The Distribution of Crisis Credit: Effects on Firm Indebtedness and Aggregate Risk*

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Abstract

We study the distribution of credit during crisis times and its impact on firm indebtedness and macroeconomic risk. Whereas policies can help firms in need of financing, they can lead to adverse selection from riskier firms and higher default risk. We analyze a large-scale program of public credit guarantees in Chile during the COVID-19 pandemic using unique transaction-level data of demand and supply of credit, matched with administrative tax data, for the universe of banks and firms. Credit demand channels loans toward riskier firms, distributing 4.6% of GDP and increasing firm leverage. Despite increased lending to riskier firms at the micro level, macroeconomic risks remain small. Several factors mitigate aggregate risk: the small weight of riskier firms, the exclusion of the riskiest firms, bank screening, contained expected defaults, and the government absorption of tail risk. We quantitatively confirm our empirical findings with a model of heterogeneous firms and endogenous default.

Keywords: Bank credit demand, bank credit supply, COVID-19, crises, debt, firm risk, macroeconomic risk, public credit guarantees
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1 Introduction

During economic crises, governments often try to help struggling firms by providing financing with the idea of increasing their chances of survival and a faster recovery. In doing so, they face different trade-offs. One is the need to quickly reach broad coverage, at the expense of distributing untargeted assistance to firms that do not need help or are too risky. Another trade-off is that although these policies might be beneficial in sparing firms from collapse in the short term, they can increase the overall indebtedness of the private sector, affecting debt overhang, repayment rates, financial stability, and fiscal cost in the longer term.

The aggregate consequences and risks of government-induced credit injections hinge critically on how debt is actually distributed throughout firms. This depends on the incentives and reactions of borrowers and lenders and on different factors that can augment or mitigate micro and macro risk-taking. On the demand side, forces can help channel funds to those firms with the highest marginal returns to capital. But limited liability could also generate demand from firms with higher default risk. On the supply side, when banks have only partial insurance from the government for the credit they grant, they will tend to lend to firms that limit their risk-taking. But other reasons (such as changes in the cost of borrowing and lending) can also affect their decisions. Understanding which incentives and conditions matter in credit markets and their quantitative consequences on the aggregate equilibrium outcome is challenging and important and also requires comprehensive micro data.

In this paper, we study how credit and risk are distributed across the whole economy when market participants face different incentives, under a massive new policy during a systemic shock. We exploit a large-scale public credit guarantee program (henceforth credit program) established by the Chilean government to support firms during the COVID-19 pandemic. This allows us to study empirically and analytically the drivers and consequences of the distribution of "crisis credit" (credit during crisis times), which has implications beyond crises. To be able to conduct the analysis, we obtain unique and detailed information on the behavior of all types of firms and banks and the overall environment where economic agents operate.

Our main results show a disconnect between the micro and macro effects. At the micro level, there is a substantial expansion of indebtedness both within firms and across different types of firms. The credit expansion is characterized by "adverse selection" on the extensive margin (selection into the program) and the intensive margin (increase in debt) by riskier firms with access to the program.[1] At the macro level, risks appear to be small, in part because risky firms weigh little in the aggregate credit allocation. Although risky firms increase their indebtedness in percentage points the most, the overall amount of credit going to less risky firms is much larger (even when it represents a smaller increase in relative terms for them). Some of the features driving the micro and macro results are due to the program design, while others are due to how the demand and supply sides respond to incentives and to general market conditions.

The transaction-level data we gather are unique in their comprehensive nature and level of detail. We employ the universe of formal bank credit to all firms collected by Chile's financial supervisory agency and high-frequency-matched administrative data for the universe of formal firms compiled by the tax authority and the central bank. The former include credit flows and balances, default history, firm loan applications, bank approvals and rejections, and terms of the individual transactions. The latter contain time-varying financial statements, input use, and sales collected monthly, plus industry and municipality of where firms are located. These micro data, especially loan applications and approvals, allow us to measure selection and to disentangle supply and demand factors determining the countrywide equilibrium allocation of credit. The micro data also enable us to estimate the macro risk by calculating risk at the firm level and then aggregating across types of firms.

In addition to offering novel data, the large and sudden quasi-natural experiment in Chile contains attractive features to study the drivers and effects of crisis credit. The size requirements of the credit program, dynamic lockdowns, and an alternative employment program, among other things, provide sources of exogenous variation and useful information to analyze the effects of the credit program implemented when the pandemic erupts. In exchange for banks granting loans at a predetermined low interest rate to most firms, the government offers to take most of the tail credit risk. The flat low interest rates alone would differentially increase demand from riskier borrowers who otherwise pay correspondingly higher risk premia under the status quo. However, to reduce this adverse selection while generating new financing, the program establishes that banks absorb the initial loss and a fraction of the remaining credit risk. New loans are not allowed to be used to repay existing loans, and firms with past due repayments are ineligible to participate in the program.[2]

[1] We use the term "adverse selection" in a loose sense to convey bank lending shifting toward riskier clients as our analysis lacks the standard presumption of asymmetric information. Our empirical estimations are based on observable information, and the model we present also has symmetric information.

[2] The policy in Chile has similarities with other public credit guarantee programs, such as those in Colombia, Italy, and the United Kingdom (OECD, 2017). It is also related to an array of credit programs in more than 142 countries that flourish with the COVID-19 pandemic (Feyen et al., 2021).

The expansion of crisis credit we uncover is fast, deep, and broad based. Whereas credit expands only to "mega" firms (those with sales over US$35 million) before the credit program is implemented, bank loans flow to much smaller firms in just a few months after the government starts guaranteeing credit risk in May 2020. Almost 25% of all eligible firms in the country receive credit, many of them previously "unbanked" (with no bank credit history). The program contributes to a sizable increase in private credit of over 4.6% (US$11 billion) of 2019 gross domestic product (GDP) during 2020, compared to a total credit expansion of 4.7% of 2019 GDP during 2019 and a collapse during past crises. Through the increase in credit across firms, the credit program counteracts the 2020 contraction of net credit granted outside its purview (when GDP suffers a 5.8% negative shock).

At the micro level, the distribution of crisis credit across firms is driven by eligibility and adverse selection on the demand side. The credit program targets small- and medium-sized enterprises (SMEs) and includes large firms. Firms ineligible for the program are at the tails of the risk distribution (firms with past due loans and safer mega firms), gearing credit toward the middle of the distribution. Among eligible firms, those that apply for guaranteed loans are riskier: they have higher pre-existing debt, pay higher interest rates, and have lower net worth. Banks exercise partial screening on the supply side by rejecting riskier firms, but the strong demand driven by adverse selection determines the equilibrium outcome. On the intensive margin, leverage increases disproportionately among risky firms that borrow through the credit program. A regression discontinuity design (RDD) suggests a causal relation, but this outcome is not merely an inevitable corollary of program usage. Guaranteed credit could have substituted for a fall in non-program credit for the firms using the program.

At the macro level, our rich data allow us to aggregate our micro results to match the overall allocation obtained from aggregate statistics and to conclude that macroeconomic risks remain contained. Several mitigating factors explain the low macro risk, notwithstanding the higher micro leverage, adverse selection, and lending to SMEs. First, less risky large firms that qualify for the program weigh more in the ex-post distribution of crisis credit simply because of their sheer ex-ante size, even when, proportionately, their debt increases the least. This complements the design condition that the riskiest firms (those with pre-crisis default history) are excluded.

Second, the combination of different expected default rates at the micro level and credit allocation to the various risk groups generates an expected aggregate default (expected loss) of roughly 0.44% of GDP. Although this implies a 9.56% aggregate expected loss of the credit granted, it is substantially lower than the expected default of the risky firms and corresponds to a sizable expansion of SME credit. Third, banks partially screen risky borrowers because of the partial guarantee and increase their capital to risk-weighted assets, which helps cushion the banking system from a further potential shock. Fourth, by absorbing the tail credit risk and providing liquidity support to banks at low cost to back lending, the government shares risk with banks. Though this risk-sharing arrangement increases the potential fiscal cost, it reduces the probability of involving the banking sector in a protracted financial crisis.

Despite the credit guarantee program being implemented to cope with the COVID-19 crisis, our findings on how credit is distributed across firms do not seem to be driven by the pandemic effects on firm performance. First, firms with both positive and negative variations in sales during the pandemic are more likely to apply and be approved for credit. Second, there is no difference in credit distribution to firms in municipalities under lockdown and those in contiguous municipalities with free circulation. Credit flows across the board regardless of how the pandemic affects firms. Third, the credit program's broad reach is not present when we analyze a simultaneous large employment protection program (or employment program), which imposes an opportunity cost for firms to use it. By comparing the demand-side behavior of the same firms facing a distinct incentive structure, we find that firms that apply for the employment program are seemingly more impacted by the pandemic and are not ex-ante riskier. The comparison of the demand for the employment and credit programs provides additional evidence that the credit granted is not specific to the pandemic effects and shows how the setup matters for the private sector response to government assistance during crisis and non-crisis times.

Returning to our larger message that crisis credit can increase indebtedness with only a limited increase in aggregate risk under certain circumstances, we further evaluate this claim quantitatively using a simple model of endogenous firm default in competitive financial markets. We model static loans to firms that vary exogenously in their revenue productivity, equity, and risk. The model justifies leverage and interest rate spreads as empirically meaningful measures of default risk. Quantitatively, it confirms how a wide variety of firms increase indebtedness and yet aggregate risks are low because little credit flows to the riskiest firms under the government policy. Counterfactual simulations show that (i) credit guarantees, willingness to lend, and a lower interest rate cap all contribute to increased credit; (ii) the cap limits lending to the riskiest firms; and (iii) a negative aggregate shock would need to be sizable (even relative to the pandemic) to increase aggregate risk substantially.

The rest of the paper is structured as follows. Section 2 succinctly describes the paper's contribution to the literature. Section 3 introduces the data used in the paper, and Section 4 provides a description of the policy response to the crisis. Section 5 studies which types of firms access credit. Section 6 analyzes the effects on firm indebtedness, and Section 7 studies the aggregate implications of the credit program. Section 8 presents a model that sheds light on the incentives to borrow during crises and a counterfactual analysis of the effects of different elements of the program on credit allocation. Section 9 concludes.

2 Contribution to the Literature

Our paper contributes to a large and growing literature on the allocation of credit during crises, which shows that financial intermediaries can transmit shocks to firms by reducing credit supply (Peek and Rosengren, 2000; Khwaja and Mian, 2008; Ivashina and Scharfstein, 2010; Jiménez et al., 2012; Becker and Ivashina, 2014). To help channel credit to firms in distress during crises, governments implement public programs using, for example, public credit guarantees or state-owned banks (Jiménez et al., 2018; Barrot et al., 2021; Gonzalez-Uribe and Wang, 2021; Akcigit et al., 2021).[3] Complementing the credit assistance, employment programs can also lessen large debt buildups, the payroll burden, and layoffs that could further impair economic recovery ex-post (Hijzen and Venn, 2011; Cahuc et al., 2018, 2021; Giupponi and Landais, 2018; Kopp and Siegenthaler, 2021).

With the COVID-19 pandemic, the interest in using government programs and distributing credit and government support has grown substantially (Amiram and Rabetti, 2020; Bennedsen et al., 2020; Granja et al., 2020; Keller and Zoller-Rydzek, 2020; Li and Strahan, 2021; Balyuk et al., 2021; Core and De Marco, 2021; Custodio et al., 2021; Duchin et al., 2021).[4] One key discussion is the distribution of crisis credit in the absence of government programs, when loans tend to go to the largest firms (Acharya and Steffen, 2020; Goel and Serena, 2020; Li et al., 2020; Chodorow-Reich et al., 2021; Greenwald et al., 2021). To compensate, government programs can aim to shift the credit allocation toward smaller firms.

A related aspect that has gained attention when corporations accumulate debt is the macroeconomic consequences of decisions at the micro level, especially when governments are involved (Banerjee and Hofmann, 2020; Brunnermeier and Krishnamurthy, 2020; Cevik and Miryugin, 2020; Demmou et al., 2021; Reinhart, 2021). High debt accumulated during crises

[3]Public credit guarantees are not originally designed as a crisis mitigation tool. In normal times, they serve as an instrument to channel credit to small, credit-constrained firms (Smith, 1983; Gale, 1990, 1991; Lelarge et al., 2010; Brown and Earle, 2017; de Blasio et al., 2018; Mullins and Toro, 2018; Bachas et al., 2021). They have been used during crises because of their ready availability.

[4]Comprehensive recounts of the programs used are available as policy trackers compiled by Harvard's Kennedy School, the IMF, and Cirera et al. (2021).

can lead to the emergence of zombie firms, low aggregate investment, and debt overhang (Caballero et al., 2008; Schivardi et al., 2017; Kalemli-Ozcan et al., 2019; Xiao, 2020; Chari et al., 2021). But public programs can also reduce bankruptcies and firm failures (Carletti et al., 2020; Demmou et al., 2020; Díez et al., 2021; Cros et al., 2021; Gourinchas et al., 2021).

What makes our paper unique is that we study how a sizable and sudden government credit program available to most firms has worked in practice. By analyzing the universe of bank loans and formal firms in a country, which we match with their credit history and firm-level attributes, we can obtain a systemic view. That is, rather than focusing on a single segment of firms (e.g., either SMEs or publicly listed firms), we can speak to the allocation of credit across the entire firm distribution and the program's aggregate impacts on both banks and the government.

Having the complete set of bank transactions, firms, and credit history can be important for evaluating the behavior of the supply and demand of credit since firms and banks can substitute across lenders, borrowers, and loan types. Moreover, it provides precise estimates of the macroeconomic impacts of the loans granted by characterizing the ex-post credit exposure of firms with different risk of default. It also enables us to identify different contributors and mitigating factors of aggregate risk accumulation. Calibrating our model to these data allow for counterfactual simulations of different policies and scenarios. Our data on loan applications and approvals allow us to estimate the actual behavior of firm demand and bank supply of public guaranteed credit and what determines the equilibrium outcome. Although we find adverse selection, as the existing literature does, we show that its contribution to aggregate risk can be contained.

3 Data

We use three administrative data sets from various sources in Chile, depending on what is appropriate for the type of analysis we perform at different points. These data sets cover the entire formal private sector in Chile in rich detail. 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, 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 to 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 sets contain monthly, firm-level information including sales, materials expenditure, value added, number of workers, wage bill, net worth, age, industry, and municipality. These data 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, which we do with monthly sales during 2020. Third, we work with publicly available firm-level data on firms that use the employment program, a policy of interest for comparison purposes. 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 anonymous tax identification codes (IDs) of workers and firms that are common across sources. To secure the privacy of workers and firms, the codes are ran by staff at the central bank that have the security clearance to access the full data. Furthermore, the Chilean tax authority requires all the results that are extracted and used in this paper to be calculated using at least 25 tax IDs. In sum, the merged data set allows us to study real and financial aspects of both the credit and the employment program covering the universe of firms. For most of the analysis, we use the 2018–2020 period of these data sets.

To construct the sample of firms we use, we start from all legal and formally registered firms in the economy (602,874 firms) that have a tax ID, which we call *formal firms*.[5] We then build several samples of the merged data that we use in different parts of the paper. Each sample has a different size and coverage (Table 1). The first sample, which we call *active firms*, is constrained to include only firms with positive sales in 2019, which amounts to 449,615 firms. We use this sample to conduct the aggregate analysis of the paper and the

[5] We exclude natural persons who use their personal tax ID 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 11).

mapping between micro and macro patterns. This sample represents 75% of firms, 92% of employment, 82% of the stock of credit, and 100% of positive value added in the economy. Among active firms, 97% are SMEs and contribute to 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 for estimating the default probability models. Starting from the active firms sample, this second sample includes the restriction of using 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 "banked" firms to estimate the model, i.e., firms that have outstanding debt as of December 2019 or receive a loan over the period 2012–2019. Otherwise, the firms are considered unbanked. Banked firms 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, which includes 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 make a more equal comparison between the two policies. We call this the *selection and leverage model* sample. This sample represents 18% of firms, 50% of employment, 44% of the stock of credit, 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 and imposes the eligibility constraints from the public credit guarantee program, namely that firms must be smaller than the sales threshold imposed by law and cannot have payments past due more than 30 days (i.e., a strict measure of default). The employment program does not have a selection constraint at the firm level (other than having positive employment). We call this the *credit program eligible firms* sample. This sample represents 17% of firms, 35% of employment, 21% of the stock of credit, 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. This sample represents 6% of firms, 14% of employment, 9% of the stock of credit, and

7% of value added.[6] 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 banked and unbanked firms.

