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Keywords: Regression Discontinuity Design, Corporate Liquidity, Financial Frictions

JEL Codes: C26, G32, E44

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Abstract

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1 Introduction

Understanding the pass-through channels of monetary policy tools to the real economy is at the core of macroeconomic analysis and is fundamental to the design of effective monetary systems and stabilization frameworks. In recent years, there has been an increasing reliance on unconventional monetary policy (UMP) tools, including interventions in corporate bond markets. These tools were designed to directly influence corporate credit markets—and, by extension, economic activity—when the traditional transmission channels of the baseline interest rate become "clogged" during periods of stress. Major tools include direct purchases of corporate debt and expansion to the collateral framework, the latter being the focus of our paper.

The collateral framework involves the central bank accepting corporate securities as collateral in its lending operations with commercial banks and other financial intermediaries. By changing eligibility criteria and haircuts applied to these assets, central banks can affect the liquidity and, thus, the demand for these assets. Despite their widespread use by the major central banks in the world, estimating their real effects on firms and respective transmission mechanisms is challenging because of a combination of both a lack of comprehensive datasets and endogeneity in eligibility criteria, simultaneity with macroeconomic shocks, and general equilibrium spillovers, which complicate causal inference and leave this question largely unanswered. This is important not only to evaluate the effectiveness of past interventions but also to improve the design of monetary policy frameworks going forward, as collateral frameworks are integral to the functioning of monetary systems in many countries.

This paper leverages the inclusion of corporate debt in the Brazilian Central Bank's collateral framework and introduces a novel dynamic regression discontinuity design (RDD) methodology to estimate the causal effects of debt eligibility on firms' financial and real economic outcomes. Additionally, we examine how this eligibility influences liquidity and pricing in both primary and secondary corporate bond markets. The policy we study, the Liquidity Facility Lines (LFL), introduced in November 2021, provides liquidity to financial institutions using corporate bonds as collateral. Each participating financial institution has an LFL limit based on the face value of deposited eligible corporate bonds, which can be used for short- and medium-term credit lines and reducing reserve requirements. This policy marked a shift from the previous reliance on Treasury securities for liquidity provisions, following recommendations made by IMF (2012) to broaden collateral eligibility.[1] Thus the policy directly improves corporate bond liquidity for financial institutions holding eligible debt, potentially affecting asset demand.

Our empirical findings indicate that eligible firms experienced, one year into the policy, a 12% increase in corporate bond debt within a year, alongside a decrease in intermediate bank/fund debt by 5%, and a 20 basis point reduction in cost of capital. However, there were no significant effects on corporate debt maturity or bank/fund interest rates. Additionally, we observed a 6% real increase in the wage bill and a temporary rise in net payment outflows to non-financial firms, driven by higher payment outflows. This increase in expenditure was partially financed by a 12% reduction in liquid cash holdings, suggesting a decreased precautionary savings motive. The policy's impact was amplified by the increased use of corporate bonds as collateral, especially in short-term repo operations, while direct trading decreased, indicating the policy incentivized a "buy-and-hold" strategy by enhancing both directly and indirectly the service flow associated with holding these bonds in the portfolio.

The identification of causal effects leverages the unique eligibility rule based on an internal risk score, which uses data from the credit registry system (SCR). Our strategy is based on comparing the outcomes of firms that are close to the eligibility threshold. However, the dynamic nature of eligibility, with firms' treatment status changing over time, presents a significant challenge in isolating the dynamic causal effects accurately for two reasons: (i) - at a given point in time, firms near a threshold may have different treatment exposures and

[1] Corporate bonds were chosen due to existing regulations and market infrastructure. The policy had wide coverage, with eligible firms representing a significant portion of the corporate bonds market, including unrated bonds. The LFL policy resulted in liquidity injections in the economy, as institutions quickly approached the limits for reserve requirement reductions and borrowing limits in the facility lines in the few months after the start of the policy.

(ii) - treatment today may partially affect treatment status in the future.

To address this challenge, we develop a novel framework for identifying and estimating causal effects in dynamic regression discontinuity designs, where treatment assignment evolves over time based on time-varying scores. Unlike traditional RDDs that assume static treatment assignment, our potential outcomes framework accounts for the influence of treatment duration on outcomes. We establish conditions under which reduced-form RD estimands can be interpreted causally as weighted sums of average marginal effects of exposure to the policy, with non-negative weights, even in the presence of time-varying scores, when the causal impact of the policy may depend on the length of exposure to the policy. Nonetheless, we argue that, even if these conditions are likely to be satisfied in a given empirical setting, the interpretation of the estimand may not be immediate, as weights reflect the margin through which units self-select into treatment on the basis of prior scores. Specifically, if the researcher is interested on the average marginal or cumulative effects of exposure to policy, the reduced-form RD estimand does not recover the parameters of interest. Motivated by this, we outline different strategies to disentangle both the average marginal and average cumulative treatment effects of exposure to policy and introduce an estimation strategy that exploits homogeneity assumptions between populations at different thresholds, while still allowing for nonstationarity and decreasing returns in the exposure to the policy, to construct more efficient estimators of effects. We also propose overidentification and placebo tests to assess the plausibility of our identifying assumptions.

Finally, we develop a semi-structural approach to examine how firm eligibility for the LFL policy affects liquidity injections from financial intermediaries, borrowing costs, and the pass-through to firms' production decisions. The model combines two key features: a segmented financial market and imperfectly elastic demand for firms' debt. We demonstrate that in inelastic financial markets, the reduced-form results imply significant quantity effects of the LFL policy, as opposed to price effects. The quantity effects act like a liquidity injection, in the form of a borrowing subsidy. Using this framework, we estimate the pass-through of liquidity injections to real outcomes, finding that a 1% increase in debt issuance raises the wage bill by 0.4% to 0.75%. This expansion is partially funded by a reduction in internal funds, with cash holdings decreasing by 0.25% to 0.35%, alongside a modest increase in external debt.

Literature Our paper contributes to several strands of the literature. Since the Great Recession, central banks have increasingly turned to operations involving corporate bonds as an additional monetary policy tool. We are the first to estimate the causal effects on real outcomes and firms' capital structure and liquidity management of indirect policies such as the collateral framework.[2] Previous papers have focused mostly on outcomes in the secondary market and a few outcomes in the primary markets of corporate bonds (Fang et al. (2020), Pelizzon et al. (2024)). We are able to do so by leveraging a new empirical setting where we can improve upon both the identification and data, both in terms of availability and frequency, and developing a novel econometric methodology appropriate to our setting. Our estimates of significant effects on firms' labor expenditure and liquidity management contribute to filling the gap noted by Nyborg (2016): even though collateral frameworks are at the core of the monetary systems, they are largely overlooked in academic research, and their far-reaching effects on the real economy are poorly understood. It also provides the first causal evidence that haircut policies, such as those advocated by Geanakoplos (2010), can directly influence the production decision of the underlying asset issuers.

We contribute to the literature on safe assets and convenience yield (Krishnamurthy and Vissing-Jorgensen (2012), Brunnermeier et al. (2022), Mota (2023)) by providing causal estimates on how (i) monetary policy affects the equilibrium service flow of privately issued

[2] A branch of literature focuses on direct corporate bond purchase programs such as the ECB‘s Corporate Sector Purchase Program (CSPP) standing facility lines backed by corporate bonds and the FED's Secondary Market Corporate Credit Facility (SMCCF). A strand of the literature empirically assesses the effects of these policies, pointing out that they boost primary market issuance, lower yields, and create spillover effects that impact both yields and liquidity in the secondary markets (Todorov (2020), Gilchrist et al. (2020), Boyarchenko et al. (2022)). Few papers focus on the effects at the firm level, as a clear method to compare eligible firms with non-eligible ones is lacking in these settings, and the existing results point to null direct real effects (Darmouni and Siani (2022), Adelino et al. (2023)). Our findings contrast with those from this literature.

securities and (ii) how the issuers respond to this increased service flow. In particular, we estimate large quantity effects (as opposed to price effects) and corresponding large real effects. Additionally, our results are related to the literature of monetary policy implementation through segmented markets and its relationship with the market micro-structure of OTC markets (Lagos and Zhang (2020), Eisenschmidt et al. (2024)).

We provide new empirical evidence on the price elasticity of demand for corporate bonds. Our findings contribute to the literature by offering an alternative to the demand system approach (e.g., Koijen and Yogo (2019)), as our estimation strategy is based on the firm's liquidity management decisions. Furthermore, we contribute to the theoretical and empirical literature on the inelasticity of financial markets (e.g., Gabaix and Koijen (2021), Darmouni et al. (2022)), providing a semi-structural approach to quantify the pass-through of interventions in these markets to firms' production decisions.

Finally, from the econometric viewpoint, our paper contributes to the literature on regression discontinuity designs (Cattaneo and Titiunik, 2022). First, we show that, in dynamic settings where eligibility to a policy is determined by a sequence of running variables (scores) at each period, RD estimands that contrast average outcomes at a given period between individuals just above and below the threshold at an *earlier* period may not have a causal interpretation, in the sense that they may not correctly reflect the *sign* of treatment effects even in an extreme setting where there is no heterogeneity in these in the population (Blandhol et al., 2022). The breakdown in this weak notion of causality stems from the fact that, if prior eligibility to the treatment partly determines future scores, and if agents on the threshold at a given period self-select into treatment *in later periods*, then individuals below and above the threshold at a given period may have different exposures (and treatment effects) in the future, which generates a "forbidden comparison" problem akin to those underlying the breakdown of two-way fixed effects estimators in staggered adoption settings (Roth et al., 2023). Moreover, we argue that even if the conditions that ensure a causal interpretation to the RD estimand hold, it is not possible to interpret the parameter directly as a cumulative or marginal effect. We then show that alternative strategies to recover these marginal or cumulative effects available in the literature, such as those considered in the literature of sequential experiments under imperfect compliance (Sojitra and Syrgkanis, 2024) or those specially tailored to specific dynamic RD designs (Cellini et al., 2010; Hsu and Shen, 2021), either impose global independence assumptions that severely limit the dynamic dependence between scores or assume constant marginal returns of exposure to the policy. These assumptions are unlikely to be satisfied in settings such as ours, where scores are tightly linked to past financial decisions of firms and treatment effects reflect firms' production and financing decisions upon exposure to the policy. As an alternative to these approaches, and inspired by a recent literature exploring the identifying power of homogeneity restrictions in cross-sectional RD designs with multi-valued treatments and a single discontinuity (Caetano et al., 2023), we propose to recover marginal and cumulative effects by exploring cross-threshold treatment homogeneity restrictions that, while constraining treatment effect heterogeneity, allow for nonstationarity and nonconstant marginal returns of exposure to the policy. We provide applied researchers with a toolkit for efficient estimation under these restrictions, which includes a minimum distance estimator that efficiently explores available overidentifying restrictions, as well as overidentifying restrictions and placebo tests to assess the plausibility of the imposed assumptions.

Outline This paper is organized as follows: Section 2 describes the institutional setting and the data, focusing on Brazil's Liquidity Facility Lines (LFL) policy and the corporate bond market. Section 3 outlines the identification strategy, using a dynamic regression discontinuity design (RDD) to estimate the causal effects of the LFL policy on firm outcomes. Section 4 presents the empirical results, showing the policy's impact on corporate bond issuance, debt structure, bond costs, and real outcomes such as the wage bill and payment outflows. Section 5 presents our semi-structural model. Finally, Section 6 concludes by summarizing the key findings and their broader implications for monetary policy and liquidity management.

2 Data and Institutional Setting

2.1 Datasets

We have access to data on the BCB's Liquidity Facility Lines (LFL) policy, including daily eligibility, letter grades at the corporate bond and firm levels, and the limits and usage by participating financial institutions. Using the credit operations dataset (SCR), which includes all bank loans, credit lines, collateral-backed loans, and mutual fund-initiated financing (e.g., factoring), we can compute the underlying risk score. This dataset also allows us to construct firm-level outcomes on total outstanding internal and foreign debt, interest rates, and maturity. We exclude operations involving the public development bank (BNDES) from the analysis.

From B3, the central depository, clearinghouse, and trade repository for OTC financial assets in Brazil, we obtain the complete record of electronic transactions involving corporate bonds. This dataset includes all asset movements—transfers, trades in primary and secondary markets, collateral use, and repo operations. It also captures holdings at the final investor and financial institution levels. We supplement this with additional data from the Brazilian Financial and Capital Markets Association (ANBIMA) and B3, providing registry details on corporate bonds, such as issuer, underwriter, issuance spread, volume, and daily outstanding quantity and (nominal) value. Finally, with data from the Brazilian Securities and Exchange Commission (CVM) and the tax registry we map mutual funds and other companies (such as insurance companies) ownership and management structure.

The Annual Report of Social Information (RAIS) is a mandatory annual submission for all formal employers in Brazil, covering all registered employment relationships, including employees with formal contracts. RAIS provides detailed employee-level data, such as employment status, job tenure, and wages. We use this dataset to construct firm-level information on the number of workers and the total wage bill.

The electronic payments dataset includes all payments associated with invoices (boletos), usually via bank transfer, as well as all interbank transfers and instant payments (PIX). In these datasets, we observe identifiers for creditor and debtor, as well as the time stamp of when the transaction is settled.

Finally, we measure firm-level cash-like holdings, which include term deposits, treasury bonds, money market mutual fund shares, and short-term repo operations (with resale obligations) involving public and private assets. For term deposits, we aggregate data from electronic records of the major registration and settlement platforms (B3, CRT4, CERC, and CSDBR), covering nearly all such deposits. Treasury holdings and treasury-backed repo operations data are sourced from the Special System for Settlement and Custody (SELIC), the centralized system for registration, settlement, and custody of federal government bonds. Data on private asset-backed repo operations comes from B3's electronic records.

2.2 Liquidity Facility Lines (LFL)

The BCB Liquidity Facility Lines (LFL), launched in November 2021, are tools to provide liquidity to financial institutions using corporate bonds as collateral. From 2000 to 2021, BCB's liquidity provisions were limited to short-term operations involving Treasury securities. The policy followed a recommendation from IMF (2012) to introduce systems for the quick acceptance of collateral beyond government securities, including valuation and haircuts; pre-determine, periodically review, and regularly disseminate haircuts for asset classes to reduce uncertainty. Corporate bonds were selected due to pre-existing regulations and financial market infrastructure requiring centralized registry, custody, and settlement of these securities.[3]

Brazil's corporate bonds (measured by outstanding debt volume) represent roughly 10% of GDP and 20% of total credit to nonfinancial corporations, making them the largest private

[3]See section IV of IMF (2012) and Vote 146/2021-BCB for a detailed exposition of the rationale behind the policy.

fixed-income security in the country and surpassing mortgage-backed securities in volume. This contrasts with the U.S., where corporate bonds account for about 40% of GDP and 42% of credit to nonfinancial firms. In Brazil, roughly half of large firms' domestic debt is financed through corporate bonds, and these issuers collectively employ 12% of formal workers while accounting for 15% of payment outflows. The instruments are typically unsecured, with an average spread of the baseline interest rate plus 3% and an average maturity of seven years. Despite their importance, the majority of corporate bonds are neither traded nor externally rated, although they are frequently used as collateral in over-the-counter markets.[4] Holdings are concentrated among the largest commercial banks (44%) and mutual or pension funds (50%). For a more detailed description of the Brazilian corporate bond market, refer to Appendix H.

Each participating financial institution has a LFL limit based on the total amount of eligible corporate bonds held in a special account in the central depository. These limits can be used for borrowing on short and medium-term lines (up to 45 days and 1 year, respectively), and to reduce reserve requirements in June 2024, the BCB mandates depository institutions to hold 20% of term and demand deposits in a BCB account. The LFL limit can reduce term deposit reserve requirements by up to 15% (3 percentage points). [5] Collateral eligibility is defined and disclosed daily using an internal risk model, we will discuss the eligibility criteria in Section 2.3 as it is central to our identification strategy for the causal effects of the LFL on firms.

The policy had broad coverage, with eligible firms representing 9.5% of the formal workers' wage bill and around 40% of long-term corporate bonds (and 50% of short-term bonds) being eligible by the end of 2022 as shown in Figure 9. Eligible firms were among the largest corporate bond issuers, with an average of 6,000 workers. Due to internal risk scoring, there

[4]The typical use as collateral occurs in short-term repo operations, in which commercial banks sell corporate bonds (with a repurchase obligation) to nonfinancial firms. These transactions provide both liquidity benefits and tax advantages, as they are exempt from the financial transaction tax that applies to term deposits.

[5]Term deposits make up about 80% of total deposits. For further details, see the complete list of requirements.

was no requirement for bonds to be rated as investment grade by major rating agencies, significantly expanding eligibility since most bonds in Brazil are unrated. This unique feature of the LFL resulted in broader economic coverage compared to typical corporate bond interventions by the Federal Reserve and European Central Bank.

