pp and 0.007. We do not find an effect in the post-crisis period (Panel B).

To evaluate whether firms increase their borrowing, we examine changes in bank loans. Because firms can access other sources of financing such as equity, we also investigate changes in issued equity. Table 3 shows the results from estimating Eq. (1) using balance sheet data to measure financial responses. We estimate these effects in levels but also in logarithmic transformation to mitigate the impact of potential outliers.

Columns (1)–(4) report the results for changes in bank loans. We find that firms that are eligible for the program increase their borrowing relative to non-eligible firms during the 2008–2013 period (Panel A). The estimated ITT effect for contemporaneous variables is 6784 EUR, which represents 2.9% of a standard deviation. The effect is stronger at 21,742 EUR one year after, representing 9.2% of a standard deviation. The TOT effect for an average take-up rate of 36% during the crisis period is 18,844 EUR for the eligibility year and 60,394 EUR one year after. The effects on the log-transformed variable are similar across the two periods. The estimated difference in growth rates is between 7.5 pp and 7.7 pp. From Fig. 4, we see that the increase at the eligibility cutoff point is of similar magnitude. The TOT effect for the log-transformed variable is estimated at 0.21. Figure A1 shows similar regression discontinuity plots for the variables without the log transformation.

The estimated effects are overall smaller for the period post-2013 (Panel B). The estimates in levels are statistically significant with a magnitude of 11,715 EUR for the eligibility year and 10,517 EUR for one year after (columns (1) and (2)). The log-transformed variable results are reported in columns (3) and (4) and are not statistically significant. The smaller and imprecise post-crisis effects suggest that target firms do not benefit as much from the subsidy in such periods, as their probability of default is low enough when the economy is doing well. When credit supply is not constrained, these firms do not face difficulties in access to bank loans.

Last, we examine equity issues, as firms might also respond to this debt subsidy by issuing equity to readjust their capital structure or as a necessary complement to finance investment. Columns (5)–(8) report the results. We find modest effects on equity issues mostly during the crisis (Panel A). The estimated effects in levels are significant at 1% level and between 1382 EUR and 1630 EUR during the crisis, but mostly not significant for the log-transformed variable. These estimated effects are of an order of magnitude and significantly smaller than the loan effects. For this reason, and because only a small fraction of firms issue equity in a given year (less than 25%), we do not regard them as first-order effects.

Taken together, these results show that the program effectively changes targeted SMEs' access to credit, allowing them to borrow more and at significantly lower rates,

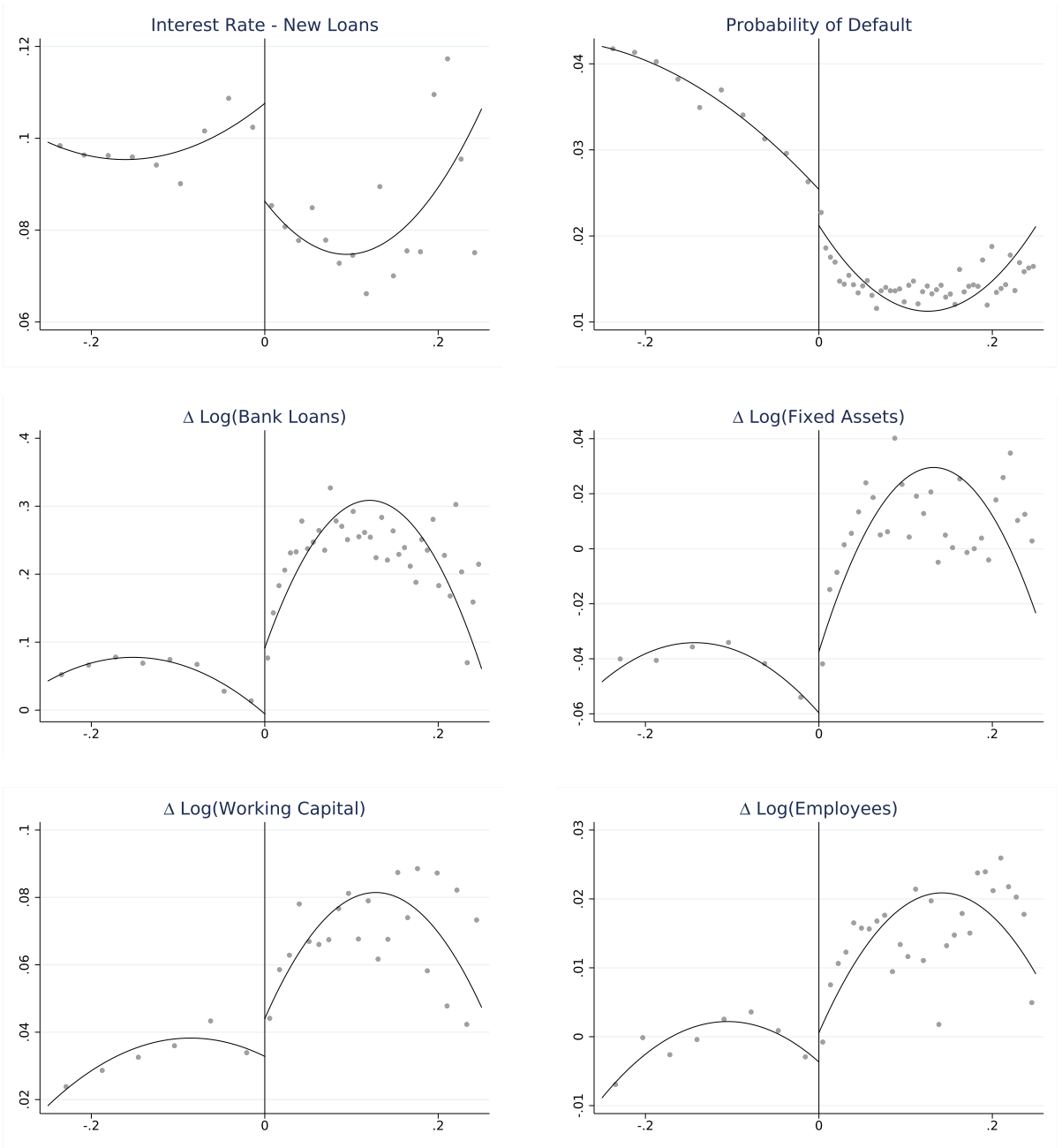

Fig. 4. Regression discontinuity plots (2008–2013). This figure shows RD plots for firm-level outcomes. The bandwidth is fixed at −0.25 to +0.25 for all variables. The order of the polynomial used is 2. The time-period considered for estimation is 2008–2013.

but mostly during the crisis period. When credit supply is abundant, the lending effects of the program become more muted. In a competitive credit market in an economy on a recovery trajectory, banks are expected to increase lending, notably to the best performing firms in the economy.

4.2. Investment and employment

In the previous section, we show that eligible firms increase their borrowing by more than non-eligible firms because they have access to significantly lower interest rates during the crisis period. In this section, we test whether eligible firms increase their investment and employment by more than non-eligible firms and calculate its sensitivity to the change in the interest rate.

Table 4 shows the results from estimating Eq. (1) using investment as the main outcome variable. The results for changes in total assets are shown in columns (1)–(4), those for changes in fixed capital appear in columns (5)–(8) and those for changes in working capital are in

Table 3
Financial responses.

	Δ Bank loans		Δ Log(Bank loans)		Δ Issued Equity		Δ Log(Issued Equity)	
	T	T + 1	T	T + 1	T	T + 1	T	T + 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 2008–2013								
Eligible	6784	21,742***	0.075***	0.077***	1630***	1382***	0.003	0.005
	[3747]	[3506]	[0.011]	[0.009]	[347]	[375]	[0.005]	[0.005]
Obs.	70,792	76,605	72,272	80,027	110,188	103,666	36,291	35,000
Bandwidth	0.141	0.204	0.152	0.235	0.268	0.300	0.193	0.220
Panel B: 2014–2018								
Eligible	11,715**	10,517*	0.010	0.017	833*	850	−0.009	0.016*
	[3792]	[4241]	[0.009]	[0.010]	[378]	[444]	[0.007]	[0.006]
Obs.	57,893	44,269	65,250	48,694	66,028	49,363	20,931	16,512
Bandwidth	0.172	0.197	0.285	0.293	0.206	0.188	0.141	0.184

This table shows the intention to treat estimates for the impact of firm certification as Leader/Excellence on bank loans growth (columns (1)–(4)) and issued equity growth (columns (5)–(8)). Panel A reports results for the period 2008–2013 and Panel B reports results for the period 2014–2018. Columns (1), (3), (5) and (7) show estimates where the dependent variable is observed at the year of the award and columns (2), (4), (6) and (8) one year after the award. All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4
Investment.

	Δ Total assets		Δ Log(Total assets)		Δ Fixed assets		Δ Log(Fixed Assets)		Δ Working capital		Δ Log(Working Capital)	
	T	T + 1	T	T + 1	T	T + 1	T	T + 1	T	T + 1	T	T + 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: 2008–2013												
Eligible	26,008***	32,777***	0.012***	0.006**	3,521**	3,376**	0.018***	0.019***	9,840**	14,700***	0.011**	−0.001
	[4785]	[4706]	[0.003]	[0.002]	[1322]	[1279]	[0.005]	[0.004]	[3418]	[3190]	[0.004]	[0.004]
Obs.	106,038	104,456	108,879	113,024	108,,322	108,573	103,168	109,613	104,355	108,937	114,192	116,816
Bandwidth	0.240	0.307	0.259	0.384	0.255	0.343	0.221	0.352	0.229	0.346	0.317	0.454
Panel B: 2014–2018												
Eligible	6881	19,395*	0.004	0.002	−559	1723	−0.006	−0.001	7699	8654	0.013**	0.006
	[7109]	[7996]	[0.003]	[0.004]	[2159]	[2492]	[0.006]	[0.007]	[4771]	[5431]	[0.005]	[0.005]
Obs.	60,509	47,075	62,020	47,748	61,353	46,972	63,842	48,305	62,144	47,715	63,389	48,840
Bandwidth	0.136	0.151	0.154	0.161	0.147	0.149	0.177	0.170	0.155	0.160	0.181	0.189

This table reports the intention to treat estimates for the impact of firm certification as Leader/Excellence on total assets growth (columns (1)–(4)), fixed assets growth (columns (5)–(8)) and working capital growth (columns (9)–(12)). Panel A reports results for the period 2008–2013 and Panel B reports results for the period 2014–2018. Columns (1), (3), (5), (7), (9) and (11) show estimates where the dependent variable is observed at the year of award and columns (2), (4), (6), (8), (10) and (12) one year after the award. All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

columns (9)–(12). Eligible firms invest significantly more in total assets than non-eligible firms during the crisis period (Panel A). The ITT estimate is 26,008 EUR when considering the year of eligibility and 32,777 EUR the year after, which represent 6% and 8% of a standard deviation change in total assets, respectively. The TOT effect for a take-up rate of 36% corresponds to 72,244 EUR and 91,047 EUR, respectively. When considering the log-transformed variable for changes in total assets, which mitigates the effect of potential outliers, we find that the effect is varies between 0.6 and 1.2 pp. A difference of 1.2 pp evaluated at the mean value of total assets corresponds to 58,041 EUR. The post-crisis estimates for changes in total assets presented in columns (1) and (2) in Panel B are not significant.