4 Government Programs and the Expansion of Crisis Credit

The economic crisis and crisis credit episode we study originates with the COVID-19 pandemic. With the health system under severe stress due to the pandemic's early spread, the Chilean government imposes mandatory lockdowns in several municipalities across the country, starting in March 12, 2020.[7] Consequently, many businesses are forced to close temporarily, either because their demand collapses or because of the impossibility of operating under lockdown. Firms' cash flows collapse. By November 2020, Chile is the country with the fourth most infections per million people in Latin America and the Caribbean (Engel et al., 2020), and the economy suffers a sizable 5.8% GDP contraction in 2020 (down from a 0.9% expansion in 2019).

In response to the crisis, the Chilean government implements large programs to help firms and to 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 existing program, called FOGAPE, is a public fund that guarantees a fraction of loans provided by banks to small firms, those with annual sales less than US$800,000.[8] In the event of default, resources are withdrawn from the fund to pay the guaranteed fraction of the loan to the bank. The program has similarities with the other public credit guarantee programs, mentioned in the introduction, used around the world.

On April 24, 2020, the National Congress of Chile approves a bill (called FOGAPE COVID-19) proposed by the Ministry of Finance that injects US$3 billion into the public credit guarantee fund (1.2% of Chile's GDP). The law also extends access to the program to medium and large firms, those with annual sales up to US$35 million.[9] Mega firms (those with

[6]Further detailed summary statistics as of December 2019 are described in Appendix Table 1, showing the main variables used in our paper for the matched firms in our sample.
[7]Chile is divided into 16 regions. Each region is divided into municipalities, which constitute the country's smallest administrative division. There are 345 municipalities in Chile.
[8]FOGAPE is an acronym for Fondo de Garantía para Pequeños Empresarios or Guarantee Fund for Small Entrepreneurs.
[9]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 US dollars using the average value of UF in pesos during 2019 and the dollar-peso exchange rate during 2020. Banks could use different sales indicators to determine each firm's sales.

annual sales above US$35 million) remain ineligible for the program. This credit program is the focus of our analysis.

Guaranteed loans are designed to finance working capital up to three months of sales, with monthly sales measured as the average for the pre-pandemic period (January to December 2019). The loans have a six-month grace period and are payable in installments during the proceeding 24 to 48 months. They have a low interest rate cap of 3.5%, equal to the monetary policy rate (0.5%) plus the inflation target (3%). The cap is notably lower than statutory caps of 20% on other loans; the capped interest rate of the loans in real terms is close to 0%. The program requires banks to restructure all of the firm's existing debts with the bank; the guaranteed loans cannot be used to repay prevailing obligations. Therefore, the program provides fresh resources to firms. Only firms that are up to date with their debt payments (no more than 30 days past due) at the moment of applying for the guaranteed loan are eligible for it.

After a firm applies for a guaranteed loan, the bank performs a risk analysis of the firm and can either accept or reject the application. The credit 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.[10] 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. As a result, for relatively high (low) default rates of the loan portfolio, the government (banks) absorbs most of the credit risk. That is, the program allows banks to transfer the tail credit risk to the government.[11]

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 of the program, more than US$8 billion or 3.3% of GDP (Figure 1). After five months (by September 2020), banks grant more than US$10 billion in guaranteed loans (4.4% of 2019 GDP). By the end of year, the program's size reaches US$11 billion (4.6% of 2019 GDP). This is large relative to the employment program in Chile and the Paycheck Protection Program (PPP) in the United States (3.1% of

[10] Small firms have annual sales between US$0 and US$0.8 million, medium firms between US$0.8 and US$3.5 million, medium-large firms between US$3.5 and US$21 million, and large firms between US$21 and US$35 million.

[11] Suppose, for example, that the ex-post default rate on a bank's loan portfolio to small firms (with a public guarantee of 85%) is 12%. Because of the deductible, the bank absorbs the first loss of 5%. Of the remaining 7% loss, the bank absorbs 1% ($= 0.15 \times 7\%$) and the government the remaining 6% ($= 0.85 \times 7\%$). In total, the bank absorbs 6% of the credit loss. Because the deductible is fixed, as the ex-post default rate increases, the fraction of risk absorbed by the bank decreases. In an extreme default event, the government absorbs most of the credit risk.

2019 GDP approved during 2020).

The employment program in Chile is the other major policy that helps firms during the crisis. On April 1, 2020, Congress approves the Employment Protection Act, enabling firms with government support to cover salaries and maintain firms' contracts with their workers while the employees are not working. The law allows distressed firms to freeze labor contracts with their employees rather than firing them. Workers' salaries are covered through withdrawals from the existing unemployment insurance fund, and the program alleviates firms' cash flows by reducing wage expenses. Like the credit program, the employment program expands on an existing program, this one a mandatory unemployment insurance program funded by three sources: workers, firms, and the government.[12]

Under the Employment Protection Act, the government injects US$2 billion into the solidarity component of the unemployment insurance fund (0.8% of Chile's GDP). Firms can either apply for total employment protection or partial protection. In the latter case, firms and workers agree on a temporary reduction of the work schedule (up to 50%). In comparison with the credit program, the key difference in incentives is that firms must shut down to qualify for the employment program so workers can stay home. The (opportunity) cost for a firm participating in the employment program is therefore the foregone output from the workers with frozen labor contracts. Both SMEs and large firms have access to the program by simply applying to it.

We plot the size of the employment program by calculating the value of the wage bill each firm saves from paying salaries to its workers with frozen labor contracts (Figure 1). Then, we sum the wage bill savings across all firms participating in the employment program. The size of the employment program is an order of magnitude smaller than the credit program. By December 2020, the employment program amounts to 0.8% of GDP, meaning that the credit program is five times larger than the employment program.

Guaranteed loans overtake overall credit in the economy during 2020. Until the credit program starts, total credit in the economy is essentially equal to non-guaranteed credit (Figure 2, Panel A). From May onward, cumulative non-guaranteed credit starts decreasing while guaranteed credit increases significantly. Also, during the initial two months of the pandemic, non-guaranteed credit to mega firms grows fast (Figure 2, Panel B), consistent

[12] The insurance fund has an individual and 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.

with the findings for the United States (Acharya and Steffen, 2020; Chodorow-Reich et al., 2021; Greenwald et al., 2021). But three months after the pandemic starts, the loan growth rate to mega firms starts decreasing and credit instead flows to SMEs and large firms. In sum, year-to-year overall credit growth during 2020 is explained exclusively by guaranteed loans to smaller firms. This contrasts strikingly with the collapse in credit during the 1998 Asian crisis and the 2009 subprime crisis, when the public credit guarantee program in Chile was negligible (Didier et al., 2021).

A sizable share of firms (almost 25% of eligible firms) obtain guaranteed loans by December 2020.[13] The adoption happens right in the program's first few months (Figure 3). This constitutes a large take-up compared to other countries that implement similar public credit guarantee programs during the pandemic. For example, in Peru, Colombia, and Mexico, 14%, 16%, and 23% of eligible firms use the credit program, respectively.[14] The low take-up is not restricted to developing countries; only 16% of firms use the 2020 public credit guarantee program in Italy (Core and De Marco, 2021). The employment program in Chile is smaller in value than the credit guarantee program, but it is also used by a significant fraction of firms, more than 16% of active firms by December 2020 (Figure 3). Around 33% of all active firms in the economy participate in either the credit or the employment program, and almost 7% of all firms participate in both programs.

In terms of applications, approvals, and usage within the active firms sample, 156,847 firms apply for credit guaranteed loans by December 2020, corresponding to 36% of all the eligible firms. Of all the loan applications, banks approve loans for 111,205 firms, indicating a high approval rate of 88%.[15] But not all firms that receive an approval end up using the program. In the estimations, we distinguish between approvals and usage to construct separate dummy indicators.

The credit guarantee program is used by banked and unbanked firms: 61% of the firms within the active firms sample that receive a guaranteed loan are banked and 39% are unbanked. This indicates that the program has an important effect on financial inclusion, by providing bank credit to a significant number of firms with no previous bank debt. Banked and unbanked firms receive 92% and 8% of the total value of guaranteed credit, respectively. Regarding size, the program is allocated primarily to smaller firms: 96% of the firms using

[13]The 25% take-up is based on the active firms sample. The take-up is higher (35%) when using the selection and leverage model sample.

[14]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.

[15]The approval rate is calculated over 126,524 firms that are not only eligible but also provide the correct paperwork to process the guaranteed loan application.

it are SMEs, and only 4% are larger firms. In comparison, 56% of the firms that use the employment program are banked firms and 96% are SMEs.

5 Credit Distribution across Firms

After showing that the credit guarantee program induces a large increase in aggregate credit, we now study the distribution of credit across firms with different risk.

5.1 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. We estimate the following cross-sectional probit model to predict default during 2019, based on attributes during 2018:

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

$Default_i$ 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 is 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).[16] This vector contains five real economic variables reported to the tax authorities: net worth, value added per worker (proxy for productivity), age, wage bill (proxy for labor intensity), and sales. It also includes two financial variables collected by the financial supervisory agency: credit stock 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 the loans. We sequentially introduce industry and municipality fixed effects into the estimation.

Table 2 presents estimates of Equation (1) using different specifications, and 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 unbanked firms.) Firms that have a higher net worth and are more productive, older, more labor intensive, and smaller have a significantly lower likelihood of default. The results remain unchanged for different sets of fixed effects. Columns 5–8 add the financial regressors, which also have little impact on the coefficients of most of these real factors with one exception: after controlling for the debt level, larger firms (according to sales) are less likely to default ex-post. Controlling for real variables like net worth, firms

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

with higher debt and ex-ante perceived to be riskier are also more likely to default ex-post. The results are robust to using different regressors and samples (Appendix Table 2).[17]

To predict risk of default during 2020, we use this model and plug in the real and financial variables in 2019. For banked firms, we predict default risk using the estimated coefficients from Table 2, column 8.[18] For unbanked firms, 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 unbanked firms is 10.7%, roughly 2 percentage points higher than for banked firms. The risk measure for banked firms is more accurate than for unbanked firms because it is based on both real and financial data.

5.2 Selection into the Government Programs

We next focus on the characteristics of the firms that participate in the credit program and contrast it with the selection into the employment program. We estimate the following cross-sectional probit model among the sample of firms that fulfill the eligibility requirements of each program:

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

$Program\, Use_i$ is a dummy equal to one if firm i, operating in sector s and located in municipality m, participates in the given public program and is zero otherwise. The variable risk is estimated from the default probability model as explained above. To assess COVID-19's impact on the program, and knowing that the relation between credit expansion and sales growth during COVID-19 is non-linear (Central Bank of Chile, 2020), we include two dummies for sales growth: a dummy equal to one for positive sales growth (and zero if no growth) and a dummy equal to one for negative sales growth (and zero if no growth). $Other\, Program\, Use_i$ is a dummy equal to one if firm i uses the other government program and is zero otherwise.

[17] 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 in 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.

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

The results are robust to using instead a logit model or a linear probability model.[19] 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 3 reports the selection results for banked firms, for which we have a more accurate measure of risk. 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 6.5 percentage points in the likelihood of using the program ($= 0.647 \times (0.13 - 0.03)$). This represents an increase of 16% relative to the average likelihood of using the program ($= 0.065/0.505$) in this regression sample. After adding industry and municipality fixed effects, the magnitude of the effect declines, indicating significant variation in the risk of firms operating in different industries and located in different municipalities (columns 2–4). That said, we find that even within firms operating in the same industry and located in the same municipality, riskier firms are significantly more likely to use the program.

We also find 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. Firms with either a positive or negative sales growth shock are 19% more likely to use the credit program. That is, guaranteed credit flows equally to firms that are differently hit (within an industry and municipality) by the pandemic. On the other hand, firms that use the employment program are 9.5% more likely to obtain a guaranteed loan, suggesting that the credit and employment programs are used as complementary public policies.

What drives this program participation, supply or demand? The answer is both, and our unique data allow 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). Riskier firms are more likely to apply for a guaranteed loan, suggesting adverse selection into the credit program (Table 4, columns 1–4). The effect declines but remains significant after adding industry and municipality fixed effects. However, conditional on applying to the program,

[19] The risk regressor of Equation (2) is itself an estimated variable (estimated from Equation 1), which could bias 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 (Appendix Table 5). We repeat this procedure for all the other probit regressions that contain risk as an independent variable; we omit reporting those results to save space, but they remain robust.

riskier firms are *less* likely to obtain the loan, indicating that banks screen loans and provide less credit to firms more likely to default (Table 4, columns 5–8). The coefficient for approvals remains unchanged after adding industry and municipality fixed effects, suggesting that banks follow a broad approval policy, across industries and municipalities.

We contrast the results of the credit program with the employment program (Table 3, columns 5–8) using an analogous probit model. Participation in the employment program is much more clearly specific to a pandemic-driven crisis. Firms that suffer negative sales growth are 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.

Furthermore, we find that firms that participate in the credit program are 5.6% more likely to participate in the employment program, confirming a complementarity between both policies. Unlike the credit program, however, after controlling for industry and municipality fixed effects, firms with different risk are equally likely to use the employment program. This indicates that the employment program leads to less adverse selection than the credit program. This result is consistent with the fact that the credit program is very cheap for firms; the real interest rate of guaranteed loans is close to zero. 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.

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. That is, only firms that are up to date with their debt payments are eligible to receive a public guaranteed loan. If we add those ineligible firms to the estimation, we still find that riskier firms are more likely to obtain the guaranteed loan (Table 5, column 2). As expected, the effect of risk is smaller than the baseline effect (reproduced in column 1) because we are comparing the firms that obtain the credit program to a broader sample of firms that includes the riskiest ones in the overall economy. Size is the other eligibility requirement of the program. The mega firms, which are the safest firms in Chile, are ineligible to receive a credit guarantee loan. As expected, when we add mega firms to the estimation, the size of the effect of risk increases (column 3). 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 (column 4).

The selection results remain unchanged when we include both banked and unbanked firms in the estimation (Appendix Table 3). For this regression, we use a different measure of risk. The risk of banked firms is based on real and financial data, while that of unbanked firms is calculated using only real data. To simplify, we report only results with industry and municipality fixed effects. The selection results are also robust to using the ex-ante spread as a simple and direct measure of risk instead of using the predicted default probability from our estimated default probability model (Appendix Table 4).

5.3 Dynamic Lockdowns and Program Selection

The result that firms with both positive and negative variations in sales during the pandemic are more likely to apply and be approved for credit indicates that credit flows across the board regardless of how the pandemic affects firms. To further investigate this issue, we analyze credit distribution to firms in municipalities under lockdown and those in contiguous municipalities with free circulation. To do so, we exploit the dynamically staggered imposition by the sanitary authorities of mandatory lockdowns across municipalities (as explained in Appendix 9). These measures aim to contain the coronavirus as it spreads over time across the country and generates a spatial heterogeneity of exposure to social distancing measures. Appendix Figure 1 presents the map of municipalities according to their overall lockdown status and shows substantial geographical variation. Appendix Figure 2 presents the weekly evolution of the cumulative number of municipalities under lockdown.

At the firm level, the lockdowns produce a plausibly exogenous source of variation of sales since the sales of firms in municipalities affected by the sanitary measures decline substantially (Briones et al., 2021). We thus define the treatment event as the week in which a municipality enters a mandatory lockdown. Treated firms are in municipalities that enter a lockdown at any point during May to July of 2020. Control firms are those in adjacent municipalities that remain open during the same period. The underlying assumption is that firms in adjacent municipalities are similar in terms of risk, firm attributes, and exposure to the pandemic and only differ in how they are affected by an exogenous decision to lock down the community.