Credit limits are determined by the collateral pool price, discounted by risk and diversification haircuts, and are updated promptly after assets are pre-positioned or withdrawn from the pledge account. Events such as interest payments and amortizations are transferred to a cash pledge account at the BCB, which is accessible by the participating financial institution. We postpone the details on collateral eligibility to the next subsection. [6]

Figure 2 shows that financial institutions participating in the policy (those with a positive limit at any point) approached the limit on long-term lines within one year of the policy's implementation. [7] Together, these results indicate a direct liquidity injection into the economy as a result of the policy.

2.3 Collateral Eligibility, Risk Score and Causal Effects

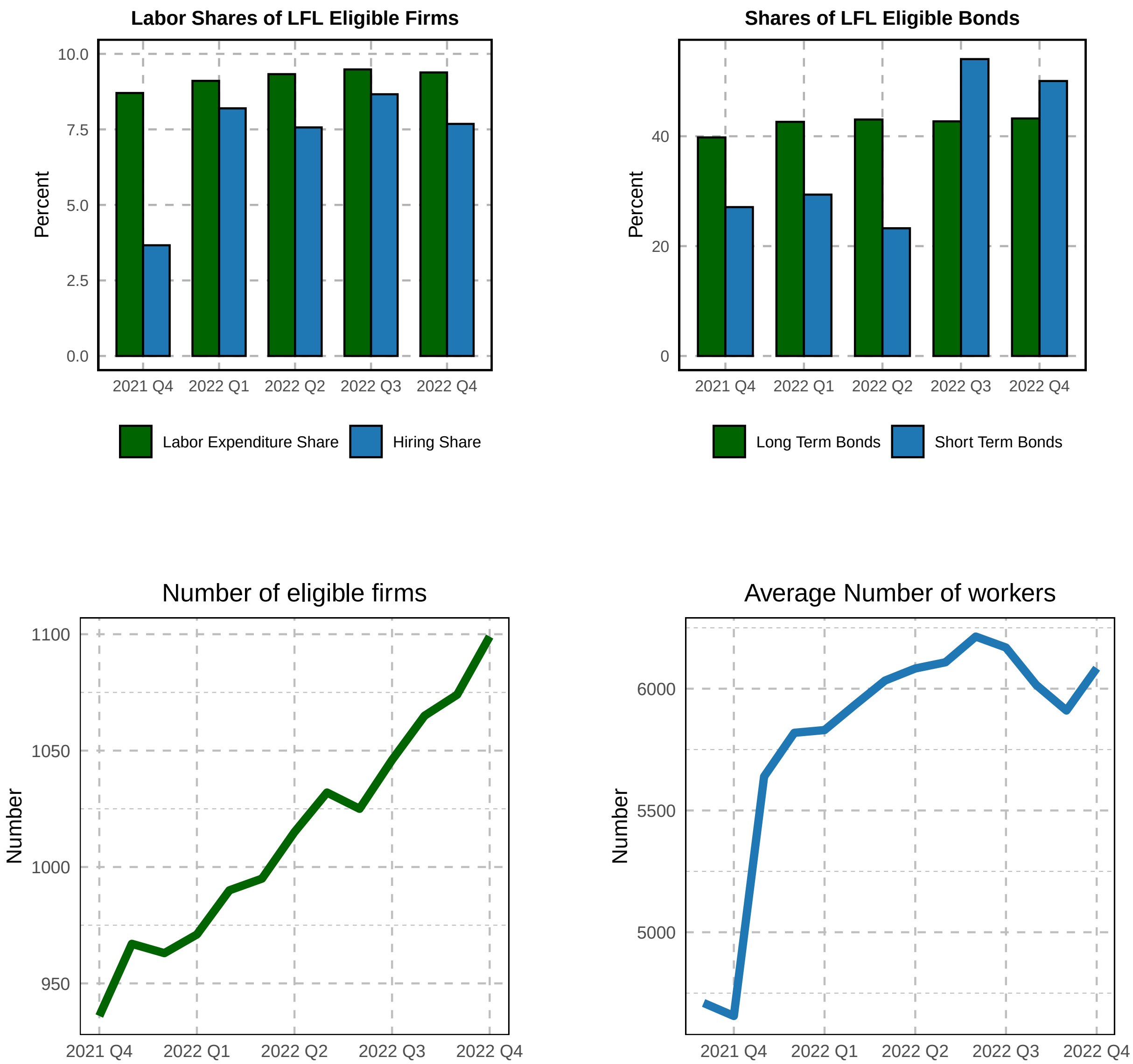
Eligibility requires the issuer to be a non-financial firm with a low-risk score according to an internal risk model. Accepted collateral includes short and long-term corporate bonds issued in local currency by eligible firms. and the payoff structure is not too complex. The first step in determining eligibility is mapping the continuous risk score to a letter grade. Table 1 shows the relationship between risk score, letter grade, and eligibility. Notably, bonds issued by firms with a letter grade of B are subject to limits on long-term lines and reduction in reserves but are ineligible for short-term lines and receive a higher haircut. Our identification strategy exploits the sharp discontinuity in eligibility around the corresponding letter grade of B.

The risk score is based on a mechanical rule using exclusive data from credit operations

[6] A complete description of the haircut applied to each corporate bond can be found in Annex I of Vote 146/2021-BCB

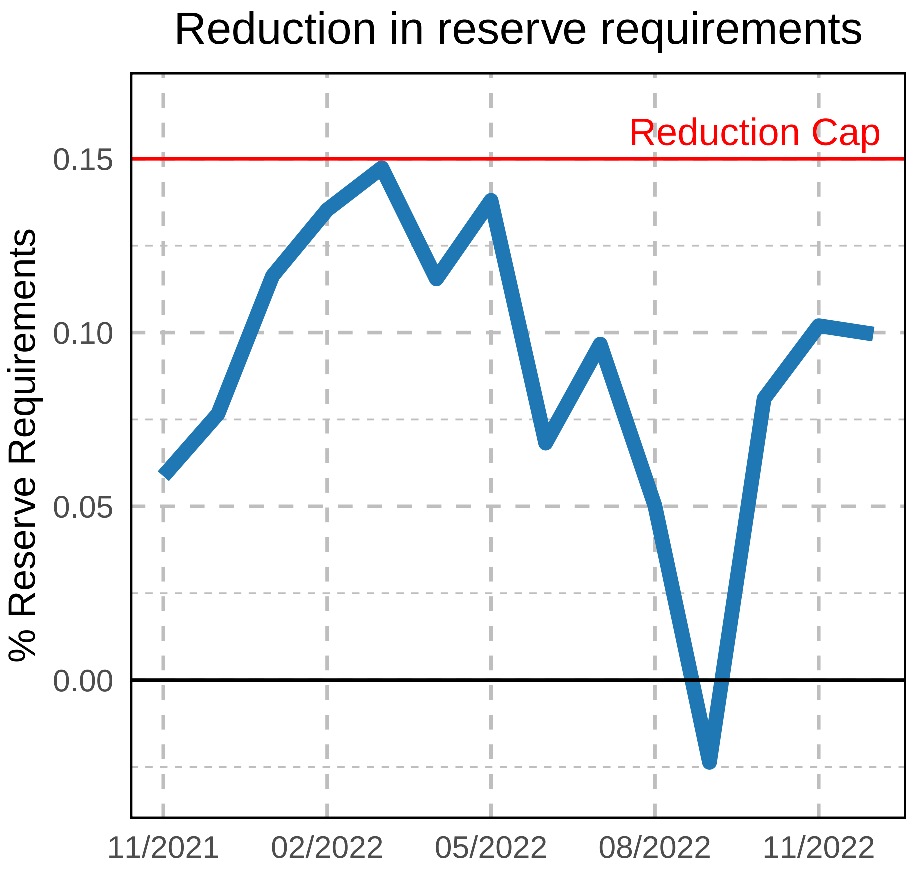
[7] The Liquidity Facility Line (LLT) window has been closed since March 2022, and the operations that were still open were concluded in March 2023.

Figure 1: LFL Eligible Firms.



Note: The graph displays four panels showing key metrics for LFL-eligible firms and bonds from Q4 2021 to Q4 2022. The labor share reflects the wage bill for formal workers in the last month of each quarter, while the hiring share represents the wage bill for newly hired workers during the quarter. Bond shares indicate the percentage of eligible corporate bonds by type each quarter. Data sources: B3 (Short Term Bonds), ANBIMA (Long Term Bonds), RAIS (wage bill and number of workers), and BCB - Department of Banking Operations and Payment Systems (LFL Eligibility).

Figure 2: LFL Take-up by Participants.



Note: The graph shows participants' institutions' take-up of Liquidity Facility Lines. The figure shows the reduction in reserve requirements as a percentage of the maximum possible reduction, netting out voluntary deposits held in the BCB. Data Sources: BCB - Department of Banking Operations and Payment Systems (LFL data, reserve requirements), BCB - Department of Open Market Operations (Voluntary Deposits).

reported by lenders in the credit registry SCR. This score is based on a weighted average of lenders' reported Allowance for Loan and Lease Losses (ALLL) for selected credit categories.[8] Importantly, this score is used exclusively for the LFL policy. The score, the SCR data, and the exact details of its computation are not public information; only an indicator of firm (and asset) eligibility is public information.

The final risk score is calculated daily and is the maximum among the credit risk scores computed using (i) the most recent month's data available in the Credit Information System (SCR), (ii) the average of the last three months' data available in the SCR, or (iii) the average of the last six months' data available in the SCR. Firms without outstanding credit operations in the SCR (including corporate bonds) are not eligible. Based on the BCB's internal score calculation rules and SCR data, we compute the score for the last business day of each month. Therefore, our analysis uses a monthly frequency, with eligibility determined by the status on the last day of each month.

Table 1: LFL Eligibility Rule

Letter	**LB (>)**	**UB (≤)**	**Haircut (%)**
AA	0	0	16%
A	0.00%	0.50%	23%
B	**0.50%**	**1.00%**	**39%**
C	**1.00%**	**3.00%**	**100%**
D	3.00%	10.00%	100%
E	10.00%	30.00%	100%
F	30.00%	50.00%	100%
G	50.00%	70.00%	100%
H	70.00%	100.00%	100%

Figure 3 shows the distribution of the score shows the score distribution of non-financial firms from November 2021 to December 2022. To visualize the score distribution, we normalize the scores as follows: firms with ratings A and B have scores normalized between 0 and 100, while firms with ratings C and D have scores normalized between -100 and 0, so

[8] Certain credit operations are excluded from the risk score calculation. These include the cession of credit rights from financial applications, operations with real estate or vehicles as collateral backed by the Federal Government Treasury, insurance, and third-party coverage.The description of the score computation can be found in section III of Chapter IX of Vote 146/2021-BCB

formalized the normalized score of firm i at time t, Z_{it}, is given by

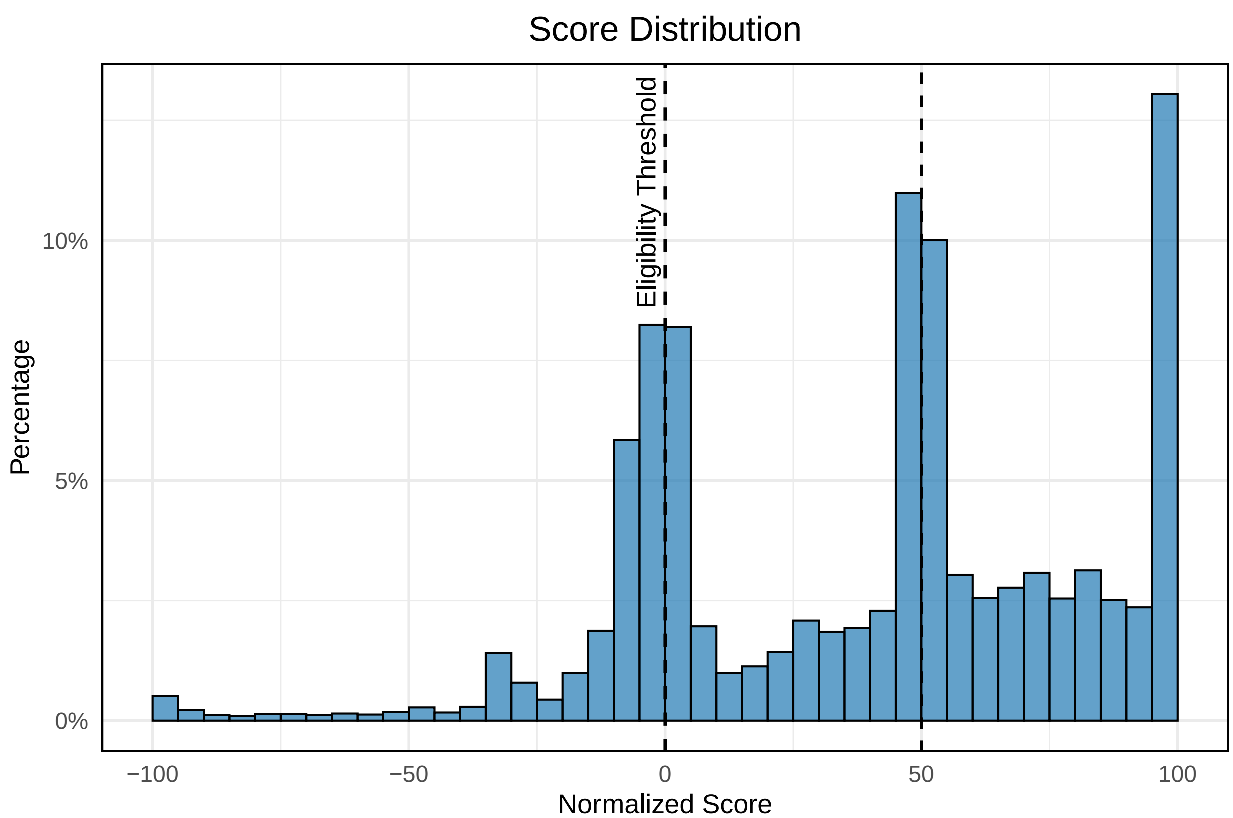
$$Z_{it} = \begin{cases} 100 \times (1 - \mathsf{Score}_{it}) & \text{if } \mathsf{Score}_{it} \in [0, 1] \\ -100 \times \frac{\mathsf{Score}_{it} - 1}{9} & \text{if } \mathsf{Score}_{it} \in [1, 10] \end{cases}.$$

The key observation from Figure 3 is the concentration of firms around the eligibility threshold. This occurs because, for a given credit operation, the score can only be reported in pre-specified discrete numbers that match the letter rating thresholds shown in Table 1. This concentration indicates that, aside from the effect of LFL eligibility, firms just above and below the threshold would be comparable. Any manipulation of the score is unlikely, as the score and its underlying data are not public, and financial institutions are mandated to properly document their credit score classification policies and procedures, making them available to the BCB and independent auditors.

We formally test both claims. Figures 4 and 5 show summary statistics for pre-policy outcomes for firms near the eligibility threshold when the policy was introduced in November 2021. The plots include fitted lines or curves on either side of the cutoff, showing no clear discontinuity in any outcome variable. Second, Table 3 shows the results of the manipulation test proposed by Cattaneo et al. (2020). The null hypothesis of no manipulation is rejected in nearly all periods.

