# The Credit-Risk Trade-Off of Loan Guarantees: Evidence from Peru's REACTIVA Program*

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**Abstract**

Can government-backed loans expand credit access and foster firm growth without undermining repayment discipline? This paper examines Peru's REACTIVA program, which provided loans equivalent to 7.3% of GDP during the COVID-19 pandemic under a progressive guarantee structure. Exploiting its quasi-experimental design, we identify the program's causal effects on firms' financial and real outcomes using two complementary sources of variation: the staggered rollout of eligibility and sharp guarantee thresholds. First, using a staggered difference-in-differences approach, we find that access to REACTIVA served as a liquidity lifeline for already banked firms, enabling them to refinance costly debt into cheaper, government-backed credit and to stabilize sales and employment without raising default risk. Second, a regression discontinuity around guarantee thresholds reveals that while REACTIVA spurred rapid credit and business growth among larger new borrowers, it also increased repayment risks in the medium term.

*JEL Codes:* E51, E58, G21, G28

*Keywords:* COVID-19, Government-guaranteed loans, Banks, Firms' debt

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# 1 Introduction

Government-backed loans are a central policy tool for expanding access to finance, particularly for small and medium-sized enterprises (SMEs) that face persistent gaps in private credit markets (Lelarge et al., 2019; Holmstrom and Tirole, 1997). These gaps become especially acute during crises, when adverse shocks to bank lending disproportionately affect SMEs, as credit sources for this segment dry up more rapidly than for larger firms. By lowering lenders' risk perceptions, guarantee programs can reduce the prohibitive borrowing costs that often prevent firms from accessing credit (Stiglitz and Weiss, 1981; Beck and Maksimovix, 2005), thereby supporting firm operations and helping to stabilize business cycles. At the same time, however, such programs may exacerbate moral hazard by weakening borrowers' incentives to repay and banks' incentives to screen, ultimately reshaping both credit allocation and default risk.

This study examines how government-backed loans affect firms' leverage and business growth. We focus on REACTIVA, a large-scale program launched by the Peruvian government to expand access to private credit and help firms meet obligations to employees and suppliers during the COVID-19 pandemic. The program provided National Government (NG) guarantees on working-capital loans and was sizable both domestically and regionally: total guarantees reached PEN S/60 billion (US $15 billion), equivalent to 21.9% of domestic private credit in 2019 or 7.3% of GDP in 2020. Its scale exceeded similar programs in other hard hit economies during the COVID-19 pandemic, such as the United States (6.8%), Portugal (5.7%), Brazil (5.0%), and Chile (4.3%).

The program unfolded in two stages, each with features that enable quasi-experimental analysis. In the first stage, eligibility was restricted to firms with established credit histories in the Peruvian formal financial system. Eligibility was staggered, determined by firms' credit records.[1] In the second stage, REACTIVA extended guarantees to firms without prior credit histories, thereby expanding access to the formal financial sector. In both stages, firms could borrow up to PEN S/10 million (US $2.6 million), repayable over 36 months, including a 12-month grace period. In addition, simple formula-based rules defined the guarantee structure: sharp loan size thresholds determined sovereign coverage at 98%, 95%, 90%, and 80% for progressively larger loans. This institutional design not only provides credible sources of identification for our analysis but also highlights a key policy trade-off: while fostering financial inclusion and supporting business growth, the program simultaneously increased repayment risks, as entrants to the financial sector were more likely to default.

[1] The criteria that determined program eligibility made it possible to identify different groups of firms that became eligible at different points in time. Further details are provided in Section 2.1.1.

This paper evaluates the effects of two key design features of the REACTIVA program: (i) its eligibility criteria and (ii) its guarantee structure. We proceed in two steps. First, we exploit the staggered rollout of eligibility to estimate the average treatment effect (ATT) of being eligible on firms' financial and real outcomes. Focusing on eligibility rather than on actual loan uptake or loan amount avoids endogeneity concerns associated with credit demand.[2] Second, we analyze the program's guarantee structure to assess how varying coverage levels influenced new borrowers' credit behavior and performance.

In the first step, we study the program's initial stage, when eligibility was restricted to firms with an established credit record before REACTIVA's launch. We exploit the staggered design of the program and estimate the ATT of being eligible at launch, as well as its dynamic effects, following Callaway and Sant'Anna (2021) and De Chaisemartin and d'Haultfoeuille (2024). We restrict the sample to multi-worker firms observed between November 2019 and July 2021 and identify effects under a parallel-trends assumption based on not-yet-treated groups.

In terms of financial outcomes, we find that eligibility at launch increased firms' direct borrowing by US \$130.8k on average, driven by US \$144.9k in new REACTIVA loans and accompanied by a reduction of US \$22.1k in other, more expensive debt. These results indicate that eligible firms used the program to expand credit access and partially restructure their liabilities toward cheaper, government-guaranteed loans. When focusing on debt balances net of REACTIVA, we find no significant effect on non-performing loans during the first 15 months of the program.

Regarding real outcomes, we find that REACTIVA had positive but delayed effects. Eligibility raised average sales within firms' sales rank groups by US \$56.3k (44.6% relative to pre-treatment). However, it did not alter their relative position in the sales distribution, indicating persistent segmentation between eligible and comparison firms. For employment, we find no effect on the average number of formal employees within eligible firms' employment rank groups. However, we find a 0.32-decile improvement in firms' position within the formal employment distribution, suggesting that eligible firms became relatively larger compared to their non-eligible counterparts. These effects emerged gradually, beginning around early 2021, roughly six months after the government completed its economic reopening plan. Overall, the evidence indicates that REACTIVA's liquidity support effectively stabilized firms' balance sheets, supported productive capacity, and mitigated employment losses during a period of severe economic disruption.

In the second step, we exploit REACTIVA's guarantee structure to determine the effects of the

[2] Burga et al. (2023) address this endogeneity by constructing a measure of each firm's total lending relationship with banks, weighting each relationship by the bank's exposure to REACTIVA loans.

guarantees on new borrowers' outcomes. In particular, we explore the idea that the impact of credit guarantees differed starkly across the distribution of new borrowers. Around the lower cutoff (US $23,685), less generous guarantees (95%) expanded firms' access to new unguaranteed loans, mildly reduced delinquency in the short run, and supported an immediate yet modest increase in sales, while employment remained largely unaffected relatively to new borrowers who received a higher guarantee (98%). Over time, repayment problems resurfaced, with non-performing loans increasing two years after treatment. By contrast, firms around the higher cutoff (US $197,370) tell a different story. The dynamic effects reveal that larger new borrowers who receive 90% of sovereign guarantee, initially contracted credit but later experienced explosive growth in unguaranteed borrowing, sales, and employment relative to new borrowers who receive a higher guarantee (95%). These gains, however, came at the cost of mounting repayment problems and widespread loan reprogramming in the medium term.

Taken together, the results suggest that lower government guarantees reshaped the allocation of credit in important ways. At the lower cutoff, reduced sovereign backing appears to have encouraged banks to extend healthier, unguaranteed loans, leading to better short-run repayment outcomes. At the higher cutoff, however, less generous guarantees did not prevent rapid credit expansion, and repayment risks mounted over time. This contrast highlights a central policy tension: while reducing guarantee coverage can foster financial discipline, by increasing creditors' exposure to risk and encouraging unguaranteed lending that supports business growth, it does not necessarily prevent the accumulation of repayment risks, particularly among larger new borrowers.

These findings show that during REACTIVA's initial phase, the staggered eligibility design acted as a liquidity lifeline for already banked firms, enabling debt restructuring without raising default risk, as information asymmetries were limited. As the program expanded to new borrowers, its progressive guarantee structure became the main margin of adjustment, driving strong credit and business growth but also higher repayment problems. Overall, the evidence points to a clear trade-off: while REACTIVA broadened financial inclusion and supported recovery, it did so at the cost of greater credit risk, especially among larger new borrowers. This pattern aligns with theories of asymmetric information. When lending relationships exist, default risk remains stable, but when information frictions are high, lower guarantees discipline smaller borrowers yet fail to contain default risk among larger ones.

**Literature Review.** This paper relates to three main strands of the literature. The first is related to SME credit and default risk. A number of studies document how access to credit affects firms' repayment behavior and default risk, emphasizing the role of financial frictions and liquidity provision in shaping firms' vulnerability to default (De Giorgi et al., 2023; Fracassi et al., 2016; Uesugi et al., 2010; Cowling et al., 2012; Carreira and Silva, 2010; Lelarge et al., 2019; Beck and Maksimovix, 2005). These papers suggest that while credit access can relax short-term liquidity constraints, it may also increase long-run vulnerabilities if poorly targeted, ultimately raising default risk. Our contribution consists on the provision of new evidence on how sovereign guarantees affected firms' default risk heterogeneously.[3]

Second, it contributes to financial inclusion literature, which studies how expanding access to credit shapes household and firm dynamics. A large body of work documents that limited access to external finance constrains investment, growth, poverty reduction and local development, and resilience, particularly for small and medium-sized firms (Beck and Maksimovix, 2005; Ayyagari et al., 2007; Burgess and Pande, 2005). Yet, recent studies caution that financial inclusion may carry risks when credit is extended to opaque or high-risk borrowers, raising concerns about repayment and sustainability (Castellanos et al., 2018; Karlan and Zinman, 2010; Cull et al., 2009). By exploiting the institutional design of REACTIVA, this paper highlights the trade-offs inherent in financial inclusion. Broader access to guaranteed loans can support business growth and stability during crises, it can also increase repayment risks when new unbanked borrowers are brought into the financial system in the medium and long-run.

Third, it contributes to government-backed loans and credit guarantee schemes. A growing body of work has examined their effectiveness in expanding credit supply and supporting firms during downturns. Evidence from France and Japan shows that credit guarantees can ease financing constraints for SMEs and stimulate entrepreneurship, though often at the cost of higher default risk and potential moral hazard (Lelarge et al., 2019; Uesugi et al., 2010). More recent evaluations of pandemic-era programs suggest that guarantees were crucial in stabilizing credit flows, but also raised medium-term repayment risks (Kalemli-Ozcan et al., 2024; Vera et al., 2022; Acosta-Henao et al., 2023; Jiménez et al., 2022). By exploiting sharp discontinuities in guarantee generosity under Peru's REACTIVA program, this paper advances this literature by showing how a progressive guarantee structure design shaped credit allocation, repayment behavior, and firm growth. In doing so, it highlights the trade-offs between supporting access to finance and safeguarding financial stability during crises.

[3] Burga et al. (2024) find that loan guarantees under REACTIVA reduced delinquency rates, albeit with substantial heterogeneity. The apparent difference from our results can be explained by two factors. First, their analysis focuses on borrower-lender relationships, whereas ours is at the borrower level. Second, they measure delinquency as a binary firm-level indicator (any loan over 30 days past due), while we examine the volume and composition of non-performing loans.

The remainder of the paper is organized as follows. Section 2 presents the institutional framework. Section 3 describes the dataset and the sample construction. Section 4 explains the empirical strategy to determine the causal effect of REACTIVA on firms' financial and real variables. Section 5 determines the effect of REACTIVA's guarantee structure. Section 6 provides the main mechanisms to interpret our estimates and Section 7 concludes. The appendices offer additional details.

# 2 Institutional framework

## 2.1 The REACTIVA program

In April 2020, the Peruvian government introduced the REACTIVA program to expand firms' access to private credit and help them meet their obligations to employees and suppliers during the COVID-19 pandemic. The program provided National Government (NG) guarantees on working-capital loans. These guarantees followed a progressive structure, covering a declining share as loan size increased. Firms could borrow up to PEN S/10 million (US $2.6 million), repayable over 36 months, including a 12-month grace period. The program was sizable: total NG guarantees amounted to PEN S/60 billion (US $15 billion), equivalent to 21.9% of domestic private credit in 2019 or 7.3% of GDP in 2020.[4]

REACTIVA was implemented in two stages. In the first stage, the government announced guarantees of up to PEN S/30 billion, targeting firms that met two conditions. First, firms' tax liabilities under coercive collection could not exceed one Tax Reference Unit (TRU) as of February 2020. Second, firms had to satisfy either (i) at least 90% of their liabilities classified as Normal or With Potential Problems (WPP, hereafter) by February 2020, or (ii) in the absence of a credit rating in February 2020, firms' debt had to be classified as Normal at some point during the previous 12 months.[5,6] In this stage, guarantees covered 98% of loans up to PEN S/30,000, 95% of loans between PEN S/30,000 and PEN S/300,000, 90% of loans between PEN S/300,000 and PEN S/5.0 million, and 80% of loans up to PEN S/10.0 million. To limit excessive borrowing, the maximum

[4]As a share of GDP, total guarantees effectively provided by REACTIVA surpassed those by similar programs in Chile (4.3%), Colombia (2.0%), Uruguay (1.0%), and Brazil (0.6%). See Bolzico and Prats Cabrera (2022)

[5]In Peru, each loan is classified into one of five credit-risk categories: Normal, With Potential Problems (WPP), Deficient (or Substandard), Doubtful, and Loss. See Appendix B for details.

[6]In both stages, eligible firms had to meet the following additional conditions: (i) they were not related to the loan-granting PFI, (ii) they were not subject to Law 30737, which establishes civil compensation to the Peruvian government for corruption and related crimes, and (iii) they did not engage in illegal activities.

guarantee per firm was the greater of (i) the firm's average monthly sales reported to the National Tax Authority (SUNAT, hereafter by its Spanish acronym) in 2019, or (ii) three times the firm's total contribution to the Public Health System (EsSalud) in 2019.

In the second stage, the government announced additional guarantees of up to PEN S/30 billion and relaxed eligibility criteria to broaden coverage. Firms were allowed to regularize tax liabilities at the time of loan application, and companies without a credit rating in the 12 months preceding the loan became eligible. The government also raised the inner thresholds and ranges of loan-size brackets in the guarantee structure, thereby increasing the average guarantee per loan. Finally, the maximum guarantee per firm was expanded to three times average monthly sales in 2019, or to the greatest of that amount, twice monthly debt in 2019, and PEN S/40,000 for micro-firms. Table 1 summarizes the main features of the two stages of REACTIVA.

Table 1: REACTIVA program: key features

| | **Stage 1** | **Stage 2** |
|---|---|---|
| **Total guarantees announced** | S/ 30 billion | S/ 30 billion |
| **Eligibility criteria** | | |
| Tax liabilities under coercive collection | $\leq$ 1 TRU by Feb. 2020 | $\leq$ 1 TRU at time of request |
| Credit rating | | |
| By Feb. 2020 | $\geq$90% Normal or WPP | $\geq$90% Normal or WPP |
| If not available by Feb. 2020 | Normal during the 12 preceding months | Normal during the 12 preceding months |
| If not available in any of the 12 months preceding the loan | -.- | ✓ |
| **NG guarantee structure** | | |
| 98% | Up to S/ 30,000 | Up to S/ 90,000 |
| 95% | S/ 30,001–S/ 300,000 | S/ 90,001–S/ 750,000 |
| 90% | S/ 300,001–S/ 5.0 MM | S/ 750,001–S/ 7.5 MM |
| 80% | S/ 5.0 MM–S/ 10.0 MM | S/ 7.5 MM–S/ 10.0 MM |
| **Maximum guarantee per firm** | | |
| All firms: | | |
| Average monthly sales (2019) | 1x | 3x |
| Firm's total EsSalud contribution (2019) | 3x | -.- |
| Alternative for micro-firms: | | |
| Average monthly sales (2019) | 1x | 3x |
| Average monthly debt (2019) | -.- | max{2x, S/ 40,000} |
| **Guaranteed loan** | | |
| Total loan cap | S/ 10.0 MM | S/ 10.0 MM |
| Maximum term | 36 months | 36 months |
| Grace period | 12 months | 12 months |

Notes: Summary based on Legislative Decree 1455, Ministerial Resolution 134-2020-EF, Legislative Decree 1485, and Ministerial Resolution 165-2020-EF.

Compared to other government-guaranteed loan programs in Latin America, REACTIVA stands out for the mechanism used to allocate liquidity and determine interest rates. Loans became available through liquidity auctions organized by the Central Bank of Peru (BCRP, hereafter by its Spanish acronym). The process had four steps. First, firms applied for REACTIVA loans at a Private Financial Institution (PFI). Second, the PFI evaluated and approved a subset of applications. Third, the PFI entered a liquidity auction for the total amount of its approved portfolio, with liquidity awarded to the institution committing to the lowest interest rate. Fourth, the BCRP provided liquidity to the winning PFI in exchange for an annual cost of 0.5% and a collateral asset under a 36-month repurchase agreement (REPO). The NG guarantee applied to this collateral, ensuring that if loans defaulted, the PFI recovered the guaranteed share of the portfolio.[7]

Table A1 reports average lending rates around the launch of REACTIVA in April 2020. It shows a general reduction, with the largest declines observed among micro, small, and medium-sized firms (MSMEs).

