Crisis Credit, Employment Protection, Indebtedness, and Risk*

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Abstract

This paper studies how credit guarantee and employment protection programs interact in assisting firms during crises times. The paper analyzes how these government programs influence credit allocation, indebtedness, and risk at both the micro and macro levels. The programs provide different incentives for firms. The low interest rate encourages riskier firms to demand government-backed credit, while banks tend to reject those credit applications. The credit demand outweighs this screening supply response, expanding micro-level indebtedness across the extensive and intensive margins among riskier firms. The uptake of the employment program is not associated with risk, as firms internalize the opportunity cost of reduced operations when sending workers home to qualify for assistance. The employment program mitigates the indebtedness expansion of the credit program by supporting firms and enabling banks to screen firms better. Macroeconomic risk of the credit program would increase by a third without the availability of the employment program.

Keywords: banking, credit demand, credit supply, crises, COVID-19, debt, employment protection, firm risk, macroeconomic risk, public credit guarantees

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1 Introduction

During economic crises, governments often try to help struggling firms 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 tradeoff 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 might increase the overall indebtedness of the private sector, 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 by 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 different exposure to the pandemic, including exposure to the exogenous implementation of mandatory lockdowns over time in different 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). We study how the different conditions of both programs 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 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 because of the pandemic for 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 approvals and rejections from banks. Further, we complement this data with information about firms' use of the employment program. We match the

[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.

financial data with administrative tax data for the universe of formal 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 that enter a lockdown are more likely to use the employment program, whereas a lockdown seems irrelevant for the credit program. Overall, this evidence suggests that firms internalize the opportunity cost of using the employment program that covers the salaries of employees who cannot work, either because of a drop in sales or restricted local mobility. Sending the employees home imposes a cost on the firm because it has to reduce its operations. In contrast, the credit program has no opportunity cost, leading to a much broader adoption. The 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 have less need for credit when they receive employment benefits. In addition, using the employment program might signal a firm in distress or a firm that has to scale down its operation. As a result, the availability of the employment protection program increases the quality of borrowers as it helps 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 credit allocation 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, particularly for riskier firms). On the demand side, riskier firms are more likely to apply for guaranteed loans. On the supply side, riskier firms are less likely to be approved, indicating that the actual allocation of guaranteed loans to riskier firms is mitigated by bank screening. Banks are more sensitive to risk when deciding about 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 is 0.45% of GDP. This aggregate risk level does not increase much if we use *ex post* default rates instead of *ex ante* expected default. Aggregate risk could have been much more significant if the program offered different conditions and incentives, as we show in our counterfactual analyses.

To perform the empirical counterfactual exercises, we perform a series of sensitivity analyses that yield nine alternative scenarios. In each, we modify different dimensions of the equilibrium allocation and policy to recalculate how credit to firms and default generate different expected losses over GDP (the product of credit to GDP and the average expected default probability). We find that factors related to the equilibrium conditions are important but relatively less relevant for aggregate risk than those associated with the policy design. For example, increasing the expected default rates of safer groups of firms or reallocating the credit granted toward firms in the riskiest category raises the expected loss from 0.27% to up to 0.66% of GDP. One would need to change the policy design substantially to obtain an aggregate risk of the credit program higher than 1% of GDP. For example, reducing the incentives for banks to screen firms and relaxing the lending cap raises the overall volume of loans and the allocation toward riskier firms, pushing the expected loss from 0.27% to 1.23% of GDP.

The aggregate expected loss calculations include the employment program's impact on borrower quality and credit quantity. In the last counterfactual exercises, we calculate the aggregate expected loss in the absence of the employment program. To isolate its effects, we proceed sequentially. First, we study the impact on the average default probability, keeping credit constant. Without the employment program, borrower quality declines, raising the baseline default probability to 8.5%. Second, we estimate the increase in credit needs without the employment program, which increases by 17% from the baseline to 4.2% of GDP. Overall, the combined effects on the default probability and credit lead to a higher aggregate expected loss of 0.36% of GDP, a third larger than the baseline scenario.

The lessons from this paper appear important 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. Even when their debt increases the least in proportional terms, their large *ex ante* sales volume makes them large recipients of new loans. Furthermore, *ex ante* and *ex post* default risk are low in general, 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 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 matching the use of a credit program with the use of a concurrent employment program to study how these programs interact. We analyze firms' applications to both the public credit guarantee and employment programs, 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 for 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

[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., 2018, 2021; Bennedsen et al., 2020; Kopp and Siegenthaler, 2021; Giupponi and Landais, 2022).

[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 et al., 2020; Balyuk et al., 2021; Chodorow-Reich et al., 2021; Duchin et al., 2021; Li and Strahan, 2021).

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., 2023). 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 respond when facing credit demand from various firms.

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., 2022; 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 the ones implemented worldwide but with unique data on the universe of lenders and firms. To the best of our knowledge, we are 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 programs to help firms and avoid inefficient bankruptcies. It first significantly expands the size and scope of an existing public credit guarantee program, providing financing to firms during the pandemic and sharing the firm credit risk with banks. The previous 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 up to three months of sales.[9] Guaranteed loans are term loans, not lines of credit, with a six-month grace period and are payable in

[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).

installments during the following 24 to 48 months.[10] They have a low interest rate cap of 3.5%, equal to the monetary policy rate (0.5%) plus the inflation target (3%).[11] The 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, the bank must postpone the repayments of the outstanding debt the firm has with the bank for six months: the firm stops making monthly installment payments on existing debt for this period and cannot pay off the principal of the existing debt. 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 so that banks retain some "skin in the game", and thus, it provides 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 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, enabling firms to cover salaries and maintain firms' contracts with their workers while the employees are not working. Like the credit program, the employment program expands on an existing program, a mandatory unemployment insurance program funded by three sources: 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

[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. In practice, 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.