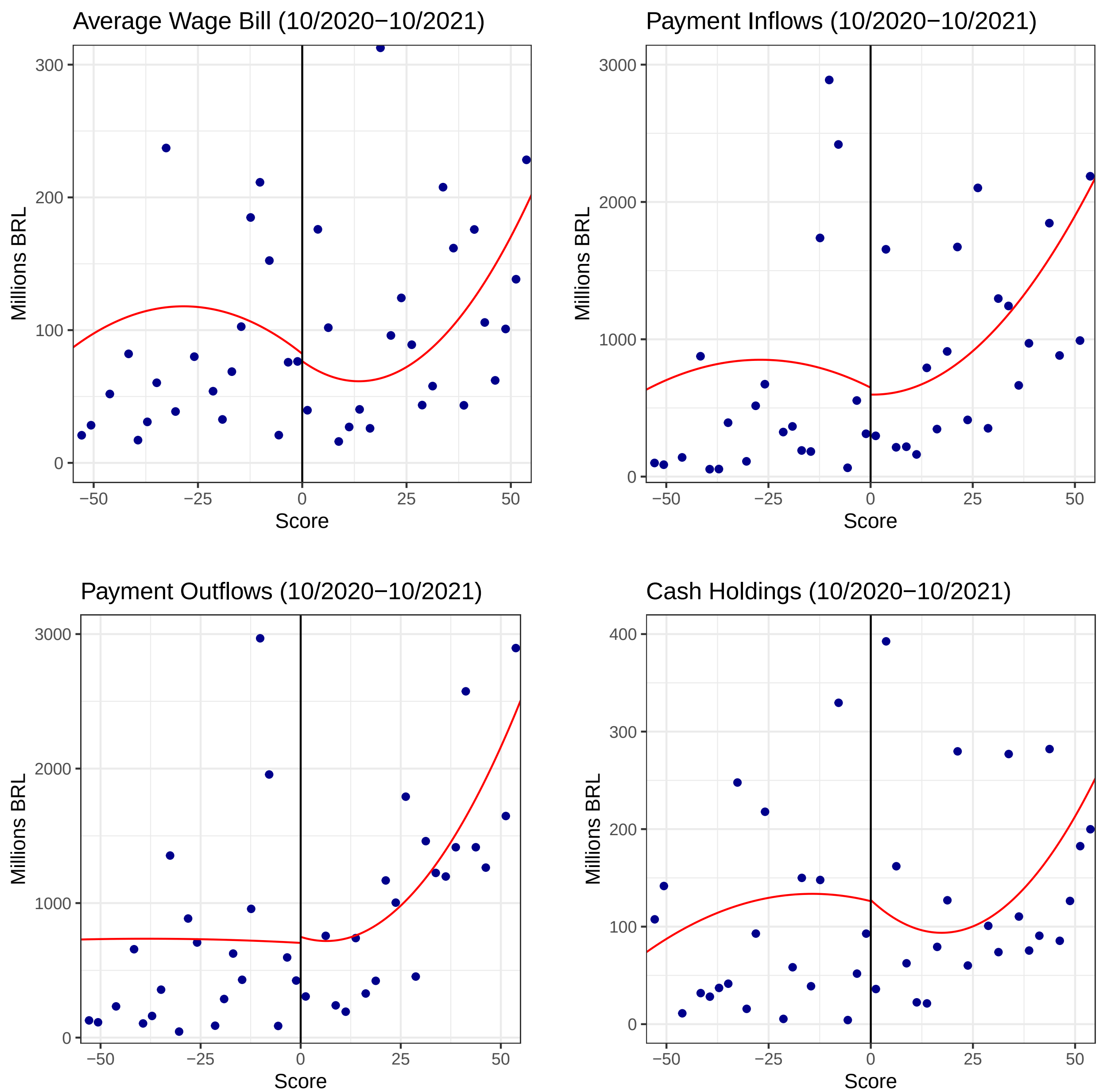
The previous results suggest that a Regression Discontinuity Design (RDD) could uncover the causal effects of the LFL policy, as units near the threshold are comparable before the policy starts. However, the dynamic nature of eligibility, with firms' treatment status changing over time, presents a challenge . The next section discusses identification in this dynamic RDD and introduces a methodology to estimate the dynamic causal effects of the LFL policy.

Figure 3: LFL Score Distribution (2021-2022).



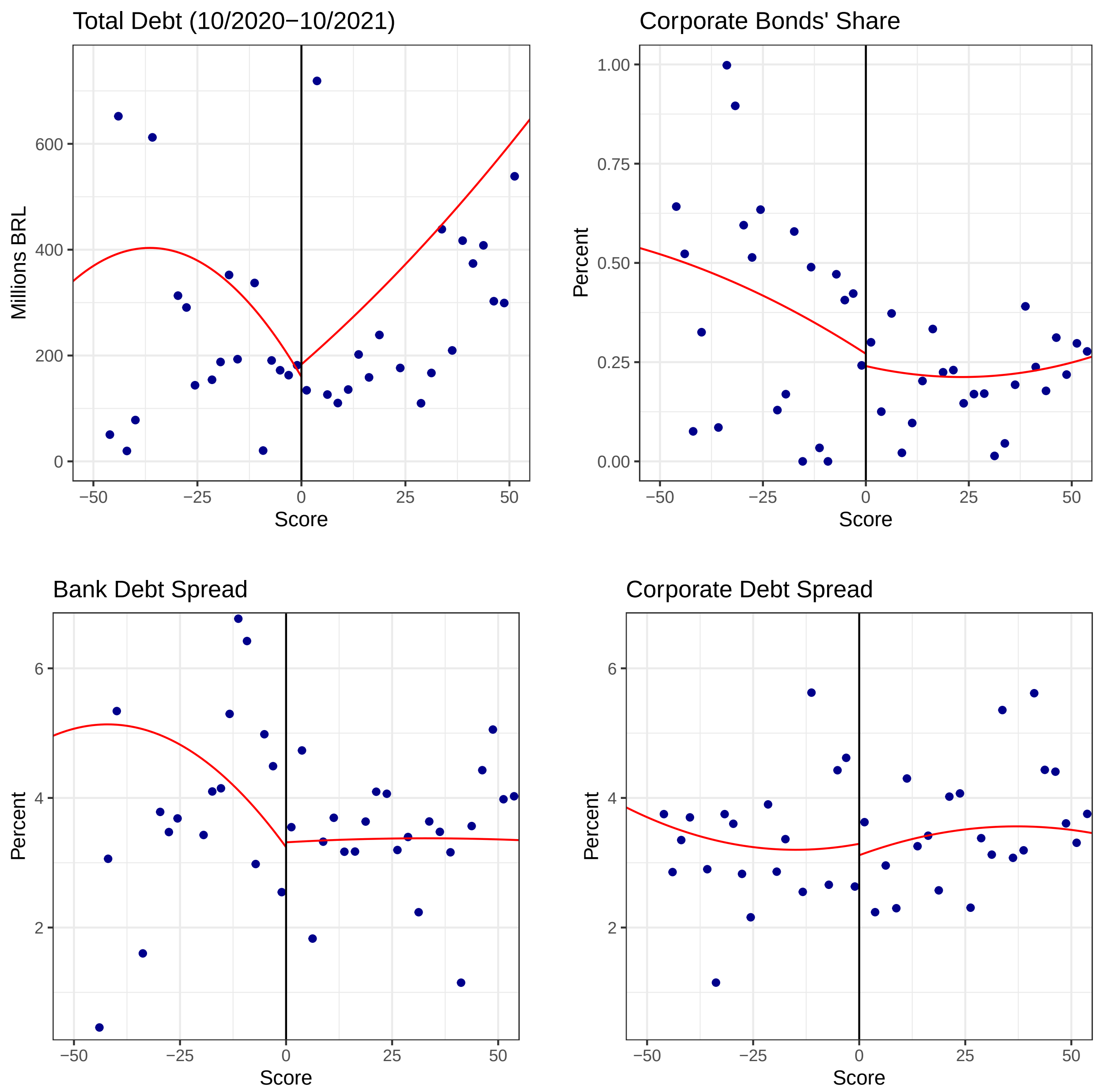
Note: The figure plots the distribution of the normalized score Z_{it} pooling the periods from November 2021 to December 2022. The sample consists of non-financial firms that are considered potential eligible firms in the LFL.

Figure 4: Summary Statistics of firms near the LFL threshold in November 2021.



Note: The graph summarizes firm statistics around the threshold. We fit a second-order polynomial on each side, using 20 equal-sized bins per side.

Figure 5: Summary Statistics of firms near the LFL threshold in November 2021.



Note: The graph summarizes firm statistics around the threshold. We fit a second-order polynomial on each side, using 20 equal-sized bins per side.

3 Identification and Estimation of Causal Effects in a Dynamic Regression Discontinuity Design

3.1 Setup

In this section, we introduce a framework to understand the causal content of regression discontinuity estimands, in a setting where treatment assignment is (partially) determined by a sequence of running variables at each time period. In the LFL policy eligibility to the program is determined by a score that is computed every month.

We consider a population of interest, and a policy intervention that begins at period $t = 1$. The sequence of outcomes of a unit in the population of interest is denoted by $(Y_t)_{t=1}^T$. We view $(Y_t)_{t=1}^T$ as a vector of random variables, defined in a common probability space $(\Omega, \mathcal{F}, \mathbb{P})$, capturing the fact that paths in observed outcomes are heterogeneous in the population of interest.

We assume that the length of exposure to the policy may exert a causal effect on the outcome. Denote by D_t the length of exposure of a unit in the population by time period t. Thus, D_t is a random variable taking values on $\{0, 1, \ldots, t\}$ and $t \mapsto D_t$ is a nondecreasing random function. We posit that, for every $t = 1, \ldots, T$, outcomes Y_t are generated according to:

$$Y_t = Y_t(0) + \sum_{j=1}^{t} \mathbf{1}\{D_t \geq j\}(Y_t(j) - Y_{t-1}(j)) ,$$

where $\{Y_t(j) : j = 0, 1, \ldots, t\}$ are the potential outcomes associated with being exposed to the policy for j periods, by period t.

We assume that exposure to the policy is (partly) determined by a sequence of continuous running variables or *scores* Z_t, $t = 1, \ldots, T$. Specifically, we assume that $D_t = \boldsymbol{D_t}(Z_1, \ldots, Z_t)$, with eligibility being affected by the Z_l crossing a threshold, which we normalize to zero. In the LFL example, the map $\boldsymbol{D_t}$ takes a simple homogeneous and additive form, $\boldsymbol{D_t}(Z_1, \ldots, Z_t) = \sum_{j=1}^{t} \mathbf{1}\{Z_j \geq 0\}$, though we consider more general potential treatment rules in our results.

3.2 What does RDD identify?

Our first goal is to understand under which interpretable conditions does a "reduced-form" RD estimand of the type:

$$\tau_{t|l} = \lim_{s \downarrow 0} \mathbb{E}[Y_t | Z_l = s] - \lim_{s \uparrow 0} \mathbb{E}[Y_t | Z_l = s], \quad t, s \in \{1, 2, \ldots, T\} \tag{1}$$

have a causal interpretation. Estimand (1) is an average contrast of the outcome of interest in period t, between units just below and above the threshold at period l. We assume the following conditions for our dynamic RD design:

Assumption 1 (Existence of limits)**.** *For each $t, l \in \{1, \ldots, T\}$, let $\boldsymbol{Z}_{t|l}(z) = (Z_1, \ldots, Z_{l-1}, z, \ldots Z_t)$ if $l \leq t$ and $\boldsymbol{Z}_{t|l} = (Z_1, Z_2, \ldots, Z_t)$ otherwise. We assume that, for each $t, l, j \in \{1, \ldots, T\}$ and $j \leq t$, the following limits exist:*

$$\phi^{+}_{t|l}(j) \coloneqq \lim_{s \downarrow 0} \mathbb{E}[Y_t(j) - Y_t(j-1) | D_t \geq j, Z_l = s] = \lim_{s \downarrow 0} \mathbb{E}[Y_t(j) - Y_t(j-1) | \boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(s)) \geq j, Z_l = s]$$

$$\phi^{-}_{t|l}(j) \coloneqq \lim_{s \uparrow 0} \mathbb{E}[Y_t(j) - Y_t(j-1) | D_t \geq j, Z_l = s] = \lim_{s \uparrow 0} \mathbb{E}[Y_t(j) - Y_t(j-1) | \boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(s)) \geq j, Z_l = s]$$

$$\omega^{+}_{t|l}(j) \coloneqq \lim_{s \downarrow 0} \mathbb{P}[D_t \geq j | Z_l = s] = \lim_{s \downarrow 0} \mathbb{P}[\boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(s)) \geq j | Z_l = s]$$

$$\omega^{-}_{t|l}(j) \coloneqq \lim_{s \uparrow 0} \mathbb{P}[D_t \geq j | Z_l = s] = \lim_{s \uparrow 0} \mathbb{P}[\boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(s)) \geq j | Z_l = s]$$

Assumption 2 (Local monotonicity of treatment rule)**.** *For each t and l, with $l \leq t$, there exists $\check{\epsilon} > 0$ such that, for every z, z' with $|z| \vee |z| \leq \check{\epsilon}$, $z \leq z' \implies \mathbb{P}[\boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(z)) \leq \boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(z'))] = 1$*

Assumption 3 (Local experiment around threshold)**.** *For every $l \in \{1, 2, \ldots, n\}$, there exists some $\bar{\epsilon} > 0$ such that, on $|Z_l| \leq \bar{\epsilon}$*

$$(\{(Y_t(j))_{j=0}^t, \boldsymbol{D}_t\}_{t=1}^T, (Z_t)_{t=1}^{l-1}) \text{ is independent of } Z_l\,.$$

Assumption 1 is a technical requirement ensuring that (1) exists. Assumption 2 is a local monotonicity assumption: it implies that, on a neighborhood of the threshold, externally increasing the score should almost surely not decrease treatment dosage. Assumption 3 is our main identification condition. It is a local independence condition that ensures a "local experiment" around the threshold.[9] This assumption ensures that, for every period l, predetermined traits – namely, potential outcomes, potential treatment functions, and the scores *prior* to l –are balanced at either side of the threshold at l. The balance condition captures the notion that, at a given period, units around the threshold do not have perfect control over their score (Lee and Lemieux, 2010). Note that the Assumption does not impose balance of scores after the reference period l, as it may well be that being above or below the threshold at l partly determines the score after l – in the LFL application, being eligible to the policy at l may increase credit scores in future periods.

Having defined our setup, we are now able to precise in which sense we require $\tau_{t|l}$ to have a causal interpretation. We adapt the notion of "weak causality" of Blandhol et al. (2022) to our setting.

Definition 1 (Weak Causality)**.** $\tau_{t|l}$ is a weakly causal parameter if, for any vector of random

[9] In the main text, we opt to state the identification assumption in terms of a local independence condition (as in, e.g., Hahn et al., 2001), instead of relying on a local continuity condition (as in, e.g., Dong, 2018; Arai et al., 2022). While the latter is known to be weaker than the former (Cattaneo and Titiunik, 2022), deriving our representation in Proposition 1 under this form of restriction would require, in our dynamic setup, to introduce additional notation that would burden the interpretation of the identification condition, while keeping the main message of the representation result intact.

variables defining potential outcomes that satisfy Assumptions 1, 2 and 3 such that $\mathbb{P}[Y_t(j) - Y_t(j-1) \geq 0] = 1$ for every $j = 1 \ldots, t$, one has $\tau_{t|l} \geq 0$. Similarly, if $\mathbb{P}[Y_t(j) - Y_t(j-1) \leq 0] = 1$ for every $j = 1 \ldots, t$, then one has that $\tau_{t|l} \leq 0$

Our notion of weak causality requires the sign of the estimand to correctly reflect a scenario where marginal effects $Y_t(j) - Y_t(j-1)$ share almost-surely the same sign.[10]

The next result characterizes the estimands $\tau_{t|l}$ under our set of assumptions.

Proposition 1. *(LATE Representation) Suppose Assumptions 1, 2, and 3 hold. We then have that:*

1. *If $l > t$, $\tau_{t|l} = 0$.*
2. *If $l = t$,*
$$\tau_{t|l} = \sum_{j=1}^{t} \omega_{t|l}(j)\phi_{t|l}(j)\,, \tag{2}$$
where
$$\omega_{t|l}(j) = \lim_{\epsilon \downarrow 0} \mathbb{P}[\boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(\epsilon)) \geq j, \boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(-\epsilon)) < j||Z_l| \leq \epsilon]$$
$$\phi_{t|l}(j) = \lim_{\epsilon \downarrow 0} \mathbb{E}[Y_t(j) - Y_t(j-1)|\boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(\epsilon)) \geq j, \boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(-\epsilon)) < j, |Z_l| \leq \epsilon]$$
In this case, the estimand is weakly causal, and the weights $\omega_{t|l}(j)$ are identified as the first-stage contrasts $\omega^{+}_{t|l}(j) - \omega^{-}_{t|l}(j)$.
3. *If $t > l$*

[10] Our notion of weak causality is less demanding than Blandhol et al.'s, as they require their parameter under study to accurately capture the sign of *average* marginal effects across predefined complier populations when the signs of average effects in these groups are all the same. In contrast, we only require our target parameter to accurately capture the signs in an extreme scenario where there is not any sign heterogeneity in treatment effects.

$$\begin{aligned}\tau_{t|l} = \sum_{j=1}^{t} \omega_{t|l}(j)\phi_{t|l}(j) + \\ (\omega^{+}_{t|l}(j) - \omega^{\downarrow}_{t|l}(j))\phi^{\downarrow}_{t|l}(j) + \omega^{\downarrow}_{t|l}(j)(\phi^{+}_{t|l}(j) - \phi^{\downarrow}_{t|l}(j)) + \\ (\omega^{\uparrow}_{t|l}(j) - \omega^{-}_{t|l}(j))\phi^{-}_{t|l}(j) + \omega^{\uparrow}_{t|l}(j)(\phi^{\uparrow}_{t|l}(j) - \phi^{-}_{t|l}(j)),\end{aligned} \tag{3}$$

where $\omega^{\downarrow}_{t|l}(j) = \lim_{\epsilon\downarrow 0} \mathbb{P}[\boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(\epsilon)) \geq j | Z_l = \epsilon]$, $\phi^{\downarrow}_{t|l}(j) = \lim_{\epsilon\downarrow 0} \mathbb{E}[Y_t(j) - Y_t(j-1) | \boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(\epsilon)) \geq j, Z_l = \epsilon]$, $\omega^{\uparrow}_{t|l}(j) = \lim_{\epsilon\uparrow 0} \mathbb{P}[\boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(\epsilon)) \geq j | Z_l = \epsilon]$ *and* $\phi^{\uparrow}_{t|l}(j) = \lim_{\epsilon\uparrow 0} \mathbb{E}[Y_t(j) - Y_t(j-1) | \boldsymbol{D}_t(\boldsymbol{Z}_{t|l}(\epsilon)) \geq j, Z_l = \epsilon]$. *The estimand is not weakly causal.*

Proof. See Appendix B.1. □

Proposition 1 clarifies under which conditions the parameter $\tau_{t|l}$ has a weakly causal interpretation. There are three main cases. When one compares outcomes determined prior to a given period, for units just below and above that given period's threshold, then, as expected, the RD estimand is zero. This is due to the fact that, in the RD design, predetermined traits are exactly balanced at a given period's threshold.