### 2.1.1 Staggered elibigility

Under REACTIVA's first-stage eligibility rules, firms qualified based on their creditworthiness. Because credit records varied across firms, eligibility was staggered. Table 2 reports the different eligibility groups and program take-up. The largest group became eligible at the program's launch in April 2020, when 76.1% of firms in our sample qualified, compared with only 4.5% in May and 1.7% in June.[8] Moreover, in total, 7.3% of firms in our sample were never eligible.

Table 2 also shows that, within each eligibility group in 2020, take-up was high: 68.7% of firms first eligible in April received a REACTIVA loan, a share that rose to more than 90% among firms first eligible after August. Moreover, the average loan size declined over time, from about US $75,000 for firms first eligible in April to less than US $60,000 for those first eligible after August. Finally, once eligible, firms waited 1.6 months to obtain a loan, on average.

[7]This process was different in other programs in the region. In April 2020, the Chilean government extended the existing Fondo de Garantía para Pequeños Empresarios (FOGAPE) to two programs: FOGAPE-Covid and FOGAPE-Reactiva. In both, firms individually negotiated interest rates, subject to ceilings of the Monetary Policy Rate plus 300 and 600 basis points, respectively. In June 2020, the Colombian government extended the Fondo Nacional de Garantía (FNG), where loan rates were also privately negotiated subject to caps. See Rishmawi and Rojas (2025) and Bolzico and Prats Cabrera (2022).

[8]The sample excludes single-worker firms and those without a credit rating in any of the 12 months preceding the loan.

Table 2: REACTIVA program: eligibility groups and take-up

| Eligibility group | Number of Firms[1] | Share of Firms | REACTIVA loans: Firms Receiving | Share Receiving[2] | Average size[2,3] | Months to Receive[2] |
|---|---|---|---|---|---|---|
| April 2020 | 45,831 | 76.1 | 31,466 | 68.7 | 75.1 | 1.9 |
| May 2020 | 2,731 | 4.5 | 2,363 | 86.5 | 68.5 | 0.5 |
| June 2020 | 1,006 | 1.7 | 773 | 76.8 | 86.3 | 0.7 |
| July 2020 | 784 | 1.3 | 665 | 84.8 | 78.5 | 0.5 |
| August 2020 | 1,488 | 2.5 | 1,426 | 95.8 | 60.6 | 0.1 |
| September 2020 | 1,631 | 2.7 | 1,603 | 98.3 | 51.3 | 0.1 |
| October 2020 | 1,244 | 2.1 | 1,232 | 99.0 | 40.7 | 0.0 |
| November 2020 | 677 | 1.1 | 658 | 97.2 | 48.2 | 0.0 |
| December 2020 | 281 | 0.5 | 254 | 90.4 | 47.0 | 0.0 |
| Jan. 2021 – Dec. 2022 | 144 | 0.2 | 73 | 50.7 | 48.0 | 0.1 |
| Never eligible | 4,395 | 7.3 | -.- | -.- | -.- | -.- |
| **Total** | 60,212 | 100.0 | 40,513 | 72.6 | 72.6 | 1.6 |

Notes: (1) Number of firms becoming eligible for the first time in each period, excluding single-worker firms and those without a credit rating in any of the 12 months prior to the loan. (2) Total is computed over 55,187 eligible firms. (3) Amounts expressed in thousands of U.S. dollars.

We focus on firms first eligible in April 2020, the largest and most representative group, to facilitate the causal interpretation of the estimated average treatment effects (Subsection 4.1). Moreover, to avoid potential violations of the No Anticipation assumption, we exclude firms that became eligible after December 2020, as they may have strategically improved their credit scores to meet the eligibility rules.

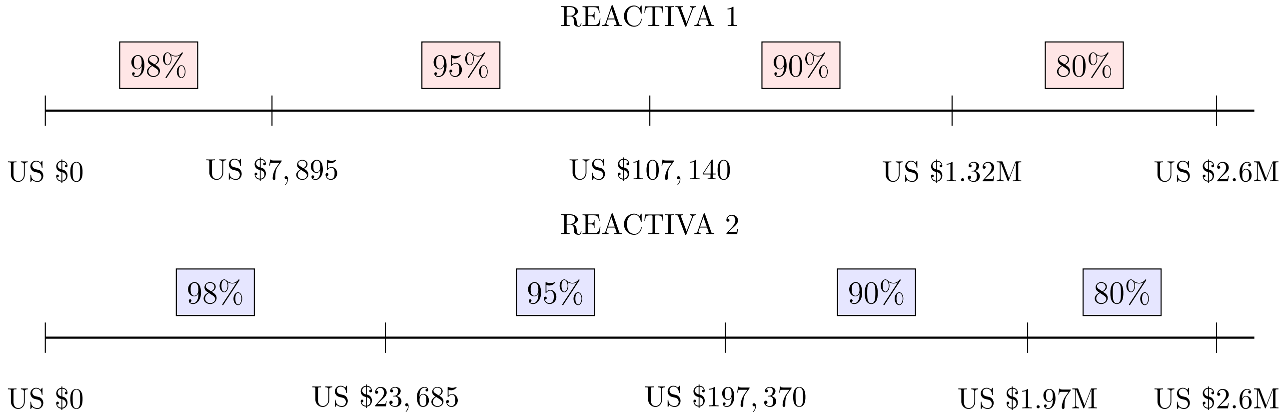
#### 2.1.2 Progressive Guarantee Structure: Generosity

The REACTIVA program was implemented in two stages:

- **REACTIVA 1**: Implemented between April 23 and June 11, 2020, through 17 auction rounds, disbursing a total of US $7,895.1 million.
- **REACTIVA 2**: Implemented between June 30 and October 1, 2020, through 27 auction rounds, disbursing a total of US $6,603.7 million.

In both stages, the maximum loan per firm was set at US $2.6 million, with four levels of government guarantee coverage: 98%, 95%, 90%, and 80%. Larger loan amounts corresponded to lower guarantee coverage. However, the loan-size thresholds associated with each coverage level changed from REACTIVA 1 to REACTIVA 2. Figure 1 illustrates these changes. In REACTIVA 2, the upper bound for the 98% guarantee tranche was tripled (from US $7,895 to US $23,685), the 95% tranche doubled (from US $107,140 to US $197,370), and the 90% tranche increased from US $1.32 million to US $1.97 million.

Figure 1: Guarantee Structure in REACTIVA 1 and 2




In this paper, we exploit the quasi-experimental, exogenous variation in guarantee generosity generated by the REACTIVA program to estimate the causal effect of lower coverage levels on new credit creation, non-performing loans, and business growth. These outcomes capture three core dimensions of the program's intended and unintended effects: its ability to stimulate additional lending (new credit creation), its implications for credit risk (non-performing loans), and its impact on firms' real performance (business growth). Our motivation lies in the potential trade-offs of government guarantees: while such programs reduce lenders' risk perceptions, expand refinancing options, and enhance firms' leverage and operations, they may also encourage excessive risk-taking, creating moral hazard and higher default rates, with potentially adverse macroeconomic consequences.

#### 2.1.3 Rescheduling

In March and April 2021, ahead of the expiration of the initial 12-month grace period, the government issued urgency decrees (DU, hereafter by its Spanish acronym) authorizing REACTIVA debtors to reschedule their loan obligations.[9] Borrowers had until July 15, 2021 to request rescheduling from their PFI, extending the loan term to up to 60 months, including a new grace pe-

[9] DU 026-2021 and DU 039-2021

riod of up to 12 months. Rescheduling was conditional on firms satisfying specific criteria regarding the annual percentage decline in sales during 4Q 2020 (Table 3).

Table 3: Rescheduling criteria by loan size

| **REACTIVA loan size** | **Annual decrease in sales (4Q 2020)** |
|---|---|
| Up to S/ 90,000 | -.- |
| S/ 90,001 – S/ 750,000 | $\geq$ 10% |
| S/ 750,001 – S/ 5.0 MM | $\geq$ 20% |
| S/ 5.0 MM – S/ 10.0 MM | $\geq$ 20% |

Source: Urgency Decrees 026-2021 and 039-2021.

This rescheduling policy, which was also extended in subsequent years, may have affected the incentives of both rescheduling and non-rescheduling firms regarding risk-taking, credit, and repayment.[10] To avoid potential confounding, we exclude firms that became eligible for the first time after December 2020 and restrict the analysis to the period ending in July 2021, when the rescheduling request window closed.

**Discussion about other guaranteed credit programs implemented by the government:** In March 2020, the government established the Business Support Fund (FAE, hereafter by its Spanish acronym) to guarantee private working-capital loans for micro and small enterprises (MYPE, hereafter by its Spanish acronym). In June and July 2020, additional FAEs were launched for specific sectors: tourism (FAE-Tourism) and agriculture (FAE-Agro).[11] Compared with REACTIVA, FAE-MYPE, FAE-Tourism, and FAE-Agro had three main differences. First, they were much smaller, offering guarantees equivalent to 0.4% of GDP in 2020 (vs. 7.3% under REACTIVA). Second, they targeted only micro and small firms and provided smaller loans and lower guarantee coverage.[12] Third, liquidity provision and interest rates were not determined by auction. Instead, firms negotiated loan terms directly with financial institutions, which in this case include credit unions.

Although FAEs coexisted with REACTIVA, their relatively small scale suggests that they did not materially affect the analysis of REACTIVA's impact, which remains the focus of this paper.

[10] See Ministerial Resolution 074-2023EF/15.

[11] See DU 076-2020 and DU 082-2020, respectively. In May 2022, the government also created FAE-Texco to support the textile and garment industry.

[12] For example, under FAE-MYPE, firms could request loans of up to PEN S/90,000, and coverage was capped at 70% for loans not exceeding PEN S/30,000. Under REACTIVA, loans were up to PEN S/ 10 MM, and coverage for loans not exceeding PEN S/ 30,000 was 95% and 98% in the first and second stage, respectively.

# 3 Data, Sample Construction, and Pre-Treatment Balance

## 3.1 Data Source: Peruvian Administrative Borrower-Lender dataset

In this paper, we mainly use the Peruvian Administrative Borrower-Lender dataset (Reporte Crediticio de Deudores, RCD hereafter by its Spanish acronym), collected by the Peruvian Superintendence of Banking, Insurance, and Private Pension Fund Administrators (SBS, hereafter by its Spanish acronym). This dataset is available monthly from January 2018 to December 2022 and contains the universe of credit operations between households and firms with each financial institution under the supervision of the SBS. For each borrower-lender pair, the RCD contains borrowers' monthly credit balance for each type of credit: consumption, mortgage, and credit-size categories such as large, medium, small, and micro-sized loans. Moreover, for each type of credit, it shows the balance by credit-risk category. Finally, the RCD contains borrowers' industry, city, and credit rating, among other variables.

We combine the RCD with two additional data sources. First, a monthly firm-level dataset containing two variables for each firm: (i) its decile in the distribution of sales and formal employment within its four-digit industry group, and (ii) the average sales and employment of firms in that decile.[13] These variables allow us to track firms' mobility in sales and labor demand following the implementation of the REACTIVA program. Second, we complement the RCD with data from the Ministry of Production (PRODUCE) containing sales and employment ranges of each firm in our sample.

Based on our final dataset, we identify four stylized facts. First, the number of borrowers in the formal financial sector rose by 80% between 2019 and 2022. Second, over the same period, the average monthly debt stock per borrower increased by 13.6%, while the stock excluding REACTIVA loans declined by 10.5%. Third, the distribution of debt by credit-size category also shifted significantly: for example, the average micro-sized loan increased from US $1.2k to US $2.8k, whereas the average large-sized loan fell from US $526.1k to US $510.3k. Finally, the credit-risk profile of the debt balance deteriorated sharply, with loans in the Doubtful and Loss categories rising by 287.1% and 258.2%, respectively, between 2019 and 2022.[14]

[13] This dataset was kindly provided by the Ministry of Economy and Finance (MEF).

[14] As described in Section 2.1, loans are classified into five credit-risk categories. See Appendix B for details.

### 3.2 Sample Construction

In this paper, we restrict the sample to multi-worker firms. When estimating the effect of REACTIVA eligibility on financial and real outcomes, we further limit the sample to firms with established credit histories before the program's launch in April 2020. To avoid behavioral responses to eligibility rules, we exclude firms that became eligible after December 2020. We also confine the sample to firms observed between November 2019 and July 2021, excluding the period affected by the first loan rescheduling program. When analyzing the effects of the guarantee structure, we focus instead on firms without prior credit histories that received REACTIVA loans. In both analyses, we trim the sample by excluding firms whose average total debt lies below the 1st or above the 99th percentile.

To identify the effect of REACTIVA, we assess covariate balance in both levels and trends before and after the program's launch across eligibility groups. We consider firms' credit-risk weighted rating, age, and the shares of micro, small, medium, and large firms.[15] Table 4 reports the balance between 45,831 firms eligible at the program's launch in April 2020 and an average of 6,523 not-yet-treated firms observed each month by December 2020. Panel A presents average pre-April 2020 values, while Panel B reports post- versus pre-April 2020 changes for both groups. In both panels, we include normalized differences between groups. We find substantial imbalance in most baseline variables.[16] Firms eligible in April 2020 exhibited better credit-risk profiles, were older, and made larger sales before the program relative to not-yet-treated firms. Regarding trends (Panel B), the only imbalance arises in the change in the share of medium-sized firms, which increased more among the not-yet-treated group. Based on these findings, we control for baseline covariate levels in the subsequent analysis.

## 4 Impact of REACTIVA: a staggered adoption design

We leverage on the staggered eligibility of the program to use a staggered difference-in-differences (DiD) approach to determine the effect of REACTIVA on firms' financial and real variables. We define treatment as being eligible for the program.[17] On the one hand, given its prevalence, we

[15]We follow the classification of micro, small, medium, and large firms based on annual sales established by Law 30056 (PRODUCE).

[16]Following Baker et al. (2025), a normalized difference greater than 0.25 in absolute value indicates imbalance between groups.

[17]Compared to defining treatment as the amount of credit requested or received, our treatment definition is more suitable for the Stable Unit Treatment Assumptions to hold. Moreover, it addresses the endogeneity concern of

Table 4: Pre–April 2020 covariate balance:
April 2020 group vs. Not–yet–treated firms

| **Covariate** | **April 2020 Group** | **Not–yet–treated firms** | **Normalized Difference** |
|---|---|---|---|
| **Panel A: Pre-April 2020 covariate average levels** | | | |
| Credit-risk rating[1] | 0.017 | 0.025 | -0.384 |
| Age | 10.019 | 7.742 | 0.254 |
| Firm-Micro | 0.395 | 0.567 | -0.351 |
| Firm-Small | 0.487 | 0.392 | 0.215 |
| Firm-Medium | 0.028 | 0.014 | 0.636 |
| Firm-Large | 0.090 | 0.028 | 0.936 |
| **Panel B: Post vs. Pre-April 2020 covariate differences** | | | |
| Credit-risk rating[1] | -0.002 | -0.050 | . |
| Age | -0.912 | -1.784 | . |
| Firm-Micro | -0.040 | -0.034 | . |
| Firm-Small | 0.036 | 0.033 | 0.070 |
| Firm-Medium | 0.001 | 0.003 | -0.674 |
| Firm-Large | 0.003 | -0.003 | . |

Notes: (1) Corresponds to credit-risk weighted average rating of a firm's balance. Lower values indicate lower risk and thus a better credit profile. Panel A reports average levels before April 2020, by group. Panel B reports the average change post- vs. pre-April 2020, by group. Let $x$ denote a covariate. Normalized difference: $(\bar{x}_{\text{April}} - \bar{x}_{\text{NYT}})/\sqrt{\frac{1}{2}(s^2_{\text{x,April}} + s^2_{\text{x,NYT}})}$. A "." indicates a non-computable statistic.

consider the firms that became eligible at the start of the program as the single treated group (Table 2). On the other hand, we consider not-yet-treated firms along 2020 as the comparison group. To reduce possible compositional effects, we drop firms in the comparison group when they become eligible. Similarly, to reduce the possibility that firms in the comparison group change their behavior in preparation of becoming eligible, we exclude firms that became eligible for the first time after December 2020. Our design with a single treatment unit group and not-yet-treated group with drop upon adoption reduces the probability of a violation of the No Anticipation assumption needed in a staggered DiD approach (see Subsection 4.1).[18] Finally, we confine our sample to those firms observed between November 2019 and July 2021, excluding the period when the first loan rescheduling program was implemented.[19]

unobserved firms' heterogeneity affecting the requested amount.

[18] This design also has an empirical implication. It yields similar estimates when using the Callaway and Sant'Anna (2021) and the dynamic De Chaisemartin and d'Haultfoeuille (2024) estimators. See Subsection 4.1.

[19] We considered firms in the treated group that were observed in each period during this treatment window.

