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Crisis Credit, Employment Protection, Indebtedness, and Risk

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Abstract

This paper studies how credit guarantee and employment protection programs interact in supporting firms during crises, using real and financial administrative data for all firms and a quantitative macroeconomic model. Low interest rates encourage riskier firms to demand government-backed loans, while banks tend to reject those applications. The credit demand outweighs this screening response, expanding indebtedness. Given the opportunity cost of shutting down, the employment program's take-up is unrelated to risk. The employment program mitigates the credit program expansion by supporting firms and enabling banks to screen them. Counterfactuals show how policy ingredients, including interest rate caps, limit macroeconomic risk.

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Crisis Credit, Employment Protection, Indebtedness, and Risk*

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1 Introduction

During economic crises, governments tend to support struggling firms to survive and recover more quickly by providing financing and assistance to keep workers employed, among other measures. In doing so, they must balance the need to quickly reach broad coverage across firms with the risk of potentially distributing untargeted assistance to firms that do not need help or are too risky. As governments inject credit during crises, or "crisis credit," and provide benefits to protect employment, they may increase overall private sector indebtedness, leading to financial instability and potential fiscal costs. The consequences of these programs hinge critically on how the benefits are distributed across different types of firms.

In this paper, we analyze how credit guarantee and employment protection programs interact in assisting firms during crisis times, using the context of the COVID-19 pandemic.[1] In particular, we analyze the adoption of those programs by firms with varying risk characteristics and differing exposure to the pandemic, including exposure to the exogenous implementation of mandatory lockdowns over time across municipalities. Moreover, we study the micro- and macro-level effects of the credit and employment programs on indebtedness and risk, given the equilibrium behavior of firms, banks, and the government. We evaluate the risk to the banking system and the government using both *ex ante* measures of expected loss (evaluated when the programs are distributed) and *ex post* default (after the pandemic shock is realized). Finally, we explore how the different conditions of each program and how different counterfactual scenarios affect aggregate indebtedness and risk. Our analysis focuses on the positive, not the normative, aspects of these two programs.

The public credit guarantee program (henceforth, the credit program) implemented in Chile in early 2020 is a large government facility that grants bank credit for 4.6% of gross domestic product (GDP). A concurrent employment insurance program allows firms to cover salaries while employees are not reporting to work due to the pandemic, amounting to 0.62% of GDP.[2] We collect transaction-level information on the universe of bank credit to all firms, including loan applications from firms and corresponding approvals or rejections from banks. Further, we complement this data with information about firms' use of the employment program. We match the financial data with administrative tax data for the universe of formal

[1]Credit and employment programs are part of a wider array of policies implemented during the COVID-19 pandemic to deal with the crisis. For comprehensive recounts, see Harvard's Kennedy School, the IMF, Cirera et al. (2021), and Feyen et al. (2021).

[2]Chile's credit guarantee program is similar to many credit programs implemented in Asia, Europe, and Latin America during the pandemic. For a review of some of those cases, see Anderson et al. (2023) and Hong and Lucas (2023a,b). Those programs differ from the well-known U.S. Paycheck Protection Program (PPP) in that the latter offers loans that effectively convert into grants to firms instead of loan guarantees.

firms. We use the unique microdata to study how the firm credit distribution aggregates into macroeconomic outcomes.

We find that the programs give different incentives to firms. Riskier firms are more likely to obtain a credit guarantee loan, while risk is not associated with the likelihood of utilizing the employment program. On the other hand, firms facing negative sales growth are more likely to use the employment program. Likewise, firms subject to lockdowns are more likely to use the employment program, whereas lockdowns seem irrelevant for the credit program. Overall, this evidence suggests that firms internalize the opportunity cost of using the employment program, which covers the salaries of employees who cannot work due to declining sales or restricted local mobility. Sending employees home imposes a cost on the firm because it has to reduce its operations. In contrast, the credit program has no such opportunity cost, leading to a much broader adoption. However, its low interest rates and government guarantees appear to be related to riskier firms using the credit program.

The fact that the credit and employment programs coexist has consequences for their adoption. Both programs are used jointly, i.e., increasing the probability of using one program increases the chances of using the other. Firms facing both positive and negative sales growth and riskier firms are more likely to use both programs, while the effect is stronger for firms facing a negative sales growth shock. Also, using both programs mitigates the increase in firm indebtedness as firms need less credit when they receive employment benefits. In addition, using the employment program might signal firms that are in distress or that need to scale down operations. As a result, the availability of the employment program increases the quality of borrowers by helping banks screen firms for the credit program.

In terms of risk and selection at the micro level, both the demand and supply sides play a role in the credit allocation in equilibrium, with demand factors driving the expansion of indebtedness. The allocation of credit is characterized by a shift in lending toward riskier firms, which is observed in both the extensive margin (selection into the program by riskier firms) and the intensive margin (increases in indebtedness, especially among riskier firms). On the demand side, riskier firms are more likely to apply for guaranteed loans. On the supply side, they are less likely to be approved, indicating that bank screening mitigates the allocation of guaranteed loans to riskier firms. Banks are more sensitive to risk when evaluating credit applications from larger firms, which are less covered by credit guarantees and would entail a more significant loss for banks in case of default.

At the macro level, we compute the credit allocation to formal firms with different risk profiles and calculate an aggregate expected loss of 0.27% of GDP for the baseline scenario, of which 41% is absorbed by the government and 59% by the banking system. This expected loss represents 1.07% of banks' equity capital. When considering formal firms and natural persons, the total expected loss rises to 0.45% of GDP. This aggregate risk level does not increase substantially if we use *ex post* default rates instead of *ex ante* expected default. However, as shown in our counterfactual analyses, aggregate risk could have been significantly higher if the program offered different conditions and incentives.

To conduct the empirical counterfactual exercises, we perform a series of sensitivity analyses that yield four alternative scenarios. In each, we modify different policy dimensions to recalculate how credit to firms and default generate different expected credit losses (the product of credit growth and the average expected default probability). We find that the policy design plays a significant role in determining aggregate risk. For example, allowing firms in default to be eligible would increase expected credit loss by a factor of 2.7 (=9.9/3.7).

In the last counterfactual scenario, we estimate the aggregate expected loss in the absence of the employment program, which affects both borrower quality and credit quantity. Without the employment program, borrower quality declines, raising the baseline default probability from 7.4% to 8.5%. Also, credit increases from the baseline of 3.6% to 4.2% of GDP. The combined effects on the default probability and credit imply that aggregate expected credit loss increases from 3.7% to 5% of credit, a third larger than the baseline scenario.

Whereas our empirical counterfactuals are informative about how policy design affects aggregate risk, they are partial equilibrium analyses that abstract from considering firms' and banks' endogenous responses. To account for their joint behavior, we develop a quantitative model of endogenous credit and default decisions. The model is based on Covas and Den Haan (2012) and incorporates key institutional features of the Chilean economy and the employment and credit policies. We calibrate the model with pre-COVID moments and implement several counterfactuals to understand the role played by different policy ingredients.

Although endogenous forces matter, the central message of the model counterfactuals is broadly in line with that of the empirical counterfactual analyses, while offering additional insights. The aggregate expected credit loss remains limited even after considering the endogenous credit response of firms and banks and the general equilibrium effects of the credit policy. Still, these endogenous responses shape the counterfactual outcomes. The model captures the increase in credit as an endogenous response to the credit guarantee and higher willingness to lend. Yet aggregate expected losses remain contained because the policy interest rate cap incentivizes lending to low-risk firms. When the cap is relaxed, banks extend more credit to high-risk firms at high rates, increasing credit losses. Another insight concerns endogenous recovery rates: higher recovery rates under default further reduce the government's burden. Given that default and interest rates are similar in the data and banks face a funding cost, the model implies that recovery rates are substantial in default for lenders to break even. Lastly, under a systemic shock such as the Global Financial Crisis, risks rise sharply, with expected credit losses more than doubling relative to normal times.

The lessons from this paper extend beyond Chile and the pandemic and could be informative for credit and employment policy responses to future crises. Our findings on aggregate indebtedness and risk suggest that several mitigating factors help constrain aggregate risk. On the policy front, the credit program imposes caps on the amount of credit at the firm level depending on its sales, excludes firms with previous defaults, and establishes an interest rate ceiling, effectively marginalizing the riskiest firms in the economy. Because the guarantee is partial, banks have skin in the game and incentives to screen firms. Other mitigating factors are related to the equilibrium behavior. Most credit flows toward large and safe borrowers. Although their debt increases the least in proportional terms, their large *ex ante* sales volume makes them major recipients of new loans. Furthermore, *ex ante* default risk is generally low (and low realized *ex post* as well) and well-capitalized banks can sustain an increase in leverage.[3] Overall, the results show that broadly distributing credit to risky firms that demand it could translate to less aggregate government or banking sector risks than the micro evidence might suggest, especially when mitigating factors exist and when an alternative employment program covers some of the firms' needs.

This paper contributes to different strands of the literature. Part of this literature relates public credit guarantees and other credit programs to employment. Some papers study how firms use credit programs in Chile, France, Portugal, the U.K., and the U.S. to employ workers or keep them on their payroll (Brown and Earle, 2017; Hubbard and Strain, 2020; González-Uribe and Wang, 2021; Albagli et al., 2023; Bonfim et al., 2023; Barrot et al., 2024). Those papers tend to find a positive effect on employment.[4] Only two papers directly compare public credit guarantees and employment programs. Custodio et al. (2022) measures the response of firms to targeted emails with information about a credit guarantee and a layoff support program in Portugal. The paper finds that new information positively affects

[3]In fact, the solvency of banks increases because of both a capitalization incentivized by regulation and a reduction of risk-weighted assets, given the program's guarantees.

[4]A separate literature studies the impact of employment programs per se, highlighting how they help firms mitigate the consequences of crises on employment (Hijzen and Venn, 2011; Cahuc et al., 2021; Bennedsen et al., 2020; Kopp and Siegenthaler, 2021; Giupponi and Landais, 2022).

applications to the employment program but not to the credit program. Autor et al. (2022) studies the distribution of the PPP and the unemployment insurance program across different U.S. households, showing that the credit program is more regressive.[5]

We complement this literature by examining the interaction between a credit program with a concurrent employment program. We analyze firms' applications to each program, the banks' responses to credit applications conditional on firms using the employment program, and the likelihood of using either program. We also estimate the increase in indebtedness among firms that use both programs. Comparing the use of both programs sheds new light on how incentives play an important role in firm demand, bank decisions about applications, and the equilibrium outcome. This is useful for the design of government assistance in times of crisis.

Another strand of the literature studies whether public credit guarantees give banks fewer incentives to screen for bad loans and increase the credit supply, particularly to risky firms. Evidence from Italy and Spain shows that firms with *ex ante* higher leverage, fragilities, and credit risk are more likely to receive publicly backed credit (Jiménez et al., 2018; Cascarino et al., 2022; Core and De Marco, 2023). Related papers study whether allocating credit to *ex ante* risky firms leads to larger *ex post* default. Evidence from France, Italy, and Japan suggests that public credit guarantees increase the probability of default (Lelarge et al., 2010; Uesugi et al., 2010; de Blasio et al., 2018). Still, others find no significant effects in Chile in 2011-2012 (Mullins and Toro, 2018) or even a reduction in the probability of credit default in Türkiye (Akcigit et al., 2021). Others estimate the excess mass of loans in the U.S. around the guarantee threshold (Bachas et al., 2021), and compare credit volumes and interest rates in Spain (Jimenez et al., 2024) and credit volumes of guaranteed and non-guaranteed loans in Germany, France, Italy, and Spain (Altavilla et al., 2025). They find that supply-side factors drive the increase in bank lending and the substitution between guaranteed and non-guaranteed loans.

Unlike previous papers, our unique data with applications and approvals for the universe of firms and banks allow us to precisely identify the supply and demand for credit. The data also enable us to study how risk and the pandemic drive credit demand and bank responses, determining the equilibrium allocation of credit across firms. The program's characteristics, with different guarantee coverage according to firm size, permit us to analyze how banks

[5]Other papers on the PPP study the distribution of credit across firms and find that previous relations between banks and firms increase a firm's chances of receiving a loan (Amiram and Rabetti, 2020; Bartik, Cullen, Glaeser, Luca, Stanton, and Sunderam, Bartik et al.; Balyuk et al., 2021; Chodorow-Reich et al., 2021; Duchin et al., 2021; Li and Strahan, 2021).

respond when facing credit demand from firms with different characteristics.

Lastly, we contribute to the literature addressing the macro-level consequences of micro-level decisions when credit becomes available for firms to borrow at a large scale. Evidence from Chile, France, Peru, Asia, and Europe shows that public credit programs, including credit guarantees, can reduce liquidity shortfalls, insolvencies, firm failures, and firm dependence on foreign currency debt while improving overall financial stability (Gourinchas et al., 2020; Diez et al., 2021; Martin et al., 2021; Acosta-Henao et al., 2024; Demmou et al., 2022; Acurio et al., 2023). But evidence from Italy, India, Japan, Europe, and the U.S. also shows that high debt accumulated during crises can lead to the emergence of zombie firms, low investment, debt overhang, financial distress, and macroeconomic problems (Caballero et al., 2008; Schivardi et al., 2021; Chari et al., 2021; Kalemli-Ozcan et al., 2022; Reinhart, 2022; Xiao, 2022), especially when governments are involved (Brunnermeier and Krishnamurthy, 2020; Demmou et al., 2021; Banerjee and Hofmann, 2022; Serhan and Fedor, 2022).

We add to this literature by examining an economy-wide government credit program, similar to those implemented worldwide, but with unique data on the universe of lenders and firms. To the best of our knowledge, this is the first paper that uses this kind of information to directly connect the micro-level credit decisions with the macro-level implications of a credit program. We contribute by analyzing under which conditions aggregate risk increases and whether changes in expected default or the program's size drive the macro effects. This has direct implications for policy analysis and the design of future credit programs to assist firms during crises.

The rest of the paper is organized as follows. Section 2 summarizes the credit guarantee and employment programs and the expansion of crisis credit. Section 3 presents the data. Section 4 describes the credit distribution across firms. Section 5 studies the effects on firm-level indebtedness. Section 6 analyzes the aggregate implications. Section 7 concludes.

2 Government Crisis Credit and Employment Programs

In response to the COVID-19 pandemic, the Chilean government implements two large-scale programs to support firms and avoid inefficient bankruptcies. It significantly expands the size and scope of an existing public credit guarantee program, providing financing to firms during the pandemic and sharing the firm's credit risk with banks. The original program, called FOGAPE, is a public fund that guarantees a fraction of loans provided by banks to small firms.[6] In the event of default, resources are withdrawn from the fund to pay the guaranteed fraction of the loan to the bank. The program is similar to the other public credit guarantee programs used worldwide.

On April 24, 2020, the Congress of Chile approves a bill (called FOGAPE COVID-19) that injects US$3 billion into the public credit guarantee fund.[7] The goal of the new program is to "promote, facilitate, and expand access to liquidity to firms, especially to those affected by the pandemic" (Ministry of Finance), expanding access beyond small firms.[8] Guaranteed loans are designed to finance working capital equivalent to up to three months of sales.[9] These loans are term loans, not lines of credit, with a six-month grace period and repayment in installments over the following 24 to 48 months.[10] They have a low interest rate cap of 3.5%, equal to the sum of the monetary policy rate (0.5%) and the inflation target (3%).[11] This cap is notably lower than the interest rate of non-guaranteed loans during the same period (9%) and implies a real interest rate close to 0%.[12]

As a condition for granting a guaranteed loan, banks are required to postpone repayments of a firm's outstanding debt for six months: during this period, the firm suspends monthly installment payments on existing debt and cannot pay off the principal. Only firms that are up to date with their debt payments (no more than 30 days past due) at the moment of applying are eligible. The bank performs a risk analysis of the firm and can either accept or reject the application. The program is partial, meaning banks retain some "skin in the game," which creates incentives to screen and monitor borrowers. The guarantee decreases with firm size: it is 85%, 80%, 70%, and 60% for small, medium, medium-large, and large firms, respectively.[13] To further align bank incentives, the guarantee is effective after applying a

[6] Small firms are defined as those with annual sales less than US$0.8 million. FOGAPE is an acronym for Fondo de Garantía para Pequeños Empresarios or Guarantee Fund for Small Entrepreneurs.

[7] The government can leverage the fund up to eight times, so it can eventually guarantee bank lending for up to US$24 billion during the program's existence.

[8] The firms more affected by the pandemic are not necessarily the riskier firms *ex ante*. For example, the pandemic severely hits well-established and profitable firms like hotels, restaurants, and casinos, with solid balance sheets and good prospects before the pandemic.

[9] Measured as the average for the pre-pandemic period (January to December 2019).

[10] The vast majority of loans (76.3%) have a maturity of 48 months.

[11] As a reference, the monetary policy rate reaches its technical minimum (0.5%) in April 2020 and remains at that level for more than one year (the Central Bank of Chile slowly starts normalizing monetary policy in August 2021). Also, the bank deposit rate during this period is, on average, 0.26%.

[12] Banks cannot charge other fees or administrative costs related to the credit program. Despite the low interest rate, many banks use the program's window of opportunity to help their customers. Also, banks face some social pressure to lend through the credit program to keep their reputation of being "good citizens."

[13] Medium firms have annual sales between US$0.8 and US$3.5 million, medium-large firms between US$3.5 and US$21 million, large firms between US$21 and US$35 million, and mega firms above US$35 million. For simplicity, in the rest of the paper, we lump together medium-large and large firms, calling all of them "large firms." The different sales limits are defined in Unidades de Fomento (UF), Chile's unit of account. We transform the values from UF to U.S. dollars using the average value of UF in pesos during 2019 and the dollar-peso exchange rate during 2020.

first-loss deductible of 5% for small firms, 3.5% for medium firms, and 2.5% for medium-large and large firms.[14]

In parallel, on April 1, 2020, Congress approves the Employment Protection Act, which allowed firms to cover salaries and maintain employment contracts while employees are not working. Like the credit program, the employment program expands an existing program, a mandatory unemployment insurance program funded jointly by workers, firms, and the government.[15] Workers' salaries are covered through withdrawals from the existing unemployment insurance fund. The government injects US$2 billion into the solidarity component of the fund.[16] Firms can apply for either total or partial employment protection.[17] The only requirement for accessing the program is a voluntary mutual agreement between the firm and the employee to freeze the employment contract. Conditional on this agreement, all applications are approved.[18]

A key difference between the two programs lies in the cost to the firms. The employment program is significantly more expensive. It does not have a direct cost, but it has an important opportunity cost: the firm has to temporarily scale down operations in proportion to the number of workers under the program, thereby losing all profits associated with reduced activity. The credit program, on the other hand, has a low direct cost (given its low interest rate) and a minimal opportunity cost.

In terms of reach, the credit program in Chile is fast and sizable. Banks provide the majority of guaranteed loans within the first two months, over US$8 billion, or 3.3% of 2019 GDP (Figure 1, Panel A). By the end of the year, the program reaches US$11.5 billion (4.6% of 2019 GDP). This amount is substantial compared to a total credit expansion of 4.7% of GDP during 2019 and stands in contrast to the credit collapse observed during past crises.

[14] As a result, when portfolio default rates are relatively high (low), the government (bank) absorbs most of the credit risk.

[15] The insurance fund has an individual and a "solidarity component." Workers contribute a fraction (0.6%) of their wages every month, which is deposited directly into their individual fund accounts. Firms contribute a fraction (2.4%) of each worker's wage (two-thirds going to the individual account, and the rest to a solidarity fund). The government makes a variable yearly fiscal contribution to the solidarity fund. When a worker is fired for reasons attributable to the firm, they can withdraw from their individual account. Once the individual account is empty, the worker can withdraw from the solidarity fund.

[16] The program lasts until October 2021. If the US$2 billion were exhausted, the employment support would end. *Ex post*, the fund is not exhausted.

[17] Under partial protection (used in only 5.5% of all employment shutdowns), firms and workers agree on a partial reduction of the work schedule (up to 50%). Total shutdown implies a complete reduction of the work schedule.

[18] If there is no agreement, firms can still adopt the program. Employees can accept it or opt out. If they refuse, two scenarios arise: (1) dismissal due to business needs if cost reductions are necessary; employers can dismiss the employee with severance under the labor code; (2) renegotiation of conditions if refusal prompts changes in hours or job roles. Evidence shows that refusals are rare. Sole proprietors cannot pay themselves under the program.

The credit program offsets the 2020 contraction of net credit granted outside its purview (as GDP suffers a 5.8% negative shock). In comparison, the employment program is much smaller in scale. Its size, calculated as the sum of wage bill savings from protected workers across all participating firms, reached just 0.62% of GDP by December 2020. This implies that the credit program was roughly seven times larger in terms of allocated funds.

A sizable share of firms, almost 24% (=102,648/434,411) of eligible firms, obtain guaranteed loans by December 2020 (Figure 1, Panel B).[19,20] This is a large take-up compared to other Latin American countries: in Peru, Colombia, and Mexico, 14%, 16%, and 23% of eligible firms, respectively, use the credit program.[21] In Italy, 16% of firms use the public credit guarantee program (Core and De Marco, 2023). The employment program is also used by a significant fraction of active firms: over 15% (=69,280/449,632) by December 2020. Approximately 31% (=140,374/449,632) of active firms participate in either program, and nearly 7% (=31,554/449,632) participate in both programs. In terms of the number of firms covered, both programs are roughly similar, which implies that the large difference in allocated funds comes from the significantly higher volume of credit given to firms.

Guaranteed loans overtake overall credit during 2020. Before the credit program starts, total credit is essentially equal to non-guaranteed credit (Figure 2, Panel A). After the program begins, cumulative non-guaranteed credit declines while guaranteed credit increases significantly. Consistent with findings for the U.S. (Li et al., 2020; Chodorow-Reich et al., 2021; Greenwald et al., 2023), non-guaranteed credit to mega firms expands rapidly during the initial two months of the pandemic (Figure 2, Panel B). However, three months after the pandemic begins, the loan growth rate to mega firms decreases, and credit shifts to small and medium firms (SMEs) and large firms. This contrasts strikingly with the collapse in credit during the 1998 Asian crisis and the 2009 subprime crisis (Didier et al., 2021), when Chile's public credit guarantee program is negligible.

As a comparison, total outstanding corporate debt increases by US$4 billion between January and December of 2020 (Figure 2, Panel C). This change can be decomposed as the sum of the change in program debt (+US$11 billion) and the change in non-program debt (-US$7 billion). The net increase in total debt of US$4 billion is significantly smaller than

[19]The 24% take-up is based on the active firms sample. The take-up is higher (36%=40,901/114,606) when considering more restrictive samples. Details about these samples can be found in Appendix Table 1.

[20]Both firms with and without credit history use the program: 61% of active firms with a guaranteed loan have a previous loan, while 39% do not. These groups receive 92.6% and 7.4% of the total guaranteed credit, respectively. Among SMEs, 8.5% of funds go to firms without loans, and 91.5% to firms with loans. Fewer qualification requirements could have increased SME participation without previous loans.

[21]This information is obtained from reports by the Central Reserve Bank and the National Institute of Statistics and Informatics of Peru, the National Guarantee Fund of Colombia, and the Government of Mexico.

the credit program because many mega firms, who did not participate in the program, either chose not to, or were unable to, roll over their existing debt maturing in 2020, resulting in a decline in outstanding non-program debt.[22]

3 Data

We use three administrative data sets from various sources in Chile. These data sets cover the entire formal private sector in Chile in rich detail, including credit flows and balances, default history, and terms of individual transactions. The firm-level data contain financial statements, input use, and sales collected monthly, along with the industry and location of firms. Below we describe the data sources, sample selection, and key variables.

First, we use granular confidential bank-to-firm information compiled by the Financial Market Commission (the financial supervisory agency) for all firms using the entire banking system. We have information on stocks and flows of credit. For stocks (i.e., loans outstanding), we have data on the amount of debt each firm has with each bank in the system every month. We also know the number of days each loan in the system is past due. For flows, we have transaction-level data on each loan received by each firm, including information on the loan amount, interest rate, and loan maturity. We complement the bank data with unique data on the credit program we analyze, including detailed information on loan applications by firms (such as the amount requested) as well as banks' decisions (such as whether a loan request is approved or rejected and the approved amount). These data allow us to measure selection and disentangle supply and demand factors in the allocation of credit across the whole economy.

Second, we use confidential administrative tax records from Chile's tax authority (Servicio de Impuestos Internos). These data contain monthly, firm-level information, including sales, materials expenditure, value added, number of workers, wage bill, net worth, age, industry, and municipality. They allow us to construct measures of pre-pandemic firm attributes (such as productivity, measured as value added per worker) as well as firm performance during the pandemic, using monthly sales during 2020.

Third, we work with publicly available firm-level data on firms that use the employment program. The employment authority (Dirección del Trabajo) publishes these data monthly, containing the dates that each firm uses the program and the number of workers in each firm

[22]In Appendix Figure 1, we plot the evolution of the number of firms using each program, distinguishing between SMEs and non-SMEs. In both programs, SMEs make up the vast majority of participants, implying that the discrepancy in the dollar value of both programs reflects a strategic policy decision to scale up the credit program, not differences in firm size across programs.

that participate in it.

We merge these data sets using unique tax identifications of workers and firms that are common across sources. To secure the privacy of workers and firms, the Central Bank of Chile mandates that the development, extraction, and publication of the results should not allow the identification, directly or indirectly, of natural or legal persons. Officials of the Central Bank of Chile processed the disaggregated data and merged them with financial records from the Financial Market Commission. The authors implemented all the analysis and neither involved nor compromised the Central Bank of Chile, the Financial Market Commission, or the Chilean tax authority.[23] The merged data set allows us to study the real and financial aspects of both the credit and the employment programs covering the universe of firms. For most of the analysis, we use the 2018–2020 period of these data sets.[24]

4 Credit Distribution across Firms

4.1 Measuring Firm Risk

To assess a firm's *ex ante* credit risk, we estimate a default probability model, which we then use in our selection models of government programs. We estimate the following cross-sectional probit model to predict default during 2019, based on attributes during 2018:[25]

$$\Pr(Default_i = 1) = \Phi(\beta Characteristics_{i,-1} + \alpha_s + \alpha_m + u_i). \quad (1)$$

$Default_i$ is a dummy equal to one if the firm defaults on a loan during 2019 (i.e., has a loan past due more than 90 days) and equal to zero otherwise. $Characteristics_{i,-1}$ is a vector of *ex ante* firm-level attributes during 2018 that the literature uses to predict default rates (Glennon and Nigro, 2005; Crawford et al., 2018).[26] This vector contains five real economic variables reported to the tax authorities: net worth, value added per worker (a proxy for productivity), age, wage bill (a proxy for labor intensity), and sales (a proxy for size). It also includes two financial variables collected by the financial supervisory agency: debt outstanding and loan spread. The spread is the difference between the weighted average

[23]This study was developed within the scope of the research agenda conducted by the Central Bank of Chile in the economic and financial affairs of its competence. The Central Bank of Chile has access to anonymized information from various public and private entities through collaboration agreements signed with these institutions. The information contained in the databases of the Chilean tax authority is of a tax nature originating in self-declarations of taxpayers presented to the authority; therefore, the veracity of the data is not its responsibility.

[24]We build different samples of these datasets depending on the analysis. The different samples are described in Appendix A and Appendix Table 1.

[25]In Appendix Table 2, we estimate firm defaults in 2020 using firm characteristics from 2019. The estimated coefficients are very similar to those in our main analysis, confirming the robustness of the probit model estimated using 2019 data. Also, the explanatory power (R-squared) remains virtually unchanged, indicating that the model is similarly informative across years.

[26]The results hold if we use firm-level data during 2016–2019.

interest rate of the loans a firm received (using the loan amounts as weights) and the risk-free rate. We calculate this measure for the loans granted during 2012–2018 to use a longer time period. The spread reflects the *ex ante* perception of risk by banks that grant loans. We sequentially introduce industry and municipality fixed effects into the estimations.

Table 1 presents estimates of Equation (1) using different specifications.[27] Columns 1–4 include the real regressors for those firms (these specifications are most useful since these variables are available for the largest set of firms and can, therefore, be used below to impute risk measures even for firms without previous loans). Firms that have a higher net worth and are more productive, older, more labor intensive, and smaller have a significantly lower likelihood of default. The results remain unchanged for different sets of fixed effects. Columns 5–8 add the financial regressors, which have little impact on the coefficients of most of these real factors, with one exception. After controlling for outstanding debt, larger firms (according to sales) are less likely to default *ex post.* Controlling for real variables like net worth, firms with higher debt and spread are also more likely to default *ex post.* The results are robust to using different regressors and samples.[28]

To predict the risk of default during 2020, we use this model and plug in the real and financial variables for 2019. For firms with previous loans, we predict default risk using the estimated coefficients from Table 1, Column 8.[29] For firms with no previous loans, which by definition do not have financial information, we predict risk for 2020 using the estimated coefficients from Column 4, that is, plugging in the values of the real variables for 2019. The predicted default probability for firms with no previous loans is 10.7%, roughly 2 percentage points higher than for firms with previous loans. The risk measure for firms with a previous loan is more accurate because it is based on both real and financial data.[30]

The model does a good job of predicting *ex post* default rates for the sample of eligible firms with previous loans, the same sample we use in the selection regression in the next section. Figure 3, Panel A, presents the binscatter plot between predicted *ex ante* default

[27] Summary statistics of the variables used for this model are presented in Appendix Table 3.

