Selection in Crisis Lending
Evidence from Chile's Government-Guaranteed Loans*

Lautaro Chittaro† Cristián Sánchez‡
Stanford University Central Bank of Chile
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Abstract

We provide a quantitative assessment of Chile's government-guaranteed loan (GGL) program introduced during the COVID-19 crisis to sustain credit to small and medium-sized enterprises. In a GGL, the government insures banks against borrower default to encourage lending during downturns. By linking tax, social security, and credit registry data with loan applications, we document that default rates among approved firms were nearly zero in 2020 but rose sharply two years later. Using an instrumental-variables strategy exploiting heterogeneity in banks' approval leniency, we show that the program primarily postponed rather than prevented defaults. Our estimates indicate that banks used private information to screen borrowers effectively, supporting the policy design that delegated credit allocation to commercial banks. To evaluate welfare, we develop a structural model of entrepreneurial default with heterogeneous firms and a GGL option. The model implies sizable and cost-effective welfare gains, limited bank rents, and modest excess risk-taking. A budget-neutral redesign could raise welfare by up to 4% relative to the actual program, albeit at the cost of reducing approved loans by at least 4%.

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†chittaro@stanford.edu (corresponding author)
‡csanchez@bcentral.cl

1. Introduction

Government-guaranteed loans (GGLs) have become a central policy instrument for channeling liquidity to small and medium-sized enterprises (SMEs) during economic crises. A GGL is a commercial bank loan that the government insures at least partially to boost credit supply. Yet it remains unclear whether banks primarily direct the fiscal resources implied by these guarantees to solvent firms facing temporary liquidity needs, to insolvent or unconstrained firms, or even retain a substantial share of these resources as rents. Subsidized credit can also encourage excessive risk-taking by banks and firms, distorting real and financial decisions. To mitigate these concerns, modern GGL programs typically have additional design features such as loan caps, or fixed interest rates.

This paper studies the impact and welfare implications of Chile's COVID-19 government-guaranteed loan program. By linking administrative tax, social security, and credit records with loan applications, we document a sharp increase in debt among approved firms. Using an IV strategy based on banks' approval leniency, we find that GGLs temporarily reduced defaults, indicating that they delayed rather than prevented distress. Moreover, we find that banks tend to approve loans to firms that would have a lower default probability even in the absence of the GGL. To evaluate welfare, we develop a structural model of entrepreneurial default that incorporates a GGL with rich design features. The model endogenously generates sorting into approved, rejected, and non-applicant firms based on solvency and liquidity conditions. We estimate sizable and cost-effective welfare gains with both limited bank rents and low excessive risk-taking. We consider alternative budget-neutral policy designs in counterfactuals and find modest improvements in welfare. These findings underscore the challenge of designing GGL programs that provide effective support while minimizing unintended distortions.

Firms that received a GGL experienced a sharp expansion of debt, followed by an increase in default rates two years after the crisis. Approximately 45% of approved firms at least doubled their debt a year after the crisis. This increase was mostly explained by the GGL itself rather than additional commercial borrowing. During the crisis year, approved firms had near-zero default rates, but these rates soon increased, surpassing those of non-applicants and approaching those of rejected firms. These patterns persist after controlling for a rich set of pre-crisis firm characteristics, including several measures of pre-crisis default risk.

Because the government guarantee covered only a fraction of each loan, banks still bore some default risk. As a result, they had incentives to screen out firms that were more severely hit by the crisis and thus more likely to default. A naive OLS approach would ignore the effect of this screening and thereby overstate the impact of the GGL on firms' default probabilities. Using an IV strategy based on banks' approval leniency, we find a temporary, negative causal effect of 2.4–5.2in the first two years after the crisis hit. By 2022, the impact on *cumulative* defaults is no longer statistically significant. This uncovers the importance of a long-run perspective to evaluate the impact of the GGL, since our results indicate that the GGL postponed defaults over time rather than permanently reducing them. Moreover, we document that banks were more likely to approve loans to firms that would have exhibited lower default rates even in the absence of the GGL. This finding supports the rationale for delegating liquidity allocation to commercial banks.

We develop a dynamic model of entrepreneurial default and GGL allocation that we use to quantify the welfare implications of the GGL program. In the model, entrepreneurs with heterogeneous long-run productivity make optimal investment and borrowing decisions. An unexpected crisis shock hits the economy, affecting firms to varying degrees and generating different liquidity needs across the firm distribution. Our model is designed to generate sorting into approved, rejected, and non-applicants to GGL, based on firms' solvency and liquidity conditions. We discipline the effect of the GGL in the model by matching our causal estimates. The model closely predicts the untargeted long-run default rates of all three groups of firms.

After estimating the model, we find considerable welfare gains from the GGL program. On average, approved firms would need to increase one-period consumption by 1.2% relative to a non-GGL scenario to be indifferent to the actual policy. This represents about 21% of the average annual growth of aggregate pre-crisis consumption in Chile. Using a money-metric welfare measure, we find that the policy surpasses its fiscal cost by 21%. However, we find that there is substantial heterogeneity in the gains across firms. Specifically, 27% of approved firms did not offset the cost, suggesting scope for improvement in the program's design.

Several papers have warned about the pervasive effects of GGL lending, and more in general subsidized credit. In particular, moral hazard can induce excessive risk-taking, and taxpayers funds can end up being captured by banks in the form of rents. We use our model to quantify both effects. Aligned with our reduced-form results, our model predicts that in the absense of a GGL, defaults would have been only 0.8 pp. higher, which indicate limited moral hazard effects. But at the firm-level, we find that near 44% of approved firms are taking larger long-run risks in the presence of the GGL, especially those that start in a more robust position to face the crisis. Since the policy both sets a fixed rate for the loan and a guaranteed share, banks extract rents from the difference between the after-guarantee break-even rate and the opportunity cost of funds. We find that only 5% of the total cost of the GGL is captured by banks.

While the evidence suggests that the GGL program was cost-effective, and did not generate excessive moral hazard or bank rents, we ask whether we can tune the program to improve welfare. We study changes of guarantees and interest rates that are budget-neutral. We find that a joint increase of a guarantee and a decrease of the fixed rate would modesltly increase the welfare gains by 4%, but concentrating the support on at least 4% less of firms.

Contribution to the literature We relate to three strands of the literature. A first group of empirical studies evaluates the effects of large-scale crisis lending programs on SMEs' real and financial outcomes. Studies of the U.S. Paycheck Protection Program (PPP) (Autor, Cho, Crane, Goldar, Lutz, Montes, Peterman, Ratner, Villar, and Yildirmaz, 2022b; Dalton, 2023; Agarwal, Ambrose, Lopez, and Xiao, 2024) document short-run gains in employment, firm survival, and debt repayment during the year of the shock. However, these effects are modest relative to the size of the program, and a substantial share of the support did not reach its intended targets (Autor, Cho, Crane, Goldar, Lutz, Montes, Peterman, Ratner, Villar, and Yildirmaz, 2022a; Granja, Makridis, Yannelis, and Zwick, 2022). The PPP provides a useful contrast to our setting, as the U.S. program lacked key design features present in other GGL schemes: it offered banks little incentive to screen borrowers, given that loans were fully guaranteed, and imposed few costs on firms, since loans were largely forgivable.

A related set of papers studies government-guaranteed loans (GGLs) as a crisis policy tool.[1] These studies also document positive short-run effects on firms' real and financial outcomes (Hackney, 2023; Jiménez, Laeven, Miera, and Peydró, 2024; Bachas, Kim, and Yannelis, 2021), although some evidence points to a crowding-out of private lending (Altavilla, Ellul, Pagano, Polo, and Vlassopoulos, 2025; Jiménez, Peydró, Repullo, and Saurina, 2018). Our paper contributes to this literature by highlighting the importance of the time horizon in assessing the effectiveness of GGLs as a crisis policy instrument. Using a longer time series, we find causal evidence that the policy postponed rather than permanently prevented defaults.

Closest to our work are studies that examine Chile's GGL program during the COVID-19 crisis. Huneeus, Kaboski, Larrain, Schmukler, and Vera (2025) shows that firms with lower pre-crisis default risk were more likely to receive guaranteed loans. Acosta-Henao, Pratap, and Taboada (2023) documents substitution of foreign lending to domestic credit within the top end of the firm size distribution. A work in progress by Cerda, Gertler, Higgins, Montoya, Parrado, and Undurraga (2023) randomizes GGL offers among a subsample of already-rejected firms, finding positive effects on liquidity but none on earnings. We complement these analyses by providing both a welfare evaluation of the program, and a framework to estimate the heterogeneity in the distribution of the gains from the program.

A second strand of the literature highlights the pernicious effects of subsidized lending (De Meza, 2002; Caballero, Hoshi, and Kashyap, 2008; Gropp, Gruendl, and Guettler, 2014; Acharya, Borchert, Jager, and Steffen, 2021; Hoshi, Kawaguchi, and Ueda, 2023; Acharya, Crosig-

[1] More broadly, smaller-scale GGL programs in normal times aim to expand credit access for firms facing collateral constraints, limited credit history, or limited financial expertise (Beck et al., 2010; Stillerman, 2024; Brown and Earle, 2017; Mullins et al., 2018; Bertoni et al., 2023).

nani, Eisert, and Eufinger, 2024; Li and Li, 2025). Moral hazard can lead highly leveraged firms to underinvest, as the returns on new projects accrue largely to lenders rather than borrowers—a phenomenon known as debt overhang (Myers, 1977; Brunnermeier and Krishnamurthy, 2020; Crouzet and Tourre, 2021; Segura and Villacorta, 2023). Other studies show that banks may capture part of the guarantee through market power (Ornelas, Pedraza, Ruiz-Ortega, and Silva, 2024; Stillerman, 2024). These negative consequences might be exacerbated if the government favors guarantees over alternative policies (e.g., direct transfers) because of their zero upfront cost and the opacity of their fiscal reporting (Lucas, 2024). In response to these concerns, modern guarantee programs incorporate safeguards to limit potential distortions. We provide quantitative evidence that features such as caps on loan size, fixed loan rates, and partial guarantees help contain these distortions.

From this second set of papers, a work by Martin, Mayordomo, and Vanasco (2025) that studies crisis GGL in Spain is closely related to ours. They investigate how banks decide which firms receive a guarantee, in a model that features debt overhang and banks' rents that arise from the limited availability of guarantees. If banks do not fully capture all the resources of the guarantee program, they pass some of this as a lower interest rate to the firms, boosting effort and output. In contrast, our setting features a GGL with a fixed interest rate, which bypasses this problem, at the expense of generating other forms of rents: those that arise from the differences between the zero-profit rate of the GGL and the rate set by the policy maker. These types of arrangements with both a guarantee and price rigidities were not exclusive to Chile, but many other countries, including Australia, Canada, Belgium, Germany, Hong Kong, Singapore, used similar designs with fixed rates, rate schedules, or rate caps.

Finally, our paper connects to the intersection of macroeconomic models with heterogeneous firms and financial frictions (Bernanke, Gertler, and Gilchrist, 1999; Buera, Kaboski, and Shin, 2011; Midrigan and Xu, 2014; Moll, 2014; Ottonello and Winberry, 2020; Camara and Sangiacomo, 2022), and structural models of corporate debt and default (Hennessy and Whited, 2005; Corbae and D'Erasmo, 2021; Kochen, 2023). We build on these frameworks to study how heterogeneous entrepreneurs, facing a crisis under varying liquidity and solvency conditions, sort into a one-shot optional GGL program, and how their behavior responds to the resulting allocation of the support.

2. Setting: Chile's GGL Program during the COVID-19

The COVID-19 crisis was an unexpected and sudden shock to firms' cash flows. In March 2020, the Chilean government implemented lockdowns and other social distancing measures to contain the spread of the virus. As a result, GDP dropped by 14% in the second quarter, year-over-year. This unexpected shock was accompanied by a sharp contraction in credit: non-GGL credit had fallen to 8% in the same period (Costa, 2021).

As a response,[2] the Chilean government launched a large-scale GGL program[3] in late April 2020 to support SMEs' liquidity needs. By May, the program represented 45% of all new loans, and by the end of 2020, it had provided credit equivalent to 4.7% of GDP (Huneeus, Kaboski, Larrain, Schmukler, and Vera, 2025)[4]. The initiative built upon a pre-existing, small-scale GGL program with different conditions, enabling a rapid rollout of the crisis lending scheme.[5]

The conditions of the GGL program were highly favorable for SMEs. The loans carried a fixed annual interest rate of 3.5%, set by the government, 3 pp above the risk-free rate at the time, and 6 pp below comparable market loans. Each loan had a maximum size of 25% of the firm's annual sales and was restricted to financing working capital. Loans were long-term, with a maturity of 3.5 years on average[6]. This term was roughly 40% longer than that of comparable market loans [7]. Repayment was scheduled in fixed installments, after a six-month grace period.

Firms seeking a GGL loan had to apply through commercial banks. Applications were submitted through the bank's website, where firms specified the requested amount, provided information about the firm and its owners, attached the required documentation, and authorized the bank to access their tax records. The bank then evaluated each application, verifying both program eligibility and the firm's creditworthiness according to its internal criteria and the conditions established by the government. After evaluation, the bank either approved or rejected the loan.

To induce banks to lend at below-market terms, the government insured them against default risk through a partial guarantee. In the event of default, the government covered a share of the outstanding debt. The *guarantee share* decreased with the firm's pre-crisis annual sales, ranging

[2]For a comprehensive overview of the Chilean government's response to the COVID-19 crisis, including household support policies, see Madeira (2023). While the GGL was not the only program aimed at supporting firms, it was the largest in scale. Huneeus, Kaboski, Larrain, Schmukler, and Vera (2025) study the interaction between the GGL and a contemporaneous employment-support program that helped firms maintain the payroll of workers who had to suspend operations. They estimate the size of the latter at 0.6% of GDP, whereas the GGL accounted for 4.7% of GDP in credit expansion.

[3]Known as *créditos FOGAPE COVID*, named after the government fund that backed the GGLs.

[4]In developed economies, GGLs during COVID-19 crisis accounted for 2–12% of GDP (Hong and Lucas, 2023). For other Latin American countries, see Bolzico and Prats Cabrera (2022)

[5]On April 24, 2020, Law 21,229 temporarily modified a pre-existing fund to dramatically expand its scope and conditions of the loans it could back up through guarantees. The fund—FOGAPE—was created in 1980 to support SME credit by providing guarantees for loans, leasing operations, and other financial products offered by public and private financial institutions. FOGAPE's primary funding sources are government contributions, fees for guarantee services, and returns on the fund's investments. See Mullins, Toro, and others (2018) for an evaluation of FOGAPE's operation under normal conditions.