## 4.1 Validity of Staggered DiD

In Appendix A, we introduce the notation we use throughout the paper, which follows that adopted by Callaway and Sant'Anna (2021) and Marcus and Sant'Anna (2021). Assumption A.1 implies that we are using a panel data with large number of units and a fixed time-periods. We decide to analyze the period between November 2019 and July 2021 and consider a balanced panel for the treated units. Assumption A.2 implies that the treatment is irreversible. We exclude any firm that loses its eligibility during the analyzed period.

Assumption A.3 implies that, given observed covariates $X_i$ that are determinants of untreated potential outcome growth, there is no anticipatory response to treatment for those firms that are eventually eligible. Our design is consistent with this assumption because we focus on a single treated group eligible at the launch of the program, and we exclude firms that earned their eligibility after December 2020.

Finally, Assumption A.4 imposes that, given observed covariates $X_i$, no firm is treated in the first period of analysis. Moreover, it requires that, for every treated period $g$, the conditional probability of belonging to a treatment group $G_g$ is uniformly bounded away from zero and one.

**Assumption 4.1. Conditional Parallel Trends based on not–yet–treated units.** For all $t = 2, ..., \tau$ and all treatment periods $g, s \in \{2, ..., \tau, \infty\}$ such that $t \geq g$ and $s > t$:

$$\mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0) \mid G_{i,g} = 1, X_i] = \mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0) \mid D_{i,s} = 0, X_i]$$

**Assumption 4.2. Conditional Parallel Trends across all periods and all groups.** For all $t = 2, ..., \tau$, all $g \in \{2, ..., \tau\}$:

$$\begin{aligned}\mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0)|G_{i,g} = 1, X_i] &= \mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0)|C_i = 1, X_i] \\ &= \mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0)|X_i]\end{aligned}$$

Assumptions 4.1 and 4.2 are two different Parallel Trends Assumptions. On the one hand, Assumption 4.1, invoked by the Callaway and Sant'Anna (2021) estimator (CS, hereafter), allows us to use a comparison group comprised of not only the never-treated firms but also those not-yet-treated by time $t$. Importantly, this assumption does not restrict all pre-treatment trends to be parallel before the first group of firms is treated. In particular, it does not require pre-treatment parallel trends for the earliest treated group. It only requires it for the later treated groups, from the time period before the first treatment period onward (Baker et al., 2025). This assumption is empirically convenient in our context, where we have data available for many time periods before the first group of firms became treated, and we have focused on this treated group only.

On the other hand, Assumption 4.2, invoked by the De Chaisemartin and d'Haultfoeuille (2024) estimator (DCDH, hereafter), allows us to use any not-yet-treated group as comparison group. In addition to the not-yet-treated firms by time $t$, we can also use any similar group by a later period. Given this, this assumption imposes more restrictions than the previous one, requiring all pre-treatment trends to be parallel across all treatment groups to identify the causal parameter of interest.

Our results are determined using the CS estimator and, therefore, are based on Assumption 4.1. The CS estimator accommodates the identification of dynamic effects, which are relevant to this study given the prolonged consequences of the COVID-19 pandemic in Peru. Moreover, it relaxes the identifying assumptions by requiring parallel pre-trends for all but the first treated group, which is the central focus of our analysis. However, we also report the dynamic DCDH estimator in the study-event plots. Given our identification design with a single treated group and not-yet-treated groups with drop upon adoption, the two estimators yield similar estimates. In particular, both estimators identify the causal parameter of interest based on comparable $2 \times 2$ DiD contrasts. However, small numerical differences arise due to distinct weighting schemes: while the CS estimator employs a doubly-robust procedure combining regression and inverse-probability weighting (IPW) adjustments, DCDH relies on simple sample-weighted mean differences.

## 4.2 Parameter of Interest: $ATT(g, t)$

We are interested in determining the average treatment effect at time $t$, for the group first treated in April 2020. More generally, let $ATT(g, t)$ denote the average treatment effect at time $t$, for the group first treated in period $g$:

$$ATT(g,t) = \mathbb{E}\Big[Y_{i,t}(1) - Y_{i,t}(0) \mid G_{i,g} = 1\Big] \tag{1}$$

Under Assumptions A.1–A.5, Callaway and Sant'Anna (2021) show that $ATT(g, t)$ is identified for post-treatment periods $t \geq g$ following a standard $2 \times 2$ difference-in-difference design:

$$ATT(g,t) = \mathbb{E}_{X|G_{i,g}=1}\Big[\mathbb{E}\Big[Y_{i,t} - Y_{i,g-1} \mid G_{i,g} = 1, X_i\Big] - \mathbb{E}\Big[Y_{i,t} - Y_{i,g-1} \mid G_{i,g'>t} = 1, X_i\Big]\Big] \tag{2}$$

In particular, the $ATT(g,t)$ can be identified as the covariate-adjusted difference of two differences: (i) the post vs. pre-treatment outcome changes for units first treated in period $g$, and (ii) the post vs. pre-treatment outcome changes for those not-yet-treated by period $t$ (Equation 2).

Replacing population expectations with their sample analogs yields the estimator:

$$\widehat{ATT}(g,t) = \frac{\sum_{i=1}^{n} \mathbb{1}\{G_i = g\}\, \omega_i^{(1)} \Big(Y_{i,t} - Y_{i,g-1}\Big)}{\sum_{i=1}^{n} \mathbb{1}\{G_i = g\}\, \omega_i^{(1)}} - \frac{\sum_{i=1}^{n} \mathbb{1}\{G_i > t\}\, \omega_i^{(0)} \Big(Y_{i,t} - Y_{i,g-1}\Big)}{\sum_{i=1}^{n} \mathbb{1}\{G_i > t\}\, \omega_i^{(0)}}$$

where $\omega_i^{(1)}$ and $\omega_i^{(0)}$ denote the observation weights for the treated and comparison groups. In our application, we estimate the $ATT(g,t)$ for $g$ = April 2020 using the CS estimator with doubly-robust weights $\omega_i^{(1)}$ and $\omega_i^{(0)}$.

## 4.3 Main Results

In the following subsections, we present the estimated average treatment effects of being eligible for REACTIVA at the program's launch. We report both average effects and their dynamics for two groups of firm-level outcomes: (i) financial and (ii) real variables.

For financial outcomes, we examine: (i) the stock of direct loans, including performing, rescheduled, refinanced, past-due, and judicially collected debt; (ii) direct REACTIVA loans; (iii) direct loans net of REACTIVA; and (iv) non-performing loans (NPLs) among debt balances net of REACTIVA.[20]

For real outcomes, we consider four variables. First, the firm's position within the sales distribution of its four-digit industry. Firms are ranked from 1 to 10, where 1 corresponds to the decile at the top and 10 to that at the bottom. Second, the average sales within the firm's rank group. Third, the firm's position within the distribution of formal employment of its four-digit industry, following the same ranking procedure as for sales. Finally, the average number of formal employees within the firm's rank group.

[20] NPLs comprise rescheduled, refinanced, past-due, and judicially collected debt.

## 4.4 Effect on Financial Outcomes

Table 5 reports the average treatment effects of REACTIVA on firms' financial outcomes. We find that the program not only expanded credit access for eligible firms at launch but also allowed them to rebalance their portfolios by partially substituting more expensive loans with cheaper, government-guaranteed credit. Being eligible for REACTIVA increased firms' direct borrowing by an average of US $130,821. This increase was mainly driven by new REACTIVA loans amounting to US $144.859, which displaced other, more expensive sources of debt by approximately US $22,136.[21] Table 5 also shows no significant effect of the program on NPLs net of REACTIVA.

Table 5: Average Treatment Effects on Financial Outcomes

| | (1) Direct Loans | (2) Direct REACTIVA Loans | (3) Direct Loans net of REACTIVA | (4) Non-Performing Loans net of REACTIVA |
|---|---|---|---|---|
| Average Treatment Effect | 130.821*** | 144.859*** | -22.136*** | 1.611 |
| | (2.292) | (1.229) | (1.969) | (2.009) |
| Observations | 853,043 | 850,123 | 841,895 | 855,292 |
| Treated Mean Pre-April 2020 | 183.868 | 0.000 | 189.719 | 1.442 |
| Control Mean Pre-April 2020 | 27.102 | 0.000 | 29.263 | 0.569 |
| Firm Fixed Effects | Yes | Yes | Yes | Yes |
| Month Fixed Effects | Yes | Yes | Yes | Yes |
| Control Variables | Yes | Yes | Yes | Yes |
| Heterogeneous Baseline Levels | Yes | Yes | Yes | Yes |

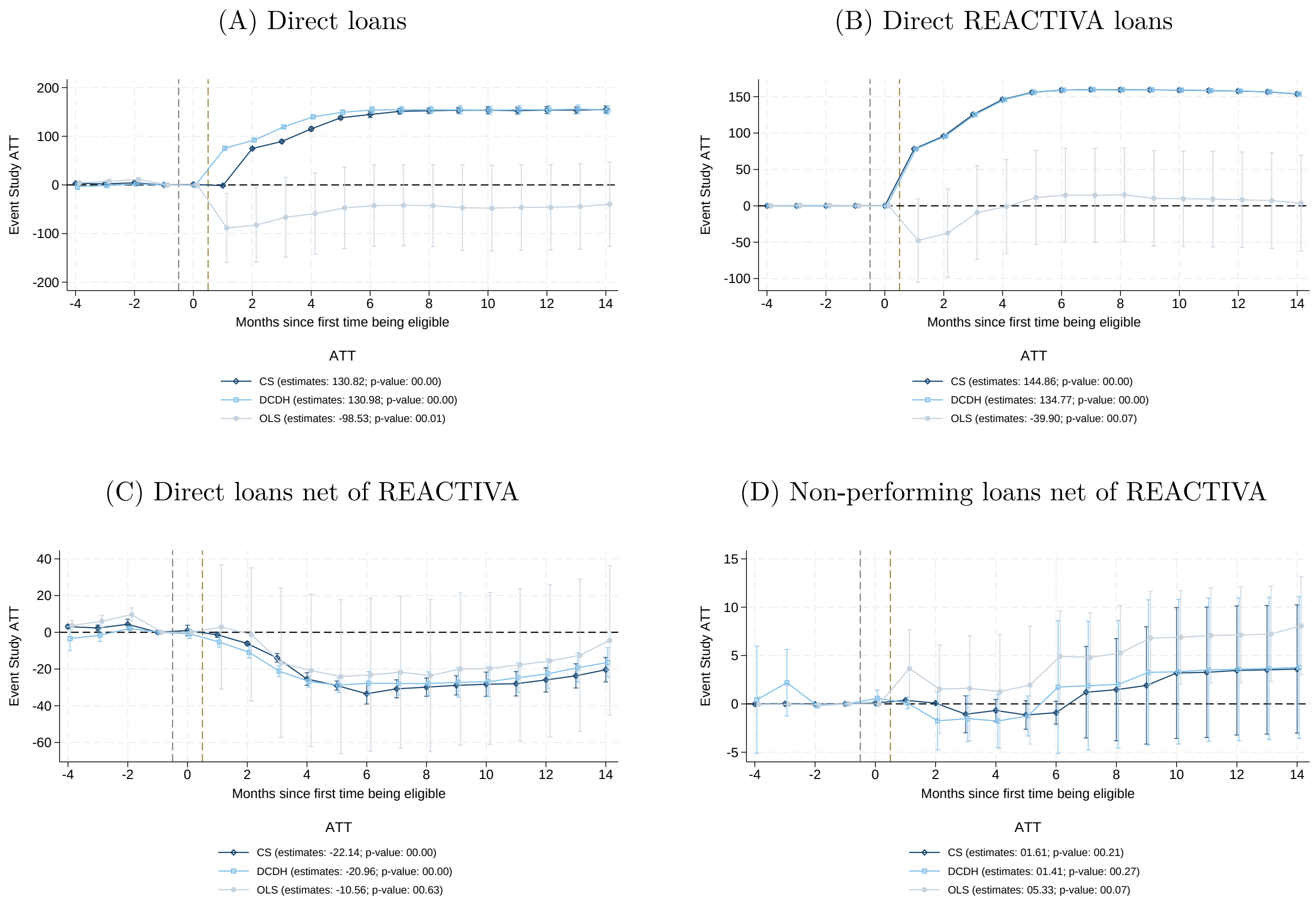
Standard errors, clustered at the firm level, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Notes: Amounts expressed in thousands of U.S. dollars. The table reports average treatment effects estimated using the CS estimator. The treatment group includes firms eligible for REACTIVA in April 2020; all other firms in the sample are not-yet-treated and serve as controls. We exclude firms whose average total debt lies below the 1st or above the 99th percentile, as well as observations with dependent-variable values above the 99.5th percentile. Single-worker firms and not-yet-treated firms upon adoption are also excluded, as are firms first eligible after January 2021. Control variables include the weighted rating two months before eligibility, firm's age, and firm type (micro, small, medium, or large). We further control for pre-April 2020 averages of weighted rating, age, and firm type interacted with the post-April 2020 dummy to capture heterogeneous baseline levels.

Figure 2 presents event-study estimates of the average treatment effects of REACTIVA on firms' financial outcomes. Four main findings emerge. First, there are no significant pre-treatment differences in most financial variables between treated and not-yet-treated firms. The only exception is for direct loans net of REACTIVA, which show mild divergence before treatment. This does not invalidate our identification strategy, as the Parallel Trends Assumption A.5 does not require pre-treatment parallel trends for the earliest treated cohort, the only treated group in our setting.

[21] This expansion in credit and rebalancing is robust to measures of loans. See Table A3 in the Appendix for the case of outstanding loans.

Second, one month after eligibility, direct loans rose sharply and significantly, consistent with REACTIVA's immediate expansion of credit access. The increase persisted for about seven months before stabilizing, indicating a lasting impact on firms' debt balances. Third, the partial substitution of other, more expensive debt was smoother and shorter-lived: firms reduced non-REACTIVA debt during the first six to eight months after eligibility, but balances began to rise again toward the year-end. Finally, considering direct loans net of REACTIVA, there is no significant effect on NPLs within 15 months after becoming eligible for the program. However, the point estimates suggest a mild upward trend towards the medium and long term.

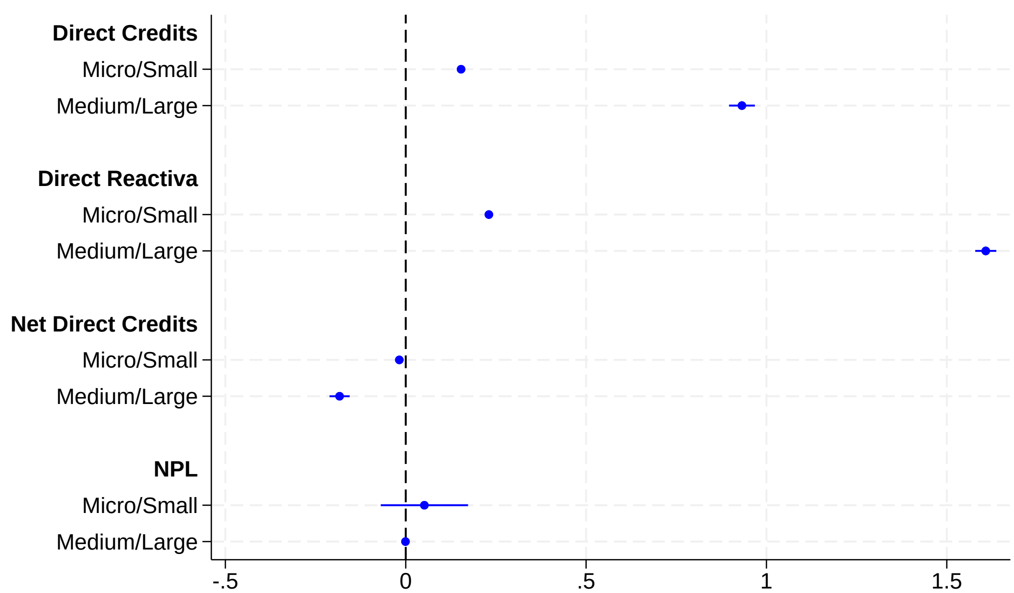
Figure 2: Event-Study Estimates of the ATT on Financial Outcomes




Notes: Amounts expressed in thousands of U.S. dollars. Each panel reports the average treatment effect at event time $e = t - g$ for firms first eligible in April 2020, and observed from November 2019 to July 2021. Event time indicates the number of months since firms became eligible. The first vertical line marks eligibility ($e = 0$), and the second marks the first month firms received a REACTIVA loan ($e = 1$). Estimates in navy blue correspond to the CS estimator, and those in sky blue to the dynamic DCDH estimator. All estimates are shown with 95% confidence intervals. The sample is identical to that used in the corresponding ATT table, and standard errors are clustered at the firm level.