[14]As a result, for relatively high (low) default rates of the loan portfolio, the government (banks) 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.

the solidarity component of the unemployment insurance fund.[16] Firms can either apply for total employment protection or partial protection.[17] Under partial protection, 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]

The only requirement for the employment program is a voluntary mutual agreement between the firm and the employee to freeze the employment contract. Conditional on this agreement, all applications to the employment program are approved. A key difference between both programs is the cost for firms. The employment program is much more expensive. It does not have a direct cost, but it has an important opportunity cost: the firm has to temporarily reduce its operation in proportion to the number of workers under the program, losing all profits associated with that reduced operation. The credit program, on the other hand, has a low direct cost (very low interest rate) and a very low opportunity cost.

In terms of reach, the credit program in Chile is fast and sizable. Banks provide the majority of the guaranteed loans in the first two months, more than US$8 billion or 3.3% of GDP (Figure 1, Panel A). By the end of the year, the program's size reaches US$11.5 billion (4.6% of 2019 GDP). This is large compared to a total credit expansion of 4.7% of GDP during 2019 and a collapse during past crises. The credit program counteracts the 2020 contraction of net credit granted outside its purview (when GDP suffers a 5.8% negative shock). The size of the employment program (calculated by summing the wage bill savings from workers under protection across all participating firms) is much smaller than the credit program. By December 2020, the employment program amounts to 0.62% of GDP, meaning that the credit program is seven times larger in terms of funds allocated than the employment program.

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

[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]Sole proprietors cannot pay themselves through the employment protection program. There are no other programs specifically targeted to sole proprietors in Chile during the pandemic.

[18]The partial shutdown is rarely used (5.5% of all employment shutdowns).

[19]The 23% take-up is based on the active firms sample. The take-up is higher (36%=40,901/114,606) when 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.

eligible firms 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 firms, more than 15% (=69,280/449,632) of active firms by December 2020. Around 31% (=140,374/449,632) of active firms participate in either program, and almost 7% (=31,554/449,632) of active firms participate in both programs. In terms of the number of firms covered, both programs are roughly similar, so the difference in the amounts of the two programs comes from the significant increase in credit given to firms.

Guaranteed loans overtake overall credit during 2020. Until the credit program starts, total credit is essentially equal to non-guaranteed credit (Figure 2, Panel A). After the credit program begins, cumulative non-guaranteed credit starts decreasing 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 grows fast during the initial two months of the pandemic (Figure 2, Panel B). But three months after the pandemic starts, the loan growth rate to mega firms starts decreasing, and credit instead flows 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 the public credit guarantee program in Chile is negligible.

As a comparison, the outstanding total corporate debt increases by US$4 billion between January and December of 2020 (Figure 2, Panel C). This 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 increase in total debt of US$4 billion is significantly smaller than the program credit because many mega firms (non-program users) do not choose to or do not have the option to roll over their existing debt that matures during 2020, which leads to a decrease 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, plus the industry and location of firms.

[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.

[22] In Appendix Figure 1, we plot the evolution of the number of firms that use each program. We also distinguish between the number of firms that are SMEs versus non-SMEs. For 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 between both programs.

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. These data are published monthly by the employment authority (Dirección del Trabajo) and contain the dates that each firm uses the employment program and the number of workers in each firm that participates in the program.

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 did neither involve nor compromise 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:

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

$Default_i$ 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).[25] 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 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.[26] 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

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

[26] Descriptive statistics of the variables used for this model are presented in Appendix Table 2.

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.[27]

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.[28] 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.

The model does a good job of predicting *ex post* risk. Figure 3, Panel A, presents the binscatter plot between predicted *ex ante* and realized *ex post* default risk (for the period after 2020).[29] There is a strong positive correlation, with *ex ante* default risk explaining 64% of the variance of *ex post* default risk. 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, the *ex post* risk is higher, whereas for values in the upper tail of the distribution, the *ex post* risk is lower. Our risk model appears to be a mean-preserving spread of the *ex post* risk, implying that *ex post* risk is less heterogeneous across firms than the prediction of our risk model.[30]

That said, banks face significant uncertainty at the beginning of 2020 about the intensity

[27] 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 3.

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

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

[30] 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.

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 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:

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

$Program\,Use_i$ 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. The variable risk 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. First, the "increase in sales" dummy 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 "decrease in sales" dummy is equal to one if sales growth is lower or equal than -2% (and zero if sales growth is greater than -2%).[31]

$Other\,Program\,Use_i$ 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

[31] Firms with sales growth between -2% and 2% capture 7% of the data.

using instead a logit model or a linear probability model.[32] 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.[33] Among firms operating in the same industry and located in the same municipality, riskier firms are more likely to obtain a guaranteed loan (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.[34] 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).[35] Riskier firms are more likely to apply for a guaranteed loan (Column 2). 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).[36] 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

[32]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 4. 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.

[33]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.

[34]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.

[35]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.

[36]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.

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 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.

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

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.[37] Instead, the employment program is more expensive as firms must 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.[38] Panel B reports the results. The main results remain unchanged.[39]

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.[40] 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

[37] 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 by risk and size, the effect of risk remains significant. See Appendix Table 5 for details and further results.

[38] 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 and we need to rely on past spreads. For this reason, the spread risk measure can be subject to similar limitations than the probit risk measure.

[39] In Appendix Table 6, 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.

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

status and illustrates substantial geographical variation.[41] To estimate selection into public policies, we run the following difference-in-differences regression:

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

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

Table 4, Panel A reports the results. 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. 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. 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).

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:

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

[41] Appendix Figure 2 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.

$\Delta Debt_i$ 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.[42] 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. Firms with and without previous loans that use the credit program increase their indebtedness by 14.5 and 13.0 percentage points, respectively, relative to non-participating firms (Columns 1 and 2).[43] 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). 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. Namely, 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 or a slowdown in repayment due to the six-month grace period established by the credit program. These results suggest that the credit program and regular credit act as complements rather than substitutes.

As an alternative test, we regress firm indebtedness on a dummy equal to one if the

[42] 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 7 and Appendix Table 8). Appendix Table 9 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.

[43] 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 have a previous loan. 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.

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 10).[44]

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 3. 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.[45]

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.[46]

In contrast, the relationship between risk and non-guaranteed debt is negative, 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 complementary in the extensive margin but substitutable in the intensive margin. The results

[44] Appendix Table 11 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.

[45] 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.

[46] Appendix Table 12 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.

suggest that the existence of a public credit guarantee program changes the way banks allocate credit across the risk distribution of firms.[47]

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.