In the second case, when both the score and the outcome of interest are in the same period, we have a similar result to static fuzzy RD designs. In this case, the reduced-form RD estimand identifies a weighted-average of complier treatment effects, where a unit in the population is defined as a complier with respect to a length of exposure j if her treatment changes from strictly below j to weakly above it when her score is perturbed from a value marginally below the threshold to a value marginally above it. The parameter aggregates each marginal treatment effect $Y_t(j) - Y_t(j-1)$ at each complier subpopulation with respect to each level of exposure j.

Finally, in the third case, when $l < t$, we have a setting where the parameter is not weakly causal. This is due to the dynamic nature of the running variable, and the possibility of being above or below the threshold at l discontinuously affecting future scores. In this case, the comparison underlying the RD estimand need not be causal, as the differences between outcomes just below and above the threshold at l may be effectively comparing units with different exposures to the treatment and that self-select into these exposures according to their expected gains. The effects of differential treatment intensity are captured by the terms $(\omega^{+}_{t|l}(j)-\omega^{\downarrow}_{t|l}(j))\phi^{\downarrow}_{t|l}(j)$ and $(\omega^{\uparrow}_{t|l}(j)-\omega^{-}_{t|l}(j))\phi^{-}_{t|l}(j)$, which measure how the probability of having an exposure of at least j by period t is affected by being below or above the threshold at l *through its effect over future scores*. The "direct" effect of being above the threshold at l does not enter these quantities, as the score entering the potential treatment function in the comparisons $\omega^{+}_{t|l}(j)-\omega^{\downarrow}_{t|l}(j)$ and $\omega^{+}_{t|l}(j)-\omega^{\downarrow}_{t|l}(j)$ is fixed. Differential exposure to the treatment violates weak causality because the parameter may reflect an individual's *margin of choice over treatment* rather than the actual effects of the policy over outcomes. For example, if there is not any treatment effect heterogeneity, so that there exists constants $\{\delta_j\}_{j=1}^{t}$ such that $Y_t(j)-Y_{t-j}(j-1)=\delta_j$ a.s., for every j, then:

$$\tau_{t|l}=\sum_{j=1}^{t}(\omega^{+}_{t|l}(j)-\omega^{-}_{t|l}(j))\delta_j\,.$$

In this case, if $\omega^{+}_{t|l}(j)<\omega^{-}_{t|l}(j)$ for some j, so that being marginally above the threshold at l generates a "compensation" effect over future scores that decreases the likelihood of having an exposure of at least j by period t – for example, in the LFL example, if the policy induces excessive risk-taking, thus increasing the probability of default over future periods –, then $\tau_{t|l}$ need not accurately reflect the sign of treatment effects, even when there is not any heterogeneity in these.

The effects of self-selection into the gains of treatment are captured by the terms $\omega^{\downarrow}_{t|l}(j)(\phi^{+}_{t|l}(j)-\phi^{\downarrow}_{t|l}(j))$ and $\omega^{\uparrow}_{t|l}(j)(\phi^{\uparrow}_{t|l}(j)-\phi^{-}_{t|l}(j))$. Its impacts over the parameter are similar to the effect of differential treatment intensity. Specifically, if units just above the threshold at l with exposure by period t of at least j have expected effects different than units that would also have exposure of at least j by t if the score at l entering the potential treatment function were externally fixed just above the threshold, but that in practice experimented a score slightly below the threshold, then the difference $\phi^{+}_{t|l}(j)-\phi^{\downarrow}_{t|l}(j)$ may be such that the parameter does not need accurately reflect the sign of treatment effect heterogeneity. Once again, the difference between $\phi^{+}_{t|l}(j)$ and $\phi^{\downarrow}_{t|l}(j)$ arises because being below and above a threshold at l may generate discontinuous effects over future scores, which generates a potential margin of self-selection into the gains of treatment as individuals with higher scores at l may have more or less "options" in the future and may self-select into these according to their expectations regarding $Y_t(j) - Y_t(j-1)$.

The following corollary provides one necessary and one sufficient restriction for weak causality to hold when $l < t$.

Corollary 1. *If $l < t$, a necessary condition for weak causality to hold is that $\omega^{+}_{t|l}(j) \geq \omega^{-}_{t|l}(j)$ for every $j = 1, \ldots t$. This condition is implied by $\lim_{\epsilon \downarrow 0} Z_{l+1}, Z_{l+2}, \ldots, Z_t | Z_l = \epsilon$ dominating $\lim_{\epsilon \uparrow 0} Z_{l+1}, Z_{l+2}, \ldots, Z_t | Z_l = \epsilon$ in first-order stochastic dominance. Moreover, a sufficient condition for weak causality is that the environment be such that $\omega^{+}_{t|l}(j) \geq \omega^{-}_{t|l}(j)$ and $\phi^{+}_{t|l}(j) = \phi^{-}_{t|l}(j)$ for every j. In this case, we can write:*

$$\tau_{t|l} = \sum_{t=1}^{l} (\omega^{+}_{t|l}(j) - \omega^{-}_{t|l}(j)) \phi^{+}_{t|l}(j) \,.$$

The corollary shows that, for $\tau_{t|l}$ to be weakly causal, it must be that the *overall effect* of being slightly above the threshold at l, on the probability of exposure being at least j, must be nonnegative, for every $j = 1, \ldots, t$. This condition is implied by being slightly above the threshold at l increasing the likelihood of observing higher scores in future periods. Finally, if we also restrict treatment effect heterogeneity in the environment, so that the average effects among individuals marginally below the threshold at l that choose an exposure of at least j by t is equal to the average effect among individuals marginally *above* the threshold at period l and that also choose an exposure of at least j by t, then we also have a weakly causal parameter.

3.3 Aggregation strategy

The previous section provided conditions for a contrast of average outcomes below and above a given period's threshold to have a weakly causal interpretation. We have seen that, unless $l \geq t$, one requires additional conditions restricting differential treatment exposure and self-selection on expected gains in order for the parameter to accurately capture the sign of effects. Weak causality of $\tau_{t|l}$ may not be sufficient for analysing the effects of a policy, though. For example, if one is interested in dynamics, it may be important to identify averages of the *marginal effects*, $Y_t(j) - Y_t(j-1)$, $t = 1, \ldots, t$, $j = 1, \ldots, t$; or a path of averages of the *cummulative* effects $Y_t(t) - Y_t(0)$, $t = 1, \ldots, T$.

We consider how restrictions on treatment effect heterogeneity may enable the identification of average marginal effects (average cummulative effects). Our approach is inspired by Caetano et al. (2023), who consider the identifying power of homogeneity restrictions in cross-sectional RD designs with multi-valued treatments and a single discontinuity. We view homogeneity assumptions as the natural type of restriction in our setup, since the interpretation of the "building-block" RD estimand $\tau_{t|l}$ when $l < t$ already necessitates this type restriction. Moreover, existing alternatives in the literature are less appealing in our setting. For example, Sojitra and Syrgkanis (2024) provide identification results for LATEs in a dynamic setting where a vector of instruments is sequentially randomized. It would be difficult to leverage their instruments in our case, because running variables are only *locally* sequentially randomized, and restricting our analysis to the subpopulation that is *always* around the threshold could generate a selection bias, insofar as past scores determine future ones. Even if one were to disregard this type of bias, however, we emphasize that, for estimation purposes, this approach would suffer from a curse of dimensionality, as the number of available observations would rapidly deteriorate as a function of T, for a given bandwidth around scores.

In Appendix B.2, we also compare our approach to other alternatives especially tailored to dynamic RD settings. Appendix B.2.1 shows that, if we couple a below-and-above threshold homogeneity condition, $\phi^{+}_{t|l}(j) = \phi^{-}_{t|l}(j)$, with a time-stationarity assumption on marginal effects, it is possible to recursively identify the full path of marginal effects. While appealing, this assumption is not well-suited to settings where there is aggregate trends or volatility in economic conditions, since in this case one would expect the effect of first entering the policy to vary across time. One expects that to be the case in our empirical application, where the effects of the policy should vary with the macroeconomic environment. In Appendix B.2.2, we show that, by conditioning on subpopulations defined by past-period exposures, it is possible to point-identify, in the subpopulation with prior exposure equal to $t-1$, the average complier marginal effects of increasing exposure from $t-1$ to t *at period* t without additional homogeneity assumptions. However, as we argue in the Appendix, this approach suffers from a curse of dimensionality, becoming considerably more demanding of the data as one entails higher values of t; and would be generally inefficient when interest lies on cummulative effects. Moreover, even if estimation was not a concern, we show that the effects identified by this strategy in our empirical application concern a complier subpopulation that is expected to be the least affected by the policy, which is also unappealing on conceptual grounds. Finally, B.2.3 discusses alternative approaches to identification available in the literature that were designed with specific dynamic RD settings in mind (Cellini et al., 2010; Hsu and Shen, 2021). We show that, when translated to our setting, the identifying assumptions in these papers either impose a constant marginal return of increasing exposure to the policy, which seems especially unappealing for the types of economic outcomes considered in our empirical application or rely on a conditional independence assumption that strongly limits the association between scores and current period potential outcomes.

In light of the limitations discussed above, we propose instead to resort to a combination of a below-and-above-threshold homogeneity assumption with a between-score homogeneity restriction to identify effects. Formally, we assume that $\phi^{+}_{t|l}(j) = \phi^{-}_{t|l}(j) = \phi_t(j)$ for every t, $l \leq t$ and $j \leq t$. Notice that, while this assumption limits heterogeneity of effects across subpopulations at different thresholds for given time period t and exposure j, it allows for time-series nonstationarity and non-constant returns in the marginal effects of increasing exposure. Under the homogeneity assumptions, we are able to identify the vector of marginal effects at t, $\boldsymbol{\phi}_t = (\phi_1(j), \phi_2(j), \ldots, \phi_t(j))$, as the unique solution of the linear system:

$$\underbrace{\begin{bmatrix} \omega^+_{t|1}(1) - \omega^-_{t|1}(1) & \omega^+_{t|1}(2) - \omega^-_{t|1}(2) & \ldots & \omega^+_{t|1}(t) - \omega^-_{t|1}(t) \\ \omega^+_{t|2}(1) - \omega^-_{t|2}(1) & \omega^+_{t|2}(2) - \omega^-_{t|2}(2) & \ldots & \omega^+_{t|2}(t) - \omega^-_{t|2}(t) \\ \vdots & \vdots & \ldots & \vdots \\ \omega^+_{t|t}(1) - \omega^-_{t|t}(1) & \omega^+_{t|t}(2) - \omega^-_{t|t}(2) & \ldots & \omega^+_{t|t}(t) - \omega^-_{t|t}(t) \end{bmatrix}}_{=\boldsymbol{\Omega}_t} \boldsymbol{\phi}_t = \begin{bmatrix} \tau_{t|1} \\ \tau_{t|2} \\ \vdots \\ \tau_{t|t} \end{bmatrix}, \tag{4}$$

under the assumption that the matrix $\boldsymbol{\Omega}_t$ has full rank, which ensures unicity of the solution of the linear system (5). The assumption of full rank requires thresholds in different periods to differentially affect exposure at t, which may be seen as a relevance condition on the first-stage contrasts (jumps) $\omega^+_{t|l}(j) - \omega^-_{t|l}(j)$.

Estimation and inference on $\boldsymbol{\phi}_t$ identified through (4) is immediate. Specifically, one may estimate $\omega^+_{t|l}(1) - \omega^-_{t|l}(1)$ with the RD estimators of the "first-stage" contrasts $\lim_{s\downarrow 0} \mathbb{E}[\mathbf{1}\{D_t \geq j\}|Z_l = s] - \lim_{s\uparrow 0} \mathbb{E}[\mathbf{1}\{D_t \geq j\}|Z_l = s]$, and the $\tau_{t|l}$ by the corresponding reduced-form contrasts. One may then estimate $\boldsymbol{\phi}_t$ by numerically solving the sample analog of the system (4), i.e. by numerically inverting the estimator $\widehat{\Omega}_t$ of Ω_t. Inference may be performed using the delta-method, under assumptions that ensure asymptotic normality of the building-block RD estimands (Calonico et al., 2014).

While appealing, the plug-in approach outlined in the previous paragraph may perform poorly in practice if $\widehat{\Omega}_t$ is nearly collinear. In this case, the estimator may exhibit substantial biases, and asymptotic approximations may deliver inference methods with severe distortions, as in a weak instrumental variable settings (Andrews et al., 2019).[11] We thus

[11] Notice that there is a tension between the validity of the homogeneity assumption $\phi^+_{t|l}(j) = \phi^+_{t|l}(j)$ and the full rank of Ω_t. Indeed, one would expect that $\phi^+_{t|l}(j) = \phi^+_{t|l}(j))$ when the score is highly persistent across time, in which case self-selection is limited as $(Z_{l+1}, Z_{l+2}, \ldots, Z_t)$ is effectively randomized when the Z_l is in a neighborhood of 0. However, this is also the case where one would not expect many variability across the rows of Ω_t.

propose to impose additional restrictions on the evolution of the effect $\boldsymbol{\phi}_t$ that may enable the construction of more efficient estimators. Specifically, assuming that the outcome of interest is included in first differences in the specification, we propose to parametrize $\boldsymbol{\psi}_t(j)$, the average cummulative effect of the policy *in levels*, for an individual at period t that has been exposed to the policy continuously over j periods, by:

$$\boldsymbol{\psi}_t(j) = \sum_{l=0}^{j-1} \sum_{s=l}^{j-1} \phi_{t-l}(j-s) = \underbrace{\delta_t}_{\text{calendar time effect}} + \underbrace{\mu_{t-j}}_{\text{cohort effect}} + \underbrace{\gamma(j)}_{\text{exposure effect}} . \tag{5}$$

In Appendix B.3, we provide an optimal-minimum distance estimator that efficiently incorporates information from the first-stage matrices $\{\hat{\Omega}_t : t = 1, \ldots, T\}$, and the reduced-form estimators $\{\hat{\tau}_{t|l} : t = 1, \ldots, T, l \leq t\}$ to estimate (5). We also show how our approach to estimation immediately yields overidentifying restrictions J-test, thus enabling researchers to assess the appropriateness of parametrization (5). We further provide computational details of the implementation of our methods, including how to efficiently recover the covariances between the building-block RD estimators in `R`; and propose a placebo test that may be implemented if pre-treatment data is available.

4 Empirical Results

4.1 Empirical Strategy

Sample: Our unit of observation is the firm. We focus on firms rated by the Central Bank of Brazil, filtering for those that participated in the corporate bond market at any point between 2020 and 2023. Financial firms are excluded, as they are ineligible for the policy. We limit the sample to firms that were active between 2020 and 2022, and further restrict it to those that had at least 100 employees at any point between 2019 and 2022. This filtering aims to exclude firms potentially created solely to issue corporate bonds without employing a workforce or participating in interfirm payments within supply chains. We use the time period 2020-2022, with outcomes at the monthly level. We stop at 2022 because RAIS labor data is only available until this period. This leaves us with 677 unique firms and 24,372 firm $\times$ month observations, as we have a complete panel for most outcomes of interest.

Table 2 presents summary statistics for firms in our samples and their respective corporate bonds in the pre-intervention period of October 2021. For firms around the threshold, differences in most outcomes, such as total debt, corporate bond share, payment outflows, wage bill, and the number of workers, are not statistically significant, with p-values well above 0.05. However, when comparing eligible firms around the threshold to all eligible firms, we observe that firms in the full sample tend to have larger total debt and wage bills. The full sample of eligible firms also has more workers on average, borrow more, and at a lower spread, indicating that they tend to be larger and less financially constrained than the marginal firm.

Econometric Implementation: We implement the dynamic RDD estimator from Section 3.3 by first differencing all outcomes of interest, ΔY_{it}, and applying a 2% winsorization to mitigate the influence of outliers. We use the normalized score Z_{it} from Section 3.3. Finally, when normalizing the estimator by $\mathbb{E}[\tilde{Y}_0 \mid Z_1 = 0]$, where $\tilde{Y}_0$ is the level of the outcome of interest, we estimate the conditional expectation via local polynomial regression. Numerical implementation details are provided in Appendix B.3.

We parametrize the cumulative treatment effects following the system defined in 5. The resulting sequence $\{\psi_t(t)\}_{t=1}^{T}$ represents the cumulative impact of an absorbing treatment over time, capturing how the treatment effect evolves as the treatment persists for a unit that maintained eligibility status.

We then present the results of the optimal estimator, derived in Appendix B.3, which is designed to minimize the variance of the estimated treatment effects. Additionally, we include the J-test for overidentifying restrictions, which tests the consistency of the imposed parametrization of cumulative treatment effects with the data.

