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

Lautaro Chittaro† Cristián Sánchez‡
Job Market Paper

Frequently updated, click here for the latest version.
This version: November 9, 2025

Abstract

We study the long-run effectiveness of government-guaranteed loan programs from recent crises. Using administrative and loan application data from the Central Bank of Chile, we track firm defaults five years after the COVID-19 shock. Our instrumental-variable estimates show that these loans postponed defaults for two years but did not reduce total defaults in the long run. Banks used private information to direct credit toward firms that would have been safer even without the program. To assess the welfare implications of the delays in defaults and banks' selection of safer firms, we build a dynamic model of heterogeneous entrepreneurs disciplined by our causal estimates. The program generated welfare gains exceeding its fiscal cost by 21%, with limited rents for banks and modest increases in aggregate risk-taking. Younger, high-growth firms are the most cost-effective group to support, yet they are the least likely to be approved since they are riskier. A budget-neutral redesign that raises guarantees for younger firms and reduces them for the rest could increase welfare by 6pp, suggesting that firm age is a practical basis for targeting future crisis lending policies.

*We are especially grateful to Martin Schneider, Monika Piazzesi, Shoshana Vasserman, and José Ignacio Cuesta for their guidance and support. We also thank Adrien Auclert, Sebastian Bauer, Rafael Berriel, Adrien Bilal, Luigi Bocola, Mauricio Calani, Ramiro de Elejalde, Sebastian Di Tella, Liran Einav, David Kohn, Arvind Krishnamurthy, Neale Mahoney, Stephen Redding, Marcelo Sena, Christopher Tonetti, Patricio Toro, and Ali Yurukoglu, and other seminar participants at Stanford University and the Central Bank of Chile for their helpful comments. We owe special thanks to Vania Martínez for her outstanding research assistance, and the Stanford Research Computing Center for providing computational resources and support. This project was supported by the Gale and Steve Kohlhagen Fellowship in Economics through a grant to the Stanford Institute for Economic Policy Research and by the Summer Internship Program of the Central Bank of Chile. The views expressed are those of the authors and do not necessarily represent the views of the Central Bank of Chile or its board members. All errors are our own.
†Stanford University, chittaro@stanford.edu (corresponding author)
‡Central Bank of Chile, csanchez@bcentral.cl

1. Introduction

In recent crises, many countries launched large-scale government-guaranteed loan (GGL) programs targeting small and medium-sized enterprises (SMEs). Under these programs, commercial banks extend credit on favorable terms, while governments provide guarantees that insure banks against firm default. Although the literature on crisis lending has emphasized the short-term positive effects of these programs on firms' outcomes, their long-term effectiveness remains unclear. Firms facing severe financial constraints may benefit, but subsidized loans could also flow to low-productivity firms or encourage excessive risk-taking.

This paper studies the long-run effectiveness of Chile's COVID-19 GGL program. By linking detailed administrative records from the Central Bank of Chile with GGL application data, we provide novel causal evidence on the long-run impact on firm default. Then, we assess the welfare implications of this policy using a structural model of heterogeneous entrepreneurial firms. Our framework incorporates realistic design features of modern GGL programs, including partial guarantees, loan caps, and fixed interest rates. The model allows us to uncover wide heterogeneity across the firm distribution in the assignment of GGLs, the welfare gains, and the effect of the policy on firms' risk-taking behavior.

Our main empirical finding is that GGLs postponed firm defaults for two years. We propose an IV strategy that leverages application-level data, heterogeneity in banks' approval policies, and pre-crisis bank–firm relationships to generate quasi-random variation in GGL assignment. Using this instrument, we estimate that the GGL program reduced firms' default probability during 2020–2021 by 5.3pp. However, this effect reversed in 2022, when approved firms experienced a surge in defaults: among firms that had not yet defaulted, the program raised the probability of default by 4pp. Overall, the cumulative effect on defaults since the onset of the crisis is not statistically significant beyond 2022. The reduced-form results further show that banks allocated GGLs to firms that would have exhibited lower long-run default risk even in the absence of the guarantee. This evidence supports delegating crisis credit allocation to commercial banks rather than assigning it directly through the government.

To assess the welfare implications of the postponed defaults induced by this policy, we develop a dynamic model of heterogeneous entrepreneurs. In the model, entrepreneurs with different long-run productivities make investment, borrowing, and default decisions in frictional credit markets. An unexpected crisis shock hits the economy, affecting firms to varying degrees. In the same period, entrepreneurs have a one-time opportunity to apply for a GGL loan that coexists with standard bank loans. Banks, in turn, decide whether to approve these applications. We quantify the model to match the short-run sorting patterns we observe in the application data and the reduced-form causal effects. The estimated model reveals that the policy was cost-effective, generating aggregate welfare gains that exceeded its fiscal cost by 21%. These gains are equivalent to a transfer of USD 1860 per approved firm. A novel insight of the model is that young, growing firms benefit the most from this policy, yet they are the least likely to be approved. By increasing the guarantee share for the youngest quartile and reducing it for the rest, welfare could rise by up to 6pp and the number of approved firms by 4pp, while keeping the budget neutral

Firms that received a GGL experience a sharp expansion of debt a year after the crisis, followed by a quick rise in default rates. Our administrative records show that 45% of approved firms at least double their pre-crisis debt a year after the crisis. The increase is driven by the GGL itself rather than additional commercial borrowing. During the crisis year, approved firms have near-zero default rates, but these rates soon increase, 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 as perceived by banks.

We estimate a negative but temporary causal effect of GGLs on firms' default probability using an IV strategy. Because the guarantees are only partial, banks retain some exposure to default risk. This design feature gives banks incentives to screen out firms that are more severely hit by the crisis and therefore more likely to default. A naïve OLS regression that ignores this endogenous screening would overstate the effect of the GGL on defaults. Using an instrument based on banks' approval leniency, we find a negative causal effect of 2.5–5.3pp on default probability during the first two years after the crisis. However, the effect on non-defaulted firms becomes positive in 2022, and the impact on *cumulative* defaults is no longer statistically significant since then. The results indicate that the GGL postponed defaults rather than permanently reducing them. This finding contrasts with much of the recent crisis-lending literature, which typically finds positive short-run effects on firms' outcomes within the year of the crisis.

Our IV strategy exploits the persistence of bank–firm relationships and heterogeneity in banks' approval policies to generate quasi-random variation in GGL assignment. The instrument adapts the approach of the judge-IV literature to a setting without explicit random assignment of evaluators (banks). Access to GGL application-level data allows us to estimate each bank's approval leniency—the systematic tendency to approve applicants with similar pre-crisis characteristics. Even among ex-ante similar firms, we find substantial dispersion in leniency across banks, intuitively, evidence of disagreement among them. We then combine this granular measure of leniency with pre-crisis bank–firm relationships, which generate heterogeneous exposure of each firm to banks' approval behavior. Because the pool of banks is relatively homogeneous and these relationships are highly persistent, we argue that the historical matching between firms and banks is unlikely to be correlated with how the crisis subsequently affected them.

Our empirical findings suggest a rationale for delegating credit allocation to banks rather than having the government assign it directly. Banks approved GGLs for firms that would have had a lower long-run default probabilities even in the absence of the program. Using a standard omitted-variable-bias argument, we compare the IV and OLS estimates to decompose how much of the observed difference in long-run default rates is explained by unobserved firm characteristics that jointly affect default risk and GGL approval. Since the causal effect on default is statistically insignificant in the long-run, 6 of the 8pp observed difference in default probability between rejected and approved firms is explained by unobservables. We argue that our information set closely mirrors that available to policymakers at the time of the crisis, as our controls include a rich set of pre-crisis variables drawn from major administrative sources—tax, social-security, and credit-registry data. Under this assumption, we interpret the omitted-variable bias as evidence that banks relied on private or relational information to screen out firms with higher default risk.

To assess the welfare implications of the GGL-induced delays in firm defaults, we develop a dynamic structural model of heterogeneous entrepreneurs operating in frictional credit markets. Entrepreneurs differ in their long-run productivity and make investment and borrowing decisions. They operate a decreasing-returns technology and face idiosyncratic, persistent productivity shocks. To finance their operations, they use defaultable short-term bank loans, which are priced by risk-neutral banks to break even on default risk. Exogenous exit shocks and replacement by new entrants generate growth dynamics in the firm distribution.

An unexpected crisis shock hits the economy, affecting firms to varying degrees and generating heterogeneity in their demand for liquidity. A one-time GGL loan is introduced, for which firms may apply, and banks decide whether to approve them. The modeled policy loan preserves key design features of the actual program: it is a long-term loan with a fixed interest rate set by the government, a cap on loan size, and a partial guarantee. The model generates endogenous sorting of firms into approved, rejected, and non-applicant groups. We discipline the model by matching short-run moments—the reduced-form causal estimates and the sorting patterns observed in the application data. The calibrated model closely reproduces the untargeted long-run default rates across all three groups of firms.

The estimated model shows that the GGL program is cost-effective, generating welfare gains that exceed its fiscal cost by 21%, or USD 1860 per approved firm. In consumption-equivalent terms, the policy corresponds to a one-period increase of 1.2% for the average approved firm—nearly 20% of Chile's annual private consumption growth rate before the pandemic. The crisis represents a large negative shock to a substantial share of risk-averse entrepreneurs, raising the value of additional liquidity. The guaranteed loan provides low-cost liquidity precisely when it is most needed, making it particularly valuable to firms. The model infers a negative mean for the crisis shock in order to match the counterfactual higher-than-usual short-run default rate that approved firms would have faced in the absence of the GGL, as implied by the IV estimates.

The model reveals substantial heterogeneity in both the assignment of GGLs, the welfare gains given approval and the behavior response to the policy. Firms with lower long-run productivity and more severe crisis shocks are more likely to apply, since the market interest rate they would face in the absence of the GGL is particularly high for these firms. While the overall approval rate is high across the firm distribution—by construction, as the model is disciplined to match the high approval rate in the data—firms with worse crisis draws are less likely to be approved when they also have higher long-run productivity. These firms have strong growth potential but limited current resources. As a result, they actively seek to invest through leverage, which increases their risk and makes them relatively less likely to be approved. Welfare gains are also unevenly distributed across firms: nearly 27% of approved firms do not offset the cost to taxpayers. The typical cost-effective firm enters the crisis in a weaker position, with fewer assets, higher leverage, and worse crisis draws.

Moral hazard can induce excessive risk-taking in the presence of subsidized credit, as a large body of literature has highlighted. While the GGL reduces aggregate short-run default rates, the model predicts a modest increase of 0.8pp in long-run default rates in the absence of the program. This mild aggregate effect masks substantial heterogeneity in risk-taking behavior across firms. Around 44% of approved firms take on more risk in the long run relative to a no-GGL scenario. These firms are in a relatively stronger position to withstand the crisis and experience the smallest welfare gains from the policy. This finding provides empirical support for modern design features that limit the GGL's potential to encourage excessive risk-taking, such as loan-size caps.

Since the policy design specifies both a fixed rate for GGLs and a guarantee, banks extract rents from the difference between the after-guarantee zero-profit rate and the policy-set rate. We find that, on aggregate, only 5% of the total taxpayer cost of the GGL is captured by banks. Rents vary across firms, increasing for relatively safer borrowers, since banks would be willing to approve a GGL even at a lower rate than the one set by the policymaker. Conversely, banks lose money on overly risky firms that apply in the hope of being misclassified as eligible. We estimate that only 0.6% of approved firms would have been rejected if banks' screening technology were perfect.