[28] Among other things, we estimate the regression using the real regressors except net worth, a variable missing for 43% of the firms. We deal with this problem by using a dummy variable to indicate if the firm reports net worth or not. We also estimate the regression for the subset of firms that have both real and financial information. Furthermore, we use loan spread for the loans granted in 2012–2018 and for those granted only in 2018. We also include lagged default probability on the right-hand side of the regression. Last, we use the 2017–2018 sales variation as an additional control. The main results are robust to these extensions and are available in Appendix Table 4.

[29] This specification includes both industry and municipality fixed effects.

[30] To ensure that our estimates are not driven by the riots Chile experienced in October 2019, we re-estimate the model excluding the fourth quarter of 2019. That is, the new estimates use data from 2018 to predict default outcomes between January and September 2019, instead of the entire year 2019. The results, reported in Appendix Table 5, remain virtually unchanged, indicating that the social unrest in late 2019 does not affect our analysis.

risk and realized *ex post* default (for the period after 2020).[31] There is a strong positive correlation, with *ex ante* default risk explaining 75% of the variance of *ex post* defaults. Figure 3, Panel B shows the correlation between the same variables but using a non-parametric fit with a local polynomial smoothing to address the uncertainty of the correlation. For the majority of the mass of the distribution (up until .25 of the horizontal axis), the fit is tight. The model performs even better if one considers only the center of the distribution, where the mass of the correlation lies on top of the 45-degree line. For values in the lower tail of the distribution of *ex ante* risk, *ex post* default is higher, whereas for values in the upper tail of the distribution, *ex post* default is lower.[32,33] Our risk model appears to be a mean-preserving spread of *ex post* default, implying that *ex post* risk is less heterogeneous across firms than the prediction of our risk model.[34]

That said, banks face significant uncertainty at the beginning of 2020 about the intensity and duration of the pandemic. Our risk measure does not capture this increased uncertainty because it is calculated using data from 2019. Nevertheless, we believe our measure of default probability can be useful because it is based on the information set that banks have about each firm before the start of the pandemic (including firm age, measures of size, productivity, and indebtedness).

4.2 Selection into the Government Programs

We next focus on the selection into the credit and employment programs by firms with different characteristics. Based on the findings in the literature (Jiménez et al., 2018; Cascarino et al., 2022; Core and De Marco, 2023), we expect that riskier firms are more likely to apply to and receive program credit. The reason is that, as the government guarantees loans and sets a ceiling on interest rates, banks are more willing to lend (since they bear only part of the risk), while riskier firms (which typically face high borrowing costs) find it cheaper to borrow. On

[31] The *ex post* default rate is only one of the alternative measures available to assess *ex post* outcomes.

[32] We assess whether the relation between predicted and realized default holds for the sample of 96,411 firms used in Table 1 and before the pandemic. When using 96,411 firms, the R-squared rises to 0.891. This suggests that the additional observations allow the model to have more predictive power and, thus, provide a better goodness of fit, as shown in Appendix Figure 2, with the points aligning above the 45-degree line. To study how the model performs before the pandemic, we regress 2018 loan defaults on 2017 firm-level characteristics. We use that model to compare the predicted versus the actual defaults in 2019. The results, shown in Appendix Figure 3, mirror the original model: the fit is tighter at the center, suggesting this effect is not pandemic-specific.

[33] To test the robustness of our results across the *ex ante* risk distribution, we divide firms into terciles: low, medium, and high risk. For each group, we interact the original risk measure with a dummy indicating tercile membership ("low risk," "medium risk," and "high risk"). We then re-estimate the main regressions with these tercile-specific variables. The results show that the main conclusions hold for applications and credit use, while for approvals, the coefficient for the low-risk group is not statistically significant, indicating weaker sensitivity among the safest firms (see Appendix Table 6).

[34] This is not explained by differences of risk between sector-location bins. In fact, Figure 3 shows that the shape of the correlation between the two types of risk is robust after residualizing the sector-location variation.

the other hand, the literature is silent about how risk is related to the use of the employment program or the interaction between the credit and employment programs.

We estimate the following cross-sectional probit model among the sample of firms that fulfill the eligibility requirements of each program:

$$\begin{aligned} \Pr(Program\,Use_i = 1) \;&=\; \Phi(\beta Risk_i + \gamma Sales\,Growth_i \\ &\quad + \psi Other\ Program\,Use_i + \alpha_s + \alpha_m + u_i). \end{aligned} \tag{2}$$

$Program\,Use_i$ is a dummy equal to one if firm i, operating in sector s and located in municipality m, participates in the given public program; it is equal to zero otherwise. $Risk_i$ is estimated from the default probability model as explained above. Knowing that the relation between credit expansion and sales growth between February and April of 2020 is non-linear (Central Bank of Chile, 2020), we include two dummies for sales growth, which capture how much firms, both positively and negatively affected by the pandemic, use the credit program. First, the dummy "increase in sales" is equal to one if sales growth is greater than or equal to 2% (and zero if sales growth is less than 2%). Second, the dummy "decrease in sales" is equal to one if sales growth is lower than or equal to -2% (and zero if sales growth is greater than -2%).[35]

$Other\ Program\,Use_i$ is a dummy equal to one if firm i uses the other (credit or employment) government program and equal to zero otherwise. The results are robust to using instead a logit model or a linear probability model.[36] Because we measure risk *ex ante*, this variable does not reflect the risk related to the COVID-19 pandemic. To capture how *ex post* characteristics are related to program selection, we use sales growth, the other program use, and fixed effects.

Table 2, Panel A reports the selection results for firms with previous loans, for which we have a more accurate measure of risk.[37] Among firms operating in the same industry and located in the same municipality, riskier firms are more likely to obtain a guaranteed loan

[35]There is no clear *ex ante* threshold for sales growth to measure how firms can be affected by the pandemic. We rely on different estimates computed at the Central Bank of Chile to use a significant variation in sales and a large number of firms above and below the threshold. Firms with sales growth below -2% and above 2% capture 93% of the data. Our results are robust to using alternative thresholds of $\pm 1\%$, $\pm 3\%$, and $\pm 4\%$ for sales growth, to flexibly account for potential non-linearities around the baseline threshold (Appendix Table 7 and Appendix Table 8).

[36]The risk regressor of Equation (2) is itself an estimated variable (estimated from Equation 1), which could bias the standard errors. Given the computational difficulties in calculating bootstrapped standard errors in non-linear probit models with two sets of fixed effects (industry and municipality), we block-bootstrap the standard errors of the model's linear version. The standard errors remain essentially unchanged relative to the non-adjusted standard errors and can be found in Appendix Table 9. We repeat this procedure for all the other probit regressions that contain risk as an independent variable; we omit to report those results to save space, but they remain robust.

[37]Because the risk regressor is estimated based on a vector of *ex ante* firm characteristics (including age and size), the probit model of Equation (2) indirectly controls for all those firm characteristics.

(Column 1). For example, a shift from 25% to 75% in the risk distribution implies an increase of 3.4 percentage points in the likelihood of using the credit program ($= 0.343 \times (0.13 - 0.03)$). This represents an increase of 7% relative to the average likelihood of using the program ($= 0.034/0.505$).

What drives this program participation, supply, or demand? The answer is both.[38] Our data allows us to decompose the probability of obtaining a guaranteed loan as the product of the probability of applying for the loan (credit demand) and the probability of the bank approving the loan conditional on receiving an application (credit supply).[39] Riskier firms are more likely to apply for a guaranteed loan (Column 2).

Non-applicant firms fall into two groups: (1) those that didn't need credit, and (2) those that expected rejection ("discouraged" borrowers). The share of each group shapes the application regression coefficient. If discouraged (riskier) firms dominate, applicants are compared mainly to them, yielding a negative coefficient. If non-discouraged (safer) firms are more common, the coefficient is positive. We observe a strong, positive, and robust link between risk and application, suggesting that most non-applicants are non-discouraged, lower-risk firms.[40]

However, conditional on applying to the program, riskier firms are less likely to obtain the loan, indicating that banks screen loans and provide less credit to firms more likely to default (Column 3).[41] Although both demand and supply factors appear relevant, demand forces are stronger than supply forces in the credit allocation. Thus, the equilibrium behavior in Column 1 shows that riskier firms use the credit program more.

Table 2, Column 3 also shows that banks are less likely to approve a credit loan application if the firm participates in the employment protection program. The employment program entails an important opportunity cost. A firm under this program has to scale down its activity, so using it might signal banks that the firm might have difficulties in paying back

[38] By the end of 2020, banks have lent 97% of the original program allocation, meaning that 3% of loans are being processed and have not been disbursed yet. Once the loans are processed, banks reach 100% of the program allocation.

[39] Among eligible firms, 36% apply for a credit guaranteed loans. Of all the loan applications, banks approve loans for 60,770 firms, indicating a high approval rate of 77% (=60,770/79,319). Not all firms that receive approval end up using the program. We distinguish between approvals and usage in the estimations to construct separate dummy indicators.

[40] Appendix Table 10 shows application regressions by period: April, May, and June–December 2020. In April—the program's launch and peak application month—the riskiest firms were most likely to apply. Sensitivity to risk declines slightly in later months but remains economically and statistically significant, reinforcing our view that non-applicants are primarily low-risk, non-discouraged firms.

[41] Table 2, Column 1 contains all the firms in our matched sample with credit and real information. Among all of those firms, some apply for the credit program (and get either approved or rejected), while others do not apply for the program. In Column 3, on the other hand, we only include the subset of firms that apply for the program.

a loan because of the lower profits of the smaller operation. Banks take this information into account when reviewing credit applications, in addition to firms' *ex ante* credit risk and the evolution of sales during the pandemic. Because banks more often reject credit applications from firms that use the employment program (and are therefore more likely to be in distress) and because the employment program provides alternative aid to firms, the availability of this program could increase the quality of the credit program's borrowers in equilibrium.

Note that Column 3 does not correct for the fact that firms self-select into applying for the program. To address this, we implement a two-step Heckman selection correction in the approval regression. In the first stage, we use the Probit model already estimated in Table 2, Column 2, which includes both applicants and non-applicants and models the likelihood of applying as a function of firm-level characteristics. We then compute the inverse Mills ratio from this model and include it in the second-stage regression, which estimates the probability of approval using only applicants (Table 2, Column 3). This procedure corrects for the endogenous selection into the applicant pool.[42] Results are shown in Appendix Table 11. The estimated risk coefficient decreases in absolute value (26.4% vs. 12.3%). This reflects the fact that the uncorrected estimate, based only on applicants, overstates banks' responsiveness to risk because it is estimated on a sample that is disproportionately riskier. The Heckman correction delivers a more consistent estimate of banks' true sensitivity to risk.

As a robustness check, we re-estimate the approval regression accounting for COVID-related risk using *ex ante* information on firms' industry and geographic exposure. Industries are classified as high- or low-contact based on Cerda et al. (2023), who consider required social interaction and the share of in-person versus digital sales.[43] Municipalities are similarly classified using Pavani et al. (2022), based on predicted lockdown durations estimated from pre-pandemic characteristics like population density, overcrowding, and development. Firms are deemed high-risk if they operate in high-contact industries and high-exposure municipalities (above the median). We construct an interaction term to capture this composite COVID risk. Appendix Table 12 shows that riskier firms are less likely to receive credit, with this effect notably stronger among those with higher *ex ante* exposure to pandemic-related shocks.

Additional regressions show that banks discriminate by firm size in their approvals

[42]Identification in the Heckman model relies on the non-linearity introduced by the inverse Mills ratio, given the first-stage Probit model. Since the same covariates are used in both stages, we do not rely on excluded variables for identification. Instead, identification comes from the functional form of the Probit model and the assumption of joint normality of the error terms. The empirical setting provides sufficient variation in the selection index to support credible identification.

[43]High-contact sectors include restaurants, education, and passenger transport, while low-contact sectors include agriculture, mining, construction, and manufacturing.

(Table 3). The absolute value of the coefficient on risk in the regressions of bank approvals triples when moving from small firms to large ones. That is, banks are more sensitive to risk in their responses to loan applications from large firms than to those from small firms. This is consistent with banks containing the higher credit risk from the credit program by more strongly rejecting applications from larger, riskier firms. Because of their size and lower effective guarantee, loans to large firms would be more costly for banks to absorb in case of default. This underscores the relevance of the design of the credit program.[44]

Our findings in Table 2 indicate that firms experiencing both positive and negative sales growth during the first months of the pandemic are significantly more likely to obtain a guaranteed loan relative to firms with no sales growth (Column 1). Firms with either positive or negative sales growth are 19% more likely to use the credit program. Namely, guaranteed credit flows equally to firms that are differently hit (within an industry and municipality) by the pandemic.

We contrast the results of the credit program with the employment program (Table 2, Column 4) using an analogous Probit model. Firms that suffer negative sales growth are much more likely to use the employment program (11.2%) compared to firms with positive sales growth (5.3%). Because firms negatively affected by the pandemic lose less by shutting down, the opportunity cost of participating in the employment program is lower, so they have more incentives to use it. In addition, we find that firms that use the employment program are 9.5% more likely to obtain a guaranteed loan (Column 1), while firms that participate in the credit program are 5.6% more likely to participate in the employment program (Column 4). That is, the probability of participating in either program increases if the firm participates in the other program.

Unlike the credit program, firms with different risks are equally likely to use the employment program. This indicates that risk is a more important predictor for credit program use than for the employment program. This result is consistent with the fact that program credit is cheap.[45] Instead, the employment program is more expensive as firms must

[44]We also implement a two-step Heckman selection correction for the approval regressions by firm size. As shown in Appendix Table 13, the estimated coefficients decline, indicating that the raw regressions overstated risk sensitivity due to selection into applying. The corrected estimates confirm that banks' responsiveness to risk is lower once this bias is addressed.

[45]Although riskier firms are more likely to obtain a public credit guaranteed loan among eligible firms, by design, the program excludes the riskiest firms from the economy. If we add those ineligible firms to the estimation, we still find that riskier firms are more likely to obtain the guaranteed loan. On the other hand, when we add ineligible mega firms to the estimation, the size of the effect of risk increases, which is consistent with the fact that these mega firms entail low risk. When we compare the firms that obtain the credit program to all firms in the economy, including those that are ineligible due to risk and size, the effect of risk remains significant. See Appendix Table 14 for details and further results.

shut down (or at least forego the output from the workers with frozen labor contracts) and stop receiving or reducing their income from operations.

Table 2, Column 5 uses as the dependent variable a dummy equal to one if the firm used both programs and equal to zero otherwise. The results show that riskier firms are 4.7% more likely to use both programs. Firms facing both positive and negative sales growth are more likely to use both programs, although the effect is stronger for firms that face a negative sales growth shock. This is driven by the incentives associated with the employment program. Furthermore, we re-estimate Equation (2) using the *ex ante* interest rate spread as an alternative measure of risk instead of using the predicted default probability from our estimated default probability model. The *ex ante* spread has the advantage of being a simple, forward-looking, and market-based measure of risk.[46] Panel B reports the results. The main results remain unchanged.[47]

4.3 Selection Based on Firm Growth: Evidence from Dynamic Lockdowns

As an alternative way to exploit the exogenous variation in firm-level sales growth, we use the staggered implementation of mandatory lockdowns across Chilean municipalities (counties) aimed at controlling for the expansion of the pandemic.[48] We define the treatment event as the week in which a municipality enters a mandatory lockdown. Treated firms are those in municipalities that enter into a lockdown at any point during May to July of 2020. Control firms are those located in adjacent municipalities that are never closed during the same period of time. Figure 4 presents the map of municipalities according to their overall lockdown status and illustrates substantial geographical variation.[49] To estimate selection into public policies, we run the following difference-in-differences regression:

$$ProgramUse_{it} = \alpha_i + \alpha_t + \beta Lockdown_i + \gamma Post_t + \delta Lockdown_i \times Post_t + u_{it}, \quad (3)$$

where $ProgramUse_{it}$ is equal to one if firm i participates in a public program in month t,

[46] Spreads for the full distribution of firms are observed only sporadically when new loans are granted. Right at the onset of the pandemic and before the credit program is established, credit to firms (beyond the mega firms) is frozen. Thus, spreads at that time are not available; thus, we need to rely on past spreads. For this reason, the spread risk measure can be subject to similar limitations as the probit risk measure.

[47] In Appendix Table 15, we re-estimate the specifications in Table 2 by adding the different specific attributes we use to estimate the default probability model in Table 1. These estimations show the direct effect of these attributes on the probability of using the credit and employment programs. Furthermore, in Appendix Table 6, we divide the risk variable into three terciles, showing that our conclusions about the relation between the use of the credit program and risk hold for different risk levels.

[48] Chile is divided into 16 regions and 345 municipalities. Each region is divided into municipalities, which constitute the country's smallest administrative division.

[49] Appendix Figure 4 presents the weekly evolution of the cumulative number of municipalities under lockdown. The blue line represents all Chilean municipalities, whereas the red line represents the municipalities we use for our study, given the inclusion requirements discussed above. The number of lockdowns starts growing during the first week of June. Those municipalities are exposed to the credit guarantee program for at least three weeks before going under lockdown. By the end of July, there are 66 municipalities under lockdown, of which 24 are used in our analysis.

$Lockdown_i$ is a dummy equal to one if firm i is located in a municipality subject to a lockdown, and $Post_t$ is a dummy equal to one after the firm's municipality enters into lockdown.[50]

A credible causal interpretation requires that firms in treated and control areas are similar in relevant pre-treatment characteristics. To evaluate this assumption, we compare firms in both groups along observable variables that predict program participation, such as size, age, and outstanding debt. The comparisons reveal no meaningful differences, lending support to the validity of our identification strategy (Appendix Table 16).

Table 4, Panel A reports the results for program use. For the credit program, the interaction term is not statistically significant, indicating that firms entering a lockdown are not more likely to use that program (column 1). This evidence is consistent with the selection results shown in Table 2, where firms with negative and positive sales growth are equally likely to take a guaranteed loan. Instead, for the employment program, we observe a positive and significant interaction term (column 2). This finding is also consistent with the selection results, in which firms with negative sales growth are substantially more likely to use employment protection than firms with positive sales growth. This result is consistent with the fact that the opportunity cost of using the employment program is lower if the firm resides in a municipality under a lockdown because a lockdown reduces economic activity and, therefore, the potential profits of firms.

To provide a sharper and more exogenous analysis, we restrict the comparison to firms within a short geographical distance. Similar firms tend to co-locate in space, indicating that nearby firms are similar in many economic characteristics. Importantly, because the virus is spread within short distances, nearby firms have similar exposure to the virus. However, around the border of a lockdown, social distancing measures are different: one firm is in lockdown while the other is not. To perform the analysis, we re-estimate Equation (3) by restricting the sample to firms that run along the municipality border. The main results remain unchanged (Table 4, Panel B).

Lastly, we augment Equation (3) by interacting firm risk with the other variables in the equation to study whether risk is correlated with the adoption of the programs within firms under a lockdown.[51] Since the lockdown regressions use CMF financial data rather than the

[50]We estimate this regression using financial data from the Financial Market Commission, rather than the merged dataset with real-sector variables. This is because only the Commission's records include firm-level geolocation, which is crucial for identifying firms along municipal borders, a key component of our identification strategy, as detailed below.

[51]Specifically, we estimate:

$$\begin{aligned} ProgramUse_{it} = & \alpha_i + \alpha_t + \beta Lockdown_i + \gamma Post_t + \eta Risk_i + \delta Lockdown_i \times Post_t \\ & + \zeta Lockdown_i \times Risk_i + \lambda Risk_i \times Post_t + \theta Lockdown_i \times Post_t \times Risk_i + u_{it}. \end{aligned}$$

merged dataset with real sector variables available only at the Central Bank of Chile, we cannot use our baseline predicted default risk measure, which relies on sales, employment, and value added. Instead, we proxy firm risk using the loan spread, a forward-looking financial variable observed in the CMF data, though only for a subset of firms with prior borrowing.

While the triple interaction term is not statistically significant for either program, the previous results hold (Appendix Table 17, Panel A). That is, within firms under a lockdown, risk does not appear to influence participation in the credit or employment programs. Credit program take-up is broad and largely unaffected by lockdowns. In contrast, employment program take-up responds to real-side shocks (e.g., sales declines or closures), not credit risk, suggesting that lockdowns affect participation mainly through real disruptions. The triple interaction remains insignificant for firms near municipal borders (Appendix Table 17, Panel B). However, the lockdown effect for the employment program, though similar in size, loses statistical significance due to the limited number of firms with spread data.

5 Effects on Firm Indebtedness

We next study the effects of using the credit program on debt at the firm level. To do so, we estimate the following cross-sectional regression:

$$\frac{\Delta Debt_i}{Sales_i} = \beta Program\, Use_i + \gamma Risk_i + \delta Sales\, Growth_i + \alpha_s + \alpha_m + u_i. \tag{4}$$

$\Delta Debt_i$ is the growth in (net) outstanding bank debt during the entire 2020, normalized by sales in 2019. This ratio focuses on the change in indebtedness, holding constant sales and thus abstracting from the sales decline in 2020.[52] The dummy $Program\, Use_i$ is defined as previously reported. $Risk_i$ corresponds to the fitted default probability value derived from the firm-level default regression estimates and used in Section 4.

Table 5 presents the results.[53] Firms with and without previous loans that use the credit program increase their indebtedness by 14.5 and 13.0 percentage points, respectively,

[52]We normalize the debt change by sales instead of assets because sales are more accurately measured and audited by the tax authority than assets. However, our results are robust to normalizing the change by 2020 sales (Appendix Table 18 and Appendix Table 19). Appendix Table 20 shows a version of this regression where the outcome is in levels, controlling for lagged firm indebtedness on the right-hand side. The main conclusions hold.

[53]Credit program eligibility requires firms to have prior loans (because only firms that are up to date with their debt payments at the moment of applying are eligible). As a result, the 51,729 firms without prior loans—used in the indebtedness regressions (Table 5, Columns 2, 4, and 6)—are excluded from the program participation regressions (Table 2). The sample of firms with prior loans is nearly identical across both sets of regressions: 62,894 firms in the program participation regressions and 62,950 in the indebtedness regressions.

relative to non-participating firms (Columns 1 and 2).[54] These are sizable effects when compared to the initial leverage ratio of 29% for firms with previous loans and 0% for firms without previous loans (which, by definition, have no previous bank debt). Given the interest in credit allocation and firm risk, we run another specification that includes an interaction term between the use of the credit program and our predicted risk measure. The results show that *ex ante* riskier firms increase their indebtedness more strongly after participating in the program (Appendix Table 21).

Returning to Table 5, firms with both positive and negative sales growth increase their leverage during 2020 by similar magnitudes. The relationship between indebtedness and the employment program is much weaker than with the credit guarantee program. The effect is significant, but an order of magnitude smaller than the effect for the credit program (Columns 1 and 2). In addition, firms that participate in both programs accumulate less debt. That is, using both programs mitigates firm indebtedness as firms have less need for credit when they receive employment benefits.

Next, we decompose the change in indebtedness into the change in public guaranteed and non-guaranteed debt. By construction, public guaranteed debt needs to increase for firms participating in the program. We find that indebtedness from publicly guaranteed debt increases by 13.9 and 11.8 percentage points for firms with and without previous loans, respectively (Columns 3 and 4). On the other hand, participating in the credit program could lead to higher or lower non-guaranteed debt. We find that indebtedness from non-guaranteed debt also increases, although the magnitude of the effect is significantly smaller (Columns 5 and 6).

The increase in non-guaranteed debt can result from incremental borrowing (as the capped guaranteed debt might not be sufficient) and/or a slowdown in repayment due to the six-month grace period established by the credit program. These results suggest that, for the firms that use the credit program, the guaranteed and non-guaranteed debt act as complements rather than substitutes. This micro result of complementarity for firms that use the credit program contrasts with the results that include mega firms (Figure 2, Panel A), which suggests that, overall, program credit compensates for the decline in non-program credit (indicating a substitutability between the two types of credit at the aggregate level).

[54] Both groups of firms with and without credit history use the credit program: 61% of the firms within the active firms sample that receive a guaranteed loan have a previous loan, while 39% do not. This indicates that the program provides bank credit to a significant number of firms with no previous bank debt. Firms with and without a previous loan receive 92.6% and 7.4% of the total value of guaranteed credit, respectively. In comparison, 56% of firms that use the employment program have a previous loan.

As an alternative test, we regress firm indebtedness on a dummy equal to one if the firm applies for a guaranteed loan and its application is approved. The dummy equals zero if the firm applies for the loan but gets rejected. The regression controls for the amount of credit solicited, capturing credit demand. We observe a significant increase in borrowing for firms that apply for the credit program and get approved, relative to the ones that apply but are rejected (Appendix Table 22).[55]

We also conduct a regression discontinuity analysis exploiting the size eligibility threshold of the credit program to establish a causal relationship between the credit program and the increase in firm indebtedness. Appendix B provides a detailed description of the discontinuity methodology used. The results can be found in Appendix Figure 5. We find that gaining eligibility to the program increases the likelihood of take-up by 14%, as shown in Panel (A), and increases firm indebtedness by 4%, as shown in Panel (B). These results are consistent with the finding that participating in the program increases firm indebtedness.[56]

Having shown that the increase in debt occurs mainly through participating in the credit program, we next study how risk is related to the accumulation of this type of debt. Table 6 shows the results of estimating Equation (4) for the sample of firms that use the credit program. We find that, within credit program users, riskier firms end up with more publicly guaranteed debt than safer firms, and this holds for firms with and without previous loans (Table 6, Columns 1 and 2). The selection results from the previous section show that riskier firms are more likely to participate in the credit program, an expansion of the extensive margin. The results in this section show that conditional on participating in the credit program, riskier firms end up with more guaranteed debt, reflecting an expansion of the intensive margin.[57]

In contrast, the relation between risk and non-guaranteed debt is negative and significant for firms with previous loans, and not significant for firms without previous loans (Columns 3 and 4). That is, in the intensive margin, more risky firms tend to substitute regular credit with program credit. The substitution is not complete in the sense that riskier firms still increase their overall leverage. This shows that regular and program credit allocations are

[55] Appendix Table 23 shows that results are robust to a specification where firm indebtedness is the outcome in levels, rather than in changes, controlling for lagged indebtedness on the right-hand side.

[56] Although these regression discontinuity results are informative to attribute causality, we do not employ them systematically in our paper because they cannot be used to capture the full distribution of firms and, thus, the aggregate effects. They estimate the effects around the discontinuity just for the largest firms. Additional regression discontinuity exercises on other variables (such as real outcomes) do not show an effect of the program, as we do observe for leverage. This type of analysis could be useful for further work that does not focus on the aggregate effects but is interested in variations around different discontinuities.

[57] Appendix Table 24 also shows that results are robust to a specification where firm indebtedness is the outcome in levels, controlling for lagged indebtedness on the right-hand side.

complementary in the extensive margin but substitutable in the intensive margin. The results suggest that the existence of a public credit guarantee program changes the way banks allocate credit across the risk distribution of firms.[58]

Table 6 further illustrates the impact of the interaction between credit and employment programs on firm indebtedness growth. The results reveal that firms utilizing both programs experience a smaller increase in indebtedness compared to those relying solely on the credit program. This finding aligns with the notion that firms require less credit when they benefit from employment support, corroborating the results from Table 5. Additionally, Table 6 indicates that this interaction is particularly pronounced for firms without prior loans. This confirms that banks may interpret the use of the employment program as an indicator of a firm's financial distress and associated risk, a concern that seems to be heightened when the firm lacks a credit history.[59]

6 Aggregate Implications

6.1 Aggregate Allocation, Expected Loss, and *Ex Post* Loss

We next study how the credit program is allocated to different types of firms according to risk and how this distribution determines aggregate expected and *ex post* loss. As mentioned earlier, the employment program affects both borrower quality and credit quantity allocated, thereby influencing aggregate expected loss. Thus, both the credit and employment programs impact these estimates of aggregate losses. Microdata indicate that by the end of 2020, banks provide guaranteed loans worth US$11,504 million, or 4.6% of 2019 GDP, including loans to firms and natural persons who borrow as firms under the program (Table 7, Panels A and B, Columns 1 and 2). These loans are distributed across firms of different risk categories.[60] Among formal firms, high-risk firms receive 7% of the guaranteed loans, whereas low- and medium-low-risk firms receive 65% of the loans (Column 3).

The main risk that pervades the program's loan allocation is the loss from the default of different tranches of the loan portfolio. This risk could be significant because the program targets firms smaller than the typically safe mega firms, even after the program excludes the riskiest firms in the country by design. To gauge the magnitude of this default risk and how

[58] In Appendix Table 25, we re-estimate Table 6 including the different specific attributes that we use to estimate the default probability model. The regressors have a similar effect on firms with and without previous loans. As above, the results are robust to the alternative specification of having firm indebtedness in levels as an outcome, controlling for lagged indebtedness on the right-hand side (Appendix Table 26).

[59] The overall conclusions on indebtedness hold when partitioning risk into terciles (Appendix Table 27).