Figure 3 presents heterogeneous effects of REACTIVA on financial outcomes by firm type. We group Micro and Small firms together, and Medium and Large firms together. For each group, the figure reports standardized point estimates and 95% confidence intervals of the average treatment effects. Two main results emerge. First, the program's effects on credit expansion and portfolio rebalancing hold across firm types but are, on average, 7.7 times larger for Medium and Large firms. For this group, being eligible for REACTIVA in April 2020 increased direct loans by 0.93 standard deviations (sd) above the mean. This increase was driven by new REACTIVA loans, which rose by 1.61 sd, and by a 0.18 sd reduction in other debt balances. Second, the program had no significant effect on NPLs net of REACTIVA for either firm group.[22]

Figure 3: Heterogeneity in Financial Outcomes by Firm Type




Notes: The figure reports standardized point estimates and 95% confidence intervals of the average treatment effect of REACTIVA eligibility on financial outcomes by firm type, estimated using the CS doubly robust estimator. Micro and small firms are grouped together, as are medium and large firms. The effects on direct credits, net direct credits, and non-performing loans are normalized by the mean of the variable's standard deviation among pre-March 2020 observations of treated firms. Direct REACTIVA loans are normalized by the mean of the variable's standard deviation among post-April 2020 observations of treated firms. We control for firm type and weighted rating before April 2020, interacted with pre- and post-April 2020 dummies, to capture heterogeneous baseline effects. The sample is identical to that used in the corresponding ATT table, and standard errors are clustered at the firm level.

[22]See Table 4 in the Appendix for details on the ATT estimates by firm type.

## 4.5 Effect on Real Outcomes

Table 6 reports the average treatment effects of REACTIVA on the real outcomes of firms eligible at launch. The program had a positive effect on both sales and formal employment. Column (1) shows that REACTIVA increased average sales within an eligible firm's sales rank group by US $56,320, equivalent to a 44.6% rise relative to pre-treatment levels. However, this growth did not translate into a statistically significant improvement in firms' relative position within the sales distribution (Column (2)). Taken together, these results suggest that treated and comparison firms remain segmented across sales deciles, with treated firms more concentrated in the upper tail of the distribution.

Table 6: Average Treatment Effects on Real Outcomes

| | **(1)** | **(2)** | **(3)** | **(4)** |
|---|---|---|---|---|
| | **Average Sales[1]** | **Sales Rank[2]** | **Average Formal Employment** | **Formal Employment Rank[2]** |
| Average Treatment Effect | 56.32*** | -0.398 | 0.505 | -0.322** |
| | (4.903) | (0.383) | (2.189) | (0.193) |
| Observations | 761,603 | 751,053 | 765,894 | 758,818 |
| Treated Mean Pre-April 2020 | 126.177 | 4.293 | 32.135 | 4.238 |
| Control Mean Pre-April 2020 | 81.113 | 4.945 | 18.057 | 4.884 |
| Firm Fixed Effects | Yes | Yes | Yes | Yes |
| Month Fixed Effects | Yes | Yes | Yes | Yes |
| Control Variables | Yes | Yes | Yes | Yes |
| Heterogeneous Baseline Levels | Yes | Yes | Yes | Yes |

Standard errors, clustered at the firm level, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Notes: (1) Amount expressed in thousands of U.S. dollars. (2) A negative ATT means that eligible firms improve their relative position in the corresponding distribution. The table reports average treatment effects estimated using the CS estimator. The treatment group includes firms eligible for REACTIVA in April 2020; all other firms in the sample are not-yet-treated and serve as controls. We exclude firms whose average total debt lies below the 1st or above the 99th percentile, as well as observations with dependent-variable values above the 99.5th percentile. Single-worker firms and not-yet-treated firms upon adoption are also excluded, as are firms first eligible after January 2021. Control variables include the weighted rating two months before eligibility, firm's age, and firm type (micro, small, medium, or large). We further control for pre-April 2020 averages of weighted rating, age, and firm type interacted with the post-April 2020 dummy to capture heterogeneous baseline levels.

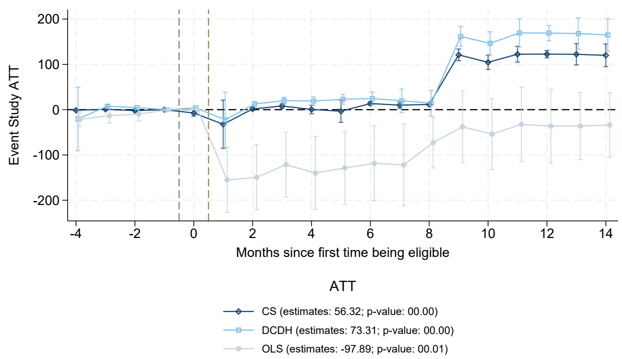
Regarding employment, Column (3) shows no significant effect on the average number of formal employees within an eligible firm's employment rank group. However, Column (4) indicates that REACTIVA improved firms' relative position in the formal employment distribution, reducing their rank by 0.32 deciles, suggesting that eligible firms became relatively larger in employment terms, likely because they retained more workers or cutting fewer jobs during the pandemic. In this sense, the program appears to have helped mitigate employment losses.[23]

[23] The findings on formal employment suggest that treated and comparison firms overlap substantially across

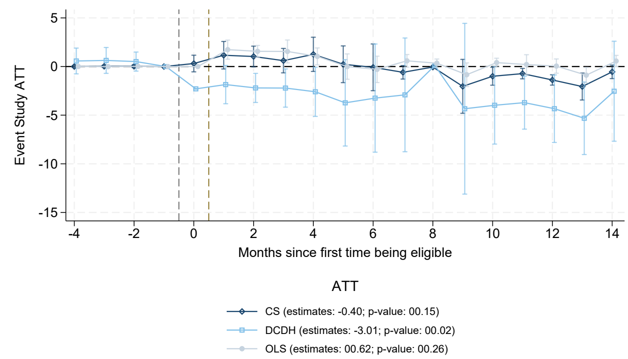
Figure 4: Event-Study Estimates of the ATT on Real Outcomes

**Panel A: Sales**

(A) Average Sales[1]

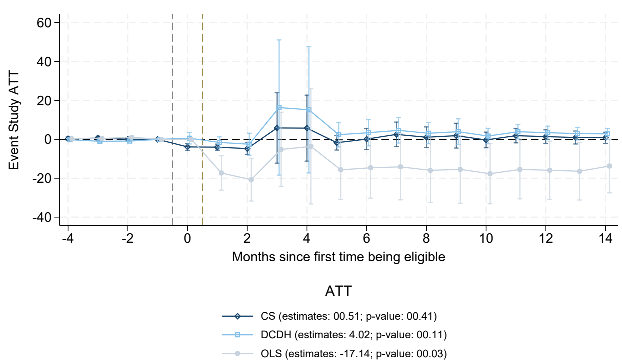



(B) Sales Rank[2]

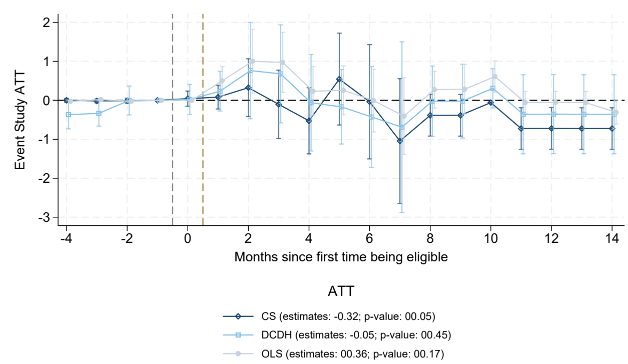



**Panel B: Formal employment**

(C) Average Formal Employment




(D) Formal Employment Rank[2]

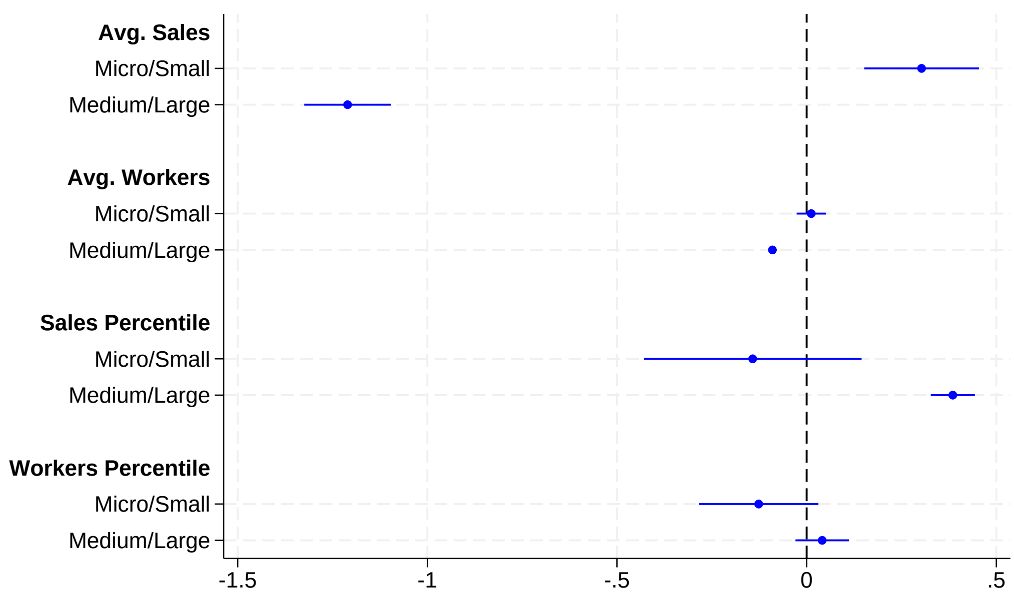



Notes: (1) Amount expressed in thousands of U.S. dollars. (2) A negative ATT means that eligible firms improve their relative position in the corresponding distribution. Each panel reports the average treatment effect at event time $e = t - g$ for firms first eligible in April 2020, and observed from November 2019 to July 2021. Event time indicates the number of months since firms became eligible. The first vertical line marks eligibility ($e = 0$), and the second marks the first month firms received a REACTIVA loan ($e = 1$). Estimates in navy blue correspond to the CS estimator, and those in sky blue to the dynamic DCDH estimator. All estimates are shown with 95% confidence intervals. The sample is identical to that used in the corresponding ATT table, and standard errors are clustered at the firm level.

employment deciles. In such cases, program effects are more likely to manifest in rank improvements rather than in average levels within rank groups.

Figure 4 presents event-study estimates of the average treatment effects of REACTIVA on firms' real outcomes. Four main findings emerge. First, the impact on average sales materialized gradually, becoming statistically significant five to six months after eligibility and strengthening toward the end of the first year. This delayed response aligns with the phased reopening of economic activity following the nationwide lockdown and it suggests that the program's liquidity support translated into higher sales only once firms had stabilized operations and demand had recovered. Second, the increase in average sales did not translate into an improvement in firms' relative position within the sales distribution, as sales ranks remained statistically unchanged around the treatment period. Third, we find no significant effect on the average number of formal employees throughout the analysis period. Finally, the formal employment rank improved modestly, but only toward the end of the first year of eligibility. Overall, these results indicate that REACTIVA supported the real side of the economy by sustaining firms' operations and generating mild positive but delayed effects on sales and employment.

Figure 5: Heterogeneity in Real Outcomes by Firm Type




Notes: The figure reports standardized point estimates and 95% confidence intervals of the average treatment effect of REACTIVA eligibility on real outcomes by firm type, estimated using the CS doubly robust estimator. Micro and small firms are grouped together, as are medium and large firms. The effects are normalized by the mean of the variable's standard deviation among pre-March 2020 observations of treated firms. We control for firm size and weighted rating before April 2020, interacted with pre- and post-April 2020 dummies, to capture heterogeneous baseline effects. The sample is identical to that used in the corresponding ATT table, and standard errors are clustered at the firm level.

Figure 5 presents heterogeneous effects of REACTIVA on real outcomes by firm type, using the same grouping and standardization approach applied to financial outcomes. The effects on sales are more pronounced than those on employment, and they differ markedly across firm types. For micro and small firms, eligibility for REACTIVA in April 2020 increased average sales within their sales rank group by 0.30 sd above the mean. In contrast, medium and large firms experienced a sharp decline of 1.2 sd in average sales, accompanied by a deterioration in their relative position within the sales distribution, equivalent to a drop of 0.39 sd in their sales rank. Regarding employment, only medium and large firms exhibit statistically significant effects: REACTIVA reduced the average number of formal employees within their employment rank group by 0.1 sd.[24] Overall, these results indicate that REACTIVA's real effects were uneven across firm sizes, supporting smaller firms while medium and large firms experienced relative contractions. This is consistent with the program's stronger liquidity relief among financially constrained firms.

# 5 Impact of REACTIVA's guarantee structure: a Regression Discontinuity Design

The progressive guarantee structure of REACTIVA allows us to implement a sharp regression discontinuity (RD) design, using the size of the first REACTIVA loan as the running variable. This framework identifies the causal effect of lower guarantee coverage on new credit creation, non-performing loans, and firm growth. The validity of the RD assumptions is discussed in Section 5.1.

We estimate the following specification:

$$y_{it}^{k} = \alpha_0 + \alpha_{j+1}\mathbb{I}\Big(LSize_i > \bar{c}_j^k\Big) + f\Big(LSize_i; \nu_j^-, \nu_j^+\Big) + X_{it}'\Gamma + \epsilon_{it}, \tag{3}$$

where $y_{it}^k$ denotes the outcome of firm $i$ (e.g., new credit, NPLs, sales) measured $t$ months after (or before) receiving its first REACTIVA loan in round $k \in \{1, 2\}$. The cutoff values $\bar{c}_j^k$ are defined as:

[24]See Table 5 in the Appendix for detailed ATT estimates by firm type.

$$\bar{c}_j^1 = \begin{cases} US\ \$7,895 & j=1 \\ US\ \$107,140 & j=2 \\ US\ \$1.32M & j=3 \end{cases} \quad \text{and} \quad \bar{c}_j^2 = \begin{cases} US\ \$23,685 & j=1 \\ US\ \$197,370 & j=2 \\ US\ \$1.97M & j=3 \end{cases}. \tag{4}$$

The coefficient of interest, $\alpha_{j+1}$, captures the causal effect of crossing the threshold, i.e., receiving a lower guarantee level, on the specified outcome.

## 5.1 Validity of RD Design

Under REACTIVA's rules, the sovereign guarantee rate declined discretely at specific loan-size thresholds: borrowers with loans above a cutoff received a lower coverage rate. To implement an RD design, we verify two key assumptions: (i) the presence of a discontinuity in coverage at the thresholds, and (ii) the smoothness of the loan-size distribution around these cutoffs.

### 5.1.1 Discontinuity at the thresholds

We first confirm whether the program was implemented as designed. In other words, whether borrowers to the right of each cutoff received lower coverage. Figures A3 - A5 show sharp drops in guarantee rates at each threshold, consistent with the law. Local linear RD estimates indicate coverage decreases of approximately 2.2 (0.03), 0.4 (0.03), and 9.7 (0.08) percentage points at the US \$7.9k, US \$78.9k, and US \$1.32m cutoffs in REACTIVA 1, and of 2.7 (0.04), 3.4 (0.1), and 3.7 (1.1) percentage points at the US \$23.7k, US \$197.4k, and US \$1.97m cutoffs in REACTIVA 2.[25]

Coverage rates immediately to the left and right of the thresholds generally align with the mandated levels. An exception arises in REACTIVA 2, where coverage just below some cutoffs is more dispersed. This likely reflects firms that first borrowed in REACTIVA 1 at a lower coverage rate and then received additional loans in REACTIVA 2, introducing heterogeneity in observed guarantees. Moreover, relatively few borrowers obtained loans close to the US \$1.97m cutoff (Figure A5).

[25]Standard errors in parentheses. Estimates are obtained using local linear regressions with MSE-optimal bandwidths and covariate-adjusted polynomials of order $p = 1$ with triangular kernels.

### 5.1.2 Smoothness of the density function

A second assumption is that firms and banks could not precisely manipulate loan sizes to position themselves around the thresholds. Although loan amounts are generally endogenous, we argue that firms lacked the ability to fine-tune requests, and banks had little incentive to grant loans just above a cutoff, particularly for new borrowers (Entrants) without credit histories, since this would imply higher credit risk under lower coverage rates. Moreover, the program's design itself introduced uncertainty: in each auction round, banks competed for REACTIVA liquidity, and their success in securing funds was not guaranteed, further limiting their ability to predict coverage precisely.

Figure A6 shows the distribution of initial REACTIVA loan amounts. Loan sizes follow a skewed, log-normal-like distribution, with most observations below the thresholds. In several cases, we observe spikes near cutoffs, consistent with bunching and possible manipulation. However, among new borrowers, the distribution appears smoother once we implement a 'donut RD', excluding observations in a narrow band around each cutoff following Barreca et al. (2011).