Next, we analyse investment in fixed assets. We find that eligible firms significantly increase their fixed assets during the crisis period by 3521 EUR more than non-eligible firms (Panel A). This effect is similar in magnitude one year after eligibility is considered (3376 EUR). These effects represent 3% of a standard deviation. The TOT effect is 9781 EUR for contemporaneous variables and 9378 EUR when the year after eligibility is considered. The results are robust to using the log transformation of the variable, with a significant coefficient of 1.8 pp in the year of certification and 1.9 pp one year after. A difference of 1.9 pp in fixed as-

Table 5
Employment.

	Δ Employees		Δ Log(Employees)		Δ Wages		Δ Log(Wages)	
	T	T + 1	T	T + 1	T	T + 1	T	T + 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 2008–2013								
Eligible	0.136**	0.247***	0.004*	0.010***	33	68*	0.003*	0.004*
	[0.045]	[0.044]	[0.002]	[0.002]	[27]	[27]	[0.002]	[0.002]
Obs.	115,530	115,264	114,434	110,721	113,914	107,818	115,838	113,071
Bandwidth	0.308	0.406	0.299	0.363	0.296	0.336	0.310	0.384
Panel B: 2014–2018								
Eligible	0.115	0.167*	0.004	0.005	−12	−60	−0.000	−0.002
	[0.066]	[0.072]	[0.002]	[0.003]	[37]	[40]	[0.002]	[0.002]
Obs.	61,837	48,615	62,485	49,185	65,243	50,602	64,917	52,588
Bandwidth	0.152	0.176	0.159	0.185	0.195	0.211	0.191	0.250

This table reports the intention to treat estimates for the impact of firm certification as Leader/Excellence on the evolution of the number of employees (columns (1)–(4)) and wage growth (columns (5)–(8)). Panel A reports results for the period 2008–2013 and Panel B reports results for the period 2014–2018. Columns (1), (3), (5) and (7) show estimates where the dependent variable is observed at the year of award and columns (2), (4), (6) and (8) one year after the award. All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

sets growth evaluated at the mean represents 12,875 EUR. The post-crisis estimates for fixed assets growth are presented in Panel B and are not statistically significant.

Last, we show the results for investment in working capital. We find that eligible firms increase their investment in working capital by more than non-eligible firms. The estimated ITT coefficient is 9840 EUR when considering contemporaneous effects and 14,700 EUR when one year after eligibility is considered. This is admittedly small at only 1% of a standard deviation. The TOT is 27,333 EUR and 40,833 EUR, respectively. The results are only robust to using the log-transformed variable for the first year with an estimated coefficient of 1.1 pp, which represents 12,961 EUR evaluated at the mean value of working capital. Working capital is the only investment variable with post-crisis effects (Panel B). The effects are very similar in terms of magnitude to those obtained for the crisis period, but they are not persistent.

Overall, these results suggest that eligible firms increase their investment by more than non-eligible firms and that these effects are more pronounced during the crisis. The improved access to bank loans in a period of credit supply contraction ensured that targeted firms continued to invest during a prolonged crisis (or that, at least, they did not decrease investment as much as other firms that had a similar starting point but could not benefit from the support program). This is confirmed with survey evidence: 36% of SME Leader firms report that the program helped them boost investment (31% mention that it also fostered innovation).[5]

Table 5 shows the results for employment. Column (1) shows that eligible firms increase their growth in number of employees compared to non-eligible firms by an additional 0.136 employees during the period of the crisis (in Panel A). This effect is persistent for one year after the award at 0.247 employees during the crisis. The magnitude of these effects ranges between 4% and 7% of a standard deviation. This represents a TOT effect of between 0.38 and 0.69 employees during the crisis for an average take-up rate of 36%. We do not find significant employment effects in the post-crisis period. Figure 4 shows that during the crisis this difference means that eligible firms are retaining more of their employees than non-eligible firms, as opposed to hiring more than non-eligible firms.[6] The regressions with the log-transformed variable show consistent results, with significant estimates between 0.004 and 0.01. These estimates represent between 0.12 and 0.31 employees evaluated at the mean. Survey evidence confirms these findings, though only 12% of the firms mention that the program helped them to create new jobs.

Columns (5)–(8) show the effects for the wage growth rate. Wages per worker in eligible firms grow by up to 68 EUR one year after the award during the crisis. This represents 3.2% of a standard deviation change in wages per worker, and a TOT of 189 EUR. When using log transformed variables, we find that wages per worker grow by up to 0.4 pp more during the crisis period than those of non-eligible firms (Panel A). This result is not significant during the period post-crisis (Panel B).

Overall, we find evidence that firms in the program make use of newly borrowed funds through the program to invest in fixed capital and working capital, as well as retain employees during the crisis. Post-crisis period effects are negligible, and wage effects are modest. Our results show that, during crises, a 1 pp decrease in the cost of debt financing is associated with contemporaneous increases of 0.67 pp in total asset growth, 1 pp in fixed asset growth, and 0.61 pp in working capital growth. A 1 pp decrease in the cost of debt financing is also associated with a contemporaneous increase in employment growth of 0.22 pp.

[5] Details on the survey can be found in Section 6.6

[6] Figure A2 graphically displays this estimate in levels for a fixed bandwidth of 0.25 and suggests that at the cutoff, eligible firms keep their employee changes at zero while non-eligible firms reduce their number of employees by 1.

4.3. Identification tests and running variable definition

Our main identifying assumption is the *local continuity assumption*. This implies that firm assignment around the different eligibility thresholds is as good as random and that firms' outcomes of interest, including interest rates, borrowing and investment, would all be a smooth function around thresholds absent treatment.

This also implies that firms do not manipulate their financial statements to meet the program criteria. Despite the design of the program making it arguably difficult to manipulate eligibility for the reasons we discussed in Sections 2 and 3, we conduct additional tests where we exclude *Net Income* as a criterion, as well as the criterion with the lowest p-values in the density tests (see Table A3). Table 6 shows the results excluding *Net income* (Panels A and B) and *Net Income, Equity/Assets* and *EBITDA growth* as criteria (Panels C and D). In these tests, we estimate our main results only for firms that meet those criteria, and therefore use only the variation around the remaining thresholds, restricting the sample of firms we use to estimate our main effects. We find a decrease in interest rates on new loans between 1.4 and 1.5 pp., an increase in borrowing growth rates between 5.6 pp and 6.8 pp, an increase in fixed asset investment between 1.2 pp and 1.9 pp, and an increase in employment growth between 0.8 and 0.9 pp. These magnitudes are in line with our previous estimates. That said, the positive coefficient on interest rates during the recovery period is no longer statistically significant.

In Panels E-F of Table 6 we remove *Net income* from the set of criteria used to construct a simplified running variable, and focus on a set of four financial variables: *sales, sales growth, EBITDA growth*, and *Equity/Assets*. In Panels G-H we further simplify the running variable to exclude Equity/Assets. Figure 5 shows the density tests and first stage regressions for these simplified running variables. We face the following trade-off while simplifying the running variable. On the one hand, by excluding some of the criteria from the running variable we are able to decrease bunching around the eligibility threshold, as bunching is mostly associated to net income. On the other hand, by ignoring some of the criteria we compromise on the first stage regression, as we ignore some of the eligibility criteria. We can have firms to the right of the cutoff point that are not eligible to the program. Using all criteria, as we did in baseline estimates, also has the advantage of increasing the external validity of the results, as more and different firms lie around different criteria thresholds.

The results using the simplified running variable are qualitatively similar to our previous estimates. The magnitudes of the financial effects are weaker, which is possibly explained by a weaker first stage. Overall our estimates are robust to using a selected sample and running variables for which no bunching around the threshold is observed.

4.4. Sales growth

One of the stated objectives of the program is to promote the growth of targeted firms. Table 7 shows evidence on sales growth and exports. Columns (1)–(4) show that eligible firms grow their sales by more than non-eligible firms. When using changes in sales, the effect is only significant one year after the certification and during the crisis period, and it corresponds to 33,230 EUR. When using the log transformation of sales growth, the effect is significant, with a magnitude of 0.9 pp in the first year and 0.6 pp one year after. The TOT effect during the crisis is thus between 1.7 and 2.5 pp. This effect is not observed during the post-crisis period as shown in Panel B.

Columns (5)–(8) report the findings of a similar test using export growth. During the economic crisis of 2008–2013, many Portuguese firms increased their exports as a way to overcome the contraction in domestic demand. We test whether exports grew more for eligible firms during this period. We find that eligible firms increased their growth in exports by 9870 EUR more than non-eligible firms during the certification year and 12,451 EUR one year after. These represent changes of approximately 7% and 8% of a standard deviation, respectively. These effects are robust to using log transformations for the 1-year period after certification with a magnitude of 8.6 pp and a TOT effect of 24 pp. These magnitudes represent 54,664 EUR (ITT) and 152,550 EUR (TOT) evaluated at the mean value of exports. The delayed effects on exports seem reasonable because firms might have to invest or adapt to increase exports. Panel B shows these effects in the post-crisis period. We do not find significant post-crisis effects.

These results are broadly consistent with survey evidence. About 60% of the firms mention that the program allowed them to expand their customer base, though only 9% mention an expansion in foreign markets. One third of firms consider that the program increased their competitive advantage.

Overall, the support offered to targeted SMEs was helpful in promoting exports, most notably when domestic demand was hampered. It is plausible that firms used financial support to adapt their activities toward international markets (Kalemli-Ozcan et al., 2016). Moreover, the credit certification provided by the program possibly facilitated the entry into new international markets and segments, ensuring to new customers that the firm was among the best performing in its country of origin.