[60] We partition firms into four groups according to their predicted default risk, from high risk to low risk. We can only perform this partition for formal firms, not for natural persons.

it is distributed, we first estimate the probability of default in 2020 for firms in each risk bin. To do so, we use the coefficients of the default risk model in Section 4 and plug in the 2019 information for the different regressors. This yields predicted *ex ante* default values for 2020 for firms that use the credit program across different risk groups. As expected, the predicted default probability declines monotonically with risk, going from 18.2% for high-risk firms to 2.1% for low-risk firms (Table 7, Panel A, Column 4).[61]

We then calculate a measure of total credit risk (i.e., expected loss) for each risk group by multiplying the dollar value of program loans (Column 2) by the default probability (Column 4).[62] As a proportion of GDP, the total credit risk related to formal firms corresponds to an expected credit loss of 0.27% (Panel A, Column 5). When using all guaranteed loans to formal firms and natural persons, the expected credit loss is 0.45% of GDP (Panel B, Column 5), which corresponds to a 9.8% default probability of the guaranteed credit (=0.45%/4.6%).

These estimates of the expected loss do not capture the increased uncertainty at the onset of the pandemic. They are calculated using the 2019 data available to banks and the government at the start of the pandemic. Thus, the estimated expected loss can represent a lower bound of the actual expected loss. To provide an alternative measure of the program's cost and with the benefit of hindsight, we use data on the actual *ex post* default rates after 2020 (Column 6).

The *ex post* default rate is monotonically increasing in the risk categories, just like the *ex ante* default rate, as shown in Section 4.1. Moreover, the *ex post* default rates of firms with low and medium-low risks are significantly higher than the ones obtained from the *ex ante* default rates. Still, even after accounting for those firms' larger *ex post* default rates and their significant share of the credit program, aggregate loss using *ex post* defaults does not increase significantly. It remains at 0.38% of GDP for formal firms (Panel A, Column 7) and 0.55% of GDP for formal firms plus natural persons (Panel B, Column 7).

Two points are worth noting about the credit allocation and aggregate loss. First, the expected default rate of 9.8% is higher than the maximum interest rate of 3.5%. Assuming a zero recovery rate and ignoring the opportunity cost of these funds, the 6.3% differential provides a benchmark of the expected loss from the credit program. To compensate, the total economic benefits of the program in terms of firms saved and impact on economic activity

[61] A small fraction of firms do not have data to be classified in a risk category. To be conservative, we assume that those firms (and natural persons) have the default probability of the riskiest group of firms.

[62] Expected loss technically ought to be equal to the probability of default times the loss given default. Given data restrictions on loan recovery rates, in the analysis, we assume that the loss given default equals one, i.e., we assume that banks do not recover anything after a borrower defaults (no partial recovery). The expected loss would be lower if we considered a recovery value.

should exceed 6% of the program's funds.

Second, the aggregate expected loss and *ex post* default are significantly determined by how much credit different firms receive. Although riskier firms are much more likely to default, the contribution to expected loss is similar across risk categories. The larger amounts granted to low-risk firms and their smaller default probability compensate across risk groups (Panel A, Column 5). In effect, there is a clear negative correlation between the default probability of each group of formal firms and the share of credit program received (Columns 3 and 4). A similar pattern arises for the measures of *ex post* default. Thus, aggregate expected and *ex post* losses are relatively contained because most loans go to safer firms.

The actual distribution of credit under the program roughly matches the weights that firms have in the economy according to sales (Figure 5). These weights show how indebtedness for different types of firms contributes to the rise in aggregate corporate debt during 2020. The weights reflect that credit allocations are proportional to firms' sales, which is consistent with the fact that the program allows firms to borrow up to three months of sales.

Extending the analysis to both guaranteed and non-guaranteed debt, Appendix C calculates how micro-level allocations across risk groups are reflected in the overall economy. This total debt allocation across risk groups is basically the same as the share that each risk group receives of the guaranteed credit. Larger, safer firms are the ones that receive the bulk of the credit, with their larger allocation given by their *ex ante* weight in the economy according to sales.

6.2 Risk Sharing between Banks and the Government

We next analyze how the aggregate risk is distributed between banks and the government. The fraction of credit risk effectively guaranteed by the government in case of default depends on the guarantees, which vary by firm size, after the corresponding deductible is applied. Table 8 reports the nominal guarantee, deductible, and effective guarantee by risk (Columns 2, 4, 5).[63] For ease of exposition, we reproduce Column 5 of Table 7 (expected loss) as the first column of Table 8.

Based on the sample of formal firms, the total credit risk estimated to be borne by the government is 0.11% of GDP (Panel A, Column 6), while that borne by banks is 0.16% of GDP (Panel A, Column 7). Thus, 59% of the total credit risk derived from the expected loss

[63] To calculate the effective guarantee, we consider the deductible and the guaranteed amount after applying the deductible, both of which depend on firm size. The effective guarantee is calculated as follows:

$$Effective\ Guarantee = ((Default\ Probability - Deductible) \times Nominal\ Guarantee) / Default\ Probability.$$

The deductible is reduced to zero for SMEs starting in July. Given that most of the guaranteed credit is granted in the program's first months (May and June), we use the values of the deductible established at the beginning of the program.

from default is absorbed by banks (=0.16%/0.27%) and 41% by the government. For formal firms and natural persons, banks and the government absorb the expected loss evenly.

The substantial share of the risk banks bear, despite the high guarantee levels, is due to the policy deductible. To illustrate this point, we recalculate Table 8 under the counterfactual scenario of a zero deductible, in which the effective guarantee converges to the nominal guarantee. In this case, the government's credit risk increases from 0.11% to 0.21% of GDP (Panel A, Column 6 of Appendix Table 28), while the banks' share falls from 0.16% to 0.06% of GDP. Namely, a zero deductible implies that the government absorbs 77% of the risk (0.21%/(0.21%+0.06%)), with banks covering the remaining 23%.

This mechanism shields the government from moderate credit default events. As default rates rise, the deductible's impact lessens, closing the gap between the effective and nominal guarantees and increasing the government's share of the risk. Essentially, the deductible structure ensures that the government primarily absorbs tail risk. For moderate defaults, the deductible substantially reduces the effective guarantee, shifting more risk to banks, as observed in 2020.

Lastly, besides calculating the expected loss relative to GDP, we calculate the expected loss relative to the equity of the banking sector. The expected loss for formal firms represents 1.07% of the banks' equity capital, while for formal firms and natural persons, it represents 1.54% of the banks' equity capital (Panel B, Column 8). These estimates indicate that the program does not pose a concern for the banking system's solvency.

6.3 How the Policy Ingredients Affect Aggregate Risk

To understand how the policy ingredients influence aggregate risk, we perform a series of sensitivity analyses yielding five empirical counterfactual scenarios. In each, we modify different dimensions of the policies and recalculate the expected loss derived from credit to eligible and formal firms.[64] The scenarios are computed sequentially. These calculations illustrate how the various mitigating factors might affect aggregate risk. They take the demand for credit and default rates as exogenous and, thus, should be considered as partial equilibrium exercises with limited firm responsiveness. The general equilibrium responses are addressed through the lens of the model in Section 7.

The expected credit loss can change because of a change in credit (given the average default probability) or the average default probability (given the credit granted). Hence, we disentangle the forces driving the expected loss. Specifically, we decompose the expected credit loss as the product of credit growth (relative to credit in 2019) and the average default

[64] We conduct these exercises only for formal firms because sales information is unavailable for natural persons.

probability. The latter is computed by weighting the default probability of each group of firms (by risk) with their corresponding credit amount in each scenario.

Table 9 presents the results. Rows a and b report the program credit relative to 2019 GDP and baseline credit (credit to program-eligible formal firms in 2019), respectively. The results in Row b are comparable to those in the quantitative model in the next section. Rows c and d report the credit-weighted interest rate and default probability, respectively. Rows e and f report the expected loss relative to baseline credit (the product of Rows b and d) and the government expected loss relative to baseline credit (the product of the guarantee and Row e), respectively.

Each column in Table 9 presents a different counterfactual scenario. Column 1 shows results for the combined policy: the use of the credit and employment programs. As a reference, Column 1, Rows a and d show that program credit is 3.6% relative to GDP and the default probability is 7.4%, as already reported in Table 7. Column 1 also shows that the program credit is 50.5%, while the expected credit loss is 3.7% and the government expected loss is 3.0%, all relative to baseline credit.[65]

We explore the role of four policy ingredients in the subsequent Table 9 columns. In Column 2, we reduce the loan cap of the credit program from three months to one month of sales. Given that most firms are at the three-month cap, we assume that credit declines to the counterfactual one-month cap for all firms. According to Column 2, if the program grants loans equivalent to one instead of three months of sales, total credit falls to 2.5% of GDP (35.3% of baseline credit), and the average default probability declines to 7%.[66] As a result, the expected credit loss decreases to 2.5% of credit.

In Column 3, we implement a scenario in which there is no eligibility constraint for firms with high default, i.e., firms with past due payments exceeding 30 days become eligible. We approximate their potential demand as three months of sales, assuming these firms demand the maximum possible credit they can obtain because they need funding. If the program lifts the default-based eligibility constraint, credit expands to 7.6% of GDP (107% of baseline credit), and the average default rate rises to 9.2%. As a result, the aggregate expected loss nearly triples, from 3.7% to 9.9% of baseline credit.

In Column 4, we explore the case of eliminating the credit guarantee. Without this

[65]In the empirical and theoretical counterfactuals, we apply a conservative estimate of the government burden using an 80% guarantee, which is the upper bound of guarantees applied in the policy. If we were to apply the effective guarantee, the government burden would be lower, while banks would bear the rest of the cost.

[66]The average default probability is weighted by credit. Thus, credit changes imply changes to those weights. In Column 2, this implies a reduction in the default probability because the firms that reduce their weight more are riskier.

protection, banks have stronger incentives to reject more loan applications. As banks assume a greater screening role, we assume that all granted loans correspond to firms with an effective zero guarantee, namely, the low-risk firms in Table 8, Column 5. In that case, total credit falls to 1.6% of GDP (22% of baseline credit), the average default probability declines to 2.1%, and expected credit losses drop sharply to 0.5% of baseline credit.

In Column 5, we evaluate the aggregate expected loss in the absence of the employment program. Without the latter, firms become riskier and more dependent on credit. Firms using only the credit program default 1.1 percentage points more than those using both programs. As borrower quality deteriorates, the average default rate rises from 7.4% to 8.5%. At the same time, financing needs increase as firms relying solely on the credit program demand about 17% more credit. Consequently, total credit rises from 3.6% to 4.2% of GDP (59% of baseline credit), pushing aggregate expected losses upward to 5.0% of baseline credit.

The interpretation of the empirical counterfactuals needs to consider that these exercises do not account for the endogenous responses of firms and banks, particularly in scenarios that alter the programs. To address this limitation, the next section develops a quantitative model to capture the joint responses of firms and banks.

7 Quantitative Model

To consider how firms and banks respond to the credit and employment programs and affect aggregate risk, we develop a quantitative model that endogenizes credit demand, lending terms, and default behavior. The model features heterogeneous firms that make credit and default decisions, calibrated to data on credit program-eligible firms. The model shows how the credit program expands and reallocates lending, and quantifies its impact on aggregate credit and default under alternative policies and shocks. This framework relaxes an important assumption in our empirical analysis that default rates follow historical values. It also enables a more accurate assessment of the government's fiscal exposure, recognizing that defaults typically involve partial recovery (whereas our empirical analysis assumes no recovery). Below, we provide an overview of the model and results; Appendix D shows the technical details.

The model builds on Covas and Den Haan (2012), a general equilibrium model of firm debt and endogenous default. We consider a full information model of the static decisions of firms that share a common aggregate productivity, A, but vary *ex ante* in their equity,

e, existing labor, l, and expected productivity, z.[67] The firm makes two contemporaneous decisions. It decides how much to adjust labor Δl given the wage, w, and an adjustment cost, $\eta < w$, that must be paid for any operating firm not in default. It also decides how much to borrow, b, given an interest rate schedule for borrowing, $r_b(b; e, z, l)$.[68]

Operating profits, π (revenues minus expenses before debt servicing), are determined by a Leontieff-type production technology that depends on aggregate productivity, firm-specific expected productivity z, capital $e + b$, labor $l + \Delta l$, and a stochastic revenue shock ε, which can lead to default when sufficiently low. That is,

$$\pi = \varepsilon z A \left(\min\{e + b, \frac{1}{\psi}(l + \Delta l)\} \right)^{\alpha} - w(l + \Delta l) - \eta |\Delta l|, \tag{5}$$

where ε is a random variable with $E(\varepsilon) = 1$, continuously distributed over $\mathbb{R}^+$ according to a cumulative distribution function, $\Phi(\cdot)$. The parameter ψ captures the fixed ratio of workers to capital.[69] Capital depreciates at rate δ. In this static model, the labor adjustment cost η is symmetric and designed to capture both the current cost of hiring additional labor and the future costs associated with (potentially temporary) reductions in labor. This cost makes the employment protection program valuable to firms.

Competitive banks price loans at zero profit based on a common cost of capital, an intermediation cost also capturing banks' willingness to lend, and default risk. Default is costly, which entails the loss of a fraction μ of firm revenues. Thus, priced competitively, interest rates reflect *ex ante* default risks and therefore vary by debt, equity, expected revenue productivity, the labor endowment of the firm, and revenue productivity risk, σ^2_{ε}. In the baseline, banks are constrained by a regulatory cap of 20% on annual interest rates.[70]

To quantify aggregate outcomes, we assume that firm-level heterogeneity in equity, labor, productivity, and risk follows a joint log normal distribution, with productivity, equity, and labor positively correlated. We calibrate the key technological and distributional parameters to match pre-COVID data, disciplining 11 parameters to fit 12 empirical moments related to interest rates, default rates, the distributions of sales, debt over sales, debt over equity, labor growth, and labor over equity, as detailed in Appendix D.

[67] We abstract from informational frictions and follow the literature on firm finance with endogenous default (e.g., Cooley and Quadrini, 2001; Covas and Den Haan, 2012; Gilchrist et al., 2014). Given the suddenness of the pandemic, we model a static problem with firm equity and labor stocks taken as given.

[68] We use the term equity in the model to be consistent with the existing literature of firm finance with endogenous default. But equity is equivalent to net worth (i.e., assets minus debt), as used in the empirical part of the paper.

[69] The fixed proportions assumption is natural given the short-term, unexpected nature of our analysis and is also simplifying, yielding $\Delta l = \psi(e + b) - l$. We abstract from explicit material costs, but if these are also a fixed proportion of capital, they can be incorporated into the ψw term, as in our calibration below.

[70] Law 18,010 sets a maximum conventional interest rate by loan size, currency, and term. For commercial loans in local currency over 90 days, the cap during our sample period is 20% per year.

We then introduce the crisis and the two programs into the analysis. The crisis is modeled through two scenarios for default risk. In the first, the economy reflects the observed default probability before the pandemic in 2019, which we refer to as a "typical year." In the second, the economy is hit with a lower mean and greater dispersion of productivity and profitability, comparable to the Great Recession of 2008–2009, leading to higher default risk. We refer to this second scenario as a "systemic shock."

With respect to the credit program, we model three key elements. First, the statutory interest rate cap is lowered from 20% to 3.5%, reflecting the program's interest rate ceiling. Second, the partial guarantee covers 80% of bank losses from default.[71] Third, to match the increase in lending observed in the data, we exogenously raise the supply of credit by lowering the intermediation cost, equivalent to a 2.4 percentage point reduction in the cost of capital. With respect to the employment program, we model it as covering the wages of employed workers, conditional on a production shutdown. Firms in this scheme earn zero revenues but retain workers, thereby avoiding the adjustment costs of reducing labor.

Table 10 reports different counterfactuals for credit, interest rates, default losses, potential government losses, and the actual government burden under alternative policy scenarios, similarly to the empirical counterfactuals in Table 9. In the model, all credit is subject to the credit program rules. However, in the data, only 43.6% of credit to eligible firms in 2019 is channeled through the program. Hence, we use this ratio to subtract out non-program credit in the combined policy, which is held constant over the counterfactuals.

Column 1 shows the combined policy counterfactual, where both policies are at play, relative to the baseline scenario with no policy, described in the table note. By construction, program credit increases by 50.3% of baseline credit. In the model, the combined policy lowers interest rates from 4.6% to 1.4%, substantially below the observed 3.5%, the maximum that could be charged under the program. The difference reflects the net effect of the increased willingness to lend (an effective drop of 2.4%) and a somewhat higher anticipated default probability.

In a typical year, the increased lending leads to a small increase in the default probability (5.9% vs. 5.6% in the baseline scenario, where the latter is calibrated to be comparable to the 5.5% observed in 2019) and an expected credit loss of 3.0% of baseline credit. The

[71] We abstract from two features of the guarantee: (i) the dependence of the guarantee level χ on firm size and (ii) the deductible. To keep the framework standard, we also abstract from other features highlighted in the empirics, such as eligibility requirements and the choice between program and non-program credit. Thus, our model applies to eligible firms and is well-suited for analyzing firm indebtedness, but less appropriate for studying extensive-margin decisions on participation.

government's share of expected credit loss is 2.4%, which is comparable to the 3.0% in Table 9, Row f.[72] Hence, the endogenous borrowing in response to the combined policy creates risks similar to those observed in the data. The actual burden, however, is considerably smaller, just 0.4% of credit, because the endogenous recovery rate conditional on default is substantial.[73] While credit at risk and government burden remain modest in a typical year, the model allows us to simulate a systemic-shock counterfactual (Rows g–j). In this scenario, the default probability under the policy increases to 14.8%, the government's expected loss increases proportionately to 6%, and the actual burden rises to 1.6%.

Columns 2–4 isolate the role of different credit policy elements by sequentially shutting off the increased willingness to lend, the interest rate cap, and the credit guarantee. All these components matter, but in terms of expected credit loss, willingness to lend is nearly twice as influential as the credit guarantee. The interest cap strongly restricts lending, especially to risky borrowers. Without it, program credit expands to 68% of baseline credit (Column 3), significantly more than its actual increase, while default rates rise to 12.7% even in a typical year. The interest rate cap makes it unprofitable for banks to lend to risky borrowers, so the credit expansion is lower in Column 1 with the interest rate cap. Furthermore, absent the credit guarantee, program credit is 35.9% of baseline credit (Column 4). This is higher than the 22.4% in the empirical counterfactual (Table 9), with banks' willingness to lend in the model explaining this difference.

The employment protection program, as modeled, ends up entirely independent of credit in the simulations (Column 5). This is because adjustment costs are calibrated to be small, so no firm opts for the employment program in the model.[74] In practice, however, a fraction of firms use the employment program regardless of whether they are in lockdown or not. For example, around 9% of firms not in lockdown use the employment program, suggesting that the adjustment costs may not be so small. If we increase the calibrated adjustment costs to match the employment take-up (at the cost of worsening the fit of other moments of the calibration), firms that use the employment protection reduce their borrowing. Moreover, with the recalibrated adjustment cost, firms borrow more when the employment program is

[72] Given that program credit equals about 3.6% of GDP and program credit is 50.5% of baseline credit, the expected credit loss of 3% of credit corresponds to a loss of 0.21% of GDP (=3*3.6/50.5), comparable to the results in Section 6.2.

[73] In the baseline scenario, the credit-weighted default rate (5.6%) exceeds the average interest rate (4.6%), implying a significant recovery rate under default for lenders to break even. If the recovery rate were zero, the expected payoff, i.e., the product of one minus the default rate and gross interest rate, would be $(1 - 0.056) \times 1.046 \approx 0.99 < 1$. Even ignoring capital and intermediation costs, lenders would be losing money. Hence, the recovery rate is substantial in both the model and the data.

[74] If we were to model explicitly the 76% of firms in lockdown, these firms would take up the employment program but no credit, as no borrowing would be needed.

not available, which is qualitatively consistent with the empirical drop in borrowing associated with the use of the employment program documented in the empirical section.

In sum, the model yields four lessons. First, the aggregate expected credit loss and the government's burden remain limited even after accounting for firms' and banks' endogenous credit response and the general equilibrium effects of the credit policy. We obtain comparable theoretical and empirical counterfactual results. Second, although the credit guarantee raises credit and default, the interest rate cap constrains them. Eliminating the cap increases credit and also expected credit losses. An analogous empirical counterfactual that allows firms in default to receive credit produces a similar outcome. An additional insight concerns endogenous recovery rates under default, which further reduce the government's burden. Given that default and interest rates are comparable in the data and banks face a funding cost, the model implies that recovery rates must be substantial for lenders to break even. Third, when the lower interest rate cap is absent, credit and credit loss are higher in the theoretical than in the empirical counterfactuals because banks are endogenously willing to lend more to high-risk firms by charging a higher interest rate. The resulting increase in default probability is similar to that observed under a systemic shock. Lastly, under a systemic shock such as the Global Financial Crisis, risks rise sharply, with expected credit losses more than doubling relative to normal times.

8 Conclusions

This paper uses a large-scale episode of a credit program and an employment program together with unique financial and real data for the universe of firms and banks in Chile to shed new light on how these policies influence the distribution of credit and the implied potential financial risks. The programs give different incentives to firms: firms internalize the cost of using the employment program, but do not do so for the credit program. As a result, higher-risk firms disproportionately borrow through the credit program, which leads to a rapid and substantial increase in indebtedness across a broad class of firms. Still, most credit volume is granted to large firms, which have a significant weight on the macroeconomic allocation.

Whereas our findings are based on the COVID-19 pandemic, we can draw more general lessons about circumstances and policy actions that can limit risk while broadly expanding credit. Although loose credit conditions inevitably generate incentives for risky firms to obtain credit at low cost, selection can also be mitigated by design or in practice. Firms with the highest risk can be effectively excluded through simple eligibility rules. When credit is allocated according to firm size (as is mostly the case in easy lending policies), the typically safer large firms tend to contain the increase in aggregate risk even when riskier firms lever up the most. Government guarantees of tail credit risk can motivate banks to quickly dispense credit and engage with risky clients. Yet, when such guarantees are partial and interest rates have low caps, banks still have incentives to provide effective screening. The existence of alternative programs, such as an employment program, partly mitigates firms' financing needs and allows banks to screen firms according to whether they use another instrument.

Although the availability of the employment program helps to contain aggregate risk, the restrictions on the credit program are more quantitatively important in our counterfactual exercises. To have a sizable effect on overall risk, credit programs would need to be even more generous, firms (especially risky ones) would need to borrow more, and/or the negative aggregate shock would need to be larger than those seen in recent history, including the pandemic. These lessons from Chile might represent some of the best guidance for governments and researchers to measure the impact of credit and employment programs on aggregate indebtedness and risk, as those estimates are not readily available in the literature.

Our findings suggest avenues for further research. First and foremost, whereas we focus on measuring the potential costs of loose credit dispensation, a cost-benefit evaluation of crisis credit policies is needed. In this paper, we presume significant macroeconomic benefits justifying such an intervention, as the expected default rate is higher than the policy interest rates. However, those benefits are not quantified here. They could include preserving firm-specific capital, avoiding inefficient firm closures, and promoting firm growth relative to the counterfactual of having less government support. Moreover, a cost-benefit analysis could include the intertemporal aspects of governments' trade-offs between immediately saving firms and possibly slowing growth or recovery. The potential benefits of credit programs should be measured vis-a-vis those of alternative programs, such as employment programs.

Second, our data explicitly cover the formal sector and measure risk only for those firms with borrowing histories. Formal firms constitute the bulk of the economy in Chile, while firms with previous loans absorb most of the crisis credit. Nevertheless, in many economies, informal sectors and firms with no bank debt can be quite prominent, limiting the effectiveness of such programs. The evidence from our paper suggests that crisis credit programs might provide financing to firms with no credit history, triggering their formalization process. While we focus on crisis credit when there is an urgency to save firms, the policies we analyze might prove beneficial in non-crisis times to foster long-term credit to underserved sectors.

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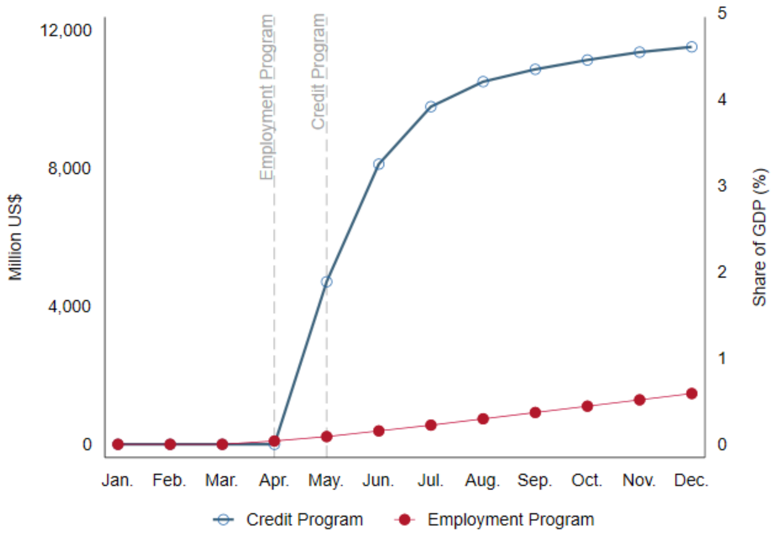
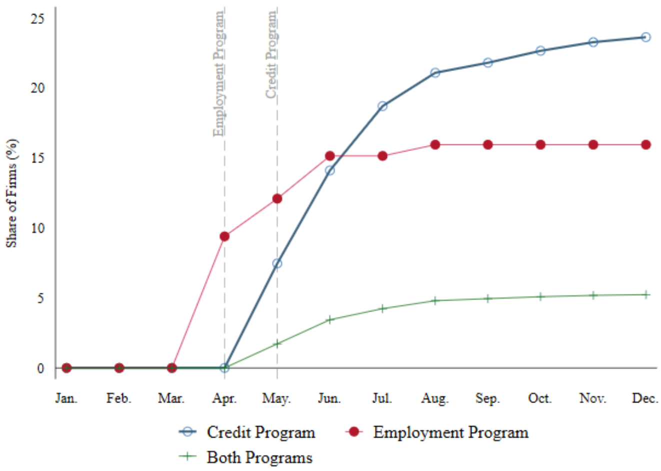
Figure 1
Reach of Public Programs



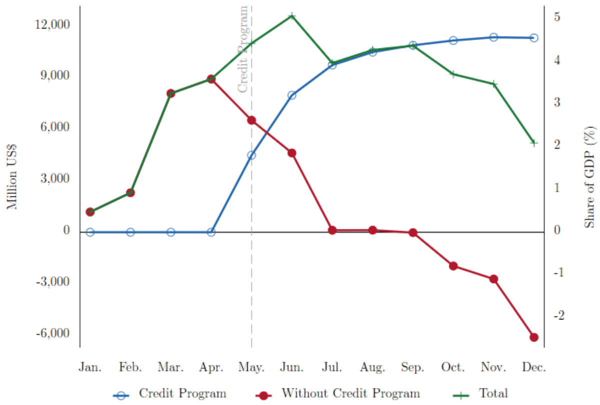
(A) Size of Public Programs



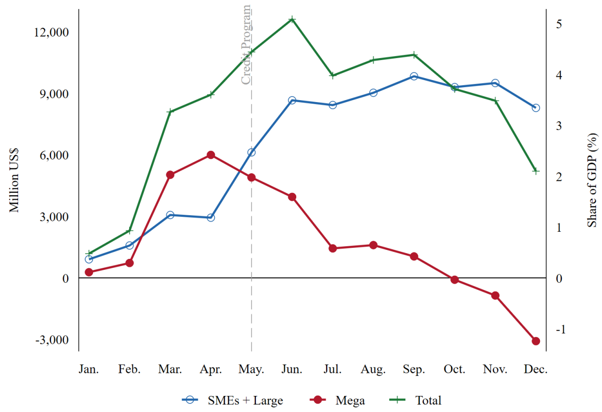
(B) Share of Firms Using Public Programs

This figure plots the size of the public programs implemented in Chile and the cumulative share of firms using public programs during 2020. Panel A plots the size in million US$ (left axis) and as a share of GDP (right axis), and considers natural persons and formal firms. Panel B displays the share of firms using the credit program, the employment program, and both programs by the end of each month during 2020. The share of firms is calculated relative to the number of eligible firms for each program from the active firms sample. The dashed vertical lines show the month when each program is implemented.