[6]Although loans could, in principle, have maturities between 2 and 4 years, GGLs were granted for an average of 3.5 years, and 57% had the maximum term of 4 years.

[7]A regression of the log of term on firm and loan characteristics yields a coefficient of -0.38 on a dummy indicating whether the loan was market-priced (i.e., not GGL) during 2020.

from 85% for the smallest bin of firm's size to 60% for the largest bin[8].

To qualify for the GGL program, firms had to report less than 28 million USD in pre-crisis annual sales and maintain a good credit history. The size threshold excluded the top 1% of Chilean firms by annual sales[9] . The credit-history requirement specified that firms could not have default records[10] at the time of application, nor have pre-crisis default records during 2019.

The GGL program combined standardized, favorable loan terms and broad eligibility with a decentralized implementation through commercial banks. While the government defined the core loan parameters (maximum size and rate) and the banks' risk exposure (through the guarantee share), individual banks retained discretion over loan approval. This institutional design provides a rich setting to study how the allocation of liquidity is shaped by the incentives faced by both banks and firms under the policy's parameters.

2.1. Data

Linked administrative records To track firms' performance before and during the crisis, we match official firms' tax records, social security data, and bank credit registry accessible through the Chilean Central Bank's access to regulatory data. We build a main panel at the firm level, with annual frequency, for the period 2019–2024. We focus on the universe of formal firms with pre-existing bank relationships as of 2019 that were eligible for the program. For these firms, we observe real outcomes (sales, employment, wage bill, assets, returns), financial variables (debt, default, delays, share of debt backed by collateral, share of debt provisioned to cover losses by the bank, bank relationships), and other characteristics (age, industry, county). Additional details on data sources and panel construction are provided in Appendix B.1.

GGL application data Crucially, we have access to GGL applications data, which allow us to identify how the program's design affected the incentives of both firms and banks. Each observation corresponds to a firm-bank pair that we match to administrative data. For each application, we observe whether the firm was approved or rejected, and the characteristics of the loan if granted (term, rate, guarantee share, maturity). Based on application and approval status, we define three groups of firms: non-applicants, rejected, and approved[11].

[8] In our main panel, the mean (median) guarantee share was 74% (85%).

[9] The threshold was set in UF, a local currency unit indexed to inflation. We convert all monetary figures from pesos to UF using the exchange rate of the corresponding period, and from UF to USD of December 31, 2025, at a rate of 27.76 UF per USD. For exposition, we round this value to 28 in the text.

[10] Chile's regulation defines a default as a payment overdue by more than 90 days, following Basel international standards.

[11] For the main panel, we aggregate it at the firm level, considering as a firm to be approved if it received at least one loan from a bank. Empirically, 72% of firms only filled one application to a single bank, and 93% at most two. Banks were not allowed to jointly lend more than 25% of pre-crisis sales in total Santander (2020).

Sample definition Our sample consists of *formal firms with pre-existing bank relationships that were eligible for the GGL program*. We adopt a conservative definition of formal firms—those with positive sales, assets, and formal employment in 2019. Although this definition excludes some records,[12] it ensures that we have a rich set of pre-crisis controls for firm characteristics. Moreover, we restrict the analysis to firms with pre-existing bank relationships, which allows us to observe default events for non-applicants on their non-GGL debt and to control for several pre-crisis risk metrics. Finally, firms were eligible if they met two criteria: annual sales below $28 million and no default records between 2019 and April 2020.

Main panel Our main panel covers 108,080 firms at annual frequency over the period 2019–2024. Table A1 summarizes firms' pre-crisis cross-sectional characteristics and details of their GGL application and assignment status. The median firm reported pre-crisis sales of 196 thousand USD and employed 7 workers, consistent with the program's focus on SMEs. Leverage, measured as bank debt to assets, was relatively low: the median firm had outstanding debt equal to 16% of assets and operated with a single bank. The median firm was 10 years old. This reflects that our sample is representative of established formal businesses with existing bank relationships, but does not capture informal firms or early-stage entrepreneurs. The application data confirm that the program was in high demand: 58% of firms applied for a GGL, and only 12% of applicants were rejected. Conditional on approval, firms received substantial liquidity support: the median loan size was 12% of pre-crisis sales. We use auxiliary datasets to construct the instrumental variable in Section 3.1 and to support the model calibration in Section 4, we detail these in those sections.

2.2. The GGL program led to a significant increase in firm debt

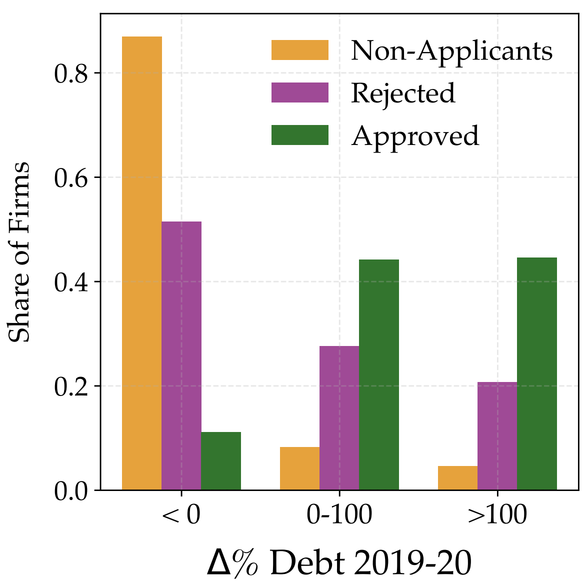
The GGL program provide liquidity to firms during the crisis, but it also led to a sharp increase in debt levels, threatening future repayment. We use our main panel to document this surge of firm debt. Figure 1a shows the distribution of debt growth from December 2019 to December 2020 ($\Delta\%$ Debt$_{20}$) across three groups: firms approved for a GGL, firms that did not apply, and firms that applied but were rejected. We cluster the debt growth into three bins: decrease, increase up to 100% and increase above 100%. Near 87% of non-applicants reduced their debt during the first year of the crisis. In contrast, 89% of the approved firms increased their debt, and 45% at least doubled their debt. This effect is driven by the mechanical effect of the GGL credit and not by other extra commercial loans. Moreover, 48% of rejected firms increased their debt during the period, signaling a demand for liquidity that is being satisfied at least partially from standard commercial loans.

The rapid surge in debt might undermine the repayment capacity of firms. Figure 1b shows

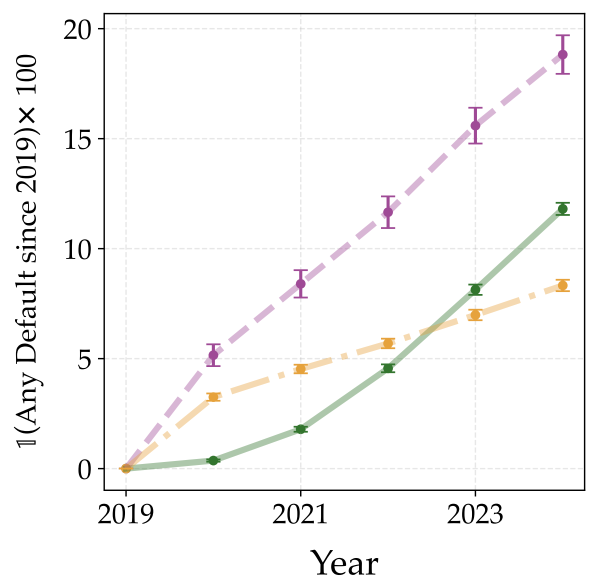
[12] The breakdown of records excluded during sample construction is reported in Appendix B.1.

the probability that a firm experiences a default event between 2019 and any given year. Since firms had to be not in default at the time of application, all groups start at 0 in 2019. Initially, approved firms are the best performing group, with a default rate 3 pp (5 pp) lower than non-applicants (rejected) firms in 2020. Over time, the default rates of approved firms steadily increase, catching up with non-applicants between 2022 and 2023. By 2024, approved firms had accumulated 4 pp more defaults than non-applicants, but 7 pp less than rejected firms. In Figure A1, we show non-cummulative default rates by group, where we drop already defaulted firms in each year. Here, we can observe better how the long-run default rates of rejected and approved firms converge at a higher level than non-applicants, which is captured in Figure 1b in the slope of the curves.

In the following section, we propose an instrumental variable strategy to disentangle how much of the difference in the observed default rates of approved and rejected firms is due to the extra funds the GGL provided relative to unobserved differences across approved and rejected firms.



(a) Debt growth across groups



(b) Cumulative default rates by group

Figure 1: Evolution of debt and default, 2019–2024. Panel (a) shows the distribution of debt growth from 2019 to 2020 (Δ% Debt$_{20}$) across three groups: firms approved for a GGL, firms that did not apply, and firms that applied but were rejected. We cluster the debt growth into three bins: decrease, increase up to 100% and increase above 100%. Panel (b) represents the cummulative default rates by group. The y-axis measures the share of firms that have experienced a default any point from 2019 to a given year. Since firms had to be not in default at the time of application, all groups start at 0 in 2019. Vertical bars represent 95% confidence intervals.

3. Reduced-Form Evidence of Selection and Effect of GGL

In this section, we use an instrumental-variable strategy to estimate the effect of the GGL on firms' default probability over time. We find that the GGL reduced the probability of default by 2.5–5.2 pp the first two years after the crisis. However, this effect is only temporary: by 2022 and on, we find no significant effect of the GGL on the probability of a default episode since 2019. Our identification strategy adapts the judge IV approach of Kling (2006); Aizer and Doyle Jr (2015) to a setting in which the bank is not randomly assigned by institutional design. Instead, we use pre-crisis bank relationships and bank-firm level approval policies estimated from application data to get quasi-random variation in GGL assignment. Our reduced-form estimates also provide a rationale for a policy design that delegates allocation decisions to banks. We find that banks approved firms with a lower default path, even in the absence of the GGL, which suggests that banks are using private information to allocate liquidity to relatively safer firms.

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Instrumental-Variables Estimates. In this section, we implement an instrumental-variables strategy to estimate the effect of the GGL on firms' default probability over time. We find that the GGL reduced the probability of default by 2.5–5.2 pp during the first two years after the crisis. However, this effect is temporary: by 2022 and onwards, we find no significant impact of the GGL on the probability of experiencing a default episode since 2019. In contrast, among firms that had not defaulted by 2022, we find a strong positive effect on the probability of default thereafter. Our identification strategy adapts the judge instrument approach of Kling (2006); Aizer and Doyle Jr (2015) to a setting in which banks are not randomly assigned by institutional design. Instead, we exploit pre-crisis firm–bank relationships and bank–firm-level approval policies estimated from loan application data to generate quasi-random variation in GGL assignment. The reduced-form evidence also provides a rationale for a policy design that delegates allocation decisions to banks. We find that banks approved firms with systematically lower default paths, even in the absence of the GGL, suggesting that banks used private information to allocate liquidity toward relatively safer firms.

3.1. Instrumental-variable strategy

We estimate the effect of the GGL on firms' default probability using the following specification:

$$\mathbb{1}\left[\text{Any default since 2019}\right]_{it} = \beta_t\, \mathbb{1}\left[\text{Approved}\right]_i + X_i'\Delta_t + u_{it} \tag{1}$$

The left-hand side is an indicator that equals one if firm i had any default since 2019 by year t. The coefficient of interest, β_t, measures the effect of GGL approval on a firm's probability of default at time t. We estimate β_t using only the sample of firms that applied for a GGL, since the divergent default paths of non-applicants of Figure 1 suggest that firms that did not apply are hardly comparable to applicants.

We exploit our linked administrative records to include a rich set of pre-crisis firm characteristics, X_i, allowing us to compare firms that entered the crisis under similar conditions. These controls include the (log of) sales, employment, wage bill, debt, assets, return on assets, and firm age as of December 2019. They also capture pre-crisis credit risk through several measures: the share of outstanding debt backed by collateral, the share of outstanding debt provisioned by banks to cover potential losses, and a model-based one-year-ahead default probability (detailed in Appendix C). In addition, we include the number of default and delay events prior to 2019.

We observe each firm's bank relationships, so we include bank fixed effects and the average size of the firm's lenders (log of bank assets). Finally, we add industry and county fixed effects[13], which account for the heterogeneous impact of the crisis across sectors (e.g., services vs. manufacturing) and across regions with different population densities. Since default may depend nonlinearly on firm characteristics, we include squared terms for all continuous controls.

Despite the rich set of controls, unobserved factors correlated with both the default probability and the approval decision may bias the OLS estimate of β_t. For instance, firms that were more severely affected by the crisis are both more likely to default and more likely to be rejected by banks.[14] To address this concern, we propose an instrumental variable that allows us to recover an unbiased estimate of β_t. The instrument exploits differences across banks in their approval

[13]The dataset includes 100 industries (comparable to NAICS 3-digit categories) and 341 counties (*comunas*, the smallest administrative unit in Chile).

[14]In our setting, the GGL is only partially guaranteed by the government, so banks still bear part of the default risk.

policies toward firms with similar pre-crisis characteristics. Pre-crisis bank relationships thus generate firm-level exposure to heterogeneities in bank-firm-level approval policies.

Instrument construction We exploit firm–bank level application data, introduced in Section 2.1, to construct the instrument. For each firm i that applied to bank b, we observe whether the loan was approved and disbursed, or rejected based on the bank's assessment of creditworthiness given the policy-set terms of the GGL.[15] We encode this outcome as $\mathbb{1}\left[Approved\right]_{ib}$, which equals one if the GGL was approved and disbursed, and zero otherwise.

We begin by recovering the *systematic component* of each bank's GGL approval policy.[16] For each bank b, we estimate:

$$\mathbb{1}\left[Approved\right]_{ib} = \underbrace{X_i' \Gamma_b}_{\text{systematic component}} + \underbrace{\eta_{ib}}_{\text{idiosyncratic component}} \tag{2}$$

The systematic component recovers the probability that bank b approves firms with a given set of pre-crisis characteristics X_i. These characteristics are the same as in (1), including an indicator for whether the firm had a pre-crisis relationship with the bank. The idiosyncratic component, η_{ib}, captures residual variation that explains why some firms are more likely to be approved by the bank despite similar observable characteristics.