The presence of visible heaps around the thresholds suggests that lenders may have strategically bunched loan amounts just below the cutoffs to secure a higher sovereign guarantee, thereby reducing their own credit exposure. This type of sorting behavior can bias standard RDD estimates, as observations immediately adjacent to the cutoff may no longer be comparable. By excluding loans within a narrow interval around the cutoffs (e.g., ±US \$50 around the first cutoff and ±US \$35 around the second), we obtain smoother density functions and reduce the risk of contamination from strategic manipulation, improving the credibility of the design. Finally, McCrary tests (Figure A7) confirm that for key thresholds in REACTIVA 2 (US \$23.7k and US \$197.4k), the loan-size distribution is smooth, supporting the validity of the RD design.

## 5.2 Main Results

In the following subsections, we present estimates for three main groups of outcomes: (i) new credit creation, measuring credit (excluding REACTIVA) generated during the program's implementation; (ii) non-performing loans, assessing whether the program affected default risk; and (iii) business growth, assessing whether additional credit promoted higher sales and employment.

Regarding new credit creation, the main outcome variables are: (i) the stock of net direct credits,[26], (ii) net outstanding credit, and (iii) the ratio of unguaranteed credit, defined as the share of the

[26]This includes both net direct credits and net outstanding credit.

unguaranteed portion of REACTIVA credit and new loans relative to total direct credits.[27] For non-performing loans, the main outcome variables are: (i) the stock of non-performing loans,[28] (ii) non-performing loans net of REACTIVA, (iii) the share of non-performing loans relative to total direct credits, and (iv) the share of reprogrammed REACTIVA credit relative to total direct credits. Finally, for business growth, the main outcomes are: (i) firms' average sales and average employment at the industry-rank level, based on the MEF dataset, and (ii) midpoint values of sales and employment, computed from the ranges reported by PRODUCE.

### 5.2.1 Effect on New Credit Creation

The estimates show that entrants receiving a lower guarantee level (95%) experience an increase in other loans starting from the month they obtained their first REACTIVA credit, with effects that grow over time. Moreover, the similarity between the estimates for net direct credits and net outstanding credit suggests that this expansion is driven by new healthy credit rather than by non-performing loans (Table 6). In contrast, new borrowers facing an even lower guarantee level (90%) display a different pattern. There is no immediate effect on new credit creation, but several months after the first REACTIVA disbursement we observe a negative effect relative to entrants with a 95% guarantee. After one year, however, credit creation rises sharply by US $75.2k and US $20.28k on net outstanding credit and net direct credit, respectively (Table 7).

For the unguaranteed credit ratio, banks assumed 12.97 percentage points more when entrants received lower government guarantees (95%), beginning in the month of their first REACTIVA credit. These effects decline over time, suggesting that banks became more willing to lend without guarantees and that the role of government backing gradually diminished (Table 6).[29]

Likewise, for new borrowers around $\vec{c}_2^2$, the initial effect on the unguaranteed credit ratio was not significant. After eight months, however, the ratio declined significantly, suggesting that this group received substantially fewer new credits relative to entrants with a 98% guarantee.[30] Nevertheless, after 12 months the share of unguaranteed debt grew significantly for entrants with a 95% guarantee relative to those with 98%.

[27] These new loans are not covered by any government guarantee.

[28] This includes rescheduled, refinanced, due, and under judicial recovery loans.

[29] Since the ratio of unguaranteed to total credit must eventually converge to one as guaranteed loans are repaid, this effect is expected to diminish over time.

[30] We ran the same regression for one and two months before receiving the first REACTIVA loan, and the estimated effect is essentially zero. Standard errors could not be computed due to insufficient variation in this subsample.

### 5.2.2 Effect on Non-Performing Loans

The estimates show that new borrowers receiving a 95% guarantee experienced a small but significant increase in both total and net non-performing loans relative to those with a 98% guarantee after four months of receiving their first REACTIVA credit. After 12 months, however, there is a modest but significant decline in non-performing loans. By 27 months, these effects reverse again, with significant increases in both measures. The non-significant estimates of the NPL credit ratio suggest that the rise in non-performing loans is largely driven by the expansion of total credit, rather than by a deterioration in borrowers' relative credit quality.

For new borrowers with a 90% guarantee, the pattern is different. There is an immediate but modest increase in total and net non-performing loans. After one year, these variables decline, however, by 24 months after receiving REACTIVA, credit quality deteriorates, resulting in a significant increase in both total and net non-performing loans. To assess whether this reflects more than scale effects, we examine the NPL credit ratio, the share of non-performing loans relative to total direct credit, and find a deterioration in credit quality after one year, followed three months later by a sharp deterioration. In short, entrants with a 90% guarantee exhibit NPL ratios 31.18 percentage points higher at 24 months compared to those receiving a 95% guarantee.[31]

Finally, regarding the reprogrammed credit ratio, new borrowers with a 95% guarantee did not show a significant change in reprogrammed REACTIVA loans relative to those with a 98% guarantee after months receiving their first REACTIVA credit. By contrast, new borrowers with a 90% guarantee displayed a significant rise in reprogramming relative to those with a 95% guarantee, suggesting that these borrowers were actively reprogrammed their REACTIVA credit.

### 5.2.3 Effect on Business Growth

Regarding real variables, we examine sales and employment of entrants up to three and two months, respectively, before receiving REACTIVA loans. The estimates show that new borrowers with a 95% guarantee had lower sales but employed more workers three months before receiving their first REACTIVA credit, even after controlling for time and industry fixed effects. However, one month before receiving REACTIVA there are no significant differences in sales or employment between new borrowers with 95% and 98% guarantees. We interpret this as evidence that, aside from the level of the REACTIVA guarantee itself, there is no discontinuity around the cutoff in other firm

[31]There are no reported point estimates or standard errors for the initial months after receiving REACTIVA loans, as there is no variation in the NPL variables during this period.

characteristics, since the initial differences (Column (1) in Table 10) did not persist. This supports the validity of the RDD design at this cutoff. Also, there is a significant instantaneous positive effect of lower generosity on average sales while workers employed effect is not significant. Regarding medium and long run effects, the estimates show that there were no significant differences.[32]

The estimates for new borrowers who received 90% of guarantee, reveal that firms just above the threshold already exhibited significantly higher sales and employment before receiving REACTIVA, which weakens the credibility of a strict RDD interpretation. Nevertheless, the dynamics are informative. After treatment, these firms experienced very large and statistically significant gains in sales: average sales rose by over US $1.0 million at six months, US $2.2 million at twelve months, and US $3.5 million at twenty-four months. Employment also increased substantially, with gains of more than 100 workers at the time of the first disbursement and nearly 250 workers two years later. Mid-point regressions confirm these patterns, showing large and persistent increases in both sales and employment. Taken together, the evidence suggests that larger firms disproportionately benefited from REACTIVA, achieving rapid growth in output and labor demand, although the presence of pre-existing differences cautions against interpreting these effects as fully causal.

## 5.3 Dynamics of the Effects

In this section, using the empirical specification 3, we examine the dynamic effects of lower guarantee generosity on various firm-level outcomes: (i) new credit creation, measured by net direct and outstanding credit (Figure 6); (ii) default risk, captured by non-performing loans (Figure 7); and (iii) business growth, proxied by firms' average sales and labor demand relative to their position within the industry (Figure 8). Figures (A) and (C) of each figure illustrate the dynamics around the first cutoff, where guarantee generosity decreases from 98% to 95%, while Figures (B) and (D) depict the effects around the second cutoff, where generosity declines from 95% to 90%. Together, these figures provide a comprehensive view of how sovereign coverage shaped firms' financial and real outcomes over time.

Since the borrowers analyzed in this exercise had no prior credit history and therefore reported little to no pre-existing debt before receiving REACTIVA loans, differences in credit variables excluding REACTIVA are interpreted as new credit creation. Figure 6 shows the dynamic effects on net direct and outstanding credit, which display similar patterns, suggesting that the stock of

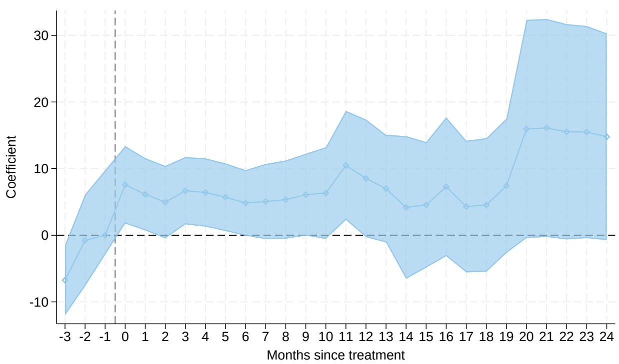
[32] We also estimate the effects on the midpoint of firms' sales and employment ranges as a broad indicator of business growth; however, we place greater emphasis on the estimates of firms' average sales and employment within their industry rank, as these provide a more granular measure of performance.

non-performing loans was not a relevant factor for borrowers around the first cutoff (98% vs. 95%). Moreover, the estimates reveal significant increases in new credit creation, particularly during the first year after receiving REACTIVA loans and again toward the end of the second year, among borrowers who obtained the less generous guarantees.

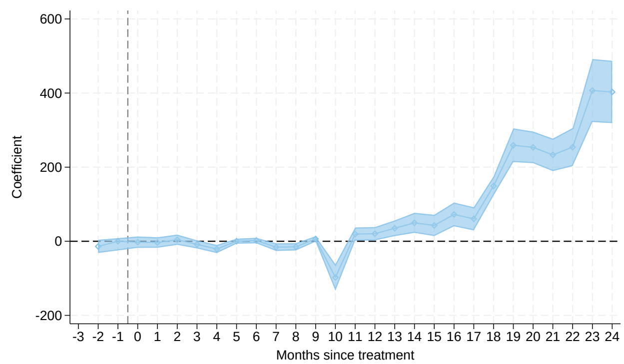
Figure 6: Effect on New Credit Creation

**Panel A: Net Direct Credits**

(A) Cut-off 1 (98% vs. 95%)

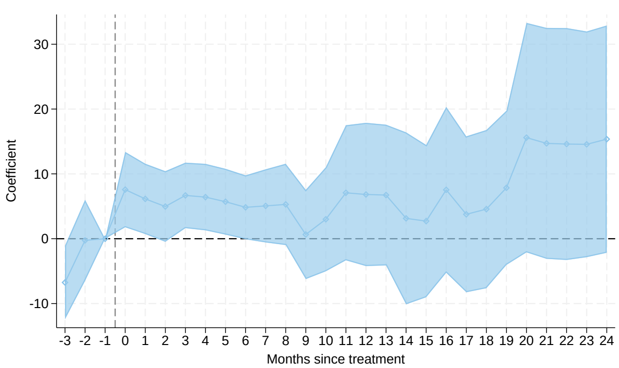



(B) Cut-off 2 (95% vs. 90%)

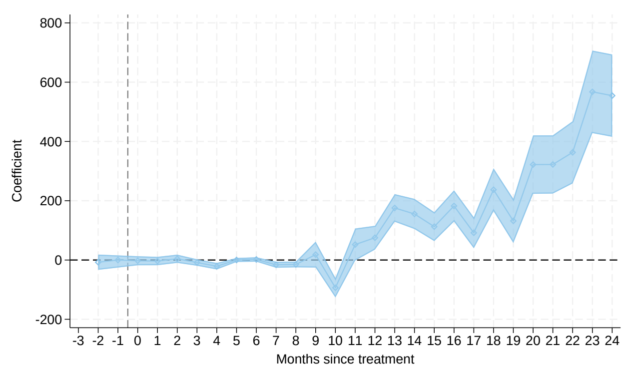



**Panel B: Net Outstanding Credit**

(C) Cut-off 1 (98% vs. 95%)




(D) Cut-off 2 (95% vs. 90%)



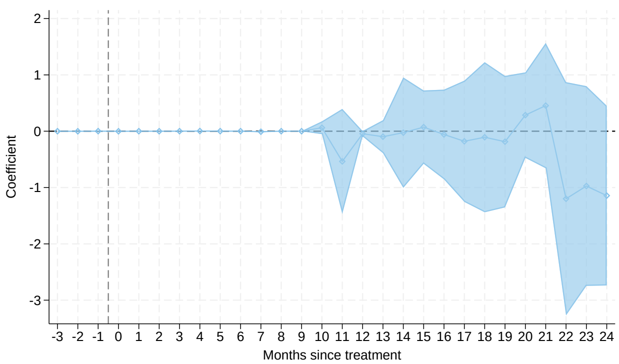

Notes: (1) Regressions include time and industry fixed effects, the initial month of debt, one month before REACTIVA, the 12-month average credit score, and a categorical variable capturing firm size. (2) All estimates are shown with 95% confidence intervals. The sample is identical to that used in the corresponding tables.

In contrast, the dynamics differ for borrowers around the second cutoff (95% vs. 90%). During the first nine months after receiving REACTIVA loans, lower guarantee generosity appears to have no discernible effect on new credit creation, followed by a brief negative response. However, from about one year onward, there emerges a persistent and growing positive effect on credit creation. Additionally, for this group, the point estimates for net direct and outstanding credit diverge, suggesting that non-performing loans played a more important role among these borrowers.

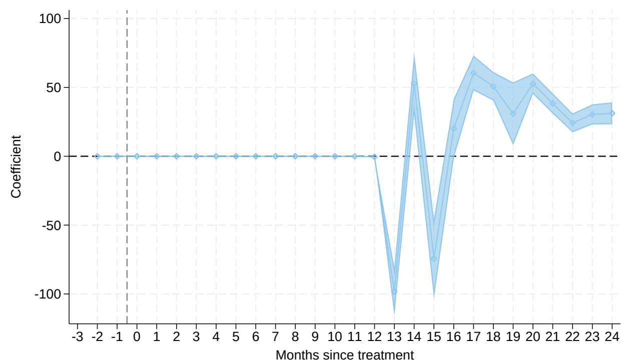
Figure 7: Effect on Non-Performing Loans

**Panel A: Non-Performing Loans**

(A) Cut-off 1 (98% vs. 95%)

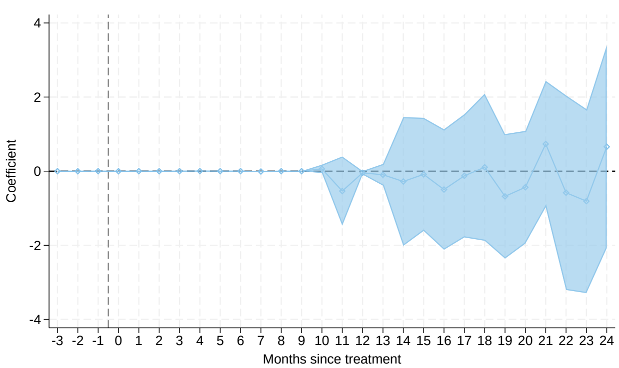



(B) Cut-off 2 (95% vs. 90%)

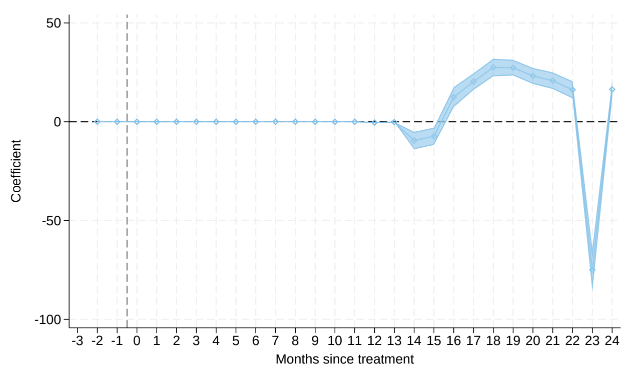



**Panel B: Net Non-Performing Loans**

(C) Cut-off 1 (98% vs. 95%)




(D) Cut-off 2 (95% vs. 90%)



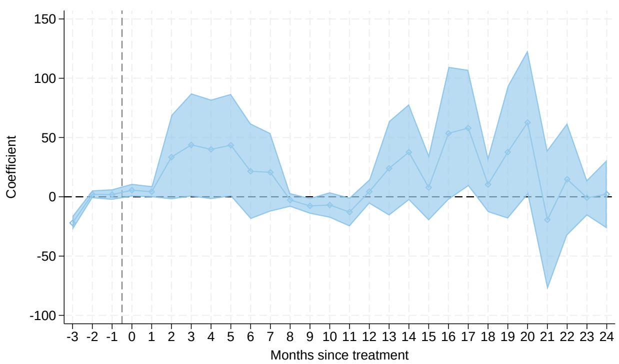

Notes: (1) Regressions include time and industry fixed effects, the initial month of debt, one month before REACTIVA, the 12-month average credit score, and a categorical variable capturing firm size. (2) All estimates are shown with 95% confidence intervals. The sample is identical to that used in the corresponding tables.