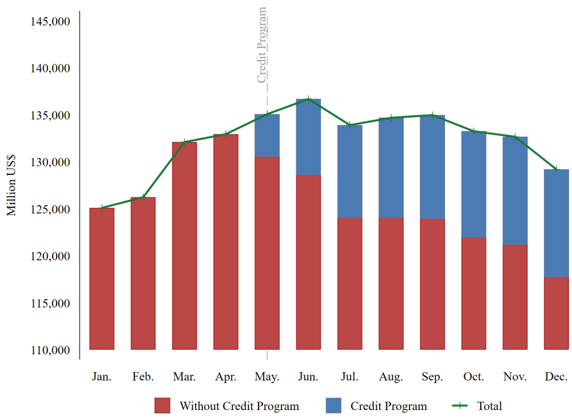
Figure 2
Credit Granted and Outstanding Corporate Debt



(A) Guaranteed and Non-Guaranteed Credit Granted



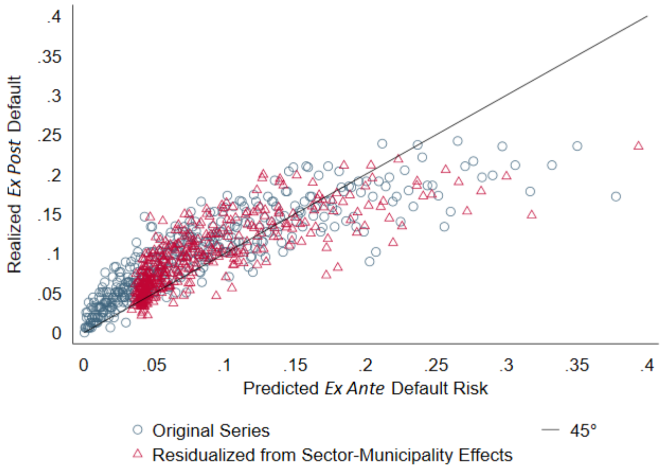
(B) Guaranteed and Non-Guaranteed Credit Granted by Firm Size



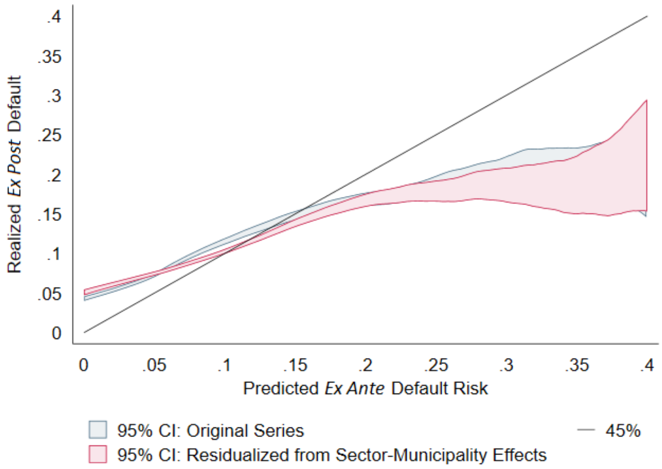
(C) Outstanding Corporate Debt

This figure plots the cumulative corporate credit granted in Chile during 2020. Panels A to C plot amounts in million US$ (left axis), considering natural persons and formal firms. Panels A and B also plot the amounts as a share of GDP (right axis). Cumulative credit is equal to the difference between the debt outstanding in a given month of 2020 and the debt outstanding in December 2019. Panel A decomposes total credit into credit guaranteed under the credit program and credit outside the program. Panel B decomposes total credit into credit granted to SMEs and large firms (eligible for the credit program) and mega firms (ineligible for the program). Panel C decomposes total outstanding debt into guaranteed debt under the credit program and non-guaranteed debt outside the program. The dashed vertical lines show the month when the credit program is implemented.

Figure 3
Correlation between *Ex Ante* Default Risk and *Ex Post* Default



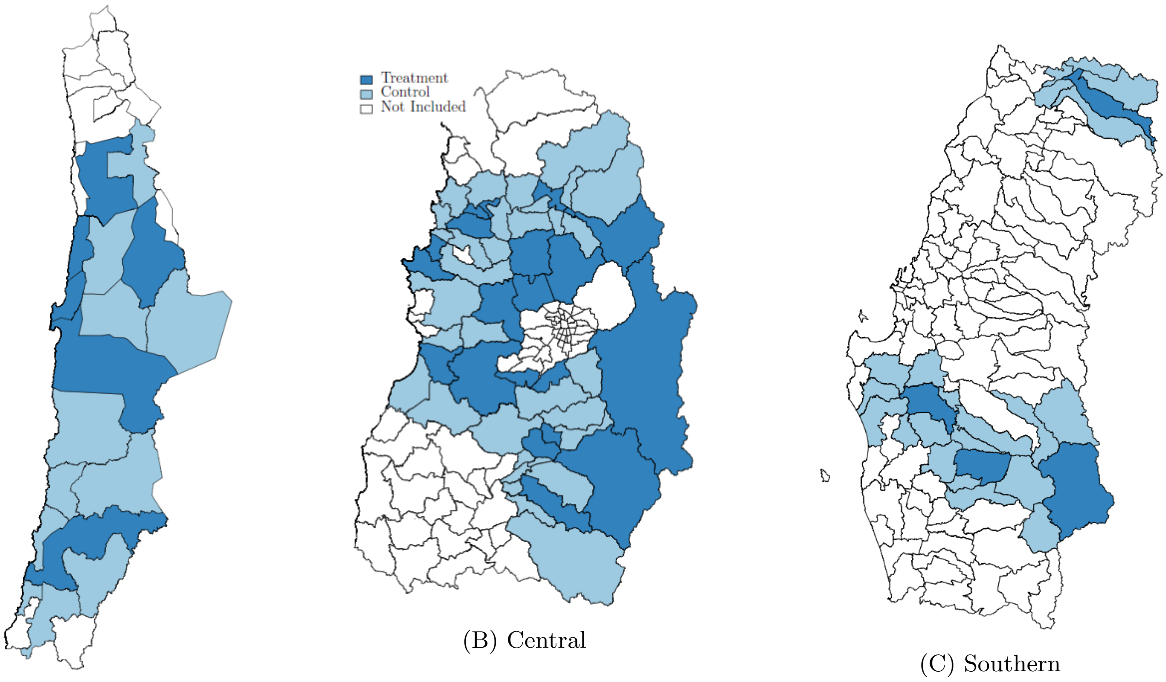
(A) Scatterplot



(B) Confidence Interval

This figure plots the correlation between predicted *ex ante* default risk and the realized *ex post* default for 2020. The figure uses the firms with previous loans of the credit program eligible firms sample, as shown in Table 2 (62,894 firms). The *ex ante* default risk is derived from the model estimates presented in Columns 4 and 8 of Table 1, where firm characteristics from 2019 are used to project probabilities of default in 2020. Although Column 8 is the most complete model of risk, we use Column 4 instead of Column 8 for firms without previous loans. *Ex post* default is a dummy equal to one if the firm has more than 90 past due days after May 2020 and zero otherwise. Panel A shows the simple correlation between the two default measures. Each dot represents a bin average of *ex ante* default risk and *ex post* default. Panel B shows the confidence intervals (CI) of the correlation using a non-parametric adjustment. The Original Series plots the average of the *ex ante* and *ex post* default pairs within each bin. The Residualized from Sector-Municipality Effects plots the same averages after controlling for sector-municipality fixed effects. Both series are split into 400 bins with an equal number of firms. Finally, the R-squared between the predicted *ex ante* default risk and the realized *ex post* default is 0.748.

Figure 4
Dynamic Lockdowns: Treatment Definition



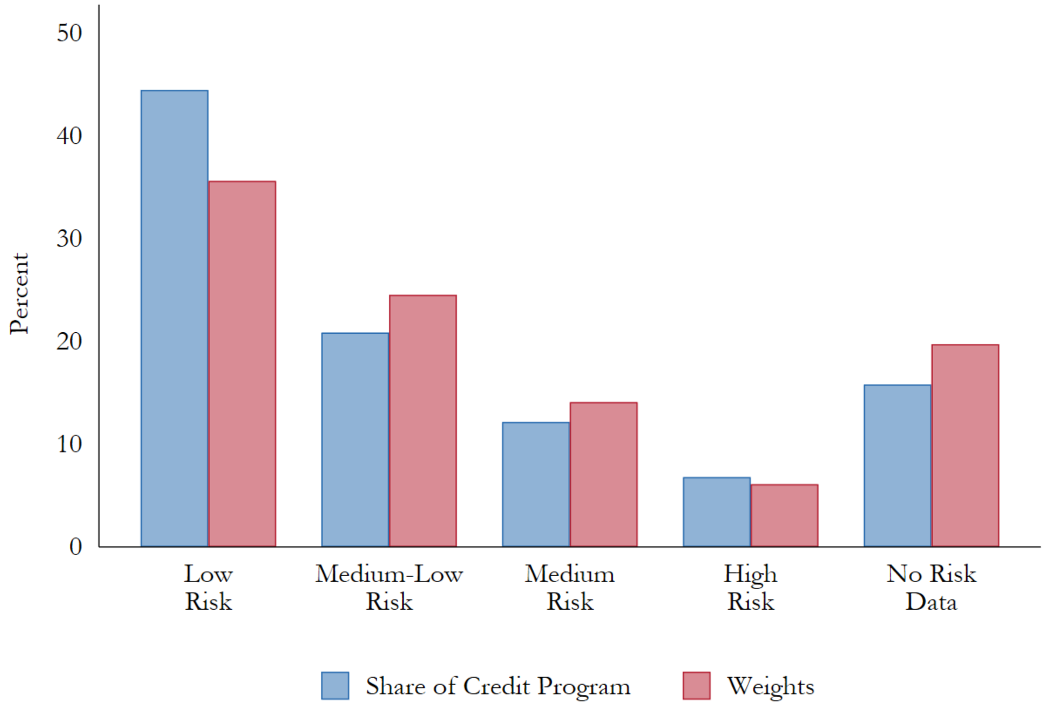
(A) Northern

(B) Central

(C) Southern

This figure shows how we identify municipalities subject to lockdown mandates over time, which we use to define the treatment for our dynamic lockdown specification (Table 4). Treated municipalities are those (i) where lockdown mandates are introduced after May 1, 2020, and (ii) that have at least one neighboring municipality that is never subject to lockdown mandates. Similarly, control municipalities are those (i) where lockdown mandates are never introduced and (ii) that have at least one neighboring municipality subject to lockdown mandates after May 1, 2020. We exclude from our analysis municipalities that do not fulfill the requirements to be included in either the treated or control group. We separate Chile into three subregions. Panel A shows the Northern region, Panel B the Central region, and Panel C the Southern region.

Figure 5
Allocation of Credit Program and Firm Risk



This figure shows the distribution of the credit program of Table 7, Panel A, Column 3, and the weights of the different risk groups of Appendix Table 29, Panel A, Column 2.

Table 1
Default Probability Model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Probit Estimation								
Log(Net Worth)	-0.011	-0.010	-0.010	-0.010	-0.009	-0.009	-0.008	-0.009
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Value Added / Number of Workers)	-0.021	-0.020	-0.018	-0.018	-0.019	-0.019	-0.017	-0.017
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Firm Age	-0.001	-0.001	-0.001	-0.001	-0.002	-0.002	-0.002	-0.002
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log(Wage Bill)	-0.010	-0.009	-0.008	-0.008	-0.008	-0.008	-0.007	-0.007
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Annual Sales)	0.007	0.006	0.003	0.003	0.000	-0.000	-0.003	-0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Debt Outstanding)					0.013	0.013	0.013	0.013
					(0.001)	(0.001)	(0.001)	(0.001)
Spread *Ex Ante*					0.003	0.003	0.003	0.003
					(0.000)	(0.000)	(0.000)	(0.000)
Dependent Variable Mean	0.088	0.088	0.088	0.088	0.088	0.088	0.088	0.088
Dependent Variable Std. Dev.	0.284	0.284	0.284	0.284	0.284	0.284	0.284	0.284
Number of firms	96,411	96,411	96,411	96,411	96,411	96,411	96,411	96,411
R^2	0.051	0.061	0.064	0.073	0.094	0.103	0.104	0.111
Industry FE	No	No	Yes	Yes	No	No	Yes	Yes
Municipality FE	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: Predicted Default Probability								
With Previous Loans	0.088	0.088	0.088	0.088	0.089	0.089	0.089	0.089
Without Previous Loans	0.113	0.113	0.107	0.107				

This table reports probit estimations of the probability of a firm with previous loans defaulting on a loan on a set of firm-level characteristics (Panel A) and the resulting predicted default probabilities for firms with and without previous loans (Panel B) for the default model sample. The dependent variable is a dummy equal to one if the firm defaults on a loan during 2019 (has a payment past due over 90 days) and zero otherwise. All explanatory variables are calculated as of December 2018. Given that the data on firms' net worth are not available for all firms, all specifications include an unreported dummy variable equal to one if the data for the firm's net worth are missing and zero otherwise. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Conversely, firms without previous loans are those with no credit records in the banking system throughout the same period. Columns 1–4 include real characteristics, and Columns 5–8 add financial characteristics. Columns 1–8 include different sets of fixed effects (FE). The table uses the 2018–2019 sample of formal firms with previous loans. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Table 2
Probability of Firms Using Public Programs

	Credit Program			Employment Program	Both Programs
	(1)	(2)	(3)	(4)	(5)
	Use	Applications	Approvals	Use	Use
Panel A: Analysis using Predicted Default Risk					
Risk	0.343	0.547	-0.264	-0.020	0.047
	(0.034)	(0.035)	(0.022)	(0.024)	(0.018)
Increase in Sales Dummy	0.195	0.187	0.014	0.053	0.064
	(0.008)	(0.008)	(0.006)	(0.007)	(0.006)
Decrease in Sales Dummy	0.193	0.190	0.014	0.112	0.102
	(0.008)	(0.007)	(0.006)	(0.007)	(0.006)
Use Employment Program	0.095	0.117	-0.009		
	(0.005)	(0.005)	(0.004)		
Use Credit Program				0.056	
				(0.003)	
Dependent Variable Mean	0.505	0.656	0.913	0.185	0.111
Dependent Variable Std. Dev.	0.500	0.475	0.281	0.389	0.315
Number of Firms	62,894	62,859	36,609	62,128	61,446
R^2	0.045	0.063	0.030	0.081	0.066
Industry FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Predicted Default Probability					
Firms with Previous Loans	0.084	0.084	0.090	0.084	0.084
Panel B: Analysis Using Spread					
Spread *Ex Ante*	0.001	0.002	-0.002	-0.001	-0.001
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Increase in Sales Dummy	0.204	0.176	0.018	0.062	0.072
	(0.010)	(0.009)	(0.008)	(0.010)	(0.009)
Decrease in Sales Dummy	0.204	0.183	0.014	0.125	0.115
	(0.010)	(0.008)	(0.008)	(0.009)	(0.009)
Use Employment Program	0.091	0.103	-0.009		
	(0.006)	(0.006)	(0.004)		
Use Credit Program				0.058	
				(0.004)	
Dependent Variable Mean	0.573	0.729	0.910	0.199	0.132
Dependent Variable Std. Dev.	0.495	0.445	0.286	0.399	0.338
Number of Firms	38,348	38,250	24,514	37,531	37,140
R^2	0.048	0.068	0.032	0.086	0.071
Industry FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Predicted Default Probability					
Firms with Previous Loans	0.107	0.107	0.107	0.106	0.106

This table reports probit estimations of the probability of a firm with previous loans using a government program on a set of firm-level characteristics. Panel A defines risk using the predicted default probability from the default probability model reported in Table 1, Column 8; Panel B defines risk using the spread between the interest rate of the loans a firm received and the risk-free rate. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. In Column 1, the dependent variable is equal to one if the firm participates in the credit program and zero otherwise, in Column 2, the dependent variable is equal to one if the firm applies to the program and zero otherwise, in Column 3 the dependent variable is equal to one if the firm's loan application is approved and zero otherwise, in Column 4 is equal to one if the firm participates in the employment program, in Column 5 is equal to one if the firm participates in both programs, and is zero otherwise. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use credit program and employment program are dummy variables equal to one for program participation and is zero otherwise. Use both programs is a dummy variable equal to one for credit and employment program participation, and is zero otherwise. The table uses the firms with previous loans of the credit program eligible firms sample. Some observations are dropped when estimating the model with fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Table 3
Probability of Firms of Different Sizes Getting Approval for the Credit Program

	Credit Program			
	(1) All Firms	(2) Small Firms	(3) Medium Firms	(4) Large Firms
Risk	−0.264	−0.249	−0.439	−0.727
	(0.022)	(0.026)	(0.086)	(0.241)
Increase in Sales Dummy	0.014	0.017	−0.005	0.002
	(0.006)	(0.007)	(0.020)	(0.036)
Decrease in Sales Dummy	0.014	0.015	−0.004	0.014
	(0.006)	(0.007)	(0.019)	(0.034)
Use Employment Program	−0.009	−0.006	−0.020	−0.028
	(0.004)	(0.004)	(0.008)	(0.020)
Dependent Variable Mean	0.913	0.908	0.915	0.899
Dependent Variable Std. Dev.	0.281	0.289	0.279	0.301
Number of Firms	36,609	27,293	6,029	1,396
R^2	0.030	0.033	0.080	0.164
Industry FE and Municipality FE	Yes	Yes	Yes	Yes
Predicted Default Probability				
Firms With Previous Loans	0.090	0.102	0.061	0.036

This table reports probit estimations of the probability of a firm with previous loans getting approved for the credit program on a set of firm-level characteristics for the credit program eligible firms sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. The dependent variable is equal to one if the firm's loan application is approved and is zero otherwise. Columns 1, 2, 3, and 4 correspond to all (small, medium, and large) firms, small firms, medium firms, and large firms, respectively. Small, medium, and large firms correspond to firms that have annual sales less than US$0.8 million, between US$0.8 and US$3.5 million, and between US$3.5 and US$35 million, respectively. Risk corresponds to the fitted values of the regression specification reported in Table 1, Column 8. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use employment program is a dummy variable equal to one for employment program participation and is zero otherwise. The table uses firms with previous loans that apply for the credit. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Table 4
Probability of Firms Using Public Programs: Dynamic Lockdowns

	(1) Use Credit Program	(2) Use Employment Program
Panel A: All Firms in Municipality		
Post × Lockdown	−0.002	0.033
	(0.004)	(0.002)
Post	0.043	−0.023
	(0.003)	(0.005)
Lockdown	0.001	0.010
	(0.008)	(0.011)
Number of Observations	28,161	32,409
Number of Firms	3,111	3,581
R^2	0.016	0.021
Region FE and Month FE	Yes	Yes
Panel B: Firms along Municipality Border		
Post × Lockdown	0.009	0.044
	(0.015)	(0.019)
Post	0.042	−0.002
	(0.006)	(0.007)
Lockdown	0.176	0.072
	(0.008)	(0.011)
Number of Observations	3,888	3,195
Number of Firms	432	355
R^2	0.037	0.042
Pair of Neighboring Municipalities FE and Month FE	Yes	Yes

This table reports panel linear regressions of the probability of using a government program for a firm located in a municipality that is subject to a lockdown mandate for the selection and leverage models sample. The dependent variables are a dummy variable equal to one if the firm participates in the credit program (Column 1) and a dummy variable equal to one if the firm participates in the employment program (Columns 2). Otherwise, the dummy variables are equal to zero. Post is a dummy variable equal to one after a lockdown mandate is implemented in the firm's municipality and is zero otherwise. Lockdown is a dummy equal to one if the firm is located in a municipality subject to a lockdown and is zero otherwise. Panel A includes region and month fixed effects. The analysis in Panel B is restricted to firms located along the border of municipalities with and without lockdown mandates and includes month fixed effects and pair of neighboring municipalities fixed effects. The latter are equal to one for each pair of municipalities that are neighbors (share a border) and zero otherwise. All pairs of municipalities in Chile receive a value. Clustered standard errors at the region level and at pair of neighboring municipalities are shown in parentheses for Panels A and B, respectively.

Table 5
Firm Indebtedness and Use of Public Programs

	Δ Debt / Sales (2019)		Δ Guaranteed Debt / Sales (2019)		Δ Non-Guaranteed Debt / Sales (2019)	
	(1) With Previous Loans	(2) Without Previous Loans	(3) With Previous Loans	(4) Without Previous Loans	(5) With Previous Loans	(6) Without Previous Loans
Use Credit Program	0.145	0.130	0.139	0.118	0.006	0.012
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Use Employment Program	0.008	0.002	0.001	0.000	0.007	0.001
	(0.002)	(0.001)	(0.000)	(0.000)	(0.002)	(0.001)
Use Credit Program × Employment Program	−0.013	−0.016	−0.003	−0.009	−0.010	−0.007
	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.002)
Increase in Sales Dummy	0.021	0.004	−0.001	0.001	0.021	0.003
	(0.003)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)
Decrease in Sales Dummy	0.017	0.002	−0.002	0.000	0.019	0.001
	(0.003)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)
Dependent Variable Mean	0.054	0.028	0.070	0.020	-0.016	0.008
Dependent Variable Std. Dev.	0.172	0.082	0.087	0.055	0.148	0.054
Number of Firms	62,950	51,729	62,950	51,729	62,950	51,729
R^2	0.190	0.359	0.627	0.644	0.017	0.019
Industry FE and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' leverage on a set of firm-level characteristics, separately for firms with and without previous loans, for the credit program eligible firms sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Conversely, firms without previous loans are those with no credit records in the banking system throughout the same period. The dependent variable is the change in the debt outstanding between December 2020 and December 2019, relative to 2019 sales. Use credit program and employment program are dummy variables for program participation. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. All columns include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. For change in debt over 2019 sales and change in guaranteed debt over 2019 sales, observations in the top and bottom 1% of the distribution are winsorized. Change in non-guaranteed debt over sales is constructed as the difference between change in debt outstanding and the change in guaranteed debt, over 2019 sales.

Table 6
Firm Indebtedness and Risk Among Credit Program Users

	Δ Guaranteed Debt / Sales (2019)		Δ Non-guaranteed Debt / Sales (2019)	
	(1) With Previous Loans	(2) Without Previous Loans	(3) With Previous Loans	(4) Without Previous Loans
Risk	0.096	0.167	-0.050	0.023
	(0.007)	(0.019)	(0.012)	(0.017)
Increase in Sales Dummy	-0.003	0.010	0.004	0.009
	(0.002)	(0.004)	(0.005)	(0.003)
Decrease in Sales Dummy	-0.007	0.004	0.001	0.004
	(0.002)	(0.004)	(0.005)	(0.003)
Use Employment Program	-0.002	-0.007	-0.003	-0.004
	(0.001)	(0.002)	(0.002)	(0.002)
Dependent Variable Mean	0.138	0.116	-0.012	0.016
Dependent Variable Std. Dev.	0.076	0.079	0.135	0.070
Number of Firms	31,756	9,068	31,756	9,068
R^2	0.033	0.092	0.028	0.077
Industry FE and Municipality FE	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' guaranteed and non-guaranteed debt on a set of firm-level characteristics, separately for firms with and without previous loans, for the credit program users sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. The dependent variable in Columns 1–2 (3–4) is the change in the stock of public guaranteed (non-guaranteed) debt between December 2020 and December 2019, relative to 2019 sales. Risk corresponds to the fitted values of the regression specifications reported in Columns 4 and 8 of Table 1 for firms without and with previous loans, respectively. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use employment program is a dummy variable equal to one for employment program participation and is zero otherwise. All columns include industry and municipality fixed effects. Bootstrapped standard errors, shown in parentheses, are clustered at the industry and municipality level. For change in guaranteed debt over 2019 sales, observations in the top and bottom 1% of the distribution are winsorized. Change in non-guaranteed debt over sales is constructed as the difference between change in debt outstanding and the change in guaranteed debt, over 2019 sales.

Table 7
Aggregate Expected Loss from the Credit Allocation

	(1) Total Credit Program (Million US$)	(2) Total Credit Program/GDP (%)	(3) Share of Credit Program (%)	(4) Default Probability (%)	(5) Expected Loss/GDP (=(2)x(4)) (%)	(6) *Ex Post* Default (%)	(7) *Ex Post* Loss/GDP (=(2)x(6)) (%)
Panel A: Risk Groups, Formal Firms							
High Risk	607	0.2	7	18.2	0.04	17.1	0.04
Medium Risk	1,087	0.4	12	9.9	0.04	12.2	0.05
Medium-Low Risk	1,863	0.8	21	5.7	0.04	11.2	0.08
Low Risk	3,972	1.6	44	2.1	0.03	6.2	0.10
No Risk Data	1,411	0.6	16	18.2	0.10	17.1	0.10
Total: Formal Firms	8,941	3.6	100	7.4	0.27	10.4	0.38
Panel B: Formal Firms + Natural Persons							
Formal Firms	8,941	3.6	78	7.4	0.27	10.4	0.38
Natural Persons	2,563	1.0	22	18.2	0.19	17.1	0.18
Total: Formal Firms + Natural Persons	11,504	4.6	100	9.8	0.45	11.9	0.55

This table shows the distribution of the aggregate of the credit allocation, for natural persons and the formal firms sample. Panel A reports statistics across the firms' risk distribution. Panel B reports statistics separately for formal firms and natural persons. Column 1 shows the total amount of guaranteed credit in dollar terms, and Column 2 normalizes Column 1 by GDP. Column 3 shows the share of guaranteed loans for each category. Column 4 shows the default probability of each category, using the model in Table 1, Columns 4 and 8. Column 5 shows the total expected loss as a share of GDP (Column 2 times Column 4). Column 6 shows the *ex post* default rate of each category, using a dummy equal to one if the firm defaults on a loan after May 2020 (has payment past due over 90 days) and zero otherwise. Column 7 shows the total *ex post* expected loss as a share of GDP. Values in Columns 4 and 6 are weighted by the total amount of guaranteed credit granted to each firm as a share of the total guaranteed credit granted to all the firms within its category. Totals by panel reported in Column 4 are calculated as the sum of the product of each category's statistic by its relative weight (Column 3). Firms are classified into risk categories based on the distribution quartiles of the fitted values of the regression specifications reported in Table 1, Columns 4 and 8. Firms with a missing risk category are assigned the risk from the high-risk category.

Table 8
Expected Loss for Banks and the Government

	(1) Expected Loss/GDP (%)	(2) Guarantee (%)	(3) Default Probability (%)	(4) Deductible (%)	(5) Effective Guarantee (=((3)-(4)) x(2)/(3)) (%)	(6) Government Expected Loss/GDP (=(1)x(5)) (%)	(7) Banks Expected Loss/GDP (=(1) x(100-(5))) (%)	(8) Banks Expected Loss/Bank Capital (%)
Panel A: Risk Groups, Formal Firms								
High Risk	0.04	82.5	18.2	4.4	62.4	0.03	0.02	0.11
Medium Risk	0.04	79.9	9.8	3.9	48.0	0.02	0.02	0.15
Medium-Low Risk	0.04	77.0	5.7	3.5	29.6	0.01	0.03	0.20
Low Risk	0.03	72.1	2.1	3.0	0.0	0.00	0.03	0.22
No Risk Data	0.10	82.5	18.2	4.4	62.4	0.06	0.04	0.26
Total: Formal Firms	0.27	76.4	7.4	3.5	40.0	0.11	0.16	1.07
Panel B: Formal Firms + Natural Persons								
Formal Firms	0.27	76.4	7.4	3.5	40.0	0.11	0.16	1.07
Natural Persons	0.19	82.5	18.2	4.4	62.4	0.12	0.07	0.47
Total: Formal Firms + Natural Persons	0.45	77.8	9.8	3.7	45.0	0.22	0.23	1.54

This table shows the distribution of the aggregate expected loss borne by the government and the banking system as a result of the credit program, for natural persons and the formal firms sample. Panel A reports statistics across the firms' risk distribution. Panel B reports statistics separately for formal firms and natural persons. Column 1 shows the total expected loss as a share of GDP. Columns 2–4 show the guarantee, the default probability of each category using the model in Table 1, and the first-loss deductible for each category, while Column 5 shows the effective guarantee, estimated as Guarantee×(Default Probability − Deductible)/Default Probability, directly by category. Columns 6 and 7 show, for each category, the fraction borne by the government estimated as Expected Loss/GDP×(1−Effective Guarantee), and the fraction borne by the banking sector, estimated as (Default Probability−Deductible)× Guarantee)/Default Probability, respectively. Column 8 normalizes Column 7 by the effective capital of the banking system. Values in Columns 2–4 are weighted by the total amount of guaranteed credit granted to each firm as a share of the total guaranteed credit granted to all the firms within its category. Totals by panel reported in Columns 2–5 are calculated as the sum of the product of each category's statistic by its relative weight (Column 3 of Table 7). Firms are classified across risk categories based on the distribution quartiles of the fitted values of the regression specifications reported in Table 1, Columns 4 and 8. Firms with a missing risk category are assigned the risk from the high-risk category.

Table 9
Empirical Counterfactual Calculations of Aggregate Risk

	(1) Combined Policy (%)	(2) Amount Cap Equal to One Month (%)	(3) Default Firms Are Eligible (%)	(4) No Credit Guarantee (%)	(5) No Employment Program (%)
(a) Program Credit/GDP	3.6	2.5	7.6	1.6	4.2
(b) Program Credit/Baseline Credit	50.5	35.3	106.9	22.4	59.0
(c) Interest Rate	3.5	3.5	3.5	3.5	3.5
(d) Default Probability	7.4	7.0	9.2	2.1	8.5
(e) Expected Credit Loss/Baseline Credit (= (b) × (d))	3.7	2.5	9.9	0.5	5.0
(f) Govt. Exp. Loss/Baseline Credit (=Guarantee × (e))	3.0	2.0	7.9	0	4.0

This table shows comparative statics of policy ingredients that potentially mitigate aggregate risk. Program credit corresponds to the credit allocated under the credit guarantee policy to formal firms. It corresponds to 3.6% of 2019 GDP, as shown in Table 7. Baseline credit corresponds to credit to program-eligible formal firms in 2019. Column 1 presents the observed scenario with the combined policies. Column 2 presents aggregate risk when reducing the credit cap to one month (rather than three months) of sales. Column 3 presents the case in which there is no eligibility constraint on previous defaults. Column 4 presents aggregate risk when there is no credit guarantee. Column 5 presents the case with no employment program. For the guarantee, we use 80% for Columns 1, 2, 3, and 5 and 0% for Column 4.