We then predict the approval probability from model (2) for all possible firm–bank combinations, regardless of whether the firm actually applied. We denote this prediction by $X_i'\widehat{\Gamma}_b$, which measures how lenient bank b is in approving firms with characteristics X_i. We clip the predicted probabilities of our linear model to lie within the $[0, 1]$ interval.

For the same set of characteristics X_i, approval probabilities vary substantially across banks. We quantify this cross-bank heterogeneity—the *disagreement* in approval policies—by computing, for each firm i, the standard deviation of $X_i'\widehat{\Gamma}_b$ across banks. Figure A2a plots the CDF of this standard deviation. For reference, maximal disagreement occurs when half the banks approve with probability 1 and the other half reject with probability 0, yielding a standard deviation of 0.5. In our data, nearly 25% of firms exhibit a standard deviation above 0.3. Such widespread heterogeneity in bank policies is consistent with evidence from other credit markets where interest rates are fixed.[17]

[15] We exclude applications that were not granted for reasons other than those mentioned above, such as missing documentation, failure to meet eligibility criteria, or voluntary withdrawal of the application.

[16] We drop marginal financial institutions with fewer than 1000 applications in total. The remaining include all major national banks with which most firms maintain relationships. This filtering has minimal impact on the sample: the instrument cannot be defined for only 2% of the firms in our main panel, which explains the difference in number of observations of Table A1 and 1.

[17] In our setting, the GGL interest rate was fixed by the government. Matcham (2025) documents substantial cross-bank variation in credit card limits in the UK, where rate dispersion is similarly limited.

Finally, we aggregate firm exposure to this cross-bank heterogeneity through pre-crisis bank relationships. We define the instrument Z_i as:

$$Z_i = \frac{1}{N_i} \sum_{b(i)} X_i' \widehat{\Gamma}_b \tag{3}$$

where N_i is the number of banks with which firm i maintained a relationship in 2019, and $b(i)$ denotes that set of banks.[18] The banking literature has widely used pre-existing bank relationships as a transmission channel for bank-level shocks or policy changes (Khwaja and Mian, 2008; Chodorow-Reich, 2014; Amiti and Weinstein, 2018; Bonfim, Ferreira, Queiró, and Zhao, 2025; Federico, Hassan, and Rappoport, 2025). Stable firm–bank relationships in Chile have been documented by Acosta-Henao, Pratap, and Taboada (2023).

Panel B of Figure A2a displays the CDF of the instrument Z_i. Because it is measured in the space of approval probabilities, its magnitude is directly interpretable. Consistent with the high overall approval rate (86% of applicants), the instrument assigns most firms a bank leniency of at least 0.5, yet still provides substantial variation across the range 0.5–1. The instrument is strongly and significantly correlated with actual approval. Moving from the 25th to the 75th percentile of its distribution increases the approval probability by 9 pp The first-stage F-statistic is 826, well above the conventional threshold of 10. First-stage estimates are reported in Table A2.

Our instrument adapts the framework of the judge IV literature (Kling, 2006; Aizer and Doyle Jr, 2015) to a context in which the "judge" (bank) is not randomly assigned by institutional design. Instead, we exploit pre-crisis firm-bank relationships as a source of quasi-exogenous exposure to heterogeneous bank approval policies. Furthermore, we estimate each bank's GGL leniency conditional on firm characteristics, so that the resulting exposure varies not only across banks but also across firms with different observable traits.

3.2. Identification and threats

Our identification strategy assumes that exposure to bank approval policies affects firms' default probabilities only through GGL assignment. Although this exclusion restriction cannot be directly tested, we present several arguments that mitigate concerns about potential violations.

First, we account for a broad set of pre-crisis firm characteristics that capture differences in both financial health and exposure to the crisis. In particular, we include 3-digit industry and county fixed effects to control for heterogeneous shocks across sectors (e.g., agriculture vs. services) and variation in local economic conditions such as population density. To further control for differences in pre-crisis credit risk, we include three complementary measures: (i) the

[18] The average (median) firm has 1.6 (1) bank relationships, as noted in Table A1.

share of outstanding debt backed by collateral, (ii) the share of outstanding debt provisioned by banks to cover expected losses, and (iii) a model-based one-year-ahead default probability. These controls substantially limit the scope for omitted variables correlated with both approval and default.

Second, a potential concern is that firms more vulnerable to the crisis might be systematically concentrated in banks that will be more lenient in their GGL approval policy. To address this, we include the identity of each bank the firm is client of and the average size of its lenders (log of bank assets) as controls. With this, we absorb any systematic differences across bank's clienteles.

Third, firms were not required to apply through their pre-crisis banks and could in principle "shop around" for more lenient lenders. Such behavior, however, was likely limited: switching banks during the crisis would have required opening new accounts and undergoing an extensive evaluation process to obtain a GGL since they would have no prior relationship data. Empirically, 12% of firms applied to banks outside their pre-crisis network. To eliminate residual concerns, our measure of exposure is constructed exclusively from pre-crisis bank relationships, which provide exposure that is not influenced by the crisis.

Finally, while our instrument isolates exogenous variation in approval decisions, GGL approval could have indirectly facilitated additional lending in subsequent years. In that case, β_t would reflect the combined effect of GGL approval and any follow-on credit expansions it induced. In Table A3 we show that our results are similar, although less precise, to excluding firms that obtained additional loans beyond the GGL in the year of the crisis. Similarly, if receiving a GGL altered firms' capacity to absorb future shocks (due to a worsening of their financial health), our estimates should be interpreted as capturing the total effect of the GGL, inclusive of such interactions.

3.3. The effect of the GGL on default probability

We report our headline estimate of the effect of the GGL on default probability in Panel A of Table 1. We present both OLS and 2SLS estimates of β_t of (1), obtained by running the regression separately for each year. In this section, we focus on the IV estimates, while the comparison with OLS and its implications for bank-side selection is deferred to the following section.

We find that the GGL reduced the probability of firm default by 2.5 pp in 2020. By 2021, the cumulative effect grew to 5 pp We interpret these estimates as the *short-run effect* of the GGL. However, after 2022, the effect is no longer statistically significant, suggesting that the initial reduction in default probability was fully reversed.

To investigate the source of this reversal, we perform a similar regression that, in each period, excludes firms that defaulted in the previous year. The results are presented in Panel B of Table 1. Because eligibility for the GGL required firms to be free of prior defaults, the 2020 coefficients are identical across Panels A and B. The IV estimates indicate a similar short-run reduction in default probability of 2.5 pp and 2.9 pp in 2020 and 2021. After 2022, however, the effect reverses: GGL approval increases the probability of default by 4 pp among firms that had not previously defaulted.

We interpret this *positive* effect on defaults as evidence of a long-run consequence of the GGL. As we showed in Figure 1, the program led to a substantial expansion in the debt of approved firms, leaving them more exposed to default risk in subsequent years. This pattern suggests that the GGL temporarily postponed, rather than permanently prevented, firm defaults.

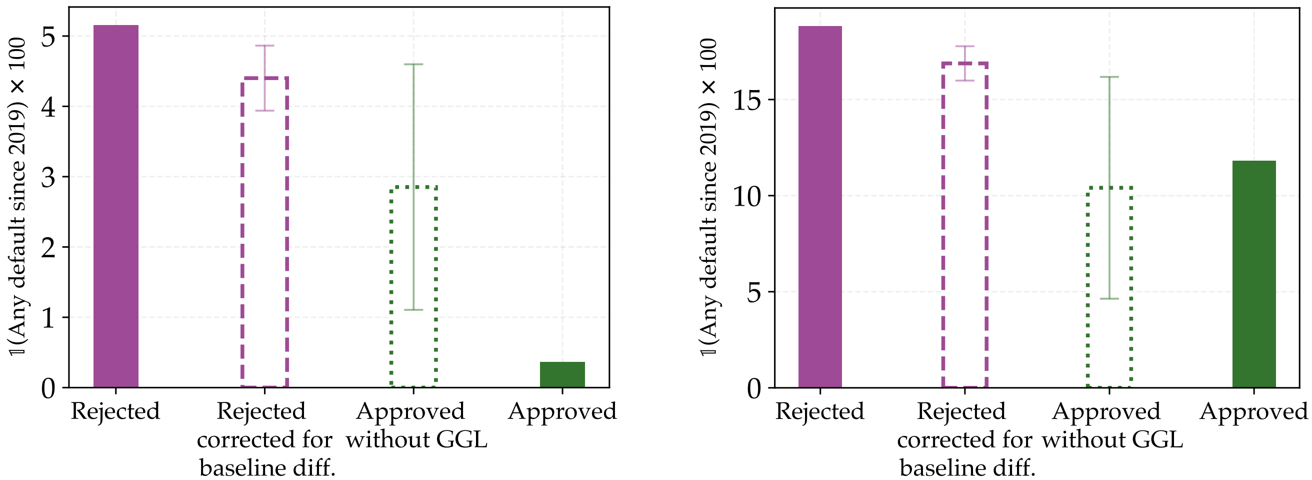
Table 1: Effect of GGL on default probability Panel A reports estimates of (1) by OLS and 2SLS, using the instrument defined in (3). Regressions are estimated separately for each year. Panel B reports the same specification, excluding in each year firms that defaulted in any previous year. We report the first-stage F-statistic. Controls for 2019 include all variables X_i defined in Section 3.1. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

	2020	2021	2022	2023	2024
Panel A	$\mathbb{1}\left[\text{Any default since 2019}\right] \times 100$				
$\mathbb{1}$ [*Approved*] (OLS)	−4.036***	−5.501***	−5.602***	−5.619***	−5.072***
	(0.236)	(0.308)	(0.364)	(0.419)	(0.456)
$\mathbb{1}$ [*Approved*] (IV)	−2.487***	−5.257***	−1.236	−1.491	1.4
	(0.891)	(1.514)	(2.012)	(2.571)	(2.944)
F-stat	826	826	826	826	826
$\mathbb{E}$[Outcome | Approved]	0.365	1.789	4.56	8.132	11.808
Obs.	61387	61387	61387	61387	61387
Panel B	$\mathbb{1}$ [Default | not defaulted since 2019] × 100				
$\mathbb{1}$ [*Approved*] (OLS)	−4.036***	−1.654***	−0.411*	−0.411	0.208
	(0.236)	(0.220)	(0.234)	(0.271)	(0.262)
$\mathbb{1}$ [*Approved*] (IV)	−2.487***	−2.902**	3.913***	−0.353	3.058*
	(0.891)	(1.273)	(1.455)	(1.832)	(1.779)
F (1st stage)	826	819	822	803	795
Obs.	61387	60867	59871	58139	55932
Controls 2019	✓	✓	✓	✓	✓
County, Industry & Bank FE	✓	✓	✓	✓	✓

3.4. Bank-side selection

By comparing the OLS and IV estimates in Table 1, we find that banks approved firms with systematically lower default probabilities, *even in the absence* of the GGL. To quantify this, we decompose the observed difference in default probability by 2020 between approved and rejected firms into three components: (i) a component explained by pre-crisis firm characteristics, (ii) a component explained by banks' private information about firms' unobservables, and (iii) a component explained by the causal effect of the GGL itself. We recover these components using the OLS and IV estimates.

Figure 2a illustrates this decomposition for 2020. Starting from the left, the first bar shows the observed default probability of rejected firms, while the last bar shows the observed default probability of approved firms. The total difference between these two groups is 4.8 pp, which correspond to the larger default of rejected firms shown in Figure 1.



(a) Cumulative default by 2020

(b) Cumulative default by 2024

Figure 2: Decomposition of default probability. The figure in panel (a) decomposes the difference in default probability between rejected (leftmost bar) and approved (rightmost bar) firms by 2020. The dashed bar shows rejected firms' default probability if they had the same pre-crisis characteristics as approved firms, which is the result of substracting the OLS estimate on the approved default probability (rightmost bar). The dotted bar shows approved firms' default probability in the absence of the GGL, which is the result of substracting the IV estimate on the approved default probability (rightmost bar). Similarly, panel (b) presents the same decomposition for the probability of experiencing any default since 2019 by 2024. Error bars represent a 95% confidence interval.

The second bar represents the counterfactual default probability of rejected firms if they shared the same pre-crisis characteristics as approved firms. We obtain this value by adding the OLS estimate to the observed default rate of approved firms. This implies that 0.8 pp out of 4.8 pp of the total difference is explained by observable pre-crisis characteristics. Conventional predictors of default, such as leverage, returns (Altman, 1968); or pre-crisis risk measures (Huneeus, Kaboski, Larrain, Schmukler, and Vera, 2025), account for this portion of the total gap.

The third bar shows the counterfactual default probability of approved firms in the absence of the GGL, obtained by subtracting the IV estimate from the observed default rate. An additional 1.5 pp of the difference is explained by unobserved firm characteristics, such as the heterogeneous impact of the crisis, that are partially captured by banks when making approval decisions.

We interpret this component as evidence of banks' private information. Because banks were only partially insured by the government guarantee, they had an incentive to allocate loans to relatively safer firms. The remaining 2.5 pp of the 4.8 pp gap reflects the short-run causal effect of the GGL itself.

This finding depends on the set of controls X_i available in our data. Table A3 reports robustness checks excluding subsets of variables from X_i in the second-stage regression. The results remain stable when dropping variables such as sales or debt, but omitting pre-crisis bank-assessed risk variables increases the estimated GGL effect in absolute value. The availability of pre-crisis risk differences help us to not overestimate the (absolute) effect of the GGL on default probability.

Overall, the decomposition shows that banks allocated GGLs to firms with lower default probabilities in the short run, even in the absence of the GGL. As we extend the analysis over time, these conclusions remain qualitatively valid. While the OLS estimates are similar across years, our IV estimates show a temporary effect of the GGL on default probability. We then recover the bank-side selection component in the long run by replicating the decomposition for 2024, as shown in Figure 2b. This is our preferred measure of selection, since it accounts for the default path over the entire duration of the crisis loan. As the figure shows, the effect of the loan is not significant in 2024, and the observed differences are mostly driven by bank-side selection (6.5 pp) and pre-crisis characteristics (1.9 pp).

Policy implications of bank-side selection Our previous result shows that banks approved loans to relatively safer firms, even in the absence of the GGL. The fact that banks systematically selected safer firms supports one rationale for delegating liquidity allocation to the banking system. If we assume that the policymaker's information set resembles ours,[19] a direct allocation policy based solely on observable pre-crisis characteristics would fail to exploit banks' private information about firms. By contrast, delegating loan approval to banks—with appropriate incentives through partial guarantees—allows the policymaker to leverage banks' private information about borrowers' repayment prospects. Whether this is desirable from a welfare perspective is examined in Section 4.