4.5. Persistence

Table 8 shows the result of estimating the ITT effect two years after eligibility. We find a smaller but significant negative coefficient for interest rates at 1.4 pp in column (1) of Panel A, suggesting that the effect on interest rates is persistent for at least two years during the crisis period. This might be related to the nature of the credit lines, which sometimes have maturities that go beyond the year of certification. However, we do not find a corresponding effect on loans on column (2). In fact, the coefficient is negative and significant, suggesting that firms decrease their borrowing two years after certification. The magnitude of this decrease is smaller than the previously estimated effects for the increase during the first two years. We do not find significant persistent effects on investment growth beyond two years post-certification (columns (3)–

Table 6
Alternative running variables and estimation sample.

	Net Income>0			
	Interest rate (new loans) (T + 1)	Δ Log(Bank loans) (T + 1)	Δ Log(Fixed Assets) (T + 1)	Δ Log(Employees) (T + 1)
	(1)	(2)	(3)	(4)
Panel A: 2008–2013				
Eligible	−0.015***	0.056***	0.012*	0.008***
	[0.002]	[0.012]	[0.005]	[0.002]
Obs.	22,126	64,369	93,284	94,713
Bandwidth	0.122	0.204	0.379	0.397
Panel B: 2014–2018				
Eligible	0.001	0.018	−0.009	0.001
	[0.001]	[0.011]	[0.007]	[0.003]
Obs.	28,824	43,089	43,425	44,040
Bandwidth	0.131	0.264	0.161	0.173
	Net Income>0, Equity/Assets>0.25, EBITDA growth>**0**			
	Interest rate (new loans) (T + 1)	Δ Log(Bank loans) (T + 1)	Δ Log(Fixed Assets) (T + 1)	Δ Log(Employees) (T + 1)
	(1)	(2)	(3)	(4)
Panel C: 2008–2013				
Eligible	−0.014***	0.068***	0.019*	0.009**
	[0.003]	[0.018]	[0.008]	[0.003]
Obs.	8376	22,108	32,395	30,484
Bandwidth	0.109	0.248	0.472	0.369
Panel D: 2014–2018				
Eligible	0.000	0.012	−0.000	−0.001
	[0.002]	[0.017]	[0.010]	[0.004]
Obs.	15,134	21,709	22,506	22,543
Bandwidth	0.131	0.228	0.147	0.149
	Simplified Running Variable			
	Interest rate (new loans) (T + 1)	Δ Log(Bank loans) (T + 1)	Δ Log(Fixed Assets) (T + 1)	Δ Log(Employees) (T + 1)
	(1)	(2)	(3)	(4)
Panel E: 2008–2013				
Eligible	−0.003**	0.021**	0.017***	0.014***
	[0.001]	[0.008]	[0.004]	[0.002]
Obs.	23,839	62,539	107,742	88,664
Bandwidth	0.115	0.247	0.465	0.330
Panel F: 2014–2018				
Eligible	−0.002	0.013	0.008	0.007**
	[0.001]	[0.009]	[0.005]	[0.002]
Obs.	35,886	63,583	63,989	58,908
Bandwidth	0.188	0.440	0.295	0.235

(continued on next page)

Table 6
(continued)

	Simplified Running Variable Excl. Equity/Assets			
	Interest rate (new loans) (T + 1)	Δ Log(Bank loans) (T + 1)	Δ Log(Fixed Assets) (T + 1)	Δ Log(Employees) (T + 1)
	(1)	(2)	(3)	(4)
Panel G: 2008–2013				
Eligible	−0.003**	0.016*	0.020***	0.014***
	[0.001]	[0.007]	[0.003]	[0.001]
Obs.	31,244	69,284	104,891	93,583
Bandwidth	0.085	0.260	0.436	0.341
Panel H: 2014–2018				
Eligible	−0.002*	0.012	−0.000	0.006***
	[0.001]	[0.007]	[0.004]	[0.002]
Obs.	51,403	80,081	86,293	93,279
Bandwidth	0.251	0.700	0.434	0.689

This table reports the regressions discontinuity estimates for the impact of firm certification as Leader/Excellence imposing a set of restrictions (related to the eligibility criteria), namely: positive net income (panel A); positive net income, equity/assets>0.25 and positive EBITDA growth (panel B). Additionally, this table also shows the regressions discontinuity estimates using alternative running variables. The simplified running variable is built considering only a set of four criteria (financial variables): the level of sales, sales growth, EBITDA growth, and equity/assets (panel C). The simplified running variable excluding Equity/Assets considers only a set of three criteria (financial variables): the level of sales, sales growth, and EBITDA growth (panel D). The dependent variables are: interest rate on new loans (column (1)), loan growth (column (2)), fixed assets growth (column (3)) and employment growth (column (4)). The time period considered for estimation is 2008–2013 (except in column (1), where the period is 2012–2013). All columns show estimates where the dependent variable is observed one year after the award (T + 1). All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

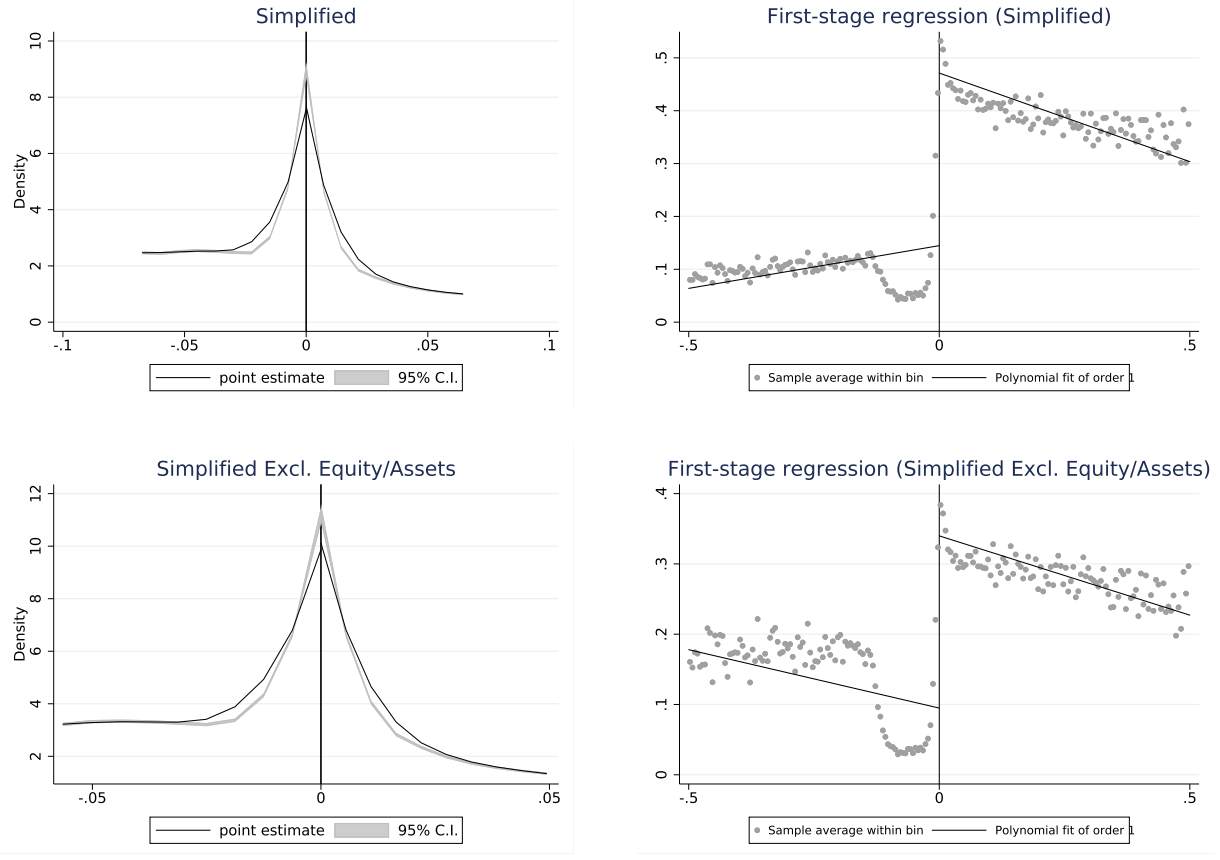

Fig. 5. Density tests of simplified running variables and first stage regressions. This figure shows the density plots around the thresholds of eligibility criteria and first-stage regressions for the simplified and the simplified excluding equity/assets running variables.

Table 7
Sales growth.

	Δ Sales		Δ Log(Sales)		Δ Exports		Δ Log(Exports)	
	T	T + 1	T	T + 1	T	T + 1	T	T + 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 2008–2013								
Eligible	80	33,230***	0.009**	0.006*	9,870***	12,451***	0.005	0.086***
	[5220]	[5663]	[0.003]	[0.003]	[1624]	[1459]	[0.021]	[0.024]
Obs.	125,034	113,584	111,148	127,215	116,593	129,193	153,015	127,466
Bandwidth	0.384	0.390	0.275	0.541	0.316	0.567	0.690	0.544
Panel B: 2014–2018								
Eligible	2005	6464	0.000	−0.002	5,614*	1955	0.013	−0.032
	[8450]	[9780]	[0.003]	[0.004]	[2442]	[2831]	[0.032]	[0.037]
Obs.	62,929	48,169	67,380	49,722	66,960	49,999	72,082	53,842
Bandwidth	0.166	0.169	0.226	0.196	0.220	0.200	0.299	0.276

This table reports the intention to treat estimates for the impact of firm certification as Leader/Excellence on sales growth (columns (1)–(4)) and exports growth (columns (5)–(8)). Panel A reports results for the period 2008–2013 and Panel B reports results for the period 2014–2018. Columns (1), (3), (5) and (7) show estimates where the dependent variable is observed at the year of award and columns (2), (4), (6) and (8) one year after the award. All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8
Persistence.

	Interest Rate (new loans) (T+2)	Δ Log(Bank loans) (T+2)	Δ Log(Assets) (T+2)	Δ Log(Fixed Assets) (T+2)	Δ Log(Working capital) (T+2)	Δ Log(Employees) (T+2)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2008–2013						
Eligible	−0.014***	−0.028**	−0.003	0.002	−0.009*	−0.006**
	[0.001]	[0.009]	[0.003]	[0.004]	[0.005]	[0.002]
Obs.	48,342	80,402	87,141	99,359	92,309	94,937
Bandwidth	0.358	0.277	0.237	0.348	0.302	0.305
Panel B: 2014–2018						
Eligible	−0.002	0.021	0.006	0.004	0.017**	0.002
	[0.002]	[0.012]	[0.004]	[0.007]	[0.006]	[0.003]
Obs.	23,851	33,841	35,703	36,627	36,944	36,555
Bandwidth	0.147	0.256	0.175	0.199	0.220	0.197

This table shows the regression discontinuity estimates of the impact of firm certification as Leader/Excellence two years after the award (T+2). Panel A reports results for the period 2008–2013 (except in column (1), where the period is 2012–2013) and Panel B reports results for the period 2014–2018. The outcome variables are the interest rate on new loans (column (1)), loan growth (column (2)), assets growth (column (3)), fixed assets growth (column (4)), working capital growth (column (5)) and employment growth (column (6)). All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(5)). The effect on employment growth is also not persistent and there is actually some reversal (column (6)).