Table 10
Simulated Impacts of Policies and Theoretical Counterfactuals

	(1) Combined Policy (%)	(2) No Increased Willingness to Lend (%)	(3) No Interest Rate Cap Reduction (%)	(4) No Credit Guarantee (%)	(5) No Employment Program (%)
(a) Program Credit/Baseline Credit	50.3	23.8	68.0	35.9	50.3
(b) Interest Rate	1.4	2.7	5.0	1.7	1.4
Typical Year					
(c) Default Probability	5.9	3.0	12.7	4.0	5.9
(d) Expected Credit Loss/Baseline Credit (=(a) × (c))	3.0	0.7	8.7	1.4	3.0
(e) Govt. Exp. Loss/Baseline Credit (=Guarantee × (d))	2.4	0.6	6.9	0	2.4
(f) Actual Burden/Baseline Credit	0.4	0.2	1.9	0	0.4
Systemic Shock					
(g) Default Probability	14.8	10.4	21.2	12.1	14.8
(h) Expected Credit Loss/Baseline Credit (=(a) × (g))	7.4	2.5	14.4	4.4	7.4
(i) Govt. Exp. Loss/Baseline Credit (=Guarantee × (h))	6.0	2.0	11.5	0	6.0
(j) Actual Burden/Baseline Credit	1.6	1.0	3.6	0	1.6

The combined policy scenario is a counterfactual with both policies relative to a case with no policy. The baseline scenario with no policy has an interest rate of 4.6%, a default probability of 5.6% in a *typical year*, and a default probability of 13.8% under a *systemic shock*. The default probability is credit-weighted. The numbers in the no employment program column are identical to the baseline scenario because no firms opt to take the program in the benchmark calibration. Systemic shock involves an *ex post* realization of a 5% drop in TFP and a 40% increase in dispersion. For the guarantee, we use 80% for Columns 1, 2, 3, and 5 and 0% for Column 4.

Appendix A: Data Samples

We use several samples of the merged data described in Section 3 in different parts of the paper, each with a different size and coverage (as shown in Appendix Table 1). To construct the samples of firms, we start from all legal and formally registered firms in the economy (602,882 firms) with a tax identification, which we call *formal firms*.[75] The first sample, which we call *active firms*, is constrained to include only firms with positive sales in 2019, which amounts to 449,632 firms. We use this sample to conduct the aggregate analysis of the paper and the mapping between micro and macro patterns. This sample represents 75% of firms, 92% of private sector employment, 82% of the debt outstanding, and 100% of positive value added in the economy. Among active firms, 97% are SMEs and contribute 43% and 27% of total employment and credit in the economy, respectively. The remaining 3% of active firms are large firms (2%) and mega firms (1%).

The second sample is used to estimate the default probability models that measure firm-level default risk. Starting from the active firms sample, this second sample includes only firms with available data on default during 2019, plus sales, number of workers, value added, firm age, municipality, and industry in December 2018. Firms in default are those with loans past due 90 days. We consider only firms with a previous loan to estimate the model, i.e., firms with outstanding debt as of December 2019 or receive a loan over the period 2012–2019. Firms with previous loans constitute 36% of active firms (capturing 79% and 87% of employment and value added, respectively).

The third sample adds further restrictions to the active firms sample. We restrict the sample to all firms with the relevant observables to perform the main regression analysis, including a measure of default risk. This sample excludes firms that use the employment program before the public credit guarantee program starts (end of April 2020) to compare the two policies more equally. We call this the *selection and leverage model* sample. This sample represents 20% of firms, 50% of employment, 44% of the debt outstanding, and 74% of value added. Although this sample is smaller than the others, it provides detailed information at the firm level that is unavailable for other firms and is essential for the regression analysis we perform.

The fourth sample starts from the selection and leverage model sample. It imposes the eligibility constraints from the public credit guarantee program, namely that firms must

[75] We exclude natural persons who use their personal tax identification to borrow as a firm. For these natural persons, we do not have the same scope of information as we do for active firms, and we exclude 818,572 tax IDs for this reason. These natural persons are only included in our aggregate analysis when we report the total value of the program and in our estimate for total expected credit loss (Table 7).

be smaller than the sales threshold imposed by law and cannot have payments past due more than 30 days (i.e., a strict default measure).[76] We call this the *credit program eligible firms* sample. This sample represents 19% of firms, 35% of employment, 21% of the debt outstanding, and 19% of value added.

The fifth sample starts from the credit program eligible firms sample and selects only the firms that actually use the credit program. We call this the *credit program users* sample, but in practice, it constitutes only the subsample of firms with the required observable data (i.e., the selection and leverage model sample). This sample represents 7% of firms, 14% of employment, 9% of the debt outstanding, and 7% of value added.[77] For some estimations, we further partition different samples based on their banking status. In particular, we split the selection and leverage model sample, the credit program eligible firms sample, and the credit program users sample into two sub-samples of firms with and without previous loans.

Although the different samples have different coverage based on the data availability, we compute the aggregate effects for *all* firms. To do so, we use the default probability for each type of firm and aggregate the total effects using the total credit allocated to each group. We also impute the default probability of the high-risk group to the firms with no risk data to avoid underestimating aggregate risk.

Appendix B: Regression Discontinuity Design Results

To support the causal claim that the increase in firm indebtedness can be attributed to the credit program, we conduct a regression discontinuity design (RDD) analysis. As explained in Section 2, there are two eligibility requirements for the credit program: size (previous year's sales) and delinquency (number of days past due) at the moment of application. While both of these margins could potentially be used as eligibility cutoffs for the RDD, we focus on size because it is a difficult variable to manipulate to meet the program's requirements. The number of days past due can be more easily changed by a firm paying off its due debt when applying to the program, thus changing its eligibility status in that margin. We focus on annual sales from October 2018 to September 2019 as the running variable for size. The size cutoff for the program is US$35 million in sales. We run a standard RDD around that cutoff using the recommended optimal bandwidth (Calonico et al., 2014), and the outcome is leverage.

[76]The employment program does not have a selection constraint at the firm level (other than having positive employment).
[77]Appendix Table 3 shows detailed summary statistics of the main variables used in our paper.

Appendix Figure 5 displays the RDD results graphically. Panel A shows the share of firms that use the credit program around the size eligibility cutoff. The share of firms with annual sales below 1 million Inflation-Indexed Unit of Account (equivalent to US$35 million) participating in the program is around 30%. Those with annual sales larger than 1 million Inflation-Indexed Unit of Account are significantly less likely to participate. We observe that some firms use the credit program even when they are above the eligibility cutoff, probably because there are different valid sales measures that firms could present when applying. Also, reported annual sales might differ from the administrative data in this paper. Despite these considerations, being larger than the eligibility cutoff significantly decreases the likelihood of a firm participating in the credit program by 14%. Panel B shows the effect of being at either side of the cutoff on leverage variation, measured as the change in (net) debt during 2020 relative to 2019 sales. Unreported RDD estimations show that crossing the size threshold and thus causally limiting the use of the credit guarantee program reduces the change in leverage by 4%, a result that is statistically different from zero.

Appendix C: From Firm to Aggregate Indebtedness

To determine how micro-level indebtedness reflects on the overall economy, we partition firms into four groups according to their predicted default risk, from high risk to low risk. The change in indebtedness in each risk group is obtained by multiplying the within-group change in the indebtedness of firms in each risk group by the weight of that group of firms in aggregate economic activity (measured by sales):

$$\underbrace{\frac{D_{gt} - D_{gt-1}}{Y_{gt-1}}}_{Within\ Change} \underbrace{\omega_{gt-1}}_{Weights} = \underbrace{\frac{D_{gt} - D_{gt-1}}{Y_{gt-1}} \omega_{gt-1}}_{Group\ Change} . \tag{6}$$

We then obtain the aggregate change in indebtedness, relative to aggregate sales, by adding the contribution of leverage of the different risk groups:

$$\sum_{g \in G} \underbrace{\left(\frac{D_{gt} - D_{gt-1}}{Y_{gt-1}} \omega_{gt-1} \right)}_{Group\ Change} = \underbrace{\frac{\Delta D_t}{Y_{t-1}}}_{Aggregate\ Change} . \tag{7}$$

G is a partition of firms according to risk, and g indexes a group of firms. $Y_{gt-1} = \sum_{i \in g} y_{it-1}$ is the sales of group g of firms, where y_{it-1} denotes firm-level sales. $D_{gt} = \sum_{i \in g} d_{it}$ is the debt outstanding of group g of firms, where d_{it} denotes firm-level debt outstanding. $\omega_{gt-1} = Y_{gt-1}/Y_{t-1}$ is the weight of group g in aggregate sales in year $t-1$. ΔD_t is the aggregate yearly change in debt between t and $t-1$ (between 2020 and 2019). Y_{t-1} is aggregate sales in 2019.[78]

Appendix Table 29 presents the results of this aggregation. We first consider the set of credit program users (Panel A).[79] The change in indebtedness for these firms takes into account both program and non-program debt.

Consistent with the results in the paper, riskier firms experience larger within-group changes in indebtedness: the leverage of high-risk firms increases by 11.58 percentage points, while the leverage of low-risk firms increases by 8.84 percentage points (Column 1). Nevertheless, riskier firms represent a smaller share of aggregate activity: high-risk firms represent only 6.1% of countrywide sales compared to the low-risk firms that represent 35.6% of aggregate sales (Column 2). As a result, the contribution of high-risk firms as a group to overall indebtedness is smaller (0.71 percentage points) than the contribution of low-risk firms (3.15 percentage points) (Column 3). The small weight of riskier firms in the aggregate, therefore, mitigates the micro selection by riskier firms documented in the paper.

Summing across all risk groups shows that the indebtedness of firms that use the credit program increases by 9.71 percentage points. Those firms experience an increase of 3.6% of GDP in guaranteed credit (Table 7) and a decline in non-guaranteed credit. Extending the analysis to include all active firms (users and non-users) by risk confirms that the higher the risk, the larger the within-change in debt (Appendix Table 29, Panel B). But the higher the risk, the smaller their weight in the economy, attenuating the increase in aggregate risk.[80]

Appendix D: Model Details

This appendix gives further details of the model in Section 7, including equations, calibration, and analysis/intuition. In addition to producing the quantitative results, the model motivates and justifies the empirical predictors of risk in our default probability model.

D.1 Firms

For convenience, we reproduce the firm's profit equation, (7), which depends on its given state including productivity, z, equity, e, and labor, l:

[78] In alternative estimations, we use value added instead of sales, obtaining similar results. However, the magnitude of change in debt relative to value added is larger than that relative to sales (as sales provide a measure of gross output). We report the estimates relative to sales to link it better to the micro part. Moreover, unlike value added, most firms in the economy report sales.

[79] The results reported in this table are only for credit-program users and therefore are not directly comparable with the numbers in Figure 2.

[80] In unreported results, we decompose indebtedness into two additional margins: banking status and size. Across the different partitions of firms, large increases in firm leverage within groups occur for firms with a relatively small weight at the aggregate level.

$$\pi = \varepsilon Az \left(\min\{e+b, \frac{1}{\psi}(l+\Delta l)\} \right)^{\alpha} - w(l+\Delta l) - \eta|\Delta l|, \tag{8}$$

Recall ε is a random variable with $E(\varepsilon)=1$, continuously distributed over $\mathbb{R}^{+}$ according to a cumulative distribution function, $\Phi(\cdot)$. Capital depreciates at rate δ.

Firms value is expected revenue net of debt servicing costs. Given its debt and equity, the firm defaults when its value turns negative:

$$\varepsilon Az(e+b)^{\alpha} + (1-\delta-\psi w)(e+b) - \eta|\psi(e+b)-l| - (1+r_b(b;e,z))\, b < 0. \tag{9}$$

When this holds with equality, it defines a threshold lower bound shock level $\underline{\varepsilon}$ below which the firm defaults:

$$\underline{\varepsilon}(b;e,z) = \frac{(\delta+\psi w + r_b(b;e,z))\, b + \eta|\psi(e+b)-l| - (1-\delta-\psi w)\, e}{Az(e+b)^{\alpha}}. \tag{10}$$

Because the sum of the user cost of capital, $\delta + r_b(b;e,z)$, and marginal labor cost (inclusive of any adjustment costs), is positive, the default threshold is increasing in debt to (expected) revenues, one of our empirical proxies for risk.[81] An immediate corollary is that the probability of default is increasing in this measure of leverage, *ceteris paribus*, justifying the use of leverage as an empirical predictor of risk in our default probability model. This is only a partial measure, however, because the pattern is true for a given distribution of ε, $\Phi(\cdot)$.

Given this optimal default behavior, the firm chooses its debt and change in labor to maximize its expected value:

$$\max_b \quad \int_{\underline{\varepsilon}(b;e,z,l)}^{\infty} \left[\varepsilon Az(e+b)^{\alpha} + (1-\delta-\psi w)(e+b) - \eta|\psi(e+b)-l| - (1+r_b(b;e,z,l))\, b\right] \Phi(d\varepsilon).$$

After integrating and substituting in the threshold condition, firm profit simplifies to

$$\max_b Az(e+b)^{\alpha}\, G(\underline{\varepsilon}(b;e,z)), \tag{11}$$

with

$$G(\underline{\varepsilon}(b;e,z,l)) \equiv \int_{\underline{\varepsilon}(b;e,z,l)}^{\infty} \varepsilon \Phi(d\varepsilon) - (1-\Phi(\underline{\varepsilon}(b;e,z,l)))\, \underline{\varepsilon}(b;e,z,l). \tag{12}$$

The first term captures the expected revenues, while the second captures the value of undepreciated capital net of labor expenses and expected debt payments.

D.2 Banks

Competitive banks lend to firms and face a constant cost of capital, $(1+r)$. In the event of default, the lender earns

$$\varepsilon Az(e+b)^{\alpha} + (1-\delta-\psi w)(e+b) - \eta|\Delta l| - \mu z(e+b)^{\alpha},$$

[81] In the special case of $\alpha = 1$, one can also see that the default threshold is increasing in debt to total assets, $b/(e+b)$.

where $\mu z (e+b)^{\alpha}$ captures the cost of verifying the state, which is proportional to the typical firm profits.

Given perfect competition, loan pricing ensures zero profits:

$$\begin{gathered}(1-\Phi(\underline{\varepsilon}(b;e,z,l)))\,(1+r_b)\,b+\int_0^{\underline{\varepsilon}(b;e,z,l)}\varepsilon Az\,(e+b)^{\alpha}\,\Phi(d\varepsilon)+\\ (1-\delta-\psi w)\,(e+b)-\eta|\Delta l|-\mu z\,(e+b)^{\alpha}-(1+r)\,b=0,\end{gathered} \tag{13}$$

where the first term is income in the event of full debt payment, the second term is income under default net of state verification costs, and the third term is the cost of capital. After some simplifying algebra, the equilibrium interest rate is therefore

$$r_b(b;e,z,l)=r+\frac{Az\,(e+b)^{\alpha}}{b}\left(\Phi\left(\underline{\varepsilon}(b;e,z,l)\right)\mu+\int_0^{\underline{\varepsilon}(b;e,z,l)}\left(\underline{\varepsilon}(b;e,z,l)-\varepsilon\right)\Phi(d\varepsilon)\right). \tag{14}$$

Here, the borrowing rate exceeds the cost of capital by a default risk premium.[82] This justifies our use of the borrowing rate as an additional empirical predictor for risk.

Is $r_b(b;e,z,l)$ increasing in b? The default premium on the right varies inversely with the leverage ratio of debt to expected revenues, $b/\left(z\,(e+b)^{\alpha}\right)$, and directly with the sum of a term combining the default probability and the state verification cost parameter, $\Phi\left(\underline{\varepsilon}(b;e,z,l)\right)\mu$, and another term capturing the total productivity lost below the break even. The last two terms are increasing in b since $\partial\underline{\varepsilon}/\partial b>0$. Whether the first term is decreasing in b depends on whether the marginal product of capital exceeds one.

D.3 Credit and Employment Protection Programs

From the firm's perspective, the competitive banking system and supply of credit are captured by Equation (14), which is a constraint in the firm's problem. We model the introduction of the credit program as a comparative static exercise. We introduce key features of the Chilean banking framework and credit program into the model in three ways.

First, we establish an initial statutory interest rate cap on credit, $\bar{r}_{b,0}$. This leads to an additional constraint:

$$r_b(b;e,z)=r+\frac{Az\,(e+b)^{\alpha}}{b}\left(\Phi\left(\underline{\varepsilon}(b;e,z)\right)\mu+\int_0^{\underline{\varepsilon}(b;e,z)}\left(\underline{\varepsilon}(b;e,z)-\varepsilon\right)\Phi(d\varepsilon)\right)\leq\bar{r}_{b,0}, \tag{15}$$

which effectively defines a limit of available credit b as a function of equity and productivity. Indeed, for low productivity firms, available credit may be zero. A first element of the credit

[82] Substituting in for $r_b\,(b;e,z)$ and further simplifying yields a useful and insightful equivalent expression for the lender's zero profit condition:

$$Az\,(e+b)^{\alpha}\,(1-G\left(\underline{\varepsilon}\,(b;e,z,l)\right))+(1-\delta-\psi w)\,(e+b)-\eta|\Delta l|-\Phi\left(\underline{\varepsilon}\,(b;e,z,l)\right)\mu z\,(e+b)^{\alpha}\ -(1+r)\,b=0,$$

where the bank's expected revenues include the share, $1-G\left(\underline{\varepsilon}\,(b;e,z)\right)$, that does not go to the firm and the undepreciated capital net of labor adjustment costs, while its costs include expected default costs and the loan's direct cost. Moreover, one can express $1-G\left(\underline{\varepsilon}\,(b;e,z)\right)=\underline{\varepsilon}+\underline{\varepsilon}\Phi\left(\underline{\varepsilon}\right)-\int_0^{\underline{\varepsilon}}\varepsilon\Phi(d\varepsilon)$. The first term is the payment without default, while the second term is the shortfall that comes from default. This decomposition is useful for incorporating the partial credit guarantee.

program is a substantially lower interest rate cap on public guaranteed credit, which we model as $\bar{r}_{b,1} < \bar{r}_{b,0}$.

Second, we introduce the partial guarantee against default, the part of the program in which the government agrees to reimburse a fraction χ of defaulted credit. We model this as banks bearing only a fraction $(1-\chi)$ of defaulted credit payments. Hence, combining these first two elements, the interest rate constraint under the program becomes

$$r_b(b;e,z) = r + \frac{Az\left(e+b\right)^{\alpha}}{b}\left(\Phi\left(\underline{\varepsilon}(b;e,z)\right)\mu + (1-\chi)\int_0^{\underline{\varepsilon}(b;e,z)}\left(\underline{\varepsilon}(b;e,z)-\varepsilon\right)\Phi(d\varepsilon)\right) \tag{16}$$
$$\leq \bar{r}_{b,1}.$$

It is straightforward to derive the expected defaulted payment on an equilibrium loan:

$$Az\left(e+b\right)^{\alpha}\left(\Phi\left(\underline{\varepsilon}\left(b;e,z\right)\right)\underline{\varepsilon}\left(b;e,z\right) - \int_{\infty}^{\underline{\varepsilon}(b;e,z)}\varepsilon\Phi(d\varepsilon)\right).$$

The expected government subsidy is then χ times this expression. For simplicity, we abstract from two features of the guarantee: (i) the dependence of the level of partial guarantee, χ, on firm size and (ii) the deductible. We calibrate χ conservatively by choosing the largest value. Both assumptions are conservative in assessing potential risks and the government burden.

Third, we introduce an exogenous increased willingness to lend that accompanies the program. To do so, we consider r as a sum of the cost of capital to banks, $\tilde{r}$, and a perceived proportional intermediation cost, c, which includes a return to equity. Clearly, it is the sum of the two that matters for the lending rate that banks are willing to lend to safe borrowers, but we distinguish between the two to emphasize that willingness to lend can come from different forces. A drop in the cost of capital $\tilde{r}$ can capture the liquidity facilities offered by the central bank discussed earlier. A decline in c under the policy could capture the fact that banks use fewer resources to screen and process loans or an exogenous increase in willingness to lend. To simplify and to stick to a more standard model in the literature, we abstract from other aspects of the program touched upon in the empirics (such as eligibility requirements and the choice between program and non-program credit). All of this makes our model best viewed as a model of eligible firms that, while appropriate for thinking about the program's impact on firm indebtedness, is less appropriate for thinking about the extensive margin choices of borrowing or program participation.

Lastly, we consider the employment protection program by modeling it as an opportunity cost to shut down without paying the negative labor adjustment cost associated with labor moving to zero. However, the firm must still pay a fraction ζ of its existing wage bill. The government subsidizes the remaining $1-\zeta$ share of the wage bill.

The firm maximization, therefore, becomes:

$$\max\{(1-\delta)\,e-\zeta\psi wl,\max_{b}Az\,(e+b)^{\alpha}\,G\,(\underline{\varepsilon}\,(b;e,z,l))\},\tag{17}$$

D.4 Calibration

We start by calibrating the model to aggregate pre-policy numbers in the data. We start with the relevant policy parameters: pre-guarantee parameters of $\chi = 0$ (absence of a government guarantee scheme) and $\bar{r}_{b,0} = 0.2$, equal to the regulated maximum interest rate.

Another key parameter of interest that we calibrate is the underlying revenue risk, σ_{ε}^2, the variance parameter of $\Phi(\cdot)$, the log normally distributed realizations of revenue. Heterogeneity in revenue risk across firms is captured by the fact that σ_{ε}^2 is itself log normally distributed across firms. Other key parameters capturing the distribution of firms include the variance of log productivity, σ_z^2, the variance of log equity, σ_e^2, the mean level of log equity, $\overline{e}$, and the correlation between log productivity and log equity, ρ.

We also need a parametric choice for $\Phi(\cdot)$ at the firm level, which we model as a log normal distribution with variance parameter, σ_{ε}^2, and for the technology parameters δ, α, and μ. We consider heterogeneity in productivity z, equity e, initial labor, l, and σ_{ε}^2 itself. We assume that e and z are jointly log normal. By normalizing mean log productivity, $\overline{z} = 1$, this distribution introduces four additional parameters: the variance of log productivity, σ_z^2; the variance of equity, σ_e^2; the mean level of log equity, $\overline{e}$; and the correlation between log productivity and log equity, ρ.

To discipline the distribution of initial labor, we make assumptions on the distribution of the labor/equity ratio, $\lambda = wl/e$. Specifically, we assume it is log normally distributed with mean log value, $\bar{\lambda}$ and variance σ_{λ}^2. The variation in λ endowments together with a large enough $\eta > 0$ could, in principle, be an incentive for firms with a larger than optimal labor endowment to choose the employment protection program. However, the small calibrated value of η does not imply this in our quantitative exercises.

Because firms could differ in underlying revenue uncertainty, something independent of average productivity, equity, and debt, we assume that σ_{ε}^2 is distributed log normally with parameters governing the mean, $\overline{\sigma}$, and variance, σ_{σ}^2. We refer to the probability density function as $H(z, e, \sigma_{\varepsilon}; \sigma_z^2, \sigma_e^2, \overline{e}, \rho)$.

With functional forms set, we calibrate the parameters themselves to data moments of the sample of eligible firms. We choose the interest rate r at 0.014 to exactly match the (credit-weighted) rate paid by firms in our sample with low debt/equity (less than 0.1) and therefore with very low default risk.[83] Although we have not done so explicitly, a simple way to model materials is to consider them proportional to labor. In that case, ψw captures labor and material expenses, both of which are proportional to capital. In our data, materials and labor amount to about three-fourths of gross output. Given a capital-output ratio of about 0.9, we calibrate $\psi w = 0.83$ (i.e., 0.75/0.9=0.83). We calibrate δ to a depreciation rate of 0.06 and set the returns to scale parameter in revenues at $\alpha = 0.8$, which is comparable to calibrations for competitive models (e.g., Buera et al., 2011; Restuccia and Rogerson, 2008) or consistent with a markup of 20% in monopolistic competition work that calibrates constant returns to scale in production but with a constant elasticity of demand.

The last 11 parameters (the total factor productivity parameter, A, four governing the exogenous joint distribution of firm productivity and equity, σ_z^2, $\overline{e}$, σ_e^2, and ρ; the two governing the distribution of revenue risk, $\overline{\sigma}$ and σ_σ^2; the cost of default, μ, and the parameters impacting labor choices, $\bar{\lambda}$, σ_λ^2, and η,) target twelve key moments in the aggregate economy. Nine are credit-related moments: the average interest rate (4.7% in the data versus 4.6% in the model); the (credit-weighted) average default (5.5% in the data vs. 5.6% in the model); the simple average (3.3 vs. 3.6), credit-weighted average (3.3 vs. 4.5), and standard deviation of debt-to-equity (6.5 vs. 5.0); the average (0.31 vs 0.26) and standard deviation (0.93 vs. 0.91) of debt-to-sales; the standard deviation of log sales (1.6 vs. 1.7); and the correlation between log sales and log equity (0.69 vs. 0.64). The final three are labor-related moments: the average change in log labor (0.04 vs. 0.04); and the average (3.3 vs. 3.0) and standard deviation (4.6 vs. 4.4) of the ratio of labor-to-equity.[84]

Given that the model is overidentified, the overall fit is reasonable, although we somewhat undershoot the debt-to-sales ratio and overshoot the debt-to-equity ratio. The calibrated parameters, $\eta = 0.0043$, $\bar{\lambda} = 1.13$, and $\sigma_\lambda^2 = 0.16$, primarily drive labor moments which they match reasonably well. The small value of η will make the employment protection program only optimal for firms in lockdown, however. The values governing risk, $\overline{\sigma} = 0.34$ and $\sigma_\sigma^2 = 0.16$, indicate both considerable risk and considerable exogenous variation in risk across firms. Other parameter values are $\sigma_z^2 = 0.24$, $\overline{e} = -1.4$, $\sigma_e^2 = 0.23$, and $\rho = -0.70$. The cost of default, $\mu = 0.26$, implies that default costs 26% of a typical year's sales. higher than the 0.15 used for the United States (Covas and Den Haan, 2012). Finally, note that

[83]Implied intermediation costs are moderate. If we set the cost of capital to the monetary policy rate of 0.5%, the cost of intermediation and the return to equity would amount to $c = 0.009$ in the benchmark.

[84]These data moments are calculated using the credit program-eligible firms sample. We use predicted (or expected) default rates rather than realized default rates to remove selection bias since eligibility selects on realized default; i.e., firms in default are ineligible.

the credit-weighted default rate and interest rate are quite close. If default amounted to no recovery, even without a cost of default, banks would receive less money than they lent in expectation (1.047*(1-0.055)<1). These values must therefore reflect substantial loan recovery even in the event of default, ensuring a gap between expected credit loss and true losses from default (even after default costs). This feature of the data underscores the importance of accounting for partial recovery and the large gap between the government's share of at-risk credit and actual government burden.

Quantitatively, we simplify the multi-tiered partial guarantee scheme in which guarantees vary by firm size by collapsing it into a single guarantee of $\chi = 0.8$. We set $\bar{r}_{b,1} = 0.035$, the statutory maximum interest rate under the program. To capture the overall increased willingness to lend, we lower the cost of intermediated capital by 1.65 percentage points.[85]

As described in Section 7, we assess the *ex post* measures under two scenarios: (i) the typical year's default rates used for the model calibration (and therefore lending decisions) and (ii) an unanticipated systemic shock to the distribution of productivity after credit is already granted.[86] That is, we consider a combination of first and second moment shocks by lowering the productivity distribution's log mean by 4.2 log points and increasing the log standard deviation by 40%.[87] These values capture a large but reasonable shock to the economy. Moreover, our combined shock lowers the value added of eligible firms by 4.9%. Such a drop in annual output is not outside of the Chilean experience.[88]

D.5 Further Intuition Behind the Credit Program Results

To build intuition for the results in Table 10, we consider the borrowing and default behavior of two different firms. Specifically, Appendix Figure 6 illustrates the credit variables for an average value of productivity, $z = 1$. We plot equity on a log basis to show details for low levels of equity.[89] The left panels illustrate these functions for firms with $\sigma_\varepsilon = 0.34$, essentially the middle of the distribution. The solid dark line illustrates the results for a firm in the benchmark banking system. Firms with equity above one (just less than a typical

[85] This value leads to an increase in lending of about 9.5% under the policy. Though not a perfect comparison, the total lending in the program equals 9.5% of the stock of firm credit in 2019. The model simulates credit without distinguishing between program and non-program credit. To measure program credit in the model, we use the percentage of program credit relative to the total observed credit of eligible firms in 2020, which amounts to 43.6%.

[86] In an alternative simulation where a systemic shock at default rates is anticipated at the time of lending, credit is dramatically reduced, interest rates are higher, and default is intermediate between the two scenarios on which we focus. However, the impacts of the policy relative to the status quo are similar to those we present for a typical year's default rate.

[87] Xiao (2022) estimates that the second moment of the productivity distribution increases by 40% during the Great Recession for the United States in a model similar to ours.

[88] In 1954, 1973, 1975, and 1982, the drop in real GDP exceeds 5%. In 2020, it declines by 5.8%.

[89] We show results relative to equity to highlight the financial aspects of the model.

year's revenues) exhibit essentially no risk of default (top-left panel) because equity acts as both collateral and firm value, deterring default. Risks can be considerable for those with equity levels below 0.2, however. The borrowing rates (second-left panel) reflect risk premia, which only become high at low levels of equity. At equity levels above one, premia are zero and borrowing rates equal the cost of capital, r.

The figure also shows that the debt levels exhibit a hump shape (third-left panel). At low levels of equity, additional equity leads to more borrowing since it lowers risk and borrowing rates. However, once borrowing rates converge to the cost of capital, debt declines with equity because firms are at their optimal risk-free level of capital. The firms simply substitute internal capital for external capital one for one, and consequently, firms with high equity (relative to productivity) do not borrow at all. In addition, leverage (debt to revenue) correspondingly declines with equity (fourth-left panel).