3.5. Smaller, higher-leverage firms drive the short-run effect

To understand which firms drive the aggregate results, we partition the sample of applicants into four groups. First, we split firms into two halves by their size using the median of assets in 2019. Within each half, we further divide them into quarters by within-half median leverage.

[19] We interpret our information set as comparable since it covers all major administrative records that the policymaker would have had access to at the time of the crisis.

Table 2 reports the IV estimates of the effect of the GGL across years and quarters.

We find that only smaller, more levered firms are driving the short-run effect of the GGL. Moreover, the effects are larger in magnitude for this subgroup: by 2021, the reduction of default probability is almost 16 pp for this subgroup. The rest of the subgroups show no significant effect either in the short-run or the long-run. We interpret this as evidence of the type of firms that benefited from the GGL: smaller, more levered firms are likely to face tighter credit constraints during the crisis, so the GGL provides a larger benefit to them. Firms with no debt or with significant assets are likely to face better access to credit, so the GGL do not provide any significant benefit in terms of default.

Table 2: Effect of the GGL across assets and leverage We report the IV estimates of (1) across subgroups of firms and years. We split firms into two halves by their assets in 2019, and within each half, we further divide them into quarters by within-half median leverage. We report the F statistic of excluded instruments of the first-stage for each subgroup. All regressions are estimated separately for each year and subgroup. Robust standard errors in parenthesis. $^{***} = p < 0.01$, $^{**} = p < 0.05$, and $^{*} = p < 0.1$

Outcome	All	Assets < p50		Assets > p50	
$\mathbb{1}\left[\text{Any default since 2019}\right] \times 100$		Lev. < p50	Lev. > p50	Lev. < p50	Lev. > p50
2020	−2.487***	−2.043	−4.292*	−2.648*	−1.212
	(0.891)	(2.915)	(2.372)	(1.450)	(1.319)
2021	−5.257***	−3.754	−15.538***	−2.553	−1.136
	(1.514)	(4.162)	(4.309)	(2.209)	(2.643)
2022	−1.236	−2.083	−9.337*	−1.407	0.889
	(2.012)	(5.847)	(5.437)	(3.079)	(4.208)
2023	−1.491	−2.530	−11.450	−2.617	−0.240
	(2.571)	(7.121)	(7.100)	(3.858)	(5.491)
2024	1.400	4.533	−8.151	3.290	−5.923
	(2.944)	(8.154)	(8.071)	(4.453)	(6.528)
Obs.	61387	15334	15302	15345	15330
F (1st stage)	826	114	161	231	183
Controls '19	✓	✓	✓	✓	✓
County, Industry & Bank FE	✓	✓	✓	✓	✓

In Table A4, we explore other outcomes of the firms. At a 5% significance threshold, we find no significant effects of the GGL on exit, (log) sales of the non-exited firms, and employment, except for employment in the long-run, which is consistent with firms struggling to meet their increased debt services.

Taking stock The reduced-form estimates suggest that GGL only reduced default in the first two years after the crisis, but in 2022 the effect is reversed. This implies that the GGL postponed defaults over time, with no accumulated effect in the long-run. Moreover, banks are using private information to assign liquidity to relatively safer firms.

4. Dynamic Model of SME default and GGL allocation

In this section, we develop a dynamic model of entrepreneurial default and GGL allocation. Entrepreneurs make investment and borrowing decisions, obtaining funds from banks through defaultable loan contracts. Banks price these loans to break even. A crisis shock hits the economy, differentially affecting firms and generating liquidity needs. During the same period, entrepreneurs may apply for a GGL, but approval is subject to bank screening, so some applications are rejected. This process generates endogenous sorting across non-applicants, approved, and rejected firms, and we estimate it using application data and the reduced-form evidence of Table 1. The model allows us to (i) infer which firms obtain GGLs in terms of their unobserved solvency and liquidity, (ii) measure the welfare gains of the policy, (iii) characterize its heterogeneous effects across the firm distribution, and (iv) evaluate counterfactual GGL interventions.

Preferences Time is discrete, and each period lasts two years.[20] There is a set of entrepreneurs with preferences over streams of consumption c_t, represented by

$$\mathbb{E}\left[\sum_{t=0}^{\infty}\beta^t u(c_t)\right], \quad \text{with } u(c) = \frac{c^{1-\sigma}-1}{1-\sigma}, \tag{4}$$

where β denotes the time discount factor and σ the coefficient of relative risk aversion.

Technology Entrepreneurs have heterogeneous technologies, characterized by a time-invariant long-run productivity level A and a time-varying, persistent idiosyncratic TFP shock z. In addition, a one-period crisis shock ε_k affects the effective capital stock in the crisis period $t = 0$; its role will be described in detail when we introduce the crisis and GGL intervention. In all other periods $t > 0$, we normalize $\varepsilon_k = 1$.

Entrepreneurs choose next-period capital k' to produce

$$y' = Az'(\varepsilon_k k')^{\alpha}, \tag{5}$$

units of the consumption good in the following period. Since we focus on SMEs, we assume $\alpha < 1$, implying decreasing returns to scale and an optimal firm size. We assume that $\ln(z)$ follows an AR(1) process, with persistence parameter ρ_z and innovation variance σ_z^2. Capital depreciates at a constant rate δ.

[20] This frequency corresponds to the median maturity of market loans before the crisis. Setting this length allows us to model standard bank loans as one-period loans.

Finance Entrepreneurs can borrow from a bank at market rates. They face a loan price schedule $q(z, b', k', A)$ per unit of debt. Debt is defaultable: in each period, after observing the new productivity shock z', the entrepreneur decides whether to default.

Default triggers three consequences. First, the entrepreneur repays only a fraction of the contracted debt, $H(b) < b$, reflecting partial recovery by the bank through collateral liquidation, renegotiation, or asset seizure. Second, as long as the entrepreneur is in default, her output is reduced to $\Lambda(y) < y$. This captures deadweight losses due to restructuring costs, the loss of complementary banking services (e.g., payment processing), or penalty rates imposed by the bank. Finally, the entrepreneur is excluded from credit markets until an i.i.d. recovery shock occurs, which restores borrowing access with probability χ.

Alternatively, entrepreneurs can save $b' \leq 0$, and earn the risk-free rate r. Saving is feasible even while in default.

Entry and exit At the beginning of each period, entrepreneurs receive an i.i.d. exit shock with probability ψ. When the shock occurs, the entrepreneur stops making investment and borrowing decisions. She still enjoys one final period of utility and decides whether to default. This final decision trades off the output loss $\Lambda(y)$ associated with default against the debt haircut $H(b')$, since there is no continuation value after exit. The exiting entrepreneur is then replaced by a new entrant who starts with no debt and an arbitrarily low level of capital.

Banks A risk-neutral, deep-pocket, representative bank prices loans to break-even:

$$q(z, b', k', A)b' = (1 + r)^{-1}\mathbb{E}\left[\mathbf{d}b' + (1 - \mathbf{d})H(b')\right], \tag{6}$$

where $\mathbf{d}$ is the expected default policy, $\mathbf{d} = (1 - \psi)\tilde{\mathbf{d}} + \psi\hat{\mathbf{d}}$, which averages the default policy $\hat{\mathbf{d}}$ when an exit shock arrives, and the default policy $\tilde{\mathbf{d}}$ without an exit shock.

Timing To summarize, the sequence of events in each period is illustrated in Figure 3.

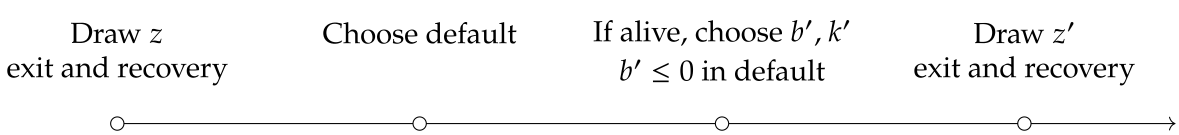


Figure 3: Model timing at $t \geq 3$. The figure illustrates the sequence of decisions within a period. At the beginning of the period, the entrepreneur draws the TFP shock z, the exit shock, and the recovery shock. She then decides whether to default. If she does not default, she chooses next-period investment k' and borrowing b'. If she defaults, she instead chooses investment k' and savings $b' \leq 0$. In the next period, the entrepreneur draws all three shocks again.

Crisis and GGL intervention At $t = 0$, there is a crisis shock with a heterogeneous impact on the capital quality of the entrepreneur ε_k[21]. We assume it is unanticipated to generate sudden liquidity needs, we allow some firms to have an actual benefit from the crisis time, reflecting the heterogeneous nature of the COVID-19 crisis (e.g. restaurants vs. online shops), but every firm draws from a common distribution $F(\varepsilon_k)$. The assumption of a temporary shock is reasonable, given the two-year period of our model and the fact that Chile's GDP was already recovered two years after the crisis. This shock is drawn at the same time as the TFP, exit, and recovery shocks.

After learning the shocks and deciding whether to default, a non-death, non-defaulting entrepreneur is eligible to apply for a GGL loan of size $q^G b^G$ at $t = 0$. We model the GGL as a special two-period loan[22] that is only available in $t = 0$ by eligible (non-death, non-defaulting) entrepreneurs. The GGL co-exists with the standard market-lending option, and both are treated *pari passu* upon default. We assume that at $t = 1$, the entrepreneur makes no GGL payment, and all GGL debt is serviced in $t = 2$. The price of the GGL loan q^G is set by the government, and we capture the GGL borrowing limit by setting the maximum amount of GGL debt to ϕA[23].

Application procedure Firms choose to apply to the GGL program. If they apply, they also choose the loan size $b^G \leq \phi A$. We assume that entrepreneurs face an idiosyncratic non-pecuniary cost of application $\kappa^{firm} \sim F(\kappa^{firm})$. This cost captures entrepreneurs' time and effort to lean about the GGL program and apply. An eligible entrepreneur will apply for a GGL if

$$V^{app}(z_0, b_0, k_0, A) - \kappa^{firm} \geq V_0^r(z_0, b_0, k_0, A, 0), \tag{7}$$

where $V^{app}(z_0, b_0, k_0, A)$ is the value of applying at an optimal size b^G, and $V_0^r(z_0, b_0, k_0, A, 0)$ is the value of not applying, represented by the repayment value when the size of the loan is zero.

After deciding to apply, the entrepreneur optimally chooses the size of the GGL loan b^G, so $V^{app}(z_0, b_0, k_0, A) = \max_{b^G} V_0(z_0, b_0, k_0, A, b^G)$. The maximum amount she can apply for is ϕA, which captures the cap on the size of the loan.

The bank receives the application of size b^G and decides to accept or reject it. To do so, it evaluates the expected payoff, considering the exposure to 2-period default risk, the guarantee share g^G that the government sets to partially insure the bank against default, and the fix price of the GGL loan q^G. We allow banks wrongly evaluate applications, so the probability of acceptance is non-zero even for firms with low expected payoff. This captures the fact that

[21]While we model it as a shock to capital quality due to computational convenience, to save on state variables, any shock to firms' cash-on-hand $y + (1 - \delta)k$ would have the same effect.

[22]Consistent with a GGL program with 4 years of maturity.

[23]While the GGL was limited to a fraction of pre-crisis sales, this modelling assumption allows us to save on state variables and still capture the scale limit of the GGL. We adjust ϕ to approximate the mean and maximum loan size of the GGL, as reported in Table A1.

default is a hard-to-predict event, and allows us to generate rejections in equilibrium. We do so by introducing a shock κ^{bank} that is drawn from a Logistic distribution with mean 0 and shape parameter σ_{bank}.

The bank accepts an application if:

$$\mathbb{E}_{t=0}\left[\underbrace{(1-\mathbf{d}_1)(1-\mathbf{d}_2)}_{\substack{\text{Full repayment}\\ \text{(FP)}}}+\underbrace{g^G\left[\mathbf{d}_1+(1-\mathbf{d}_1)\mathbf{d}_2\right]}_{\substack{\text{taxpayer cost}\\ \text{(TP)}}}-\underbrace{q^G b^G(1+r)^2}_{\substack{\text{Cost of funds}\\ \text{(CF)}}}+\kappa^{bank}\right]\geq 0 \tag{8}$$

Equation (8) splits the components of the bank's expected payoff. The term FP represents the repayment of the loan when firms do not default in $t = 1$ and $t = 2$. The term TP represents the subsidy that the government gives to incentivize the bank to lend to risky firms. It is paid either when the firm defaults in $t = 1$ or $t = 2$ and is equivalent to the expected taxpayer cost of the government guarantee. We assume that the government makes all insurance payments at the terminal period, so the bank do not gain from early insurance payments[24] The bank compares these two terms with the cost of opportunity of lending to the firm CF and the idiosyncratic shock κ^{bank} that captures bank's ability to correctly evaluate the application.

The logit assumption implies the probability of approval is:

$$\mathbb{P}(\text{approve})=\frac{\exp\left(\frac{FP+TP-CF}{\sigma_{bank}}\right)}{1+\exp\left(\frac{FP+TP-CF}{\sigma_{bank}}\right)} \tag{9}$$

Once the bank approves the application, the funds are transferred to the entrepreneur, and she decides to invest k_1 and borrow b_1 any extra amount from the bank at the price schedule given by (6).

The figure 4 summarizes the timing and payoffs of the application process.

In Appendix D, we detail the Bellman equations of the entrepreneur and in Appendix E, we describe the numerical solution method.

Discussion Our model generates sorting of entrepreneurs across non-applicants, approved and rejected. Everything else equal, a larger draw of κ^{firm} will push the entrepreneur not to apply for a GGL. In turn, κ^{bank} controls the sorting into rejected or approved. In absence of

[24]This captures the institutional fact that the guarantee only covered the principal, not the interest accrued, according to Article 19 of its regulation (CMF, 2016).