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 loss and aggregate *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 program and employment program 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.[48] 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 it is distributed, we first estimate the default probability for 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

[47] In Appendix Table 13, 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 14).

[48] 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.

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).[49]

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).[50] 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, as we do, 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 should exceed 6% of the program's funds.

Second, the aggregate expected loss and *ex post* default are significantly determined

[49] 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.

[50] 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 repayment). The expected loss would be lower if we considered a recovery value.

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).[51] 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 from default is absorbed by the 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.

[51]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.

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 banks' equity capital, while for formal firms and natural persons, it represents 1.54% of 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 Equilibrium Allocation and Policy Affect Aggregate Risk

To understand how the equilibrium allocation and policy features influence aggregate risk, we perform a series of sensitivity analyses yielding nine empirical counterfactual scenarios. In each, we modify different dimensions of the equilibrium and policy and recalculate the expected loss over GDP derived from credit to formal firms (excluding natural persons).[52] The scenarios are computed sequentially, but the different dimensions could interact in practice. 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 not only partial equilibrium exercises, but exercises with limited firm responsivity.

The expected loss to GDP can change because of a change in credit (given the average default probability) or a change in the average default probability (given the credit granted). Hence, we disentangle the forces driving the expected loss. Specifically, we decompose the expected loss to GDP as the product of credit to GDP and the average default probability. The latter is computed by weighting the default probability of each group of firms with their corresponding credit amount in each scenario.

Table 9 presents the results. Columns 1 and 2 report the credit to GDP and the average default probability, respectively. Column 3 reports the expected loss (the product of the values in Columns 1 and 2). Each row represents a different counterfactual scenario. For ease of exposition, line 1 reproduces the baseline scenario originally reported in Table 7, Panel A.

We begin with two mitigating factors related to the equilibrium outcome. In the first alternative scenario, we compute aggregate risk with a higher average default rate. We increase default rates for all risk groups by five percentage points, equivalent to the increase in the average default rate experienced in Chile during the 2008 global financial crisis. Under this alternative scenario, aggregate risk is 0.45%, compared to a baseline for formal firm loans of 0.27%.

As a second scenario, we compute aggregate risk such that all the program credit to formal firms (3.6% of GDP) is allocated to the riskiest group of firms. Under this alternative scenario, the aggregate risk jumps to 0.66%. In these two scenarios, the increase in aggregate

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

expected loss is driven entirely by a compositional effect: total credit (Column 1) remains unchanged, but the average default probability (Column 2) increases because a higher fraction of credit is allocated to riskier firms.

We then explore the role of four mitigating factors driven by the credit policy design. As a third alternative scenario, we explore the case in which there is a complete guarantee of the loans and no deductible for banks. By relaxing these restrictions, banks have the incentive to approve all credit applications. As banks play no screening role, we assume that all the funds are allocated based on demand. We consider that this counterfactual can affect two margins. The intensive margin of firms that already participate in the credit program and the extensive margin of firms that do not participate but apply for program credit.

For both margins, we use the observed credit application amount to measure how much firms would demand in this scenario.[53] Credit increases from 3.6% to 8.3% of GDP, whereas the average default probability does not change meaningfully (only up to the second decimal). Firms that apply for credit but are not approved, those affected by the extensive margin of this counterfactual, are relatively riskier, with an *ex ante* default probability of 10.9% compared to the baseline of 7.4%. Nevertheless, they weigh significantly less than firms affected by the intensive margin (the weight is 0.8% for firms affected by the extensive margin versus 7.5% for firms affected by the intensive margin). Thus, the weighted average default probability does not change meaningfully. Combining the effects on credit and default, we find that aggregate risk increases from the baseline of 0.27% to 0.62% in this third alternative scenario.

In the fourth alternative scenario, we increase the loan cap of the credit program from three to six months of sales. Given that most firms are at the three-month cap, we assume that credit increases to the counterfactual six-month cap for all firms. Credit to GDP increases to 15.1%, while the default probability declines 0.4 percentage points to 7.0%.[54] The increase in credit dominates the reduction in the default probability, thereby increasing the aggregate risk to 1.06% of GDP.

In the fifth alternative scenario, we combine the two previous scenarios: the bank approval rate is equal to 100% of credit demand, and the loan cap is increased from three

[53]We omit the demand of firms that do not originally apply to the credit program because we do not have clear guidance from the data to shed light on this margin.

[54]The default probability slightly declines because the baseline scenario uses the observed credit demand to compute the weights of the average default probability, which is slightly smaller than the three-month cap. Thus, the weights used in computing the average default probability in this alternative scenario change. The result that the default probability declines suggests that the firms that increase their weights more (those that originally demand less than the three-month cap) are relatively safer.

to six months of sales. In this case, aggregate risk increases are driven by the rise in credit to GDP. The average default probability does not change significantly. Furthermore, the aggregate risk increases the most in this scenario, around 4.5 times, from 0.27% in the baseline scenario to 1.23%.

Next, we implement a case in which there is no eligibility constraint for firms with high default, i.e., firms with past due payments exceeding 30 days are now eligible. We approximate their potential demand as three months of sales, assuming these firms would demand the maximum possible credit they could obtain because they need funding. The credit allocation increases to 7.6% of GDP and the average default rate increases to 9.2% because these firms are relatively riskier. Under this scenario, we find that aggregate expected loss increases from 0.27% to 0.7% of GDP.

The alternative scenarios above suggest that the factors related to the equilibrium conditions have less impact on aggregate risk than those driven by the policy design. Unless one relaxes the cap on the loan amount and transfers the full risk to the government, these comparative statics cannot deliver an aggregate risk higher than 1% of GDP or 20% of program credit. Although the aggregate risk of the program is large relative to its size, just redistributing the allocated credit toward the existing risky firms or even increasing the default rate does not substantially raise the expected loss to GDP. The available supply of credit would need to rise and the demand would need to match it to increase aggregate risk more substantially. Having a larger mass of risky firms could also increase aggregate risk.[55]

To conclude the counterfactual analyses, we assess the aggregate expected loss without the employment program. The previous aggregate expected loss calculations incorporate the employment program's effects on borrower quality and credit quantity. We now isolate the impact of the employment program in three steps.