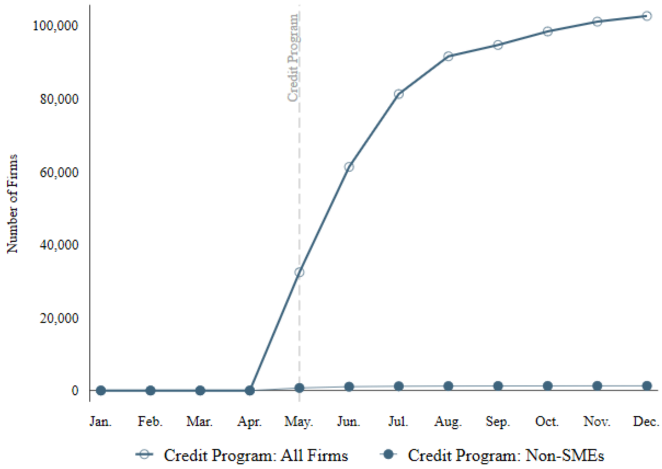
Lastly, the distribution of credit by equity level (bottom-left panel), constructed by multiplying the debt levels by the density of firms, $H(z, e, \sigma_\varepsilon; \sigma_z^2, \sigma_e^2, \overline{e}, \rho)$, is disproportionately tilted toward safe firms. The equity distribution for this productivity is tightly distributed between 0.1 and 0.7, where default rates are generally low. While low-equity, higher-default risk firms have high leverage, they constitute a negligible share of the total borrowing, with substantially more credit going toward safe firms, consistent with the low overall levels of predicted credit-weighted default among eligible firms.

For comparison, Appendix Figure 6 also shows the same outcomes for firms with a higher σ_ε of 0.80, about one standard deviation out into the right tail of the revenue uncertainty distribution (right-hand side panels). We choose this far tail for clear illustrative purposes and focus first on the black lines. Default rates are much higher (top-right panel). The borrowing rate now reflects a substantial default risk premium (second-right panel), which declines with equity to the cost of intermediate capital as the default rate goes to zero. Interest rates reflect risk premia and are therefore a good measure of equilibrium risk. Indeed, the flat segment at very low levels of equity in the second-right panel, which is the result of the interest rate cap at 20%, is reflected in the default rates of the top-right panel. The default risk and substantial risk premia are higher despite lower levels of debt (third-right panel) and comparable levels of leverage (fourth-right panel). Together, the top and fourth panels display that both risk and leverage decline with equity, showing that leverage is a proxy for default risk. This is consistent with our empirical interest in leverage. Moreover, it is consistent with our empirical results that default is increasing in debt but decreasing in firm value (Table 1, Column 5). Importantly, overall credit going to these high-risk firms is quite low (bottom-right panel) in the benchmark since their higher risk level σ_ε is less common. Hence, although individual firms may face high default risks exceeding 20%, overall risk levels are low.

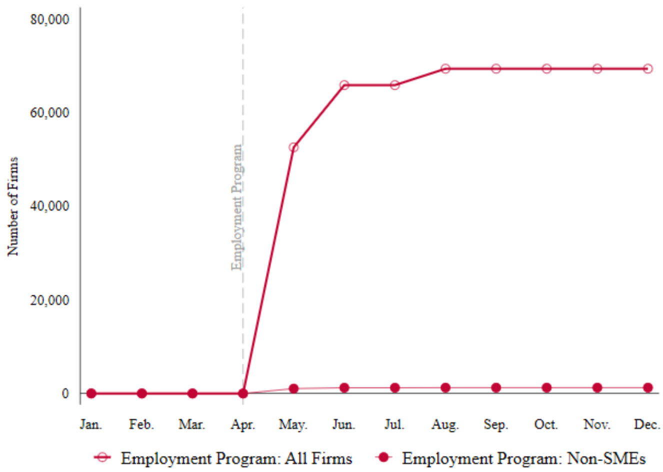
The dashed lines in Appendix Figure 6 show the corresponding patterns under the credit program. Using either the right or left panels, one sees a similar pattern. The interest rates drop across the board, but especially so for firms whose borrowing was high risk in the benchmark scenario. This is the result of the lower borrowing rate, which makes it illegal to charge high risk premia to these firms and so effectively lowers the credit channeled toward the riskier firms. This is the reason that the lower borrowing limit reduces credit and is so effective in limiting default and government risk. However, the interest rate declines for all firms, and this induces borrowing among the safer firms, those with higher equity but also those facing less revenue risk (i.e., the left panels). The combination of the credit guarantee and the fall in intermediation costs, therefore, leads to an increase in credit, but the reduced interest rate limit ensures that credit is channeled to those who are not high risk.

In sum, under both the benchmark and program, very little credit goes to firms with high default probability because they are a small share of those borrowing, and the interest cap further constrains the riskiest borrowers. This result from quantitative theory is consistent with the empirical results (Table 7 and Appendix Table 29).

Appendix Figure 1
Reach of Public Programs, All Firms, Non-SMEs, and SMEs



(A) Number of Firms Using Credit Program

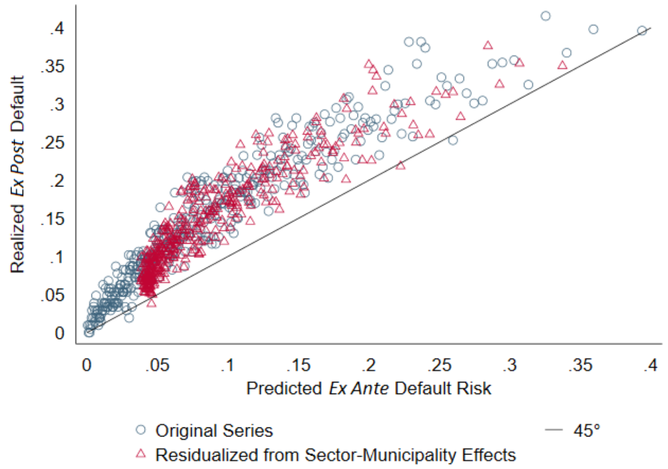


(B) Number of Firms Using Employment Program

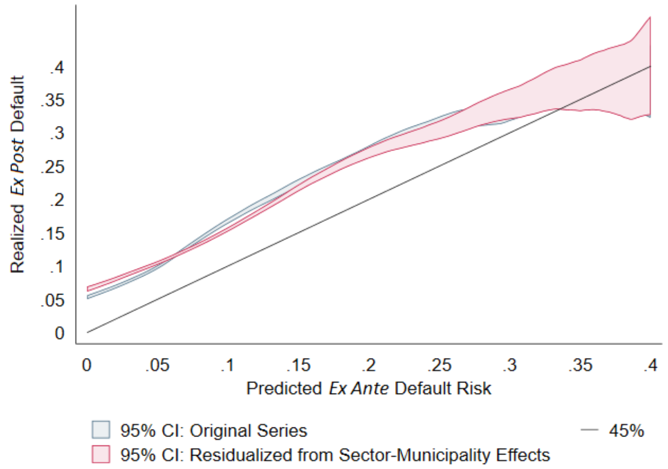
This figure plots the number of firms using the credit and employment program during 2020. Panel A plots the number of firms using the credit program. Panel B displays the number of firms using the employment program. Both panels differentiate between the total number of firms and the number of non-SMEs. Therefore, the vertical distance between pairs of dots represents the number of SMEs. The dashed vertical lines show the month when each program is implemented.

Appendix Figure 2

Correlation between *Ex Ante* Default Risk and *Ex Post* Default using Table 1 Sample of firms



(A) Scatterplot

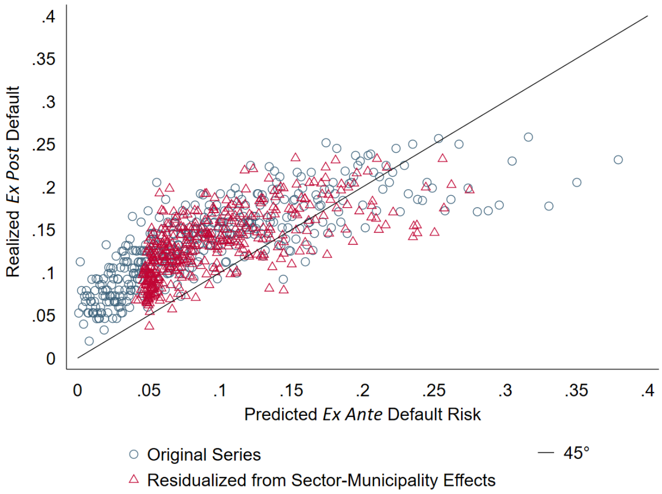


(B) Confidence Interval

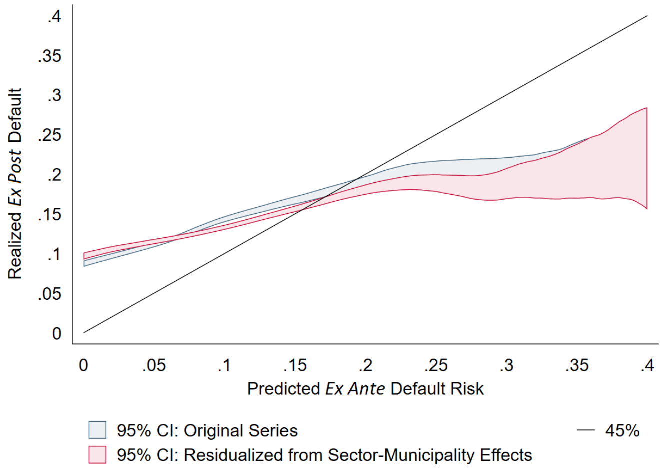
This figure plots the correlation between predicted *ex ante* default risk and the realized *ex post* default for 2020. The figure uses the firms with previous loans on a set of firm-level characteristics, as shown in Table 1 (96,411 firms). The *ex ante* default risk is derived from the model estimates presented in Columns 4 and 8 of Table 1, where firm characteristics from 2019 are used to project probabilities of default in 2020. Although Column 8 is the most complete model of risk, we use Column 4 instead of Column 8 for firms without previous loans. *Ex post* default is a dummy equal to one if the firm has more than 90 past due days after May 2020 and zero otherwise. Panel A shows the simple correlation between the two default measures. Each dot represents a bin average of *ex ante* default risk and *ex post* default. Panel B shows the confidence intervals (CI) of the correlation using a non-parametric adjustment. The original series plots the average of the *ex ante* and *ex post* default pairs within each bin. The residualized from sector-municipality effects plots the same averages after controlling for sector-municipality fixed effects. Both series are split into 400 bins with an equal number of firms. The R-squared between the predicted *ex ante* default risk and the realized *ex post* default is 0.891.

Appendix Figure 3

Correlation between *Ex Ante* Default Risk 2019 and *Ex Post* Default 2019



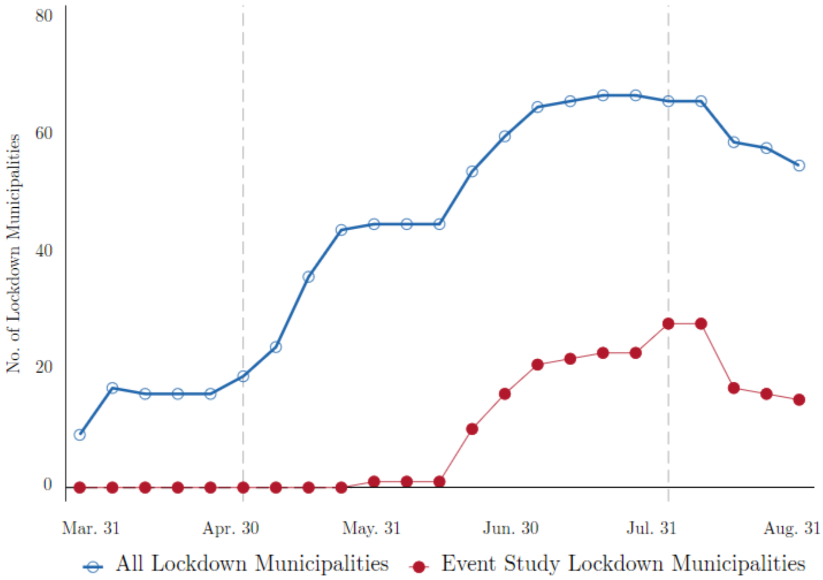
(A) Scatterplot



(B) Confidence Interval

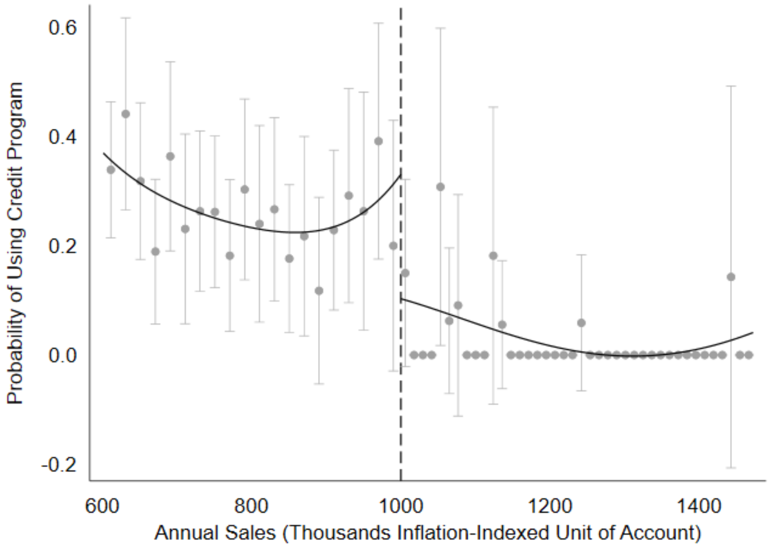
This figure plots the correlation between predicted *ex ante* default risk and the realized *ex post* default for 2019. The figure uses the active firms of the credit program firms sample in 2018–2019 (60,551 firms). The *ex ante* default risk is derived from the similar model estimates presented in Columns 4 and 8 of Table 1, but with firm characteristics from 2017 are used to project default probabilities for 2018. Although Column 8 is the most complete model of risk, we use Column 4 instead of Column 8 for firms without previous loans. *Ex post* default is a dummy equal to one if the firm has more than 90 past due days after 2019 and zero otherwise. Panel A shows the simple correlation between the two default measures. Each dot represents a bin average of *ex ante* default risk and *ex post* default. Panel B shows the confidence intervals (CI) of the correlation using a non-parametric adjustment. The original series plots the average of the *ex ante* and *ex post* default pairs within each bin. The residualized from sector-municipality effects plots the same averages after controlling for sector-municipality fixed effects. Both series are split into 400 bins with an equal number of firms. The R-squared between the predicted *ex ante* default risk and the realized *ex post* default is 0.510.

Appendix Figure 4
Number of Municipalities Subject to Lockdown Mandates Over Time

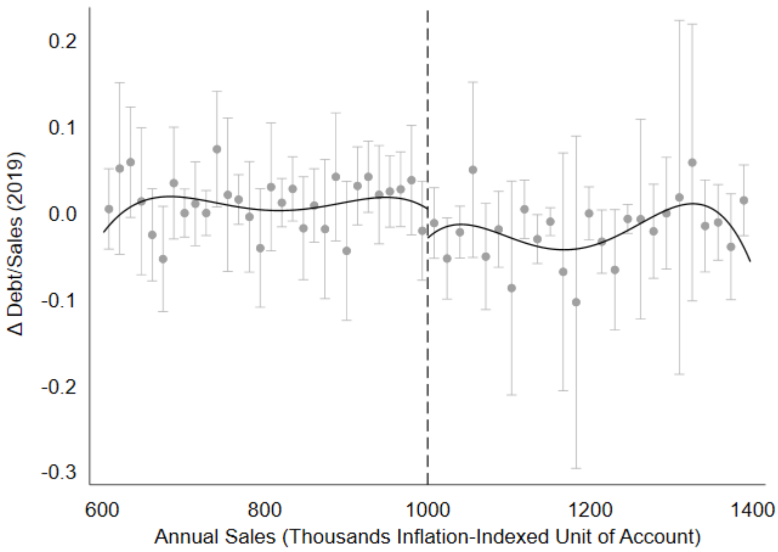


This figure shows the evolution of the number of municipalities subject to lockdown mandates over time for (a) all the municipalities in Chile and (b) all the municipalities included in the dynamic lockdown event study. The dashed vertical lines show, respectively, the starting and ending dates considered in the dynamic lockdown event study. This figure uses publicly available data.

Appendix Figure 5
Consequences of Being Eligible for the Credit Program: Evidence from RDD



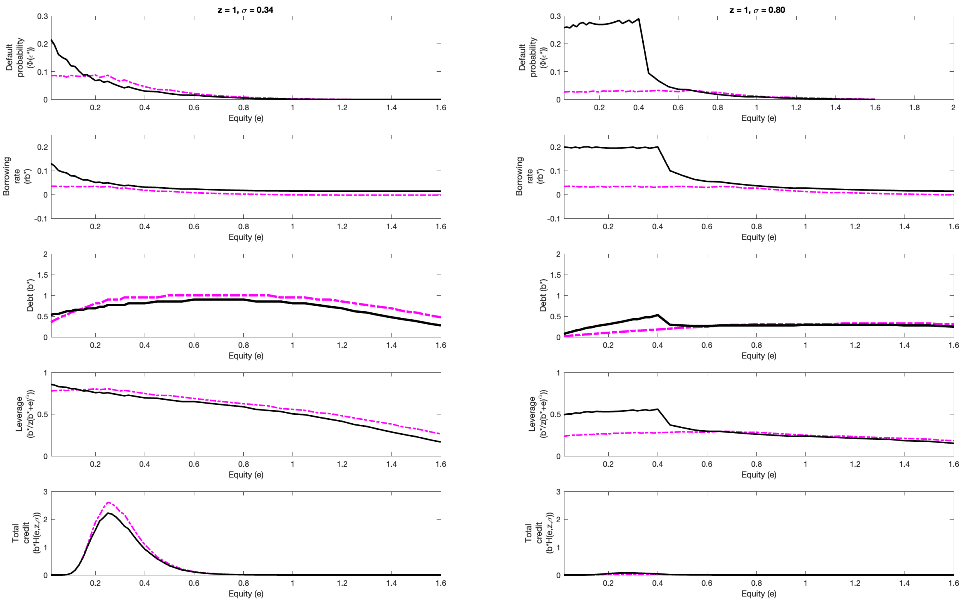
(A) Effects on Program's Take-Up



(B) Effects on Firm Leverage

This figure plots the effects of firm eligibility for the credit program on the probability of using the program (Panel A) and on firm leverage (Panel B). The estimates are obtained from a regression discontinuity design (RDD) around the size eligibility threshold for the program of 1 million in Inflation-Indexed Unit of Account (between October 2018 and September 2019). The point estimate (standard error) of Panel A is -0.14 (0.05), and Panel B is -0.04 (0.02). Leverage is the change in the debt outstanding between December 2020 and December 2019, relative to 2019 sales. The dashed vertical line shows the size eligibility threshold. The figure uses the selection and leverage models sample.

Appendix Figure 6
Simulated Model



This figure shows borrowing outcomes (y-axis) as a function of equity (x-axis) for firms with two different values of uncertainty (or equity-independent risk), σ. Both panels are for a firm with average productivity, $z = 1$, but the panels on the left have a moderate level of uncertainty, $\sigma = 0.34$, while the panels on the right are for a high level of uncertainty, $\sigma = 0.80$. The solid dark line illustrates the results for a firm in the benchmark banking system. The dashed line illustrates the results under the counterfactual policy credit program.

Appendix Table 1
Size and Coverage of Different Samples

	(1) Number of Firms	(2) Share of Total Formal Firms (%)	(3) Share of Employment (%)	(4) Share of Credit Stock (%)	(5) Share of Value Added (%)
Panel A: Universe of Firms					
Formal Firms	602,882	100	100	100	100
Active Firms	449,632	75	92	82	100
Credit Program Eligible Firms	434,411				
Credit Program Users	102,648				
Panel B: Firms with Observables for Firm-Level Estimations					
Default Model	96,411	16	61	51	67
Selection and Leverage Model	119,153	20	50	44	74
Firms With Previous Loans	63,867				
Firms Without Previous Loans	55,286				
Credit Program Eligible Firms	114,606	19	35	21	19
Firms With Previous Loans	59,541				
Firms Without Previous Loans	55,065				
Credit Program Users	40,901	7	14	9	7
Firms With Previous Loans	30,937				
Firms Without Previous Loans	9,964				

This table reports summary statistics of the different samples used in this paper, i.e., formal firms sample, active firms sample, default model sample, and selection and leverage model sample. The sample of active firms corresponds to the set of firms with positive sales during 2019. The default model sample corresponds to the set of firms used to estimate the default model. The selection and leverage model sample corresponds to the set of firms used in the selection and default analysis in this paper. Columns 1 to 5 show, for each sample, respectively, the number of firms with data, the share of firms and aggregate employment they represent in the economy, the share of aggregate bank credit stock they capture, and the share of aggregate value added they generate. Employment and value added are calculated by aggregating data from tax records of all firms in Chile. Firms are classified across size categories based on their annual sales, according to the criteria defined by the tax authority. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. On the other hand, firms without previous loans are those with no credit records in the banking system throughout the same period.

Appendix Table 2

Default Probability Model: Default Probability in 2020 using Firms Characteristics of 2019

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Probit Estimation								
Log(Net Worth)	-0.008	-0.007	-0.007	-0.007	-0.006	-0.006	-0.006	-0.006
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Value Added / Number of Workers)	-0.021	-0.020	-0.019	-0.018	-0.019	-0.019	-0.017	-0.017
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Firm Age	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log(Wage Bill)	-0.008	-0.008	-0.007	-0.007	-0.007	-0.007	-0.006	-0.006
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Annual Sales)	0.001	0.001	-0.003	-0.003	-0.005	-0.005	-0.008	-0.007
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Debt Outstanding)					0.013	0.013	0.013	0.013
					(0.001)	(0.001)	(0.001)	(0.001)
Spread *Ex Ante*					0.003	0.003	0.003	0.003
					(0.000)	(0.000)	(0.000)	(0.000)
Dependent Variable Mean	0.082	0.082	0.082	0.082	0.082	0.082	0.082	0.066
Dependent Variable Std. Dev.	0.274	0.274	0.274	0.274	0.274	0.274	0.274	0.249
Number of firms	99,845	99,845	99,845	99,845	99,845	99,845	99,845	99,845
R^2	0.054	0.064	0.067	0.075	0.098	0.107	0.107	0.115
Industry FE	No	No	Yes	Yes	No	No	Yes	Yes
Municipality FE	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: Predicted Default Probability								
With Previous Loans	0.082	0.082	0.082	0.082	0.083	0.083	0.082	0.082
Without Previous Loans	0.107	0.108	0.101	0.102				

This table reports probit estimations of the probability of a firm with previous loan defaulting on a loan on a set of firm-level characteristics (Panel A) and the resulting predicted default probabilities for firms with and without previous loans (Panel B) for the default model sample. The dependent variable is a dummy equal to one if the firm defaults on a loan during 2020 (has payment past due over 90 days) and zero otherwise. All explanatory variables are calculated as of December 2019. Given that the data on firms' net worth are not available for all firms, all specifications include an unreported dummy variable equal to one if the data for the firm's net worth are missing and zero otherwise. Firms with previous loans are those that either have bank credit outstanding in December 2020 or receive a bank loan over the period 2012-2020. Columns 1–4 include real characteristics, and Columns 5–8 add financial characteristics. Columns 1–8 include different sets of fixed effects (FE). Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Appendix Table 3
Summary Statistics: Firm-Level Characteristics

	(1) Mean	(2) Median	(3) Std. Dev.	(4) Number of Firms
Annual Sales (Million US$)	0.84	0.17	2.63	114,679
With Previous Loans	0.55	1.00	0.50	114,679
Debt Outstanding (Million US$)	0.35	0.02	4.12	59,563
Debt Outstanding/Annual Sales	0.31	0.08	0.93	59,563
Firm Age (Years)	9.81	7.08	7.86	114,679
Net Worth (Million US$)	1.29	0.03	264.23	60,421
Number of Workers	25.85	5.00	176.93	114,679
Sales, Increase	0.32	0.00	0.47	114,679
Sales, Decrease	0.59	1.00	0.49	114,679
Spread *Ex Ante*	0.10	0.09	0.06	38,424
Value Added/Number of Workers	0.03	0.01	0.17	114,679
Wage Bill (Million US$)	0.16	0.03	1.22	114,679

This table reports firm-level summary statistics for the credit program eligible firms sample. Amounts are in million US$ as of December 2019. With previous loans is a dummy variable equal to one if the firm has bank credit outstanding in December 2019 or receives a bank loan over the period 2012–2019 and is zero otherwise. Sales, increase (decrease) dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Spread *ex ante* is calculated as the average spread charged on bank credit obtained by each firm over the period 2012–2019. Predicted default probability corresponds to the fitted values of the regression specification reported in Table 1, Column 8. Observations in the top and bottom 1% are dropped for those variables included in the calculation of ratios.