Consistent with Figure 6, the dynamic effects on non-performing loans differ sharply between the two groups of borrowers. Among firms around the first cutoff (98% vs. 95%), there is virtually no change in non-performing loans during the first ten months after receiving REACTIVA loans. Thereafter, the estimated effects remain statistically insignificant, and the standard errors widen.

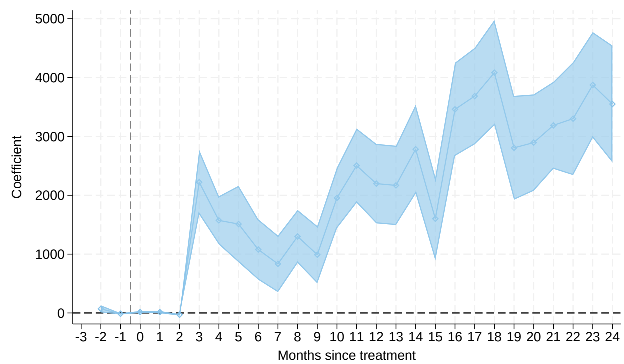
Figure 8: Effect on Business Growth

**Panel A: Average sales within firms' sales rank**

(A) Cut-off 1 (98% vs. 95%)

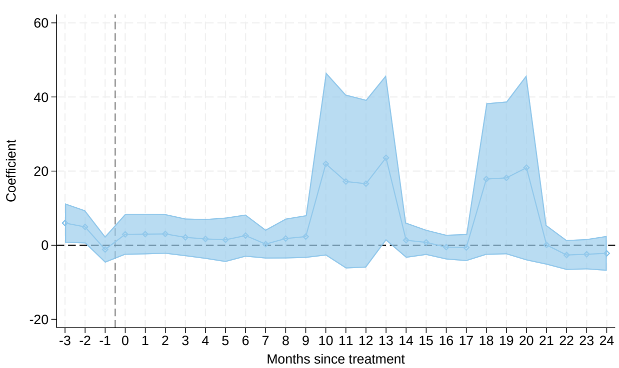



(B) Cut-off 2 (95% vs. 90%)

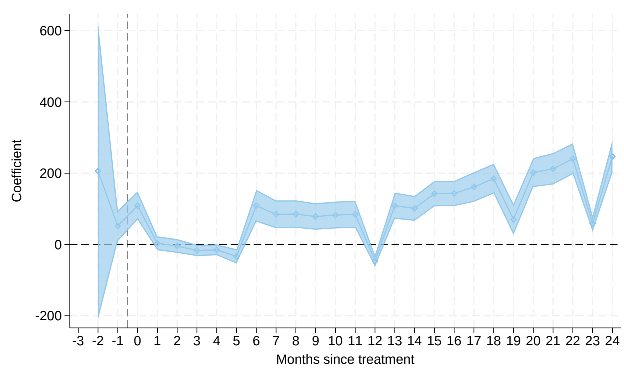



**Panel B: Average workers within firms' employment rank**

(C) Cut-off 1 (98% vs. 95%)




(D) Cut-off 2 (95% vs. 90%)




Notes: (1) Regressions include time and industry fixed effects, the initial month of debt, one month before REACTIVA, the 12-month average credit score, and a categorical variable capturing firm size. (2) All estimates are shown with 95% confidence intervals. The sample is identical to that used in the corresponding tables.

In contrast, among borrowers around the second cutoff (95% vs. 90%), the effect of lower guarantee generosity on non-performing loans is positive, persistent, and statistically significant from roughly the fifteenth month onward. A similar pattern emerges when considering non-performing loans net of REACTIVA. The effect is initially transitory and negative around the fourteenth and fifteenth months after receiving REACTIVA, but subsequently turns positive and significant, indicating a delayed deterioration in loan performance among this group.

Finally, Figure 8 examines how differences in guarantee generosity translate into business performance, focusing on firms' sales and employment growth. Among borrowers around the first cutoff (98% vs. 95%), the results point to modest and short-lived improvements in sales during the first five months after receiving REACTIVA loans caused by the lower generosity granted. The effect becomes statistically insignificant thereafter and eventually reverses toward the end of the second year. This muted response is consistent with the limited increase in credit creation observed earlier for this group, suggesting that the additional liquidity provided by the higher guarantee did not translate into sustained real expansion. Similarly, the estimates for employment rank show no significant, indicating that firms around the first cut-off largely maintained their pre-existing workforce levels despite the temporary boost in credit availability.

In contrast, borrowers around the second cutoff (95% vs. 90%) exhibit a markedly different pattern. Firms benefiting from the less generous 90% guarantee experienced a strong and persistent increase in sales, which becomes statistically significant shortly after program implementation and continues to rise for up to two years. This sustained growth in sales coincides with the prolonged expansion in credit observed in Figure 6, suggesting that the additional credit was used to finance productive activities. The effects on employment are more moderate but follow a similar upward trajectory, with significant gains emerging after roughly one year. This lagged response is consistent with firms first using credit to support working capital before expanding their workforce.

Taken together, these results highlight heterogeneous real effects of guarantee generosity across the firm-size distribution. Smaller new borrowers benefiting from the 95% guarantee appear to have used the short-term credit expansion primarily to scale up productive activities especially in the short-run relative to those receiving the more generous 98% guarantee. In contrast, medium-sized new borrowers subject to the 90% guarantee seem to have leveraged the program to expand output and employment more persistently, but at the cost of higher non-performing loans compared to those receiving the 95% guarantee.

**Results around the first cutoff (US $23,685)**

New borrowers just above the first threshold obtained significantly but limited more credit, both in terms of net direct loans and outstanding debt, and they increased their reliance on unguaranteed credit (Table 6). In the short run, these firms also exhibited small but negative changes in non-performing loans, suggesting that REACTIVA provided effective liquidity relief preventing the increase on default risk for this group (Table 8). However, relatively mild repayment problems reemerged in the medium term, as NPLs began to rise about two years after treatment. On the real side, firms experienced an immediate but modest expansion in sales, while employment remained largely unaffected. This pattern suggests that credit relief primarily helped firms manage liquidity and stabilize sales rather than stimulate labor demand (Table 10).

**Results around the second cutoff (US $197,370)**
New borrowers just above the higher threshold exhibit small but significantly positive effects on sales and employment even before receiving REACTIVA, which weakens the validity of a strict RD interpretation. Nevertheless, the dynamics are still informative.

In the short run, these firms contract their borrowing, but by one to two years after treatment there emerges a persistent and growing positive effect on credit creation and a sharp rise in the share of unguaranteed debt (Table 7). This expansion is accompanied by significant and persistent increases in sales and employment, suggesting that larger new borrowers disproportionately benefited from the program (Table 11). At the same time, repayment problems accumulate, since the effect on non-performing loans is positive, persistent, and statistically significant from roughly the fifteenth month onward. These patterns point to strong but worrying effects: REACTIVA supported rapid growth among larger new borrowers, but also created repayment risks in the medium term.

# 6 Potential Mechanisms

The results discussed in the previous section reveal a consistent narrative of REACTIVA's implementation in the Peruvian credit market. The staggered eligibility design primarily served as a liquidity lifeline for already banked firms, facilitating debt restructuring without increasing default risk. As the program expanded to new borrowers, however, its guarantee design became the dominant margin of adjustment, shaping credit growth, repayment behavior, and business performance. The progressive guarantee structure was likely intended to promote screening among larger new borrowers, as banks retained a greater share of credit risk for them than for smaller borrowers. Yet, our findings indicate that these larger new borrowers experienced substantial credit expansion and business growth, but at the cost of a pronounced increase in default risk, consistent with the presence of financial frictions and asymmetric information in this segment. Overall, the evidence underscores a fundamental trade-off between financial inclusion and credit quality inherent in large scale government guarantee programs. We next discuss potential mechanisms through which government-backed loans may influence the evolution of credit, default risk, and firm growth. The list below is intended to guide the interpretation of our previous results.

1. **Relationship lending and information:** In REACTIVA's first stage, eligibility was restricted to firms with established credit relationships. These firms already possessed hard information (past repayment records, collateral) and a established relationship with banks (soft information). As a result, banks faced low information asymmetry and maintained strong incentives for monitoring. This setting limited moral hazard and enabled efficient credit reallocation: eligible firms substituted costly pre-existing loans for cheaper, government-backed loans, improving liquidity without raising default risk. The delayed but positive effects on sales and employment reflect that these firms used liquidity relief primarily to preserve productive capacity and avoid layoffs rather than to expand aggressively.

2. **Bank incentives under different guarantee levels.** Less generous guarantees increase banks' exposure to risk, encouraging stricter screening and more prudent lending. Yet, at the higher cutoff, larger borrowers still experienced substantial increases in default risk despite supposedly tighter screening. This suggests that banks' risk-taking may vary with borrower size: lenders could be less risk-averse toward larger firms (Beck and Maksimovix (2005); Jimenez et al. (2014)), either because of size biases, because firm size is used as a heuristic for creditworthiness, or because the marginal cost of monitoring declines with borrower size (Holmstrom and Tirole (1997)).

3. **Risk-taking induced by cheaper credit.** Lower borrowing costs can induce firms to undertake riskier projects, reflecting a classic moral hazard problem. Larger borrowers with no credit history, often able to secure lower interest rates than smaller borrowers may have expanded aggressively into higher-risk investments. While this boosted revenues in the short term, it also increased the likelihood of repayment difficulties in the medium to long run.

4. **Information opacity of new borrowers.** New borrowers lack hard information on their financial activities, making credit assessment more difficult. For larger firms, opacity is even greater, as monitoring complex operations is more costly for lenders (Stiglitz and Weiss (1981); Holmstrom and Tirole (1997); Diamond (1984)). As a result, these borrowers were not adequately monitored and tended to overborrow, increasing repayment risks over time. A key example of this opacity is that larger new borrowers could provide records of

their sales but not reliable accounting information on profitability or liquidity, creating a mismatch between rapid sales expansion and the liquidity needed to service their debts.

# 7 Conclusions

This paper examines the effects of the Peruvian Government-guaranteed loans program (REACTIVA), one of the largest initiatives in the Latin American and Caribbean region launched during the COVID-19 pandemic. We evaluate its impact on firms' financial and real outcomes, focusing on two key design features: (i) its eligibility criteria and (ii) its guarantee structure.

Exploiting the staggered rollout of eligibility, we estimate the average treatment effect of being eligible at launch in April 2020. Under the initial eligibility rules, we find that REACTIVA effectively mitigated financial distress among eligible firms by expanding access to credit and enabling the substitution of costly liabilities with cheaper, government-guaranteed loans. Considering the first 15 months after becoming eligible, we find no significant increase in non-performing loans among debt balances net of REACTIVA, suggesting that this credit expansion did not weaken repayment discipline in the short run. We also find that being eligible gradually generated higher sales and modest employment gains. Taken together, the evidence shows that REACTIVA met its primary objective of stabilizing firms' finances and sustaining production during the pandemic, without undermining credit discipline in the short run.

Regarding REACTIVA's guarantee structure, we exploit its progressiveness and sharp cutoffs to determine the effects on new borrowers' outcomes. We find that the generosity of government-backed loans reshaped credit allocation, repayment dynamics, and firm outcomes in heterogeneous, and at times paradoxical, ways. At the lower cutoff (95% vs. 98%), less generous guarantees encouraged banks to extend healthier, unguaranteed loans, improved short-run repayment outcomes, and supported a mild sales' growth in the short run. Yet these benefits came with limited employment effects, suggesting that firms primarily used REACTIVA credit to stabilize liquidity rather than expand labor demand.

At the higher cutoff (90% vs. 95%), larger new borrowers initially contracted their borrowing but later experienced explosive growth in credit, sales, and employment. While this suggests that larger firms disproportionately benefited from the program, repayment problems soon mounted, with both non-performing and reprogrammed loans rising steeply in the medium run. The evidence therefore points to strong but worrying effects: REACTIVA supported rapid growth among larger firms, but at the cost of heightened repayment risks.

The mechanisms explored in this paper help explain these contrasting dynamics. Less generous guarantees appeared to discipline lending at the lower cutoff, but not at the higher one, where opacity and firm size may have reduced banks' monitoring incentives. Cheaper credit encouraged riskier investment strategies, while the absence of reliable accounting information from new borrowers led banks to rely excessively on sales as a signal of repayment capacity, overlooking liquidity and profitability. This mismatch between sales expansion and repayment ability ultimately amplified credit risk.

Taken together, the findings underscore a central policy trade-off. Government-backed loans can expand financial inclusion, ease liquidity constraints, and support business growth during crises. Yet their design is crucial. Overly generous guarantees risk undermining repayment discipline, while insufficient monitoring of opaque borrowers can fuel repayment risks despite apparent growth. Future policy should therefore balance the objectives of broadening access to credit and safeguarding financial stability, tailoring guarantee generosity and monitoring requirements to the characteristics of borrowers.

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# Appendix

## A Staggered Difference-in-Difference framework

We introduce the notation we use throughout the paper, which follows that adopted by Callaway and Sant'Anna (2021) and Marcus and Sant'Anna (2021). We consider a case with $\tau$ periods and denote a particular time period by $t = 1, ..., \tau$. Let $D_t$ be a binary variable equal to one if a unit is treated in period $t$ and equal to zero otherwise. Also, define a treatment period $g \in \{1, ..., \tau\}$. Let $G_g$ be a dummy variable that is equal to one if a unit is first treated in treatment period $g$, and define $C$ as a dummy variable that is equal to one for units that are not treated by period $\tau$. This means that $C = 1$ for the never-treated units. Therefore, for each unit, one of the $G_g$ or $C$ is equal to one. Finally, let $Y_t(1)$ and $Y_t(0)$ be the potential outcomes at time $t$ with and without the treatment, respectively. The observed outcome in each period can be expressed as $Y_t = D_t Y_t(1) + (1 - D_t)Y_t(0)$.

**Assumption A.1. Sampling.** For every period $t$, the sample of observe outcomes $Y_{it}$, treatment condition $D_{it}$, and covariates $X_{it}$ across units $i$ is independent and identically distributed.

**Assumption A.2. Staggered treatment design.** For $t = 2, ..., \tau$,

$$D_{i,t-1} = 1 \text{ implies that } D_{i,t} = 1$$

**Assumption A.3. No anticipation.** For all $t = 2, ..., \tau$, $g \in \{2, ..., \tau\}$ such that $t < g$,

$$\mathbb{E}[Y_{it}|G_{i,g} = 1, X_i] = \mathbb{E}[Y_{it}(0)|G_{i,g} = 1, X_i]$$

**Assumption A.4. Strong overlap.** $P(G_{i,1} = 1|X_i) = 0$. Moreover, for some $\epsilon > 0$, and all $g \in \{2, ..., \tau\}$, $\epsilon < P(G_{i,g} = 1|X_i) < 1 - \epsilon$

**Assumption A.5. Conditional Parallel Trends based on not–yet–treated units.** For all $t = 2, ..., \tau$, all $g, s \in \{2, ..., \tau\}$ such that $t \geq g$, $s > t$:

$$\mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0)|G_{i,g} = 1, X_i] = \mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0)|D_{i,s} = 0, X_i]$$

**Assumption A.6. Conditional Parallel Trends across all periods and all groups.** For all $t = 2, ..., \tau$, all $g \in \{2, ..., \tau\}$:

$$\begin{aligned}\mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0)|G_{i,g} = 1, X_i] &= \mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0)|C_i = 1, X_i] \\ &= \mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0)|X_i]\end{aligned}$$

# B REACTIVA Program - Additional Figures and Tables

Table A1: **Average interest rate by type of credit**

| **Type of credit** | **Currency** | **Mar-20** | **Jun-20** | **Sep-20** | **Change** (in bps) |
|---|---|---|---|---|---|
| Corporate | Domestic | 3.75 | 3.15 | 2.31 | -102.0 |
| Large-sized firms | Domestic | 5.79 | 3.23 | 4.40 | -197.5 |
| Medium-sized firms | Domestic | 8.87 | 4.70 | 4.38 | -433.0 |
| Small-sized firms | Domestic | 26.02 | 7.80 | 10.10 | -1707.0 |
| Micro-sized firms | Domestic | 46.84 | 18.19 | 22.59 | -2645.0 |
| Consumer credit | Domestic | 44.97 | 41.78 | 41.47 | -334.5 |
| Mortgages | Domestic | 6.73 | 6.88 | 6.75 | 8.5 |
| Corporate | Foreign | 2.64 | 2.64 | 1.95 | -34.5 |
| Large-sized firms | Foreign | 4.64 | 5.13 | 4.60 | 22.5 |
| Mortgages | Foreign | 5.69 | 6.21 | 5.87 | 35.0 |

Notes: Last column corresponds to the difference between the interest rate in the first quarter of 2020 and the average interest rate during the second and third quarters of 2020. The difference is expressed in basis points (bps).
Source: Report in March, June, and September 2020 published by the Superintendence of Banking, Insurance and Private Pension Fund Administrators (SBS).