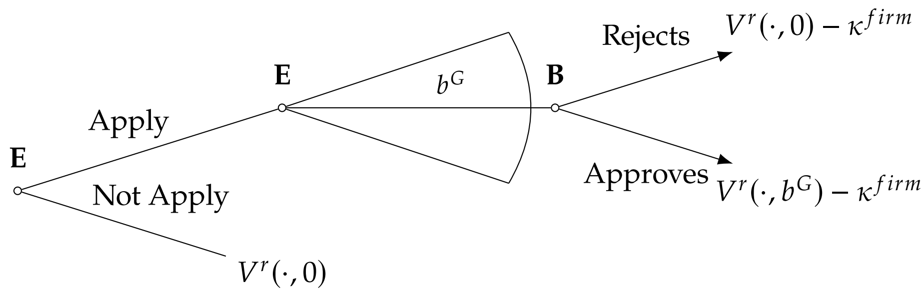


Figure 4: GGL application process. The figure shows the timing and payoffs of the application process. **E** represents the entrepreneur, **B** represents the bank. $V^r(\cdot, b^g)$ represents the value at the repayment state when the size of the loan is b^g, and κ^{firm} is the idiosyncratic cost of application.

κ^{bank}, the bank would only accept applications if $FP + TP - CF \geq 0$. Note that this implies a rent component to the bank approval decision that is the product of a fixed price of the GGL loan q^G but a variable return that depends on the risk profile of the firm. The shape parameter σ_{bank} will control how sensitive the bank is to this rent $FP + TP - CF$ to reject an application. In the extreme, if σ_{bank} is zero, the model produces no rejections in equilibrium since a too risky entrepreneur would fully anticipate the rejection.

5. Model Quantification

We quantify the model in three steps. We first preset parameters from the literature, or extracted directly from the data. Second, we estimate our model when no crisis nor GGL intervention happens, to match moments of the pre-crisis period. Last, we estimate our model when crisis and GGL intervention happen, to match moments of the post-crisis period, and the estimated effects of the GGL on default probability.

Preset parameters Following Herranz, Krasa, and Villamil (2015), who estimate the distribution of CRRA parameters among U.S. entrepreneurs, we set the coefficient of relative risk aversion to $\sigma = 1.5$, the median of their estimates.

We build an auxiliary dataset to recover the production function parameters. This dataset intends to track firms with similar characteristics to the main sample. We use firms in the period 2016–2019 that are within the percentiles 5 and 95 of the distribution of sales, wage bill, assets, debt, returns, leverage and age of firms in the main sample.

On this auxiliary dataset, we estimate the following production function:

$$\ln(\text{sales}_{it}) = \ln A_i + \alpha \ln(\text{assets}_{it}) + \mathbf{\Gamma}' \widetilde{X}_{it} + \widetilde{\epsilon}_{it} \tag{10}$$

where $\tilde{X}_{it}$ is a vector of variables that are likely to affect sales, but we do not explicitly include in the structural model[25]

This regression recovers the an estimate of the capital shape $\alpha = 0.23$, and the long-run TFP $\ln \tilde{A}_i$ of each firm. For the numerical solution, we bin the firms into 4 bins of TFP, taking the mean of $\ln(\tilde{A}_i)$ as representative of the bin. We adopt $\rho_z = 0.966$ and $\delta = 0.10$ from Kochen (2023).[26] Given ρ_z and the estimated standard deviation of $\tilde{\epsilon}_{it}$, we recover the implied volatility of the TFP innovation $\sigma_z = 0.138$.

The risk-free rate is set to $r = 2.7\%$, corresponding to the average pre-crisis deposit rate.[27] The probability of firm exit is $\psi = 4\%$, the average annual exit rate in 2017–2019, while the recovery probability is set to $\chi = 25\%$, to match the share of pre-crisis firms that recovered from default.

The haircut function is defined as

$$H(b) = \begin{cases} hb, & \text{if } y + (1-\delta)k - hb > 0, \\ \underline{k}, & \text{otherwise,} \end{cases}$$

where $h = 0.15$ corresponds to the average collateral-to-debt ratio in 2019 in our main panel, and $\underline{k}_h$ is set to an arbitrary low value, so consumption is always positive in the event of default.

We set q^G to match the APR of 3.5%, the fix rate of the GGL. For simplicity, we abstract size-specific guarantees of our setting, and we set a uniform guarantee share of 74%, which corresponds to the average guarantee share in our main panel.

Pre-crisis estimation We estimate the discount factor β and the cost of default λ using our model in a world without crisis nor GGL intervention. We search for the combination of $\{\beta, \lambda\}$ that matches a mean default rate of 6% and a mean leverage of 30% our pre-crisis auxiliary dataset.

Crisis estimation Armed with estimated $\{\beta, \lambda\}$ from the pre-crisis estimation, we now can estimate crisis and GGL application parameters. Table 3 summarizes all the parameters of the model. We parametrize the crisis shock as a log-normal distribution with mean μ_ε and standard deviation σ_ε, and the cost of application as normally distributed with mean μ_{app} and standard deviation σ_{app}. We search for $\{\mu_\varepsilon, \sigma_\varepsilon, \mu_{app}, \sigma_{app}, \sigma_{bank}\}$ that minimizes the sum of percentage deviation of a set of moments of the post-crisis period. These moments are: the share of applicants over eligible firms (58%), the share of rejected over applicants (14%), the default

[25] Logs of age (in levels and squared).

[26] All rates are reported on an annual basis. Since one model period corresponds to two years, we compound rates accordingly.

[27] Source: International Financial Statistics, International Monetary Fund.

rate of approved firms in 2021 (1.8%), the default rate of rejected firms in 2021 (8.4%), the default rate of non-applicants firms in 2021 (4.5%), and the effect of the loan estimated by IV regression β^{IV}_{2021} of Table 1, (-5.2pp).

Table 3: Model parameters We report the parameters of the model. The first group of parameters is preset, based on literature, extracted directly from the data or estimated through (10). The second group of parameters is estimated through GMM to match leverage (30%) and default rate (6%) in the pre-crisis. Last we use indirect inference to estimate the last group of parameters, to match the sorting into applicants and rejected, the default rates by group in 2021 and the effect of the loan estimated by IV regression β^{IV}_{2021} of Table 1. Parameters with * are reported annually, but in the model they are compounded to match the two-year period.

Method	Parameter	Description	Value	Comment
Preset	σ	Risk aversion	1.5	Herranz et al. ('15)
	δ	Depreciation rate	0.10*	Kochen ('25)
	ρ_z	Persistence TFP shock	0.966*	Kochen ('25)
	h	Fractional haircut	85%	Share of collateral (15%)
	R_f	Risk-free rate	2.7%*	Observed
	ψ	Exit shock	4%*	Observed
	χ	Re-entry probability	25%	Observed
	α	Capital shape	0.23	Aux. Regressions (10)
	$\ln A$	Long-run TFP	9.3 ± 1.7	4 bins, Aux. Regressions (10)
	σ_z	Volatility TFP shock	0.138	Aux. Regressions (10)
Estimated (pre-crisis)	β	Discount factor	0.966	-
	λ	Cost of default	0.0566	-
Estimated (crisis)	μ_ε	Mean crisis shock	0.805	-
	σ_ε	Volatility crisis shock	0.439	-
	μ_{app}	Mean application cost	9.48×10^{-4}	-
	σ_{app}	Volatility application cost	3.92×10^{-3}	-
	σ_{bank}	Bank evaluation noise	4.54×10^{-4}	-

To estimate β^{IV} within the model, we compute for each firm the probability of approval given by (9) for each firm in the simulation. Then, we draw a uniform shock and compare this to the probability of approval. We use this uniform shock as an instrument and we run a 2SLS regression on the default status of applicant on the approval status using the instrument, as the model counterpart of the IV regression of Section 3.

Model Fit Our model closely matches the default patters and the sorting of firms into applicants, rejected and non-applicants. Figure 5 shows the model fit on the cumulative default rates of approved, rejected and non-applicants through 2021 and 2023, which we interpret as $t = 1, 2$ of the model. In particular, we can replicate the patters of low default for approved firms in 2021: high default rates for rejected firms and mid default rates for non-applicants. Even if the estimation during crisis is overidentified, the model accommodates the data well.

The model also successfully predicts the default rates of the three groups of firms in 2023, which are not targeted in the estimation. Moreover, it reproduces a large default increase for

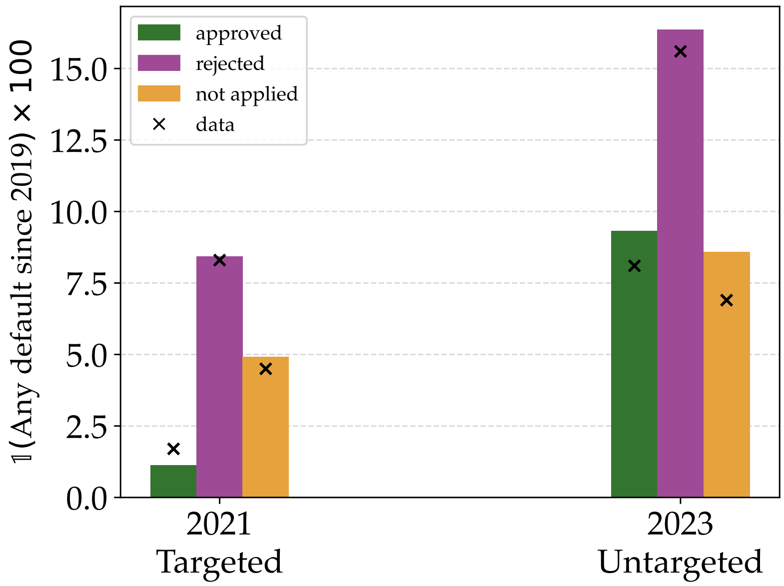


Figure 5: Model fit, predictions and data. The figure shows the model fit for default rates of approved, rejected and non-applicants firms through 2021 and 2023. The bars are model generated moments, and the crosses are the data counterparts. Moments in 2021 are targeted in the estimation, while moments in 2023 are untargeted. The rest of the moments targeted are the share of applicants over eligible firms in the model (data) is 62% (58%), the share of rejected over applicants is 15% (14%), and the β^{IV}_{2021} coefficient is -5.5pp (-5.2pp).

approved firms in 2023, even larger than those that did not apply for a loan, but less than rejected firms. These predictions validate the model as a tool to study the effect of the crisis and the GGL on default probability.

6. Results of the Structural Model

In this section, we first use our estimated model to learn who receives the support in terms of long-run TFP and crisis shock, which generate heterogeneity in solvency and liquidity, respectively. Then, we discuss the welfare gains of the policy and its cost-effectiveness, on aggregate and across the firm distribution. Then, we measure the extent of potential negative consequences of the policy, such as excess risk-taking and rent-seeking. Again, the model allows us to detect which type of firms are more likely to take excess risk and generate bank rents. Finally, we study counterfactual designs of the policy that are budget-neutral to assess whether we can improve the cost-effectiveness of the policy.

6.1. Who applied and who received a GGL

We use the model to infer which firms applied and received a GGL. We focus on the firms' long-run TFP A and crisis shock ε_k, which generate heterogeneity in solvency and liquidity, respectively. Figure 6, left panel, shows the probability of applying for a GGL conditional on being eligible across the firm distribution. Firms are more likely to apply when the crisis hurts them more and their scale is smaller. A larger shock increases the risk of default, so firms would face higher borrowing costs using standard bank loans. For the same shock bin, firms with a larger long-run TFP are safer, since the upside of a recovery shock is larger. Then they are less likely to apply for a GGL, and instead they they use standard bank loans or their own savings.

The right panel of Figure 6 shows the probability of receiving a GGL conditional on applying. Overall, the probability of receiving a GGL is large since the model approximates the observed approval rate of 86%. Conditional on long-run TFP, the probability of receiving a GGL increases as the shock increases. Everything else equal, banks are more likely to approve firms that are safer, since they are exposed to default risk, albeit by a smaller share due to the guarantee. This even allows firms that were positively affected to apply for a larger loan: firms in the best crisis shock bin apply for loans that are on average 0.9% of their pre-crisis sales, whereas firms in the worst crisis shock bin apply for loans that are on average 0.6% of their pre-crisis sales.

An interesting finding is the fact that while firms that were benefited by crisis do not show different probabilities of receiving a GGL across long-run TFP, when the shock is severe, the probability of receiving a GGL decreases with long-run TFP. The reason behind this counterintuitive finding is that firms with a larger A are firms that have a larger upside from recovering their relatively larger production scale. Then, these firms are those that are investing the most, even in absense of the policy, and so, exposing themselves to more risk. A GGL allows them to increase their investment without getting relatively more expensive debt in the market. The bank is willing to lend to them but relatively less to a firm that had a bad crisis draw, but a lower investment demand due to its smaller long-run scale.

Firms who benefit from the crisis time are safer, so tend to be more likely to satisfy the cutoff condition (8).

Interestingly, the probability of receiving a GGL tend to decrease with long-run TFP, specially at the worst draws of the crisis. This is because larger firms are optimally choosing a larger GGL loan to recover. Larger scale firms are willing to leverage more to recover from the bad draw. This implies larger GGL loans than their lower scale counterparts, larger even in proportion to their scale. The operating binding constraint that prevents them from maxing out the policy loan is the maximum amount of risk the bank is willing to bear accoring to (8). Then, these firms are taking more risk than their lower scale counterparts, so they are less likely to being approved

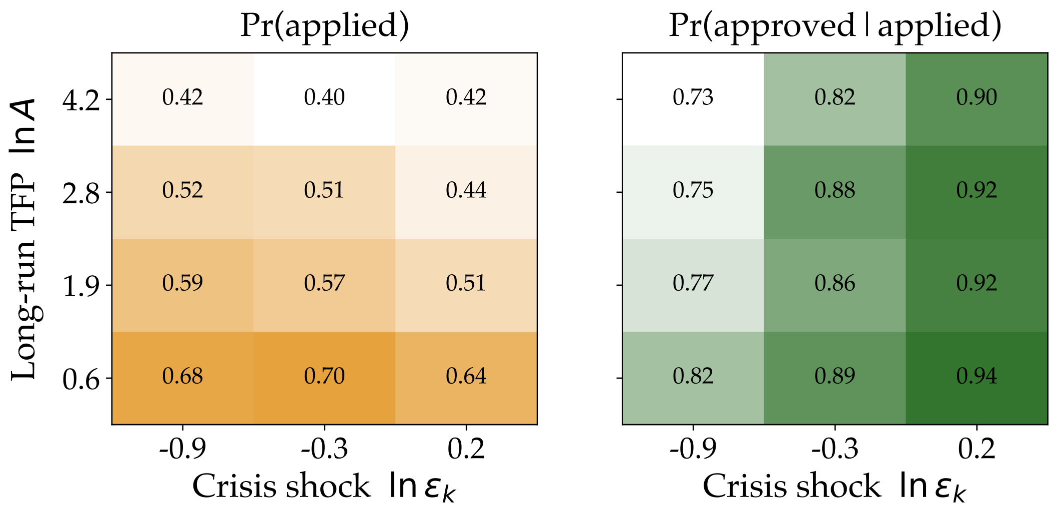


Figure 6: Who applies and who receives a GGL The left panel shows the probability of applying for a GGL conditional on being eligible for a GGL, across groups of firms with different long-run TFP and crisis exposure. Each cell represents a pair of the long-run TFP ($\ln A$), and bins of the crisis shock ($\ln \varepsilon_k$). The color represents the probability of applying for a GGL, which is reported in the center of the cell. Analogously, the right panel shows the probability of receiving a GGL conditional on applying for a GGL across the firm distribution.

for a GGL.