Appendix Table 4

Default Probability Model: Different Regressors and Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Estimation results								
Log(Net Worth)	−0.010	−0.009	−0.009	−0.007	−0.010	−0.007	−0.010	−0.009
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Value Added/Number of Workers)	−0.018	−0.017	−0.015	−0.014	−0.018	−0.012	−0.017	−0.016
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Firm Age	−0.001	−0.002	−0.001	−0.001	−0.001	−0.003	−0.002	−0.003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log(Wage Bill)	−0.008	−0.007	−0.007	−0.006	−0.008	−0.005	−0.009	−0.008
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Annual Sales)	0.003	−0.003	0.005	−0.003	0.003	−0.000	0.008	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Debt Outstanding)		0.013	0.013	0.012		0.010		0.012
		(0.001)	(0.001)	(0.001)		(0.001)		(0.001)
Spread *Ex Ante*		0.003	0.003	0.003		0.001		0.003
		(0.000)	(0.000)	(0.000)		(0.000)		(0.000)
Spread 2018				0.004				
				(0.000)				
Default Probability						0.189		
						(0.002)		
Sales Variation							−0.040	−0.034
							(0.002)	(0.002)
Dependent Variable Mean	0.088	0.088	0.080	0.080	0.088	0.088	0.090	0.090
Dependent Variable Std. Dev.	0.284	0.284	0.271	0.271	0.284	0.284	0.286	0.286
Number of Firms	96,411	96,411	69,308	69,308	96,328	96,328	92,811	92,811
R^2	0.073	0.111	0.068	0.117	0.073	0.256	0.091	0.123
Industry FE and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Predicted Default Probability								
With Previous Loans	0.088	0.089	0.079	0.079	0.088	0.089	0.090	0.091
Without Previous Loans	0.107		0.091		0.107		0.097	

This table reports probit estimations of the probability of a firm defaulting on a loan on a set of *ex ante* firm-level characteristics for the default model sample. The dependent variable is a dummy variable equal to one if the firm defaults on a loan during 2019 (has payment past due over 90 days) and zero otherwise. Each model is first estimated using real regressors, and then with real and financial regressors. Columns 1 and 2 are also displayed in Table 1, and Columns 4 and 8 are used as a benchmark. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Conversely, firms without previous loans are those with no credit records in the banking system throughout the same period. Spread *ex ante* is calculated as the average spread charged on bank credit obtained by each firm over the period 2012–2018. Spread 2018 is the spread charged on bank credit obtained by each firm during 2018. Columns 1–8 include industry and municipality fixed effects, and a different set of controls. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Appendix Table 5

Default Probability Model: Excluding the Fourth Quarter of 2019

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Probit Estimation								
Log(Net Worth)	-0.009	-0.009	-0.009	-0.009	-0.008	-0.008	-0.008	-0.008
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Value Added / Number of Workers)	-0.017	-0.016	-0.015	-0.015	-0.016	-0.015	-0.014	-0.015
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Firm Age	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log(Wage Bill)	-0.008	-0.008	-0.007	-0.007	-0.007	-0.007	-0.006	-0.006
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Annual Sales)	0.005	0.004	0.001	0.002	-0.001	-0.001	-0.003	-0.004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Debt Outstanding)					0.012	0.012	0.011	0.011
					(0.001)	(0.001)	(0.001)	(0.001)
Spread *Ex Ante*					0.003	0.002	0.002	0.002
					(0.000)	(0.000)	(0.000)	(0.000)
Dependent Variable Mean	0.071	0.071	0.071	0.071	0.071	0.071	0.071	0.071
Dependent Variable Std. Dev.	0.257	0.257	0.257	0.257	0.257	0.257	0.257	0.257
Number of firms	95,852	95,852	95,852	95,852	95,852	95,852	95,852	95,852
R^2	0.051	0.062	0.063	0.073	0.096	0.106	0.105	0.115
Industry FE	No	No	Yes	Yes	No	No	Yes	Yes
Municipality FE	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: Predicted Default Probability								
With Previous Loans	0.071	0.071	0.071	0.071	0.072	0.072	0.072	0.072
Without Previous Loans	0.093	0.093	0.088	0.088				

This table reports probit estimations of the probability of a firm with previous loan defaulting on a loan on a set of firm-level characteristics (Panel A) and the resulting predicted default probabilities for firms with and without previous loans (Panel B) for the default model sample. The dependent variable is a dummy equal to one if the firm defaults on a loan during January and September 2019 (has payment past due over 90 days) and zero otherwise. All explanatory variables are calculated as of December 2018. Given that the data on firms' net worth are not available for all firms, all specifications include an unreported dummy variable equal to one if the data for the firm's net worth are missing and zero otherwise. Firms with previous loans are those that either have bank credit outstanding in September 2019 or receive a bank loan between January 2012 and September 2019. Columns 1–4 include real characteristics, and Columns 5–8 add financial characteristics. Columns 1–8 include different sets of fixed effects (FE). Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Appendix Table 6

Probability of Firms Using Public Programs into Three Groups of Risk

	Credit Program			Employment Program	Both Programs
	(1)	(2)	(3)	(4)	(5)
	Use	Applications	Approvals	Use	Use
Low Risk	1.707	2.194	0.105	0.112	0.476
	(0.274)	(0.271)	(0.194)	(0.202)	(0.172)
Medium Risk	1.830	2.142	-0.311	0.211	0.504
	(0.106)	(0.105)	(0.071)	(0.075)	(0.058)
High Risk	0.540	0.754	-0.252	0.008	0.114
	(0.042)	(0.043)	(0.027)	(0.030)	(0.023)
Increase in Sales Dummy	0.194	0.186	0.014	0.053	0.064
	(0.008)	(0.007)	(0.006)	(0.007)	(0.006)
Decrease in Sales Dummy	0.193	0.189	0.014	0.112	0.102
	(0.008)	(0.007)	(0.006)	(0.007)	(0.006)
Use Employment Program	0.093	0.115	-0.009		
	(0.005)	(0.005)	(0.004)		
Use Credit Program				0.055	
				(0.003)	
Dependent Variable Mean	0.505	0.656	0.913	0.185	0.111
Dependent Variable Std. Dev.	0.500	0.475	0.281	0.389	0.315
Number of Firms	62,894	62,859	36,609	62,128	61,446
R^2	0.049	0.068	0.031	0.081	0.068
Industry FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Predicted Default Probability					
Firms with Previous Loans	0.084	0.084	0.090	0.084	0.084

This table reports probit estimations of the probability of a firm with a previous loan using a government program on a set of firm-level characteristics. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. In Column 1, the dependent variable is equal to one if the firm participates in the credit program and zero otherwise, in Column 2, the dependent variable is equal to one if the firm applies to the program and zero otherwise, in Column 3 the dependent variable is equal to one if the firm's loan application is approved and zero otherwise, in Column 4 is equal to one if the firm participates in the employment program, in Column 5 is equal to one if the firm participates in both programs, and is zero otherwise. Low, medium, and high risk correspond to fitted values of the regression specification reported in Table 1, Column 8, divided into terciles. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use credit program and employment program are dummy variables equal to one for program participation and is zero otherwise. Use both programs is a dummy variable equal to one for credit and employment program participation, and is zero otherwise. The table uses the firms with previous loans of the credit program eligible firms sample. Some observations are dropped when estimating the model with fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Appendix Table 7
Probability of Firms Using Public Programs: Thresholds between 1% and 4%

	Credit Program			Employment Program	Both Programs
	(1)	(2)	(3)	(4)	(5)
	Use	Applications	Approvals	Use	Use
Risk (1%)	0.352	0.556	-0.264	-0.016	0.051
	(0.034)	(0.035)	(0.022)	(0.024)	(0.018)
Risk (2%)	0.343	0.547	-0.264	-0.020	0.047
	(0.034)	(0.035)	(0.022)	(0.024)	(0.018)
Risk (3%)	0.336	0.539	-0.265	-0.022	0.046
	(0.034)	(0.035)	(0.022)	(0.024)	(0.018)
Risk (4%)	0.330	0.533	-0.265	-0.024	0.043
	(0.034)	(0.035)	(0.022)	(0.024)	(0.018)
Increase in Sales Dummy (1%)	0.260	0.244	0.022	0.069	0.085
	(0.009)	(0.008)	(0.007)	(0.008)	(0.007)
Increase in Sales Dummy (2%)	0.195	0.187	0.014	0.053	0.064
	(0.008)	(0.008)	(0.006)	(0.007)	(0.006)
Increase in Sales Dummy (3%)	0.164	0.163	0.009	0.046	0.054
	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
Increase in Sales Dummy (4%)	0.148	0.149	0.007	0.037	0.044
	(0.007)	(0.006)	(0.006)	(0.006)	(0.005)
Decrease in Sales Dummy (1%)	0.255	0.241	0.021	0.141	0.130
	(0.009)	(0.008)	(0.007)	(0.008)	(0.007)
Decrease in Sales Dummy (2%)	0.193	0.190	0.014	0.112	0.102
	(0.010)	(0.008)	(0.008)	(0.009)	(0.009)
Decrease in Sales Dummy (3%)	0.163	0.165	0.009	0.105	0.092
	(0.007)	(0.006)	(0.006)	(0.006)	(0.005)
Decrease in Sales Dummy (4%)	0.147	0.152	0.007	0.097	0.083
	(0.007)	(0.006)	(0.005)	(0.006)	(0.005)
Number of Firms	62,894	62,859	36,609	62,128	61,446
Industry FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes

This table reports probit estimations of the probability of a firm with a previous loan using a government program on a set of firm-level characteristics, and using alternative thresholds for sales growth, defined in parentheses. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. In Column 1, the dependent variable is equal to one if the firm participates in the credit program and zero otherwise, in Column 2, the dependent variable is equal to one if the firm applies to the program and zero otherwise, in Column 3 the dependent variable is equal to one if the firm's loan application is approved and zero otherwise, in Column 4 is equal to one if the firm participates in the employment program, in Column 5 is equal to one if the firm participates in both programs, and is zero otherwise. Risk corresponds to the fitted values of the regression specification reported in Table 1, Column 8. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to the threshold, and zero if sales growth is between the threshold defined. Use credit program and employment program are dummy variables equal to one for program participation and is zero otherwise. Use both programs is a dummy variable equal to one for credit and employment program participation, and is zero otherwise. The table uses the firms with previous loans of the credit program eligible firms sample. Some observations are dropped when estimating the model with fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Appendix Table 8
Firm Indebtedness and Use of Public Programs: Thresholds between 1% and 4%

	Δ Debt / Sales (2019)		Δ Guaranteed Debt / Sales (2019)		Δ Non-Guaranteed Debt / Sales (2019)	
	(1) With Previous Loans	(2) Without Previous Loans	(3) With Previous Loans	(4) Without Previous Loans	(5) With Previous Loans	(6) Without Previous Loans
Use Credit Program (1%)	0.145	0.130	0.139	0.118	0.006	0.012
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Use Credit Program (2%)	0.145	0.130	0.139	0.118	0.006	0.012
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Use Credit Program (3%)	0.145	0.130	0.139	0.118	0.006	0.012
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Use Credit Program (4%)	0.145	0.130	0.139	0.118	0.006	0.012
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Use Employment Program (1%)	0.008	0.002	0.001	0.000	0.007	0.001
	(0.002)	(0.001)	(0.000)	(0.000)	(0.002)	(0.001)
Use Employment Program (2%)	0.008	0.002	0.001	0.000	0.007	0.001
	(0.002)	(0.001)	(0.000)	(0.000)	(0.002)	(0.001)
Use Employment Program (3%)	0.008	0.002	0.001	0.000	0.008	0.001
	(0.002)	(0.001)	(0.000)	(0.000)	(0.002)	(0.001)
Use Employment Program (4%)	0.008	0.002	0.001	0.000	0.008	0.001
	(0.002)	(0.001)	(0.000)	(0.000)	(0.002)	(0.001)
Use Credit Program × Employment Program (1%)	−0.013	−0.016	−0.003	−0.009	−0.009	−0.007
	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.002)
Use Credit Program × Employment Program (2%)	−0.013	−0.016	−0.003	−0.009	−0.010	−0.007
	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.002)
Use Credit Program × Employment Program (3%)	−0.013	−0.016	−0.003	−0.009	−0.010	−0.007
	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.002)
Use Credit Program × Employment Program (4%)	−0.013	−0.016	−0.003	−0.009	−0.010	−0.007
	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.002)
Increase in Sales Dummy (1%)	0.023	0.003	−0.001	0.001	0.024	0.003
	(0.003)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)
Increase in Sales Dummy (2%)	0.021	0.004	−0.001	0.001	0.021	0.003
	(0.003)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)
Increase in Sales Dummy (3%)	0.018	0.003	−0.000	0.001	0.019	0.002
	(0.003)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)
Increase in Sales Dummy (4%)	0.018	0.003	0.001	0.001	0.017	0.002
	(0.002)	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)
Decrease in Sales Dummy (1%)	0.020	0.001	−0.003	−0.000	0.023	0.001
	(0.003)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)
Decrease in Sales Dummy (2%)	0.017	0.002	−0.002	0.000	0.019	0.001
	(0.003)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)
Decrease in Sales Dummy (3%)	0.015	0.001	−0.002	0.000	0.017	0.001
	(0.003)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)
Decrease in Sales Dummy (4%)	0.014	0.001	−0.001	0.000	0.016	0.001
	(0.002)	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)
Number of Firms	62,950	51,729	62,950	51,729	62,950	51,729
Industry FE and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' leverage on a set of firm-level characteristics for the credit program eligible firms sample, and using alternative thresholds for sales growth, defined in parentheses. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Conversely, firms without previous loans are those with no credit records in the banking system throughout the same period. The dependent variable is the change in the debt outstanding between December 2020 and December 2019, relative to 2019 sales. Use credit program and employment program are dummy variables for program participation. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to the threshold, and zero if sales growth is between the threshold defined. All columns include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. For change in debt over 2019 sales and change in guaranteed debt over 2019 sales, observations in the top and bottom 1% of the distribution are winsorized. Change in non-guaranteed debt over sales is constructed as the difference between change in debt outstanding and the change in guaranteed debt, over 2019 sales.

Appendix Table 9
Probability of Firms Using Public Programs: Bootstrapped Standard Errors

	Use Credit Program					Use Employment Program				
	Probit	Linear Probability Model				Probit	Linear Probability Model			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Risk	0.647	0.650	0.540	0.404	0.341	0.084	0.082	0.070	−0.019	−0.024
	(0.042)	(0.043)	(0.034)	(0.033)	(0.035)	(0.025)	(0.025)	(0.023)	(0.023)	(0.024)
Increase in Sales Dummy	0.216	0.211	0.206	0.192	0.189	0.046	0.032	0.035	0.039	0.041
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)
Decrease in Sales Dummy	0.210	0.205	0.199	0.190	0.188	0.119	0.105	0.104	0.099	0.099
	(0.008)	(0.007	(0.007	(0.008)	(0.008)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Use Employment Program	0.098	0.098	0.103	0.089	0.096					
	(0.005)	(0.006	(0.005)	(0.005)	(0.005)					
Use Credit Program						0.059	0.059	0.061	0.053	0.056
						(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Dependent Variable Mean	0.505	0.505	0.505	0.505	0.505	0.182	0.182	0.184	0.183	0.184
Dependent Variable Std. Dev.	0.500	0.500	0.500	0.500	0.500	0.386	0.386	0.387	0.386	0.388
Number of Firms	62,927	62,927	62,918	62,925	62,916	62,927	62,927	62,918	62,925	62,916
R^2										
Industry FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Municipality FE	No	No	Yes	No	Yes	No	No	Yes	No	Yes

This table reports probit and linear estimations of the probability of firms with previous loans using a government program on a set of firm-level characteristics for the credit program eligible firms sample. The dependent variable is equal to one if the firm participates in the credit program (Columns 1–5), is equal to one if the firm participates in the employment program (Columns 6–10), and is zero otherwise. Risk corresponds to the fitted values of the regression specification reported in Table 1, Column 8. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use credit program and employment program are dummies equal to one for program participation. Bootstrapped standard errors, shown in parentheses, are clustered at the industry and municipality levels.

Appendix Table 10
Probability of Firms Using Credit Program: Split by Period

	Applications to Credit Program		
	(1)	(2)	(3)
	April	May	June to December
Panel A: Analysis using Predicted Default Risk			
Risk	0.260	0.129	0.122
	(0.017)	(0.029)	(0.026)
Increase in Sales Dummy	0.041	0.124	0.059
	(0.005)	(0.008)	(0.007)
Decrease in Sales Dummy	0.048	0.132	0.046
	(0.005)	(0.008)	(0.007)
Use Employment Program	0.034	0.066	0.008
	(0.003)	(0.005)	(0.004)
Dependent Variable Mean	0.096	0.324	0.231
Dependent Variable Std. Dev.	0.295	0.468	0.421
Number of Firms	62,018	62,807	62,761
R^2	0.035	0.028	0.018
Industry FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Predicted Default Probability			
Firms with Previous Loans	0.084	0.084	0.084
Panel B: Analysis Using Spread			
Spread *Ex Ante*	0.002	-0.001	0.001
	(0.000)	(0.001)	(0.000)
Increase in Sales Dummy	0.043	0.128	0.057
	(0.007)	(0.011)	(0.009)
Decrease in Sales Dummy	0.053	0.131	0.050
	(0.007)	(0.011)	(0.009)
Use Employment Program	0.037	0.066	-0.009
	(0.004)	(0.006)	(0.006)
Dependent Variable Mean	0.120	0.372	0.233
Dependent Variable Std. Dev.	0.325	0.483	0.423
Number of Firms	37,613	38,213	38,164
R^2	0.036	0.029	0.023
Industry FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Predicted Default Probability			
Firms with Previous Loans	0.106	0.107	0.107

This table reports probit estimations of the probability of a firm with previous loan applying a government program on a set of firm-level characteristics. Panel A defines risk using the predicted default probability from the default probability model reported in Table 1, Column 8; Panel B defines risk using the spread between the interest rate of the loans a firm received and the risk-free rate. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. In Column 1, the dependent variable is equal to one if the firm applies to the program during April and zero otherwise, in Column 2 the dependent variable is equal to one if the firm applies to the program during May and zero otherwise, in Column 3 the dependent variable is equal to one if the firm applies to the program between June and December and zero otherwise. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use credit program and employment program are dummy variables equal to one for program participation and is zero otherwise. Use both programs is a dummy variable equal to one for credit and employment program participation, and is zero otherwise. The table uses the firms with previous loans of the credit program eligible firms sample. Some observations are dropped when estimating the model with fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Appendix Table 11
Probability of Firms Using Credit Program: Heckman Correction

		Heckman Correction	
		1st Stage	2nd Stage
	(1)	(2)	(3)
	Approvals	Applications	Approvals
Panel A: Analysis using Predicted Default Risk			
Risk	-0.264	0.547	-0.123
	(0.022)	(0.035)	(0.066)
Increase in Sales Dummy	0.014	0.187	0.072
	(0.006)	(0.008)	(0.027)
Decrease in Sales Dummy	0.014	0.190	0.073
	(0.006)	(0.007)	(0.027)
Use Employment Program	-0.009	0.117	0.021
	(0.004)	(0.005)	(0.014)
Inverse Mills Ratio			0.186
			(0.083)
Dependent Variable Mean	0.913	0.656	0.913
Dependent Variable Std. Dev.	0.281	0.475	0.281
Number of Firms	36,609	62,859	36,593
R^2	0.030	0.063	0.030
Industry FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Predicted Default Probability			
Firms with Previous Loans	0.090	0.084	0.090
Panel B: Analysis Using Spread			
Spread *Ex Ante*	-0.002	0.002	-0.002
	(0.000)	(0.001)	(0.000)
Increase in Sales Dummy	0.018	0.176	0.051
	(0.008)	(0.009)	(0.033)
Decrease in Sales Dummy	0.014	0.183	0.049
	(0.008)	(0.008)	(0.033)
Use Employment Program	-0.009	0.103	0.006
	(0.004)		
Inverse Mills Ratio			0.108
			(0.101)
Dependent Variable Mean	0.910	0.729	0.911
Dependent Variable Std. Dev.	0.286	0.445	0.285
Number of Firms	24,514	38,250	24,444
R^2	0.032	0.068	0.032
Industry FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Predicted Default Probability			
Firms with Previous Loans	0.107	0.107	0.107

This table reports probit estimations of the probability of a firm with previous loan using a government program on a set of firm-level characteristics, and implementing a two-step Heckman selection correction in the approval estimation. Panel A defines risk using the predicted default probability from the default probability model reported in Table 1, Column 8; Panel B defines risk using the spread between the interest rate of the loans a firm received and the risk-free rate. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. In Column 1, the dependent variable is equal to one if the firm's loan application is approved and zero otherwise, in Column 2, the dependent variable is equal to one if the firm applies to the program and zero otherwise in Column 3 the dependent variable is equal to one if the firm's loan application is approved and zero otherwise. Column 2 is the first-stage, and Column 3 is the second-stage of two-step Heckman selection correction. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use employment program is a dummy variable equal to one for program participation and zero otherwise. The table uses the firms with previous loans of the credit program eligible firms sample. Some observations are dropped when estimating the model with fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Appendix Table 12

Probability of Firms Using Public Programs: Including COVID Risk Variables

	Credit Program	
	(1)	(2)
	Original	COVID Risk Variables
Risk	-0.264	
	(0.022)	
Low COVID Risk × Risk		-1.418
		(0.123)
High COVID Risk × Risk		-1.746
		(0.360)
Increase in Sales Dummy	0.014	0.096
	(0.006)	(0.041)
Decrease in Sales Dummy	0.014	0.091
	(0.006)	(0.041)
Use Employment Program	-0.009	-0.065
	(0.004)	(0.023)
Use Credit Program		
Dependent Variable Mean	0.913	0.915
Dependent Variable Std. Dev.	0.281	0.279
Number of Firms	36,609	37,313
R^2	0.030	0.007
Industry FE	Yes	No
Municipality FE	Yes	No
Predicted Default Probability		
Firms with Previous Loans	0.090	0.090

This table reports probit estimations of the probability of a firm with a previous loan getting approved for the credit program on a set of firm-level and COVID Risk characteristics. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Column 1 and 2 correspond to the original estimation of Table 2, Column 3, and the interactions of COVID Risk Exposure and Municipality Exposure, respectively. Risk corresponds to the fitted values of the regression specification reported in Table 1, Column 8. Low COVID Risk is a dummy variable equal to one if COVID Risk Exposure and Municipality Exposure dummy variables are equal to one and is zero otherwise. High COVID Risk is a dummy variable equal to one if COVID Risk Exposure and Municipality Exposure dummy variables are equal to zero and is zero otherwise. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use credit program and employment program are dummy variables equal to one for program participation and is zero otherwise. Use both programs is a dummy variable equal to one for credit and employment program participation, and is zero otherwise. The table uses the firms with previous loans of the credit program eligible firms sample. Some observations are dropped when estimating the model with fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Appendix Table 13
Probability of Firms of Different Sizes Getting Approval for the Credit Program: Heckman Correction

	Credit Program							
	All Firms		Small Firms		Medium Firms		Large Firms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage
Risk	0.547	−0.123	0.563	−0.082	1.916	−0.352	2.422	−0.507
	(0.035)	(0.066)	(0.038)	(0.077)	(0.161)	(0.206)	(0.324)	(0.690)
Increase in Sales Dummy	0.187	0.072	0.196	0.087	0.124	0.008	0.093	0.022
	(0.008)	(0.027)	(0.008)	(0.031)	(0.020)	(0.026)	(0.034)	(0.043)
Decrease in Sales Dummy	0.190	0.073	0.197	0.085	0.131	0.010	0.102	0.034
	(0.007)	(0.027)	(0.008)	(0.032)	(0.019)	(0.026)	(0.033)	(0.043)
Use Employment Program	0.117	0.021	0.114	0.028	0.081	−0.016	0.178	−0.004
	(0.005)	(0.014)	(0.006)	(0.016)	(0.011)	(0.013)	(0.019)	(0.051)
Inverse Mills Ratio		0.186		0.211		0.031		0.097
		(0.083)		(0.093)		(0.085)		(0.198)
Dependent Variable Mean	0.656	0.913	0.646	0.908	0.728	0.918	0.576	0.900
Dependent Variable Std. Dev.	0.475	0.281	0.478	0.289	0.445	0.275	0.494	0.300
Number of Firms	62,859	36,593	47,784	27,271	10,037	5,944	4,504	1,359
R^2	0.063	0.030	0.063	0.032	0.110	0.070	0.127	0.162
Industry FE and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Predicted Default Probability								
Firms With Previous Loans	0.084	0.090	0.095	0.102	0.055	0.061	0.031	0.036

This table reports probit estimations of the probability of a firm with previous loan getting approved for the credit program on a set of firm-level characteristics for the credit program eligible firms sample, and implementing a two-step Heckman selection correction in the approval estimation. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Columns 1–2, 3–4, 5–6, and 7–8 correspond to all (small, medium, and large) firms, small firms, medium firms, and large firms, respectively. In the first-stage columns, the dependent variable is equal to one if the firm applies to the program and zero otherwise. In the second-stage columns, the dependent variable is equal to one if the firm's loan application is approved and is zero otherwise. Small, medium, and large firms correspond to firms that have annual sales less than US\$0.8 million, between US\$0.8 and US\$3.5 million, and between US\$3.5 and US\$35 million, respectively. Risk corresponds to the fitted values of the regression specification reported in Table 1, Column 8. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use employment program is a dummy variable equal to one for employment program participation and is zero otherwise. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Appendix Table 14
Probability of Firms Using Credit Program: Different Samples

	Use Credit Program			
	(1)	(2)	(3)	(4)
	Only Eligible Firms	Eligible Firms + Firms with Past Due Payment	Eligible Firms + Mega Firms	All Firms
Risk	0.343	0.094	0.419	0.157
	(0.034)	(0.032)	(0.034)	(0.033)
Increase in Sales Dummy	0.195	0.206	0.193	0.210
	(0.008)	(0.008)	(0.008)	(0.008)
Decrease in Sales Dummy	0.193	0.208	0.190	0.211
	(0.008)	(0.008)	(0.008)	(0.008)
Use Employment Program	0.095	0.088	0.098	0.095
	(0.005)	(0.005)	(0.005)	(0.005)
Dependent Variable Mean	0.505	0.478	0.498	0.483
Dependent Variable Std. Dev.	0.500	0.500	0.500	0.500
Number of Firms	62,894	66,430	63,781	67,263
R^2	0.045	0.039	0.048	0.043
Industry FE and Municipality FE	Yes	Yes	Yes	Yes
Predicted Default Probability				
Firms With Previous Loans	0.084	0.087	0.083	0.086

This table reports probit estimations of the probability of a firm with previous loan obtaining a public guaranteed loan on a set of firm-level characteristics for different sub-samples of firms within the selection and leverage models sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. The dependent variable is equal to one if the firm obtains a guaranteed loan. Column 1 includes only firms eligible for the program, Column 2 includes all eligible firms plus firms with debt payments past due (ineligible), Column 3 includes all firm plus the mega firms (ineligible), and Column 4 includes all firms in Columns 1, 2, and 3. Risk corresponds to the fitted values of the regression specification reported in Table 1, Column 8. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use employment program is a dummy equal to one for employment program participation and is zero otherwise. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Appendix Table 15
Probability of Firms Using Public Programs: Including Firm Characteristics

	Credit Program			Employment Program	Both Programs
	(1)	(2)	(3)	(4)	(5)
	Use	Applications	Approvals	Use	Use
Risk	0.810	0.983	-0.154	-0.036	0.185
	(0.044)	(0.045)	(0.031)	(0.031)	(0.024)
Log(Net Worth)	-0.021	-0.023	0.004	-0.008	-0.007
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Log(Value Added/Number of Workers)	-0.001	-0.004	0.007	-0.026	-0.013
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Firm Age	-0.073	-0.071	0.003	0.010	-0.006
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Log(Wage Bill)	-0.002	-0.003	0.004	0.026	0.018
	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)
Log(Annual Sales)	0.070	0.069	-0.003	-0.004	0.009
	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)
Increase in Sales Dummy	0.153	0.145	0.014	0.049	0.053
	(0.008)	(0.007)	(0.006)	(0.007)	(0.006)
Decrease in Sales Dummy	0.153	0.150	0.014	0.108	-0.008
	(0.008)	(0.007)	(0.006)	(0.007)	(0.005)
Use Employment Program	-0.020	-0.024	0.021		
	(0.009)	(0.009)	(0.007)		
Use Credit Program				-0.025	
				(0.006)	
Dependent Variable Mean	0.505	0.656	0.913	0.185	0.111
Dependent Variable Std. Dev.	0.500	0.475	0.281	0.389	0.315
Number of Firms	62,894	62,859	36,609	62,128	61,446
R^2	0.070	0.093	0.032	0.098	0.087
Industry FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes

This table reports probit estimations of the probability of a firm with previous loans using a government program on a set of firm-level characteristics. The dependent variable is equal to one if the firm participates in the credit program (Columns 1–3), is equal to one if the firm participates in the employment program (Column 4), is equal to one if the firm participates in both programs (Column 5), and is zero otherwise. Risk corresponds to the fitted values of the regression specification reported in Table 1, Column 8. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use credit program and employment program are dummy variables equal to one for program participation and is zero otherwise. Use both programs is a dummy variable equal to one for credit and employment program participation, and is zero otherwise. The table uses the firms with previous loans of the credit program eligible firms sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan between 2012 and 2019. Some observations are dropped when estimating the model with fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level.

Appendix Table 16

Probability of Firms Using Public Programs: Dynamic Lockdowns, Covariate Balance Test

	(1) Treatment Municipality	(2) Control Municipality	(3) Diff.	(4) N
Panel A: All Firms in Municipality				
Elegible (Days Past Due)	0.899	0.866	0.033	3,884
	(0.010)	(0.006)	[2.633]	
Elegible (Sales)	0.995	0.993	0.002	3,884
	(0.002)	(0.002)	[0.616]	
Elegible (Days past due and sales)	0.899	0.866	0.033	3,884
	(0.010)	(0.006)	[2.633]	
With Previous Loans	0.898	0.909	-0.011	3,884
	(0.009)	(0.005)	[-1.055]	
Firm Age (Years)	11.334	10.914	0.421	3,883
	(0.245)	(0.133)	[1.525]	
Debt Outstanding (Million US$)	0.199	0.278	-0.080	3,884
	(0.029)	(0.060)	[-0.733]	
Fogape Credit Amount (Million US$)	0.098	0.080	0.018	1,593
	(0.008)	(0.005)	[1.894]	
Proportion of Small firms	0.767	0.799	-0.032	3,884
	(0.014)	(0.007)	[-2.085]	
Proportion of Medium firms	0.164	0.138	0.026	3,884
	(0.012)	(0.006)	[1.962]	
Proportion of Large firms	0.063	0.056	0.008	3,884
	(0.008)	(0.004)	[0.898]	
Panel B: Firms along Municipality Border				
Elegible (Days Past Due)	0.900	0.866	0.034	541
	(0.017)	(0.022)	[1.235]	
Elegible (Sales)	0.997	0.991	0.005	541
	(0.003)	(0.006)	[0.841]	
Elegible (Days Past Due and Sales)	0.900	0.866	0.034	541
	(0.017)	(0.022)	[1.235]	
With Previous Loans	0.899	0.897	0.002	541
	(0.016)	(0.020)	[0.083]	
Firm Age (Years)	11.453	9.787	1.666	541
	(0.419)	(0.420)	[2.751]	
Debt Outstanding (Million US$)	0.179	0.261	-0.082	541
	(0.049)	(0.105)	[-0.772]	
Fogape Credit Amount (Million US$)	0.100	0.053	0.046	237
	(0.013)	(0.009)	[2.686]	
Proportion of Small firms	0.761	0.810	-0.048	541
	(0.024)	(0.026)	[-1.344]	
Proportion of Medium firms	0.181	0.139	0.042	541
	(0.022)	(0.023)	[1.313]	
Proportion of Large firms	0.055	0.043	0.012	541
	(0.013)	(0.013)	[0.609]	

This table reports summary statistics of the treatment and control municipalities. Treatment municipality are firms located in a municipality subject to lockdown, and control municipality otherwise. Amounts are in million US$ as of December 2019. Elegible by days past due is a dummy variable equal to one if the firm are up to date with their debt payments (no more than 30 days past due). Elegible by Sales is a dummy variable equal to one if the firm have annual sales below US $35 millions. Elegible by days past due and sales is the interaction of both variables. With Previous Loans is a dummy variable equal to one if the firm has bank credit outstanding in December 2019 or receives a bank loan over the period 2012–2019 and is zero otherwise. Small, Medium, and Large Firms are dummy variables equal to one if the firm has annual sales less than US$0.8 million, between US$0.8 and US$3.5 million, and between US$3.5 and US$35 million, respectively. Standard errors are shown in parentheses. t – statistic is shown in brackets.