To estimate selection into public policies by firms affected by the lockdown measures, we run the following regression:

$$Program\,Use_{it} = \beta Lockdown_i + \gamma Post_{it} + \delta Lockdown_i \times Post_{it} + \alpha_l + \alpha_t + u_{it}. \qquad (3)$$

$Program\,Use_{it}$ is equal to one if firm i participates in a public program in week t and is zero otherwise. $Lockdown_i$ is a dummy equal to one if firm i is in a municipality subject to a lockdown and is zero otherwise. $Post_{it}$ is a dummy equal to one after the firm's municipality enters a lockdown and is zero otherwise. α_l and α_t denotes location and month fixed effects, respectively.[20]

The interaction term between $Lockdown_i$ and $Post_{it}$ is not statistically significant, indicating that firms entering a lockdown are not more likely to use the credit program (Table 6, Panel A, column 1). For the employment program, on the other hand, we observe a positive and significant interaction term: firms in lockdown are significantly more likely to use the employment program (column 2). The fact that firms entering a lockdown are more likely to use the employment program is consistent with our previous selection results in which firms with positive variations in sales during the pandemic are more likely to use the employment program. This likely reflects the different incentives for selection into the programs and the larger opportunity costs for selecting into the employment program for firms not otherwise impacted.

To provide an even sharper 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 hard to distinguish according to many economic characteristics. Importantly, because the virus spreads across municipality boundaries, nearby firms have similar exposure to it. 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) and restrict the sample to firms that run along the municipality border. The results remain unchanged (Table 6, Panel B).

In sum, we find that riskier firms attain public guaranteed credit even though banks are less willing to supply to these firms. We interpret this as a robust measure of credit demand during crises and a response to the supply incentives rather than something particular about a pandemic-driven crisis.

6 Effects on Firm Indebtedness

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

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

$\Delta Debt_i$ is the growth in (net) bank debt during the entire year of 2020, normalized by sales in 2019. This ratio focuses on the change in indebtedness, holding constant sales and thus

[20] We use two different levels of geographic aggregation to perform our analysis: at the region level (where the municipality is located) and at the pair of neighboring municipalities level.

abstracting for the sales decline in 2020.[21] The dummy $ProgramUse_i$ is defined as previously reported. $Risk_i$ corresponds to the fitted value of the firm-level default regression.

Banked and unbanked firms that use the credit program increase their indebtedness by 14.3 and 12.7 percentage points, respectively, relative to non-participating firms (Table 7, columns 1 and 2). These are sizable effects when compared to the initial leverage ratio of 29% for banked firms and 0% for unbanked firms (which by definition have no previous bank debt). Firms with both positive and negative sales growth increase their leverage during 2020, reaffirming that the credit granted is not specific to the pandemic effects on firm performance.

The relation 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 3 and 4). In addition, firms that participate in both programs accumulate less debt. This result suggests that the complementarity between the credit and employment programs, also shown in the selection regressions, helps firms to contain their indebtedness during the crisis.

Additional evidence suggests that the increase in firm indebtedness can be attributed to the credit program per se. To support this claim, we conduct an RDD analysis. As explained in Section 4, 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 at the moment of applying to the program, thus changing its eligibility status in that margin. For size, we focus on annual sales from October 2018 to September 2019 as the running variable. 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.

Figure 4 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 US$35 million that participate in the program is around 30%. Those with annual sales larger than US$35 million 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,

[21] 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 in debt by assets and net worth in 2019 and to normalizing by 2020 sales, assets, and net worth.

reported annual sales might be different from the administrative data we use 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.

Next, we decompose the change in indebtedness into the change in public guaranteed debt and non-guaranteed debt. By construction, public guaranteed debt needs to increase for firms participating in the program. We find that indebtedness coming from public guaranteed debt increases by 13.9 and 11.8 percentage points for banked and unbanked firms, respectively (Table 8, columns 1 and 2). On the other hand, participating in the credit program could lead to higher or lower non-guaranteed debt. We find that indebtedness coming from non-guaranteed debt also increases, although the magnitude of the effect is significantly smaller (columns 3 and 4). In other words, guaranteed and non-guaranteed debt are to some extent complementary to each other during the pandemic.

Having shown that the increase in debt occurs mostly through participating in the credit program, we next study how risk is related to the accumulation of this type of debt. We find that, within credit program users, riskier firms end up with more public guaranteed debt than safer firms, and this holds for both banked and unbanked firms (Table 9, 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 (an expansion of the intensive margin). In contrast, the relation between risk and non-guaranteed debt is negative, significant for banked firms and not significant for unbanked firms (columns 3 and 4). That is, in the absence of a public guarantee, banks lend to safer firms. The results indicate that the existence of a public credit guarantee program changes the way banks allocate credit according to firm risk.

7 Aggregate Implications

In this section, we analyze the aggregate implications of the credit guarantee program. We first study how indebtedness increases for different types of firms and how those firms, in turn, contribute to the rise in aggregate corporate debt during 2020. We then study how the risk that the credit program expansion entails is handled and shared between the banking system that grants the loans and the government that guarantees those loans. While Section 4 uses official aggregate data to display the patterns, here we compute the aggregate statistics from the micro data, allowing us to connect the micro and macro estimates.[22]

7.1 From Firm Indebtedness 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 specific 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 stock of debt of group g of firms, where d_{it} denotes firm-level credit stock. $\omega_{gt-1} = Y_{gt-1}/Y_{t-1}$ 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.[23]

Table 10 presents the results of this aggregation. We first consider the set of credit program users (Panel A). The change in indebtedness for these firms takes into account both program and non-program debt. Riskier firms experience larger within-group changes in indebtedness: the leverage of high-risk firms increases by 10.7 percentage points, while the leverage of low-risk firms increases by 9.18 percentage points (column 1). That said, riskier firms represent a smaller share of aggregate activity: high-risk firms represent only 7.1% of

[22] The size of the aggregate indebtedness from the micro data are a bit smaller than that obtained from the aggregate data because we do not use information on indebtedness for firms that have missing information on sales or have not submitted their data to the tax authority.

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

countrywide sales compared to the low-risk firms that represent 41.2% of aggregate sales (column 2). As a result, the contribution of high-risk firms as a group to overall indebtedness is smaller, 0.11 percentage points, than the contribution of low-risk firms, 0.53 percentage points (column 3). The small weight of riskier firms in the aggregate therefore mitigates the micro adverse selection documented in the previous sections. Summing across all risk groups shows that the indebtedness of firms that use the credit program increases by 1.35 percentage points. This change in debt corresponds to 2.9% of the GDP reported by macro statistics. Those formal firms experience an increase of 3.6% of GDP in guaranteed credit (Table 11) and a decline in non-guaranteed credit.

Next, we extend the analysis to include the non-program users.[24] The leverage of program users increases by 9.71 percentage points during 2020, while the leverage of non-users actually falls by 1.06 percentage points (Panel B). However, given that program users have a low weight in the aggregate economy (13.9% compared to 86.1% of non-users), total indebtedness across all firms increases by only 0.44 percentage points. Last, partitioning all active firms (users and non-users) by risk confirms that the higher the risk, the larger the within-change in debt (Panel C). But the higher the risk, the smaller their weight in the economy, thereby attenuating the increase in aggregate risk.[25] This analysis shows why the micro results presented in previous sections can have a relatively small impact at the macro level.

7.2 Risk Sharing between Banks and the Government

Banks and the government share, absorb, and mitigate the risks that the credit program entails by dealing with the expected default risk from the loans granted, unexpected credit risk, and funding needs for the banks. To understand the risks involved, it helps to measure the magnitude of the program allocation across groups of firms. We focus here on firms by risk category.[26]

Micro data indicate that by the end of 2020, banks provide guaranteed loans worth US$11,467 million, or 4.6% of 2019 GDP, including loans to formal firms and natural persons who borrow as firms under the program (Table 11, Panel B, columns 1 and 2). These loans

[24] We use Equations (5) and (6) to partition firms into credit program users and non-users and measure the contribution of these groups to overall indebtedness.

[25] 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 happen for firms with a relatively small weight at the aggregate level.

[26] As an alternative, Appendix Table 6 reports the results of partitioning firms by size. This is useful because the public guarantee varies by size (from 85% for small firms to 60% for large firms, as explained in Section 4), effectively determining how firms and the government share risk. Furthermore, the size categorization allows us to cover all firms in our sample, permitting a better mapping between the debt levels computed from micro and macro data.

are distributed across firms of different risk categories. High-risk firms receive 7% of the guaranteed loans to formal firms, whereas low- and medium-low-risk firms receive 65% (Panel A, column 3). That is, most of the loans go to safer firms. In fact, the actual distribution of credit under the program matches the weights that firms have in the economy according to sales (Figure 5). Therefore, the aggregate debt allocation across risk groups (including guaranteed and non-guaranteed debt) mentioned in the previous section is basically the same as the share that each risk group receives of the guaranteed credit reported in Table 11. For both total and guaranteed debt, 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.

One risk that pervades the program's loan allocation is the loss from default of different tranches of the portfolio. This risk could be significant because the program targets firms smaller than the typically safe mega firms (even when some precautions are taken to exclude the riskiest firms in the country). 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, defined by the different levels of coverage of the credit guarantee. To do so, we use the coefficients of the default risk model in Section 5 and plug in the 2019 information for the different regressors. This yields predicted default values for 2020 for firms that use the credit program across different categories. As expected, the predicted default probability declines monotonically with risk, going from 18.17% for high-risk firms to 2.05% for low-risk firms (Panel A, column 4).[27]

We then calculate a measure of total credit risk (i.e., expected loss) for each bin size by multiplying the dollar value of program loans by the predicted default probability. As a proportion of GDP, the total credit risk corresponds to an expected credit loss of 0.27% (Panel A, column 8). The contribution to expected loss is similar across risk categories because the larger amounts granted to low-risk firms and their smaller default probability compensate across buckets. When considering all guaranteed loans to formal firms and natural persons, the expected credit loss is 0.44% of GDP (Panel B, column 8), which corresponds to a 9.56% default probability of the guaranteed credit (0.44%/4.6% = 9.56%) (Panel B, column 4). This estimate of expected loss seems large relative to the credit granted because it is conservative. It not only imputes the highest default probability to formal firms with no risk data and to

[27] A negligible fraction of firms do not have sales data and therefore cannot be classified by size. Likewise, a small fraction of firms do not have data to be classified in a risk category. To be conservative, we assume that firms with no sales or risk data (including naturals persons) have the default probability of the riskiest group of firms.

natural persons but also ignores any partial repayment or recovery from firms that eventually default. Still, for a program that channels significant credit to SMEs, it is a useful benchmark to assess the expected loss based on historical default estimates.

In terms of risk sharing between the government and banks, 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.[28] Because of the significantly large first-loss deductible relative to the default probability, banks absorb the vast majority of the expected loss. The total credit risk estimated to be borne by the government is 0.15% of GDP (Panel B, column 9), while that borne by banks is 0.29% of GDP (Panel B, column 10). Thus, two-thirds of the total credit risk derived from the expected loss from default (0.29%/0.44% = 66%) is absorbed by the banks and one-third by the government.

Banks cover for this credit risk by increasing their loan-loss provisions (by roughly US$1 billion). They also contain credit risk by more strongly rejecting applications from larger risky firms, which would be more costly for banks to absorb in case of default because of their size and the lower effective guarantee (Table 12). In fact, the absolute value of the coefficient on risk of bank approvals (in response to loan applications) triples when moving from the small firms to the large ones. That is, banks are more sensitive in their responses to loan applications from large risky firms than to those of small risky firms.

Credit risk has an expected and unexpected component. The expected component is the expected loss from a loan exposure, while the unexpected component is the loss that exceeds the expectation (i.e., tail risk). As explained above, the program's first-loss deductible is large relative to the expected default probability, so banks absorb the vast majority of the expected credit risk. On the other hand, the deductible is small relative to the default probability in a tail event, meaning that the government absorbs most of the unexpected credit risk.

To cover unexpected credit risk, banks hold capital in their balance sheets. Bank regulation requires banks to have a minimum capital adequacy ratio, defined as capital expressed as a percentage of its risk-weighted credit exposures. Assets with higher unexpected risk have higher capital charges, requiring more capital. Basel III regulation in Chile requires banks to comply with this rule by maintaining a capital adequacy ratio above 8%. During

[28] 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), for the analysis, we use the values of the deductible established at the beginning of the program.

2020, the capital adequacy ratio of the banking industry in Chile increases by 1.8 percentage points, from 12.8% in December 2019 to 14.7% in December 2020 (Table 13). This is the result of both an increase in bank capital (the numerator of the ratio) and a decrease in risk-weighted assets (the denominator of the ratio). Regarding the numerator, bank capital rises by 10%, from US$37,514 million to US$41,275 million. This rise is driven, in part, by an increase in loan-loss provisions above the legal minimum. Banks constitute additional provisions that count as bank capital to cover for unexpected credit risks that cannot be easily identified, such as the risk associated with the restructuring of existing debt required by the credit program.

The transfer of unexpected credit risk from banks to the government effectively reduces banks' risk-weighted assets, the denominator of the capital adequacy ratio. Loans are classified in five risk categories, with an associated capital charge depending on its risk. To reflect the government's backing of unexpected risk, the Financial Market Commission allows banks to reclassify the fraction of the loans guaranteed by the government to the risk category 2 (with a capital charge of only 10%), from the previous typical risk weight asset category 5 (with a capital charge of 100%). As a consequence, banks reclassify US$10 billion worth of public guaranteed loans. Risk-weighted assets in category 5 decrease by US$10 billion, while those in category 2 increase by US$1 billion. This reduces the total risk-weighted assets by US$9 billion, out of a total reduction of almost US$11 billion during 2020. In sum, the sharing of unexpected credit risk between banks and the government improves the banks' solvency measures and helps make the banking sector more prepared for unexpected loan defaults.

A third way in which banks and the government as a whole share risk is through the provision of liquidity support to banks. The Central Bank of Chile complements the credit program established by the Ministry of Finance by offering liquidity facilities to banks at a low interest rate.[29] This financing is allocated conditional on the banks actually lending to firms. Consequently, banks do not have to raise funds in capital markets or crowd out other loans. Implicitly, the government takes the risk in case the program fails and banks face solvency problems. Banks use this financing as needed, and the allocation of these funds is mostly based on their demand for it. In fact, the banks that grant more loans under the credit guarantee program use the central bank liquidity facility more intensively (Figure 6).

[29]The liquidity facility of the Central Bank of Chile is named the Facility of Credit Conditional on Lending Increase (FCIC).

8 Model

In this section, we use a simple model of heterogeneous firms' credit and default decisions to help (i) motivate the empirical predictors of risk in our default probability model, (ii) quantify and understand the relation between elements of the credit policy and aggregate outcomes in the context of standard theory, (iii) simulate potential outcomes under a systemic shock, and (iv) more closely quantify the government's burden in a world in which default generally involves partial repayment.

Although we have micro evidence of potential adverse selection (at least based on observables), the key message of our aggregate empirical analysis is that this appears relatively unimportant in the aggregate because most credit goes to relatively safe firms. Hence, we abstract from informational frictions, instead following the existing literature of firm finance with endogenous default (e.g., Cooley and Quadrini, 2001; Gilchrist et al., 2014) and especially Covas and Haan (2012). Moreover, given the suddenness of the pandemic crisis and credit guarantee, we model a simple static problem taking firm equity as given. We calibrate the model to our data on eligible firms, illustrate the expansion and distribution of credit from the program, and quantify the program's impact on aggregate credit and default under various policies and shocks. The latter is helpful because it relaxes a key assumption in our above empirical analysis that default rates follow historical values.

8.1 Firm's Problem

Consider a firm with equity, e, and expected productivity, z, that decides how much to borrow, b, given an interest rate schedule for borrowing, $r_b(b; e, z)$.[30] Its profits (i.e., revenues minus expenses before debt servicing), π, depend on its expected productivity, z, capital (i.e., total assets), $e + b$, and a stochastic revenue shock, ε:

$$\pi = \varepsilon z \left(e + b\right)^{\alpha}, \tag{7}$$

where ε is a random variable with $E(\varepsilon) = 1$, continuously distributed over $\mathbb{R}^+$ according to a cumulative distribution function, $\Phi(\cdot)$. Capital depreciates at rate δ.[31] Given its debt and equity, the firm defaults when its value turns negative:

$$\varepsilon z \left(e + b\right)^{\alpha} + \left(1 - \delta\right)\left(e + b\right) - \left(1 + r_b(b; e, z)\right) b < 0. \tag{8}$$

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

[31] We abstract from explicit labor and material costs. If these costs are a fixed proportion of capital, however, they are easily incorporated into the depreciation term as in our calibration below.