First, borrower quality deteriorates because the default probability for firms using only the credit program is 1.1 percentage points higher than for firms using both programs. This raises the average default probability from 7.4% to 8.5%, increasing the expected loss from 0.27% to 0.31% of GDP (seventh scenario).

Second, firms have a greater need for credit without the employment program. We estimate that firms using only the credit program require 17% more credit than those relying on both programs. This raises credit from 3.6% to 4.2% of GDP without the employment program, leading to an increased aggregate loss of 0.31% of GDP (eighth scenario). Third,

[55]In Appendix Table 16, we report additional results separating the counterfactual calculations of aggregate risk related to the credit program between the shares absorbed by the government and banks.

when combining the effects of lower borrower quality and higher credit demand in the absence of the employment program, the aggregate expected loss rises to 0.36% of GDP (ninth scenario), a third larger than the baseline scenario of 0.27%.

7 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 previous credit history, triggering a formalization of those firms. Although 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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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*.[56] 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

[56] 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).[57] 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.[58] For some estimations, we further partition different samples based on their banking status. In particular, we split the selection and leverage model sample, 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 firms 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.

[57] The employment program does not have a selection constraint at the firm level (other than having positive employment).

[58] Appendix Table 2 shows detailed summary statistics of the main variables used in our paper.

Appendix Figure 3 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):

$$\underbrace{\frac{D_{gt}-D_{gt-1}}{Y_{gt-1}}}_{Within\ Change}\underbrace{\omega_{gt-1}}_{Weights} = \underbrace{\frac{D_{gt}-D_{gt-1}}{Y_{gt-1}}\omega_{gt-1}}_{Group\ Change}. \tag{5}$$

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

$$\sum_{g\in G}\underbrace{\left(\frac{D_{gt}-D_{gt-1}}{Y_{gt-1}}\omega_{gt-1}\right)}_{Group\ Change} = \underbrace{\frac{\Delta D_t}{Y_{t-1}}}_{Aggregate\ Change}. \tag{6}$$

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.[59]

Appendix Table 15 presents the results of this aggregation. We first consider the set of credit program users (Panel A).[60] 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.

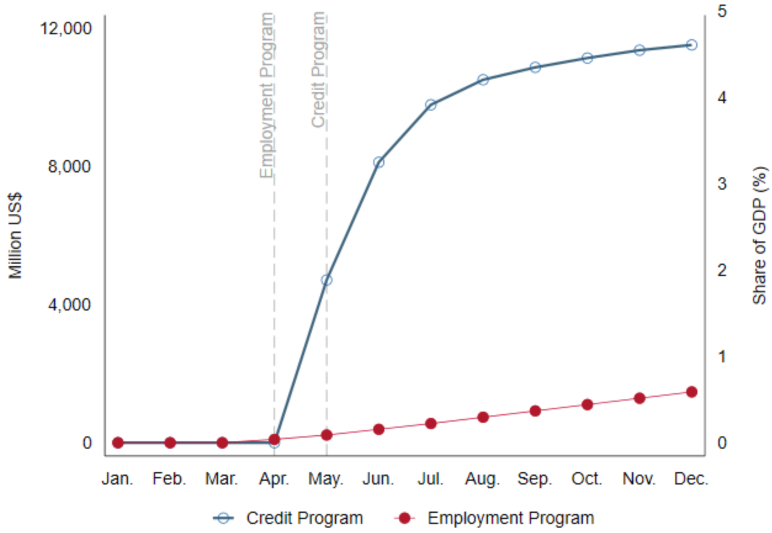
Summing across all risk groups shows that the indebtedness of firms that use the credit program increases by 9.71 percentage points. Those firms experience an increase of 3.6% of GDP in guaranteed credit (Table 7) and a decline in non-guaranteed credit. Extending the analysis to include all active firms (users and non-users) by risk confirms that the higher the risk, the larger the within-change in debt (Appendix Table 15, Panel B). But the higher the risk, the smaller their weight in the economy, attenuating the increase in aggregate risk.[61]

[59]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.

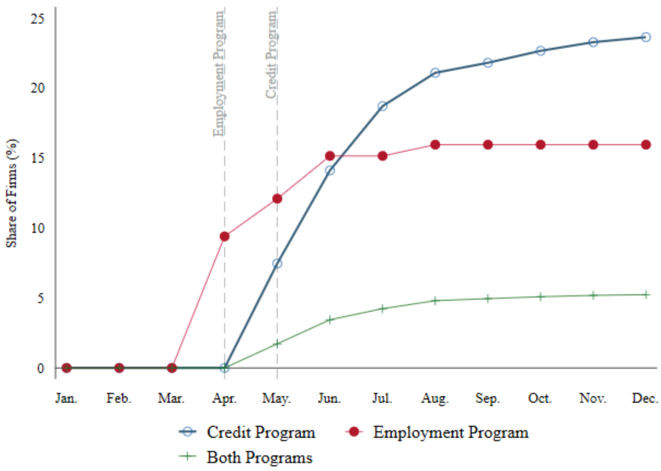
[60]The results reported in this table are only for credit-program users and therefore are not directly comparable with the numbers in Figure 2.

[61]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.

Figure 1
Reach of Public Programs



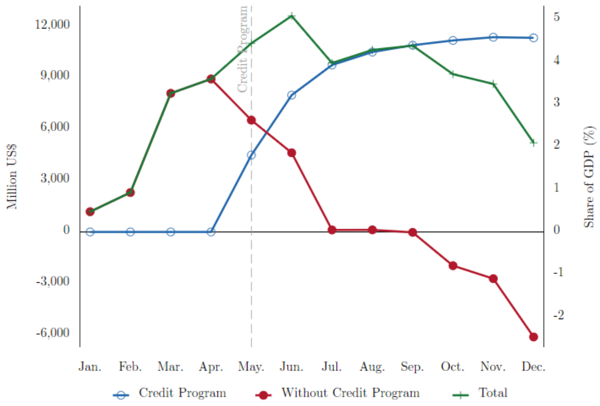
(A) Size of Public Programs



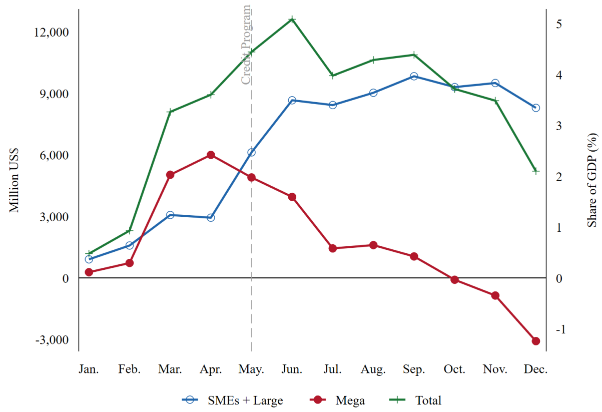
(B) Share of Firms Using Public Programs

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.