A broader lesson from the model is that younger firms with greater growth potential are the most cost-effective group to support. Firms in the youngest quartile achieve welfare gains that exceed the taxpayers' cost by 57pp more than the rest. However, they are also the least likely to be approved for a GGL, with an approval rate 41pp lower. Because younger firms have not yet reached their optimal scale, their marginal return to investment is high. At the same time, they have limited resources and therefore seek to borrow, exposing themselves to higher risk and higher market rates. The GGL is particularly valuable to them because it provides low-cost, long-term credit that allows them to grow before servicing the debt.

Given the larger effectiveness of the program for younger firms, we propose a counterfactual design that conditions the guarantee share on firm age. We argue that age targeting is easily observable and captures information about a firm's growth potential. This contrasts with size targeting, which is much more common in a wide range of policy interventions. We consider a budget-neutral counterfactual GGL that increases the guarantee share for the youngest quartile and decreases the fixed interest rate for the oldest quartile. Such a design could increase aggregate welfare by 6pp and the number of approved firms by 4pp relative to the original program.

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

[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).

(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, Crosignani, 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 a sudden and severe 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% year-over-year in the second quarter. This contraction was accompanied by a sharp drop in credit: non-guaranteed credit had fallen by 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 alleviate SMEs' liquidity shortages. By May 2020, GGLs accounted for 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 was built on an existing, small-scale GGL program, enabling a rapid rollout of the crisis lending scheme.[5]

The conditions of the GGL program were highly favorable to SMEs. The government set a fixed annual interest rate of 3.5% for all GGLs. This rate was 3pp above the risk-free rate at the time and 6pp below comparable market loans. Each loan had a maximum size of 25% of the firm's 2019 annual sales, and it was restricted to working-capital financing. Loans were long-term, with a maturity of 3.5 years on average.[6] This term was 40% longer than that of comparable market loans.[7] Repayment was scheduled in fixed installments, after a six-month grace period. In turn, the government offered a partial guarantee that compensated banks for

[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.

part of the losses in case of default. The *guarantee share* decreased with the firm's pre-crisis annual sales, ranging from 85% for the smallest bin of firm's size to 60% for the largest bin.[8]

Firms applied for GGL loans through commercial banks. Applications were submitted through banks' websites, where firms specified the requested amount, provided details about the firm and its owners, attached the required documentation and forms, and authorized access to their tax records. The bank then evaluated each application, verifying both program eligibility and the firm's creditworthiness, based on its internal criteria given the government's conditions on loans terms. After evaluation, the bank either approved or rejected the loan.

To be eligible for the GGL program, firms had to report annual pre-crisis sales below USD 28 million and have 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, or have pre-crisis default records during 2019.

The GGL program combined standardized and favorable loan terms with broad eligibility and a decentralized screening 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 for each application. This institutional design offers a natural setting to study how the allocation of credit is shaped by the incentives faced by both banks and firms under this rich set of policy parameters.

2.1. Data

Linked administrative records To track firm performance before and during the crisis, we match official firm tax records, social security data, and bank credit registry, all three available through the Chilean Central Bank's regulatory data access. We construct a firm-level panel at annual frequency covering 2019–2024. We focus on the universe of formal firms with established 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 by banks to cover expected losses, bank relationships), and additional characteristics (age, industry, county). Additional details on data sources and panel construction are provided in Appendix B.1.

[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 ease of 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, in line with Basel international standards.

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

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. While this definition excludes some records[12], it ensures that we observe 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 among non-applicants on non-GGL debt and to control for multiple pre-crisis risk indicators derived from bank credit records. Finally, firms were eligible if they met two criteria: annual sales below USD 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 on 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 the ratio of 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. These features reflect our sample is representative of established formal businesses with existing bank relationships, but does not capture informal firms or extremely early-stage startups. 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, which we detail in the respective sections.

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

The GGL program provided liquidity to firms during the crisis, but it also led to a sharp increase in debt. We use our main panel to document this surge in firm debt. Figure 1a shows the

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

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

distribution of debt growth from December 2019 to December 2020 across three groups of firms according to their GGL status: approved applicants, rejected applicants, and non-applicants. We also group debt growth into three bins: decrease, increase up to 100% and increase above 100%. Nearly 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 pre-crisis debt. This pattern is mechanically driven by the GGL loans rather than by additional commercial borrowing. Moreover, 48% of rejected firms increased their outstanding debt during the period, signaling that liquidity demand was only partially met by the GGL, and standard commercial loans filled the gap.

The rapid surge in debt might weaken firms' repayment capacity. Figure 1b shows the share of firms that experienced a default event between 2019 and each subsequent year. Since firms were required to be free of default at the time of application, all groups start with no defaults in 2019. Initially, approved firms are the best-performing group, with a default share 3pp (5pp) lower than non-applicants (rejected). Over time, the cumulative default share of approved firms steadily increases, catching up with non-applicants between 2022 and 2023. By 2024, the share of approved firms that had experienced a default was 4pp larger than non-applicants, but 7pp less than rejected firms. In Figure A1, we show the non-cumulative share of defaulting firms by group, excluding, in each year, firms that had already defaulted. The share of defaulting approved and rejected firms converges to a higher level than that of non-applicants, which in Figure 1b is reflected in the similar slope of the curves.

In the following section, we propose an instrumental-variable strategy to disentangle how much of the difference in observed default rates between approved and rejected firms arises the temporary liquidity that GGL provided, as opposed to unobserved differences between the two groups that determine both the GGL allocation and their default probability.

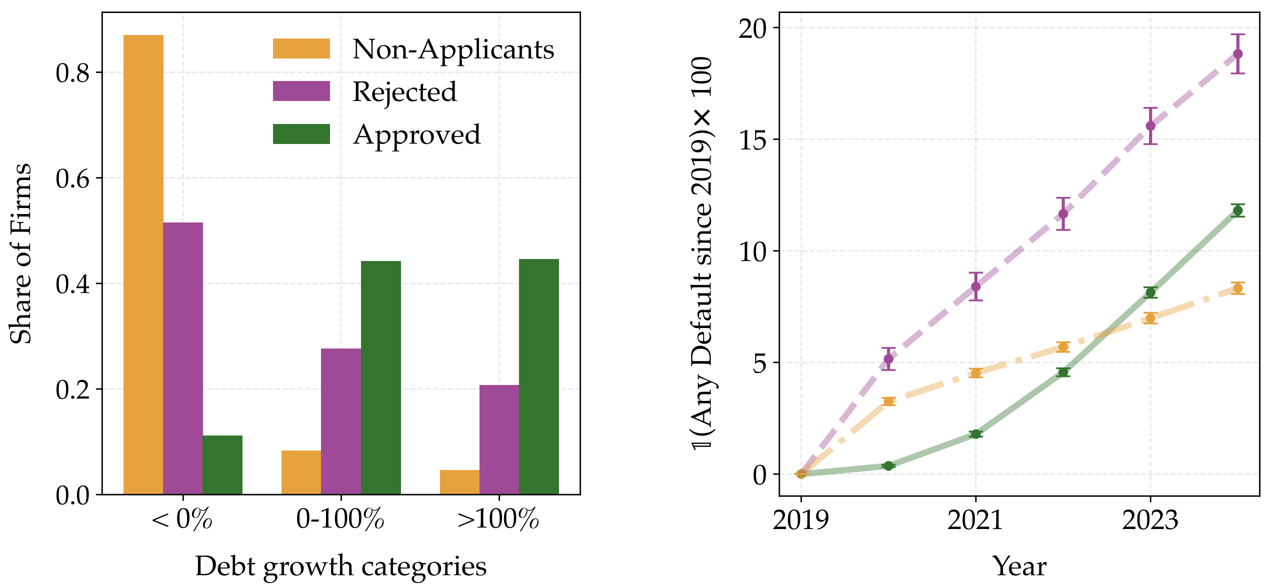
3. Reduced-Form Evidence of Selection and Effect of GGL

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 2.5–5.2pp 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

Figure 1: Evolution of debt and default. The left panel shows the distribution of debt growth from 2019 to 2020 for firms that did not apply, and firms that applied but were rejected, and firms that were approved. We cluster the debt growth into three categories: decrease, increase up to 100% and increase above 100%. The right panel represents the cumulative default rates by group, measured as the share of firms that have experienced any default since 2019. 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.

(a) Debt growth across groups

(b) Cumulative default rates by group



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.

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 2.5–5.2pp 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 a default episode since 2019, and instead, by 2022, we find a strong positive effect on the probability of default conditional on firms that have not defaulted yet. Our identification strategy adapts the judge IV 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 probabilities, even in the absence of the GGL, suggesting that banks used private information to allocate liquidity toward relatively safer firms.

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 2.5–5.2pp 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 policies toward firms with similar pre-crisis characteristics. Pre-crisis bank relationships thus generate firm-level exposure to heterogeneity 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:

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

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

[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.

[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.

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]

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 9pp The first-stage F-statistic is 826, 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

[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.

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

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 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 2.5pp in 2020. By 2021, the cumulative effect grew to 5pp 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 2.5pp and 2.9pp in 2020 and 2021. After 2022, however, the effect reverses: GGL approval increases the probability of default by 4pp 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.

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 4.8pp, which correspond to the larger default of rejected firms shown in Figure 1.

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

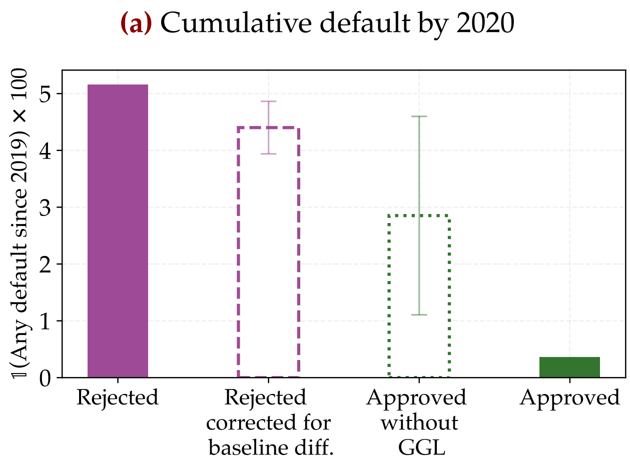
	2020	2021	2022	2023	2024
Panel A	$\mathbb{1}$ [Any default since 2019] × 100				
$\mathbb{1}$ [*Approved*] (OLS)	−4.036***	−5.501***	−5.602***	−5.619***	−5.072***
	(0.236)	(0.308)	(0.364)	(0.419)	(0.456)
$\mathbb{1}$ [*Approved*] (IV)	−2.487***	−5.257***	−1.236	−1.491	1.4
	(0.891)	(1.514)	(2.012)	(2.571)	(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	✓	✓	✓	✓	✓

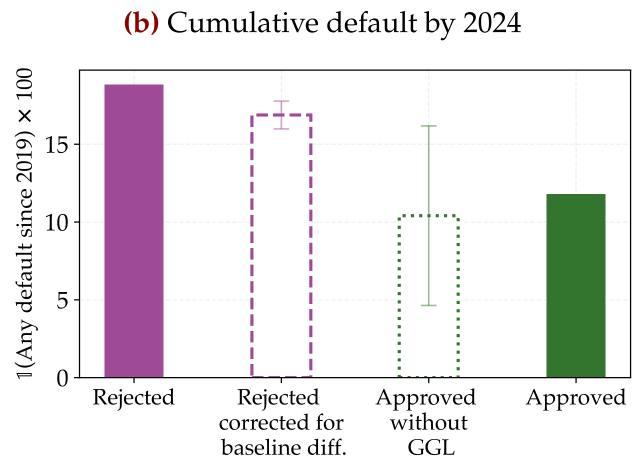
The second bar represents the counterfactual default probability of rejected firms if they shared the same pre-crisis characteristics as approved firms. We obtain this value by adding the OLS estimate to the observed default rate of approved firms. This implies that 0.8pp out of 4.8pp of the total difference 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 1.5pp 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 2.5pp of the 4.8pp 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

Figure 2: Decomposition of default probability. The left panel decomposes the difference in observed default probability between rejected (leftmost bar) and approved (rightmost bar) firms in 2020. The dashed bar shows the default probability of rejected firms if they had the same pre-crisis characteristics as approved firms. This is obtained by subtracting the OLS estimate from the approved firms' default probability (rightmost bar). The dotted bar shows the default probability of approved firms in the absence of the GGL. This is obtained by subtracting the IV estimate from the observed default probability of approved firms (rightmost bar). The right panel presents the same decomposition for the probability of experiencing any default since 2019, measured by 2024. Error bars indicate 95% confidence intervals.