6.2. Welfare gains of the GGL intervention

We use the quantified model to estimate the welfare gains of the GGL intervention. We first present results in terms of consumption equivalent (CE), and then we use a money-metric welfare gain measure that allows us to compare the gains with the taxpayer cost of the policy.

We define the CE of the GGL intervention as the percentage increase in one-period consumption from a scenario with crisis but no GGL intervention that is needed to make entrepreneurs indifferent between that counterfactual scenario and the factual case with both crisis and GGL. Due to the application process, we can consider values at different stages of the application. We choose to compute the CE after the GGL allocation is decided. We prefer this metric because we want to have a measure that is not influenced by the shocks on the application procedure[28].

We find that the GGL implies a CE of 1.2% on the average approved firm. This are substantial gains: they represent 21% of the average pre-crisis private consumption annual growth rate. Welfare gains are heterogeneous across firms. The 10th percentile of firms has a CE of 0.18%, while the 90th percentile has a CE of 1.81%.

[28]For example, if we include the non-pecuniary cost of application κ^{firm} is included in the value of the GGL since firms with a non-pecuneary benefit of applying will tend to self select into application.

Firms that enter to the crisis in a weaker position to absorb the shock experience larger gains. The top tercile of pre-crisis leverage has an average CE 0.45 pp.larger than the bottom tercile, where the difference for the terciles of capital is -0.15 pp.. These firms tend to be growing firms, that is, firms that have not yet reached their optimal scale. This complements the reduced-form finding of Table 2 where we show that low-assets, high-leverage firms drive the aggregate effects on default.

Welfare gains are negatively correlated with firm's crisis shock. The firms that are in the 10th percentile of the crisis shock have a CE of 1.49%, whereas those in the 90th percentile have a CE of 0.58%, which indicates that the gains are larger for firms that are more harmed by the crisis. On the contrary, the differences across long-run TFP have lower variation, ranging from 1.15% to 1.19%.

The model calibration suggests a rather accurate bank screening process. Due to the noise κ^{bank}, banks can misclassify firms as creditworthy when they are not. Only 0.6% would not have received a GGL in the absence of the friction. These "lucky gamblers" firms enjoy 0.33 pp. larger gains than the average approved.

6.3. Cost-effectiveness

We use a money-metric welfare gain measure to compare the gains with the taxpayer's cost of the policy, and assess the cost-effectiveness of the policy. We define MWG as the monetary transfer that the entrepreneur should receive in a no-policy scenario to reach the same value as in the factual policy scenario. The expected taxpayer cost (TP) is formally defined in (8), and corresponds to the expected payment of the government to the bank in case of default.

The policy was cost-effective: on aggregate, MWG exceeds TP by 21%. On one hand, firms value postponing defaults, especially those that are hitted harder by the crisis. On the other, while defaults increase in the second period, pushing up the taxpayer's costs, their increase is not abnormally high, but rather moderate. As a benchmark, they tend to be similar to the default rates of non-applicants. Then, the vast majority of the loans are repaid, keeping the taxpayer's costs within reasonable levels.

However, the aggregate cost-effectiveness masks substantial heterogeneity across the firm distribution. Near 27% of approved firms did not compensate for the cost. In Figure 7 we compare the cumulative budget expenditure as we descend by the distribution of cost-effectiveness. We measure cost-effectiveness as the ratio of MWG to TP. Almost 60% of the budget reaches cost-effective firms. The average firm in the region of 20-40% cumulative cost is more cost effective (109pp.), enters to the crisis in a more levered position (16pp.), with lower assets (-31%), and crisis hit them harder than the average firm in the region of 60-80% cumulative cost. This comparison suggests that the larger returns are on firms that are in a worse position to absorb the crisis shock.

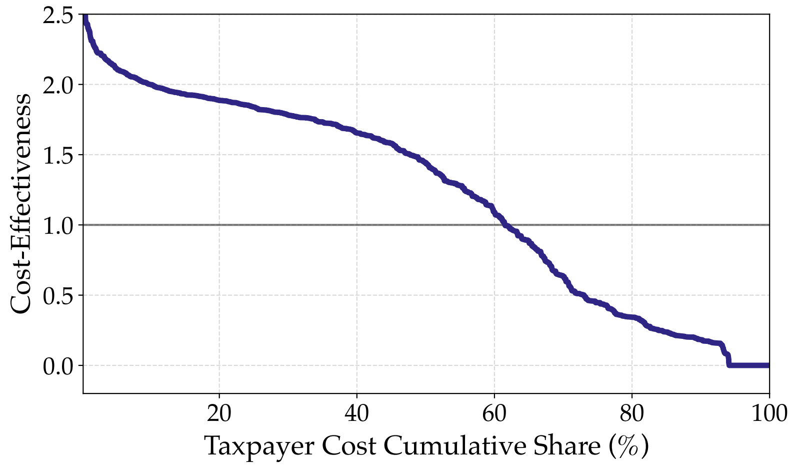


Figure 7: Cost-effectiveness heterogeneity and budget allocation The figure displays the distribution of the welfare-to-cost ratio across firms. We measure cost-effectiveness as the ratio of the monetary-metric welfare gain to the taxpayer cost. We sort firms by this ratio, and we compute the cumulative budget expenditure as we descend by the cost-effectiveness distribution.

6.4. Bank rents

As part of the GGL design, the government set *both* the guarantee share and a fixed loan rate. Consequently, banks earned rents on approved loans, whose magnitude depended on the borrower's risk profile. For instance, consider a firm with zero default probability at $t = 1, 2$: the bank would be willing to lend at any rate between the two-period compounded risk-free rate r and $1/q^G$, capturing the difference as rent. When default risk is positive, the rent is jointly determined by the expected default probability, the guarantee share, and the loan rate.

Using our calibrated model, we can quantify the size of these rents, and how taxpayer costs are distributed across banks and entrepreneurs. From (8), and abstracting from the mean-zero noise in the evaluation process, we decompose the taxpayer cost as $TP = (CF - TP) + R$, where R represents the rent accruing to banks that would make (8) hold with equality. The term in brackets corresponds to the transfer to entrepreneurs—the funds received minus the expected repayment.

We find that most of the taxpayers' costs reach the entrepreneurs. On aggregate, bank rents account for only 5% of the total budget. Per unit of loan, rents are positively correlated with the crisis shock ε_k and negatively correlated with pre-crisis leverage. Firms that entered the crisis with lower debt or benefited from favorable shocks were safer, allowing banks to capture a larger margin, since they would have got a loan anyway. Overall, this evidence suggests that the program generated modest bank rents relative to its total cost, indicating that the GGL's fiscal burden primarily benefited entrepreneurs rather than financial intermediaries.

6.5. Moral hazard and excess risk

A major concern of any subsidized lending program is moral hazard. Access to cheap credit might incentivize firms to expand their exposure to risk by borrowing and increasing risk exposure. A strand of the literature warns about the consequences of moral hazard in crisis lending programs, on firm's performance and credit allocation (Acharya, Borchert, Jager, and Steffen, 2021; Crouzet and Tourre, 2021; Hoshi, Kawaguchi, and Ueda, 2023; Segura and Villacorta, 2023; Li and Li, 2025; Martin, Mayordomo, and Vanasco, 2025). This concern also applies to our setting, given that firms with GGL have higher long-run default rates than the average non-GGL firm.

However, an alternative perspective is that differences in observed default path across firms might be due to selection, rather than moral hazard. In Section 3 we show that long-run default rates differences *among applicants* are driven by selection rather that the effect of the GGL itself. Our structural model reproduces this reduced-form finding, so we can use it to quantify the excess risk-taking induced by the GGL across the firm distribution.

We define excess risk-taking as the extra percentage points of default probability in the factual case compared to a no-GGL scenario. We use two default metrics, both evaluated from $t = 0$, default in $t = 1$ and any default in $t = 1, 2$. We refect to them as short and long-run default probability.

In the short-run, we find that 81% of the firms do not take extra risk. This reflects the temporary effect that the loan has on default rates, consistent with the results of Section 3. Large excess risk-taking is concentrated: the share of firms that are increase their default probability by more than 1pp is less than 10%. On the contrary, in the long-run, when the loan has to be repaid, the policy induces more risk-taking. Only 56% of the firms do not take extra risk. Among the firms that take extra risk, the magnitudes are larger by one order of magnitude than in the short-run. Risk-averse behavior is stronger when firms enter the crisis in a less levered position, with higher assets and with a better crisis shock draw. This confirms that the moral hazard component is quantitatively relevant along the distribution of firms.

Budget-neutral counterfactual interventions Our previous finding suggests that the GGL had sizable welfare gains and was cost-effective. While it did not induced excessive rent-seeking by banks, it did induce excess risk-taking on firms that were in a better position during the crisis. These last two facts suggest that there might be scope for improvement.

In this section, we study a set of counterfactual designs of GGL that are budget-neutral to assess whether we can improve the cost-effectiveness of the policy. By design, the GGL hardly can attain an optimal allocation of liquidity, since it only sets a guaranteed share, a fixed price, and a maximum amount of debt, where optimal liquidity needs are firm-specific. The spirit of these counterfactuals is to suggest what a finetuned version of the GGL would look like, and assess whether the factual design was too generous or too stingy.

We jointly increase the guarantee share and decrease the fixed loan rate. By increasing the guarantee share, we expand the set of firms that banks would be willing to lend to. To rebalance the budget, we reduce the fixed GGL rate, so banks would earn a lower return on all loans, and reduce the overall approval of GGLs.

Figure 8 shows the welfare gains of the counterfactual designs relative to baseline, based on the money-metric welfare gain measure. Potential welfare increases are of the order of 0-4% relative to baseline. However, these gains quickly drop for guarantees lower that 65% and rates higher that 4%. Gains are the largest for an average guarantee share of 85% and rate near 3.1%, but after that, gains flatten out. Taken together, these results suggest that the actual design of the GGL was close to the most cost-effective design.

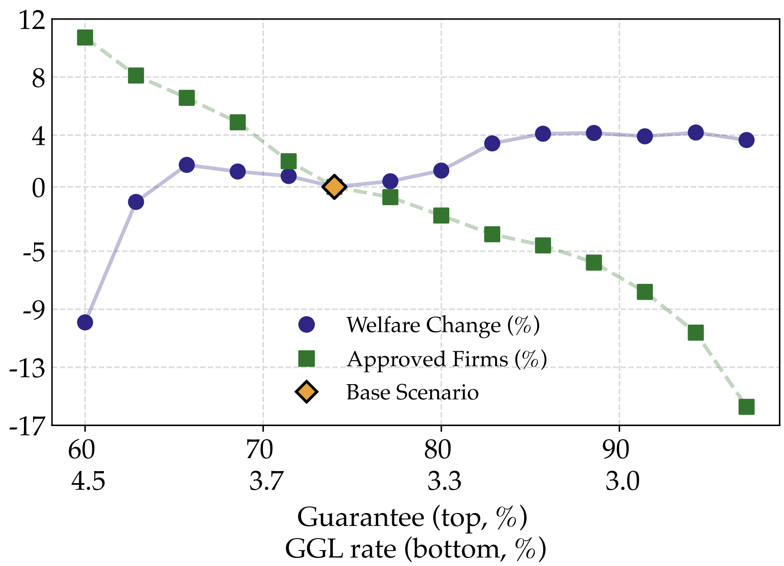


Figure 8: Budget-neutral counterfactuals The figure shows budget-neutral counterfactuals of the GGL intervention. On each scenario, we jointly increase the guarantee share and reduce the fixed loan rate, which we report in the x-axis. The left, blue, y-axis measures the money-metric welfare gain of the counterfactual relative to baseline. The right, green, y-axis measures the number of approved firms relative to baseline. The black diamond represents the baseline scenario.

To understand why the gains can increase with a higher guaranteed share and a lower fixed rate, we study how the approval distribution changes as we modify the guarantee share and the fixed rate. First, the set of approved firms is reduced. For a guarantee share of 85% and rate close to 3.1%, the set of approved firms is reduced 4% compared to baseline. The gains on welfare are driven by focusing on firms that are willing to pay a higher rate to access the GGL.

7. Conclusions

This paper presents new evidence on the short- and long-run impact of government-guaranteed loans (GGLs) as a crisis policy tool, using Chile's 2020 GGL program as a case study. Our results indicate that time horizon matters to assess the impact of government support. We further provide empirical support for the role of bank in the allocation of crisis credit: we find that they actively screened borrowers using private information, so by providing the right incentives, the policymaker can leverage banks' more accurate assessment of borrower's types.

To quantify the welfare effects of the program, we provide a framework that accounts for heterogeneity in firms' solvency and liquidity conditions, a rich set of realistic design features of the program and potential negative consequences of the policy, such as excessive risk-taking and bank rents. By disciplining the model with our causal estimates and the observed characteristics of the program, we recover which firms receive the support in terms of long-run TFP and crisis shock, which generate heterogeneity in solvency and liquidity, respectively. Moreover, we find that the design keeps in check excessive risk-taking and bank rents.

Our framework provides a quantitative tool to study the design of crisis-lending programs and assess their welfare implications, incorporating forces that are central to the policy design process, such as selection, bank rent-seeking, and moral hazard. Future research could extend this framework to benchmark the efficiency of GGL as a vehicle to provide liquidity relative to other policy tools, such as grants or tax credits.