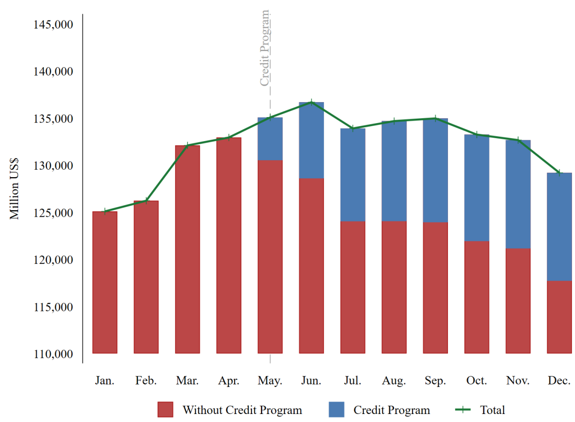
Figure 2
Credit Granted and Outstanding Corporate Debt



(A) Guaranteed and Non-Guaranteed Credit Granted



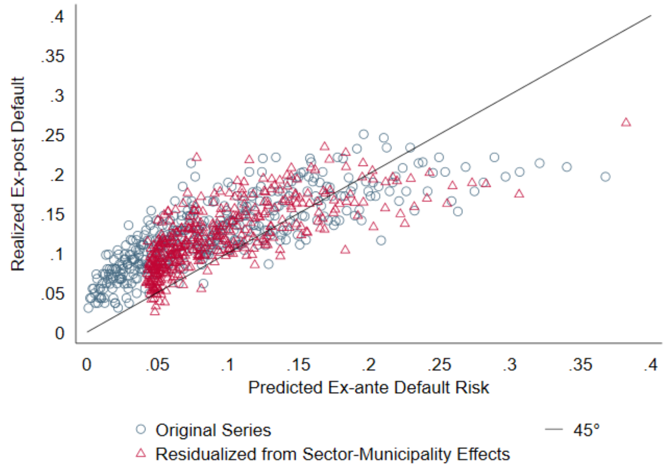
(B) Guaranteed and Non-Guaranteed Credit Granted by Firm Size



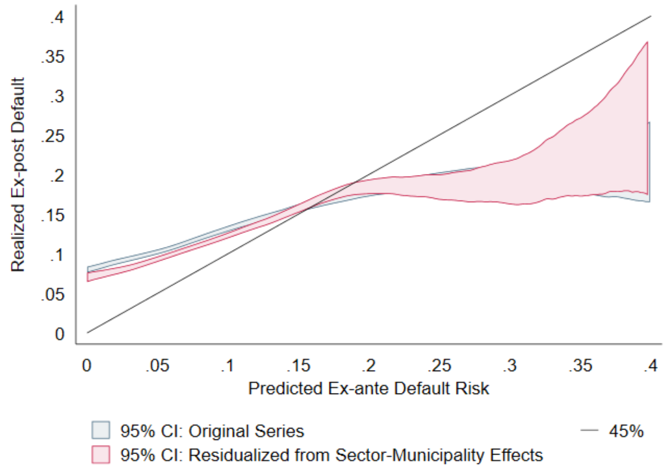
(C) Outstanding Corporate Debt

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 each program is implemented.