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 (6.5pp) and pre-crisis characteristics (1.9pp).

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

[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.

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. 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 16pp 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.

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. Structural 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

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 parentheses. $^{***} = p < 0.01$, $^{**} = p < 0.05$, 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	✓	✓	✓	✓	✓

applications are rejected. This process generates endogenous sorting across non-applicants, approved, and rejected firms, which we discipline using application data and the reduced-form evidence in Table 1. The model allows us to (i) infer which firms obtain GGLs based on 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 capital in the crisis period ($t = 0$); its role is described

[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.

in detail when we introduce the crisis and GGL intervention. In all other periods, we normalize $\varepsilon_k = 1$. Entrepreneurs choose next-period capital k' to produce

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

units of the consumption good. Because we focus on SMEs, we assume $\alpha < 1$, which implies 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.

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

Default triggers three consequences for the entrepreneurs. First, they repay 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 they stay in default, their 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, they are excluded from credit markets until an i.i.d. recovery shock restores borrowing access with probability χ.

Alternatively, entrepreneurs can save $b' \leq 0$, and earn the risk-free rate r. Saving remains 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, they cease making investment and borrowing decisions but still enjoy one final period of utility and decide whether to default. This final decision trades off the output loss $\Lambda(y)$ from default against the debt haircut $H(b')$, since there is no continuation value after exit. Exiting entrepreneurs are replaced by new entrants who start with no debt and an arbitrarily low level of capital.

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

$$q(z, b', k', A)b' = \begin{cases} (1+r)^{-1}\mathbb{E}\left[\mathbf{d}b' + (1-\mathbf{d})H(b')\right], & \text{if } b' > 0 \\ (1+r)^{-1}b', & \text{if } b' \leq 0, \end{cases} \tag{6}$$

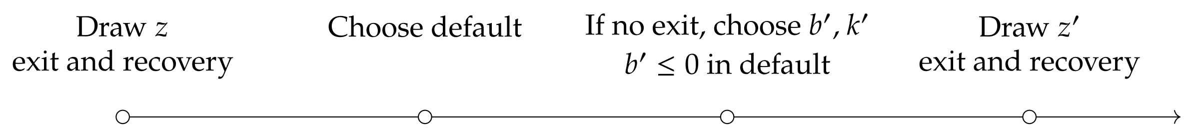
where $\mathbf{d}$ is the expected default policy, given by $\mathbf{d} = (1-\psi)\tilde{\mathbf{d}} + \psi\hat{\mathbf{d}}$, which averages the default policy $\hat{\mathbf{d}}$ when an exit shock arrives, and the default policy $\tilde{\mathbf{d}}$ without an exit shock.

Budget constraint We have now introduced all elements to define the budget constraint of the entrepreneur, which is given by:

$$c = \begin{cases} y(z,b,k,A) + (1-\delta)k\varepsilon_k - b + q(z,b',k',A)b' - k', & \text{if not in default, and} \\ \Lambda(y(z,b,k,A)) + (1-\delta)k\varepsilon_k - H(b) + q(z,b',k',A)b' - k', & \text{if in default, with } b' \leq 0 \end{cases} \tag{7}$$

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 each period, entrepreneurs draw the TFP shock z, the exit shock, and the recovery shock. They then decide whether to default. If they do not default, they choose next-period investment k' and borrowing b'. If they default, they instead choose investment k' and savings $b' \leq 0$. At the start of the following period, all three shocks are drawn 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] This shock is drawn at the same time as the TFP, exit, and recovery shocks. We assume that the crisis is unanticipated, generating sudden liquidity needs. Some firms may benefit from the crisis, reflecting the heterogeneous nature of the COVID-19 shock (e.g., restaurants versus online retailers), but all firms draw from a common distribution $F(\varepsilon_k)$. The assumption of a temporary shock is reasonable given the two-year length of each model period and the fact that Chile's GDP had already recovered two years after the crisis.

After learning the shocks and deciding whether to default, any entrepreneur who has neither exited nor defaulted is eligible to apply for a GGL loan at $t = 0$. We model the GGL as a special two-period loan.[22] The GGL co-exists with the standard market-lending. We assume that at $t = 1$, the entrepreneur makes no GGL payment, and all GGL debt is serviced in $t = 2$. Further, we assume that the firm cannot selectively default, and in the event of default, no GGL payment is made, whereas market debt suffers the standard haircut $H(b')$. The GGL price q^G is set by the government, and the borrowing limit is captured by a cap ϕA.[23]

[21] 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 qualitative effect.

[22] This assumption aligns with a GGL program that has 4 years of maturity, 40% longer than comparable market loans.

[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 calibrate ϕ to approximate the mean and maximum loan size of the GGL as reported in Table A1.

GGL application procedure Firms decide whether to apply for the GGL program. If they do, they also choose the loan size $b^G \leq \phi A$. Each entrepreneur faces an idiosyncratic non-pecuniary application cost κ^{app}. This cost captures entrepreneurs' time and effort to learn about the GGL program and apply. We assume it is normally distributed with mean μ_{app} and standard deviation σ_{app}. An eligible entrepreneur applies for a GGL if

$$V^{app}(z_0, b_0, k_0, A) - \kappa^{app} \geq V_0^r(z_0, b_0, k_0, A, 0), \tag{8}$$

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, corresponding to the repayment value when GGL size is zero. Entrepreneurs who apply optimally choose 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 fixed price of the GGL loan q^G. We allow banks to make errors in evaluating applications, so that even firms with low expected payoffs have a non-zero probability of being approved. This feature captures the fact that default is a hard-to-predict event and allows rejections to arise in equilibrium, as we observe in the data. We model this by introducing a shock κ^{bank} drawn from a logistic distribution with mean zero and scale parameter σ_{bank}.

The bank accepts an application if:

$$\underbrace{\mathbb{E}_{t=0}\left[(1-\mathbf{d}_1)(1-\mathbf{d}_2)\right]}_{\substack{\text{Full repayment} \\ \text{(FP)}}} + \underbrace{g^G \mathbb{E}_{t=0}\left[\mathbf{d}_1 + (1-\mathbf{d}_1)\mathbf{d}_2\right]}_{\substack{\text{Taxpayer cost} \\ \text{(TC)}}} - \underbrace{q^G b^G (1+r)^2}_{\substack{\text{Cost of funds} \\ \text{(CF)}}} + \kappa^{bank} \geq 0. \tag{9}$$

Equation (9) decomposes the components of the bank's expected payoff.[24] The term FP represents the full repayment of the loan when firms do not default in either $t = 1$ or $t = 2$. The term TC represents the subsidy that the government gives to incentivize the bank to lend to risky firms. It is paid 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 all guarantee payments occur at the terminal period, so the bank does not gain from early insurance payments.[25]

[24] Expectations are taken at $t = 0$, after all period-specific shocks but κ^{bank} are drawn, since banks only receive applications from entrepreneurs that have neither exited nor defaulted.

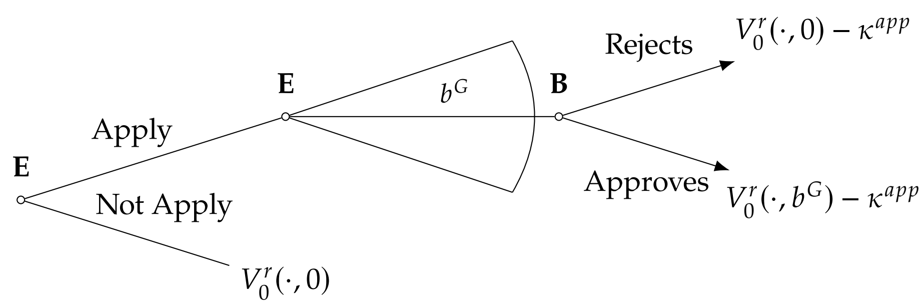
[25] This assumption reflects the institutional fact that the guarantee only covered the principal, not accrued interests, according to Article 19 of its regulation (CMF, 2016).

The bank compares these expected payoffs FP and TC with the opportunity cost of lending CF and the idiosyncratic shock κ^{bank}, which captures the bank's ability to correctly evaluate the application. Under the logistic assumption, the probability of approval is given by:

$$\mathbb{P}(\text{approve}) = \frac{\exp\left(\frac{FP+TC-CF}{\sigma_{bank}}\right)}{1+\exp\left(\frac{FP+TC-CF}{\sigma_{bank}}\right)}. \tag{10}$$

Once the bank approves the application, the funds are transferred to the entrepreneurs. Then, they decide how much to invest k_1 and how much additional amount to borrow b_1 from the bank at the schedule given by (6). Figure 4 summarizes the timing and payoffs of the application process. In Appendix D, we present the Bellman equations of the entrepreneurs and define the equilibrium, and Appendix E describes the numerical solution method.

Figure 4: GGL application process. The figure illustrates the timing and payoffs of the application process. **E** denotes the entrepreneur, **B** the bank. $V_0^r(\cdot, b^g)$ is the value at the repayment state when the size of the loan is b^g, and κ^{app} is the idiosyncratic cost of application.



Entrepreneur sorting Our model generates endogenous sorting of entrepreneurs into three groups: non-applicants, approved, and rejected. Everything else equal, a higher draw of κ^{app} discourages an entrepreneur from applying for a GGL. Conditional on applying, the sorting between approved and rejected is determined by κ^{bank}. In the absence of κ^{bank}, banks would approve applications only if the expected total payoff from the loan is nonnegative, as stated in (9) when $\kappa^{bank} = 0$. This cutoff condition introduces a rent component in the bank's approval decision: while the GGL loan has a fixed price q^G, its return varies with the firm's risk profile. The parameter σ^{bank} governs how sensitive banks are to this rent when deciding whether to reject an application. In the limiting case where $\sigma^{bank} = 0$, no rejections occur in equilibrium, since overly risky entrepreneurs perfectly anticipate rejection and self-select out of applying.

5. Model Quantification

We quantify the model in three steps described in detail below. First, we preset a subset of parameters to values estimated in the literature or match them directly to data. Second, we estimate pre-crisis parameters in the model based on data before the COVID crisis. Third, we estimate crisis and GGL parameters using data from the post-crisis period.