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

$$\underline{\varepsilon}\left(b;e,z\right)=\frac{\left(\delta+r_b(b;e,z)\right)b-\left(1-\delta\right)e}{z\left(e+b\right)^{\alpha}}. \tag{9}$$

Because the user cost of capital, $\delta + r_b(b;e,z)$, is positive, the default threshold is increasing in debt to (expected) revenues, one of our empirical proxies for risk.[32] An immediate corollary is that the probability of default is increasing in this measure of leverage, justifying the use of leverage as an empirical predictor of risk in our default probability model. This is only a partial measure, however, because the pattern is true for a given distribution of ε, $\Phi(\cdot)$.

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

$$\max_{b} \quad \int_{\underline{\varepsilon}(b;e,z)}^{\infty}\left[\varepsilon z\left(e+b\right)^{\alpha}+\left(1-\delta\right)\left(e+b\right)-\left(1+r_b(b;e,z)\right)b\right]\Phi(d\varepsilon).$$

After integrating and substituting in the threshold condition, this simplifies to

$$\max_{b} z\left(e+b\right)^{\alpha}G\left(\underline{\varepsilon}\left(b;e,z\right)\right), \tag{10}$$

with

$$G\left(\underline{\varepsilon}\left(b;e,z\right)\right)\equiv\int_{\underline{\varepsilon}(b;e,z)}^{\infty}\varepsilon\Phi(d\varepsilon)-\left(1-\Phi\left(\underline{\varepsilon}\left(b;e,z\right)\right)\right)\underline{\varepsilon}\left(b;e,z\right). \tag{11}$$

The first term captures the expected profits, while the second captures the value of undepreciated capital net of expected repayment.

8.2 Banks

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

$$\varepsilon z\left(e+b\right)^{\alpha}+\left(1-\delta\right)\left(e+b\right)-\mu z\left(e+b\right)^{\alpha},$$

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

Given perfect competition, loan pricing ensures zero profits:

$$\begin{aligned}\left(1-\Phi(\underline{\varepsilon})\right)\left(1+r_b\right)b+\left(\int_0^{\underline{\varepsilon}}\varepsilon z\left(e+b\right)^{\alpha}\Phi(d\varepsilon)+\Phi\left(\underline{\varepsilon}\right)\left(\left(1-\delta\right)\left(e+b\right)-\mu z\left(e+b\right)^{\alpha}\right)\right)\\ -\left(1+r\right)b=0,\end{aligned} \tag{12}$$

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

$$r_b(b;e,z)=r+\frac{z\left(e+b\right)^{\alpha}}{b}\left(\Phi\left(\underline{\varepsilon}(b;e,z)\right)\mu+\int_0^{\underline{\varepsilon}(b;e,z)}\left(\underline{\varepsilon}(b;e,z)-\varepsilon\right)\Phi(d\varepsilon)\right). \tag{13}$$

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

Here the borrowing rate exceeds the cost of capital by a default risk premium.[33] This justifies our use of the borrowing rate as an additional empirical predictor for risk.

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

8.3 Credit Program

From the firm's perspective, the competitive banking system and supply of credit is captured by Equation (13), which is a constraint in the firm's problem. We model the introduction of the credit program as a comparative static exercise. We introduce key features of the Chilean banking framework and credit program into the model in three fashions.

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

$$r_b(b;e,z) = r + \frac{z\left(e+b\right)^{\alpha}}{b}\left(\Phi\left(\underline{\varepsilon}(b;e,z)\right)\mu + \int_0^{\underline{\varepsilon}(b;e,z)}\left(\underline{\varepsilon}(b;e,z)-\varepsilon\right)\Phi(d\varepsilon)\right) \leq \bar{r}_{b,0}, \quad (14)$$

which effectively defines a limit of available credit b as a function of equity and productivity. Indeed, for low productivity firms, available credit may be zero. A first element of the credit program is a substantially lower interest rate cap on public guaranteed credit, which we model as $\bar{r}_{b,1} < \bar{r}_{b,0}$.

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

$$\begin{aligned} r_b(b;e,z) = r + \frac{z\left(e+b\right)^{\alpha}}{b}\left(\Phi\left(\underline{\varepsilon}(b;e,z)\right)\mu + (1-\chi)\int_0^{\underline{\varepsilon}(b;e,z)}\left(\underline{\varepsilon}(b;e,z)-\varepsilon\right)\Phi(d\varepsilon)\right) \\ \leq \bar{r}_{b,1}. \end{aligned} \quad (15)$$

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

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

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

$$z\left(e+b\right)^{\alpha}\left(1-G\left(\underline{\varepsilon}\left(b;e,z\right)\right)+\left(1-\delta\right)\left(e+b\right)-\mu\Phi\left(\underline{\varepsilon}\left(b;e,z\right)\right)\right)-\left(1+r\right)b=0,$$

where the bank's expected revenues include the share, $1-G\left(\underline{\varepsilon}\left(b;e,z\right)\right)$, that does not go the firm and the undepreciated capital, while its costs include expected default costs and the loan's direct cost.

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

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

Before turning to the quantitative exercise, we calibrate the model using aggregate data, as explained in detail in Appendix 9. But given their relevance, we present here the parameter values related to the credit program. We start with pre-guarantee parameters of $\chi = 0$ (absence of a government guarantee scheme) and $\bar{r}_{b,0} = 0.2$, equal to the regulated maximum interest rate. The multi-tiered partial guarantee scheme in which guarantees vary by firm size is then collapsed to a single guarantee of $\chi = 0.8$. We set $\bar{r}_{b,1} = 0.035$, the statutory maximum interest rate under the program. To capture the overall increased willingness to lend, we lower the cost of intermediated capital by 1.9 percentage points.[34] Other parameters of interest that we calibrate in Appendix 9 are the variance parameter of $\Phi(\cdot)$, σ_ε^2, the variance of log productivity, σ_z^2, the variance of equity, σ_e^2, the mean level of log equity, $\bar{e}$, and the correlation between log productivity and log equity, ρ.

8.4 Credit Program Implications

Moving to the results, Figure 7 illustrates the credit variables for a moderate value of productivity, $z = 1.8$. We plot equity on a log basis to show details for low levels of equity.[35]

[34]This value leads to an increase in lending of about 9.9% under the policy. Though not a perfect comparison, the total lending in the program equals 9.5% of the stock of firm credit in 2019.

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

The left panels illustrate these functions for firms with $\sigma_\varepsilon = 0.22$, essentially the middle of the distribution. The solid dark line illustrates the results for a firm in the benchmark banking system. These firms are overwhelmingly low risk. Default becomes non-negligible only at equity levels below one (top-left panel) because equity acts as both collateral and firm value, deterring default. Nonetheless, risks remain relatively low since the default rates remain below 0.08 (i.e., 8%) even with zero equity for firms with average productivity and risk. The borrowing rates (second-left panel) reflect risk premia, which also remain low. At moderate levels of equity, premia are zero and borrowing rates equal the cost of capital, r, but interest rates do not exceed 6% for these firms.

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

Last, the distribution of credit by equity level (bottom-left panel), constructed by multiplying the debt levels by the density of firms, $H(z, e, \sigma_\varepsilon; \sigma_z^2, \sigma_e^2, \bar{e}, \rho)$, is disproportionately tilted toward safe firms. The equity distribution for this productivity is tightly distributed between 2 and 4, where default rates are low. While low-equity, higher-default risk firms have high leverage, they constitute a negligible share of the total borrowing with substantially more credit going toward safe firms, consistent with the low overall levels of predicted credit-weighted default among eligible firms.

For comparison, Figure 7 also shows the same outcomes for firms with a higher σ_ε of 0.50, about one standard deviation out into the right tail of the revenue uncertainty distribution (right-hand side panels). We choose this far tail for clear illustrative purposes and focus first on the black lines. Default rates are much higher (top-right panel). The borrowing rate now reflects a substantial default risk premium (second-right panel), which declines with equity to the cost of intermediate capital as the default rate goes to zero. Interest rates reflect risk premia and are therefore a good measure of equilibrium risk. Indeed, the flat segment at very low levels of equity in the second-right panel, which is the result of the interest rate cap, is reflected in the default rates of the top-right panel. The default risk and substantial risk premia are higher despite lower levels of debt (third-right panel) and comparable levels of leverage (fourth-right panel). Together, the top and fourth panels display that both risk and leverage decline with equity, showing that leverage is a proxy for default risk. This is consistent with our empirical interest in leverage. Moreover, it is consistent with our empirical results that default is increasing in debt but decreasing in firm value (Table 2, column 5). Importantly, overall credit going to these high-risk firms is quite low (bottom-right panel) since their higher risk level σ_ε is less common.

The dashed lines in Figure 7 show the corresponding patterns under the credit program. The program has very little impact on the default rates of firms with the typical risk profile (top-left panel). The drop in interest rates occurs across the board (second-left panel). Despite no large drop in default risk, this happens to the typical firm because the fall in intermediation costs leads to lending at lower interest rates. The third panel shows that debt expands in response to lower interest rates (third-left panel). The expansion is across a wide range of equity, including previous non-borrowers, and thus the increase in debt includes both an intensive and extensive margin. The fourth panel down shows that leverage correspondingly increases (fourth-left panel). Again, most of the expanded credit is for safe firms (bottom-left panel).

The program has somewhat different impacts on high-risk firms (right panels). Among other things, it affects firms with substantial default risk (second-right panel). The drop in interest rates from the policy is stronger for riskier firms (second-right panel). This is the result of intermediation costs dropping, the guarantee lowering the needed risk premia, and the statutory cap declining (which is binding for many more low-equity firms). The cap induces banks to grant credit to firms with lower default rates than in the benchmark (top-right panel) despite the partial credit guarantee. The statutory rates are simply much lower than they would lend at under the benchmark in order to compensate for high default rates (a concern since the guarantee is only partial). To lower default rates, banks lend substantially less to these high-risk firms. At moderate risk levels (equity levels over 2), however, the default guarantee allows banks to take on more risk among moderate-risk firms.

Thus, given the reduced interest rate cap, debt to the riskiest firms (those with low equity and higher revenue risk) actually declines slightly under the program, though it expands somewhat for those with greater equity (third-right and fourth-right panels). Again, the riskiest firms receive a negligible share of credit under the program (bottom-right panel). The key point is that the program expands credit through lower interest rates stemming from the lower intermediation costs and partial credit guarantee. However, under both the benchmark and program, very little credit goes to firms with high default probability because they are a small share of those borrowing and the interest cap further constrains the riskiest borrowers.

This result from quantitative theory is consistent with the empirical results (Tables 10 and 11).

8.5 Counterfactual Exercises

We then study the model's implications for aggregate credit, average borrowing rates, and ex-post delinquency measures, using the calibrated distribution of firms. For delinquency, we distinguish between (i) expected credit loss as a fraction of total credit (i.e., the expected credit-weighted default rate), (ii) the government's share of expected loss, and (iii) the expected burden on the government from the partial guarantee. The second measure is most comparable to those in column 9 in Table 11 (but with total credit as the denominator rather than GDP), and given our simplified guarantee, it is calculated as simply 80% of expected credit loss. The final measure is the true burden of the government that accounts for the fact that even defaulted credit has partial repayment, and the guarantee is only for the unpaid portion.

We assess these ex-post measures under two scenarios: (i) the typical year's repayment used for the model calibration (and therefore lending decisions) and (ii) an unanticipated systemic shock to the distribution of productivity after credit is already granted.[36] That is, we consider a combination of first and second moment shocks by lowering the productivity distribution's log mean by 4.2 log points and increasing the log standard deviation by 40%.[37] These values capture a large but reasonable shock to the economy. Moreover, our combined shock lowers the value added of eligible firms by 4.9%. Such a drop in annual output is not outside of the Chilean experience.[38]

We then present the quantitative predictions under the combined government policy and assess the contributions of the willingness to lend, the interest rate cap, and the credit guarantee by eliminating them one at a time (Table 14). The combined policy leads to a 9.8% increase in overall credit. Comparing across columns shows that "no increased willingness to lend" and the credit guarantee both contribute to this increase as eliminating them lowers credit. On the contrary, the reduction in the interest rate cap acts to limit the increase in credit. The increase in credit is accompanied by a 2.5 percentage point lower average interest rate, with all three components of the policy driving it.

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

[37] Xiao (2020) estimates that the second moment of the productivity distribution increases by 40% during the Great Recession for the United States in a model similar to ours.

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

The key message of the model is that despite the increased credit, the policy does not increase risk substantially in a typical year (Table 14, middle rows). Indeed, expected credit loss actually falls 0.3 percentage points under the policy. The government's share of total expected credit loss is 2.9% of total credit. Since program credit amounted to roughly 5% of GDP, this is of comparable magnitude to the expected loss of 0.27% relative to GDP for formal firms in the purely empirical calculations (Table 11, column 8). Nevertheless, the model predicts that the true burden on the government is an order of magnitude smaller because much of the debt in default is still repaid (again, an implication of comparable interest rates and credit-weighted default rates.) Comparing across the components shows that the lower interest rate cap on program loans plays an important role in mitigating risks: it prevents high interest rate loans that would expand credit to the riskiest firms. Without the reduction in the interest rate cap, the expected credit loss would be about 50% higher (4.5% versus 2.9%).

Last, we come to our experiments with a systemic shock. Delinquency with the policy can be considerably higher if a large systemic shock is experienced ex-post (Table 14, bottom rows). The systemic shock itself is quite important, increasing expected credit loss to 12.7% in the benchmark. Still, we see that the policy itself does not increase the share of expected credit loss since credit flows disproportionately to safe firms. However, given the higher expected loss share, the government burden under the guarantee program is substantial. The 10% value implies a threefold increase over those in a typical year. Nevertheless, the actual burden of 1.2% remains an order of magnitude smaller. These burdens are all a result of the credit guarantee, which only has mild impacts in the model simulations. The key point here is that the model implies that a large systemic shock is necessary for the program to become substantially more costly because of low repayment.

9 Conclusions

This paper uses a large-scale episode of crisis credit together with unique financial and real data for the universe of formal firms and banks in Chile to shed light on the distribution of credit and the implied potential financial risks. The program implemented increases credit rapidly, substantially, and broadly to a wide class of firms. Even though the program suffers from adverse selection, in which higher risk firms disproportionately borrow, we find that most credit is given to relatively safe firms and aggregate risks remain low.

The evidence suggests that our findings are not particular to the COVID-19 pandemic, and so we draw general lessons about circumstances and policy actions that can limit risk while still rapidly and broadly expanding credit. The loose credit conditions inevitably generate incentives for risky firms to obtain credit at low cost (the degree of adverse selection we observe in the credit program is absent in the more costly employment program). But adverse selection can also be mitigated, by design or in practice. The highest risk firms can be effectively excluded through simple eligibility rules and low interest rate caps. When overall default rates are low, especially among the largest borrowers, most credit granted under easy lending policies is naturally distributed to larger and safer firms, even if riskier firms lever up the most. Government guarantees of tail credit risk can motivate banks to quickly dispense credit and engage with risky clients, and yet when such guarantees are partial, banks have incentives to still provide effective screening. We find these lessons in the example of Chile, but we believe that they represent some of the best guidance for governments that existing evidence and theory offer.

Our findings also have limitations and certainly suggest further avenues for research. First and foremost, we quantify the changes in leverage and aggregate risks that are the result of crisis credit. We presume that there are benefits justifying such an intervention, but we do not measure, quantify, or explicitly model these. A cost-benefit evaluation of crisis credit policies clearly requires such analysis. Related, we allude to, but not explicitly consider, the intertemporal aspects of the trade-offs that governments face between immediately saving firms and potentially slowing growth or recovery. Second, we discuss the distribution of credit across classes of firms for a policy with limited targeting, but we do not analyze the distributional consequences across classes or distributions of workers and/or consumers. In principle, the matched employer-employee feature of our data enables this, which can be important for follow-up work. Third, our data explicitly cover the formal sector, and we have direct risk evidence only for those firms with borrowing histories. The former constitutes the bulk of the economy in Chile, and the latter absorbs the bulk of crisis credit. Nevertheless, in many economies, informal sectors and unbanked firms can be quite prominent, which can limit the effectiveness of such programs. Fourth, 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 credit to underserved sectors.