Figure 3
Correlation between *ex ante* Default Risk and *ex post* Default



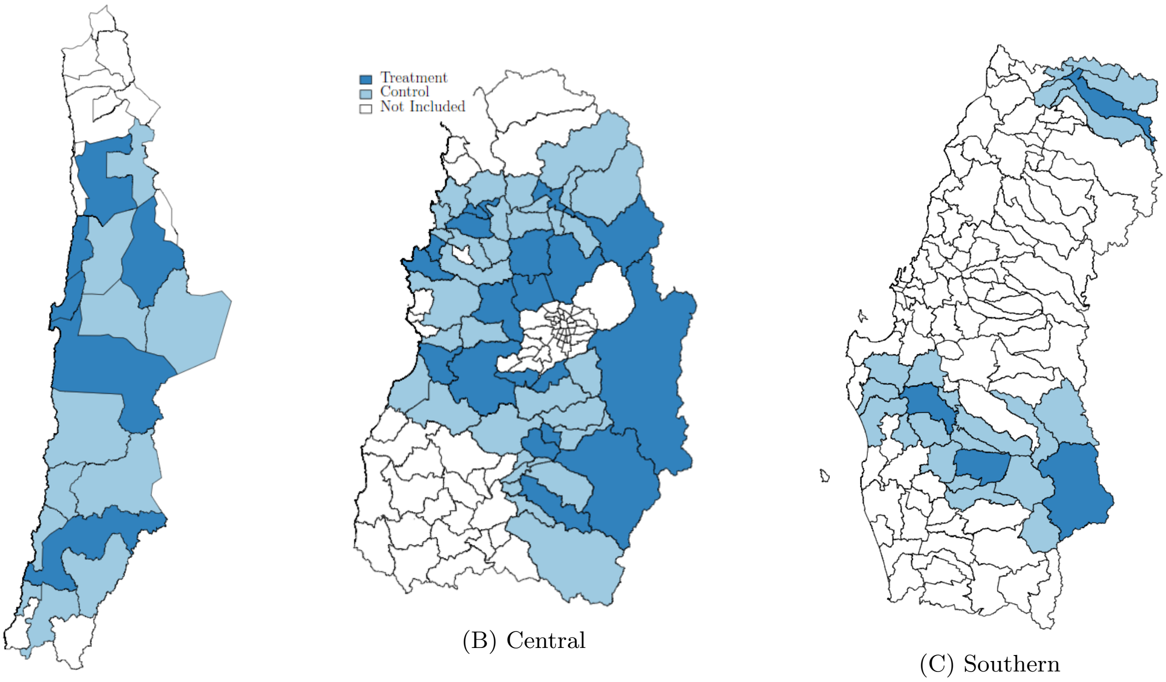
(A) Scatterplot



(B) Confidence Interval

This figure plots the correlation between predicted *ex ante* default risk and the realized *ex post* default. *ex ante* default risk is the predicted default probability from the model in Table 1, Columns 4 and 8. 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 equal to zero otherwise. Panel A shows the simple correlation between both measures of default. Each dot is a bin between the average *ex ante* default risk and the average *ex post* default. Panel B shows the confidence intervals (CI) of the correlation using a non-parametric adjustment. The original series shows the average *ex ante* and *ex post* default pairs for each bin. Residualized from sector-municipality effects means that the average *ex ante* and *ex post* default pairs are computed after controlling for sector-municipality fixed effects. Both series are split into 400 bins with an equal number of firms.

Figure 4
Dynamic Lockdowns: Treatment Definition



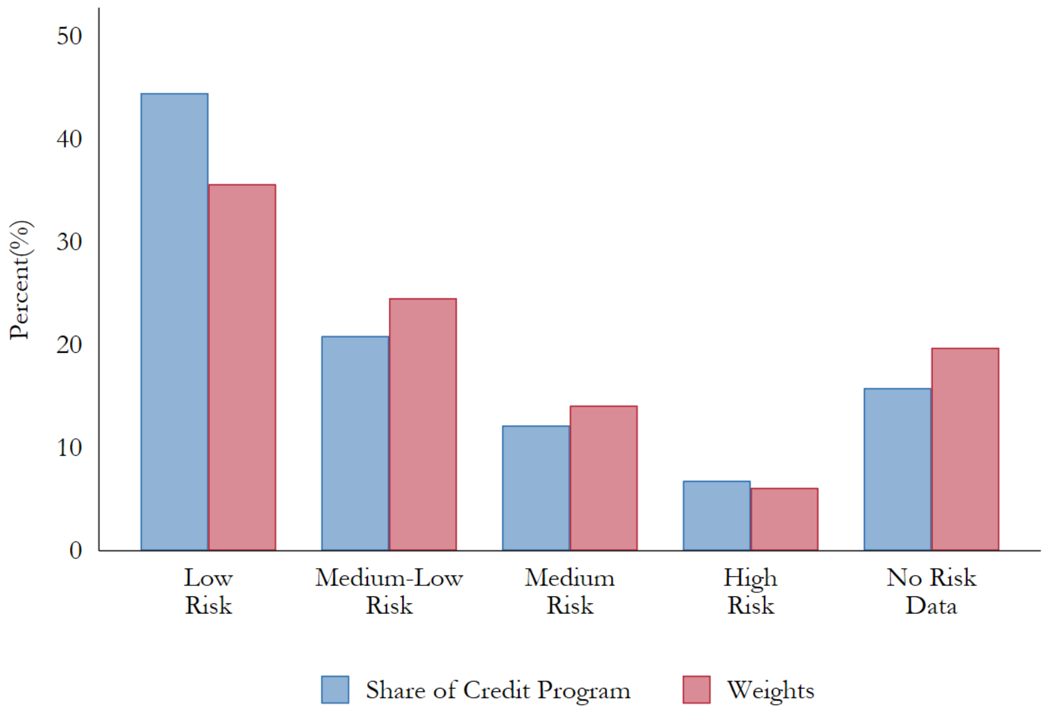
(A) Northern

(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 15, 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	0.076
Dependent Variable Std. Dev.	0.284	0.284	0.284	0.284	0.284	0.284	0.284	0.264
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 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 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 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). 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 a previous loan 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 Size Getting Approval for the Credit Program

	Credit Program			
	(1)	(2)	(3)	(4)
	All Firms	Small Firms	Medium Firms	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 loan 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 firms, small firms, medium firms, and large firms, 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.

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	0.005	0.019
	(0.003)	(0.000)
Post	0.025	−0.009
	(0.004)	(0.001)
Lockdown	−0.002	0.022
	(0.002)	(0.014)
Number of Observations	103,932	110,439
Number of Firms	11,483	12,202
R^2	0.009	0.010
Region FE and Month FE	Yes	Yes
Panel B: Firms along Municipality Border		
Post × Lockdown	0.007	0.028
	(0.008)	(0.005)
Post	0.028	0.002
	(0.003)	(0.004)
Lockdown	0.090	0.068
	(0.005)	(0.003)
Number of Observations	14,796	17,172
Number of Firms	1,644	1,908
R^2	0.013	0.012
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 variable is 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. 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 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. 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	18.17	0.04	17.11	0.04
Medium Risk	1,087	0.4	12	9.88	0.04	12.19	0.05
Medium-Low Risk	1,863	0.8	21	5.69	0.04	11.17	0.08
Low Risk	3,972	1.6	44	2.05	0.03	6.24	0.10
No Risk Data	1,411	0.6	16	18.17	0.10	17.11	0.10
Total: Formal Firms	8,941	3.6	100	7.40	0.27	10.44	0.38
Panel B: Formal Firms + Natural Persons							
Formal Firms	8,941	3.6	78	7.40	0.27	10.44	0.38
Natural Persons	2,563	1.0	22	18.17	0.19	17.11	0.18
Total: Formal Firms + Natural Persons	11,504	4.6	100	9.80	0.45	11.93	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* probability of default 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 missing a 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's 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	18.17	4.4	62.4	0.03	0.02	0.11
Medium Risk	0.04	79.9	9.88	3.9	48.0	0.02	0.02	0.15
Medium-Low Risk	0.04	77.0	5.69	3.5	29.6	0.01	0.03	0.20
Low Risk	0.03	72.1	2.05	3.0	0.0	0.00	0.03	0.22
No Risk Data	0.10	82.5	18.17	4.4	62.4	0.06	0.04	0.26
Total: Formal Firms	0.27	76.4	7.40	3.5	40.0	0.11	0.16	1.07
Panel B: Formal Firms + Natural Persons								
Formal Firms	0.27	76.4	7.40	3.5	40.0	0.11	0.16	1.07
Natural Persons	0.19	82.5	18.17	4.4	62.4	0.12	0.07	0.47
Total: Formal Firms + Natural Persons	0.45	77.8	9.80	3.7	45.0	0.22	0.23	1.54