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.

To recover the production function parameters, we extend the main sample of Section 2.1 include firms with similar pre-crisis characteristics. Specifically, we use firms observed between 2016 and 2019 whose sales, wage bill, assets, debt, returns, leverage, and age fall between the 5th and 95th percentiles of the corresponding distributions in the main sample. Using this pre-crisis sample, we estimate the following production function:

$$\ln(\text{sales}_{it}) = \ln A_i + \alpha \ln(\text{assets}_{it}) + \mathbf{\Gamma}' \widetilde{X}_{it} + \widetilde{\epsilon}_{it}, \tag{11}$$

where $\tilde{X}_{it}$ is a vector of variables that are likely to affect sales directly, but we do not explicitly include in the structural model.[26] This regression yields the capital shape $\alpha = 0.23$, firm-specific 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).[27] Given ρ_z and the estimated standard deviation of $\tilde{\epsilon}_{it}$, we recover an TFP innovation volatility of $\sigma_z = 0.138$.

The risk-free rate is set to $r = 2.7\%$, corresponding to the average pre-crisis deposit rate.[28] The probability of firm exit is $\psi = 4\%$, matching the average annual exit rate observed between 2017 and 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

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

[26] We include the logarithm of firm age (in levels and squared).

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

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

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 annual percentage rate of 3.5%, the fixed rate of the GGL. For simplicity, we abstract from size-specific guarantees and instead assume a uniform guarantee share of 74%, corresponding to the average share in our main panel.

Pre-crisis estimation We estimate the discount factor β and the cost of default λ by simulated method of moments (SMM). We simulate our model assuming no crisis and no GGL. Then, we search for the combination of β, λ that generates a mean default rate of 6% and a mean leverage (debt-to-assets ratio) of 30%, as observed in the pre-crisis period.

Crisis estimation Using the estimated values of β, λ from the pre-crisis estimation, we next estimate the parameters governing the crisis shock and the GGL application process. Table 3 summarizes all model parameters. We parameterize the crisis shock as lognormally distributed with mean μ_ε and standard deviation σ_ε. The goal is to estimate $\mu_\varepsilon, \sigma_\varepsilon, \mu_{app}, \sigma_{app}, \sigma_{bank}$. We use SMM under the assumption that the crisis shock occurs and the GGL option is available. We search for the set that minimizes the squared sum of percentage deviations between model-generated and observed post-crisis moments. These target moments are: the share of applicants among eligible firms (58%), the share of rejected firms among applicants (14%), the default rate of approved firms in 2021 (1.8%), the default rate of rejected firms in 2021 (8.4%), the default rate of non-applicant firms in 2021 (4.5%), and the estimated effect of the GGL on default probability from the IV regression, β^{IV}_{2021}, reported in Table 1 (−5.2pp).

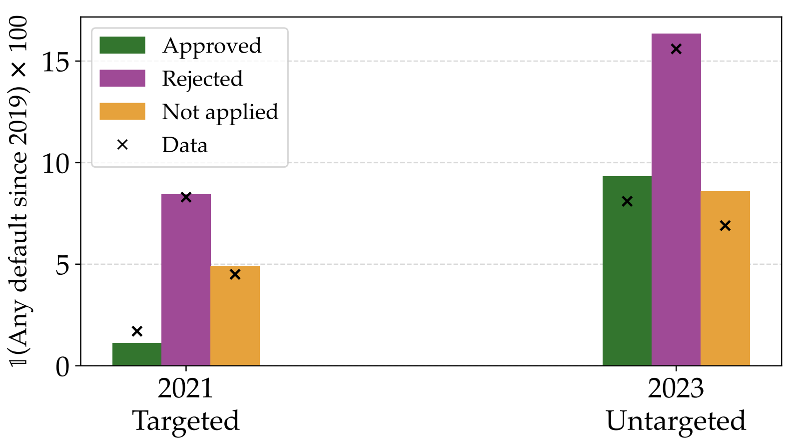
To estimate β^{IV} within the model, we first compute, for each simulated firm, the probability of approval given by equation (10). We then draw a uniform random shock and compare it to this probability to determine approval outcomes. The same uniform draw is used as an instrument in a two-stage least squares (2SLS) regression of default status on approval status among applicants, which we run in the simulated data. This procedure provides the exogenous variation in approval that serves as the model counterpart to the IV regression in Section 3.

Model Fit The model closely replicates the observed default patterns and the sorting of firms into applicants, rejected, and non-applicants. Figure 5 compares the model's cumulative default rates for approved, rejected, and non-applicant firms in 2021 and 2023, which correspond to $t = 1, 2$ in the model. In particular, the model reproduces the pattern of low default rates among approved firms in 2021, high default rates among rejected firms, and intermediate rates among non-applicants. Although the crisis-period estimation is overidentified, the model fits all the targeted moments closely.

Table 3: Model parameters We report the estimated parameters of the model. The first group of parameters is preset, either taken from the literature, directly read from the data, or estimated through equation (11). The second group is estimated by SMM using our model under no crisis and no GGL to match leverage (30%) and default rate (6%) of the pre-crisis period. The third group is also estimated by SMM under the assumption that the crisis shock occurs and the GGL option is available. Parameters marked with (*) are reported on an annual basis for ease of interpretation, but are compounded in the estimation to match the two-year periodicity of the model.

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 (11)
	$\ln A$	Long-run TFP	9.3 ± 1.7	4 bins, Aux. Regressions (11)
	σ_z	Volatility TFP shock	0.138	Aux. Regressions (11)
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}	-

Figure 5: Model fit, predictions and data. The figure shows the model fit for the default rates of approved, rejected, and non-applicant firms in 2021 and 2023. Bars represent model-generated moments, while crosses denote their data counterparts. Moments for 2021 are targeted in the estimation, whereas those for 2023 are untargeted. The remaining moments are: the share of applicants among eligible firms, 62% (58% in the data); the share of rejected firms among applicants, 15% (14%); and the estimated IV coefficient for 2021 of Table 1, −5.5pp (−5.2pp).



Validation As a validation check, the model closely predicts the default rates of the three firm groups in 2023, which are not targeted in the estimation. Thus, it can reproduce the sharp increase in defaults among approved firms in 2023, as well as the observed difference in long-run default of approved and rejected firms. These results confirm that the model provides a useful framework to study the effects of the crisis and the GGL on default probabilities in the short and the long run.

6. Results of the Structural Model

In this section, we first use the estimated model to recover which entrepreneurs receive support in terms of their unobserved long-run productivity and crisis shocks, which generate heterogeneity in solvency and liquidity, respectively. We then discuss the welfare gains of the policy and its cost-effectiveness, both in aggregate and across the firm distribution. Given the subsidized nature of the intervention, we quantify how much excess risk-taking and rent extraction the loans induce. Finally, we study budget-neutral counterfactual designs to assess whether the policy's cost-effectiveness can be improved.

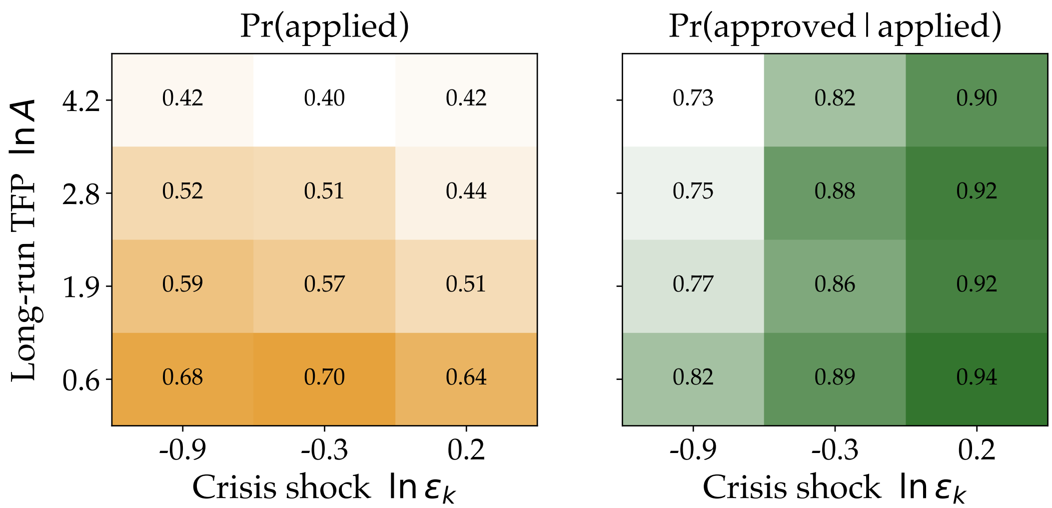
6.1. Who applied and who received a GGL

Our calibrated model allows us to infer which type of entrepreneurs decide to apply, and which ones are approved for a GGL based on their unobserved long-run productivity A and crisis shock ε_k. Everything else equal, these variables generate heterogeneity in solvency and liquidity, respectively. Figure 6, left panel, shows the probability of applying for a GGL across bins of long-run TFP and crisis-shock draws. As ε_k increases, applications decline. Entrepreneurs seek to smooth consumption and recover their capital stock from the crisis shock. So, the more adversely affected they are, the more they value bringing resources to the crisis period. The GGL provides a low-cost way to do so. Even firms that benefit from the crisis ($\varepsilon_k > 0$) prefer to reduce their outstanding market debt to save resources for later. They use the GGL just as a cheaper version to attain this lower level of debt.

Application probability also decreases with long-run TFP. Two mechanisms drive this pattern, both in the same direction. First, as firm scale increases, entrepreneurs operate in a region of the utility function that is less concave, weakening the smoothing motive. Second, larger firms tend to be more likely to be rejected when facing adverse shocks, which discourages them from applying in the first place.

The right panel of Figure 6 shows the probability of receiving a GGL conditional on applying. Overall, this probability is high, as the model is calibrated to match the unconditional approval rate of 86%. Conditional on long-run TFP, approval probability rises with the crisis shock. Because the guarantee is only partial, banks still bear some default risk, and therefore tend to approve firms that appear safer and experience milder crisis shocks. Entrepreneurs with the

Figure 6: Applications and approvals. The left panel shows the probability of applying for a GGL, conditional on being eligible, across bins of firms defined by long-run TFP ($\ln A$) and crisis impact ($\ln \varepsilon_k$). The intensity of the color indicates the probability of application, which is reported at the center of each cell. The right panel shows the probability of receiving a GGL, conditional on applying, across the same groups.



most favorable shocks also apply for larger GGLs: on average 9% of pre-crisis sales, compared with 6% among firms with the most adverse shocks.

Interestingly, the probability of receiving a GGL declines with long-run TFP, particularly within the most severe draws of the crisis shock. Damaged firms with high TFP are those with the greatest growth potential. To rebuild their productive scale, they must take on more leverage, which increases risk exposure. Under the cutoff condition in equation (9), banks are less willing to bear that risk, and as a result, approval rates are lower for these firms.

6.2. Welfare gains of the GGL intervention

Using the quantified model, we recover the welfare gains generated by the GGL intervention. We first report results in terms of consumption equivalents (CE). The government guarantee entails a fiscal cost determined by loan size, default rates, and the guarantee share. To compare this taxpayer cost with the benefits received by entrepreneurs, we also compute money-metric welfare gains (MWG). Together, these measures allow us to assess the program's cost-effectiveness. Finally, to understand the mechanisms behind the aggregate results, we use the model to decompose welfare gains across the firm distribution.