Bandwidth Choice: We select a cohort-specific RDD bandwidth, h_s, ensuring consistency across all time periods and outcomes. Following the bandwidth selection method from Calonico et al. (2020), we base this choice on the pre-treatment (October 2021) wage bill. We use two distinct MSE-optimal bandwidth selectors (for firms below and above the cutoff) and apply a triangular kernel in all specifications.

Visualization of Cumulative Treatment Effects The path $\{\psi_t(t)\}_{t=1}^{T}$ represents the cummulative effects of an absorbing state. This cumulative effect is estimated by working with outcomes in the first differences. For a percentage change interpretation, each estimate can be normalized by an estimator of $\mathbb{E}[Y_0 \mid Z_1 = 0]$, where Y_0 is the pre-period outcome in levels. Under our identification assumption, this is valid because units around the threshold are comparable.

This approach ensures that we track changes in outcomes for the same group of firms. Tracking the same group is important for maintaining consistency and comparability in the analysis. By focusing on stable cohorts, we ensure that observed changes in outcomes are not attributable to mechanical variations in the composition of firms. This reduces the risk of confounding effects from firms entering or exiting the cohort, which could distort treatment effect estimates, and allows for a more precise comparison of within-firm changes across different outcomes.

4.2 The effects on primary corporate bonds market

We first estimate the policy's causal effect on primary corporate bond markets. Figure 20 plots the estimates of the dynamic causal effects of eligibility to the LFL policy on firms' outstanding debt. shows the dynamic causal effects of LFL policy eligibility on firms' outstanding debt. There is a significant 12% increase in corporate bond debt after one year in the policy, with a slight rise in foreign debt. However, this is partially offset by a 5% decrease in intermediate bank/fund debt. Overall we observe a decrease in the cost of capital associated with corporate bonds of around 20 basis points. Figure 16 shows that we do not find any statistically significant effect of the policy on corporate debt maturity or bank/fund interest rates charged to eligible firms. Thus we can show that the implied reduction in issuance spread of corporate bonds was around 67 basis points.

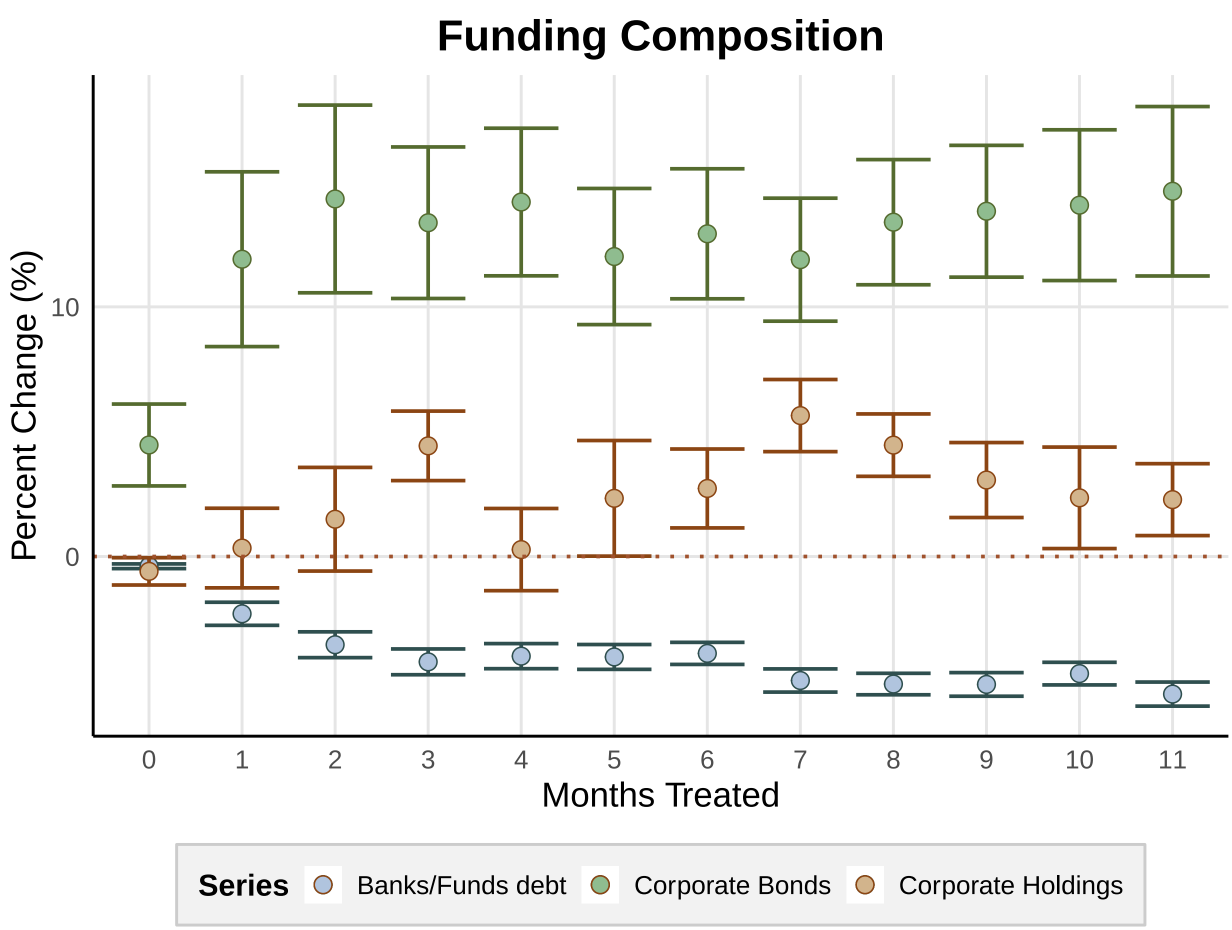
We estimate changes in safe asset holdings following eligibility, holdings by around 12%. We conclude that eligibility led firms to increase net total debt, as the rise in corporate bond issuance was only partially offset by reductions in other forms of intermediated financing. Notably, there was a catalyst effect from firms' use of internal funds. In the next subsection, we will link this to a reduction in firms' precautionary savings, likely driven by the expectation of improved future credit access.

4.3 Effects on bond liquidity in secondary markets

We show how corporate bond liquidity is affected in the secondary markets. To quantify this effect, we measure firm-level turnover liquidity in the over-the-counter (OTC) market through three specific operations: (i) trading in the secondary market, (ii) using corporate bonds as collateral in short-term repurchase (repo) operations, and (iii) utilizing them as collateral in other financial transactions, including those involving the LFL facilities. We define turnover liquidity on a monthly basis at the asset level using three metrics: (i) the total quantity traded in the secondary market divided by the outstanding market quantity, (ii) the total quantity posted as collateral in repo operations on the last business day of the month divided by the outstanding market quantity, and (iii) the total quantity posted as collateral in other operations divided by the outstanding market quantity. Finally, we aggregate these metrics at the firm level using a volume-weighted average based on the outstanding nominal value of each asset.

We estimate an increase in service flow from a "buy-and-hold" corporate bond strategy in Figure 9. Initially, there was a mechanical rise in collateral posted under LFL, primarily from commercial banks' holdings already used in short-term repo operations. However, one

Figure 6: Dynamic Treatment Effects on Capital Structure.



Note: The figure shows estimates of cumulative treatment effects on outstanding debt volume and safe asset holdings. with error bars representing 95% confidence intervals. Differences in Debt volume are normalized by pre-period total debt.

year after the policy, banks compensate by increasing repo usage relative to initial volumes while trading activity decreases.

4.4 The real effects of eligibility

We estimate that the policy had real effects. Figure 20 shows that eligible firms causally increased the wage bill by about 6 % (in real terms) one year after becoming eligible. and temporarily increased net payment outflows to other non-financial firms (Figure 8). In the appendix we show that this result is driven by an increase in payment outflows rather than a decrease in payment inflows).

Combining the results from the previous subsection, we conclude that the use of internal finance amplified the real effects of the increased demand for corporate bonds. We interpret this effect as a reduction in precautionary savings, as firms typically accumulate safe assets to self-insure against liquidity shocks. This trend is evident in the time series of firm holdings in our sample in Figure 13, where we observe a significant increase in safe asset holdings during the COVID crisis.

While there are no estimates of the real effects of collateral eligibility on firms, some papers examine the real effects of direct purchases of corporate bonds (Darmouni and Siani (2022), Adelino et al. (2023)), and others discuss firm responses to increases in corporate debt convenience yield (Mota (2023)). The prevailing wisdom in this literature suggests that such policies do not have direct real effects; instead, firms typically increase cash holdings, repay debt, or distribute payments as dividends. Our results sharply contrast with this view. Not only do we observe real effects, but these effects are amplified by firms' use of internal funds.

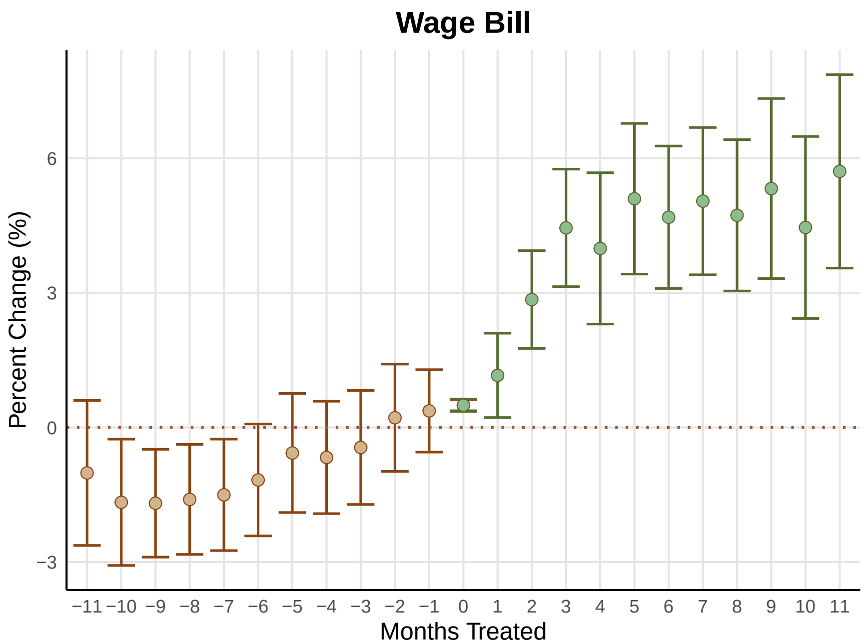


Figure 7: Dynamic Treatment Effects on Wage Bill.

Note: The figure shows estimates of cumulative treatment effects on the wage bill, with error bars representing 95% confidence intervals. Wage bill differences are normalized by the pre-period wage bill.

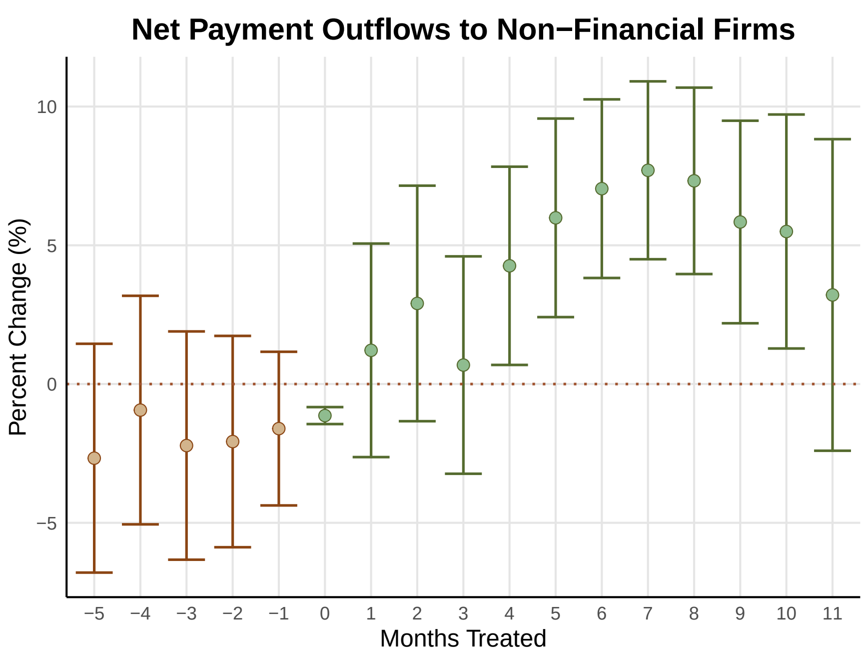
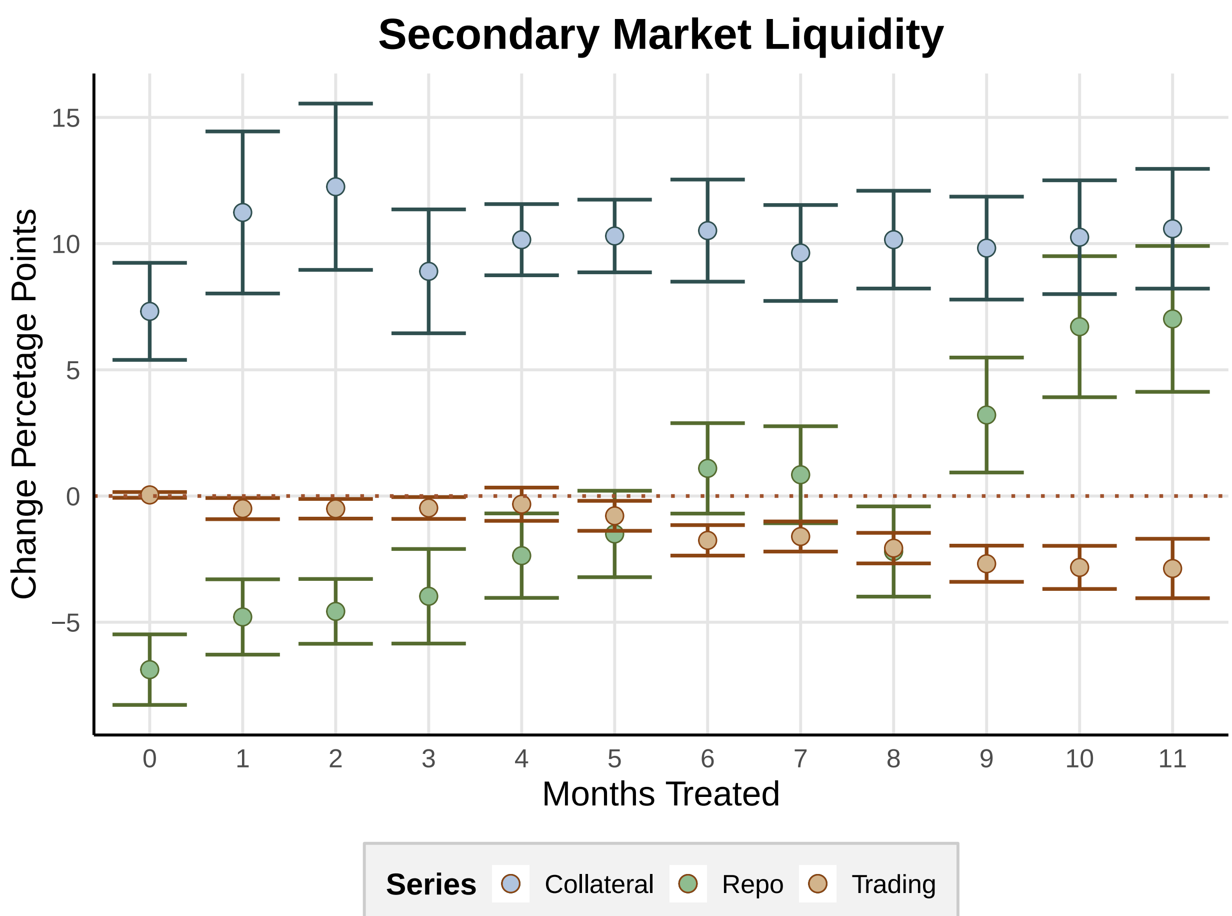


Figure 8: Dynamic Treatment Effects on Net Payment Outflows to Non-Financial Firms.

Note: The figure shows estimates of cumulative treatment effects on the net payment outflows to non-financial firms, with error bars representing 95% confidence intervals. Net payment outflows are normalized by total outflows from October 2020 to October 2021.

Figure 9: Dynamic Treatment Effects on OTC markets turnover liquidity.



Note: The figure shows estimates of cumulative treatment effects on turnover liquidity of corporate bonds,with error bars representing 95% confidence intervals. We measure firm-level turnover liquidity in the OTC market through three types of operations: (i) trading in the secondary market, (ii) use as collateral in short-term repo operations, and (iii) use as collateral in other operations (including LFL). Turnover liquidity is defined monthly at the asset level as: (i) the total traded quantity in the secondary market divided by the outstanding market quantity, (ii) the total quantity posted as collateral in repo operations on the last business day of the month divided by the outstanding market quantity, and (iii) the total quantity posted as collateral in other operations divided by the outstanding market quantity. We aggregate this at the firm level using the volume-weighted average based on the outstanding nominal value of each asset. Effective observations are unique firms with positive weights.