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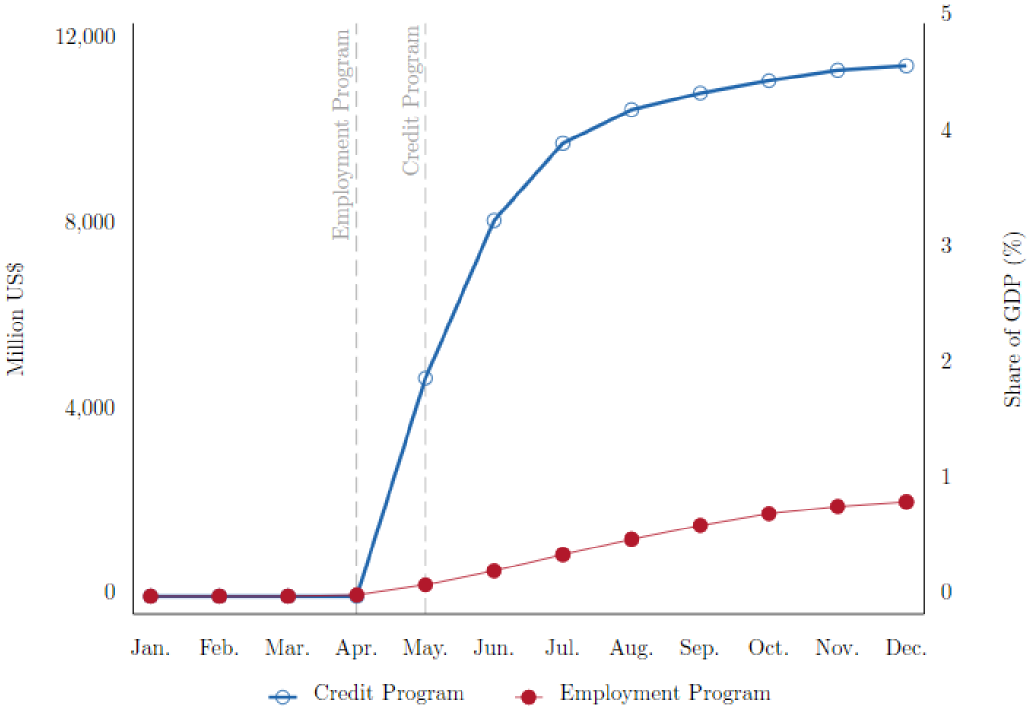
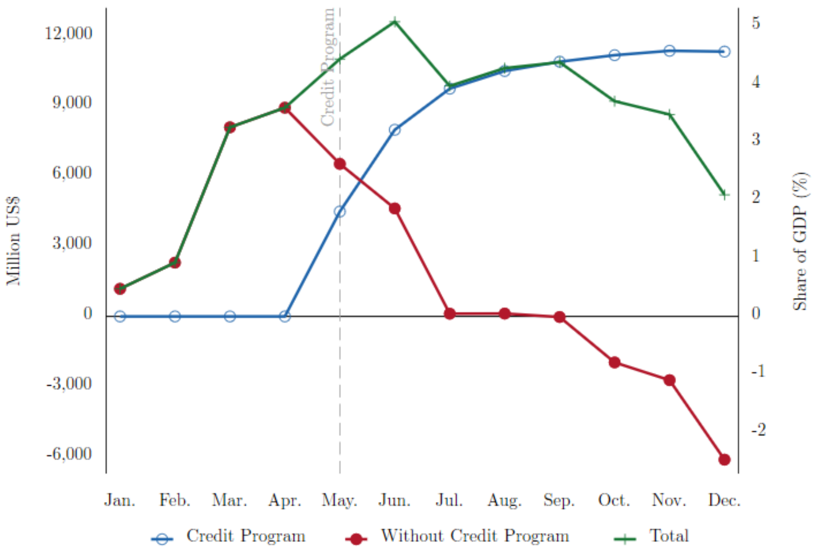
Figure 1
Size of Public Programs

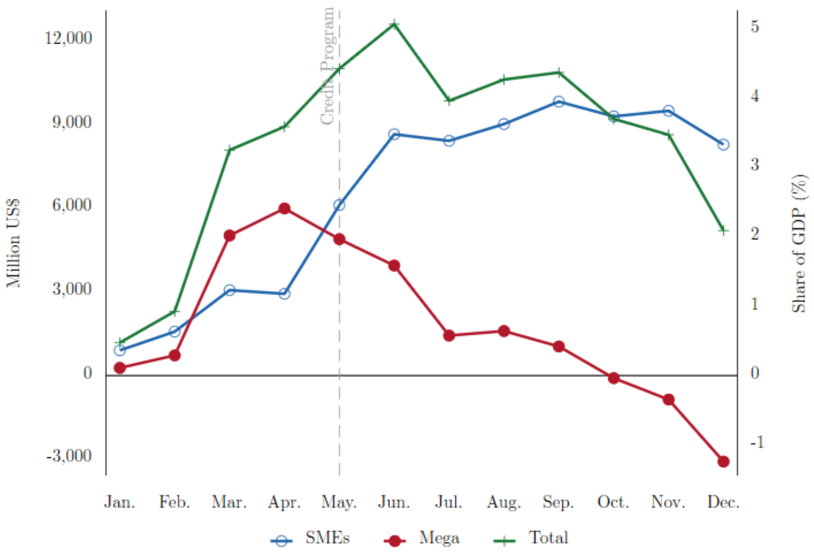


This figure plots the size of the public programs implemented in Chile during 2020 to support firms, in million US$ (left axis) and as a share of GDP (right axis). The size of the credit program is the cumulative amount of guaranteed credit disbursed by the end of each month; the size of the employment program is the cumulative value of the wage bill saved by firms participating in the employment program at the end of each month. The dotted vertical lines show the month in which each program is implemented. The figure uses natural persons and the formal firms sample.

Figure 2
Evolution of Total Corporate Credit Granted



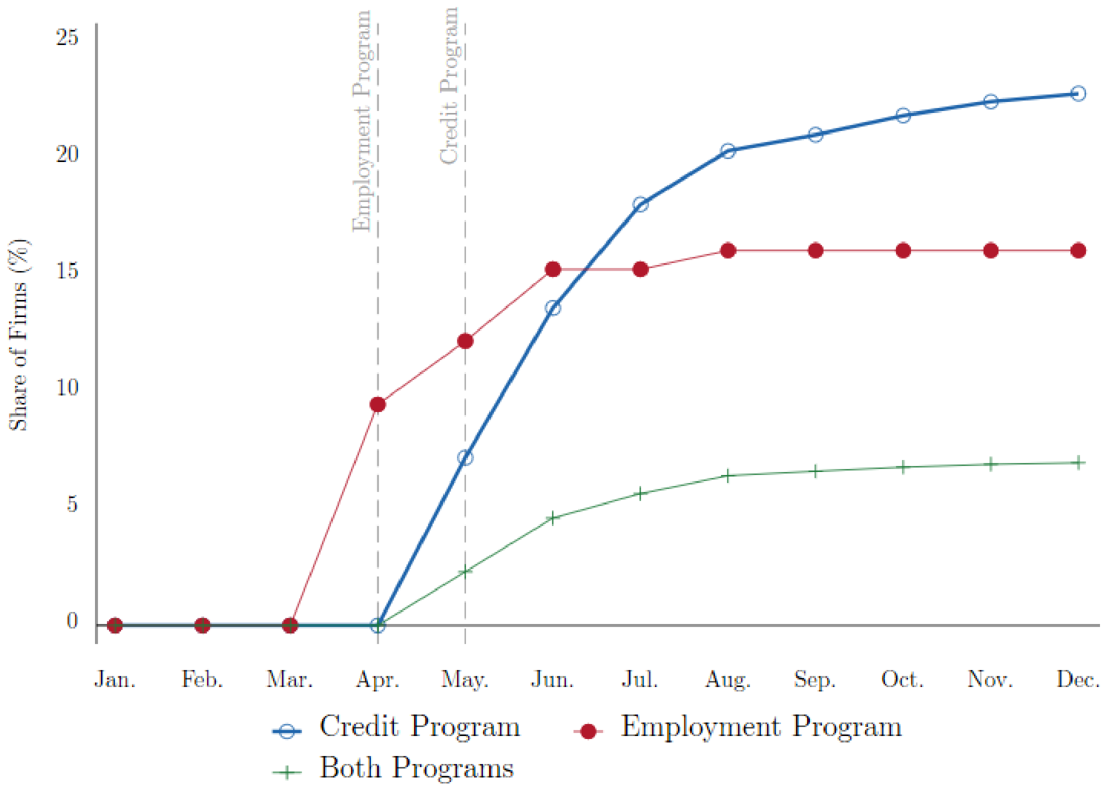
(a) Guaranteed and Non-Guaranteed Credit



(b) By Firm Size

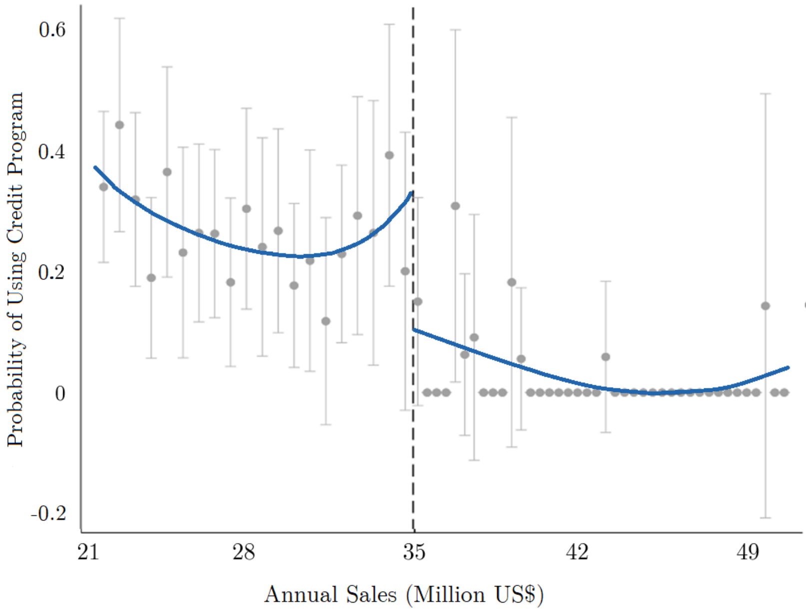
This figure plots the cumulative corporate credit granted in Chile during 2020, in million US$ (left axis) and as a share of GDP (right axis). Cumulative credit is equal to the difference between the credit stock in a given month of 2020 and the credit stock in December 2019. The top panel decomposes total credit into credit guaranteed under the credit program and credit outside the program. The bottom panel decomposes total credit into credit granted to SMEs and large firms (which are eligible for the credit program) and to mega firms (ineligible for the program). The dotted vertical line shows the month in which the credit program is implemented. The figure uses natural persons and the formal firms sample.

Figure 3
Share of Firms Using Public Programs

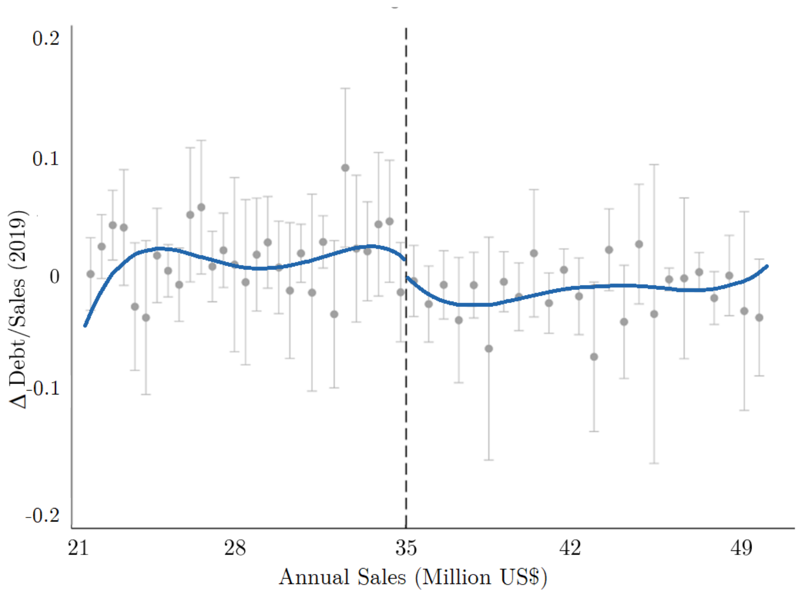


This figure plots the cumulative share of firms using the credit program, the employment program, and both programs by the end of each month during 2020. The share of firms is calculated relative to the number of eligible firms for each program. The dotted vertical lines show the month in which each program is implemented. The figure uses the credit program eligible firms of the active firms sample.

Figure 4
Consequences of Being Eligible for the Credit Program



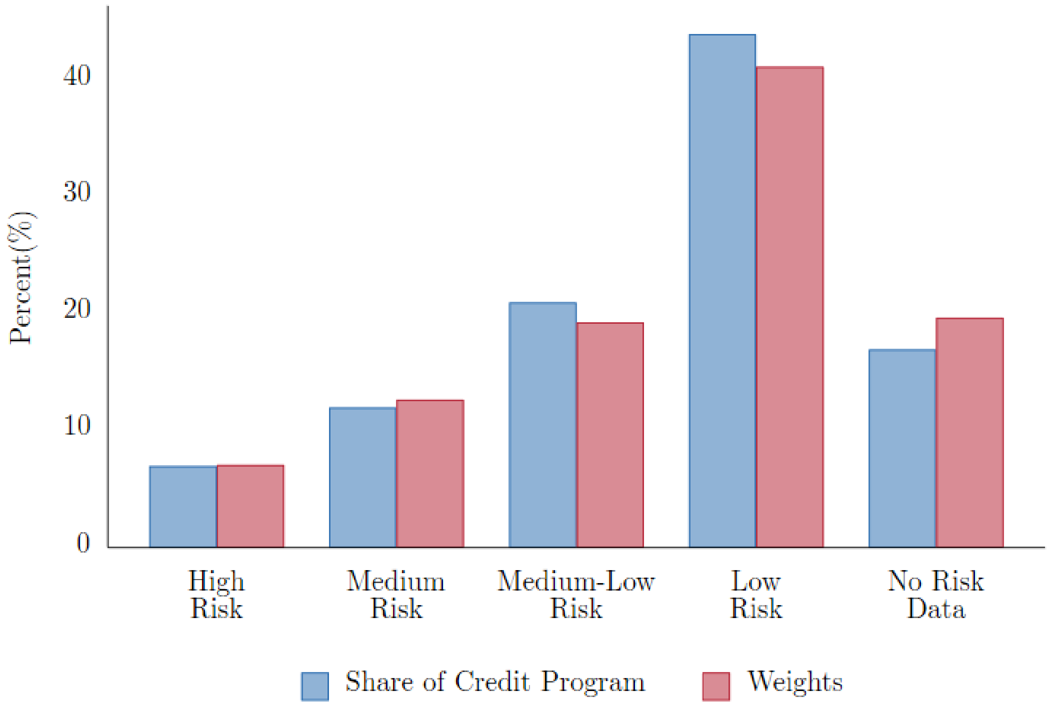
(a) Effects on Program's Take-Up



(b) Effects on Firm Leverage

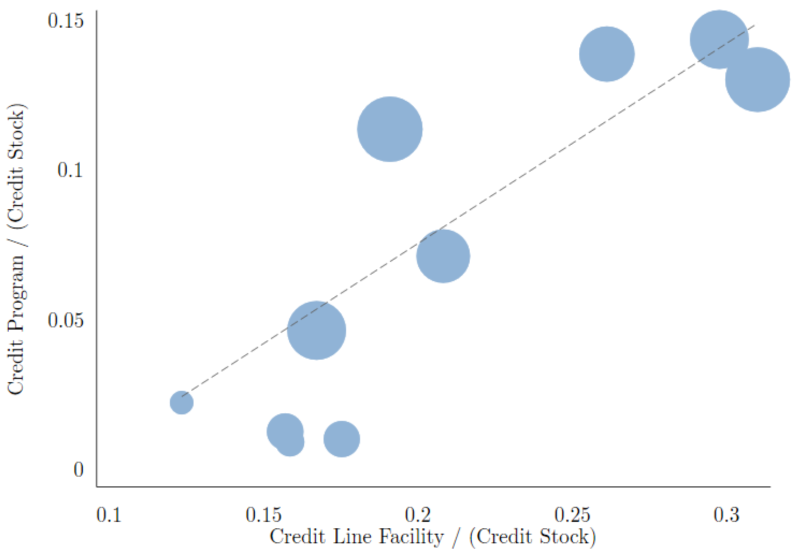
This figure plots the effects of firm eligibility for the credit program on the probability of using the program (top panel) and on firm leverage (bottom panel). The estimates are obtained from a regression discontinuity design (RDD) around the size eligibility threshold for the program of US$35 million in sales (between October 2018 and September 2019). Leverage is the change in the credit stock between December 2020 and December 2019, relative to 2019 sales. The dotted vertical line shows the size eligibility threshold. The figure uses the selection and leverage models sample.