Aggregate welfare gains We define CE as the percentage increase in one-period consumption from a scenario with the crisis but no policy, which makes entrepreneurs indifferent between that counterfactual and the factual case with both the crisis and the GGL. Because of the application process, we can use values at different stages of the loan assignment to compute CE. Our preferred metric uses $V_0^r(\cdot, b^g)$, the value after the guaranteed loan of size b^g is assigned, as it is not directly affected by the shocks that influence the application process. For example, a welfare measure computed before loan assignment would also capture the expected gains of rejected firms.

We find that the GGL implies a CE of 1.2% on the average approved firm. These are substantial gains. They represent 21% of the average pre-crisis private consumption annual growth rate in Chile. These gains are comparable to estimates of other stabilization policies in Chile. Garcia-Villegas and Heresi (2025) evaluate the CE of the Chilean fiscal rule. Their estimates for agents with access to credit, mapped into our one-period consumption framework, imply a CE of 2.2%, which is in line with our findings.

When comparing its fiscal costs to the policy, we find that the policy was effective, exceeding its budget by 21%. 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), formally defined in (9), is the expected payment of the government to the bank in case of default. In dollar terms, these gains are equivalent to 1860 USD per approved firm.[29]

We define CE as the percentage increase in one-period consumption, relative to a scenario with the crisis but no policy, that makes entrepreneurs indifferent between that counterfactual and the factual case. Because of the application process, CE can be computed at different stages of loan assignment. Our preferred measure uses $V_0^r(\cdot, b^g)$, the value after the guaranteed loan of size b^g is assigned, since it is not directly affected by the shocks that influence the application process. For instance, a welfare measure computed before loan assignment would also capture the expected gains of rejected firms.

We find that the GGL yields a CE gain of 1.2% for the average approved firm. These are substantial gains, equivalent to 21% of Chile's average annual pre-crisis private consumption growth rate. They are comparable in magnitude to other stabilization policies in Chile. For example, Garcia-Villegas and Heresi (2025) estimate that the country's fiscal rule generates a CE gain of 2.2% when mapped into our one-period consumption framework, a similar magnitude to our findings.

Comparing benefits and fiscal costs, we find that the policy was cost-effective, generating welfare gains that exceeded its budget by 21%. We define the money-metric welfare gain (MWG)

[29] To estimate the dollar value of GGLs, we first estimate the tax cost of the GGL program in the data using the observed annual default rates of approved firms, the observed guarantee of each loan, and assuming a fixed repayment schedule of the loan. Then, we multiply the tax burden in the data by the model-based cost-effectiveness ratio, and normalize it by the size of the approved firms in the main sample.

as the monetary transfer that an entrepreneur would need in the no-policy scenario to achieve the same value as in the factual policy scenario. The expected taxpayer cost (TC), formally defined in equation (9), corresponds to the government's expected payment to banks in case of default. In dollar terms, these gains amount to USD 1860 per approved firm.[30]

6.3. Welfare gain heterogeneity

While the policy had sizable welfare gains and was cost-effective, these summary results mask substantial heterogeneity across the firm distribution. We unpack this heterogeneity to gain insight into what drives the headline results, and to draw intuition about how the policy could be improved.

Firms that enter the crisis in a weaker position to absorb the shock experience larger gains from the policy. The top third of firms by pre-crisis leverage have an average CE 0.45pp larger than the bottom third, where the difference across the top and bottom thirds of capital is -0.15pp. These firms tend to be growing firms, that is, firms that will invest more since they have not yet reached their optimal scale. This finding complements the reduced-form finding of Table 2 where we show that low-asset, high-leverage firms drive the aggregate effects on default.

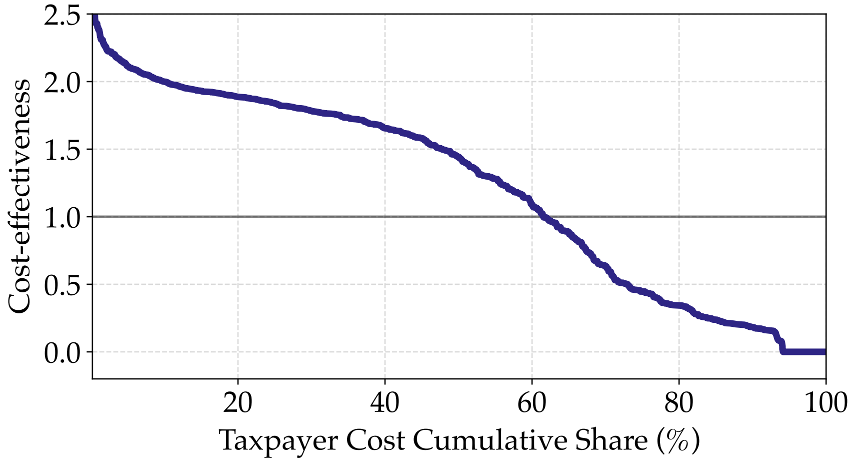
Welfare gains are negatively correlated with the crisis shock. Firms in the 10th percentile of the crisis shock distribution have an average CE of 1.49%, whereas those in the 90th percentile have a CE of 0.58%. This difference shows that gains are larger for firms more severely affected by the crisis. Variation in CE across long-run TFP is smaller, ranging from 1.15% to 1.19%.

By construction, all approved firms experience welfare gains, since taking the loan is an optimally chosen option. However, not all of them compensate for the associated fiscal cost: nearly 27% of approved firms do not offset the taxpayer cost. Figure 7 compares the cumulative taxpayer cost as we move down the distribution of cost-effectiveness. We measure cost-effectiveness as the ratio of MWG to TC for each firm, where a value of one indicates that the firm's benefit exactly equals the taxpayer cost. Almost 60% of the budget is allocated to cost-effective firms, indicating that larger firms with larger GGLs are less likely to be cost-effective.

The most cost-effective firms are those that entered the crisis in a weaker position. To characterize a typical cost-effective firm, we focus on the region corresponding to 20–40% of cumulative taxpayer cost of Figure 7. The average firm in this range has a cost-effectiveness ratio of 1.09. They enter the crisis with higher leverage (16 pp), lower assets (-31%), and was hit harder by the crisis than the average firm in the 60–80% cumulative-cost region. Taken together,

[30] To estimate the dollar value of GGLs, we first compute the fiscal cost of the program using observed default rates and guarantee shares, assuming a fixed repayment schedule. We then scale the estimated tax burden by the model-based cost-effectiveness ratio and normalize it by the size of approved firms in the main sample.

Figure 7: Cost-effectiveness heterogeneity and budget allocation. The figure shows what share of the taxpayer cost is allocated to cost-effective firms. Cost-effectiveness is measured as the ratio of the monetary-metric welfare gain to the taxpayer cost for each firm. A value of 1 means that the firm's benefit matches the taxpayer cost. We sort firms by this ratio and plot, on the x-axis, the cumulative share of taxpayer cost as we move from the most to the least cost-effective firms.



these results indicate space for policy improvement, since one quarter of firms received about two-fifths of the total budget, even though they valued the policy less than its cost.

6.4. Unintended consequences of the GGL

In the previous section, we showed that some firms did not value the support enough to offset its cost to taxpayers. In this section, we further examine two additional concerns associated with subsidized loan programs: bank rents and moral hazard.

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, with the magnitude of those rents depending on the borrower's risk profile. For example, consider a firm with zero default probability at $t = 1, 2$. The bank would be willing to approve a GGL 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 quantify the size of these rents and how taxpayer costs are distributed between banks and entrepreneurs. From equation (9), abstracting from the mean-zero noise in the evaluation process, we decompose taxpayer costs as $TP = (CF - TP) + R$, where R represents the rent accruing to banks, which makes equation (9) hold with equality. The term in brackets corresponds to the net transfer to entrepreneurs: the funds received minus the expected repayment.

We find that most of the taxpayer dollars ultimately reached entrepreneurs. On aggregate, bank rents accounted 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 larger rents, since they would have received relatively cheap loans even without the guarantee. Overall, this evidence suggests that the program generated modest bank rents relative to its total cost, implying that the GGL's fiscal burden primarily benefited entrepreneurs rather than financial intermediaries.

The model calibration suggests that banks screened firms with a high degree of accuracy. Because of the noise term κ^{bank}, some firms were misclassified as creditworthy when they were not—that is, firms with a strictly negative bank rent. Only 0.6% of approved firms would have received a GGL in the absence of this friction. These "lucky gamblers" enjoyed welfare gains 0.33 higher than the average approved firm.

Moral hazard A major concern with any subsidized lending program is moral hazard. Access to cheap credit may encourage firms to take on additional risk exposure by investing more in risky projects, or increasing leverage, exposing themselves to higher default risk. A strand of the literature warns about the consequences of such behavior in crisis lending programs, both for firm performance and for 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, as firms that received GGLs display higher long-run default rates than comparable non-GGL firms.

However, an alternative explanation is that differences in observed default rates may reflect selection rather than moral hazard. Firms may self-select into cheap credit programs because they need liquidity to survive the crisis. In Section 3, we show that long-run differences in default rates *among applicants* are driven by selection, as there is no significant effect on long-run defaults. Our structural model replicates this reduced-form finding and allows us to quantify the excess risk-taking induced by the GGL across the firm distribution, over and above the selection effect.

To quantify the implications of moral hazard, we define excess risk-taking as the increase in default probability in the factual scenario relative to a no-GGL counterfactual. We consider two default metrics, both evaluated from $t = 0$: default in $t = 1$, which we refer to as short-run default probability, and any default in $t = 1, 2$, which we refer to as long-run default probability.

On aggregate, we find moderate excess risk-taking. In the short run, the loan reduces the default probability by 4.6% relative to a no-policy crisis scenario, reflecting the liquidity provided by the GGL and its long maturity. At the time of repayment ($t = 2$), is when moral hazard impacts the aggregate results, although the effect is modest. Our calibrated model estimates an aggregate excess default rate of 0.8% in the long run. This finding is consistent with the main takeaway of Section 3: the GGL postponed defaults from the short run to the long run, without significantly altering cumulative defaults.

However, at the firm level, there is substantial heterogeneity. In the short run, we find that 81% of firms do not take on additional risk. This reflects the temporary effect of the loan on default rates, consistent with the results of Section 3. Excess risk-taking is limited in the short run: the share of firms whose default probability increases by more than 1 pp is below 10%. In contrast, in the long run—when the loan must be repaid—the policy induces more risk-taking. Only 56% of firms avoid taking extra risk. Risk-taking behavior is stronger among firms that enter the crisis with lower leverage, higher assets, and more favorable crisis shocks. This group is the same as the one that benefits less from the policy in welfare terms. This confirms that the moral hazard component is quantitatively relevant precisely among firms that are better positioned to face the crisis.