5 Model

In this section, we implement a semi-structural approach that builds on a theoretical model of segmented financial markets to address the following questions: (1) To what extent does the LFL policy induce liquidity injections in eligible firms? (2) How do these injections affect firms' production and financial decisions?

LFL and Liquidity Injections: We analyze the relationship between firm eligibility and the induced liquidity injections from financial intermediaries, using a segmented financial market model.

We develop a model based on the asset-liability management (ALM) practices of commercial banks. In this framework, a set of commercial units handles both loan origination and deposit collection from firms, while a separate ALM sets banks' internal fund transfer prices (FTP). The ALM sets FTP rates that account for liquidity costs and regulatory requirements, thus ensuring that commercial units fully internalize these costs and benefits. When a unit makes a corporate loan or purchases a firm's bond, it "borrows" from the ALM at a defined FTP; conversely, deposits collected from firms are "lent" to the ALM at a set price, aligning incentives across divisions and managing liquidity risk efficiently.[12] In the context of the LFL policy, the ALM determines whether to access central bank facilities and how much corporate bond collateral to post. A key implication is that the transmission mechanism of firm eligibility operates directly through its effect on the FTP charged to lending units, thus shaping their bond demand. If intermediaries' demand for corporate bonds is sufficiently price-inelastic, the reduction in the FTP can be passed through to either bond prices or the quantity borrowed, and, in the latter case, it effectively operates as a liquidity injection that is a subsidy to firm borrowing.[13] See Figure 21 for a diagram.

Finally, we develop an approach to estimate this elasticity based on firms' trade-offs

[12]See Moody's (2018) for a brief description of the fund transfer pricing methodology of commercial banks.

[13]For example, in Europe, the collateral haircuts imposed by the ECB on corporate bonds are incorporated by banks into the FTP. See, for instance, this report.

between borrowing and holding cash-like assets. We leverage the fact that we do observe from our data, both the firm borrowing cost and return by holding safe assets (from the OTC data). Our results indicate that the demand for corporate bonds is relatively inelastic, with price elasticity of demand between 1.06 and 1.73, magnitudes in the range of those estimated for the U.S. corporate bond market (Darmouni et al. (2022)). While the literature usually follows the demand system approach from Koijen and Yogo (2019), our method is based on price theory arguments. Firms in our sample hold liquid asset reserves while also carrying debt.[14] The trade-off between the marginal cost of holding cash and the marginal benefit, driven by the firm's liquidity risk, shapes their liquidity management decisions. Rather than holding cash, firms may issue debt to address liquidity needs, though this approach carries risks, particularly due to the potential need for large issuance that could impact market prices. The magnitude of this price impact (which affects the liquidity risk) depends on market elasticity, and we develop our strategy around this observation.

5.1 Setup

5.1.1 Asset Generating Unit

The AGU is a short-lived financial intermediary that can invest in firms' debt instruments but needs to raise funds B_t from the ALM. We assume firm debts are imperfect substitutes in the following form. The (ex-post) aggregation of returns to enter the utility function of the intermediary is given by:

$$\tilde{B}_{t+1} = \left(\int \eta_i^{\frac{1}{\varepsilon}} \left(R_{i,t} \omega_{i,t} B_t \right)^{\frac{\varepsilon-1}{\varepsilon}} di \right)^{\frac{\varepsilon}{\varepsilon-1}}, \tag{6}$$

where B_t is the total investment in firm debt, $R_{i,t}$ is the ex-post return, and $\omega_{i,t}$ denotes the portfolio weight of firm i's debt, satisfying

[14]That is not specific to our setting; recently, this fact has been well documented in the US and Europe.

$$\int \omega_{i,t}\, di = 1. \tag{7}$$

The term $\eta_i > 0$ is a firm-specific demand shifter, and $\varepsilon > 1$ governs the elasticity of substitution between different firms' debts. As $\varepsilon \to \infty$, then we have the perfect substitute case.

To access funds B_t for investment in corporate debt, the AGU obtains financing from the ALM, which provides B_t at a fund transfer price $\mu(B_t)$, where $\mu(\cdot)$ is a well-behaved function. Additionally, the ALM supplements the investment by $(1+\tilde{\tau}_c)B_{t+1}^{LFL}$, where

$$B_{t+1}^{LFL} = \left(I(i) \int \eta_i^{\frac{1}{\varepsilon}} \left(R_{i,t}\omega_{it}B_t\right)^{\frac{\varepsilon-1}{\varepsilon}} di \right)^{\frac{\varepsilon}{\varepsilon-1}}, \tag{8}$$

where $I(i)$ is an indicator function, with value 1 if i is eligible to the LFL policy. Conversely, for each dollar not allocated to eligible firms, the ALM deducts $(1-\tilde{\tau}_l)B_{t+1}^{NLFL}$, defined analogously:

$$B_{t+1}^{NLFL} = \left((1-I(i)) \int \eta_i^{\frac{1}{\varepsilon}} \left(R_{i,t}\omega_{it}B_t\right)^{\frac{\varepsilon-1}{\varepsilon}} di \right)^{\frac{\varepsilon}{\varepsilon-1}} \tag{9}$$

Finally, the expected utility of the AGU is:

$$\mathbb{E}_t \left[\eta_b^{\frac{1}{\varepsilon}} \frac{\tilde{B}_{t+1}^{1-\gamma}}{1-\gamma} + \tilde{\tau}_c B_t^{LFL} - \tilde{\tau}_l B_t^{NLFL} - \mu(B_t)B_t \right], \tag{10}$$

where we set $\gamma = \frac{1}{\varepsilon}$. Taken the distribution of returns $R_{i,t}$, the eligibility status $I(i)$, $\mu(\cdot)$, $\tilde{\tau_{c,t}}$ and $\tilde{\tau_{l,t}}$ as given, the AGU maximizes (10), subject to (6) - (9).

One key modeling assumption first is that the AGU is short-lived or exhibits myopic decision horizons. This assumption is analogous to the assumption in Consumer Asset Pricing Models of Gabaix and Koijen (2021) but within the context of Intermediary Asset Pricing Models (e.g., He and Krishnamurthy (2013)). However, the interpretation differs: while the latter emphasizes behavioral finance aspects of household portfolio decisions (e.g., the "narrow framing" in Barberis et al. (2006)), the AGU in our model may be driven by short-term performance incentives prevalent in financial markets (e.g., Morris and Shin (2015); Morris et al. (2017); and, specifically for corporate bond markets, Darmouni et al. (2022)). A critical implication of this distinction is that the AGU's risk aversion need not align with that of households.

We show in the appendix that we can decompose the relative demand between eligible and non-eligible firms' debt in this setting as:

$$\frac{b_{i,t}^{LFL}}{b_{j,t}^{NLFL}} = \left(\frac{\eta_i}{\eta_n}\right)\left(\frac{S_{i,t}}{S_{j,t}}\right)^{\varepsilon-1}\left(\frac{1+\tau_{c,i,t}}{1-\tau_{l,j,t}}\right)^{\varepsilon}, \tag{11}$$

where $\left(\frac{1+\tau_{c,i,t}}{1-\tau_{l,j,t}}\right) := \left(\frac{1-\mathbb{P}_{i,t}}{1-\mathbb{P}_{j,t}}\right)\left(\frac{1+\bar{\tau}_{c,t}}{1-\bar{\tau}_{l,t}}\right)$ has an interpretation of a (risk-adjusted) relative transfer between eligible and noneligible firms. The implied liquidity injection (as a % of total debt) for the marginal firms is given by:

$$L_{i,t}^{LFL} = 1 - \left(\frac{1+\tau_{c,i,t}}{1-\tau_{l,i,t}}\right)^{-\varepsilon}.$$

Remark: perfectly elastic markets We briefly discuss what would happen if the market were perfectly elastics, i.e., $\varepsilon \to \infty$, which is a common assumption in many asset pricing and macro-finance models. If we restrict to firms with the same default risk and η_i (and for, simplicity, assuming the LFL does not affect risk), as will be the case for firms close the eligibility threshold, we can rearrange (11):

$$\left(\frac{b_{i,t}^{LFL}}{b_{j,t}^{NLFL}}\right)^{\frac{1}{\varepsilon}} = \left(\frac{S_{i,t}}{S_{j,t}}\right)^{\frac{\varepsilon-1}{\varepsilon}}\left(\frac{1+\tau_{c,t}}{1-\tau_{l,t}}\right).$$

Taking the limit as $\varepsilon \to \infty$ we will have that $\left(\frac{1+\tau_{c,t}}{1-\tau_{l,t}}\right) = \left(\frac{S_{j,t}}{S_{i,t}}\right)$, so changes in prices will directly reflect changes in implied subsidies. In cases where effects on cost of capital are small, assuming perfectly elastic markets would imply small subsidies.

5.1.2 Liability Generating Unit

This unit determines $R^f_{i,t}$, the interest rate on cash-like assets that the firm i will receive at time t. For our analysis, it is sufficient that there exists cross-sectional variation in $R^f_{i,t}$. Such variation may arise from high turnover in a firm's cash holdings, as banks typically offer lower remuneration to firms with high turnover due to the associated liquidity risk. This could be implemented by offering term deposit contracts that penalize early withdrawals (in terms of return) or by providing lower returns in checking accounts.

In the next subsection, we discuss how the firm's borrowing cost is linked to the return on cash-like assets, forming the basis for our estimation strategy of the structural parameter ε.

5.2 Firms

For this part, we only require that the following first-order condition holds for the firm,

$$\frac{1}{R^f_{it}} = \frac{\varepsilon - 1}{\varepsilon - 2}\mathbb{E}_t\left[\Lambda_{t+1} s_{i,t+1}\right] , \tag{12}$$

where R^f_{it} represent the interest rate on cash-like assets that firm i faces, $s_{i,t+1}$ denote the spread between the cost of capital and the savings rate, and Λ_{t+1} be the firm's stochastic discount factor (SDF). The left-hand side of the equation captures the marginal cost of holding one unit of cash for the next period, while the right-hand side represents the marginal benefit, which reflects the firm's liquidity risk. Rather than holding cash, a firm might choose to issue debt when liquidity is needed, but the uncertainty around interest rates and the potential necessity to issue a large volume introduces risk. In the appendix, we present one model where this condition holds.[15]

[15] Our approach builds on the assumption that firms simultaneously borrow and save, creating a trade-off in liquidity management. The three largest financial institutions in our Brazilian empirical setting referenced this in a recent proposal (September 2024), suggesting a Central Bank Digital Currency use case that would enable firms to use term deposits as collateral for short-term working capital loans. They argued that this would impact both the return on deposits and the expected spread, influencing the trade-off we emphasize and enhancing risk-sharing between banks and firms.

It's worth noting a logical implication of market elasticity for firms' liquidity management decisions. If markets are not perfectly elastic, then firms internalize the price impact of a potential large debt issuance in case there is a liquidity shock or an immediate need for external finance, which can lead to a larger liquidity preference.[16]

5.3 Identification, estimation, and results

We require estimates of some crucial parameters to answer our original questions. First, in Section 5.3.1, we outline a strategy to identify and estimate the elasticity of substitution in the demand for bonds ϵ. Having recovered ϵ, Section 5.3.2 proposes a strategy to recover the relative transfer rate between eligible and non-eligible firms implied by the policy. Section 5.3.3 then uses these estimates to compute the effect of LFL-implied subsidies.

5.3.1 Identifying and estimating the elasticity parameter ε

Rearranging equation (12), we have

$$\mathbb{E}_t[s_{i,t+1}] = \mathbb{E}_t[(1-\Lambda_{t+1})s_{i,t+1}] + \underbrace{\frac{\varepsilon-2}{\varepsilon-1}}_{\beta}\frac{1}{R^f_{it}}\,. \tag{13}$$

Using (13), we generate an estimating equation, which we take to the data, as follows. First, we consider a two-way fixed effects approximation to $\mathbb{E}_t[(1-\Lambda_{t+1})s_{i,t+1}]$, i.e. we replace it with $\alpha_i + \gamma_t$ in (13). This produces the following linear specification:

$$s_{i,t+1} = \alpha_i + \gamma_t + \beta\frac{1}{R^f_{i,t}} + e_{i,t+1}\,, \tag{14}$$

[16] A simple case where this would hold is if the deposit rate is fixed and the marginal benefit decreases with cash holdings. With cash buffers, firms borrow less during liquidity shocks. Given $\frac{\varepsilon-1}{\varepsilon-2} > 1$, the marginal benefit curve flattens, making firms hold more cash. Generally, this requires the marginal benefit curve to be steeper than the marginal cost curve in the relevant region.

where the error term satisfies the sequential exogeneity assumption:

$$\mathbb{E}[e_{i,t+1}|R_{i,t}, R_{i,t-1}, \ldots] = 0\,.$$

Consistent estimation of β in short-panels may then be performed by first taking the first-difference of (14):

$$\Delta s_{i,t+1} = \delta_t + \beta\Delta\frac{1}{R^f_{i,t}} + \Delta e_{i,t+1}\,, \tag{15}$$

and then estimating (15) through a generalized method of moments estimator, using $\frac{1}{R^f_{i,t-c}}$, $c \leq 1$, as instruments to $\Delta\frac{1}{R^f_{i,t}}$, and including a full set of time dummies as controls, in analogy to Arellano and Bond (1991).

The validity of our proposed estimation strategy hinges on two requirements. First, estimation of (15) presupposes the existence of both time-series as well as cross-sectional variation in the firm-level savings rate $R^f_{i,t}$. This is indeed the case in our empirical setting, where we observe substantial variability in the return to firms' cash-like holdings both between firms as well as over time (see Figures 17 and 18 in the Appendix).

Secondly, the validity of our approach requires the two-way fixed effects structure to be indeed a good approximation to the conditional expectation $\mathbb{E}_t[(1-\Lambda_{t+1})s_{i,t+1}]$, i.e. that $\mathbb{E}_t[(1-\Lambda_{t+1})s_{i,t+1}] \approx \alpha_i + \gamma_t$. Our option for a two-way fixed effects approximation rests on a "double" justification for this method. To see this, notice that, if the economy were on an aggregate steady-state, then Λ_{t+1} would be constant over time, and the only source of variability in $\mathbb{E}_t[(1-\Lambda_{t+1})s_{i,t+1}]$ would rest on cross-sectional heterogeneity in the expectations over $s_{i,t+1}$. In this case, $\mathbb{E}_t[(1-\Lambda_{t+1})s_{i,t+1}] \approx \alpha_i$, and a one-way fixed effects strategy leveraging time series variation in the savings rate would be sufficient to identify effects. Conversely, if the economy was not in a steady-state, but there was no cross-sectional heterogeneity in firms' assessments regarding $s_{i,t+1}$, then $\mathbb{E}_t[(1-\Lambda_{t+1})s_{i,t+1}] \approx \gamma_t$, and a one-way fixed effects approximation that leverages cross-sectional variation in the savings-rate would suffice. Therefore, when we employ a two-way fixed effects approximation, we are able to construct a “doubly-robust” estimator, in the sense that its validity hinges on either one of the two cases being valid.

Empirical Strategy and Results: We estimate specification (15) using the Arellano-Bond type estimator described in the section (5.3.1). We measure the return on cash-like assets $R^f_{i,t}$ as follows. The majority of firms' cash-like holdings have returns indexed to the baseline interest rate, including term deposits, portions of treasury holdings, money market fund shares, and returns on repo operations. At the end of each month, we compute $R^f_{i,t} := R^f_t S^f_{i,t}$, where R^f_t is the baseline rate and $S^f_{i,t}$ represents the volume-weighted average spread over the risk-free rate for all of the firm's cash-like instruments, with adjustments for any instruments with predetermined returns.[17]. We measure the borrowing spread $s_{i,t}$ as the difference between the weighted average interest rate on all observed domestic credit operations—excluding foreign debt, for which interest rates are unavailable in our dataset—and the corporate bond rate in month t, relative to the saving interest rate $R^f_{i,t}$. We estimate using monthly data from January 2020 to December 2023, restricting firms as in the empirical analysis. Results are robust to restricting to the pre-LFL treatment period, using quarterly differences, and including all firms that issue corporate bonds.

Table 4 reports our estimation results. We find $\hat{\varepsilon} = 2.31$, corresponding to a price elasticity of demand of approximately 1.31. This estimate aligns with the inelastic market hypothesis proposed by Gabaix and Koijen (2021), which posits that financial markets exhibit significantly lower elasticity than commonly assumed in standard asset pricing and macro-finance models. Additionally, our results are consistent with findings from the U.S. corporate bond market; for example, Darmouni et al. (2022) estimate elasticities between 0.7 and 1.9, depending on the type of financial institution.[18] However, the interpretation of our estimates differs slightly: while this literature focuses on security-level or institution-specific

[17]For instance if a firm has a return of 90% of the baseline interest rate, and the (annualized) baseline interest rate is 3%, then the (annualized) return on cash-like instruments is $R^f_{i,t} = 1.027$.