Figure 5
Share of Credit Program and Weights

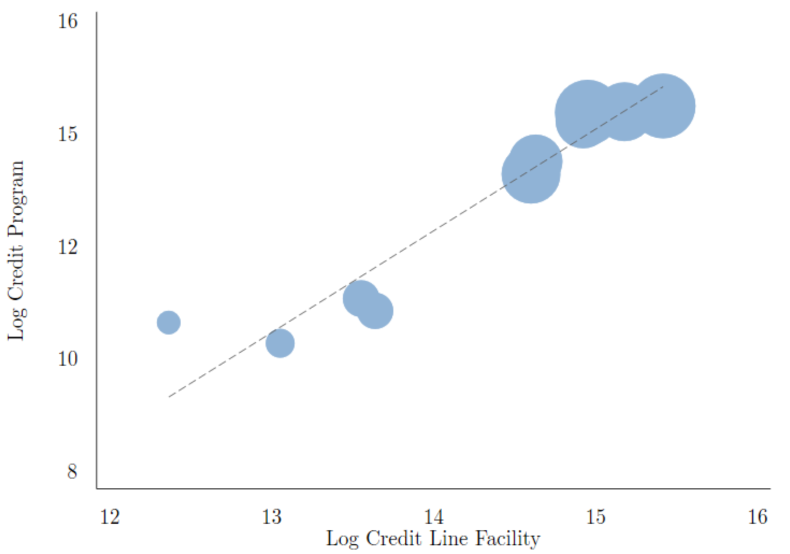


This figure shows the distribution of the credit program displayed in Table 11, Panel A, column 3, and the weights of the different risk groups displayed in Table 10, Panel A, column 2 across risk categories.

Figure 6
Relation between Liquidity Support and Guaranteed Loans



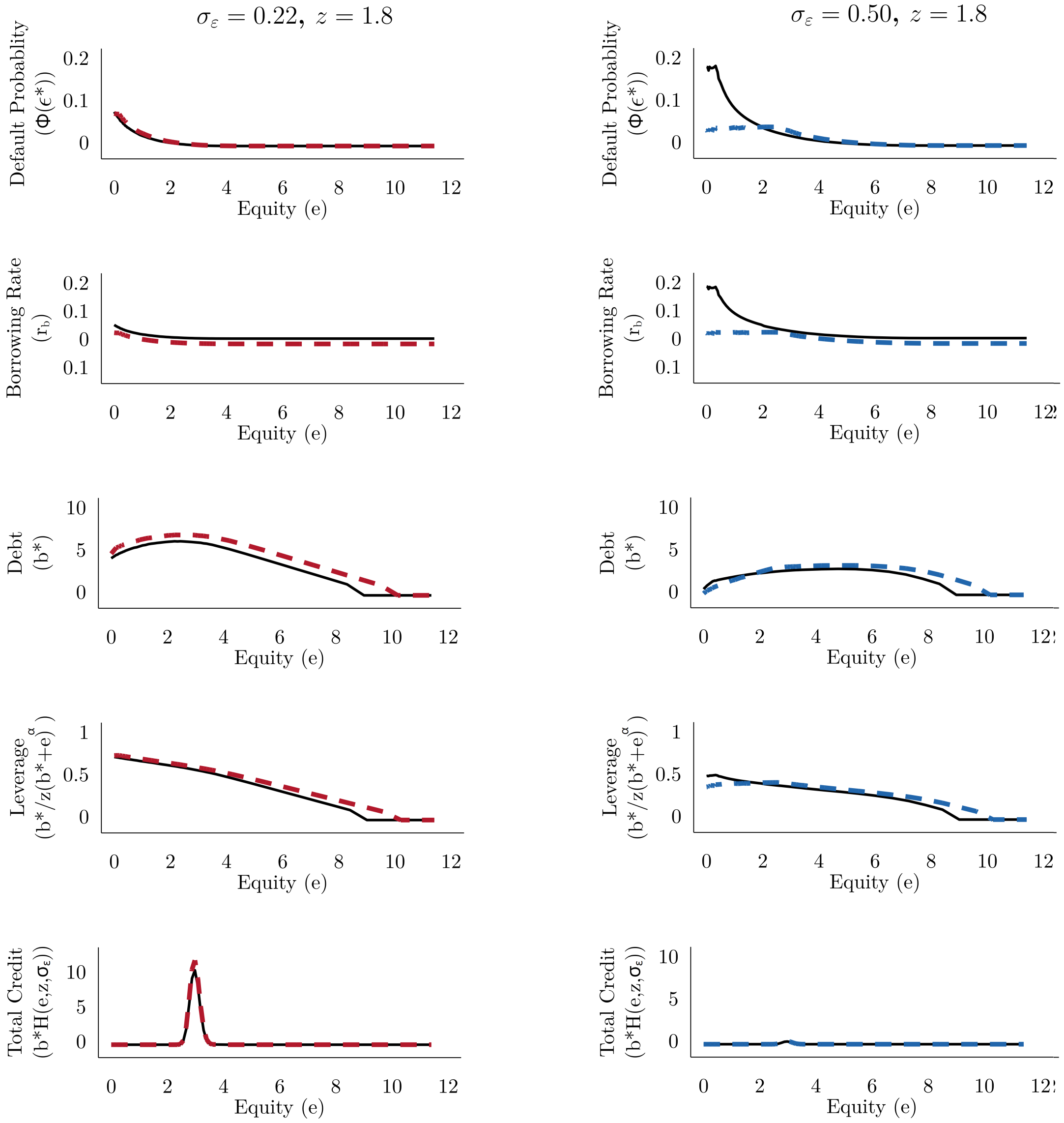
(a)



(b)

This figure shows the relation between the amount of liquidity support received by banks from the Central Bank of Chile (through its credit line facility) and the total amount of guaranteed credit provided by banks. The top panel normalizes the variables by the credit stock of each bank as of December 2019. The bottom panel uses the log of the variables. Each bubble represents a bank, and the size of the bubble represents total credit stock granted by the bank as of December 2019. The figure uses publicly available information from the Financial Market Commission of Chile and the Central Bank of Chile.

Figure 7
Simulated Model



This figure shows borrowing outcomes (y-axis) as a function of equity (x-axis) for firms with two different values of uncertainty (or equity-independent risk), σ. Both panels are for a firm with moderate productivity, $z = 1.8$, but the panels on the left have a moderate level of uncertainty, $\sigma = 0.22$, while the panels on the right are for a higher uncertainty, $\sigma = 0.50$. The solid dark line illustrates the results for a firm in the benchmark banking system. The dashed line illustrates the results under the counterfactual policy credit program.

Table 1
Summary Statistics

	(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	602,874	100	100	100	100
Active Firms	449,615	75	92	82	100
Credit Program Eligible Firms	434,394				
Credit Program Users	102,688				
Panel B: Firms with Observables for Firm-Level Estimations					
Default Model	96,424	16	61	51	67
Selection and Leverage Model	119,153	18	50	44	74
Banked Firms	67,240				
Unbanked Firms	51,913				
Credit Program Eligible Firms	114,606	17	35	21	19
Banked Firms	62,927				
Unbanked Firms	51,679				
Credit Program Users	40,901	6	14	9	7
Banked Firms	31,782				
Unbanked Firms	9,119				

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. Banked firms are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019.

Table 2
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)	-0.009^{***}	-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^{***}	0.002^{**}	0.002^{***}	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(Credit Stock)					0.013^{***}	0.013^{***}	0.013^{***}	0.013^{***}
					(0.001)	(0.001)	(0.001)	(0.001)
Spread Ex-Ante					0.003^{***}	0.003^{***}	0.003^{***}	0.003^{***}
					(0.000)	(0.000)	(0.000)	(0.000)
Dependent Variable Mean	0.088	0.088	0.088	0.088	0.088	0.088	0.088	0.088
Dependent Variable Std. Dev.	0.284	0.284	0.284	0.284	0.284	0.284	0.284	0.284
Number of Firms	96,424	96,424	96,424	96,424	96,424	96,424	96,424	96,424
R^2	0.051	0.061	0.064	0.073	0.095	0.103	0.104	0.112
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								
Banked Firms	0.088	0.088	0.088	0.088	0.089	0.089	0.089	0.089
Unbanked Firms	0.113	0.113	0.107	0.107				

This table reports probit estimations of the probability of a banked firm defaulting on a loan on a set of firm-level characteristics (Panel A) and the resulting predicted default probabilities for banked and unbanked firms (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. Banked firms are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Columns 1–4 include real characteristics, and columns 5–8 add financial characteristics. Columns 1–8 include different sets of fixed effects (FE). Standard errors, shown in parentheses, are clustered at the industry and municipality level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3

Probability of Firms Using Public Programs

	Use Credit Program				Use Employment Program			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Risk	0.647***	0.536***	0.400***	0.337***	0.084***	0.071***	−0.013	−0.016
	(0.043)	(0.035)	(0.033)	(0.034)	(0.025)	(0.023)	(0.023)	(0.024)
Increase in Sales Dummy	0.216***	0.211***	0.197***	0.195***	0.046***	0.047***	0.052***	0.053***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
Decrease in Sales Dummy	0.210***	0.205***	0.195***	0.193***	0.119***	0.118***	0.112***	0.112***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
Use Employment Program	0.098***	0.102***	0.088***	0.095***				
	(0.005)	(0.005)	(0.005)	(0.005)				
Use Credit Program					0.059***	0.061***	0.053***	0.056***
					(0.003)	(0.003)	(0.003)	(0.003)
Dependent Variable Mean	0.505	0.505	0.505	0.505	0.183	0.185	0.183	0.185
Dependent Variable Std. Dev.	0.500	0.500	0.500	0.500	0.387	0.388	0.387	0.389
Number of Firms	62,927	62,881	62,917	62,871	62,927	62,222	62,803	62,102
R^2	0.020	0.031	0.036	0.045	0.019	0.055	0.052	0.081
Industry FE	No	No	Yes	Yes	No	No	Yes	Yes
Municipality FE	No	Yes	No	Yes	No	Yes	No	Yes
Predicted Default Probability								
Banked Firms	0.084	0.084	0.084	0.084	0.084	0.084	0.084	0.084

This table reports probit estimations of the probability of a banked firm using a government program on a set of firm-level characteristics for the credit program eligible firms sample. Banked firms 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 participates in the credit program (columns 1–4), is equal to one if the firm participates in the employment program (columns 5–8), and is zero otherwise. Risk corresponds to the fitted values of the regression specification reported in Table 2, column 8. Increase (decrease) in sales dummy is equal to one if the firm experiences a positive (negative) variation in sales between February 2020 and April 2020. Use credit program and employment program are dummy variables equal to one for program participation and are zero otherwise. Some observations are dropped when estimating the model with fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4
Probability of Firms Using the Credit Program: Applications versus Approvals

	Credit Program Applications				Credit Program Approvals			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Risk	0.845***	0.733***	0.599***	0.538***	−0.222***	−0.245***	−0.226***	−0.257***
	(0.048)	(0.037)	(0.034)	(0.035)	(0.018)	(0.019)	(0.02)	(0.021)
Increase in Sales Dummy	0.206***	0.201***	0.189***	0.186***	0.019***	0.020***	0.018***	0.019***
	(0.007)	(0.008)	(0.008)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)
Decrease in Sales Dummy	0.204***	0.198***	0.190***	0.188***	0.019***	0.020***	0.019***	0.019***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Use Employment Program	0.122***	0.126***	0.111***	0.117***	−0.011***	−0.011***	−0.011***	−0.010***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Dependent Variable Mean	0.649	0.649	0.649	0.649	0.920	0.918	0.919	0.918
Dependent Variable Std. Dev.	0.477	0.477	0.477	0.477	0.272	0.274	0.272	0.275
Number of Firms	62,927	62,862	62,913	62,848	36,701	36,025	36,593	35,918
R^2	0.032	0.045	0.050	0.061	0.008	0.024	0.016	0.033
Industry FE	No	No	Yes	Yes	No	No	Yes	Yes
Municipality FE	No	Yes	No	Yes	No	Yes	No	Yes
Predicted Default Probability								
Banked Firms	0.084	0.084	0.084	0.084	0.090	0.090	0.090	0.090

This table reports probit estimations of the probability of a banked firm applying for the credit program and the probability of getting approved on a set of firm-level characteristics for the credit program eligible firms sample. The dependent variable is equal to one if the firm applies for a guaranteed loan (columns 1–4), is equal to one if the firm's loan application is approved, conditional on having applied (columns 5–8), and is zero otherwise. Risk corresponds to the fitted values of the regression specification reported in Table 2, column 8. Increase (decrease) in sales dummy is equal to one if the firm experiences a positive (negative) variation in sales between February 2020 and April 2020 and is zero otherwise. Use employment program is a dummy variable equal to one for employment program participation and is zero otherwise. Some observations are dropped when estimating the model with fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5
Probability of Firms Using the Credit Guarantee: Different Samples

	Use Credit Program			
	(1) Only Eligible Firms	(2) Eligible Firms + Firms with Past Due Payment	(3) Eligible Firms + Mega Firms	(4) All Firms
Risk	0.337***	0.084***	0.412***	0.147***
	(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	62,871	66,407	63,758	67,240
R^2	0.045	0.039	0.048	0.043
Industry FE and Municipality FE	Yes	Yes	Yes	Yes
Predicted Default Probability				
Banked Firms	0.084	0.087	0.083	0.086

This table reports probit estimations of the probability of a banked firm 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. 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 2, column 8. Increase (decrease) in sales dummy is equal to one if the firm experiences a positive (negative) variation in sales between February 2020 and April 2020 and is zero otherwise. 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6
Probability of Firms Using Public Programs: Dynamic Lockdowns

	(1) Use Credit Program	(2) Use Employment Program
Panel A: All Firms in Municipality		
Post	0.025***	−0.009***
	(0.004)	(0.001)
Lockdown	−0.002	0.022
	(0.002)	(0.014)
Lockdown × Post	0.005	0.019***
	(0.003)	(0.000)
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	0.028***	0.002
	(0.003)	(0.004)
Lockdown	0.090***	0.068***
	(0.005)	(0.003)
Lockdown × Post	0.007	0.028***
	(0.008)	(0.005)
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 obtains a guaranteed loan (column 1) and a dummy variable equal to one if a firm uses employment protection (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 is 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7
Firm Indebtedness and Use of Public Programs

	Δ Debt / Sales (2019)			
	(1)	(2)	(3)	(4)
	Banked	Unbanked	Banked	Unbanked
Use Credit Program	0.143***	0.127***	0.145***	0.130***
	(0.001)	(0.001)	(0.001)	(0.001)
Use Employment Program			0.008***	0.002**
			(0.002)	(0.001)
Use Credit Program			−0.013***	−0.016***
× Employment Program			(0.003)	(0.003)
Increase in Sales Dummy	0.021***	0.004***	0.021***	0.004***
	(0.003)	(0.001)	(0.003)	(0.001)
Decrease in Sales Dummy	0.017***	0.002***	0.017***	0.002*
	(0.003)	(0.001)	(0.003)	(0.001)
Debt (2019)/Sales (2019)	0.290	0.000	0.290	0.000
Dependent Variable Mean	0.054	0.028	0.054	0.028
Dependent Variable Std. Dev.	0.172	0.082	0.172	0.082
Number of Firms	62,927	51,679	62,927	51,679
R^2	0.191	0.359	0.191	0.359
Industry FE and Municipality FE	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' leverage on a set of firm-level characteristics, separately for banked and unbanked firms, for the credit program eligible firms sample. Banked firms 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 the change in the credit stock 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 is a dummy variable equal to one if the firm experiences a positive (negative) variation in sales between February 2020 and April 2020 and is zero otherwise. All columns include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. For each dependent variable, observations in the top and bottom 1% of the distribution are dropped. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8
Firm Indebtedness and Use of Public Programs:
Guaranteed versus Non-Guaranteed Debt