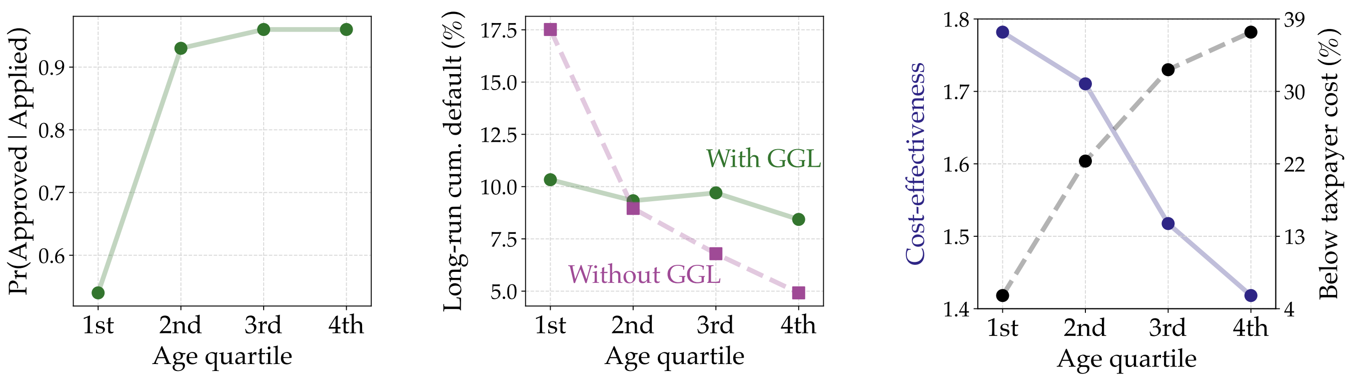
6.5. Using firms' age as targeting variable for policy design

We motivate a novel targeting variable for policy design: firm age. Our structural model shows that GGL were more likely to be allocated to firms that entered the crisis in relatively stronger positions–those with larger assets, lower leverage, higher long-run productivity, and more favorable crisis shocks. However, the model also indicates that these firms benefit the least from the policy and that the intervention is less cost-effective for them. Moreover, this group is where banks capture larger rents and where excess risk-taking is more prevalent. Because the GGL program is a coarse instrument, it can hardly be expected to achieve a first-best allocation of credit that fully avoids these inefficiencies. Instead, we show that firm age is a useful and easily observable targeting variable that can help mitigate these issues when long-run productivity and the impact of the crisis are unobserved by policymakers.

Figure 8 presents key model statistics by quartiles of firm age, measured at the onset of the crisis. The left panel shows the probability of loan approval conditional on applying. Firms in the youngest quartile are the least likely to be approved, about 40pp lower than any other quartile. Because the production function is concave and young firms begin with few assets, they exhibit high marginal returns to investment. To scale up, they must borrow more, which increases their leverage and risk exposure. Given this additional risk, banks are less willing to approve GGLs for these firms.

Figure 8: Heterogeneity by firm age. The figure shows model results across four bins of firm age measured at the onset of the crisis. The left panel reports the probability of loan approval by age. The middle panel compares default rates in a no-GGL scenario (pink, dashed) with those under the GGL (green). The right panel presents cost-effectiveness, defined as the median money-metric welfare gain relative to taxpayer costs within each bin (blue, left y-axis). A value of 1 indicates that the firm's welfare gain exactly matches the taxpayer cost. The black dashed line (right y-axis) shows the share of firms whose welfare gains do not offset taxpayer costs.

(a) Approvals (b) Default (c) Welfare



The middle panel of Figure 8 compares long-run cumulative default rates across age quartiles, showing both the GGL and no-GGL scenarios. Without the GGL, default rates would have been about 7.5pp higher for the youngest quartile. In contrast, all other age groups would have experienced slightly lower default rates in the absence of the program, reflecting moral hazard. For young firms, the GGL allows them to leverage safely to reach scale: because the guaranteed loan is long-term, repayment is delayed, so they can build assets before servicing the debt. Without the GGL, they would rely on market credit with shorter maturities, forcing them to repay sooner and depend on favorable productivity draws to avoid default. Older firms, by contrast, are closer to their optimal scale, so the subsidized credit mainly increases debt without much expansion of assets, increasing their likelihood of default.

The right panel of Figure 8 reports the median cost-effectiveness of the GGL across the same quartiles of firm age. The youngest firms are the most cost-effective: their welfare gains exceed taxpayer costs by roughly 80%, whereas for the oldest firms the excess is closer to 40%. Consistently, only about 4% of firms in the youngest group fail to offset taxpayer costs. These patterns reflect two key nonlinearities in the model. First, younger firms begin with fewer assets and thus consume less, implying higher marginal utility of consumption. Second, they also face higher marginal returns to capital, since they operate far below their optimal scale. As a result, they place greater value on a long-term loan that enables them to build assets before repayment.

6.6. Budget-neutral counterfactual interventions

In this section, we evaluate budget-neutral counterfactual designs of the GGL program to assess whether its cost-effectiveness can be improved. Our previous findings show that the GGL generated sizable welfare gains and was cost-effective on aggregate. Moreover, it did not create large bank rents, and aggregate excess risk-taking was modest. However, these effects are far from uniform across the firm distribution: we find that 27% of firms did not value the policy enough to offset its fiscal cost, and 44% of firms took on more risk than they would have in the absence of the GGL. These patterns suggest scope for policy improvement. By construction, the GGL cannot achieve a fully optimal allocation of liquidity, since it only sets a guarantee share, a fixed loan rate, and a maximum loan size, whereas the most cost-effective allocation of the budget would require granular conditioning on each firm state. The goal of these counterfactual exercises is to illustrate how a more fine-tuned version of the program could perform and to evaluate whether the observed policy design was too generous or too restrictive.

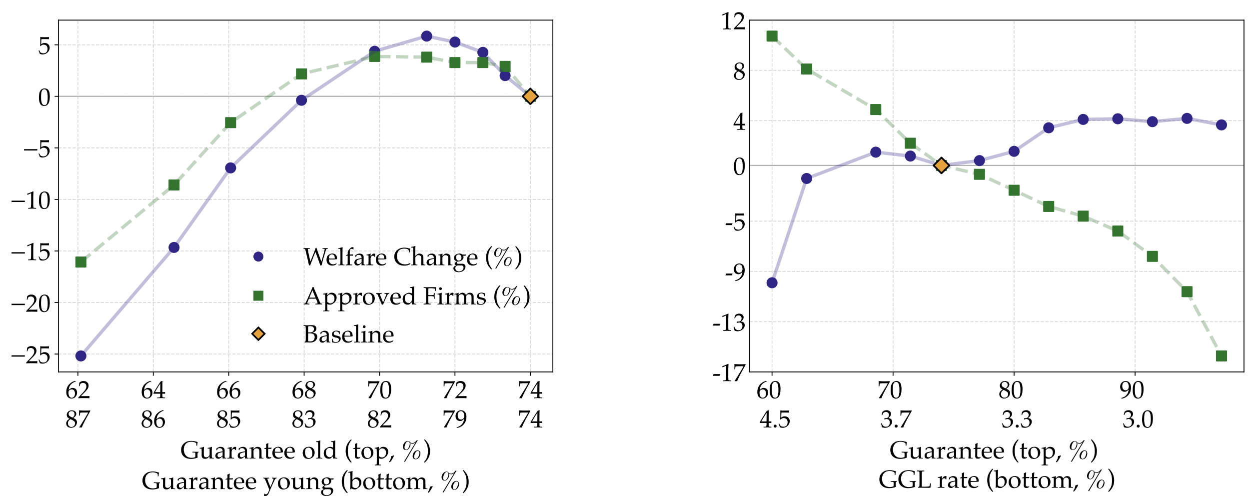
Motivated by the results of Section 6.5, we evaluate a counterfactual budget-neutral design that increases the guarantee share for the youngest quartile of firms, and decreases it for the rest. The goal is to increase the approval rate of younger, high-growth firms, that are the most cost-effective. Figure 9, left panel shows the number of approved firms and the sum of MWG relative to baseline of different pairs of guarantees for the youngest quartile and the rest. As we increase the guarantee for the young and reduce it for the rest, the number of approved firms increases, up to 4%, to fall quickly below baseline. This is a result of a monotonic increase in the approval of the young and a monotonic decrease in the approval of the rest. Since younger are more cost-effective, the aggregate MWG increases, to peak at 4% relative to baseline. For guarantees for the young above 83% (and below 63% for the rest), the policy becomes worse than baseline.

To contrast with an age-based targeting scheme, we provide an alternative set of budget-neutral counterfactuals. Instead of conditioning on observable firm characteristics, we expand the guarantee to all firms. To preserve budget neutrality, we simultaneously lower the fixed GGL rate, which reduces banks' returns on all loans and leads to a lower overall approval rate. The right panel of Figure 9 plots the welfare gains and number of approved firms for this counterfactual. Reducing the loan rate increases the number of firms willing to apply, but bank rejections rise faster, so total approvals fall. The GGL becomes concentrated among more cost-effective firms, raising aggregate welfare gains by up to 4pp relative to baseline. However, these efficiency gains plateau as approvals continue to decline, indicating that the marginal approved firm has a cost-effectiveness ratio close to the average. Overall, it is possible to achieve welfare gains 4pp higher than baseline by reducing the number of approved firms by at least 4pp.

Figure 9: Budget-neutral counterfactuals. The figure presents budget-neutral counterfactual designs of the GGL intervention. In the left panel, the guarantee share is increased for the youngest quartile of firms and reduced for the rest. In the right panel, the guarantee share is increased for all firms while the fixed loan rate is lowered. The y-axis in each plot shows the percentage change in both aggregate welfare gains (in monetary terms) and the number of approved firms relative to the baseline. The yellow diamond marks the baseline scenario.

(a) Different guarantees for old and young firms

(b) Change guarantee and rate for all firms



7. Conclusions

This paper provides new evidence on the short- and long-run impact of government-guaranteed loans as a crisis policy tool, using Chile's 2020 program as a case study. Our results show that the time horizon is crucial for assessing the effectiveness of government support. We also provide empirical evidence on the role of banks in allocating crisis credit: they actively screened borrowers using private information, suggesting that policymakers can leverage banks' superior assessment of borrower risk by setting appropriate incentives.

To quantify the welfare effects of the program, we develop a framework that accounts for heterogeneity in firms' solvency and liquidity, incorporates the program's rich set of design features, and allows for distortions such as moral hazard and bank rents, factors policymakers consider in the design of these modern GGL programs. Disciplining the model with our causal estimates, we find that the policy was highly cost-effective, generating welfare gains that exceeded its fiscal cost by roughly 21%.

Our framework highlights that the largest welfare gains accrue to younger, high-growth firms. This points to a promising direction for policy design: targeting firm age, an easily observable characteristic that captures much of the heterogeneity driving welfare effects. This is not a common practice, where the typical segmentation is by sales, number of employees, or sector. Such an approach could enhance the effectiveness of future interventions and has broader implications for policies aimed at supporting high-potential firms in other settings.

References

Viral V Acharya, Lea Borchert, Maximilian Jager, and Sascha Steffen. Kicking the Can Down the Road: Government Interventions in the European Banking Sector. *The Review of Financial Studies*, 34(9):4090–4131, January 2021. ISSN 0893-9454. doi: 10.1093/rfs/hhab002. URL https://doi.org/10.1093/rfs/hhab002. _eprint: https://academic.oup.com/rfs/article-pdf/34/9/4090/39831120/hhab002.pdf.

Viral V Acharya, Matteo Crosignani, Tim Eisert, and Christian Eufinger. Zombie credit and (dis-) inflation: evidence from Europe. *The Journal of Finance*, 79(3):1883–1929, 2024. Publisher: Wiley Online Library.

Miguel Acosta-Henao, Sangeeta Pratap, and Manuel Taboada. Four facts about relationship lending: The case of Chile 2012-2019. *Journal of Corporate Finance*, 80:102415, 2023. Publisher: Elsevier.