[18]Darmouni et al. (2022) find lower elasticities than Bretscher et al. (2022), who find elasticities for individual bonds ranging from one to 14 and argue that the discrepancy likely stems from the use of less granular security groupings than Bretscher et al. (2022).

elasticities, our estimates capture the aggregate elasticity of firm-level debt. This distinction is important, as we note that the aggregate debt elasticity is important for firms' liquidity management decisions and the subsequent pass-through effects from the financial to production decisions. Thus, our estimation strategy departs from the demand-system approach of Koijen and Yogo (2019) by incorporating the interaction between demand and debt supply, mediated by liquidity risk, as captured in (12). Furthermore, our estimates imply a risk aversion parameter of $\hat{\gamma} = 0.43$, indicating that the AGU is risk-averse but less so than a typical household with log utility function over consumption.

In the next subsection, we combine our elasticity estimates with reduced-form dynamic RDD results to quantify the implicit debt subsidy induced by the LFL policy for marginal firms (those near the eligibility threshold).

5.3.2 Identifying and estimating the relative subsidy rate implied by the LFL policy

Having backed-out estimates for the elasticity parameter ϵ, we are able to identify the relative transfer rate between eligible and non-eligible firms implied by the LFL policy as follows. Let $b_{i,t}(d)$ and $S_{i,t}(d)$, $d \in \{0, 1\}$, denote the potential bond levels and spreads associated with being externally assigned eligibility to the LFL policy continuously over t periods since its implementation ($d = 1$) or never being assigned to it during this period ($d = 0$). The first-order conditions of the AGU problem imply that the relative debt of a firm i eligible to the LFL for t periods, $b_{i,t} = b_{i,t}(1)$, and a firm j never eligible to the policy over this period, $b_{j,t} = b_{j,t}(0)$, satisfy:

$$\frac{b_{i,t}(1)}{b_{j,t}(0)} = \left(\frac{S_{i,t}(1)}{S_{j,t}(0)}\right)^{\varepsilon-1} \left(\frac{\eta_i}{\eta_j}\right)^{\varepsilon} \left(\frac{1+\tau_{c,i,t}(1)}{1-\tau_{l,i,t}(0)}\right)^{\varepsilon},$$

or, taking logs

$$[\log(b_{i,t}(1)) - \log(b_{j,t}(0))] = (\varepsilon - 1)[\log(S_{i,t}(1)) - \log(S_{j,t}(0)))] + \varepsilon[\log(\eta_i) - \log(\eta_j)] + \varepsilon \log\left(\frac{1 + \tau_{c,i,t}(1)}{1 - \tau_{l,i,t}(0)}\right),$$

where η_l is the share parameter for the bonds b_l of firm l in the CES aggregator. This parameter reflects an idiosyncratic preference for a firm's bonds – beyond those captured by the asset return — due to unmodeled factors such as complementarity or agency concerns. Importantly, we assumed this parameter to be invariant to a firm's participation in the LFL policy. Using this fact, we may average the relation across eligible firms i and non-legible firms j on the threshold of eligibility in the first-period to obtain:

$$\begin{aligned} &\mathbb{E}[\log(b_{i,t}(1)) | \, \mathrm{Score}_{i,1} = 0] - \mathbb{E}[\log(b_{j,t}(0)) | \, \mathrm{Score}_{j,1} = 0] = \\ &(\epsilon - 1)(\mathbb{E}[\log(S_{i,t}(1)) | \, \mathrm{Score}_{i,1} = 0] - \mathbb{E}[\log(S_{j,t}(0)) | \, \mathrm{Score}_{j,1} = 0]) \\ &+ \epsilon \left(\mathbb{E}[\log(1 - \tau_{c,i,t}(1)) | \, \mathrm{Score}_{i,1} = 0] - \mathbb{E}[\log(1 - \tau_{l,i,t}(0)) | \, \mathrm{Score}_{j,1} = 0]\right), \end{aligned} \tag{16}$$

where we have used the RD assumption that individuals around the threshold of eligibility should be balanced with respect to predetermined traits (in this case, the shifters η_l). Now, our RD strategy identifies the differences in conditional expectations – the cumulative effects of exposure to the policy (the $\psi_t(t)$ from our empirical setting) on bonds and spreads – in the formula above. Consequently, having obtained an estimate of ϵ, we back out an estimate of the relative risk-adjusted transfer at the marginal firm.

Empirical Strategy and Results: We first note that up to a first-order approximation, we can recast (16) as:

$$\psi_t^b(t) \approx (\varepsilon - 1)\psi_t^S(t) + \varepsilon \psi_t^\tau(t), \tag{17}$$

where $\psi_t^b(t)$ and $\psi_t^S(t)$ represent the reduced-form cumulative effect estimands for total debt and total cost of capital. $\{\psi_t^\tau(t)\}_{t=0}^T$ has the interpretation of a sequence of implied subsidies on the marginal firm. We thus follow a plug-in approach to get the point estimates

$$\widehat{\psi_t^\tau(t)} = \left(\frac{\widehat{\psi_t^b(t)} - (\hat{\varepsilon} - 1)\widehat{\psi_t^S(t)}}{\hat{\varepsilon}} \right),$$

where $\widehat{\psi_t^b(t)}$, $\widehat{\psi_t^S(t)}$ are the estimates from the empirical section and $\hat{\varepsilon}$ is the elasticity estimate from the previous subsection. Figure 19 shows, of 4%, which implies, on average, the empirical results. We estimate a stable induced subsidy, on average of 4%, which implies on average a liquidity injection (as a percentage of total debt) of 8.5%. Given that the average haircut on marginal firms' corporate bonds in the LFL is 39%, a back-of-the-envelope calculation suggests that each 1% reduction in the haircut by the BCB induced a 0.14% direct liquidity injection by the financial sector to the marginal firms. In the next subsection we discuss the pass-through for firms' production and liquidity management decisions.

5.3.3 Recovering the corporate effects of LFL-implied transfers

Let $P_{it}(a, s, \{\bar{\tau}_j\}_{j=1}^t)$ be a policy function chosen by the firm and $\psi_t(t)$ the reduced form dynamic RDD estimand associated with the outcome $P_{i,t}$. This policy function depends crucially on three key parts: on an aggregate state vector A_t^d, which may depend on the policy being in place since period 1 ($d = 1$), or not ($d = 0$); on idiosyncratic state variables S_{it} not affected by the policy, and on the sequence of exposures to the policy from its implementation, which in our model translate into a sequence of subsidy rates $\{\bar{\tau}_j\}_{j=1}^t$.

In this section, we are interested in computing the effects of LFL-implied transfers on a firm that is always eligible, i.e. we are interested in computing, for an alternative path of subsidies $\{\tilde{\tau}_j\}_{j=1}^t$, the expected effect:

$$\mathbb{E}[P_{it}(A_t^1, S_{it}, \{\tilde{\tau}_j\}_{j=1}^t) - P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1})|\text{score}_{i,1} = 0]\,. \tag{18}$$

In Appendix D.2, we show that, under high-level assumptions on monotonicity and differentiability of the policy functions, we can bound, up to a first-order approximation,

$$\min_{j=1,\ldots,t} \psi_t(t)\frac{\tilde{\tau}_j}{\psi_j^{\tau}(j)} \leq \mathbb{E}[P_{it}(A_t^1, S_{it}, \{\tilde{\tau}_j\}_{j=1}^t) - P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1})|\text{score}_{i,1} = 0] \leq \max_{j=1,\ldots,t} \psi_t(t)\frac{\tilde{\tau}_j}{\psi_j^{\tau}(j)} \,. \tag{19}$$

With this representation, we *partially* identify the counterfactual outcomes from a given subsidy path. We quantify the policy's effects by assessing changes in wage bills, cash holdings, and supplier payments from a (permanent) subsidy that induces a 1% increase in debt issuance and determine the required subsidy amount.

Empirical Results: Figure 10 presents the results. A 1% induced increase in debt issuance is estimated to raise the wage bill by 0.4–0.75%, partially funded through internal funds, with cash holdings decreasing by 0.2–0.35%. The results also show a small increase in external debt and a temporary rise in net payment outflows to suppliers. Achieving this would require a (permanent) subsidy equivalent to 0.8% of total debt issuance. The implied effect on issuance spreads is small. These findings relate to recent estimates on the direct effects of quantitative easing on firms, primarily through indirect mechanisms such as the portfolio rebalancing channel. Evidence suggests that quantitative easing impacts firms' capital structure—specifically increasing cash holdings and investment in a lesser magnitude—while having a near-zero, albeit statistically significant, effect on corporate bond prices (see Selgrad (2023)).[19]

Our results directly address the Central Bank's capacity to "target" the real economy by employing additional policy tools that enhance firms' borrowing opportunities, particularly when financial markets are illiquid. The inclusion of corporate bonds in the Collateral Framework under the LFL policy can be interpreted as a form of haircut policy, as advocated by Geanakoplos (2010). Our estimates indicate that the Central Bank can directly influence private agents' (net) leverage by affecting both the demand for their debt instruments and their liquidity management policies. This dual effect impacts both the supply and demand for funds, ultimately influencing employment and

[19]More broadly to the empirical literature on the real effects of financial shocks such as Chodorow-Reich (2014) and Huber (2018), where the effects are expected to come from credit rationing in the crisis period. In the Brazilian context, similar large quantity and small price effects are observed following working capital supply shocks, which induce permanent increases in sales and employment (Orestes et al. (2024)).

generating spillovers throughout the supply chain. Importantly, our estimates are estimated in recovery periods rather than crisis periods, indicating that the Central Bank's collateral policies can effectively stimulate economic activity outside financial crises.

6 Conclusion

We estimated that the LFL policy significantly influenced firms' financial and real outcomes. By allowing eligible corporate bonds to be used as collateral, the policy facilitated increased corporate bond issuance, reduced reliance on intermediate bank/fund debt, and lowered bond costs. This, in turn, led to a real impact on firms' operations, including a rise in the wage bill and net payment outflows, financed partially by a reduction in liquid cash holdings. The policy's influence extended beyond direct financial metrics, encouraging a "buy-and-hold" strategy among institutions, as evidenced by the decreased trading activity and increased use of bonds as collateral in repo operations.

These findings highlight the broader implications of expanding collateral eligibility to include corporate bonds within monetary policy frameworks. By affecting firms' and banks liquidity management strategies, the LFL policy contributed to easing borrowing constraints and promoting economic activity, even for relatively large firms that have access to capital markets.

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Figure 10: Responses to an 0.8% increase in borrowing subsidy

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Appendices

A Data Appendix

A.1 Sample Summary Statistics

Table 2: Summary statistics for the sample used in the analysis

	Around the Threshold			Eligible (All)
Firms (Outcomes 10/2021)	Non-eligible	Eligible	p-value (RDD)	
Total Debt (USD Mil.)	42.4	40.6	0.53	121
Corporate Bonds Share	34%	32%	0.86	45%
Payment Outflows (USD Mil.)	140	152	0.35	606
Wage Bill (USD Mil.)	17.8	16.5	0.38	56
Number of Workers	1619	1552	0.46	4464
Number of Firms	**115**	**112**	-	**680**
Assets (Outcomes 10/2021)	Non-eligible	Eligible	p-value (RDD)	
Issuance Spread (% over baseline)	4.78%	4.67%	0.72	3.80%
Outstanding Volume (BRL Mil.)	261	258	0.93	362
Maturity (Years)	7.6	6.9	**0.02**	7.1
Turnover Liquidity Trading (% monthly)	2.30%	2.10%	0.71	2.62%
Premium (% face value)	99.50%	100.50%	0.18	100.50%
Number of Assets	**164**	**294**	-	**1451**

B Econometric Appendix

B.1 Proof of Proposition 1

We first begin noticing that, by Assumption 3, $\lim_{\epsilon \downarrow 0} \mathbb{E}[Y_t(0)|Z_l = \epsilon] - \lim_{\epsilon \uparrow 0} \mathbb{E}[Y_t(0)|Z_l = \epsilon] = 0$ for any t or l. We then consider each of the three cases separately:

(a) *Case* $t < l$. Recall that:

$$Y_t = Y_t(0) + \sum_{j=1}^{t} \mathbf{1}\{D_t \geq j\}(Y_t(j) - Y_{t-1}(j)) \tag{20}$$

But, then, for any $|\epsilon| < \bar{\epsilon}$ and $j \in \{1, \ldots, t\}$:

$$\begin{aligned}
\mathbb{E}[\mathbf{1}\{D_t \geq j\}(Y_t(j) - Y_{t-1}(j))|Z_l = \epsilon] &= \\
\mathbb{E}[\mathbf{1}\{D_t(Z_1, \ldots, Z_t) \geq j\}(Y_t(j) - Y_{t-1}(j))|Z_l = \epsilon] &= \\
\mathbb{E}[\mathbf{1}\{D_t(Z_1, \ldots, Z_t) \geq j\}(Y_t(j) - Y_{t-1}(j))|Z_l = \epsilon, |Z_l| \leq \bar{\epsilon}] &= \\
\mathbb{E}[\mathbf{1}\{D_t(Z_1, \ldots, Z_t) \geq j\}(Y_t(j) - Y_{t-1}(j))||Z_l| \leq \bar{\epsilon}] &,
\end{aligned}$$

where the last equality uses the fact that the random variable whose conditional expectation is being taken, $\psi_t(j)$, is a measurable function of $\boldsymbol{D}_t$, $Y_t(j)$, $Y_t(j-1)$ and $\{Z_s\}_{s=1}^t$, with $t < s$. Consequently, it follows from Assumption 3 that this random variable is independent from Z_l, conditional on $|Z_l| \leq \bar{\epsilon}$, and therefore $\mathbb{E}[\psi_t(j)|Z_l = \epsilon, |Z_l| \leq \bar{\epsilon}] = \mathbb{E}[\psi_t(j)||Z_l| \leq \bar{\epsilon}]$. The desired conclusion then follows from the additivity of conditional expectations, since then $\lim_{\epsilon \downarrow 0} \mathbb{E}[Y_t|Z_l = \epsilon] - \lim_{\epsilon \uparrow 0} \mathbb{E}[Y_t|Z_l = \epsilon] = \lim_{\epsilon \downarrow 0} \mathbb{E}[Y_t(0)|Z_l = \epsilon] - \lim_{\epsilon \uparrow 0} \mathbb{E}[Y_t(0)|Z_l = \epsilon] + \sum_{j=1}^{t}(\lim_{\epsilon \downarrow 0} \mathbb{E}[\psi_t(j)|Z_l = \epsilon] - \lim_{\epsilon \uparrow 0} \mathbb{E}[\psi_t(j)|Z_l = \epsilon]) = 0$.

(b) *Case* $t = l$. Take $|\epsilon| < \bar{\epsilon}$ and $j \in \{1, \ldots, t\}$ and observe that:

$$\begin{aligned}
\mathbb{E}[\mathbf{1}\{D_t \geq j\}(Y_t(j) - Y_{t-1}(j))|Z_t = \epsilon] &= \\
\mathbb{E}[\mathbf{1}\{D_t(Z_1, \ldots, \epsilon) \geq j\}(Y_t(j) - Y_{t-1}(j))|Z_t = \epsilon] &= \\
\mathbb{E}[\mathbf{1}\{D_t(Z_1, \ldots, \epsilon) \geq j\}(Y_t(j) - Y_{t-1}(j))|Z_r = \epsilon, |Z_t| \leq |\epsilon|] &= \\
\mathbb{E}[\mathbf{1}\{D_t(Z_1, \ldots, \epsilon) \geq j\}(Y_t(j) - Y_{t-1}(j))||Z_t| \leq |\epsilon|] &,
\end{aligned}$$

where the last equality follows from Assumption 3. Consequently, taking $\epsilon \in (0, \bar{\epsilon} \wedge \check{\epsilon})$, we have:

$$\mathbb{E}[\mathbf{1}\{D_t \geq j\}(Y_t(j) - Y_{t-1}(j))|Z_t = \epsilon] - \mathbb{E}[\mathbf{1}\{D_t \geq j\}(Y_t(j) - Y_{t-1}(j))|Z_t = -\epsilon] =$$
$$\mathbb{E}[(\mathbf{1}\{D_t(Z_1, \ldots, \epsilon) \geq j\} - \mathbf{1}\{D_t(Z_1, \ldots, -\epsilon) \geq j\})(Y_t(j) - Y_{t-1}(j))||Z_t| \leq \epsilon] =$$
$$\mathbb{E}[\mathbf{1}\{D_t(Z_1, \ldots, \epsilon) \geq j, D_t(Z_1, \ldots, -\epsilon) < j\}(Y_t(j) - Y_{t-1}(j))||Z_t| \leq \epsilon]\,,$$

where the last equality follows from local monotonicity (Assumption 2). The desired conclusion then follows from $\mathbb{E}[\mathbf{1}\{D_t(Z_1, \ldots, \epsilon) \geq j, D_t(Z_1, \ldots, -\epsilon) < j\}(Y_t(j) - Y_{t-1}(j))||Z_t| \leq \epsilon] = \mathbb{P}[D_t(Z_1, \ldots, \epsilon) \geq j, D_t(Z_1, \ldots, -\epsilon) < j||Z_t| \leq \epsilon]\mathbb{E}[(Y_t(j) - Y_{t-1}(j))||Z_t| \leq \epsilon, D_t(Z_1, \ldots, \epsilon) \geq j, D_t(Z_1, \ldots, -\epsilon) < j]$ and the Assumption on the existence of limits.