	Δ Guaranteed Debt / Sales (2019)		Δ Non-Guaranteed Debt / Sales (2019)	
	(1)	(2)	(3)	(4)
	Banked	Unbanked	Banked	Unbanked
Use Credit Program	0.139***	0.118***	0.008***	0.011***
	(0.000)	(0.001)	(0.001)	(0.001)
Use Employment Program	0.001***	0.000*	0.008***	0.001***
	(0.000)	(0.000)	(0.002)	(0.001)
Use Credit Program	−0.003**	−0.009***	−0.010**	−0.006***
× Use Employment Program	(0.001)	(0.002)	(0.003)	(0.001)
Increase in Sales Dummy	−0.001	0.001**	0.023***	0.002***
	(0.001)	(0.000)	(0.003)	(0.001)
Decrease in Sales Dummy	−0.002***	0.000	0.021***	0.002***
	(0.001)	(0.000)	(0.003)	(0.000)
Dependent Variable Mean	0.070	0.020	-0.018	0.007
Dependent Variable Std. Dev.	0.087	0.055	0.140	0.045
Number of Firms	62,927	51,679	62,927	51,679
R^2	0.628	0.645	0.021	0.020
Industry FE and Municipality FE	Yes	Yes	Yes	Yes

This table reports linear regressions of the change in firms' guaranteed and non-guaranteed debt on a set of firm-level characteristics, separately for banked and unbanked firms, for the credit program eligible firms sample. Banked firms 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. Use credit program and employment program are dummy variables for program participation. Increase (decrease) in sales dummy is equal to one if the firm experiences a positive (negative) variation in sales between February 2020 and April 2020 and is zero otherwise. All columns include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. For each dependent variable, observations in the top and bottom 1% of the distribution are dropped. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9
Firm Indebtedness and Risk among Credit Program Users

	Δ Guaranteed Debt / Sales (2019)		Δ Non-Guaranteed Debt / Sales (2019)	
	(1) Banked	(2) Unbanked	(3) Banked	(4) Unbanked
Risk	0.095***	0.171***	−0.065***	−0.020
	(0.007)	(0.019)	(0.011)	(0.014)
Increase in Sales Dummy	−0.003	0.010**	0.007*	0.006**
	(0.002)	(0.004)	(0.004)	(0.003)
Decrease in Sales Dummy	−0.007***	0.004	0.004	0.004
	(0.002)	(0.004)	(0.004)	(0.003)
Dependent Variable Mean	0.138	0.116	-0.013	0.015
Dependent Variable Std. Dev.	0.076	0.079	0.128	0.062
Number of Firms	31,782	9,119	31,782	9,119
R^2	0.033	0.091	0.029	0.066
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 banked and unbanked firms, for the credit program users sample. Banked firms 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 2 for unbanked and banked firms, respectively. Increase (decrease) in sales dummy is equal to one if the firm experiences a positive (negative) variation in sales between February 2020 and April 2020 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 each dependent variable, observations in the top and bottom 1% of the distribution are dropped. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 10

Changes in Aggregate Firm Indebtedness

	Δ Debt / Sales (2019)		Δ Debt / Sales (2019)
	(1)	(2)	(3)
	Within Change (p.p.)	Weights (%)	Group Change (= (1) × (2)) (p.p.)
	0.4		
Panel A: Risk Groups (Credit Program Users)			
High Risk	10.70	7.1	0.11
Medium Risk	9.29	12.7	0.16
Medium-Low Risk	9.70	19.3	0.26
Low Risk	9.18	41.2	0.53
No Risk Data	10.75	19.7	0.29
Total		100.0	1.35
Panel B: Use of the Credit Program (Active Firms)			
Users	9.71	13.9	1.35
Non-Users	−1.06	86.1	−0.91
Total		100.0	0.44
Panel C: Risk Groups (Active Firms)			
High Risk	4.34	1.8	0.08
Medium Risk	3.18	4.1	0.13
Medium-Low Risk	2.26	8.4	0.19
Low Risk	−0.15	59.3	−0.09
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 between credit program users and non-users. Panel C 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 2, 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.

Table 11
Expected Loss for the Banking Industry and the Government

	(1) Total Credit Program (Million US$)	(2) Total Credit Program/GDP (%)	(3) Share of Credit Program (%)	(4) Default Probability (%)	(5) Guarantee (%)	(6) Deductible (%)	(7) Effective Guarantee (%)	(8) Expected Loss/GDP (=(2)×(4)) (%)	(9) Government's Expected Loss/GDP (=(7)×(8)) (%)	(10) Banks' Expected Loss/GDP (=(8)-(9)) (%)
Panel A: Risk Groups, Formal Firms										
High Risk	606	0.2	7	18.17	82.5	4.4	35.8	0.04	0.01	0.03
Medium Risk	1,086	0.4	12	9.86	79.9	4.0	32.3	0.04	0.01	0.03
Medium-Low Risk	1,868	0.8	21	5.68	77.0	3.5	28.2	0.05	0.01	0.04
Low Risk	3,975	1.6	44	2.05	72.1	3.0	21.1	0.03	0.01	0.03
No Risk Data	1,489	0.6	17	18.17	82.5	4.4	35.8	0.11	0.04	0.07
Total: Formal Firms	9,022	3.6	100	7.48	76.5	3.5	27.3	0.27	0.09	0.18
Panel B: Formal Firms + Natural Persons										
Total Formal Firms	9,022	3.6	79	7.48	76.5	3.5	27.3	0.27	0.09	0.18
Natural Persons	2,445	1.0	21	18.17	82.5	4.4	35.8	0.17	0.06	0.11
Total: Formal Firms + Natural Persons	11,467	4.6	100	9.76	77.6	3.7	29.1	0.44	0.15	0.29

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 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 2. Columns 5 and 6 show the guarantee and the first-loss deductible for each category, while column 7 shows the effective guarantee after taking into account the deductible. Columns 8, 9, and 10 show, for each category, the total expected loss as share of GDP (column 2 times column 4), the fraction borne by the government (column 7 times column 8), and the fraction borne by the banking sector (column 8 minus column 9), respectively. Values in columns 4–7 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 4–7 are calculated as the sum of the product of each category's statistic by its relative weight (column 3). Firms are classified across risk categories based on the distribution quartiles of the fitted values of the regression specifications reported in Table 2, columns 4 and 8. Firms with missing risk category are assigned the risk from the high-risk category.

Table 12
Probability of Different Firms Getting Approval for the Credit Program

	Credit Program Approvals			
	(1) All Firms	(2) Small Firms	(3) Medium Firms	(4) Large Firms
Risk	−0.257***	−0.246***	−0.439***	−0.755***
	(0.021)	(0.025)	(0.082)	(0.238)
Increase in Sales Dummy	0.019***	0.022***	0.008	−0.010
	(0.006)	(0.008)	(0.019)	(0.035)
Decrease in Sales Dummy	0.019***	0.022***	0.005	0.002
	(0.006)	(0.007)	(0.019)	(0.034)
Use Employment Program	−0.010***	−0.008*	−0.015*	−0.026
	(0.004)	(0.004)	(0.008)	(0.020)
Dependent Variable Mean	0.918	0.913	0.918	0.902
Dependent Variable Std. Dev.	0.275	0.282	0.275	0.298
Number of Firms	35,918	26,623	5,916	1,392
R^2	0.033	0.036	0.082	0.171
Industry FE and Municipality FE	Yes	Yes	Yes	Yes
Predicted Default Probability				
Banked Firms	0.090	0.102	0.061	0.036

This table reports probit estimations of the probability of a banked firm getting approved for the credit 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's loan application is approved and is zero otherwise. Columns 1, 2, 3, and 4 correspond to all banked firms, small firms, medium firms, and large firms, respectively. Risk corresponds to the fitted values of the regression specification reported in Table 2, column 8. Increase (decrease) in sales dummy is equal to one if the firm experiences a positive (negative) variation in sales between February 2020 and April 2020 and is zero otherwise. 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 13
Capital Adequacy Indicator of the Banking Industry

	2019	2020	Change
Regulatory Capital/ Total Risk-Weighted Assets (RWA)	12.8%	14.7%	1.8%
Panel A: Regulatory Capital (Million US$)			
Common Equity Tier 1	28,645	30,163	1,519
Subordinated Bonds	8,050	9,423	1,373
Additional Provisions	820	1,689	869
Total	37,514	41,275	3,761
Panel B: Risk-Weighted Assets (Million US$)			
RWA 1 (0%)	0	0	0
RWA 2 (10%)	1,969	4,562	2,592
RWA 3 (20%)	4,867	3,849	−1,018
RWA 4 (60%)	66,675	68,726	2,052
RWA 5 (100%)	218,781	204,417	−14,364
Total	292,292	281,554	−10,738
Panel C: Assets (Million US$)			
Assets 1	0	0	0
Assets 2	19,690	45,620	25,920
Assets 3	24,335	19,245	−5,090
Assets 4	111,125	114,543	3,418
Assets 5	218,781	204,417	−14,364
Total	373,931	383,825	9,894

This table shows the capital adequacy indicator for the banking industry in Chile for 2019 and 2020 and the change between both years. The capital adequacy ratio is the ratio of regulatory capital to risk-weighted assets. Regulatory capital is the sum of common equity tier 1, subordinated bonds, and additional provisions (Panel A). Assets are classified into five risk categories, each with a different capital charge, depicted in parentheses (Panel B). The first category includes the safest assets, such as cash and deposits in the central bank; the second category includes financial instruments issued by the Chilean government and credits guaranteed by the government; the third category includes interbank loans; the fourth category comprises low-risk mortgages; and the fifth category includes all other assets not included in the previous categories. Panel C reports total (unweighted) assets by risk category. The table is created using publicly available information from the Financial Market Commission.

Table 14
Simulated Impacts of Policies Relative to Benchmark

Relative to Benchmark:	(1) Combined Policy (%)	Policy Components (2) No Increased Willingness to Lend (%)	(3) No Interest Rate Cap Reduction (%)	(4) No Credit Guarantee (%)
Δ Credit	9.8	-7.8	16.8	6.0
Δ Interest Rate	-2.5	-1.3	-1.5	-2.6
Typical Year Repayment (% Total Credit)				
Δ Expected Credit Loss	-0.3	-1.7	1.4	-0.8
Govt. Expected Credit Loss	2.9	1.9	4.3	0.0
Actual Burden	0.2	0.1	0.5	0.0
Systemic Shock Repayment (% Total Credit)				
Δ Expected Credit Loss	-0.2	-2.4	1.9	-1.0
Govt. Expected Credit Loss	10.0	8.3	11.7	0.0
Actual Burden	1.2	0.9	1.7	0.0

The numbers are all relative to benchmark results, i.e., the calibrated no-intervention economy. The Δs refer to differences: policy simulation value minus benchmark simulation value. The benchmark results are the following: credit = 0.97; interest rate = 4.0%; "Expected Credit Loss"= 4.0% (typical year repayment); and "Expected Credit Loss" = 12.7% (systemic shock repayment). "Expected Credit Loss" is simply credit-weighted default. Since the government's share of expected credit loss ("Govt. Expected Credit Loss") and the government's actual burden ("Actual Burden") are zero in the benchmark—because there is no government guarantee—the values reported for both are the actual values. The government's share of expected credit loss is simply 0.8×"Expected Credit Loss" under policies with the guarantee rate of 0.8. However, the actual guarantee is only that fraction of losses. The actual burden is therefore less than this because of partial repayment of the expected credit loss. Credit loss and actual burden are computed relative to total credit.

Appendix Table 1
Summary Statistics: Firm-Level Characteristics

	(1) Mean	(2) Median	(4) Std. Dev.	(5) Number of Firms
Annual Sales (Million US$)	0.95	0.19	2.97	114,606
Banked	0.56	1.00	0.50	114,606
Credit Stock (Million US$)	0.40	0.02	4.65	59,541
Credit Stock/Annual Sales	0.29	0.08	0.77	58,841
Firm Age (Years)	9.81	7.08	7.86	114,606
Net Worth (Million US$)	1.46	0.03	297.99	60,394
Number of Workers	25.27	4.00	176.08	114,606
Sales, Decrease	0.59	1.00	0.49	114,606
Sales, Increase	0.32	0.00	0.47	114,606
Spread Ex-Ante	0.10	0.09	0.06	38,415
Value Added/Number of Workers	0.04	0.02	0.20	114,606
Wage Bill (Million US$)	0.18	0.03	1.38	114,606

This table reports firm-level summary statistics for the credit program eligible firms sample. Amounts are in million US$ as of December 2019. Banked 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. Default probability is a dummy variable equal to one if the firm defaults on a loan during 2019 (i.e., has a loan past due more than 90 days) and is zero otherwise. Sales, decrease (increase) is equal to one if the firm experiences a negative (positive) variation in sales between February 2020 and April 2020 and is zero otherwise. 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 2, column 8. Observations in the top and bottom 1% are dropped for those variables included in the calculation of ratios.

Appendix Table 2
Default Probability Model: Different Regressors and Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Probit Estimation								
Log(Net Worth)	−0.010***	−0.009***	−0.009***	−0.007***	−0.010***	−0.006***	−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***	−0.011***	−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***	−0.002***	−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***	−0.006***	−0.006***	−0.008***	−0.004***	−0.009***	−0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Annual Sales)	0.002***	−0.003***	0.005***	−0.003***	0.002**	0.000	0.007***	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Credit Stock)		0.013***		0.012***		0.010***		0.012***
		(0.001)		(0.001)		(0.001)		(0.001)
Spread Ex-Ante		0.003***				0.001***		0.003***
		(0.000)				(0.000)		(0.000)
Spread 2018				0.004***				
				(0.000)				
Default Probability						0.226***		
						(0.002)		
Sales Variation							−0.040***	−0.034***
							(0.002)	(0.002)
Dependent Variable Mean	0.088	0.088	0.080	0.080	0.089	0.089	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	96,424	96,424	69,317	69,317	95.928	95.928	92,802	92,802
R^2	0.073	0.112	0.068	0.117	0.073	0.284	0.092	0.124
Industry FE and Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Predicted Default Probability								
Banked Firms	0.088	0.089	0.079	0.079	0.089	0.089	0.090	0.091
Unbanked Firms	0.107		0.091		0.108		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 2, columns 4 and 8, and are used as a benchmark. Banked firms are those that either have bank credit outstanding in December 2019 or receive a bank loan over the period 2012–2019. Unbanked firms are those with no credit records in the banking system over the period 2012–2019. 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table 3
Probability of Firms Using Public Programs: Including Unbanked Firms

	Credit Program			Employment Program
	(1)	(2)	(3)	(4)
	Use Credit Program	Credit Program Applications	Credit Program Approvals	Use Employment Program
Unbanked Firms Risk	0.302***	0.395***	−0.291***	−0.049
	(0.040)	(0.040)	(0.039)	(0.030)
Banked Firms Risk	0.308***	0.543***	−0.265***	−0.024
	(0.028)	(0.033)	(0.022)	(0.020)
Banked	0.299***	0.313***	0.022***	0.022***
	(0.005)	(0.005)	(0.005)	(0.004)
Increase in Sales Dummy	0.157***	0.165***	0.020***	0.058***
	(0.006)	(0.005)	(0.006)	(0.005)
Decrease in Sales Dummy	0.159***	0.171***	0.022***	0.111***
	(0.005)	(0.005)	(0.006)	(0.005)
Use Employment Program	0.083***	0.109***	−0.008**	
	(0.004)	(0.004)	(0.003)	
Use Credit Program				0.054***
				(0.002)
Dependent Variable Mean	0.357	0.481	0.911	0.165
Dependent Variable Std. Dev.	0.479	0.500	0.285	0.371
Number of Firms	114,542	114,566	47,630	118,090
R^2	0.135	0.155	0.030	0.080
Industry FE and Municipality FE	Yes	Yes	Yes	Yes
Predicted Default Probability				
Unbanked Firms	0.094	0.094	0.104	0.093
Banked Firms	0.084	0.084	0.090	0.086