Sumit Agarwal, Brent W. Ambrose, Luis A. Lopez, and Xue Xiao. Did the Paycheck Protection Program Help Small Businesses? Evidence from Commercial Mortgage-Backed Securities. *American Economic Journal: Economic Policy*, 16(3):95–132, August 2024. ISSN 1945-7731. doi: 10.1257/pol.20220181. URL https://www.aeaweb.org/articles?id=10.1257%2Fpol.20220181.

Anna Aizer and Joseph J Doyle Jr. Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges. *The Quarterly Journal of Economics*, 130(2):759–803, 2015. Publisher: MIT Press.

Carlo Altavilla, Andrew Ellul, Marco Pagano, Andrea Polo, and Thomas Vlassopoulos. Loan guarantees, bank lending and credit risk reallocation. *Journal of Financial Economics*, 172: 104137, October 2025. ISSN 0304-405X. doi: 10.1016/j.jfineco.2025.104137. URL https://www.sciencedirect.com/science/article/pii/S0304405X2500145X.

Edward I Altman. Financial ratios, discriminant analysis and the prediction of corporate bankruptcy. *The journal of finance*, 23(4):589–609, 1968. Publisher: JSTOR.

Mary Amiti and David E Weinstein. How much do idiosyncratic bank shocks affect investment? Evidence from matched bank-firm loan data. *Journal of Political Economy*, 126(2):525–587, 2018. Publisher: University of Chicago Press Chicago, IL.

Cristina Arellano. Default risk and income fluctuations in emerging economies. *American economic review*, 98(3):690–712, 2008. Publisher: American Economic Association.

David Autor, David Cho, Leland D. Crane, Mita Goldar, Byron Lutz, Joshua Montes, William B. Peterman, David Ratner, Daniel Villar, and Ahu Yildirmaz. The $800 Billion Paycheck Protection Program: Where Did the Money Go and Why Did It Go There? *Journal of Economic Perspectives*, 36(2):55–80, May 2022a. ISSN 0895-3309. doi: 10.1257/jep.36.2.55. URL https://www.aeaweb.org/articles?id=10.1257/jep.36.2.55.

David Autor, David Cho, Leland D. Crane, Mita Goldar, Byron Lutz, Joshua Montes, William B. Peterman, David Ratner, Daniel Villar, and Ahu Yildirmaz. An evaluation of the Paycheck Protection Program using administrative payroll microdata. *Journal of Public Economics*, 211:104664, July 2022b. ISSN 0047-2727. doi: 10.1016/j.jpubeco.2022.104664. URL https://www.sciencedirect.com/science/article/pii/S0047272722000664.

Natalie Bachas, Olivia S Kim, and Constantine Yannelis. Loan guarantees and credit supply. *Journal of Financial Economics*, 139(3):872–894, 2021. Publisher: Elsevier.

Thorsten Beck, Leora F Klapper, and Juan Carlos Mendoza. The typology of partial credit guarantee funds around the world. *Journal of Financial Stability*, 6(1):10–25, 2010. Publisher: Elsevier.

Ben S. Bernanke, Mark Gertler, and Simon Gilchrist. Chapter 21 The financial accelerator in a quantitative business cycle framework. In *Handbook of Macroeconomics*, volume 1, pages 1341–1393. Elsevier, January 1999. doi: 10.1016/S1574-0048(99)10034-X. URL https://www.sciencedirect.com/science/article/pii/S157400489910034X.

Fabio Bertoni, Massimo G. Colombo, and Anita Quas. The long-term effects of loan guarantees on SME performance. *Journal of Corporate Finance*, 80:102408, June 2023. ISSN 0929-1199. doi: 10.1016/j.jcorpfin.2023.102408. URL https://www.sciencedirect.com/science/article/pii/S0929119923000573.

Javier Bolzico and Joan Oriol Prats Cabrera. Esquemas de garantía pública para créditos bancarios en tiempos de COVID-19 en América Latina y el Caribe. Technical Report IDB-DP-937, Inter-American Development Bank, 2022. URL https://doi.org/10.18235/0004193.

Diana Bonfim, Miguel A. Ferreira, Francisco Queiró, and Sujiao (Emma) Zhao. Fiscal Policy and Credit Supply in a Crisis. *American Economic Review*, 115(6):1896–1935, June 2025. doi: 10.1257/aer.20221499. URL https://www.aeaweb.org/articles?id=10.1257/aer.20221499.

J David Brown and John S Earle. Finance and growth at the firm level: Evidence from SBA loans. *The Journal of Finance*, 72(3):1039–1080, 2017. Publisher: Wiley Online Library.

Markus Brunnermeier and Arvind Krishnamurthy. Corporate debt overhang and credit policy. *Brookings Papers on Economic Activity*, 2020(2):447–502, 2020. Publisher: Johns Hopkins University Press.

Francisco J. Buera, Joseph P. Kaboski, and Yongseok Shin. Finance and Development: A Tale of Two Sectors. *American Economic Review*, 101(5):1964–2002, August 2011. ISSN 0002-8282. doi: 10.1257/aer.101.5.1964. URL https://www.aeaweb.org/articles?id=10.1257/aer.101.5.1964.

Ricardo J Caballero, Takeo Hoshi, and Anil K Kashyap. Zombie lending and depressed restructuring in Japan. *American economic review*, 98(5):1943–1977, 2008. Publisher: American Economic Association.

Santiago Camara and Maximo Sangiacomo. Borrowing constraints in emerging markets. *arXiv preprint arXiv:2211.10864*, 2022.

Maikol Cerda, Paul Gertler, Sean Higgins, Ana María Montoya, Eric Parrado, and Raimundo Undurraga. The Causal Impact of Covid-19 Government-backed Loans on MSMEs Liquidity and Earnings. Technical report, Inter-American Development Bank, 2023.

Gabriel Chodorow-Reich. The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis. *The Quarterly Journal of Economics*, 129(1):1–59, 2014. Publisher: MIT Press.

CMF. Reglamento de Administración del Fondo de Garantía para Pequeños y Medianos Empresarios, Artículo 19. Technical report, Comisión para el Mercado Financiero, 2016. URL https://www.fogape.cl/wp-content/uploads/2016/10/Reglamento-FOGAPE-actualizado.pdf.

Dean Corbae and Pablo D'Erasmo. Reorganization or liquidation: Bankruptcy choice and firm dynamics. *The Review of Economic Studies*, 88(5):2239–2274, 2021. Publisher: Oxford University Press.

Rosanna Costa. Crisis COVID-19 y sus desafíos, 2021. Published: Speech given as Board Member of the Central Bank of Chile.

Nicolas Crouzet and Fabrice Tourre. Can the cure kill the patient? Corporate credit interventions and debt overhang. *Corporate credit interventions and debt overhang (June 1, 2021)*, 2021.

Michael Dalton. Putting the Paycheck Protection Program into Perspective: An Analysis Using Administrative and Survey Data. *National Tax Journal*, 76(2):393–437, June 2023. ISSN 0028-0283. doi: 10.1086/724591. URL https://www.journals.uchicago.edu/doi/full/10.1086/724591. Publisher: The University of Chicago Press.

David De Meza. Overlending? *Economic Journal*, 112(477):F17–F31, 2002. ISSN 0013-0133. doi: 10.1111/1468-0297.00681.

Stefano Federico, Fadi Hassan, and Veronica Rappoport. Trade Shocks and Credit Reallocation. *American Economic Review*, 115(4):1142–69, April 2025. doi: 10.1257/aer.20200704. URL https://www.aeaweb.org/articles?id=10.1257/aer.20200704.

Salomon Garcia-Villegas and Rodrigo Heresi. Rethinking Fiscal Rules in Resource-Rich Economies. October 2025. URL https://www.salomongarcia.com/_files/ugd/0fd62f_651fb499762c40758cdd0e8042b0179b.pdf.

João Granja, Christos Makridis, Constantine Yannelis, and Eric Zwick. Did the paycheck protection program hit the target? *Journal of Financial Economics*, 145(3):725–761, September 2022. ISSN 0304-405X. doi: 10.1016/j.jfineco.2022.05.006. URL https://www.sciencedirect.com/science/article/pii/S0304405X22001131.

Reint Gropp, Christian Gruendl, and Andre Guettler. The Impact of Public Guarantees on Bank Risk-Taking: Evidence from a Natural Experiment*. *Review of Finance*, 18(2):457–488, April 2014. ISSN 1572-3097. doi: 10.1093/rof/rft014. URL https://doi.org/10.1093/rof/rft014.

John Hackney. Small Business Lending in Financial Crises: The Role of Government-Guaranteed Loans*. *Review of Finance*, 27(1):247–287, February 2023. ISSN 1572-3097. doi: 10.1093/rof/rfac002. URL https://doi.org/10.1093/rof/rfac002.

Christopher A Hennessy and Toni M Whited. Debt dynamics. *The journal of finance*, 60(3): 1129–1165, 2005. Publisher: Wiley Online Library.

Neus Herranz, Stefan Krasa, and Anne P. Villamil. Entrepreneurs, Risk Aversion, and Dynamic Firms. *Journal of Political Economy*, 123(5):1133–1176, 2015. ISSN 00223808, 1537534X. URL http://www.jstor.org/stable/10.1086/682678. Publisher: The University of Chicago Press.

Gee Hee Hong and Deborah Lucas. COVID-19 Credit Policies around the World: Size, Scope, Costs, and Consequences. *Brookings Papers on Economic Activity*, 2023(1):289–345, 2023. ISSN 1533-4465. URL https://muse.jhu.edu/pub/1/article/919361. Publisher: Johns Hopkins University Press.

Takeo Hoshi, Daiji Kawaguchi, and Kenichi Ueda. Zombies, again? The COVID-19 business support programs in Japan. *Journal of Banking & Finance*, 147:106421, 2023. Publisher: Elsevier.

Federico Huneeus, Joseph P Kaboski, Mauricio Larrain, Sergio L Schmukler, and Mario Vera. Crisis Credit, Employment Protection, Indebtedness, and Risk. Technical report, CESifo Working Paper, 2025.

Gabriel Jiménez, José-Luis Peydró, Rafael Repullo, and Jesús Saurina. Burning Money? Government Lending in a Credit Crunch. Technical report, 2018.

Gabriel Jiménez, Luc Laeven, David Martinez Miera, and José-Luis Peydró. Public Guarantees, Private Banks' Incentives, and Corporate Outcomes: Evidence from the COVID-19 Crisis. ECB Working Paper 2913, European Central Bank, March 2024. URL https://ssrn.com/abstract=4756098.

Asim Ijaz Khwaja and Atif Mian. Tracing the impact of bank liquidity shocks: Evidence from an emerging market. *American Economic Review*, 98(4):1413–1442, 2008. Publisher: American Economic Association.

Jeffrey R Kling. Incarceration length, employment, and earnings. *American Economic Review*, 96 (3):863–876, 2006. Publisher: American Economic Association.

Federico Kochen. Finance over the life cycle of firms. *Unpublished manuscript*, 2023.

Wenhao Li and Ye Li. Firm quality dynamics and the slippery slope of credit intervention. Technical report, National Bureau of Economic Research, 2025.

Deborah Lucas. How Much Do Guarantees and Bailouts Cost the Government?, May 2024. URL https://papers.ssrn.com/abstract=5194420.