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A. Additional Tables and Figures

Table A1: Descriptive statistics The table report key statistics of the cross-section of firms in our main panel. It covers 108,080 formal, active firms that were eligible for the GGL program, across 100 industries and 341 counties. The panel *Applications* highlights key statistics related to GGL application and assignment. Variables flagged as *only approved* are only observed for firms that were approved for the GGL. The panel *Pre-crisis characteristics* describes the cross-section of firms before the crisis. All continuous variables are winzorized to 1 and 99 percentile

	Mean	Std. Dev.	10th Pct.	Median	90th Pct.
Applications					
Applied to GGL (indicator)	0.58	0.49	0.00	1.00	1.00
Approved for GGL (indicator)	0.50	0.50	0.00	1.00	1.00
GGL size/sales (%) *only approved*	14.33	10.03	5.00	12.18	24.88
Guarantee share (%) *only approved*	74.14	18.98	42.50	85.00	85.00
Pre-crisis characteristics (2019)					
Sales (Th. USD)	818.14	1981.39	30.75	195.90	1797.04
Employees (units)	26.97	66.13	1.00	7.00	58.00
Wage bill (Th. USD)	131.19	319.90	3.57	29.47	293.12
Assets (Th. USD)	1231.90	3834.43	16.45	139.78	2402.70
Debt to assets (%)	34.43	56.34	1.16	15.75	80.10
Returns on 2019 assets (%)	35.62	83.31	-23.40	22.77	112.29
Number of banks (units)	1.60	0.97	1.00	1.00	3.00
Bank average assets (M USD)	0.04	0.01	0.03	0.04	0.05
Age (years)	11.47	7.35	3.00	10.00	23.00
Past default events (units)	0.01	0.09	0.00	0.00	0.00
Past delay events (units)	0.04	0.23	0.00	0.00	0.00
Share of debt backed by collateral (%)	15.10	25.67	0.00	0.00	55.98
Share of debt provisioned by the bank (%)	2.02	3.39	0.22	1.16	2.88
Model-implied probability of default (%)	2.14	1.52	0.56	1.79	4.18

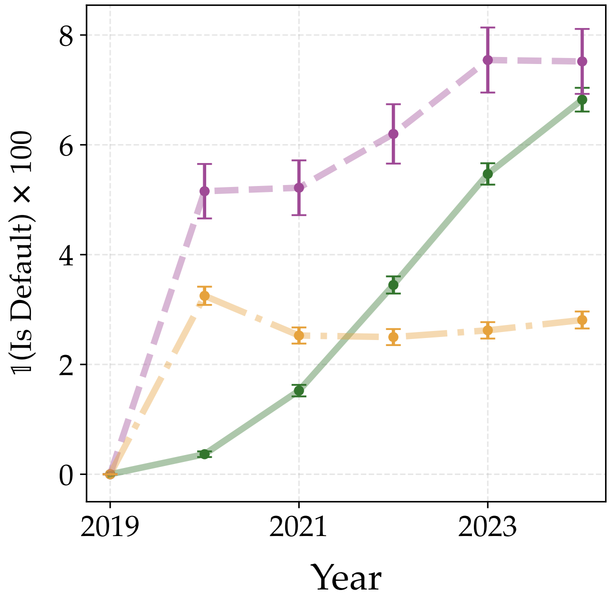
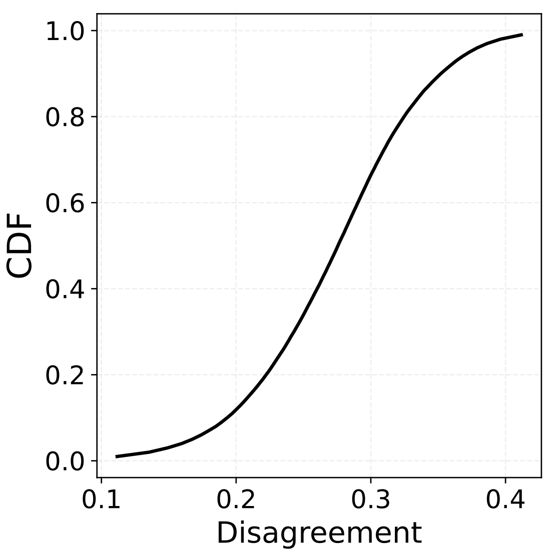
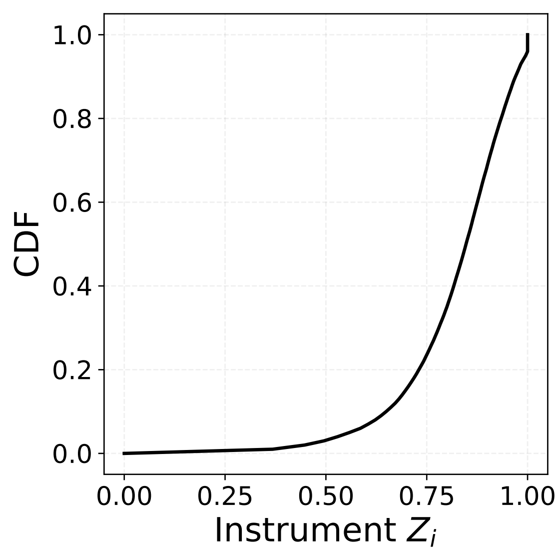


Figure A1: Non-cummulative default rates by group The figure shows the non-cummulative default rates by group. The y-axis measures the share of firms that have experienced a default in a given year. In each period, we drop already defaulted firms. Vertical bars represent 95% confidence intervals.



(a) Disagreement across banks



(b) CDF of the instrument

Figure A2: Instrument The left panel shows the CDF of the standard deviation of the systematic component of the GGL approval policy, $\widehat{\Gamma}_b X_i$, defined in (2) within firm across banks. It represents how much banks disagree in their approval policy for a firm with given characteristics. The right panel shows the CDF of the instrument Z_i defined in (3).

Table A2: First stage results We report the first stage results of the instrument (3) on the approval decision of the bank, conditinal on application. We report the F statistic of excluded instruments. Controls 1́9 include all variables X_i defined in Section 3.1. Robust standard errors in parenthesis. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$

	$\mathbb{1}$ [Approved]
Z	0.572***
	(0.020)
F (1st stage)	826
Adj R2	0.073
Obs.	61387
Controls '19	✓
County & Industry & Bank FE	✓

Table A3: Robustness The table shows the estimates of β_t for the specification of (1), on alternative subsets of firms. The first two panels reproduce the results of Table 1 for easy comparison. The third panel runs the same specification, but only on firms that sell every period, to address concerns about survival bias. The fourth panel runs the same specification, but only on firms that did not receive any other bank loan in the year of the crisis, to control for the joint effect of the GGL and other bank loans. Controls '19 include all variables X_i defined in Section 3.1. Standard errors in parenthesis. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$

	$\mathbb{1}$[Any Default since 2019]				
	2020	2021	2022	2023	2024
Baseline	−2.487***	−5.257***	−1.236	−1.491	1.4
	(0.891)	(1.514)	(2.012)	(2.571)	(2.944)
F-stat	826	826	826	826	826
Obs.	61387	61387	61387	61387	61387
Dropping defaults	−2.487***	−2.902**	3.913***	−0.353	3.058*
	(0.891)	(1.273)	(1.455)	(1.832)	(1.779)
F-stat	826	819	822	803	795
Obs.	61387	60867	59871	58139	55932
Always Sell	−2.302***	−5.69***	−4.231**	−3.109	0.817
	(0.839)	(1.357)	(1.801)	(2.467)	(3.011)
F-stat	688	688	688	688	688
Obs.	53001	53001	53001	53001	53001
No market loan	−1.07	−3.19**	−0.328	1.968	5.085
	(1.074)	(1.548)	(2.156)	(2.784)	(3.260)
F-stat	511	511	511	511	511
Obs.	38883	38883	38883	38883	38883

Table A4: Other outcomes of the firms The table shows the estimates of β_t for a similar specification to (1), but on alternative outcomes of the firms. The top panel uses as outcome a dummy $\mathbb{1}$[any exit since 2020], that takes value one if the firm has no sales during a whole year. The second panel uses as outcome the log of sales, on a subset of firms that have sales throughout the period. The third panel uses as outcome the log of employment, on the same last subset of firms. All regresions are estimated separately for each year, only on firms that applied to the GGL program. Controls '19 include all variables X_i defined in Section 3.1. Standard errors in parenthesis. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$

	2020	2021	2022	2023	2024
	$\mathbb{1}$[any exit since 2020]				
Approved (OLS)	-	−2.872***	−4.63***	−5.884***	−6.844***
	-	(0.272)	(0.365)	(0.435)	(0.487)
Approved (IV)	-	−2.251	−1.933	−0.372	−3.206
	-	(1.394)	(2.019)	(2.516)	(2.964)
$\mathbb{E}$[Outcome | Approved]	-	2.244	5.042	8.631	12.652
Obs.	61387	61387	61387	61387	61387
	Ln(Sales)				
Approved (OLS)	0.06***	0.065***	0.07***	0.084***	0.109***
	(0.009)	(0.011)	(0.012)	(0.014)	(0.018)
Approved (IV)	−0.059	−0.089	−0.156*	−0.185*	−0.232*
	(0.059)	(0.073)	(0.082)	(0.097)	(0.120)
Obs.	53001	53001	53001	53001	53001
	Ln(Employment)				
Approved (OLS)	0.06***	0.065***	0.07***	0.084***	0.109***
	(0.009)	(0.011)	(0.012)	(0.014)	(0.018)
Approved (IV)	−0.059	−0.089	−0.156*	−0.185*	−0.232*
	(0.059)	(0.073)	(0.082)	(0.097)	(0.120)
Obs.	53001	53001	53001	53001	53001
Controls '19	✓	✓	✓	✓	✓
County & Industry FE	✓	✓	✓	✓	✓

Table A5: Dropping variables The table reports the β_t, defined in (1) by OLS and IV, across different subsets of the pre-crisis control variables X_i. Column "Baseline" shows the results of Table 1. Each column in "Control dropped" shows the results of dropping the corresponding variable from the controls X_i, measured at 2019. "Sales", "Debt", "Age" are measured in logs. "Collateral" represents the share of debt collateralized, "Provision" represents the share of debt that bank reserves to cover losses, "Pr. Default" represents the one-year-ahead default probability, based on the model defined in Appendix C. Since the controls contain both linear and squared terms, we drop both at the same time. Controls '19 include all variables X_i defined in Section 3.1, except the variables that are dropped. Robust standard errors in parenthesis. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$

Sample	Baseline	Control dropped					
		Sales	Debt	Age	Collateral	Provision	Pr. Default
Outcome	$\mathbb{1}$[Any Default since 2019]						
$\mathbb{1}$[Approved] (OLS)	−4.036***	−4.035***	−4.042***	−4.031***	−4.058***	−4.302***	−4.063***
	(0.236)	(0.236)	(0.236)	(0.236)	(0.237)	(0.244)	(0.237)
$\mathbb{1}$[Approved] (IV)	−2.487***	−2.471***	−2.551***	−2.482***	−3.113***	−8.350***	−3.250***
	(0.891)	(0.890)	(0.871)	(0.858)	(0.855)	(0.876)	(0.850)
Obs.	61387	61387	61387	61387	61387	61387	61387
Controls '19	✓	✓	✓	✓	✓	✓	✓
FE	✓	✓	✓	✓	✓	✓	✓

B. Data Sources

In this section, we provide details on the data sources used in the paper, the construction of the main panel, and the auxiliary datasets used to build the instrument of Section 3.1 and the calibration of the model in Section 4.

B.1. Data sources

Tax records The Internal Revenue Service (*Servicio de Impuestos Internos*, SII) provides the Central Bank of Chile with raw tax records for all firms in Chile, which are then processed by the Central Bank of Chile to generate consistent statistics. We use a combination of both.

We extract annual sales, expenditure in materials, and total sales from January to April of 2020 at firm level from database *SIIFDJ*. We compute firm's age as the number of years since the first year of positive sales, starting from 1997. We use raw annual declaration records from database *F22 DETALLE* to extract assets value per year. We also use raw VAT declaration records from database *F29 DETALLE* to recover the firm's location based on the most popular county across all sales or purchase records of the firm. Firms' industry classification at various levels of aggregation is extracted from database *SIIRUT*, one of Central Bank of Chile's harmonized data sets, which contains identification and industry classification records for all formal firms since 1997.

Social security records The Social Security Administration (*Servicio de Previsión Social*, SPSDAT) provides the Central Bank of Chile with social security records for all firms in Chile. From database *SPSDAT*, we extract annual wage bill and employment of each firm.

Credit registry The Financial Market Commission (*Comisión para el Mercado Financiero*, CMF), regulatory agency of the Chilean financial market, provides the Central Bank of Chile with several datasets on bank debt.

From C11 statement (*C11*), we extract debt, default (defined as overdue payment for 90 days or more), delay (defined as overdue payment in at least 30 days), share of debt backed by collateral, share of debt provisioned to cover losses, and bank relationships. From D58 statement (*D58*), we extract information on whether a loan application was approved or rejected, the size of the loan, its government backed guarantee rate, the maturity term, and the grace period. From E20 statement (*E20*), we extract applications for government guaranteed loans. From D32 statement (*D32*), we extract market loans with information on rate, size, and maturity term.

Other datasets We extract assets value at bank level from database *MB1*, which is part of the datasets provided to the Central Bank of Chile by the CMF.

Data disclaimer This study was developed within the scope of the research agenda conducted by the Central Bank of Chile (CBC) in economic and financial affairs of its competence. The CBC has access to anonymized information from various public and private entities, by virtue of collaboration agreements signed with these institutions.

To secure the privacy of workers and firms, the CBC mandates that the development, extraction, and publication of the results should not allow the identification, directly or indirectly, of natural or legal persons. Officials of the Central Bank of Chile processed the disaggregated data. All the analysis was implemented by the authors and did not involve nor compromise the these institutions.

The information contained in the databases of the Chilean IRS are of a tax nature originating in self-declarations of taxpayers presented to the Service; therefore, the veracity of the data is not the responsibility of the Service.

B.2. Details of main panel construction

The construction of the main panel introduced in Section 2.1 involves linking the datasets in Appendix B.1. Matching variables at firm level from different datasets results in several missing matches, that we drop from our main sample. We describe here the breakdown of the observations lost in the sample construction.