Table A2: Credit-risk categories by size-credit: main feature

| **Category** | **Corporate, Large and Medium Credit** | **Small and Micro Credit** |
|---|---|---|
| **Normal** | Timely payments; financially sound (liquidity, low leverage) | On time or up to 8 days past due |
| **With Potential Problems (WPP)** | Good financials but potential weakening; or $\geq$2 delays of 16–60 days in the last 6 months | 9–30 days past due |
| **Deficient/Substandard** | Weak financials or cash flow; or 61–120 days past due | 31–60 days past due |
| **Doubtful** | Critical financial condition; or 121–365 days past due | 61–120 days past due |
| **Loss** | Insolvency or suspension of payments; or >365 days past due | >120 days past due |

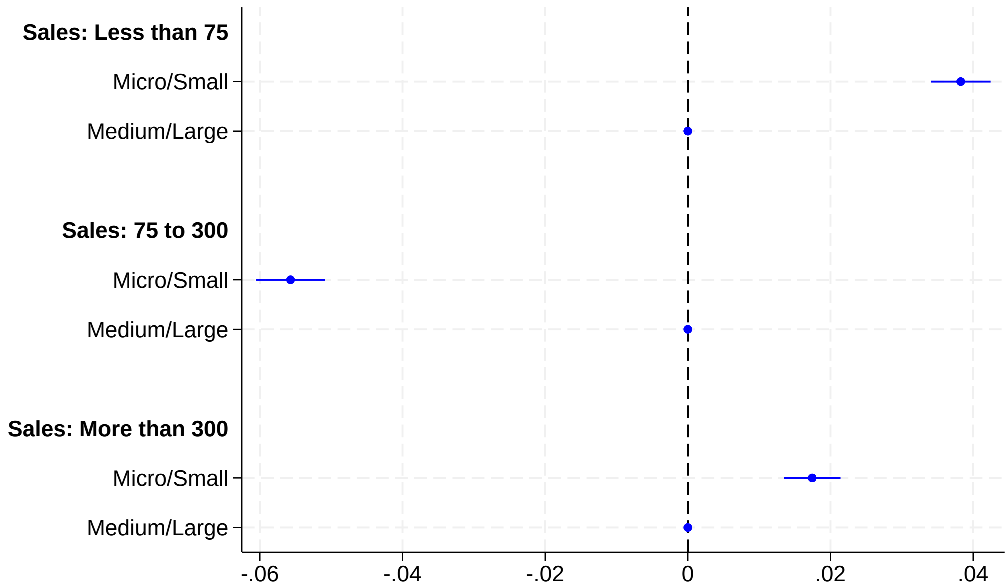
Note: Summary based on SBS definitions. Thresholds for mortgage credit differ and are omitted.

Table A3: Average Treatment Effects on Financial Outcomes: Outstanding Debt

| | **Outstanding loans** | **Outstanding REACTIVA loans** | **Outstanding loans net of REACTIVA** |
|---|---|---|---|
| Average Treatment Effect | 128.699*** | 144.859*** | -20.259*** |
| | (1.830) | (1.229) | (1.458) |
| Observations | 853,017 | 850,123 | 841,046 |
| Treated Mean Pre-April 2020 | 182.426 | 0.000 | 188.794 |
| Control Mean Pre-April 2020 | 26.534 | 0.000 | 29.141 |
| Firm Fixed Effects | Yes | Yes | Yes |
| Month Fixed Effects | Yes | Yes | Yes |
| Control Variables | Yes | Yes | Yes |
| Heterogeneous Baseline Levels | Yes | Yes | Yes |

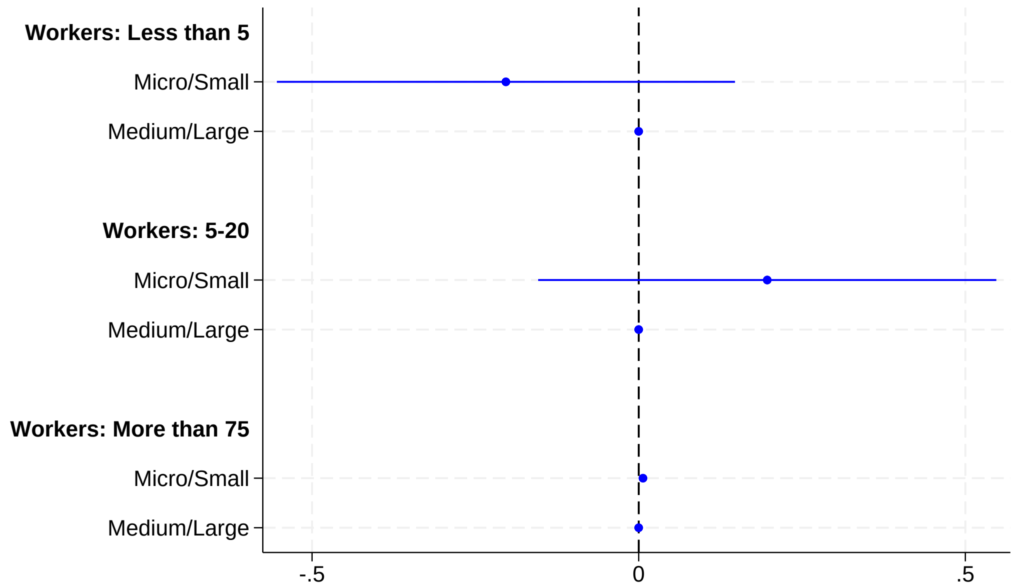
Standard errors, clustered at the firm level, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Note: Amounts expressed in thousands of U.S. dollars. The table reports average treatment effects estimated using the CS estimator. The treatment group includes firms eligible for REACTIVA in April 2020; all other firms in the sample are not-yet-treated and serve as controls. We exclude firms whose average total debt lies below the 1st or above the 99th percentile, as well as observations with dependent-variable values above the 99.5th percentile. Single-worker firms and not-yet-treated firms upon adoption are also excluded, as are firms first eligible after January 2021. Control variables include the weighted rating two months before eligibility, firm's age, and firm type (micro, small, medium, or large). We further control for pre-April 2020 averages of weighted rating, age, and firm type interacted with the post-April 2020 dummy to capture heterogeneous baseline levels.

Figure A1: Heterogeneity in the Probability of Sales Range by Firm Size




Notes: The figure reports standardized point estimates and 95% confidence intervals of the average treatment effect of REACTIVA eligibility on the probability that a firm's sales (in taxable units) fall within a given range, estimated using the CS doubly robust estimator. Micro and small firms are grouped together, as are medium and large firms. The effects are normalized by the mean of the variable's standard deviation among pre-March 2020 observations of treated firms. We control for firm size and weighted rating before April 2020, interacted with pre- and post-April 2020 dummies, to capture heterogeneous baseline effects. The sample is identical to that used in the corresponding ATT table, and standard errors are clustered at the firm level.

Figure A2: Heterogeneity in the Probability of Workers Range by Firm Size




Notes: The figure reports standardized point estimates and 95% confidence intervals of the average treatment effect of REACTIVA eligibility on the probability that a firm's number of workers falls within a given range, estimated using the CS doubly robust estimator. Micro and small firms are grouped together, as are medium and large firms. The effects are normalized by the mean of the variable's standard deviation among pre-March 2020 observations of treated firms. We control for firm size and weighted rating before April 2020, interacted with pre- and post-April 2020 dummies, to capture heterogeneous baseline effects. The sample is identical to that used in the corresponding ATT table, and standard errors are clustered at the firm level.

Table 4: Heterogeneity in Financial Outcomes by Firm Size

| | **Direct Loans** | | **Direct REACTIVA Loans** | | **Direct Loans net of REACTIVA** | | **Non-Performing Loans (NPLs)** | |
|---|---|---|---|---|---|---|---|---|
| | **Micro/ Small** | **Medium/ Large** | **Micro/ Small** | **Medium/ Large** | **Micro/ Small** | **Medium/ Large** | **Micro/ Small** | **Medium/ Large** |
| Average Treatment Effect | 0.153*** | 0.932*** | 0.231*** | 1.608*** | -0.018*** | -0.183*** | 0.052 | -0.001 |
| | (0.004) | (0.018) | (0.002) | (0.015) | (0.004) | (0.014) | (0.062) | (0.002) |
| Observations | 663,005 | 36,797 | 663,180 | 36,681 | 644,278 | 34,571 | 661,316 | 37,793 |
| Treated Mean Pre-April 2020 | 96.782 | 673.254 | 0.000 | 0.000 | 99.469 | 710.489 | 0.662 | 5.824 |
| Control Mean Pre-April 2020 | 19.595 | 119.117 | 0.000 | 0.000 | 21.024 | 139.437 | 0.555 | 0.742 |
| Firm Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Control Variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Heterogeneous Baseline Levels | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Standard errors, clustered at the firm level, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Notes: Amounts expressed in thousands of U.S. dollars. The table reports average treatment effects by firm type estimated using the CS estimator. Micro and small firms are grouped together, as are medium and large firms. The treatment group includes firms eligible for REACTIVA in April 2020; all other firms in the sample are not-yet-treated and serve as controls. We exclude firms whose average total debt lies below the 1st or above the 99th percentile, as well as observations with dependent-variable values above the 99.5th percentile. Single-worker firms and not-yet-treated firms upon adoption are also excluded, as are firms first eligible after January 2021. Control variables include the weighted rating two months before eligibility, firm's age, and firm type (micro, small, medium, or large). We further control for pre-April 2020 averages of weighted rating, age, and firm type interacted with the post-April 2020 dummy to capture heterogeneous baseline levels. The dependent variables are normalized using the mean of the standard deviation of the variable among pre-March 2020 observations of treated firms observed from November 2019 to July 2021, except for direct REACTIVA loans, which are normalized using post-April 2020 observations.

Table 5: Heterogeneity in Real Outcomes by Firm Size

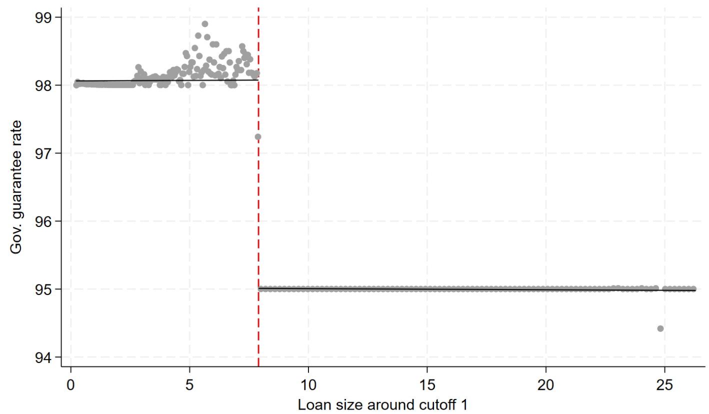
| | **Average Sales[1]** | | **Average Workers** | | **Sales Rank[2]** | | **Employment Rank** | |
|---|---|---|---|---|---|---|---|---|
| | **Micro/ Small** | **Medium/ Large** | **Micro/ Small** | **Medium/ Large** | **Micro/ Small** | **Medium/ Large** | **Micro/ Small** | **Medium/ Large** |
| Average Treatment Effect | 0.303*** | -1.210*** | 0.012 | -0.090*** | -0.142 | 0.385*** | -0.126* | 0.041 |
| | (0.077) | (0.058) | (0.020) | (0.004) | (0.147) | (0.030) | (0.080) | (0.036) |
| Observations | 524,232 | 35,687 | 572,849 | 36,513 | 520,067 | 35,867 | 558,588 | 37,752 |
| Treated Mean Pre-April 2020 | 1.9e+05 | 2.0e+06 | 16.372 | 111.424 | 4.825 | 1.402 | 4.722 | 1.804 |
| Control Mean Pre-April 2020 | 1.7e+05 | 1.9e+06 | 12.080 | 80.171 | 5.236 | 1.583 | 5.130 | 2.341 |
| Firm Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Control Variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Heterogeneous Baseline Levels | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Standard errors, clustered at the firm level, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

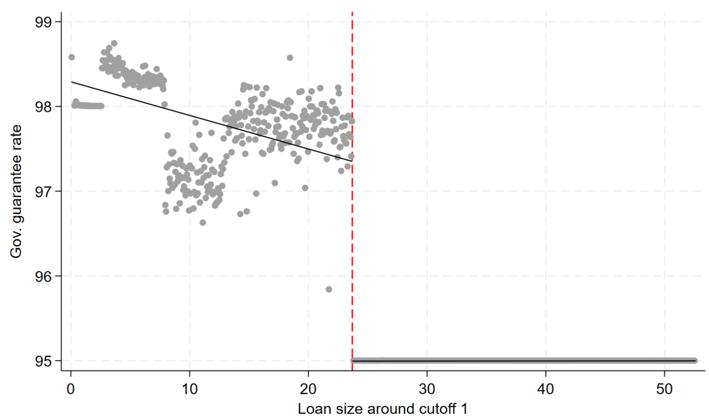
Notes: (1) Amount expressed in thousands of U.S. dollars. (2) A negative ATT means that eligible firms improve their relative position in the corresponding distribution. The table reports average treatment effects by firm type estimated using the CS estimator. Micro and small firms are grouped together, as are medium and large firms. The treatment group includes firms eligible for REACTIVA in April 2020; all other firms in the sample are not-yet-treated and serve as controls. We exclude firms whose average total debt lies below the 1st or above the 99th percentile, as well as observations with dependent-variable values above the 99.5th percentile. Single-worker firms and not-yet-treated firms upon adoption are also excluded, as are firms first eligible after January 2021. Control variables include the weighted rating two months before eligibility, firm's age, and firm type (micro, small, medium, or large). We further control for pre-April 2020 averages of weighted rating, age, and firm type interacted with the post-April 2020 dummy to capture heterogeneous baseline levels. The dependent variables are standardized using the mean of the variable's standard deviation among pre-March 2020 observations of treated firms observed from November 2019 to July 2021.

# C RDD - Additional Figures and Tables

Figure A3: Sovereign coverage from 98% to 95%



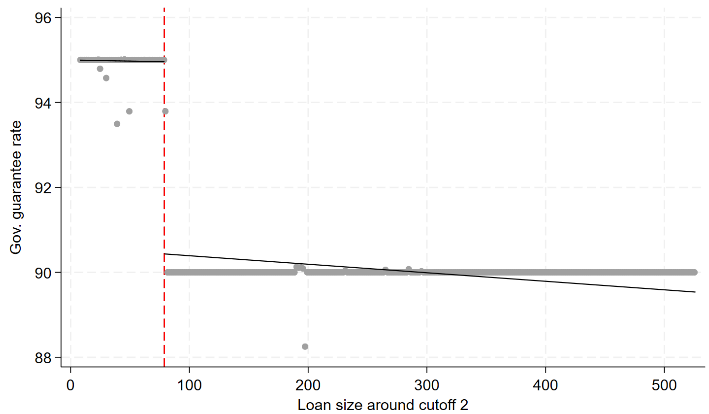

(A) REACTIVA 1



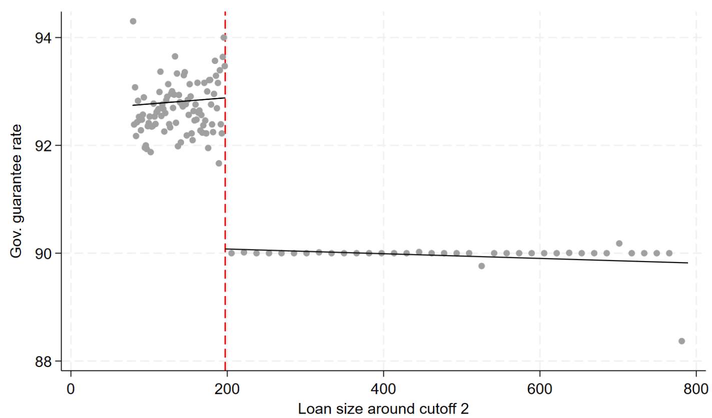

(B) REACTIVA 2

Notes: This figure illustrates the sharp discontinuity in the sovereign guarantee rate for the first REACTIVA loan, controlling for firm size (measured by sales). Panel A (REACTIVA 1): Threshold at US $7,895. The sample includes (i) early-takers, and (ii) non-flex active borrowers. Panel B (REACTIVA 2): Threshold at US $23,685. The sample includes (i) late-takers, and (ii) flex active borrowers.