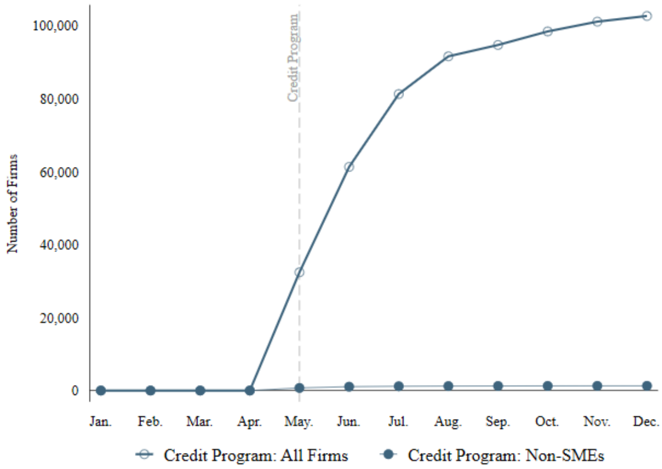
This table shows the distribution of the aggregate expected loss borne by the government and the banking system as a result of the credit program, for natural persons and the formal firms sample. Panel A reports statistics across the firms' risk distribution. Panel B reports statistics separately for 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
Counterfactual Calculations of Aggregate Risk

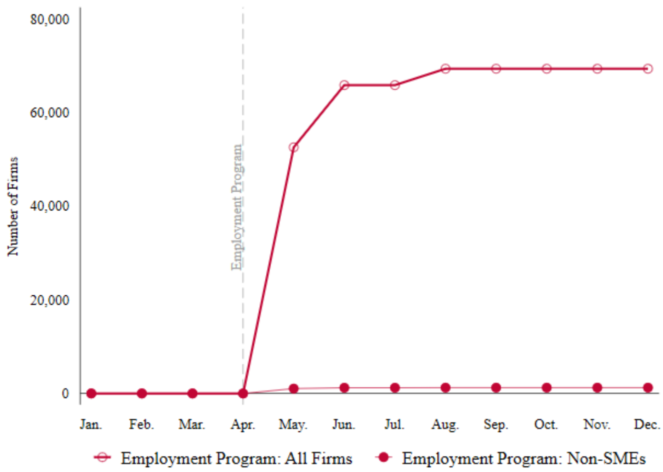
		(1) Credit/GDP (%)	(2) Average Default Probability (%)	(3) Expected Loss/GDP (= (1) × (2)) (%)
	Baseline	3.6	7.4	0.27
Equilibrium				
1	2008 Global Financial Crisis Default Rate	3.6	12.4	0.45
2	All Credit Allocated to Riskiest Firms	3.6	18.2	0.66
Policy				
3	Complete Guarantee and No Deductible	8.3	7.4	0.62
4	Amount Cap Equal to 6 Months of Sales	15.1	7.0	1.06
5	Complete Guarantee and No Deductible & Amount Cap Equal to 6 Months of Sales	16.6	7.4	1.23
6	Firms in Default Are Also Eligible	7.6	9.2	0.70
7	No Employment Program, Keeping Credit Constant	3.6	8.5	0.31
8	No Employment Program, Keeping Default Probability Constant	4.2	7.4	0.31
9	No Employment Program	4.2	8.5	0.36

This table shows comparative statics of policy ingredients that potentially mitigate aggregate risk. The baseline number from Table 8 is reported for reference. Row 1 presents aggregate risk in the case in which default rates are shifted upwards in a magnitude similar to how default rates increased during the 2008 global financial crisis (5 percentage point increase). Row 2 presents aggregate risk in the case in which all the credit allocated in the program goes to the riskiest firms. Row 3 presents aggregate risk when there is a complete guarantee and no deductible (and thus, all the allocation is driven by demand forces). Row 4 presents aggregate risk allowing a cap of 6 months (rather than the 3 months) of sales as credit. Row 5 merges the policies of Row 3 and 4. Row 6 presents the case in which there is no eligibility constraint on previous default behavior. Calculations in Rows 3 and 5 exclude the No Risk Data category. Rows 7, 8, and 9 present the case in which there is no employment program. Row 7 focuses on the effect on default probability. Row 8 focuses on the effect on credit allocated. Row 9 considers both the effects on default probability and credit allocated.

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



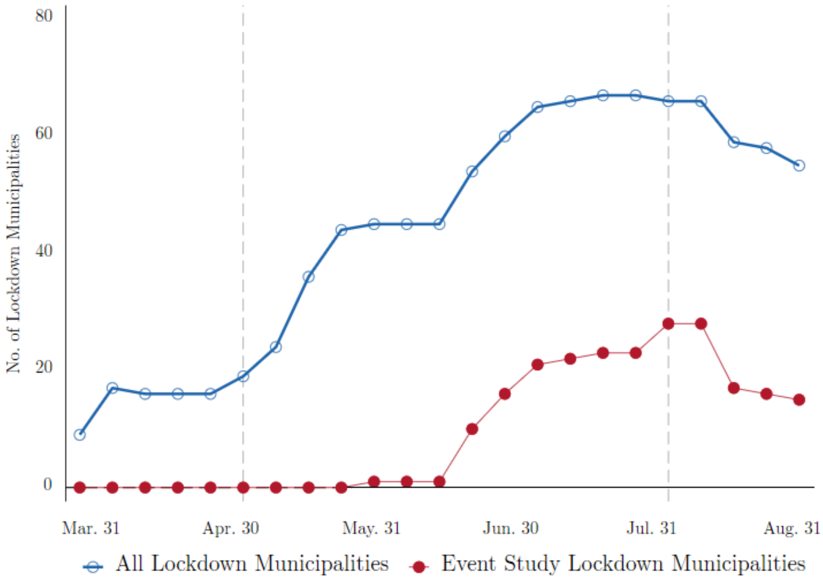
(A) Number of Firms Using Credit Program