Panel B shows the results for the post-crisis effects. Consistent with the previous tests, we do not find significant impact of the program on interest rates or borrowing rates. Interestingly, we find a positive effect on working capital during the post-crisis period. Because we do not find significant results on interest rates and debt responses, these real effects are unlikely to be a result of the debt subsidy. In the next subsection we discuss potential effects of the credit certification beyond the interest rate subsidy.

4.6. Certification effect

In this section, we test the impact of the certification as an *SME-Excellence* firm. Table 9 shows the results for financial outcomes. Top-rated firms benefit from the same formal conditions in terms of the credit guarantee, but they differ on the public rating. This may still affect the conditions offered by the sponsor bank if the rating is expected to have an impact. It may also impact borrowing conditions with other banks or the relation of firms with other stakeholders including clients and suppliers. Columns (1) and (2) show that there are no significant differences in the cost of financing around the threshold between leader-eligible and excellence-eligible firms during the crisis period (Panel A), even though financing costs are marginally smaller one year after certification in the post-crisis period (Panel B). We also find that excellence-eligible firms do not borrow more. If anything, there is some evidence that these firms borrow less (columns (3) and (4)) than non-eligible firms.

Table 9
Credit certification - financial effects.

	Interest rate (new loans)		Δ Log(Bank loans)	
	T	T + 1	T	T + 1
	(1)	(2)	(3)	(4)
Panel A: 2008–2013				
Excellence Eligible	0.005	0.009	−0.097*	−0.013
	[0.008]	[0.005]	[0.049]	[0.041]
Obs.	2029	4084	6072	7967
Bandwidth	0.205	0.244	0.233	0.289
Panel B: 2014–2018				
Excellence Eligible	−0.003*	−0.004**	0.017	0.022
	[0.001]	[0.001]	[0.015]	[0.018]
Obs.	11,273	10,198	20,972	14,836
Bandwidth	0.252	0.296	0.432	0.349

This table reports the intention to treat estimates for the impact of firm certification as Excellence when compared to Leader on the interest rate on new loans (columns (1) and (2)), and bank loans growth (columns (3) and (4)). Panel A reports results for the period 2008–2013 (except in columns (1) and (2), where the period is 2012–2013) and Panel B reports results for the period 2014–2018. Columns (1) and (3) show estimates where the dependent variable is observed at the year of award and columns (2) and (4) one year after the award. All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10
Credit certification - sales growth.

	Δ Sales		Δ Log(Sales)		Δ Exports		Δ Log(Exports)	
	T	T + 1	T	T + 1	T	T + 1	T	T + 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 2008–2013								
Excellence Eligible	10,822	−102,413*	−0.009	−0.020	22,285	−26,003	0.325	−0.069
	[45,593]	[45,027]	[0.012]	[0.013]	[13,566]	[13,922]	[0.171]	[0.130]
Obs.	7151	6657	9276	7845	5990	6301	5857	10,291
Bandwidth	0.247	0.238	0.303	0.272	0.213	0.227	0.208	0.332
Panel B: 2014–2018								
Excellence Eligible	115,638***	108,948***	0.023***	0.019***	9218	17,878**	−0.069	0.019
	[15,663]	[17,601]	[0.004]	[0.005]	[5,109]	[6147]	[0.045]	[0.051]
Obs.	15,442	15,093	6531	13,129	20,531	13,164	17,669	16,691
Bandwidth	0.263	0.333	0.286	0.287	0.370	0.288	0.312	0.375

This table shows the intention to treat estimates for the impact of firm certification as Excellence on sales growth (columns (1)–(4)) and exports growth (columns (5)–(8)). Panel A reports results for the period 2008–2013 and Panel B reports results for the period 2014–2018. Columns (1), (3), (5) and (7) show estimates where the dependent variable is observed at the year of award and columns (2), (4), (6) and (8) one year after the award. All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In Table 10 we examine the impact of the extra rating notch on firm growth. Given that we do not reject the null hypothesis for financial effects, we focus only on sales growth and export growth. The idea is that the credit certification might be a positive signal to clients and suppliers, which would allow firms to increase their sales by more. In the survey, reputational benefits are the main reason to apply to the program (with 90% of respondents mentioning that this was important or very important). Lower financing costs are the second most important reason (75%).

During the crisis period (Panel A), we do not find consistently significant credit certification effects. In Panel B, for the post-crisis period, we find robust evidence that excellence-eligible firms grow their sales by more than non-eligible firms. The magnitudes are non-negligible at 115,638 EUR for the first year, and 108,948 EUR the year after. Using log-transformed variables, the magnitudes are at an additional 2.3 pp and 1.9 pp in the certification year and one year after, respectively. Regarding export growth, we find significant effects without the log-transformed variables, one year after the award. The effects correspond to 17,878 EUR.

We conclude that most of the impact of the credit certification program during the crisis period is associated with the financial subsidy and access to less expensive bank loans and less so to the attributed credit rating. Interestingly, we do find an impact on growth associated with having the top credit rating during expansion periods. We further discuss the possible mechanism for this effect in Section 6.

4.7. Heterogeneous effects

In this section, we study the heterogeneity of the impact of subsidized credit by examining subsamples of firms that are expected to be exposed differently to financial

frictions. The size of the subsidy and the benefits from the credit certification are expected to be larger for firms that face more financing frictions. Firms that ex ante faced very little financial frictions are unlikely to derive considerable benefits from the program. Table 11 shows the results. We use default probability, size and tangibility to classify firms into groups facing high and low financial frictions. Smaller firms are expected to face more information asymmetry, while low tangibility firms have lower pledgeability of their assets.

We first show the estimates for the crisis period. Panels A and B show the results for firms with default probabilities above and below the median. We find that riskier firms experience a slightly larger improvement in financ-

Table 11
Heterogeneous effects.

	2008–2013			
	Interest Rate (new loans) (T + 1)	Δ Log(Bank loans) (T + 1)	Δ Log(Fixed Assets) (T + 1)	Δ Log(Employees) (T + 1)
	(1)	(2)	(3)	(4)
Panel A: High probability of default				
Eligible	−0.018***	0.092***	0.014**	0.006*
	[0.003]	[0.011]	[0.005]	[0.003]
Obs.	9517	35,908	53,893	49,887
Bandwidth	0.133	0.314	0.602	0.508
Panel B: Low probability of default				
Eligible	−0.015***	0.063***	0.019***	0.011***
	[0.002]	[0.014]	[0.006]	[0.002]
Obs.	18,024	46,726	67,621	68,419
Bandwidth	0.174	0.208	0.351	0.366

	Interest Rate (new loans) (T + 1)	Δ Log(Bank loans) (T + 1)	Δ Log(Fixed Assets) (T + 1)	Δ Log(Employees) (T + 1)
	(1)	(2)	(3)	(4)
Panel C: Larger				
Eligible	−0.014***	0.037**	0.018***	0.014***
	[0.002]	[0.013]	[0.005]	[0.003]
Obs.	18,677	48,441	66,696	67,499
Bandwidth	0.182	0.242	0.428	0.443
Panel D: Smaller				
Eligible	−0.007*	0.123***	0.005	0.008***
	[0.003]	[0.012]	[0.005]	[0.002]
Obs.	8750	35,806	56,644	47,594
Bandwidth	0.086	0.330	0.589	0.359

	Interest Rate (new loans) (T + 1)	Δ Log(Bank loans) (T + 1)	Δ Log(Fixed Assets) (T + 1)	Δ Log(Employees) (T + 1)
	(1)	(2)	(3)	(4)
Panel E: High Tangibility				
Eligible	−0.016***	0.104***	0.009*	0.012***
	[0.002]	[0.013]	[0.005]	[0.002]
Obs.	8750	35,806	56,644	47,594
Bandwidth	0.148	0.208	0.285	0.370
Panel F: Low Tangibility				
Eligible	−0.016***	0.024*	0.021**	0.006*
	[0.002]	[0.011]	[0.006]	[0.002]
Obs.	14,886	43,076	55,132	53,239
Bandwidth	0.122	0.430	0.525	0.474

(continued on next page)

Table 11
(continued)

	2014–2018			
	Interest Rate (new loans) (T + 1)	Δ Log(Bank loans) (T + 1)	Δ Log(Fixed Assets) (T + 1)	Δ Log(Employees) (T + 1)
	(1)	(2)	(3)	(4)
Panel G: High probability of default				
Eligible	−0.000	0.006	0.005	0.007
	[0.002]	[0.017]	[0.012]	[0.004]
Obs.	9146	13,295	12,257	15,283
Bandwidth	0.157	0.285	0.194	0.338
Panel H: Low probability of default				
Eligible	0.001	0.022	−0.002	0.005
	[0.001]	[0.014]	[0.008]	[0.003]
Obs.	23,035	33,103	36,035	36,412
Bandwidth	0.138	0.217	0.164	0.173

	Interest Rate (new loans) (T + 1)	Δ Log(Bank loans) (T + 1)	Δ Log(Fixed Assets) (T + 1)	Δ Log(Employees) (T + 1)
	(1)	(2)	(3)	(4)
Panel I: Larger				
Eligible	−0.001	0.018	0.001	0.012***
	[0.002]	[0.011]	[0.008]	[0.003]
Obs.	21,835	33,525	29,256	31,359
Bandwidth	0.154	0.394	0.163	0.216
Panel J: Smaller				
Eligible	0.005	0.066*	0.000	0.001
	[0.003]	[0.029]	[0.016]	[0.006]
Obs.	9164	12,346	14,561	13,331
Bandwidth	0.092	0.087	0.088	0.079

	Interest Rate (new loans) (T + 1)	Δ Log(Bank loans) (T + 1)	Δ Log(Fixed Assets) (T + 1)	Δ Log(Employees) (T + 1)
	(1)	(2)	(3)	(4)
Panel K: High Tangibility				
Eligible	0.001	−0.006	−0.002	0.006
	[0.002]	[0.015]	[0.007]	[0.004]
Obs.	9164	12,346	14,561	13,331
Bandwidth	0.154	0.225	0.172	0.205
Panel L: Low Tangibility				
Eligible	0.000	0.032*	−0.001	0.005
	[0.002]	[0.016]	[0.009]	[0.003]
Obs.	18,754	26,838	30,218	30,137
Bandwidth	0.152	0.252	0.219	0.216

This table reports the heterogeneity of the regression discontinuity estimates for the impact of firm certification as Leader/Excellence on: firm default probabilities; firm size; and tangibility. Panels A to F refer to the 2008–2013 period (except in column (1), where the period is 2012–2013). Panels G to L refer to the 2014–2018 period. Firm default probability is defined using the internal credit risk model managed by Banco de Portugal. Firm sized corresponds to total assets. Tangibility is defined as the ratio of tangible assets to total assets. Sample cuts are based on the sample median for all panels. The dependent variables are: interest rate on new loans (column (1)), loan growth (column (2)), fixed assets growth (column (3)), and employment growth (column (4)). All columns show estimates where the dependent variable is observed one year after the award. All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

ing conditions, with a more pronounced decrease in interest rates and a larger increase in loans. However, the real effects of the program are larger for low-risk firms, which show a more pronounced increase in investment and employment.