This table reports probit estimations of the probability of a firm using, applying, and getting approved for a government program on a set of firm-level characteristics for the credit program eligible firms sample. The dependent variable is a dummy equal to one if the firm obtains a guaranteed loan (column 1); a dummy equal to one if a firm applies for a guaranteed loan (column 2); a dummy equal to one if a firm gets approved for a guaranteed loan (column 3); and a dummy equal to one if a firm uses the employment program (column 4). Otherwise, the dummy variables are equal to zero. Risk corresponds to the fitted values of the regression specification reported in Table 2, columns 4 and 8, for unbanked and banked firms, respectively. Banked is a dummy equal to one if the firm has bank credit outstanding in December 2019 or receives a bank loan over the period 2012–2019. Increase (decrease) in sales dummy is equal to one if the firm experiences a positive (negative) variation in sales between February 2020 and April 2020. Columns 1–4 include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table 4
Probability of Firms Using Public Programs: Spread Ex-Ante

	Credit Program			Employment Program
	(1)	(2)	(3)	(4)
	Use Credit Program	Credit Program Applications	Credit Program Approvals	Use Employment Program
Spread Ex-Ante	0.002***	0.003***	−0.001***	−0.001
	(0.001)	(0.001)	(0.000)	(0.000)
Increase in Sales Dummy	0.143***	0.133***	0.015*	0.046***
	(0.011)	(0.010)	(0.008)	(0.009)
Decrease in Sales Dummy	0.141***	0.136***	0.015*	0.105***
	(0.011)	(0.009)	(0.008)	(0.009)
Use Employment Program	0.087***	0.112***	−0.010**	
	(0.007)	(0.007)	(0.005)	
Use Credit Program				0.054***
				(0.004)
Dependent Variable Mean	0.517	0.656	0.926	0.190
Dependent Variable Std. Dev	0.500	0.475	0.262	0.393
Number of Firms	36.212	36.156	20.656	37.739
R^2	0.071	0.095	0.037	0.084
Industry FE and Municipality FE	Yes	Yes	Yes	Yes
Predicted Default Probability				
Banked Firms	0.059	0.059	0.064	0.060

This table reports probit estimations of the probability of a firm using, applying, and getting approved for a government program on a set of ex-ante firm-level characteristics for the firms with spread data among the credit program eligible firms sample. The dependent variable is a dummy equal to one if the firm obtains a guaranteed loan (column 1); a dummy equal to one if a firm applies for a guaranteed loan (column 2); a dummy equal to one if a firm gets approved for a guaranteed loan (column 3); and a dummy equal to one if a firm uses employment protection (column 4). All dummy variables are zero otherwise. Spread ex-ante is calculated as the average spread charged on bank credit obtained by each firm over the period 2012–2019. Increase (decrease) in sales dummy is equal to one if the firm experiences a positive (negative) variation in sales between February 2020 and April 2020 and is zero otherwise. Columns 1–4 include industry and municipality fixed effects. Standard errors, shown in parentheses, are clustered at the industry and municipality level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table 5
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 a banked firm using a government program on a set of firm-level characteristics for the credit program eligible firms sample. Banked firms are those with credit records in the banking system over the period 2012–2019. The dependent variable is equal to one if the firm participates in the credit program (columns 1–4), is equal to one if the firm participates in the employment program (columns 5–8), and is zero otherwise. Risk corresponds to the fitted values of the regression specification reported in Table 2, column 8. Increase (decrease) in sales dummy is equal to one if the firm experiences a positive (negative) variation in sales between February 2020 and April 2020 and is zero otherwise. 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 level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table 6
Risk Sharing between the Banking Industry and the Government

	(1) Total Credit Program (Million US$)	(2) Total Credit Program/GDP (%)	(3) Share of Credit Program (%)	(4) Default Probability (%)	(5) Guarantee (%)	(6) Deductible (%)	(7) Effective Guarantee (%)	(8) Expected Loss/GDP (=(2)×(4)) (%)	(9) Government's Expected Loss/GDP (=(7)×(8)) (%)	(10) Banks' Expected Loss/GDP (=(8)-(9)) (%)
Firm Size										
Small	2,264	0.9	25	9.22	85.0	5.0	39.0	0.08	0.03	0.05
Medium	2,372	1.0	27	5.97	80.0	3.5	33.0	0.06	0.02	0.04
Medium-Large	3,322	1.3	37	3.45	70.0	2.5	19.0	0.05	0.01	0.04
Large	1,008	0.4	11	2.49	60.0	2.5	0.0	0.01	0.00	0.01
No Sales Data	56	0.0	0	9.22	85.0	5.0	39.0	0.00	0.00	0.00
Total: Formal Firms	9,022	3.6	100	5.47	75.3	3.4	25.6	0.20	0.06	0.14

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 the formal firms sample. Panel A reports statistics across the firm's risk distribution. Column 1 shows the total amount of guaranteed credit in dollar terms. Column 2 normalizes column 1 by GDP. Column 3 shows the share of guaranteed loans for each category, and column 4 shows the default probability of each category, using the model in Table 2. Columns 5 and 6 show the guarantee and the first-loss deductible for each category. Column 7 shows the effective guarantee after accounting for the deductible. Columns 8–10 show, for each category, the total expected loss as share of GDP (column 2 times column 4), the fraction borne by the government (column 7 times column 8), and the fraction borne by the banking sector (column 8 minus column 9), respectively. Values in columns 4–7 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 reported in columns 4–7 are calculated as the sum of the product of each category's statistic by its relative weight (column 3). Firms are classified across size categories based on their annual sales according to the criteria defined by the tax authority. Firms with missing size data are assigned the risk of small firms.

Appendix A: Dynamic Lockdowns and Use of Policies

To control the expansion of the virus, sanitary authorities in Chile follow a strategy of dynamic lockdowns, according to which some municipalities are subject to a mandatory lockdown depending on the virus propagation. The introduction of lockdowns is staggered across municipalities across time. We exploit spatial heterogeneity of exposure to social distancing measures to study the effect of dynamic lockdowns on the use of public policies.

We define the treatment event as the week a municipality enters a mandatory lockdown. Treated firms are in municipalities that initiate a lockdown at any point from May to July of 2020.[39] Control firms are those located in adjacent municipalities that are never closed during the same period of time. Suppose municipality A enters a lockdown the first week of June. Control firms are those located in neighboring municipalities to A but are never in lockdown during our sample, say municipality B.[40] To be selected into our sample, municipalities must have at least one neighbor with a different treatment status.[41] Municipalities exclusively surrounded by others in the same treatment status are left out of the analysis. Chile has a total of 345 municipalities, of which 153 are used in the study.

Appendix Figure 1 presents the map of municipalities according to their overall lockdown status and inclusion in the analysis. Blue municipalities (i.e., treatment municipalities) go into lockdown at any point during May to July and have at least one neighboring municipality that never enters a lockdown during the same period. Light blue municipalities (i.e., control municipalities) never go into lockdown during May to July and have at least one neighboring municipality that goes under lockdown during the same period. White municipalities are not included in the analysis because all adjacent municipalities have the same status: either they are all under lockdown during this period or not.

Appendix Figure 2 presents the weekly evolution of the cumulative number of municipalities

[39] The treatment period starts in May because that is the first month in which the credit and employment policies are available. The treatment period ends in July because after that month, some municipalities start ending the lockdowns.

[40] If municipality B is also a neighbor of municipality C, which enters a lockdown at some point, then municipality B appears in the data twice: once as a control for municipality A and a second time as a control for municipality C.

[41] That is, a treatment municipality must have at least one adjacent control municipality, and a control municipality must have at least one adjacent treatment municipality.

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.

Appendix B: Calibration of the Model

Our quantitative exercises require calibration for which we use aggregate numbers. We also need a parametric choice for $\Phi(\cdot)$ at the firm level, which we model as a log normal distribution with variance parameter, σ_ε^2, and for the technology parameters δ, α, and μ. We consider heterogeneity in productivity z, equity e, and σ_ε^2 itself. We assume that e and z are jointly log normal. By normalizing mean log productivity, $\overline{z} = 1$, this distribution introduces four additional parameters: the variance of log productivity, σ_z^2; the variance of equity, σ_e^2; the mean level of log equity, $\overline{e}$; and the correlation between log productivity and log equity, ρ. Because firms could differ in underlying revenue uncertainty, something independent of average productivity, equity, and debt, we assume that σ_ε^2 is distributed log normally with parameters governing the mean, $\overline{\sigma}$, and variance, σ_σ^2. We refer to the probability density function as $H(z, e, \sigma_\varepsilon; \sigma_z^2, \sigma_e^2, \overline{e}, \rho)$.

We set the interest rate r at 0.014 to exactly match the (credit-weighted) rate paid by firms in our sample with low debt/equity (less than 0.1) and therefore with very low default risk.[42] Although we have not done so explicitly, a simple way to model labor and materials is to consider them Leontief with capital. In that case, δ captures capital depreciation as well as labor and material expenses as a proportion of capital. In our data, materials and labor amount to about three-fourths of gross output. With a depreciation rate of 0.06, and a capital-output ratio of about 0.9, we calibrate $\delta = 0.9$ (i.e., $(0.75 + 0.06)/0.9$). We set the returns to scale parameter in revenues at $\alpha = 0.8$, which is comparable to calibrations for competitive models (e.g., Buera et al., 2020; Restuccia and Rogerson, 2008) or consistent with a markup of 20% in monopolistic competition work that calibrates constant returns to scale in production but with a constant elasticity of demand.

The last seven parameters (the four governing the exogenous joint distribution of productivity and equity, σ_z^2, $\overline{e}$, σ_e^2, and ρ; the two governing the distribution of revenue risk, $\overline{\sigma}$ and σ_σ^2; and the cost of default, μ) target eight key moments in the aggregate economy:

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

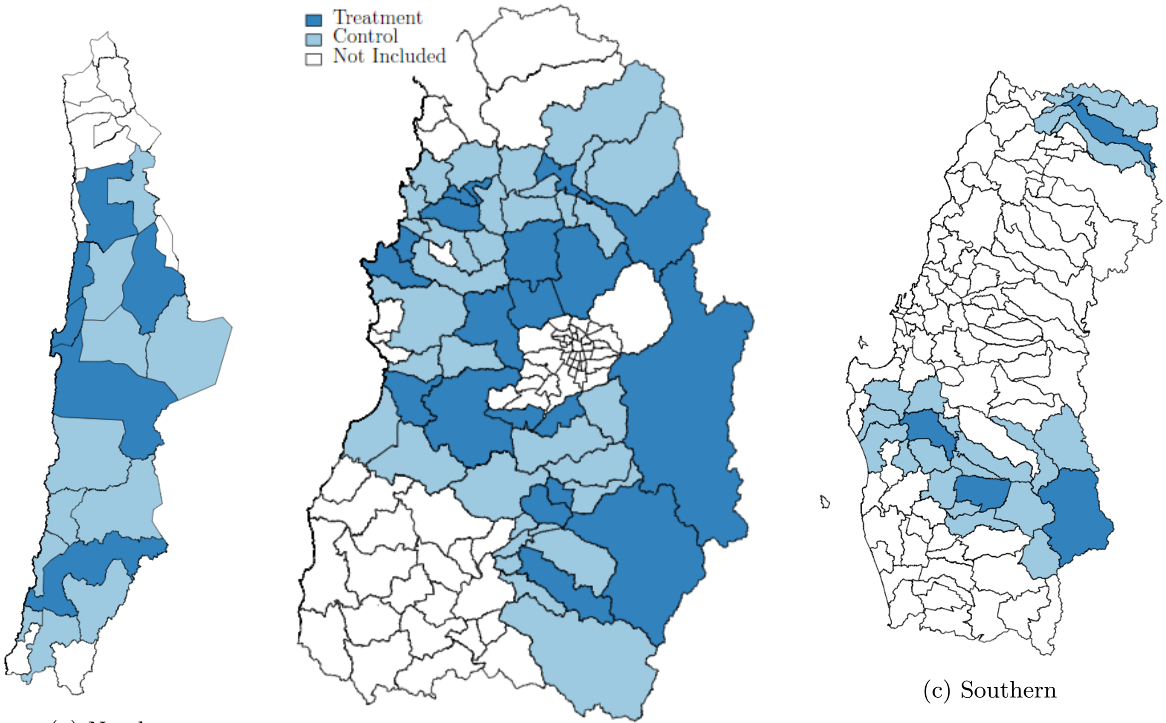
the aggregate ratio of debt to equity (0.02 in the data versus 0.02 in the model), the (credit-weighted) average (16.9 versus 16.7) and standard deviation (24.2 versus 25.3) of debt to equity as well as the unweighted standard deviation of debt to equity (13.2 versus 12.5), the (credit-weighted) averages of the predicted default rate (0.04 versus 0.04) and loan interest rate (4.0% versus 4.0%), the standard deviations of log sales (1.8 versus 1.6), and the standard deviation of log equity (1.7 versus 1.5).[43]

The overall fit is good, although we somewhat underestimate the raw variation in debt to equity and overestimate the credit-weighted variation in debt to equity. The values governing risk, $\overline{\sigma} = 0.22$ and $\sigma_{\sigma}^2 = 0.15$, indicate both considerable risk and considerable exogenous variation in risk across firms. Other parameter values are $\sigma_z^2 = 0.16$, $\overline{e} = 3.18$, $\sigma_e^2 = 1.55$, and $\rho = -0.999$.[44] The cost of default, $\mu = 0.11$, implies that default costs 11% of a typical year's sales and is comparable to the 0.15 used for the United States (Covas and Haan, 2012). Finally, note that the credit-weighted default rate and interest rate are identical. If default amounted to no repayment, even without a cost of default, banks would receive less money than they lent in expectation (1.04*0.96<1). These values must therefore reflect substantial loan repayment even in the event of default, insuring a gap between expected credit loss and true losses from default (even after default costs). We return to this in our quantitative analysis of the policy below.

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

[44]The almost perfectly negative correlation between (log) equity and (log) productivity is needed to generate large levels and variation of debt to equity. Nevertheless, the correlation of (log) sales and (log) equity is positive in both the model and data, though it is somewhat lower in the model (0.23 versus 0.72).

Appendix Figure 1
Dynamic Lockdowns: Treatment Definition



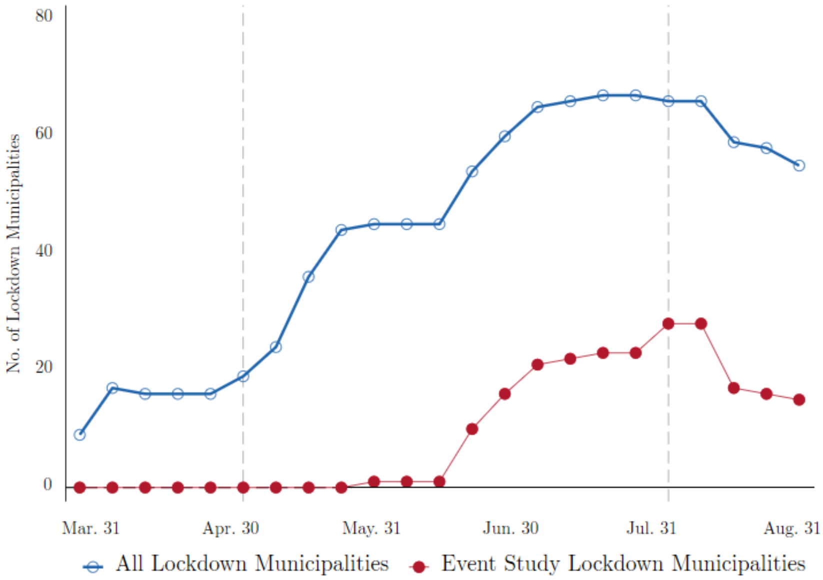
(a) Northern

(b) Central

(c) Southern

This figure shows how we identify municipalities that are subject to lockdown mandates over time, which we use to define the treatment for our dynamic lockdown specification. 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.

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 dotted vertical lines show, respectively, the starting and ending dates considered in the dynamic lockdown event study. This figure uses publicly available data.