We start with the set of firms with positive sales in 2019, 925,072 unique IDs (namely *RUT* codes). Merging with social security records results in 367,577 unique IDs, narrowing down the sample to formal firms. Adding credit registry data reduces the sample to 197,123 firms. Thus, our sample is representative of formal firms with pre-existing bank relationships. This allow us to track default events of non-approved firms on their non-GGL debt, as well as avoid comparing firms with debt records with firms that had no previous bank relationship, presumably riskier. We add assets from the F22 statement, which results in 133,003 unique IDs. We further drop sectors with a large share of non-profit organizations (education, health and public administration sectors), and financial sectors, since their access to credit is hardly limited to bank loans. This step results in 122,807 firms. Inconsistencies in the GGL data such as firms receiving a loan but not in the application records are dropped, reducing the sample to 120,330 unique IDs. Last, we apply the eligibility criteria for GGL. We drop all firms with sales above $28M in 2019, reducing the sample to 119,054 unique IDs. We further drop all firms that were already closed by the time of the release of the program (no sales from January 2020 to April 2020), decreasing the sample to 112,459 firms. Last, we drop all firms that were not in default in

2019, nor between January 2020 and April 2020, achieving a final sample of 108,080 firms.

C. Model-driven Default Probability

Our main panel has two measure that capture bank's perception of firm risk before the crisis impact: the share of outstanding debt backed by collateral, and the share of outstanding debt provisioned by banks to cover potential losses, both observed at the end of 2019. To complement this with a measure of risk based on realized defaults events, we build a prediction model of one-year ahead default probability.

We estimate the following logit model for the one-year ahead default probability π_{it+1}:

$$\ln \frac{\pi_{it+1}}{1 - \pi_{it+1}} = B' X_{it} + \epsilon_{it+1}$$

where X_{it} is a vector of firm characteristics and B is a vector of coefficients. We follow Huneeus, Kaboski, Larrain, Schmukler, and Vera (2025), who calibrate a default prediction model in the same setting, to choose the set of controls. We use (log of) sales, employment, wage bill, and debt and age as well as industry and county fixed effects. For flexibility, we add squared terms for all continuous controls. We estimate this model during a pre-crisis period of 2012-2019. We report the estimated coefficients in Table A6. Last, for all firms in our main panel, we predict the one-year ahead default probability using the fitted model, which we use as a control (denoted model-driven default probability) in the main analysis.

Table A6: Model-driven default probability The table reports the estimated coefficients of the model used to predict the one-year ahead default probability, fitted during a pre-crisis period of 2012-2019. We use the predictions of this model to construct a third independent metric of firm's pre-crisis risk, additional to the share of collateral and provision, that we use as a control in the main analysis of Section 3.1.

	$\mathbb{1}$[Will default]
Ln(Sales)	0.608***
	(0.061)
Ln(Sales)^2	−0.039***
	(0.003)
Ln(Debt)	0.218***
	(0.024)
Ln(Debt)^2	0.002*
	(0.001)
Ln(Wage Bill)	0.097**
	(0.038)
Ln(Wage Bill)^2	−0.025***
	(0.002)
Ln(Employment)	0.326***
	(0.016)
Ln(Employment)^2	0.024***
	(0.003)
Ln(Age)	1.093***
	(0.036)
Ln(Age)^2	−0.354***
	(0.010)
Obs.	1085838

D. Model Details

In this section, we present the Bellman equations that describe the entrepreneur's problem. We denote the state at the beginning of period t by $s_t = (z_t, b_t, k_t, A)$, where z_t is TFP, b_t is debt, k_t is capital, and A is long-run productivity.

Bellman equations for $t \geq 3$ The entrepreneur has no more GGL payments to make after $t = 2$, so for $t \geq 3$ the model is stationary. We drop time subscripts and denote next-period variables with a prime.

The value of a non-exiting entrepreneur before choosing to default or not is:

$$V(s) = \max_{d \in \{0,1\}} \left\{ (1-d)V^r(s) + dV^d(s) \right\} \tag{11}$$

where d is a default indicator. The default policy is denoted by $\tilde{\mathbf{d}}(s)$.

If the entrepreneur chooses to repay debt, the value is:

$$\begin{aligned} V^r(s) = & \max_{b',k'} u(c) + \beta \mathbb{E}_{z'} \left[\psi V^e(s') + (1-\psi)V(s') \right] \\ \text{s.t.} \quad & c + k' + b = y(s) + (1-\delta)k + q(z, b', k', A)b' \\ & k' \geq 0 \\ & b' \in [\underline{b}, \overline{b}] \end{aligned} \tag{12}$$

where c is consumption, b' and k' are the borrowing and investment decisions, and $q(z, b', k', A)$ is the price schedule given by (6). The continuation value depends on the exit shock probability ψ, and the value in the event of exit $V^e(s')$ (defined below).

The value of a non-exiting entrepreneur when she chooses to default is:

$$V^d(s) = \max_{b',k'} u(c) + \beta\mathbb{E}\left[\psi\left(\chi V^e(s') + (1-\chi)V^{ed}(s')\right) + (1-\psi)\left(\chi V(s') + (1-\chi)V^d(s')\right)\right] \quad (13)$$

$$\text{s.t.} \quad c + k' + H(b) = \Lambda(y(s)) + (1-\delta)k + (1+r)^{-1}b'$$
$$k' \geq 0$$
$$b' \in [\underline{b}, 0]$$

At default, three things happen: first, the entrepreneur reduces her debt to $H(b')$; second, her output is reduced by $\Lambda(y(s))$; third, she remains excluded from the debt market until a recovery shock with probability χ occurs. This is captured by the constraint $b' \in [\underline{b}, 0]$. The continuation values capture the probability of recovery from default χ and the probability of exit ψ, which yields a last period of utility captured by $V^e(s')$ and $V^{ed}(s')$.

Conditional on receiving an exit shock, the entrepreneur receives a last period of utility. She decides whether to default, trading off the debt haircut $H(b')$ and the output reduction $\Lambda(y(s))$, since there is no continuation value after an exit shock. The value of the entrepreneur in the event of exit is given by:

$$V^e(s) = \max_{d\in\{0,1\}} \left\{(1-d)V^{er}(s) + dV^{ed}(s)\right\} \quad (14)$$

where $V^{er}(s) = u(y(s) + (1-\delta)k - b)$ and $V^{ed}(s) = u(\Lambda(y(s)) + (1-\delta)k - H(b))$ are the values in the event of exit when not defaulting and defaulting, respectively. The default policy is denoted by $\tilde{\mathbf{d}}(s)$.

Crisis and GGL intervention Periods $t \in \{0, 1, 2\}$ feature the crisis and the GGL intervention, which alter the problem described above and break stationarity. At $t = 0$, there is an unanticipated crisis shock $\varepsilon_k \sim F(\varepsilon_k)$ that affects capital quality, so the entrepreneur's effective capital in period $t = 0$ is $\varepsilon_k k_0$. This shock is drawn at the same time as the TFP, exit, and recovery shocks. After observing the crisis shock, a non-exiting, non-defaulting entrepreneur is eligible to apply for a GGL loan of size $q^G b^G$ at $t = 0$. For ease of exposition, we describe the payoffs backwards from $t = 2$ to $t = 0$.

At $t = 2$, the non-exiting entrepreneur faces a default decision and must repay the GGL debt if she does not default. This introduces a new state variable, the GGL debt b^G. If the entrepreneur chooses to repay, the value is:

$$
\begin{aligned}
V_2^r(s_2, b^G) &= \max_{b_3, k_3} u(c_2) + \beta \mathbb{E}_{z_3} \left[\psi V^e(s_3) + (1-\psi) V(s_3) \right] \qquad (15) \\
\text{s.t.} \quad c_2 + k_3 + b_2 + b^G &= y(s_2) + (1-\delta) k_2 + q(z_2, b_3, k_3, A) b_3 \\
k_3 &\geq 0 \\
b_3 &\in [\underline{b}_3, \overline{b}_3]
\end{aligned}
$$

The default value function $V_2^d(s_2, b^G)$ follows analogously, with the entrepreneur defaulting on both the regular debt b_2 and the GGL debt b^G. Note that the continuation values at $t \geq 3$ are the ones described in (12) and (13), so we drop the time index for simplicity. An analogous problem is solved in the event of an exit shock.

At $t = 1$, the non-exiting entrepreneur chooses whether to default. In this case, no GGL payment is due, so the problem is:

$$
\begin{aligned}
V_1^r(s_1, b^G) &= \max_{b_2, k_2} u(c_1) + \beta \mathbb{E}_{z_2} \left[\psi V^e(s_2) + (1-\psi) V_2(s_2, b^G) \right] \qquad (16) \\
\text{s.t.} \quad c_1 + k_2 + b_1 &= y(s_1) + (1-\delta) k_1 + q(z_1, b_2, k_2, A) b_2 \\
k_2 &\geq 0 \\
b_2 &\in [\underline{b}_2, \overline{b}_2]
\end{aligned}
$$

with an analogous problem in the event of an exit shock.

At $t = 0$, the crisis hits and the application process occurs. After GGLs are allocated, the value of a non-exiting, non-defaulting entrepreneur is given by:

$$
\begin{aligned}
V_0^r(s_0, b^G) &= \max_{b_1, k_1} u(c_0) + \beta \mathbb{E}_{z_1} \left[\psi V^e(s_1) + (1-\psi) V_1(s_1, b^G) \right] \qquad (17) \\
\text{s.t.} \quad c_0 + k_1 + b_0 &= y(s_0) + (1-\delta) \varepsilon_k k_0 + q(z_0, b_1, k_1, A) b_1 + q^G b^G \\
k_1 &\geq 0 \\
b_1 &\in [\underline{b}_1, \overline{b}_1]
\end{aligned}
$$

where q^G is the price of the GGL loan set by the policymaker, and $q^G b^G$ are the fresh funds received from the GGL. This is the value we use to evaluate the welfare gains of the policy. We prefer this metric because it is free from any non-pecuniary cost or noise in the approval process.

Before allocating a GGL, the bank needs to evaluate the expected payoff of the entrepreneur. They approve it with some probability according to (9). Before that, the entrepreneur decides how much to apply for—or whether to apply at all. If she applies, she incurs a non-pecuniary application cost $\kappa^{\text{firm}} \sim F(\kappa^{\text{firm}})$.

The value of an application is:

$$V_0^{app}(s_0) = \max_{b^G} \mathbb{P}(\text{approve} \mid s_0, b^G) \cdot V_0^r(s_0, b^G) + \Big(1 - \mathbb{P}(\text{approve} \mid s_0, b^G)\Big) \cdot V_0^r(s_0, 0) - \kappa^{\text{firm}} \tag{18}$$

$$\text{s.t.} \quad b^G \in [0, \phi\, \varepsilon_k k_0]$$

where $\mathbb{P}(\text{approve} \mid s_0, b^G)$ is the approval probability given the state s_0 and loan size b^G, as defined in (9). To decide whether to apply, the entrepreneur compares the value of applying, $V_0^{app}(s_0)$, to the value of not applying, $V_0^r(s_0, 0)$.

Only non-defaulting, non-exiting entrepreneurs are eligible to apply for a GGL loan. Thus, at $t = 0$, the value for an entrepreneur who neither exits nor defaults is:

$$V_0^r(s_0) = \max\{V_0^r(s_0, 0), V_0^{app}(s_0)\} \tag{19}$$

One step earlier, a non-exiting entrepreneur decides whether to default after observing the crisis shock, knowing she can apply for a GGL loan only if she does not default. If an exit shock occurs, the firm cannot apply and instead decides whether to default based solely on current-period utility.

Equilibrium. A Markov perfect equilibrium consists of a collection of policy functions for consumption, borrowing, investment, and default decisions, conditional on default status and exit draw, denoted by $\{c^d, c^r, c^{er}, c^{ed}, b^{d\prime}, b^{r\prime}, b^{er\prime}, b^{ed\prime}, k^{d\prime}, k^{r\prime}, k^{er\prime}, k^{ed\prime}, \tilde{d}, \hat{d}\}$, an application policy b'^G, an approval probability $\mathbb{P}(\text{approve})$, a price schedule $q(z, b', k', A)$, and value functions $\{V^r, V^d, V^e, V^{er}, V^{ed}, V^{app}\}$, and initial conditions s_0, ε_k, such that the following hold:

1. For $t \geq 3$, (11)–(14) and (6) are satisfied.
2. For $t \in \{0, 1, 2\}$, (15)–(19), (6) and (9) are satisfied.

E. Numerical Method

We solve, simulate, and calibrate the model using a discrete approximation standard in default models (Arellano, 2008). We define a grid of normalized capital values of size n_k, where capital is normalized by A so that the same grid can be used across different productivity scales. We also set a grid of leverage values of size n_l, where leverage $l = b/k$ is the ratio of debt to capital. Using leverage as a state variable makes it independent of scale. The short-run persistent TFP shock z is discretized using Tauchen's method. We divide long-run productivity A into four bins, taking the mean of $\ln(\tilde{A}_i)$ as the representative value for each bin, as estimated in equation (10).

We first solve and calibrate the model in an environment with neither a crisis nor a GGL intervention, matching the leverage and default rates observed in the pre-crisis period. For the no-crisis case, we apply value function iteration on the discrete grid. Algorithm 1 summarizes the procedure.

Algorithm 1 Value Function Iteration on a Discrete Grid. This algorithm describes the procedure used to solve the model in the absence of a crisis or GGL intervention. Its returns the continuation value functions and price schedule needed to solve the three remaining three periods of crisis and GGL intervention by backward induction in a posterior step.

Initialize: Create grids for k, l, z, and A.
Set initial guesses for $V(z, l, b, A)$ and $V^d(z, l, b, A)$, and for the price schedule $q(z, l', k', A)$.
repeat
    for each state (z, l, b, A) on the grid **do**
        Compute $V^d(z, l, b, A)$ using (13).
        Compute $V^r(z, l, b, A)$ using (12).
        Update the default policy when no exit shock occurs $\tilde{\mathbf{d}}(z, l, b, A)$ and $V(z, l, b, A)$ using (11).
        Update $q(z, l', k', A)$ using (6). Note that the default policy upon an exit shock can be pre-computed.
    end for
    Update $V(z, l, b, A)$, $V^d(z, l, b, A)$, and $q(z, l', k', A)$.
until values and prices converge.

For calibration, we simulate 10,000 firms over 100 periods and retain the last 25 periods to minimize the influence of arbitrary initial conditions. We compute average leverage and default rates over this subsample and search for parameter values that minimize the distance between model-generated moments and the corresponding 2019 pre-crisis targets.

To solve and calibrate the crisis and GGL-intervention scenarios, we use backward induction from $t = 2$ to $t = 0$, taking as continuation values at $t = 2$ the value functions obtained in the no-crisis step. The policy choice b^G is also discretized over a grid.

We run this computationally intensive solution and calibration procedure on an NVIDIA A100 GPU cluster using Python's JAX library. We thank Stanford University and the Stanford Research Computing Center for providing computational resources and support that contributed to these results.