Figure A4: Sovereign coverage from 95% to 90%



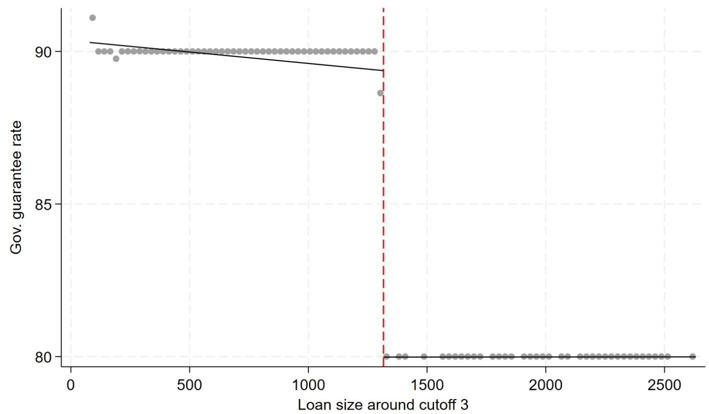

(A) REACTIVA 1



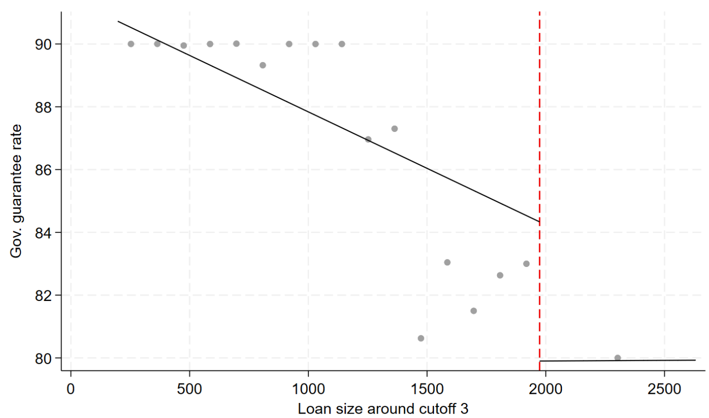

(B) REACTIVA 2

Notes: This figure illustrates the sharp discontinuity in the sovereign guarantee rate for the first REACTIVA loan, controlling for firm size (measured by sales). Panel A (REACTIVA 1): Threshold at US $78,947. The sample includes (i) early-takers, and (ii) non-flex active borrowers. Panel B (REACTIVA 2): Threshold at US $197,368. The sample includes (i) late-takers, and (ii) flex active borrowers.

Figure A5: Sovereign coverage from 90% to 80%



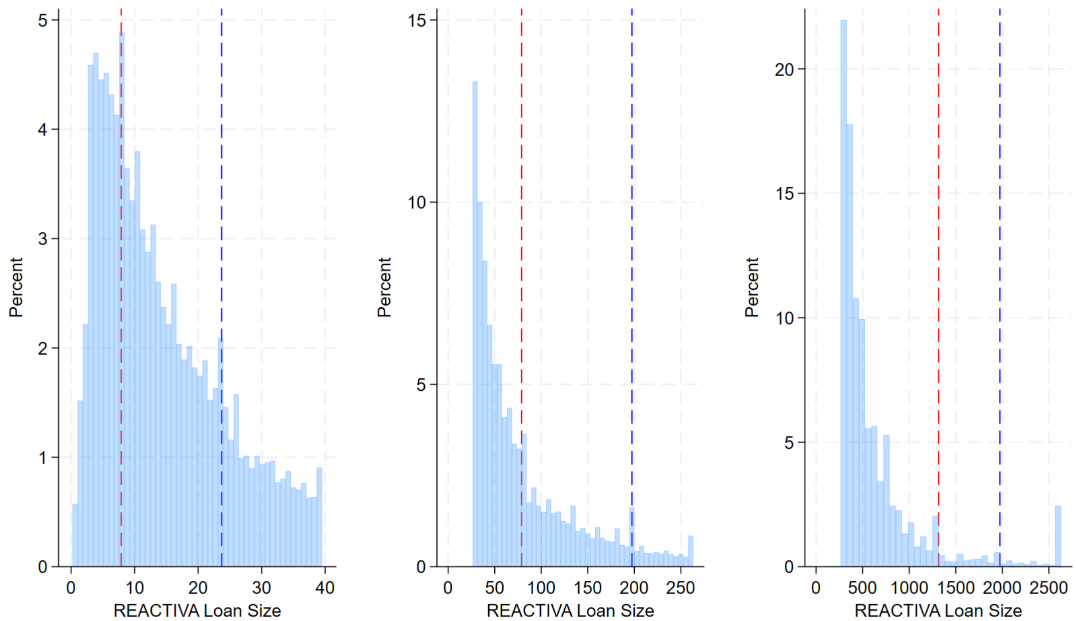

(A) REACTIVA 1




(B) REACTIVA 2

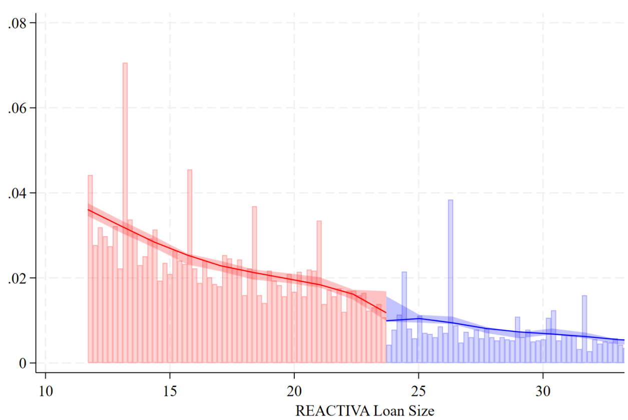
Notes: This figure illustrates the sharp discontinuity in the sovereign guarantee rate for the first REACTIVA loan, controlling for firm size (measured by sales). Panel A (REACTIVA 1): Threshold at US $1 315,789. The sample includes (i) early-takers, and (ii) non-flex active borrowers. Panel B (REACTIVA 2): Threshold at US $1 973,684. The sample includes (i) late-takers, and (ii) flex active borrowers. However, the effective number of observations to the right and to the left of this plot is 13 and 91, respectively.

Figure A6: Distribution of REACTIVA loan sizes around the three generosity-level thresholds

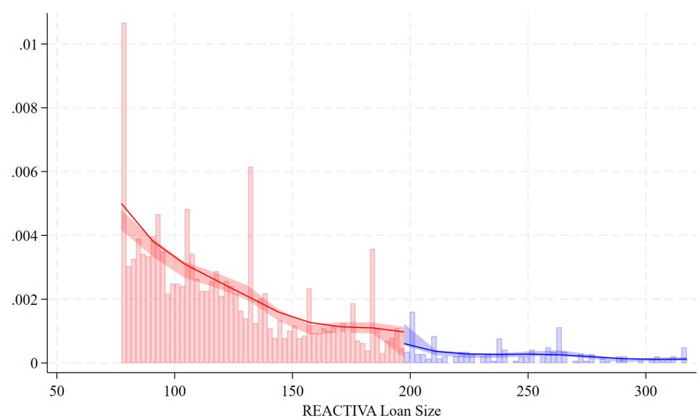



Each panel corresponds to one threshold, showing the histogram of loan sizes for the full sample of REACTIVA borrowers. The red dashed line indicates the REACTIVA 1 threshold, and the blue dashed line indicates the REACTIVA 2 threshold.

Figure A7: Mc Crary Test for only entrants




(A) Cutoff 98% - 95%




(B) Cutoff 95% - 90%

Table 6: Impact on New Credit Creation Around $\bar{c}_1^2 = US\ \$23,685$

| | **(1)** t = 0 | **(2)** 4m after | **(3)** 8m after | **(4)** 12m after | **(5)** 24m after |
|---|---|---|---|---|---|
| Net direct credits | 7.577** | 6.421** | 5.352* | 8.540* | 14.79* |
| | (2.967) | (2.617) | (2.993) | (4.506) | (7.918) |
| Net outstanding credit | 7.578** | 6.418** | 5.293* | 6.828 | 15.36* |
| | (2.967) | (2.617) | (3.200) | (5.638) | (8.942) |
| Unguaranteed credit ratio | 12.97*** | 11.10*** | 11.20** | 11.23* | 8.082 |
| | (3.018) | (3.879) | (4.594) | (6.683) | (5.828) |
| Observations | 1465 | 1465 | 1448 | 1448 | 1342 |

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Notes: The table reports the RD estimates around the first threshold $\bar{c}_1^2 = US\ \$23,685$, for borrowers, referred to as Entrants, who obtained credit due to the flexibilization of REACTIVA 2. Estimates are shown at different horizons. Columns (1) presents the instantaneous effect on the outcome variables, while Columns (2)-(5) report effects 4, 8, 12, and 24 months after receiving credit. All regressions include time and industry fixed effects, the initial month of debt one month prior to REACTIVA, the 12-month average credit score, and a categorical variable capturing firm size.

Table 7: Impact on New Credit Creation Around $\bar{c}_2^2 = US\ \$197,370$

| | **(1)** t = 0 | **(2)** 4m after | **(3)** 8m after | **(4)** 12m after | **(5)** 24m after |
|---|---|---|---|---|---|
| Net direct credits | -2.449 | -20.93*** | -14.96*** | 20.28** | 402.9*** |
| | (7.766) | (5.585) | (4.974) | (9.150) | (42.89) |
| Net outstanding credits | -2.452 | -20.93*** | -14.96*** | 75.21*** | 554.6*** |
| | (7.766) | (5.585) | (4.974) | (20.49) | (70.87) |
| Unguaranteed credit ratio | 1.719 | -2.267 | -1.047 | 8.255*** | 78.59*** |
| | (2.410) | (1.974) | (1.751) | (2.165) | (5.337) |
| Observations | 632 | 629 | 625 | 623 | 590 |

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Notes: The table reports the RD estimates around the second threshold $\bar{c}_2^2 = US\ \$197,370$, for borrowers, referred to as Entrants, who obtained credit due to the flexibilization of REACTIVA 2. Estimates are shown at different horizons. Columns (1) presents the instantaneous effect on the outcome variables, while Columns (2)-(5) report effects 4, 12, 24, and 27 months after receiving credit. All regressions include time and industry fixed effects, the initial month of debt one month prior to REACTIVA, the 12-month average credit score, and a categorical variable capturing firm size.

Table 8: Impact on Non-Performing Loans Around $\bar{c}_1^2 = US\ \$23,685$

| | **(1)** t = 0 | **(2)** 4m after | **(3)** 12m after | **(4)** 24m after | **(5)** 27m after |
|---|---|---|---|---|---|
| Non-performing loans | -0.00120 | 0.00294* | -0.0444* | -1.144 | 2.770* |
| | (0.000736) | (0.00156) | (0.0243) | (0.814) | (1.471) |
| Net non-performing loans | -0.00120 | 0.00294* | -0.0444* | 0.659 | 3.222* |
| | (0.000736) | (0.00156) | (0.0243) | (1.396) | (1.863) |
| NPL credit ratio | -0.00508 | 0.0112 | -0.00146 | -0.661 | 5.484 |
| | (0.00316) | (0.00688) | (0.00907) | (2.147) | (3.491) |
| Reprogrammed credit ratio | -0.00508 | 0.0112 | -13.77 | -5.163 | -21.00 |
| | (0.00316) | (0.00688) | (11.06) | (11.63) | (17.53) |
| Observations | 1465 | 1465 | 1448 | 1342 | 947 |

Standard errors in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

Notes: The table reports the RD estimates around the first threshold $\bar{c}_1^2 = US\ \$23,685$, for borrowers, referred to as Entrants, who obtained credit due to the flexibilization of REACTIVA 2. Estimates are shown at different horizons. Columns (1) presents the instantaneous effect on the outcome variables, while Columns (2)-(5) report effects 4, 12, 24, and 27 months after receiving credit. All regressions include time and industry fixed effects, the initial month of debt one month prior to REACTIVA, the 12-month average credit score, and a categorical variable capturing firm size.

Table 9: Impact on Non-Performing Loans Around $\bar{c}_2^2 = US\ \$197,370$

| | **(1)** t = 0 | **(2)** 4m after | **(3)** 12m after | **(4)** 24m after | **(5)** 27m after |
|---|---|---|---|---|---|
| Non-performing loans | 0.00280*** | 0 | -0.384*** | 31.18*** | 67.91*** |
| | (0.000380) | (.) | (0.0357) | (4.055) | (25.98) |
| Net non-performing loans | 0.00280*** | 0 | -0.384*** | 16.35*** | 81.88* |
| | (0.000380) | (.) | (0.0357) | (2.208) | (48.15) |
| NPL credit ratio | 0.00167*** | 0 | -0.205*** | 4.823*** | 21.03* |
| | (0.000226) | (.) | (0.0191) | (0.942) | (12.37) |
| Reprogrammed credit ratio | 0.00167*** | 0 | 27.35*** | -49.91*** | 166.8* |
| | (0.000226) | (.) | (9.118) | (12.30) | (97.44) |
| Observations | 632 | 629 | 623 | 590 | 368 |

Standard errors in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

Notes: The table reports the RD estimates around the second threshold $\bar{c}_2^2 = US\ \$197,370$, for borrowers, referred to as Entrants, who obtained credit due to the flexibilization of REACTIVA 2. Estimates are shown at different horizons. Columns (1) presents the instantaneous effect on the outcome variables, while Columns (2)-(5) report effects 4, 12, 24, and 27 months after receiving credit. All regressions include time and industry fixed effects, the initial month of debt one month prior to REACTIVA, the 12-month average credit score, and a categorical variable capturing firm size.

Table 10: Impact on Sales and Labor Demand Around $\bar{c}_1^2 = US\ \$23,685$

| | (1) 3m before | (2) 1m before | (3) t = 0 | (4) 6m after | (5) 12m after | (6) 24m after |
|---|---|---|---|---|---|---|
| Average sales | -22.03*** | 1.866 | 5.587** | 21.54 | 4.439 | 2.178 |
| | (3.019) | (2.275) | (2.722) | (20.56) | (5.222) | (14.74) |
| Average workers | 5.981** | -1.157 | 2.939 | 2.586 | 16.60 | -2.202 |
| | (2.716) | (1.828) | (2.813) | (2.905) | (11.56) | (2.405) |
| Observations | 147 | 712 | 937 | 952 | 947 | 900 |
| Mid-point sales | -51.25 | 48.99 | 74.84** | 119.6*** | 120.9*** | 120.1** |
| | (37.97) | (36.36) | (37.05) | (38.35) | (38.65) | (59.15) |
| Mid-point workers | -0.284 | 0.0109 | 2.742 | -2.276* | -1.948 | -1.725 |
| | (1.890) | (1.936) | (2.182) | (1.379) | (1.415) | (1.194) |
| Observations | 224 | 1107 | 1465 | 1451 | 1448 | 1362 |

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Notes: The table reports the RD estimates around the first threshold $\bar{c}_1^2 = US\ \$23,685$, for borrowers, referred to as Entrants, who obtained credit due to the flexibilization of REACTIVA 2. Estimates are shown at different horizons. Columns (1)-(2) present effects on the outcome variables three and one months before the borrower received credit, Column (3) presents the instantaneous effect, while Columns (4)-(6) report effects 6, 12, and 24 months after receiving credit. All regressions include time and industry fixed effects, the initial month of debt one month prior to REACTIVA, the 12-month average credit score, and a categorical variable capturing firm size.

Table 11: Impact on Sales and Labor Demand Around $\bar{c}_2^2 = US\ \$197,370$

| | (1) 2m before | (2) 1m before | (3) t = 0 | (4) 6m after | (5) 12m after | (6) 24m after |
|---|---|---|---|---|---|---|
| Average sales | 69.99** | -14.50** | 16.04* | 1078.6*** | 2198.1*** | 3550.5*** |
| | (30.72) | (5.957) | (8.651) | (262.2) | (344.7) | (507.0) |
| Average workers | 205.9 | 51.95** | 109.1*** | 108.7*** | -46.21*** | 247.3*** |
| | (212.4) | (21.85) | (20.15) | (22.71) | (8.509) | (23.22) |
| Observations | 153 | 403 | 510 | 507 | 505 | 455 |
| Mid-point sales | 1798.0*** | 450.8*** | 373.3** | 618.3*** | 618.3*** | 2465.1*** |
| | (414.8) | (102.5) | (146.5) | (149.3) | (150.0) | (205.3) |
| Mid-point workers | 39.80** | 33.39*** | 32.85*** | 13.16*** | 17.11*** | 33.10*** |
| | (17.37) | (1.792) | (4.153) | (2.861) | (2.838) | (3.125) |
| Observations | 195 | 506 | 633 | 626 | 623 | 591 |

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Notes: The table reports the RD estimates around the second threshold $\bar{c}_2^2 = US\ \$197,370$, for borrowers, referred to as Entrants, who obtained credit due to the flexibilization of REACTIVA 2. Estimates are shown at different horizons. Columns (1)-(2) present effects on the outcome variables two and one months before the borrower received credit, Column (3) presents the instantaneous effect, while Columns (4)-(6) report effects 6, 12, and 24 months after receiving credit. All regressions include time and industry fixed effects, the initial month of debt one month prior to REACTIVA, the 12-month average credit score, and a categorical variable capturing firm size.