Appendix Table 17

Probability of Firms Using Public Programs: Dynamic Lockdowns, Including Spread

	(1) Use Credit Program	(2) Use Employment Program
Panel A: All Firms in Municipality		
Post × Lockdown × Spread	0.001	0.000
	(0.001)	(0.001)
Post × Lockdown	-0.010	0.029
	(0.009)	(0.007)
Post × Spread	-0.001	0.001
	(0.001)	(0.000)
Lockdown × Spread	-0.002	-0.003
	(0.001)	(0.003)
Post	0.051	-0.031
	(0.009)	(0.001)
Lockdown	0.023	0.041
	(0.017)	(0.031)
Spread	0.000	-0.004
	(0.000)	(0.004)
Number of Observations	28,161	32,409
Number of Firms	3,111	3,581
R^2	0.017	0.025
Region FE and Month FE	Yes	Yes
Panel B: Firms along Municipality Border		
Post × Lockdown × Spread	-0.000	-0.002
	(0.003)	(0.005)
Post × Lockdown	0.015	0.067
	(0.049)	(0.058)
Post × Spread	-0.003	0.002
	(0.002)	(0.002)
Lockdown × Spread	-0.000	0.013
	(0.006)	(0.006)
Post	0.070	-0.019
	(0.027)	(0.023)
Lockdown	0.193	-0.049
	(0.072)	(0.066)
Spread	-0.002	-0.013
	(0.002)	(0.004)
Number of Observations	3,888	3,195
Number of Firms	432	355
R^2	0.040	0.052
Pair of Neighboring Municipalities FE and Month FE	Yes	Yes

This table reports panel linear regressions of the probability of using a government program for a firm located in a municipality that is subject to a lockdown mandate for the selection and leverage models sample. The dependent variables are a dummy variable equal to one if the firm participates in the credit program (Column 1) and a dummy variable equal to one if the firm participates in the employment program (Column 2). Otherwise, the dummy variables are equal to zero. Post is a dummy variable equal to one after a lockdown mandate is implemented in the firm's municipality and is zero otherwise. Lockdown is a dummy equal to one if the firm is located in a municipality subject to a lockdown and is zero otherwise. Spread is calculated as the average spread charged on bank credit obtained by each firm over the period 2012–2019. Panel A includes region and month fixed effects. The analysis in Panel B is restricted to firms located along the border of municipalities with and without lockdown mandates and includes month fixed effects and pair of neighboring municipalities fixed effects. The latter are equal to one for each pair of municipalities that are neighbors (share a border) and zero otherwise. All pairs of municipalities in Chile receive a value. Clustered standard errors at the region level and at pair of neighboring municipalities are shown in parentheses for Panels A and B, respectively.

Appendix Table 18
Firm Indebtedness and Use of Public Programs, over Sales (2020)

	Δ Debt / Sales (2020)		Δ Guaranteed Debt / Sales (2020)		Δ Non-Guaranteed Debt / Sales (2020)	
	(1) With Previous Loans	(2) Without Previous Loans	(3) With Previous Loans	(4) Without Previous Loans	(5) With Previous Loans	(6) Without Previous Loans
Use Credit Program	0.190	0.182	0.170	0.160	0.020	0.022
	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.001)
Use Employment Program	0.016	0.000	−0.003	−0.002	0.019	0.002
	(0.004)	(0.001)	(0.001)	(0.000)	(0.004)	(0.001)
Use Credit Program × Employment Program	0.005	0.001	0.025	0.010	−0.020	−0.010
	(0.006)	(0.006)	(0.002)	(0.004)	(0.005)	(0.003)
Increase in Sales Dummy	−0.008	−0.027	−0.044	−0.017	0.036	−0.010
	(0.007)	(0.003)	(0.003)	(0.002)	(0.007)	(0.002)
Decrease in Sales Dummy	−0.001	−0.020	−0.032	−0.011	0.031	−0.008
	(0.007)	(0.003)	(0.003)	(0.002)	(0.007)	(0.002)
Dependent Variable Mean	0.066	0.040	0.088	0.029	-0.022	0.011
Dependent Variable Std. Dev.	0.270	0.141	0.138	0.094	0.227	0.085
Number of Firms	62,041	50,468	62,041	50,468	62,041	50,468
R^2	0.140	0.258	0.418	0.452	0.016	0.020
Industry FE and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' leverage on a set of firm-level characteristics, separately for firms with and without previous loans, for the credit program eligible firms sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Conversely, firms without previous loans are those with no credit records in the banking system throughout the same period. The dependent variable is the change in the debt outstanding between December 2020 and December 2019, relative to 2020 sales. Use credit program and employment program are dummy variables for program participation. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. All columns include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. For change in debt over 2020 sales and change in guaranteed debt over 2020 sales, observations in the top and bottom 1% of the distribution are winsorized. Change in non-guaranteed debt over sales is constructed as the difference between change in debt outstanding and the change in guaranteed debt, over 2020 sales.

Appendix Table 19

Firm Indebtedness and Risk among Credit Program Users, Over Sales (2020)

	Δ Guaranteed Debt / Sales (2020)		Δ Non-guaranteed Debt / Sales (2020)	
	(1) With Previous Loans	(2) Without Previous Loans	(3) With Previous Loans	(4) Without Previous Loans
Risk	0.143	0.222	-0.083	0.097
	(0.014)	(0.038)	(0.017)	(0.027)
Increase in Sales Dummy	-0.118	-0.138	-0.026	-0.077
	(0.007)	(0.013)	(0.010)	(0.011)
Decrease in Sales Dummy	-0.095	-0.110	-0.028	-0.074
	(0.007)	(0.013)	(0.010)	(0.011)
Use Employment Program	0.018	0.005	-0.003	-0.006
	(0.002)	(0.004)	(0.003)	(0.003)
Dependent Variable Mean	0.172	0.161	-0.012	0.027
Dependent Variable Std. Dev.	0.150	0.165	0.192	0.113
Number of Firms	31,648	9,030	31,648	9,030
R^2	0.092	0.126	0.024	0.083
Industry FE and Municipality FE	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' guaranteed and non-guaranteed debt on a set of firm-level characteristics, separately for firms with and without previous loans, for the credit program users sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Conversely, firms without previous loans are those with no credit records in the banking system throughout the same period. The dependent variable in Columns 1–2 (3–4) is the change in the stock of public guaranteed (non-guaranteed) debt between December 2020 and December 2019, relative to 2020 sales. Risk corresponds to the fitted values of the regression specifications reported in Columns 4 and 8 of Table 1 for firms with and without previous loans, respectively. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use employment program is a dummy variable equal to one for employment program participation and is zero otherwise. All columns include industry and municipality fixed effects. Bootstrapped standard errors, shown in parentheses, are clustered at the industry and municipality level. For change in guaranteed debt over 2020 sales, observations in the top and bottom 1% of the distribution are winsorized. Change in non-guaranteed debt over sales is constructed as the difference between change in debt outstanding and the change in guaranteed debt, over 2020 sales.

Appendix Table 20
Firm Indebtedness and Use of Public Programs, in Levels

	Debt / Sales (2019)		Guaranteed Debt / Sales (2019)		Non-Guaranteed Debt / Sales (2019)	
	(1) With Previous Loans	(2) Without Previous Loans	(3) With Previous Loans	(4) Without Previous Loans	(5) With Previous Loans	(6) Without Previous Loans
Debt (2019) / Sales (2019)	0.913				0.907	
	(0.006)				(0.006)	
Use Credit Program	0.150	0.132	0.139	0.118	0.011	0.014
	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Use Employment Program	0.009	0.001	0.001	0.000	0.008	0.001
	(0.004)	(0.002)	(0.000)	(0.000)	(0.004)	(0.002)
Use Credit Program × Employment Program	−0.014	−0.018	−0.003	−0.009	−0.010	−0.009
	(0.005)	(0.003)	(0.001)	(0.002)	(0.005)	(0.003)
Increase in Sales Dummy	0.008	0.002	−0.001	0.001	0.008	0.001
	(0.006)	(0.003)	(0.001)	(0.000)	(0.006)	(0.003)
Decrease in Sales Dummy	0.005	−0.002	−0.002	0.000	0.006	−0.002
	(0.006)	(0.003)	(0.001)	(0.000)	(0.006)	(0.003)
Dependent Variable Mean	0.318	0.031	0.070	0.020	0.249	0.011
Dependent Variable Std. Dev.	0.652	0.149	0.087	0.055	0.643	0.135
Number of Firms	62,950	51,729	62,950	51,729	62,950	51,729
R^2	0.857	0.118	0.627	0.644	0.861	0.013
Industry FE and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' leverage on a set of firm-level characteristics, separately for firms with and without previous loans, for the credit program eligible firms sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Conversely, firms without previous loans are those with no credit records in the banking system throughout the same period. The dependent variable is the outstanding debt as of December 2020, relative to 2019 sales. Use credit program and employment program are dummy variables for program participation. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Debt (2019) / Sales (2019) coefficients are missing in Columns 3 and 4 because guaranteed debt in 2019 is zero. Debt (2019) / Sales (2019) coefficients are missing for Columns 2 and 6 because, by definition, firms in those groups have no previous loans and thus Debt (2019) is zero. All columns include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. For debt over 2019 sales and guaranteed debt over 2019 sales, observations in the top and bottom 1% of the distribution are winsorized. Non-guaranteed debt over sales is constructed as the difference between debt outstanding and the guaranteed debt, over 2019 sales.

Appendix Table 21
Firm Indebtedness and Use of Public Programs, Including Risk

	Δ Debt / Sales (2019)		Δ Guaranteed Debt / Sales (2019)		Δ Non-Guaranteed Debt / Sales (2019)	
	(1) With Previous Loans	(2) Without Previous Loans	(3) With Previous Loans	(4) Without Previous Loans	(5) With Previous Loans	(6) Without Previous Loans
Use Credit Program × Risk	0.195	0.083	0.067	0.053	0.128	0.030
	(0.017)	(0.021)	(0.006)	(0.015)	(0.016)	(0.013)
Risk	−0.176	0.003	0.014	0.022	−0.190	−0.019
	(0.013)	(0.006)	(0.002)	(0.002)	(0.012)	(0.006)
Use Credit Program	0.129	0.121	0.133	0.113	−0.004	0.009
	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
Use Employment Program	0.009	0.002	0.001	0.000	0.008	0.001
	(0.002)	(0.001)	(0.000)	(0.000)	(0.002)	(0.001)
Use Credit Program × Employment Program	−0.014	−0.016	−0.004	−0.009	−0.010	−0.007
	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.002)
Increase in Sales Dummy	0.019	0.004	−0.000	0.001	0.019	0.002
	(0.003)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)
Decrease in Sales Dummy	0.015	0.002	−0.002	0.000	0.017	0.001
	(0.003)	(0.001)	(0.001)	(0.000)	(0.003)	(0.001)
Dependent Variable Mean	0.054	0.028	0.070	0.020	-0.016	0.008
Dependent Variable Std. Dev.	0.172	0.082	0.087	0.055	0.148	0.054
Number of Firms	62,950	51,729	62,950	51,729	62,950	51,729
R^2	0.193	0.359	0.629	0.645	0.021	0.019
Industry FE and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' leverage on a set of firm-level characteristics, separately for firms with and without previous loans, for the credit program eligible firms sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Conversely, firms without previous loans are those with no credit records in the banking system throughout the same period. The dependent variable is the change in the debt outstanding between December 2020 and December 2019, relative to 2019 sales. Risk corresponds to the fitted values of the regression specifications reported in Columns 4 and 8 of Table 1 for firms without and with previous loans, respectively. Use credit program and employment program are dummy variables for program participation. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. All columns include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. For change in debt over 2019 sales and change in guaranteed debt over 2019 sales, observations in the top and bottom 1% of the distribution are winsorized. Change in non-guaranteed debt over sales is constructed as the difference between change in debt outstanding and the change in guaranteed debt, over 2019 sales.

Appendix Table 22
Firm Indebtedness and Use of Public Programs, Controlling for Credit Demand

	Δ Debt / Sales (2019)		Δ Guaranteed Debt / Sales (2019)		Δ Non-Guaranteed Debt / Sales (2019)	
	(1)	(2)	(3)	(4)	(5)	(6)
Approval to Credit Program	0.041	0.042	0.037	0.037	0.004	0.005
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Applied Amount)	0.011	0.011	0.011	0.011	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Use Employment Program		-0.007		-0.002		-0.005
		(0.002)		(0.001)		(0.001)
Increase in Sales Dummy		0.021		0.011		0.010
		(0.004)		(0.002)		(0.003)
Decrease in Sales Dummy		0.008		0.003		0.005
		(0.004)		(0.002)		(0.003)
Dependent Variable Mean	0.104	0.104	0.100	0.100	0.004	0.004
Dependent Variable Std. Dev.	0.179	0.179	0.098	0.098	0.146	0.146
Number of Firms	78,776	78,776	78,776	78,776	78,776	78,776
R^2	0.040	0.041	0.067	0.069	0.021	0.022
Industry FE and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' leverage on a set of firm-level characteristics, separately for two specifications, for the firms that applied to the credit program. The dependent variable in Columns 1–2 is the change in the debt outstanding between December 2020 and December 2019, relative to 2019 sales. The dependent variable in Columns 3–4 (Columns 5–6) is the change in the stock of public guaranteed (non-guaranteed) debt between December 2020 and December 2019, relative to 2019 sales. Approval to the credit program and use employment program are dummy variables for program approval and participation, respectively. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. All columns include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. For change in debt over 2019 sales and change in guaranteed debt over 2019 sales, observations in the top and bottom 1% of the distribution are winsorized. Change in non-guaranteed debt over sales is constructed as the difference between change in debt outstanding and the change in guaranteed debt, over 2019 sales.

Appendix Table 23
Firm Indebtedness and Use of Public Programs in Levels, Controlling for Credit Demand

	Debt / Sales (2019)		Guaranteed Debt / Sales (2019)		Non-Guaranteed Debt / Sales (2019)	
	(1)	(2)	(3)	(4)	(5)	(6)
Debt (2019) / Sales (2019)	0.929	0.928			0.918	0.917
	(0.006)	(0.006)			(0.005)	(0.005)
Approval to Credit Program	0.036	0.037	0.037	0.037	-0.001	-0.000
	(0.003)	(0.003)	(0.001)	(0.001)	(0.003)	(0.003)
Log(Applied Amount)	0.011	0.012	0.011	0.011	0.001	0.001
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Use Employment Program		-0.012		-0.002		-0.010
		(0.002)		(0.001)		(0.002)
Increase in Sales Dummy		0.017		0.011		0.005
		(0.007)		(0.002)		(0.006)
Decrease in Sales Dummy		-0.007		0.003		-0.011
		(0.006)		(0.002)		(0.006)
Dependent Variable Mean	0.314	0.314	0.100	0.100	0.214	0.214
Dependent Variable Std. Dev.	0.627	0.627	0.098	0.098	0.608	0.608
Number of Firms	78,776	78,776	78,776	78,776	78,776	78,776
R^2	0.758	0.759	0.067	0.069	0.784	0.785
Industry FE and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' leverage on a set of firm-level characteristics, separately for two specifications, for the firms that applied to the credit program. The dependent variable in Columns 1–2 is the outstanding debt as of December 2020, relative to 2019 sales. The dependent variable in Columns 3–4 (Columns 5–6) is the change in the stock of public guaranteed (non-guaranteed) debt between December 2020 and December 2019, relative to 2019 sales. Approval to the credit program and use employment program are dummy variables for program approval and participation, respectively. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Debt (2019) / Sales (2019) coefficients are missing in Columns 3 and 4 because guaranteed debt in 2019 is zero. All columns include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. For debt over 2019 sales and guaranteed debt over 2019 sales, observations in the top and bottom 1% of the distribution are winsorized. Non-guaranteed debt over sales is constructed as the difference between debt outstanding and the guaranteed debt, over 2019 sales.

Appendix Table 24
Firm Indebtedness and Risk among Credit Program Users, in Levels

	Guaranteed Debt / Sales (2019)		Non-guaranteed Debt / Sales (2019)	
	(1) With Previous Loans	(2) Without Previous Loans	(3) With Previous Loans	(4) Without Previous Loans
Debt (2019) / Sales (2019)			0.934	
			(0.005)	
Risk	0.096	0.167	-0.011	0.027
	(0.007)	(0.019)	(0.015)	(0.027)
Increase in Sales Dummy	-0.003	0.010	-0.012	0.015
	(0.002)	(0.004)	(0.008)	(0.005)
Decrease in Sales Dummy	-0.007	0.004	-0.014	0.006
	(0.002)	(0.004)	(0.008)	(0.003)
Use Employment Program	-0.002	-0.007	-0.004	-0.006
	(0.001)	(0.002)	(0.002)	(0.002)
Dependent Variable Mean	0.138	0.116	0.231	0.020
Dependent Variable Std. Dev.	0.076	0.079	0.499	0.129
Number of Firms	31,756	9,068	31,756	9,068
R^2	0.033	0.092	0.872	0.116
Industry FE and Municipality FE	Yes	Yes	Yes	Yes

This table reports linear regressions of the firms' guaranteed and non-guaranteed debt on a set of firm-level characteristics among firms that use the credit program, separately for with and without previous loans, for the credit program users sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Conversely, firms without previous loans are those with no credit records in the banking system throughout the same period. The dependent variable in Columns 1–2 (3–4) is the stock of public guaranteed (non-guaranteed) debt between December 2020 and December 2019, relative to 2019 sales. Risk corresponds to the fitted values of the regression specifications reported in Columns 4 and 8 of Table 1 for firms without and with previous loans, respectively. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Debt (2019) / Sales (2019) coefficients are missing in Columns 1 and 2 because guaranteed debt in 2019 is zero. Debt (2019) / Sales (2019) coefficients is missing for Column 4 because, by definition, firms in this group have no previous loans and thus Debt (2019) is zero. Use employment program is a dummy variable equal to one for employment program participation and is zero otherwise. All columns include industry and municipality fixed effects. Bootstrapped standard errors, shown in parentheses, are clustered at the industry and municipality level. For guaranteed debt over 2019 sales, observations in the top and bottom 1% of the distribution are winsorized. Non-guaranteed debt over sales is constructed as the difference between debt outstanding and the guaranteed debt, over 2019 sales.

Appendix Table 25
Firm Indebtedness and Risk among Credit Program Users: Including Firm Characteristics

	Δ Guaranteed Debt / Sales (2019)		Δ Non-guaranteed Debt / Sales (2019)	
	(1) With Previous Loans	(2) Without Previous Loans	(3) With Previous Loans	(4) Without Previous Loans
Risk	0.037	0.243	-0.144	0.037
	(0.010)	(0.050)	(0.016)	(0.046)
Log(Net Worth)	0.002	0.005	0.000	0.002
	(0.000)	(0.001)	(0.001)	(0.001)
Log(Value Added/Number of Workers)	-0.002	0.004	-0.003	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Firm Age	0.005	0.013	-0.020	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Log(Wage Bill)	0.002	0.006	-0.003	0.000
	(0.000)	(0.001)	(0.001)	(0.001)
Log(Annual Sales)	-0.013	-0.024	0.003	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Increase in Sales Dummy	0.001	0.014	0.004	0.008
	(0.002)	(0.004)	(0.005)	(0.003)
Decrease in Sales Dummy	-0.003	0.008	0.001	0.004
	(0.002)	(0.004)	(0.004)	(0.003)
Use Employment Program	-0.001	-0.004	-0.001	-0.004
	(0.001)	(0.002)	(0.002)	(0.002)
Dependent Variable Mean	0.138	0.116	-0.012	0.016
Dependent Variable Std. Dev.	0.076	0.079	0.135	0.070
Number of Firms	31,756	9,068	31,756	9,068
R^2	0.059	0.154	0.040	0.077
Industry FE and Municipality FE	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' guaranteed and non-guaranteed debt on a set of firm-level characteristics among firms that use the credit program, separately for firms with and without previous loans, for the credit program users sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Conversely, firms without previous loans are those with no credit records in the banking system throughout the same period. The dependent variable in Columns 1–2 (3–4) is the change in the stock of public guaranteed (non-guaranteed) debt between December 2020 and December 2019, relative to 2019 sales. Risk corresponds to the fitted values of the regression specifications reported in Columns 4 and 8 of Table 1 for firms without and with previous loans, respectively. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use employment program is a dummy variable equal to one for employment program participation and is zero otherwise. All columns include industry and municipality fixed effects. Bootstrapped standard errors, shown in parentheses, are clustered at the industry and municipality level. For change in guaranteed debt over 2019 sales, observations in the top and bottom 1% of the distribution are winsorized. Change in non-guaranteed debt over sales is constructed as the difference between change in debt outstanding and the change in guaranteed debt, over 2019 sales.

Appendix Table 26
Firm Indebtedness and Risk among Credit Program Users in Levels: Including Firm Characteristics

	Guaranteed Debt / Sales (2019)		Non-guaranteed Debt / Sales (2019)	
	(1) With Previous Loans	(2) Without Previous Loans	(3) With Previous Loans	(4) Without Previous Loans
Debt (2019) / Sales(2019)			0.937	
			(0.005)	
Risk	0.037	0.243	-0.090	0.092
	(0.010)	(0.050)	(0.023)	(0.069)
Log(Net Worth)	0.002	0.005	0.003	0.004
	(0.000)	(0.001)	(0.001)	(0.001)
Log(Value Added/Number of Workers)	-0.002	0.004	-0.004	-0.000
	(0.001)	(0.001)	(0.001)	(0.003)
Firm Age	0.005	0.013	-0.014	-0.000
	(0.001)	(0.001)	(0.001)	(0.002)
Log(Wage Bill)	0.002	0.006	-0.003	0.002
	(0.000)	(0.001)	(0.001)	(0.002)
Log(Annual Sales)	-0.013	-0.024	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.002)
Increase in Sales Dummy	0.001	0.014	-0.011	0.015
	(0.002)	(0.004)	(0.008)	(0.005)
Decrease in Sales Dummy	-0.003	0.008	-0.013	0.006
	(0.002)	(0.004)	(0.008)	(0.003)
Use Employment Program	-0.001	-0.004	-0.002	-0.006
	(0.001)	(0.002)	(0.002)	(0.003)
Dependent Variable Mean	0.138	0.116	0.231	0.020
Dependent Variable Std. Dev.	0.076	0.079	0.499	0.129
Number of Firms	31,756	9,068	31,756	9,068
R^2	0.059	0.154	0.872	0.116
Industry FE and Municipality FE	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' guaranteed and non-guaranteed debt on a set of firm-level characteristics among firms that use the credit program, separately for firms with and without previous loans, for the credit program users sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Conversely, firms without previous loans are those with no credit records in the banking system throughout the same period. The dependent variable in Columns 1–2 (3–4) is the the stock of public guaranteed (non-guaranteed) debt between December 2020 and December 2019, relative to 2019 sales. Risk corresponds to the fitted values of the regression specifications reported in Columns 4 and 8 of Table 1 for firms without and with previous loans, respectively. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Debt (2019) / Sales (2019) coefficients are missing in Columns 1 and 2 because guaranteed debt in 2019 is zero. Debt (2019) / Sales (2019) coefficients is missing for Column 4 because, by definition, firms in this group have no previous loans and thus Debt (2019) is zero. Use employment program is a dummy variable equal to one for employment program participation and is zero otherwise. All columns include industry and municipality fixed effects. Bootstrapped standard errors, shown in parentheses, are clustered at the industry and municipality level. For guaranteed debt over 2019 sales, observations in the top and bottom 1% of the distribution are winsorized. Non-guaranteed debt over sales is constructed as the difference between debt outstanding and the guaranteed debt, over 2019 sales.

Appendix Table 27

Firm Indebtedness and Risk among Credit Program Users into Three Groups of Risk

	Δ Guaranteed Debt / Sales (2019)		Δ Non-Guaranteed Debt / Sales (2019)	
	(1) With Previous Loans	(2) Without Previous Loans	(3) With Previous Loans	(4) Without Previous Loans
Low Risk	0.019	0.251	0.120	−0.041
	(0.057)	(0.127)	(0.104)	(0.124)
Medium Risk	0.138	0.205	−0.068	0.004
	(0.022)	(0.049)	(0.039)	(0.043)
High Risk	0.097	0.177	−0.043	0.016
	(0.009)	(0.023)	(0.015)	(0.021)
Increase in Sales Dummy	−0.003	0.011	0.004	0.009
	(0.002)	(0.004)	(0.005)	(0.003)
Decrease in Sales Dummy	−0.007	0.004	0.002	0.004
	(0.002)	(0.004)	(0.004)	(0.003)
Use Employment Program	−0.002	−0.007	−0.003	−0.004
	(0.001)	(0.002)	(0.002)	(0.002)
Dependent Variable Mean	0.138	0.116	-0.012	0.016
Dependent Variable Std. Dev.	0.076	0.079	0.135	0.071
Number of Firms	31,792	9,127	31,792	9,127
R^2	0.034	0.092	0.028	0.077
Industry FE and Municipality FE	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' guaranteed and non-guaranteed debt on a set of firm-level characteristics among firms that use the credit program, separately for with and without previous loans, for the credit program users sample. Firms with previous loans are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. The dependent variable in Columns 1–2 (3–4) is the change in the stock of public guaranteed (non-guaranteed) debt between December 2020 and December 2019, relative to 2019 sales. Low, medium, and high risk correspond to fitted values of the regression specification reported in Table 1, Column 4 and 8 for firms with and without previous loans respectively, divided into terciles. Increase (decrease) in sales dummy is equal to one if sales growth is greater (lower) than or equal to 2% (-2%), and zero if sales growth is between -2% and 2%. Use employment program is a dummy variable equal to one for employment program participation and is zero otherwise. All columns include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. For change in guaranteed debt over 2019 sales, observations in the top and bottom 1% of the distribution are Winsorized. Change in non-guaranteed debt over sales is constructed as the difference between change in debt outstanding and the change in guaranteed debt, over 2019 sales.

Appendix Table 28
Expected Loss for Banks and the Government with Zero Deductible

	(1) Expected Loss/GDP (%)	(2) Guarantee (%)	(3) Default Probability (%)	(4) Deductible (%)	(5) Effective Guarantee (=((3)-(4)) x(2)/(3)) (%)	(6) Government Expected Loss/GDP (=(1)x(5)) (%)	(7) Banks Expected Loss/GDP (=(1) x(100-(5))) (%)	(8) Banks Expected Loss/Bank Capital (%)
Panel A: Risk Groups, Formal Firms								
High Risk	0.04	82.5	18.2	0.0	82.5	0.03	0.01	0.11
Medium Risk	0.04	79.9	9.9	0.0	79.9	0.03	0.01	0.15
Medium-Low Risk	0.04	77.0	5.7	0.0	77.0	0.03	0.01	0.20
Low Risk	0.03	72.1	2.1	0.0	0.0	0.00	0.03	0.22
No Risk Data	0.10	82.5	18.2	0.0	82.5	0.08	0.02	0.26
Total: Formal Firms	0.27	76.4	7.4	0.0	76.4	0.21	0.06	1.07
Panel B: Formal Firms + Natural Persons								
Formal Firms	0.27	76.4	7.4	0.0	76.4	0.21	0.06	1.07
Natural Persons	0.19	82.5	18.2	0.0	82.5	0.16	0.03	0.47
Total: Formal Firms + Natural Persons	0.45	77.8	9.8	0.0	77.8	0.35	0.10	1.54

This table shows the distribution of the aggregate expected loss borne by the government and the banking system as a result of the credit program, for natural persons and the formal firms sample when the deductible is zero. Panel A reports statistics across the firms' risk distribution. Panel B reports statistics separately for formal firms and natural persons. Column 1 shows the total expected loss as a share of GDP. Columns 2–4 show the guarantee, the default probability of each category using the model in Table 1, and the first-loss deductible for each category (fixed to zero), while Column 5 shows the effective guarantee, estimated as Guarantee×(Default Probability − Deductible)/Default Probability, directly by category. Columns 6 and 7 show, for each category, the fraction borne by the government estimated as Expected Loss/GDP×(1−Effective Guarantee), and the fraction borne by the banking sector, estimated as (Default Probability−Deductible)× Guarantee)/Default Probability, respectively. Column 8 normalizes Column 7 by the effective capital of the banking system. Values in Columns 2–4 are weighted by the total amount of guaranteed credit granted to each firm as a share of the total guaranteed credit granted to all the firms within its category. Totals by panel reported in Columns 2–5 are calculated as the sum of the product of each category's statistic by its relative weight (Column 3 of Table 7). Firms are classified across risk categories based on the distribution quartiles of the fitted values of the regression specifications reported in Table 1, Columns 4 and 8. Firms with a missing risk category are assigned the risk from the high-risk category.

Appendix Table 29
Changes in Aggregate Firm Indebtedness

	Δ Debt / Sales (2019) (1) Within Change (p.p.)	(2) Weights (%)	Δ Debt / Sales (2019) (3) Group Change (= (1) × (2)) (p.p.)
Panel A: Risk Groups (Credit Program Users)			
High Risk	11.58	6.1	0.71
Medium Risk	9.89	14.1	1.39
Medium-Low Risk	9.57	24.5	2.35
Low Risk	8.84	35.6	3.15
No Risk Data	10.75	19.7	2.12
Total		100.0	9.71
Panel B: Risk Groups (Active Firms)			
High Risk	3.95	1.6	0.06
Medium Risk	4.19	3.8	0.16
Medium-Low Risk	2.61	8.5	0.22
Low Risk	−0.23	59.6	−0.14
No Risk Data	0.48	26.4	0.13
Total		100.0	0.44

This table shows the contribution of different groups of firms to the aggregate change in firm indebtedness for the credit program users (within the active firms sample) (Panel A) and the active firms sample (Panel B). Change in firm indebtedness is measured as the difference in the stock of credit between December 2020 and December 2019, relative to 2019 sales. Panel A divides firms according to their level of risk among credit program users. Panel B divides firms according to their level of risk, considering all active firms (including users and non-users). High, medium, medium-low, and low risk groups are all equally sized and constructed by using the fitted values of the regression specifications reported in Table 1, Columns 4 and 8. Firms with no available data on their risk-fitted values are included in the residual group (no risk data). Column 1 shows the change in percentage points within each group. Column 2 shows the share of sales that each group category accounts for in the different samples. Column 3 is the product of Columns 1 and 2.