Panels C and D show the results for small and large firms as measured by total assets. Interestingly, we find that the effect on interest rates is more pronounced for larger firms, but that the increase in bank loans is more pronounced for smaller firms. This might be explained by banks still bearing some risk despite the guarantee or banks still capturing a larger fraction of the subsidy when negotiating with smaller firms. Regarding the use of the funds, we find that while larger firms seem to invest both in fixed capital and human capital, small firms seem to mostly invest in human capital.

Panels E and F show the results for firms with high and low tangibility with respect to the sample median. Low-tangibility firms are expected to have less pledgeable assets and therefore to benefit more from the subsidy. Contrary to this idea, we find the negative impact on interest rates on new loans to be similar for both groups, and the growth rate in bank loans to be smaller for low-tangibility firms. However, low tangibility firms show greater sensitivity in terms of fixed asset investment. These results suggest that the subsidy alleviates frictions related to a lack of pledgeable assets.

The evidence regarding heterogeneous effects is consistent with the idea that firms that lack other sources of collateral benefit the most from the subsidized credit. This is reasonable because the program offers SMEs the government guarantee as an alternative source of collateral. Interestingly, we do not find smaller firms to benefit the most in terms of the size of the subsidy. The estimated ITT effect on interest rates for the smaller firms in the sample is smaller than for large firms at 0.9 pp. Despite the modest effect in terms of interest rates, we do see the smaller firms increasing their borrowing at a higher rate (12.3 pp), which suggests that these firms were potentially financially constrained during the crisis.

We then repeat the analysis for the post-crisis period. Panels G and H show the results for firms with high and low probability of default. We do not find any meaningful differences. Panels I-J show the results for larger and smaller firms. Larger firms show positive effects on employment in the recovery period. When we compare high and low tangibility firms during recoveries (Panels K and L) we find that low tangibility firms marginally increase their borrowing, but we do not see an effect on interest rates. This result suggest that firms with low collateral might still benefit from the program in recovery times in what comes to access to financing. We do not find meaningful real effects.

Overall, these results support our initial evidence that the program mostly generates real effects during the crisis. In what is more, we do not find striking heterogeneous treatment effects based on size, risk, or tangibility during the crisis period, which suggests that amongst SME firms even the larger ones and the ones with more pledgeable assets face constraints when accessing credit during these periods. Differently, during recovery periods, we find some evidence that only smaller firms and firms with low tangibility benefit from the program by increasing their loans.

5. How sensitive are estimates to sample selection and methodological choices?

In this section, we study the sensitivity of our main estimates to sample selection and methodological choices in our main specification.

5.1. Bandwidth selection, polynomial order and inclusion of covariates

In this section, we use alternative bandwidths, which results in a different estimation sample, alternative polynomial order of the distance to threshold, and the inclusion of covariates. These results are presented in appendix A.

Table A5 presents the results when using a fixed bandwidth of 0.2, 0.25 and 0.3 and a polynomial of order 2 of the distance to threshold. As a reference, one standard deviation in our running variable is 0.72. Overall, the results are robust to using alternative and fixed bandwidths across outcomes. The investment in working capital estimate is the only one that is not robust to alternative samples. The magnitudes for all of the other outcomes are similar to those estimated with the optimal bandwidth.

Table A6 shows the results of estimating the coefficients of interest with an optimal bandwidth and including a polynomial of order 1 (Panel A) or a polynomial of order 3 (Panel B) of the distance to threshold. Overall, the estimates are similar in magnitude to those previously estimated, except again for the investment in working capital.

Figure A2 shows the regression discontinuity plots for a fixed bandwidth at −0.25 to +0.25 for all variables and an order of the polynomial of 1. The results are overall consistent with the previous estimates.

In Table A7 we include firm-level covariates to mitigate the concern that firms around the threshold differ systematically. In Panel A, we control for firm size. In Panel B, we control for firm size and the net income to equity ratio, motivated by the fact that we observe a significant discontinuity of net income around zero. In Panel C, we control for size, leverage, EBITDA/assets and age. The results across panels in this table are overall similar in size and magnitude. Compared to our previous estimates, the effect on interest rates is smaller at approximately 1.1 pp, but so is the estimate for investment in fixed assets at about 1 pp, which suggests a similar sensitivity.

Last, Table A8 shows the results of OLS estimation with a fixed bandwidth of 0.25 and an eligibility indicator. In this specification, we do not account for the distance to threshold in our estimation:

$$y_{i,t} = \alpha_0 + \beta_1 \times Eligible\ (0/1)_{i,t} + \epsilon_{i,t} \quad (2)$$

where i and t are firm and year indexes, respectively. The identifying assumption in this case is local random assignment of firms around the threshold. The effects are similar in signs and magnitudes to those previously estimated, supporting the idea that our estimates are not driven by a specific choice of RD specification or running variable.

5.2. Alternative running variables and estimation sample

In this section, we use alternative running variables to that used in our baseline results. First, we use the Euclidean distance of the criteria variables to threshold in the $\mathbb{R}^n$ space, where n corresponds to the number of criteria in each year. The Euclidean distance has the advantage of using all the criteria in a given year and does not requires the choice of a single criterion for each firm in a given year. Table A9 reports the results. Overall, the results are consistent with our baseline estimates. The financial effects are mostly significant when considering contemporaneous effects, while the real effects are mostly observed one year after eligibility. The estimated effect on interest rates is approximately 1 pp, while the impact on fixed asset growth is as large as 2.3 pp one year after the firm is eligible. The impact on employment growth is 0.7 pp.

Second, we use a single accounting variable each year to determine the distance to threshold. The selected accounting variable corresponds to the criterion that the most firms fail to achieve in a given year. Table A10 presents the results. The estimates are in line with the baseline results: for an average decrease in interest rates of 1.4 pp, investment in fixed assets increases by 3.3 pp and growth in employees increases by 1.9 pp.

Third, we exploit the introduction of new criteria into the program over time. Table A11 shows the results. In Panel A we restrict the sample to firms that meet all the previous year's criteria and only use the newly introduced criteria to define the running variable. In Panel B, we only use the newly introduced criteria in each year to define the running variable but do not impose that firms have to meet existing criteria. Overall, the results are consistent with those previously estimated.

Last, we exclude firms that were previously treated but are no longer eligible in a given year. By doing so, our counterfactual includes only firms that were never treated. Table A12 reports the results. Overall, the previously estimated results are not sensitive to excluding non-eligible previously treated firms from the estimation sample.

5.3. Firm fixed effects estimates

In this section, we present firm fixed effects regressions using the full sample to estimate ITT effects. We want to ensure that the main results are not driven by methodological choices underlying the use of an MRDD. Because in firm fixed effects estimates we exploit within-firm variation, i.e., firms that become eligible/ineligible for the program, we use the full sample period to avoid limiting this variation. All variables are observed one year post eligibility for the award. In addition to firm fixed effects (δ_i), the regressions include year (δ_t), industry-year ($\delta_{j,t}$) and region (δ_r) fixed effects. The regressions include also a set of firm-level covariates ($X'_{i,t}$): size, age, leverage and profitability:

$$y_{i,t} = \alpha_0 + \beta_1 \times Eligible_{i,t} + \gamma X'_{i,t} + \delta_i + \delta_t + \delta_{j,t} + \delta_r + \epsilon_{i,t} \quad (3)$$

where *i, t, j,* and *r* are firm, year, industry and region indexes, respectively. There are 11 administrative regions in Portugal.

The results from estimating Eq. (3) are shown in Table A13. In column (1), we report the results for financing costs estimated using data from financial statements. We employ financial data instead of loan flows data to have information for the whole sample period, and not only for the years when the firm obtains a new loan. This is relevant because with firm fixed effects, we are using the firm as its own counterfactual. We find a negative and significant coefficient of −1.2 pp, which is consistent with our RDD estimate but of smaller magnitude. In columns (2) and (3) we report the impact on bank loan growth, which increases by 8635 EUR or 0.013 pp. This result is also consistent with the RD estimate despite the smaller magnitude.

In columns (4) and (5), we estimate the impact on investment. We find a positive effect on changes in fixed assets of 6447 EUR, and 1.2 pp when using the log-transformed variable. The estimated sensitivity of investment in fixed assets to the cost of debt financing is similar: for a 1 pp decrease in interest rates, we find an increase of 0.01 pp in fixed asset investment, which is of identical magnitude to our RD estimates.

Last, columns (6) and (7) report the results for employment. We find a positive impact on changes in the number of employees of 0.168 and 0.7 pp when using the log-transformed variable. The sensitivity of employment to the cost of debt financing is larger when using firm fixed effects.

These fixed effects estimates are overall consistent with the results obtained with the MRDD.

6. Discussion and survey evidence

In this section we discuss the previous findings, including the implications of the program for firm performance and efficiency of credit allocation, and we further inform this discussion with results from a survey directed at managers of Portuguese SMEs.

6.1. Do eligible firms perform better?

Our main empirical results focus on financial outcomes, investment, employment, and sales growth. In Table 12 we examine the effects on firms' risk and profitability. We find that eligible firms become less likely to default on their bank loans during the crisis period. Performance, measured by ROA, increases for eligible firms during the crisis period when compared to non-eligible firms. Eligible firms also become more productive, when we consider the ratio of sales to the book value of assets (marginal revenue product of capital - MRPK), but only in the post-crisis period. EBITDA does not grow more for eligible firms in any of the periods considered.