(B) Number of Firms Using Employment Program

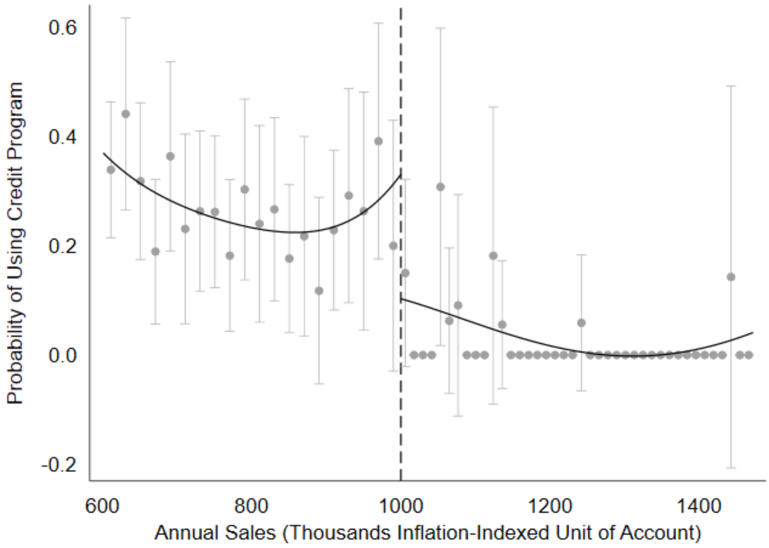
This figure plots the number of firms using the credit and employment program during 2020. Panel A plots the number of firms using the credit program. Panel B displays the number of firms using the employment program. Both panels differentiate between the total number of firms and the number of non-SMEs. 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
Number of Municipalities Subject to Lockdown Mandates Over Time

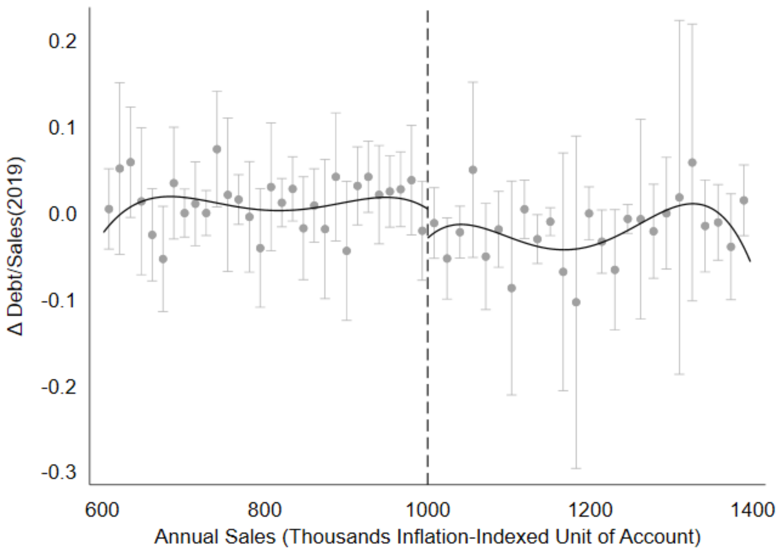


This figure shows the evolution of the number of municipalities subject to lockdown mandates over time for (a) all the municipalities in Chile and (b) all the municipalities included in the dynamic lockdown event study. The dashed vertical lines show, respectively, the starting and ending dates considered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

Appendix Figure 3
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 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
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 3
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 defaulted on a loan during 2019 and is 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, Columns 4 and 8, and 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. On the other hand, 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. The mean and standard deviation of the dependent variable are reported. 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 4
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 5
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 6
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 7
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 8

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 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 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 9
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 10
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 11

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 12
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
Deuda (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 13
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 14
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 15
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.

Appendix Table 16

Counterfactual Calculations of Aggregate Risk for Government and Banks

		(1) Default Probability (%)	(2) Guarantee (%)	(3) Deductible (%)	(4) Effective Guarantee (%)	(5) Expected Loss/GDP (%)	(6) Government's Expected Loss/GDP (= (5) × (4)) (%)	(7) Banks' Expected Loss/GDP (= (5) × (1 − (4))) (%)
	Baseline	7.4	76.40	3.50	40.26	0.27	0.11	0.16
Equilibrium								
1	2008 Global Financial Crisis Default Rate	12.4	81.40	8.50	25.60	0.45	0.11	0.33
2	All Credit Allocated to Riskiest Firms	18.2	82.50	4.40	62.52	0.65	0.41	0.25
Policy								
3	Complete Guarantee and No Deductible	7.4	100.00	0.00	100.00	0.62	0.62	0.00
4	Amount Cap Equal to 6 Months of Sales	7.0	76.19	3.49	38.34	1.06	0.41	0.65
5	Complete Guarantee and No Deductible & Amount Cap Equal to 6 Months of Sales	7.4	100.00	0.00	100.00	1.23	1.23	0.00
6	Firms in Default Are Also Eligible	9.2	80.39	4.18	43.94	0.70	0.31	0.39

This table shows comparative statics of policy ingredients that potentially mitigate aggregate risk. The baseline number from Table 8 is reported for reference. Row 1 presents aggregate risk in the case in which default rates are shifted upwards in a magnitude similar to how default rates increased during the 2008 global financial crisis (5 percentage point increase). Row 2 presents aggregate risk in the case in which all the credit allocated in the program goes to the riskiest firms. Row 3 presents aggregate risk when there is a complete guarantee and no deductible (and thus, all the allocation is driven by demand forces). Row 4 presents aggregate risk allowing a cap of 6 months (rather than the 3 months) of sales as credit. Row 5 merges the policies of rows 3 and 4. Row 6 presents the case in which there is no eligibility constraint on previous default behavior. Calculations in Rows 3 and 5 exclude the No Risk Data category.