Carlos Madeira. The Impact of the Covid Pandemic Public Policies in Chile on Consumption. *Economía LACEA Journal*, May 2023. doi: 10.31389/eco.6.

Alberto Martin, Sergio Mayordomo, and Victoria Vanasco. Banks vs. firms: who benefits from credit guarantees? Technical report, Working Paper, 2025.

William Matcham. Risk-based borrowing limits in credit card markets. *Available at SSRN 4926974*, 2025.

Virgiliu Midrigan and Daniel Yi Xu. Finance and Misallocation: Evidence from Plant-Level Data. *American Economic Review*, 104(2):422–458, February 2014. ISSN 0002-8282. doi: 10.1257/aer.104.2.422. URL https://www.aeaweb.org/articles?id=10.1257/aer.104.2.422.

Benjamin Moll. Productivity Losses from Financial Frictions: Can Self-Financing Undo Capital Misallocation? *American Economic Review*, 104(10):3186–3221, October 2014. ISSN 0002-8282. doi: 10.1257/aer.104.10.3186. URL https://www.aeaweb.org/articles?id=10.1257/aer.104.10.3186.

William Mullins, Patricio Toro, and others. Credit guarantees and new bank relationships. *Central Bank of Chile Working Paper*, 820, 2018.

Stewart C Myers. Determinants of corporate borrowing. *Journal of financial economics*, 5(2): 147–175, 1977. Publisher: Elsevier.

Jose Renato Haas Ornelas, Alvaro Pedraza, Claudia Ruiz-Ortega, and Thiago Christiano Silva. Market Power and the Transmission of Loan Subsidies. *The Review of Corporate Finance Studies*, 13(4):931–965, November 2024. ISSN 2046-9128. doi: 10.1093/rcfs/cfae015. URL https://doi.org/10.1093/rcfs/cfae015.

Pablo Ottonello and Thomas Winberry. Financial Heterogeneity and the Investment Channel of Monetary Policy. *Econometrica*, 88(6):2473–2502, 2020. ISSN 1468-0262. doi: 10.3982/ECTA15949. URL https://onlinelibrary.wiley.com/doi/abs/10.3982/ECTA15949. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.3982/ECTA15949.

Banco Santander. FAQ COVID loans. Technical report, Santiago de Chile, 2020. URL https://banco.santander.cl/uploads/000/014/555/c3d1a44d-11a4-4879-a0f5-1f5a694ce59b/original/Preguntas_frecuentes_Credito_Covid.pdf.

Anatoli Segura and Alonso Villacorta. Firm-bank linkages and optimal policies after a rare disaster. *Journal of Financial Economics*, 149(2):296–322, 2023. Publisher: Elsevier.

David Stillerman. Loan guarantees and incentives for information acquisition. *Available at SSRN 4411553*, 2024.

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-cumulative default rates by group. The figure shows non-cumulative default rates by group. The y-axis measures the share of firms that experience a default in a given year. In each period, firms that have already defaulted are excluded from the sample. Vertical bars represent 95% confidence intervals.

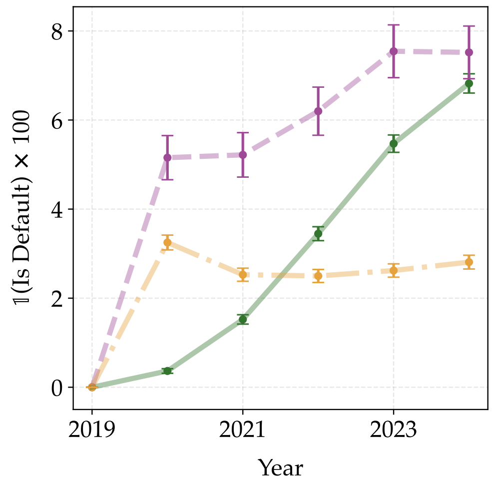


Figure A2: Instrument variation. The left panel shows the CDF of the standard deviation of the systematic component of the GGL approval policy, $\widehat{\Gamma}_b X_i$, defined in equation (2). It is estimated within each firm across all banks, independently if the firm was a client from the bank or not. This measure captures how much banks differ in their approval policies even for the same firm. The right panel shows the CDF of the instrument Z_i, defined in equation (3).

(a) Disagreement across banks

(b) CDF of the instrument

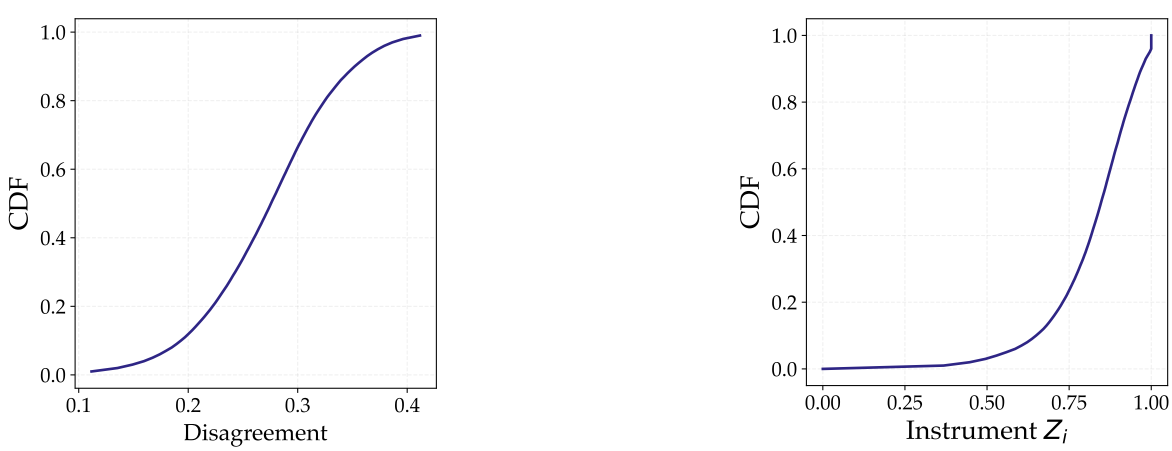


Table A2: First stage results We report the first stage results of the instrument (3) on the approval decision of the bank, conditional on application. We report the F statistic of excluded instruments. Controls 1́9 include all variables X_i defined in Section 3.1. Robust standard errors in parentheses.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

Figure A3: Evidence of bank screening. The figure shows the differences between the OLS and IV coefficient estimates from equation (1). Point estimates correspond to the values reported in Table 1. Bars represent 95% confidence intervals based on 200 bootstrap replications.

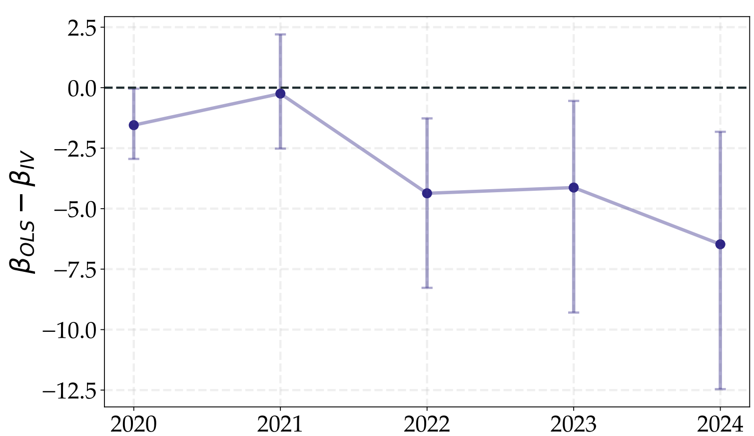


Table A3: Robustness The table shows the estimates of β_t for the specification of (1), on alternative subsets of firms. The first two panels reproduce the results of Table 1 for easy comparison. The third panel runs the same specification, but only on firms that sell every period, to address concerns 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. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, 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 regressions 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. Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, 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 parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, 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:

$$V(s) = \max_{d \in \{0,1\}} \left\{(1-d)V^r(s) + dV^d(s)\right\} \tag{12}$$

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:

$$\begin{aligned} V^r(s) = \max_{b',k'} u(c) + \beta \mathbb{E}_{z'} \left[\psi V^e(s') + (1-\psi)V(s')\right] \\ \text{s.t.} \quad c + k' + b = y(s) + (1-\delta)k + q(z, b', k', A)b' \\ k' \geq 0 \\ b' \in [\underline{b}, \overline{b}] \end{aligned} \tag{13}$$

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:

$$V^d(s) = \max_{b',k'} u(c) + \beta \mathbb{E}\left[\psi\left(\chi V^e(s') + (1-\chi)V^{ed}(s')\right) + (1-\psi)\left(\chi V(s') + (1-\chi)V^d(s')\right)\right] \tag{14}$$

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

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:

$$V^e(s) = \max_{d \in \{0,1\}} \left\{(1-d)V^{er}(s) + dV^{ed}(s)\right\} \tag{15}$$

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:

$$V_2^r(s_2, b^G) = \max_{b_3,k_3} u(c_2) + \beta \mathbb{E}_{z_3} [\psi V^e(s_3) + (1-\psi)V(s_3)] \tag{16}$$
$$\text{s.t.} \quad c_2 + k_3 + b_2 + b^G = y(s_2) + (1-\delta)k_2 + q(z_2, b_3, k_3, A)b_3$$
$$k_3 \geq 0$$
$$b_3 \in [\underline{b}_3, \overline{b}_3]$$

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 (13) and (14), 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:

$$V_1^r(s_1, b^G) = \max_{b_2,k_2} u(c_1) + \beta \mathbb{E}_{z_2} \left[\psi V^e(s_2) + (1-\psi)V_2(s_2, b^G)\right] \tag{17}$$
$$\text{s.t.} \quad c_1 + k_2 + b_1 = y(s_1) + (1-\delta)k_1 + q(z_1, b_2, k_2, A)b_2$$
$$k_2 \geq 0$$
$$b_2 \in [\underline{b}_2, \overline{b}_2]$$

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:

$$V_0^r(s_0, b^G) = \max_{b_1,k_1} u(c_0) + \beta \mathbb{E}_{z_1} \left[\psi V^e(s_1) + (1-\psi)V_1(s_1, b^G)\right] \tag{18}$$
$$\text{s.t.} \quad c_0 + k_1 + b_0 = y(s_0) + (1-\delta)\varepsilon_k k_0 + q(z_0, b_1, k_1, A)b_1 + q^G b^G$$
$$k_1 \geq 0$$
$$b_1 \in [\underline{b}_1, \overline{b}_1]$$

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 (10). 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:

$$V_0^{app}(s_0) = \max_{b^G} \mathbb{P}(\text{approve} \mid s_0, b^G) \cdot V_0^r(s_0, b^G) + \Big(1 - \mathbb{P}(\text{approve} \mid s_0, b^G)\Big) \cdot V_0^r(s_0, 0) - \kappa^{\text{firm}} \tag{19}$$

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

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 (10). 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:

$$V_0^r(s_0) = \max\{V_0^r(s_0, 0), V_0^{app}(s_0)\} \tag{20}$$

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 $t \geq 3$, (12)–(15) and (6) are satisfied.

2. For $t \in \{0, 1, 2\}$, (16)–(20), (6) and (10) 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 (11).

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 (14).
    Compute $V^r(z, l, b, A)$ using (13).
    Update the default policy when no exit shock occurs $\tilde{\mathbf{d}}(z, l, b, A)$ and $V(z, l, b, A)$ using (12).
    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.