Taken together, these results support positive effects on firms' performance measured by loan default rates, return on assets, and productivity. The results on default rates contrast with those in Lelarge et al. (2010), who find an increase of 6 pp in the probability of bankruptcy for a non-targeted program in France.

Table 12
Firm performance.

	Loan default T + 1	ROA T + 1	MRPK T + 1	Δ Log(EBITDA) T + 1
	(1)	(2)	(3)	(4)
Panel A: 2008–2013				
Eligible	−0.015**	0.007***	−0.129	−0.006
	[0.005]	[0.001]	[0.085]	[0.014]
Obs.	99,928	96,602	129,502	77,384
Bandwidth	0.310	0.244	0.572	0.209
Panel B: 2014–2018				
Eligible	−0.009	−0.001	0.205***	−0.000
	[0.006]	[0.002]	[0.029]	[0.015]
Obs.	44,991	44,974	51,949	45,509
Bandwidth	0.162	0.121	0.238	0.194

This table shows the intention to treat estimates for the impact of firm certification as Leader/Excellence on the firm's performance outcomes: Default, ROA, MRPK and Δ Log(EBITDA). *Default* takes the value of 1 if a firm has at least one credit overdue for more than 90 days in a given year, and 0 otherwise. MRPK corresponds to the ratio of sales to the gross book value of total assets. All columns show estimates where the dependent variable is observed one year after the award. All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13
Low interest rate firms.

	Interest rate (new loans) T + 1	Δ Log(Bank loans) T + 1	Δ Log(Fixed Assets) T + 1	Δ Log(Employees) T + 1
	(1)	(2)	(3)	(4)
Panel A: 2008–2013				
Eligible	−0.002	0.004	−0.002	−0.005
	[0.004]	[0.053]	[0.024]	[0.010]
Obs.	1663	2163	2691	2728
Bandwidth	0.119	0.235	0.352	0.363
Panel B: 2014–2018				
Eligible	0.005***	0.032	0.001	0.016**
	[0.001]	[0.021]	[0.013]	[0.006]
Obs.	9351	12,851	11,983	12,192
Bandwidth	0.135	0.293	0.170	0.185

This table reports the regression discontinuity estimates for the impact of firm certification as Leader/Excellence. The dependent variables are: interest rate on new loans (column (1)), loan growth (column (2)), fixed assets growth (column (3)), and employment growth (column (4)). All columns show estimates where the dependent variable is observed one year after the award (T + 1). All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.2. Does the program alleviate financial constraints?

In this subsection, we study wether the program affects real outcomes by loosening financial frictions that mostly affect the quantity of credit available (credit rationing) or simply by subsidizing the marginal cost of borrowing. To identify frictions that mostly affect quantities, we focus on firms that are less likely to face price frictions, i.e., firms that already borrowed at the lowest interest rates. For these firms, the partial credit guarantee should have only a very small price impact (if any). Table 13 shows the results. When we restrict the sample to firms in the lowest quartile of interest rates, we do not find significant treatment effects on interest rates (as expected), and we do not find that these firms borrow more during the crisis period. We also do not find significant real effects. This result suggests that these firms did not face meaningful quantity-specific frictions. We find a very modest positive effect on interest rates (0.005 pp) during the recovery period, which might be explained by these firms increasing their credit by more than non-eligible firms.

Following the analysis in Banerjee and Duflo (2014), one testable implication of the hypothesis that these firms were not facing severe quantity-related frictions is that, while they could still find it optimal to increase their overall borrowing if the cost of borrowing is lower, they would likely reduce their usage of unsubsidized credit, as opposed to exhausting all available sources of credit. To test whether firms substitute non-guaranteed credit with guaranteed credit we use first, we use a measure of substitu-

Table 14
Credit substitution.

	Credit substitution		Credit substitution (0/1)		Credit lines (potential amount)		Δ unused credit lines	
	T	T + 1	T	T + 1	T	T + 1	T	T + 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 2008–2013								
Eligible	−0.380	−0.131	0.036***	0.019***	63,391*	512,189*	0.192	0.040
	[0.300]	[0.203]	[0.003]	[0.004]	[29,409]	[25,727]	[0.182]	[0.098]
Obs.	85,122	96,536	125,231	112,359	111,762	109,476	86,680	91,529
Bandwidth	0.237	0.240	0.469	0.369	0.342	0.343	0.253	0.204
Panel B: 2014–2018								
Eligible	0.571	1.927	0.025***	0.009	64,009*	63,373	−0.583	−2.328
	[0.869]	[1.752]	[0.006]	[0.005]	[31,731]	[42,259]	[0.802]	[1.552]
Obs.	47,494	37,158	45,971	38,211	47,271	36,366	47,222	37,085
Bandwidth	0.201	0.234	0.161	0.264	0.184	0.213	0.196	0.231

This table shows the intention to treat estimates for the impact of firm certification as Leader/Excellence on credit substitution. *Credit Substitution* is measured as the growth in non-guaranteed credit extended to the firm, scaled by its initial total credit. Guaranteed credit includes guarantees from the public sector and financial institutions. *Credit Substitution (0/1)* correspond to an indicator variable taking the value of 1 if *Credit Substitution* is positive, and 0 otherwise. *Credit Lines (Potential Amount)* corresponds to the total unused amount of credit lines available to the firm. In the last two columns, Δ *Unused Credit Lines* represents the growth in unused credit lines, scaled by firm's initial total credit. Panel A reports results for the period 2008–2013 and Panel B reports results for the period 2014–2018. Columns (1), (3), (5) and (7) show estimates where the dependent variable is observed at the year of award and columns (2), (4), (6) and (8) one year after the award. All regressions include a polynomial of order 2 of the distance to threshold. Standard errors are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

tion by Altavilla et al. (2021). To measure the credit substitution faced by firm *i*, we considered the change in non-guaranteed credit (NGC_i) extended to that firm, scaled by its initial total credit (TC_i):

$$y_i = \frac{NGC_{i,t} - NGC_{i,t-1}}{TC_{i,t-1}} \quad (4)$$

Substitution occurs if non-guaranteed credit declines upon the firm becoming eligible, i.e., if $y_i < 0$. Hence, we measure credit substitution as the negative of y_i. As a second measure of credit substitution, we use an indicator variable that is equal to one when substitution is positive, and zero otherwise. We also examine how firms use credit lines. In the data we cannot measure with precision the use of credit lines, as this is mingled with outstanding amounts of term loans. That said, we can observe the unused amounts of credit lines.

Table 14 shows the results. We find some evidence consistent with firms substituting more expensive credit for less expensive one through the program. When we use a continuous measure of substitution, we do not find statistically significant results (columns (1) and (2)). However, when we use an indicator variable that is equal to 1 if the firm has substituted any amount of non-guaranteed credit, we do find significant effects (columns (3) and (4)). Firms that are eligible to the program are more likely to substitute non-guaranteed credit for guaranteed one. This substitution is arguably small, as it does not show up in the continuous measure.

We also find that eligible firms tend to have a larger amount of unused credit lines, thus suggesting that quantity restrictions are not an issue for these firms, on average (columns (5)–(8)). The change in the unused amount of credit lines is not statistically significant.

Overall our evidence is not consistent with the notion that these firms faced severe credit constraints, and is more in line with the interpretation that real effects are due to a cost of capital subsidy, or a price effect. To the extent to which this subsidy reduced the wedge between the internal cost of capital and the external cost of capital of the firms, some price-related frictions are possibly alleviated. Given the possibility to borrow at lower rates, firms tend to substitute existing and more expensive credit for less expensive one, at least partially, as opposed to exhausting all financing sources.

6.3. Does credit allocation improve?

In this section we study the effect of the program on aggregate total factor productivity (TFP) and credit allocation. We follow the identification strategy in Bertrand et al. (2007) and Sraer and Thesmar (2023), who estimate the contribution of banking deregulation to change in aggregate TFP, and exploit variation on treatment exposure at industry level. Industry exposure corresponds to the share of eligible firms to the program in a given industry. We look at the industry mean and variance of *log-MRPK*, where *log-MRPPK* is the natural logarithm of the ratio of sales to the book value of assets. Table 15 shows the results. Our estimation sample includes firms within a bandwidth of [−0.15 ; 0.15], and the regressions include industry and year fixed effects.

In columns (1) and (2) we show the results for the cross-sectional variance in *log-MRPK* during the crisis period (Panel A) and the post crisis period (Panel B). The estimated coefficient shows a reduction in cross-sectional variance for more exposed industries in both periods, though the post crisis effects do not seem to be persistent beyond one year. Because our measure of exposure changes on an yearly basis, the underlying assumption is that the treatment effects occur fast enough, or that industry exposure moves slowly enough. As indus-

Table 15
Moments of log-MRPK distribution around eligibility thresholds.

	Var(log-MRPK)		Mean(log-MRPK)		Cov(log-MRPK,log-GVA)	
	T	T + 1	T	T + 1	T	T + 1
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Period 2008–2013						
Industry Exposure	−0.272*	−0.237*	0.091	0.065	0.090	0.017
	[0.144]	[0.124]	[0.065]	[0.051]	[0.057]	[0.045]
Obs.	762	846	762	846	762	846
R-Squared	0.817	0.833	0.950	0.965	0.730	0.779
Panel B: Period 2008–2018						
Industry Exposure	−0.338**	−0.187	0.130**	0.051	−0.005	−0.060
	[0.155]	[0.130]	[0.058]	[0.050]	[0.042]	[0.057]
Obs.	1409	1224	1409	1224	1409	1224
R-Squared	0.759	0.803	0.942	0.955	0.689	0.708

In columns (1) and (2), the dependent variable is the variance of the firm-level natural logarithm of the ratio of sales to the gross book value of total assets (log-MRPK). In columns (3) and (4), the dependent variable is the mean of log-MRPK. In columns (5) and (6), the dependent variable is the covariance between log-MRPK and log of Gross Value Added (GVA). Industry exposure equals the share of firms eligible to the program in each industry (CAE Rev.3 - 2 digit), in each year. The sample was restricted to the bandwidth [−0.15 ; 0.15]. All regressions include industry and year fixed effects. Standard errors clustered at the industry level are reported in brackets. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

try exposure is expected to change relatively fast, because eligibility thresholds also change on a yearly basis, the relevant assumption is that treatment effects occur fast enough. This seems plausible as we are analyzing small firms, for which credit allocation and investment decisions, being also smaller, can be implemented faster. We interpret these results as being consistent with improved credit allocation.

In columns (3) and (4) we estimate the impact of industry exposure on mean(*log-MRPK*) for the crisis (Panel A) and post crisis periods (Panel B). An expansion of credit supply to previously constrained firms should lower the mean log-MRPK (Sraer and Thesmar (2023)). However, in our setting, we do not find consistent evidence of lower log-MRPK during the crisis period. Finally, in columns (5) and (6) we estimate the impact of the program on the covariance between log output (measured as gross value added) and log MRPK. We do not find statistically significant evidence of such an impact.

Overall these results are consistent with improved credit allocation in industries with greater exposure to the program likely associated with the price subsidy to targeted firms, but not with an expansion of credit supply to previously constrained firms.

6.4. What are the estimated costs and benefits of the program?

We find that the benefits of the program are more pronounced in the crisis period than in its aftermath. However, we also expect costs to be more subdued, as firms are less likely to default and activate the government guarantee when the economy is doing well. To conform this, we perform a cost-benefit analysis in the spirit of Barrot et al. (2019). We start with the costs. Ex-ante, the program does not entail costs beyond those already implied in the regular operation of IAPMEI, the public institution with the mission to support SMEs along several dimensions. Ex-post, the costs refer to the activation of the government guarantee in case of loan default. We consider a guarantee that covers 70% of loan exposure. This is a conservative estimate, as this is the highest coverage in any of the credit lines for which we could obtain information. We multiply this by the amount in credit overdue by firms that had the SME Leader or Excellence status, in the year after the certification. Based on these estimates, during the crisis years this implied potential losses for the government of around 42 EUR million per year. During the recovery years, this amount decreased to 17 EUR million. We cannot forget that the firms pay a small fee to have access to credit lines. These fees varied across credit lines. We consider a fee of 0.65%, which was common in many of the credit lines for which we have documentation. When we multiply this by the amount of new loans granted in the year after firms become Leader or Excellence, we estimate that the government received, on average, 7 EUR million during the crisis years, per year (and 8 EUR million during the recovery). The net cost was 34 EUR million per year in the crisis period and 10 EUR million in the recovery period.

To estimate the benefits, we use the coefficients reported in Tables 4 and 5. Starting with employment, we find that being eligible for the program leads to the creation of 0.136 jobs in the first year and 0.247 jobs in the following in year, during the crisis period. On average, eligible firms create 0.192 jobs per year. If we consider the take-up rate of 36% observed throughout these years, on average each firm creates 0.535 jobs per year (treatment effect on the treated). Given that there were, on average, 5465 SME Leader or Excellence firms in each of the crisis years, a total of 2924 jobs per year were created. Using the coefficients estimated for the recovery period (and a take-up rate of 63%), 1689 jobs per year were created. We can do the same exercise for investment. The estimates for the treatment effect on the treated point to an addi-

tional 81,646 EUR of investment per firm, per year, during the crisis periods. Given the number of Leader and Excellence firms during this period, this means an additional total investment of 446 EUR million. During the recovery period this number decreases to 196 EUR million. We can now compare the costs and benefits of the program. During the crisis years, the cost of each job created was 11,788 EUR. During the recovery, the cost per job was down to 5784 EUR. The cost per EUR invested was 0.08 EUR in the crisis period and 0.05 EUR in the recovery. These numbers show that even though the benefits of the program were less salient during the recovery, its targeted nature also significantly reduced the (absolute and relative) costs. These costs are significantly below those estimated for the US by Brown and Earle (2017), who find a value close to 25,000 USD (over three years), but above those document for France by Barrot et al. (2019) (3200 EUR).

By expanding on the approach proposed by Sraer and Thesmar (2023) implemented in the previous subsection to examine allocation, we can compute as well the productivity gains per unit of cost. As a first step, we calculate the impact of the program on aggregate total factor productivity (TFP) following Sraer and Thesmar (2023):

$$\Delta\log(TFP) \approx \overbrace{-\frac{\alpha}{2}\left(1+\frac{\alpha\theta}{1-\theta}\right)\sum_{s=1}^{S} k_s \Delta\widehat{\Delta\sigma^2}(s)}^{\text{Within-Industry}} \underbrace{-\frac{\alpha}{2}\left(1+\frac{\alpha\theta}{1-\theta}\right)\sum_{s=1}^{S}(\phi_s - k_s)\left(\Delta\widehat{\Delta\mu}(s) + \Delta\widehat{\Delta\sigma_{MRPK,py}}(s) + \frac{1}{2}\frac{\alpha\theta}{1-\theta}\Delta\widehat{\Delta\sigma^2}(s)\right)}_{\text{Cross-Industry}} \quad (5)$$

This formula decomposes misallocation into two terms. The first one captures how the policy change affects within-sector misallocation. The second one measures how the policy change induces cross-industry reallocation of production, which, in turn, affects aggregate efficiency. This last term corresponds to the sum of changes in industry output, weighted by the difference, for each industry, between output share and capital share. This difference is larger for more distorted industries, i.e., those with a higher sales-to-capital ratio than average. Hence, through this last term, TFP is lower if output increases more in distorted sectors.

We first calculate the within-industry reallocation effect (the first term of the expression). This term corresponds to the weighted average of predicted variance decrease (the product of the coefficient times treatment), times the term in α and θ. We assume α and θ to take the values of 0.33 and 0.83, respectively, as in Sraer and Thesmar (2023). For the predicted variance decrease we multiply the exposure of industry s to the program, calculated as the fraction of eligible firms in that industry, by our estimated treatment effect. k_s corresponds to the share of industry s in the total capital stock of the pre-program economy (measured as total assets in industry s over the economy's total assets in 2007). We compute $\Delta\Delta\sigma^2(s)$ as the aggregate industry exposure (considering all editions of the program in our sample), multiplied by treatment effect coefficient from Table 15 (columns (1) and (2)).

Next, we calculate the cross-industry reallocation effect, which corresponds to the second term of the expression. We compute ϕ_s as the share of industry s in the total gross value added of the pre-program economy. $\Delta\Delta\mu(s)$ refers to the treatment effect of the program on the mean log-MRPK in industry s and is computed as the aggregate industry exposure multiplied by the treatment effect coefficients from Table 15 (columns (3) and (4)). $\Delta\Delta\sigma_{MRPK,py}(s)$ refers to the treatment effect of the program on the covariance between log output and log MRPK in industry s and is computed as the aggregate industry exposure multiplied by the treatment effect coefficients from Table 15 (columns (5) and (6)).

Table A14 shows the within- and cross-industry reallocation, as well as the total estimated change in TFP. As in Sraer and Thesmar (2023), our estimates suggest that increases in aggregate productivity come mostly from within-industry reallocation. The effects related to cross-industry reallocation are negligible. This is true for both periods (2008–2013 and 2008–2018) and both samples (excluding and not excluding industries with less than 10 firms). We estimate that the program induced a positive change in TFP during the crisis period between 5.7 (when the sample is not censured), and 8.5 (when we exclude industries with less than 10 firms). These results are in the same order of magnitude as Sraer and Thesmar (2023) (about 3), but more pronounced.

Going back to the aggregate TFP estimations reported above, we can now compute the dollar gains in output per unit of costs. The baseline estimate of aggregated change in TFP is 8.5 for the crisis period. We multiply this by the average amount of total sales by SME Leader firms in this period. Total TFP gains in this period amount to 2.2 trillion EUR (1.3% of GDP). If we divide this by the costs of the program in this period, we find that for each euro spent, there was an increase of 66 euros on TFP.

6.5. What happens when firms stop being eligible?

Previous results show that some of the effects of the program are long-lived (Table 7). On the one hand, this is consistent with the fact that the decrease in financing costs takes time to be reflected in firms' investment and growth decisions. On the other hand, it may also reflect the fact that many firms are certified for more than one consecutive year. An important question is then to understand what happens when firms stop being eligible for the program.

In Table A15 we report the results of a fixed-effects panel estimation where we examine outcomes in the year firms stop being eligible, for the entire sample period. Fi-

nancial and real effects of the program persist for at least one year after the firm stops being eligible, as both investment and employment continue to increase. As mentioned above, this might reflect the protracted effect of relieving financing costs on firms' decisions.

6.6. How do firms perceive the program?

The design of the program and the richness of the data available allow for an encompassing and precise characterization of the financial and real effects of *SME-Leader* (and *SME-Excellence*) certification. However, not all the effects of the program may be measured by these outcomes. To further inform the discussion of the results, we complement the analysis based on our quasi-experimental setting with a survey directed at managers of Portuguese SMEs.

The aim of the survey was to collect managers' perceptions of the *SME-Leader Program* including the application process, benefits and costs. First, it includes questions to assess whether the firm applied for the program or has received any certification in the past and the motivation for doing so. This helps us understand the selection of firms into the program. Second, it includes questions on the respondents' perception of the application process, the advantages and disadvantages of the program, and the perceived impact on firms' access to and cost of credit.

The survey was distributed via email to all the firms with contact information (email address) in the ORBIS database, which includes information on the vast majority of Portuguese private firms. We conducted a web search for companies that had been certified in the past if a valid email address was not available in the ORBIS dataset to increase participation of certified firms. These cases amount to 4372 firms. A link to the online survey was emailed to all these firms.Due to quota constraints, the survey was distributed over four weeks starting in the first week of June 2020. Of the 189,135 firms invited to participate in the survey, we obtained 5413 responses, of which 3584 are complete surveys. This corresponds to a 3% response rate. The replies to the survey are reported in Appendix D.

The sample of respondents is mostly composed of firms that have never been certified (78%). From those that were certified as *SME-Leader* in the past (22%), 42% were also awarded *SME-Excellence* status (Figure D1). Using data from ORBIS, we characterize the sample of respondents according to: 1) sector; 2) firm size; and 3) geographical distribution. The sample is primarily composed of micro and small firms operating in the services sector (Figures D2 and D3). Regarding the geographical distribution, we observe a concentration of respondents along the coast of Portugal, particularly around Lisbon and Porto, and the capital cities of the autonomous regions of Madeira and Azores (Figure D4).

Among the reasons that prompted application, managers highlight the *reputation benefits* of the program (considered *very important* by 50% of the managers) (Figure D18). This confirms that the program is widely perceived to be a certification mechanism. As illustrated in Appendix C, firms widely advertise their certification, helping them to boost their reputation with customers and other stakeholders. For example, 60% of the firms consider that program helped them to expand their customer base and markets (Figure D23) and 26% consider that it gave them an advantage over competitors (Figure D24).

The second most important reason to apply is related to *lower financing costs* (considered *very important* by 38% of the managers), which is consistent with our previous results. Banks play an important role in encouraging the firms to apply: 77% of the managers reported that the *bank's proposal* was *important* or *very important* for starting the application process. Banks granting loans to certified firms benefit from significant relief in the capital requirements associated with these exposures, as the component where the risk is ultimately borne by the sovereign has attached a zero risk-weight. The certification of firm's competitors is the least relevant factor in firms' decision to apply for the program, although it is still mentioned as being at least *important* by 44% of the firms. The second less relevant reason for applying is the financing of *investment needs*. Despite the quantitative evidence of the positive effects of the program on firms' investment (Table 4), we should recall that the survey was implemented in the Summer of 2020. This was a period of high uncertainty, in which investment prospects were subdued. Moreover, in the years preceding the pandemic, credit supply to firms had been broadly unconstrained.

When we ask firms that were certified about the impact on the cost of credit, 46% of the respondents confirm that there was a decrease in funding costs (Figures D32 and D33), supporting the quantitative results obtained in the empirical estimations. However, 50% of the certified respondents report that financing costs *remained unchanged* after obtaining the certification. Out of the firms reporting a decrease in their financing costs, most report that this decrease comes from the partner bank, i.e., the bank that submitted the firms' application to the program and through which the firm can access loans with government guarantees. Nevertheless, nearly one-third of the certified firms also benefit from lower financing costs when borrowing from other banks.

When comparing the *SME-Leader* with the *SME-Excellence* certification, managers highlight *financing costs* as the main benefit of being certified as *SME-Excellence* (Figure D28). However, only 26% of the managers mention this advantage, what might explain why this perception is not supported by our quantitative analysis. Managers perceive many other benefits attached to having the top quality certification: *relationships with customers* (19%), *relationships with suppliers* (19%), and *access to markets* (9%).

6.7. Why do not all eligible firms apply?

Table A1 shows that not all firms that are eligible are certified as *SME-leader* firms. In the early years of the program, take-up rates were below 20%, possibly due to a lack of awareness about the program. Over time, take-up rates increased, with approximately two-thirds of eligible firms being certified in the most recent years.

One of the reasons why we conducted the survey was precisely to understand why not all eligible firms become part of the program. General awareness of the program in 2020, when the survey was implemented, was rela-

tively high, with 70% of the managers of non-certified firms mentioning that they knew about the *SME-Leader Program* (Figure D5). However, only 20% of these managers actively sought information about it, and even a smaller percentage (4%) had applied for certification in the past.

When asked about the reasons for not applying to the program, managers highlighted factors related to the application process. These include a lack of *compliance with the criteria* (16%), the *bureaucracy of the process* (15%) and limited *availability of manager's time* (14%) (Figure D8). Several managers also mentioned that their firm does not need financing (13%). The percentage of respondents that claim to not have applied due to the perception that the financial and reputation benefits of the program are not relevant is smaller (8 to 9%)

Banks play an important role in the promotion of the program. Nearly two-thirds of the firms first heard about the program from their bank (Figure D16). IAPMEI, the agency that administers the program, also has an important role in raising awareness about the program.

In most cases, the application process is initiated by the firm's main bank (44%) or one of the other banks of the firm (20%) (Figure D17). For 27% of the respondents, the firm started the process.

6.8. Policy implications and external validity

Government guarantees on loans to small firms (or other forms of support for SMEs) were an important tool worldwide to help firms facing sudden liquidity shocks at the onset of the COVID-19 pandemic (Gourinchas et al., 2020; Granja et al., 2020). Our results on the *SME-Leader Program* may offer relevant insights for policy in this type of setting, notably when facilitating the recovery.

The program was implemented in 2008 to mitigate the effects of the global financial crisis. The initial goal of policymakers was to ensure that the best quality SMEs were not excluded from credit markets, against a background of tighter credit supply. Nevertheless, the program remained active even when the economy was recovering both from the global financial crisis and later from the euro area sovereign debt crisis. When the pandemic started, the access to loans with government guarantees was expanded substantially beyond the universe of *SME-Leader* firms, but the technology and institutional knowledge offered by the program were helpful in quickly rolling out the loans to firms in need.

However, our results show that the program was effective in improving firms' outcomes, notably investment and employment, only while credit supply remained tight and macroeconomic conditions challenging. Once the economy started to recover, most of the effects of the program became more muted. As such, the program has a strong countercyclical effect mostly during recessions.

Although the effects of the program were smaller during the recovery period of the economy, that does not mean that it was useless. The results show that there were still some positive effects in terms of bank borrowing, asset growth (mostly through working capital), and growth in the number of employees. Moreover, during the economic recovery period, the benefits of the program accrued more from its certification component than from the subsidy (Table 10).

The targeted nature of the program is plausibly important in explaining this outcome. Most public support programs for SMEs are non-targeted, covering virtually all small firms in a country. This feature of the program allows it to offer a certification component, in a manner similar to that enjoyed by larger firms when rated by credit rating agencies. Moreover, by targeting firms with low credit risk, the design of the program seems to alleviate potential perverse incentives of banks when allocating the credit with government guarantees, mitigating excessive risk-taking. By being targeted, the program does not focus on financially constrained firms. Quite the opposite. The program targets viable firms that could nevertheless experience difficulties in access to funding due to the emergence of the global financial crisis. While this limits the external validity and the comparison to previous studies, it also sheds light on a unique program design that was much discussed during the pandemic. Untargeted support entails large fiscal costs and can promote zombification (Acharya et al., 2022). By offering causal and granular evidence on a targeted program, we show that supporting a subset of viable small firms is enough to generate substantive gains during crisis periods, while keeping costs controlled during recoveries. Given that only SMEs with low credit risk have access to these government-guaranteed loans, the fiscal costs are much smaller than those underlying a universal access program, as these firms are significantly less likely to default in good times. This allows fiscal policy to act countercyclically, with higher costs attached to the program during crises and recessions, but with negligible costs when aggregate default risk is low.[7]

Another important dimension of the program is that the allocation is determined both by the government, through IAPMEI, and the banks. Indeed, although the criteria are established by the government every year, banks also play an important role in the process. As shown in the survey results, banks are often those that initiate the process and invite firms to apply, thereby suggesting that banks exert further screening on which firms should be supported through the program (Figures D16 and D17).[8]

The unique design of the program allows for a precise identification of its effects. That said, in the dimensions in which a comparison is possible, the estimated effects of the *SME-Leader Program* are within the range obtained for other programs with government guarantees (Table B2). As reference points, Lelarge et al. (2010) find that debt

[7] The need to focus on targeted support to SMEs during the COVID-19 pandemic has been emphasised both by academics (Elenev et al., 2020; Drechsel and Kalemli-Ozcan, 2020; Bailey et al., 2021) and policymakers (Gopinath, 2020; ESRB, 2021). Raguram Rajan wrote that "governments and central banks responded to the pandemic with unprecedented economic support. Because of the urgent need, the help many provided to companies was quick and untargeted. Many firms obtained grants and access to credit was eased. However, as the pandemic drags on, that corporate support needs to become more targeted.", *Financial Times*, 27 December 2020.

[8] Due to the selection problems arising from this, all the reported results are anchored on ITT estimates, as discussed in Section 3.

growth increases by 0.69 pp in the first two years for French firms with government guarantees and Gonzalez-Uribe and Wang (2020) find a 0.032 increase in the probability of external debt issuance. Mullins et al. (2018) document an increase of 2.6% in debt growth for Chilean firms, while de Blasio et al. (2018) find a 50% increase in debt growth for Italian firms over two years. Still on external validity, Portugal is representative of a significant part of the European countries in terms of macroeconomic indicators and the weight of SMEs in the economy.

7. Conclusion

Small firms often face frictions in access to external financing that may limit their ability to invest. This is especially true during crisis periods, when these frictions may be more acute. In this paper, we estimate the sensitivity of small firms' investment and employment growth to the cost of bank financing. For this purpose, we exploit the variation in the cost of debt financing generated by eligibility for a stimulus program adopted in Portugal for small and medium enterprises. The *SME-Leader Program* offers firms a loan guarantee and a credit certification (rating) issued by a government agency. An important distinctive feature of this program is that it targets small firms with low credit risk. Eligible firms have access to subsidized bank credit, and to a public credit rating.

The rich design of the program allows for the use of a multidimensional regression discontinuity design. In this setting, we are able to establish a causal effect between access to finance through the program and firm-level outcomes, which has thus far proven difficult in the literature. The program design also allows us to estimate the effect of credit certification for small firms by exploiting variation in the level of ratings around the eligibility threshold for the top certification. The importance of ratings is well established for large and listed companies but not for private and small firms.

Overall, we find that the program has a positive impact on small firms' investment, employment and revenue growth. These effects are more pronounced during the crisis but modest in the post-crisis period. During the crisis, a 1 percentage point (pp) decrease in the cost of debt financing for small firms is associated contemporaneous increases of 0.5 pp in total asset growth, 1 pp in fixed asset investment, and 0.5 pp in working capital investment. A 1 pp decrease in the cost of debt financing is also associated with a contemporaneous increase in employment growth of 0.25 pp. These estimates do not consider potential positive or negative externalities to non-eligible firms, nor the potential heterogeneity of these effects across firms that ex ante face different levels of financial frictions. Overall, the real effects of the program seem mostly driven by a price of credit mechanism, as opposed to a quantity of credit one: the government subsidizes the marginal cost of capital, fostering investment and employment as new projects become viable. We show that the benefits of the program are more pronounced during the crisis, but so are the costs. The cost-per-job in the recovery period is half of the one estimated for the crisis period (5,784 € and 11,788 €, respectively). For each euro spent, there was an increase of 66 euros on TFP.

These results have relevant policy implications, as they suggest that government programs promoting access to credit during economic downturns can successfully help firms to continue to invest. Similar programs were implemented around the world at the onset of the COVID-19 pandemic. However, most of these programs are not targeted. This is important to avoid large fiscal costs, as well as to avoid the proliferation of zombie firms and promote an efficient reallocation of resources in the economy. Our paper offers causal evidence that supporting the best small firms during a financial crisis by providing them with subsidized credit has positive and lasting effects on firms' investment and growth. Supporting them through recoveries seems to bring less tangible benefits. Still, the targeted support also means that costs are much smaller in such periods.

Data availability

Data is confidential, but Banco de Portugal will make it available for researchers for reproducibility. Codes are available here: DOI: 10.17632/wx3bp64nm3.2

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jfineco.2023.01.004.

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