(c) *Case $t > l$:* the representation in this case follows immediately from the definition of conditional expectations and the existence of limits. To see why the representation is not weakly causal, it suffices to provide an example of a setting where potential outcomes and selection into treatment satisfy Assumptions 1, 2, and 3; treatment effects have almost-surely the same sign, and the estimand has the opposite sign. We consider $3 = t > l = 1$, though similar arguments can be made for other choices of (t, l) with $t > l$. We assume that $Z_1 = \mathcal{U}[-1, 1]$ and that $Z_j = \mathbf{1}\{Z_1 > 0\}\mathcal{U}[-1, 0] + \mathbf{1}\{Z_1 < 0\}\mathcal{U}[0, 1]$ for $j \in \{2, 3\}$. We consider $\boldsymbol{D}_3(\boldsymbol{z}) = \sum_{j=1}^{3} \mathbf{1}\{z_j \geq 0\}$, and assume that $Y_t(0) = 0$ a.s., and $Y_t(j) - Y_t(j-1) = \delta_j$ a.s. for $j \in \{1, 2, 3\}$. Observe that this is a setting that satisfies Assumptions 1, 2, and 3. Specifically, it exhibits an extreme version of the "compensation effect" discussed in the main text. Using the representation in the statement of the theorem, we obtain that:

$$\tau_{3|1} = -\delta_2\,,$$

thus showing that the estimand is not weakly causal.

B.2 Alternative identification strategies

In this Appendix, we discuss alternative identification strategies to the one adopted in the main text.

B.2.1 Identification based on time-series stationarity restrictions

Suppose that, for each t, average marginal effects below and above the threshold at t are homogeneous and that these marginal effects are stationary, i.e. $\phi^+_{t|t}(j) = \phi^-_{t|t}(j) = \phi(j)$ for every $t = 1, \ldots, T$ and $j \in \{1, \ldots, t\}$. In this case, we are able to write the following system:

$$\begin{bmatrix} \omega^+_{1|1}(1) - \omega^-_{1|1}(1) & 0 & 0 & \ldots & 0 \\ \omega^+_{2|2}(1) - \omega^-_{2|2}(1) & \omega^+_{2|2}(2) - \omega^-_{2|2}(2) & 0 & \ldots & 0 \\ \vdots & \vdots & \vdots & \ldots & \vdots \\ \omega^+_{T|T}(1) - \omega^-_{T|T}(1) & \omega^+_{T|T}(2) - \omega^-_{T|T}(2) & \omega^+_{T|T}(3) - \omega^-_{T|T}(3) & \ldots & \omega^+_{T|T}(T) - \omega^-_{T|T}(t) \end{bmatrix} \begin{bmatrix} \phi(1) \\ \phi(2) \\ \vdots \\ \phi(T) \end{bmatrix} = \begin{bmatrix} \tau_{1|1} \\ \tau_{2|2} \\ \vdots \\ \tau_{T|T} \end{bmatrix} .$$

If the identified lower-triangular matrix of first-stage contrasts is invertible, which will be the case under the relevance condition $\min_{t=1,\ldots,T} |\omega^+_{t|t}(t) - \omega^-_{t|t}(t)| > 0$, then it is possible to identify the vector $(\phi(1), \ldots, \phi(T))$. Indeed, observe that, under this Assumption, marginal effects may be identified recursively. First, one identifies $\phi(1)$ by:

$$\phi(1) = \frac{\tau_{1|1}}{\omega^+_{1|1}(1) - \omega^-_{1|1}(1)} ,$$

then, one may identify $\phi(2)$ by:

$$\phi(2) = \frac{\tau_{2|2} - (\omega^+_{2|2}(1) - \omega^-_{2|2}(1))\phi(1)}{\omega^+_{2|2}(2) - \omega^-_{2|2}(2)} ,$$

and so on.

The recursive approach discussed in this section is especially suited to settings where the marginal effects of a given exposure remain stable over time. This does not seem the case in our setting, which encompasses a recovery from a sanitary emergency that rattled financial and real markets. In this case, it does not seem plausible to assume that the effect of being exposed to the LFL for one period to be the same at the beginning of our time window as well as in the end of it.

Remark 1. An alternative stationarity condition that can be used for identification is to assume that, for a fixed threshold l, $\phi^+_{t|l}(j) = \phi^-_{t|l}(j) = \phi(j)$ for every $t = 1, \ldots, T$. Under this assumption, below-and-above marginal effects around a fixed threshold are homogeneous and stationary across-time. Using this assumption, one obtains, the following triangular system:

$$\begin{bmatrix} \omega^+_{1|l}(1) - \omega^-_{1|l}(1) & 0 & 0 & \ldots & 0 \\ \omega^+_{2|l}(1) - \omega^-_{2|l}(1) & \omega^+_{2|l}(2) - \omega^-_{2|l}(2) & 0 & \ldots & 0 \\ \vdots & \vdots & \vdots & \ldots & \vdots \\ \omega^+_{T|l}(1) - \omega^-_{T|l}(1) & \omega^+_{T|l}(2) - \omega^-_{T|l}(2) & \omega^+_{T|l}(3) - \omega^-_{T|l}(3) & \ldots & \omega^+_{T|l}(T) - \omega^-_{T|l}(t) \end{bmatrix} \begin{bmatrix} \phi(1) \\ \phi(2) \\ \vdots \\ \phi(T) \end{bmatrix} = \begin{bmatrix} \tau_{1|l} \\ \tau_{2|l} \\ \vdots \\ \tau_{T|l} \end{bmatrix},$$

which is identified under a relevance condition on the main diagonal of the first-stage matrix. One advantage of this assumption is that it only requires knowledge of the score at a single period (for example, the first period score if $l = 1$).

B.2.2 Identification by conditioning on past exposures

Yet another alternative identification strategy consists in conditioning on the realisation of prior exposures, relying on the fact that $x \mapsto D_x$ is nondecreasing. Specifically, consider the following RDD estimand:

$$\tilde{\phi}_{t|t} = \frac{\lim_{\epsilon\downarrow 0} \mathbb{E}[Y_t|D_{t-1} = t-1, Z_t = \epsilon] - \lim_{\epsilon\downarrow 0} \mathbb{E}[Y_t|D_{t-1} = t-1, Z_t = \epsilon]}{\lim_{\epsilon\downarrow 0} \mathbb{E}[D_t|D_{t-1} = t-1, Z_t = \epsilon] - \lim_{\epsilon\downarrow 0} \mathbb{E}[D_t|D_{t-1} = t-1, Z_t = \epsilon]}$$

Under Assumptions 1, 2, and 3, this estimand identifies the LATE:

$$\tilde{\phi}_{t|t} = \mathbb{E}[Y_t(t) - Y_t(t-1)|\boldsymbol{D}_t(\boldsymbol{Z}_{t|t}(\epsilon)) = t, \boldsymbol{D}_t(\boldsymbol{Z}_{t|t}(-\epsilon)) = t-1, D_{t-1} = t-1],$$

i.e. we identify the average marginal effect of changing the exposure from $t-1$ to t, among the individuals around the threshold at t who had exposure prior exposure of $t-1$, and whose exposure is changed if their scores were perturbed to be marginally above the threshold (scores).

While this approach does not require homogeneity assumptions to identify marginal treatment effects, we note that it may be extremely demanding of the data for larger values of t, as we effectively restrict ourselves to the subpopulation that is at the threshold around t, but that has been always treated prior to that. If treatment adoption substantially affects future scores, moving them away from a vicinity of the threshold, we expect this subpopulation to be rather small, for a given bandwidth around the score, which would severely hinder estimation. In addition, the proposed approach only identifies marginal effects without homogeneity assumptions. If one were to identify cummulative effects, then homogeneity assumptions that allowed comparing different $\tilde{\phi}_{t|t}$ would be required. In this case, and if there are overidentifying restrictions, we believe that estimation of cummulative effects that *impose* the homogeneity assumptions – such as the ones in the main text – would be preferable on efficiency grounds.

Remark 2 (On the compliers captured by $\tilde{\phi}_{t|t}$)**.** In our empirical application, $\tilde{\phi}_{t|t}$ captures the average marginal effect among those firms that were exposed to the policy for $t-1$ periods, but remain around the threshold at t. Given that we expect the policy to operate through the relaxation of borrowing constraints, and that our running variable measures default risk, one expects this subpopulation to be precisely the one that is least affected by the policy. Therefore, from a policy perspective, this does not appear to be an interesting parameter, even if estimation risk was not a concern.

B.2.3 Comparison with other identification strategies in the literature

Cellini et al. (2010) propose a recursive identification strategy of average marginal effects in a dynamic RD design. When translated to our setting, their key identifying condition requires constant marginal effects of increasing exposure, i.e. that $Y_t(j) - Y_t(j-1)$ does not depend on j.[20] This assumption seems especially restrictive for economic outcomes over a longer time window, where one would expect "decreasing returns" to policy exposure as agents actions converge to their "steady-state" policy choice. As an alternative to Cellini et al., Hsu and Shen (2021) propose identifying marginal effects in dynamic RDDs under an assumption that, when translated to our setting with potential treatment function $\boldsymbol{D}_t(\boldsymbol{z}) = \sum_{j=1}^{t} \mathbf{1}\{z_j \geq 0\}$, would require that, for $z \in (-\epsilon, 0)$:

$$Y_2(0) \perp Z_2 | Z_1 = z\,.$$

[20]Their approach also requires an homogeneity around the threshold. See Lemma 2.1 of Hsu and Shen (2021) foir details.

Notice that this Assumption limits association between the second-period score and the unexposed second-period potential outcome, for units just below the first-period threshold. It is unlikely to be satisfied in our empirical setting, where the current period credit score is expected to correlate with the unexposed potential outcome, even after controlling for the first-period score, insofar as scores exhibit a contemporaneous association with outcomes beyond those mediate by the treatment.

B.3 Identification and optimal-estimation under (5)

B.3.1 Point Estimates and Var/Cov Matrix:

Consider the stacked model:

$$I_{o,j,t,s} = \alpha_{o,t,s} + \beta^{+}_{1,o,t,s}\mathbf{1}\{Z_{j,s} \geq c\}(Z_{j,s} - c) + \beta^{+}_{1,o,t,s}\mathbf{1}\{Z_{j,s} < c\}(Z_{j,s} - c) + \gamma_{o,t,s}\mathbf{1}\{Z_{j,s} \geq c\} + \epsilon_{j,s}\,,$$

where $I_{o,j,t,s}$ is an input $o \in \{D, Y\}$ at time t for unit j, using the jump at s. We estimate the regression coefficients by constructing a dataset stacked over o, j, t, and s, and then applying OLS with weights $K\left(\frac{Z_{j,s}-c}{h_s}\right)$. By clustering standard errors at the unit level, we account for serial correlation and obtain robust variance estimates. The estimated $\gamma_{o,t,s}$ corresponds to the jumps in both the reduced form and the first stage. The resulting joint covariance matrix is appropriate for the Delta Method. Here, h_s is a cohort-specific bandwidth, and we adjust the standard errors following Calonico et al. (2014).

In our empirical strategy, we set a fixed bandwidth, h_s, for each cohort across all time periods and outcomes. This approach ensures comparability of the same group of units across all outcomes (and time). However, the methodology can be easily extended to a more general case, where the bandwidth is allowed to vary with j, t, and s.

B.3.2 Optimal weighting scheme and variance estimator

Recall that from our identification results, the system in (4) given by:

$$\Omega_t \phi_t = \boldsymbol{\tau_t}\,,$$

Note that there exists a matrix R such that the restrictions implied by (5) can be recast as:

$$\boldsymbol{a}^* - \boldsymbol{V}^* R\beta = \mathbf{0}_{T(T+1)/2}$$

where $\boldsymbol{a}^{*'} = (\boldsymbol{\tau}_1^{'}, \boldsymbol{\tau}_2^{'}, \ldots, \boldsymbol{\tau}_T^{'})'$, and:

$$\boldsymbol{V}^* = \begin{bmatrix} \Omega_1 & \mathbf{0}_{1\times 2} & \ldots & \mathbf{0}_{1\times T} \\ \mathbf{0}_{2\times 1} & \Omega_2 & \ldots & \mathbf{0}_{2\times T} \\ \vdots & \vdots & \ddots & \vdots \\ \mathbf{0}_{T\times 1} & \mathbf{0}_{T\times 2} & \ldots & \Omega_T \end{bmatrix}$$

and β is the matrix with D parameters encoding the cumulative exposure effects, calendar time effects and cohort effects, with R being a $T(T+1)/2 \times D$ matrix appropriately piecing these cumulative effects in terms of average marginal effects.

The weighted minimum distance estimator can now be written as:

$$\min_b (\widehat{\boldsymbol{a}^*} - \widehat{\boldsymbol{V}^*} Rb)' W (\widehat{\boldsymbol{a}^*} - \widehat{\boldsymbol{V}^*} Rb),$$

with W a $T(T+1)/2 \times T(T+1)/2$ weighting matrix. This implies that:

$$\hat{b} = (R' \widehat{\boldsymbol{V}^*}' W \widehat{\boldsymbol{V}^*} R)^{-1} R' \widehat{\boldsymbol{V}^*}' W \widehat{\boldsymbol{a}^*}$$

It follows that the optimal choice of weights is given by:

$$W^* = \mathbb{V}[\widehat{\boldsymbol{a}^*} - \widehat{\boldsymbol{V}^*} R\beta]^{-1}$$

But, by the properties of row-wise vectorization:

$$\widehat{\boldsymbol{V}^*} R\beta = (\beta' R' \otimes \mathbb{I}_{T(T+1)/2}) \operatorname{vec}(\widehat{\boldsymbol{V}^*})$$

Implying that

$$(W^*)^{-1} = \mathbb{V}[\widehat{\boldsymbol{a}^*}] + (\beta' R' \otimes \mathbb{I}_{T(T+1)/2}) \mathbb{V}\left[\operatorname{vec}(\widehat{\boldsymbol{V}^*})\right] (\beta' R' \otimes \mathbb{I}_{T(T+1)/2})' - \operatorname{cov}(\widehat{\boldsymbol{a}^*}, \widehat{\boldsymbol{V}^*} R\beta) - \operatorname{cov}(\widehat{\boldsymbol{V}^*} R\beta, \widehat{\boldsymbol{a}^*})$$

But

$$\text{cov}(\widehat{\boldsymbol{a}^*}, \widehat{\boldsymbol{V}^*}R\beta) = \text{cov}(\widehat{\boldsymbol{V}^*}R\beta, \widehat{\boldsymbol{a}^*})' = \text{cov}(\widehat{\boldsymbol{a}^*}, \text{vec}(\widehat{\boldsymbol{V}^*}))\left((R\beta)' \otimes \mathbb{I}_{T(T+1)/2}\right)',$$

implying that we may estimate W^* using a preliminary estimator for β (e.g. by implementing the minimum distance estimator using identity weights). In the numerical implementation, we consider a multi-step procedure to estimate W^*. Specifically, we start by solving the minimum distance problem using the identity weights and compute a first-step estimate $\hat{W}_1$ of W^*. In the following steps, we sequentially re-estimate W^* by re-estimating β using the minimum-distance estimator that uses the previous-step estimate of the weighting matrix. We stop this procedure after 10 iterations.

Asymptotic variance formula Suppose that, for a sequence $c_n \to \infty$:

$$c_n \begin{bmatrix} (\text{vec}(\widehat{\boldsymbol{V}^*}) - \text{vec}(\boldsymbol{V}^*)) \\ (\widehat{a^*} - a^*) \end{bmatrix} \xrightarrow{d} \mathcal{N}(\mathbf{0}, \mathbb{V})$$

It is then possible to see that:

$$c_n(\hat{b} - \beta) = (R'\boldsymbol{V}^{*\prime}W\boldsymbol{V}^*R)^{-1}[R'\boldsymbol{V}^{*\prime}Wc_n(\widehat{\boldsymbol{a}^*} - \boldsymbol{a}^*) + R'c_n(\widehat{\boldsymbol{V}^*} - \boldsymbol{V}^*)'W\boldsymbol{a}^*] + o_p(1)\,.$$

i.e. we can discard the uncertainty in the inverse. But then, by noting that:

$$R'c_n(\widehat{\boldsymbol{V}^*} - \boldsymbol{V}^*)'W\boldsymbol{a}^* = ((W\boldsymbol{a}^*)' \otimes R')c_n(\text{vec}(\widehat{\boldsymbol{V}^*}) - \text{vec}(\boldsymbol{V}^*))$$

we can calculate the variance formula as

$$\begin{aligned}\mathbb{V}\left[c_n(\hat{b} - \beta)\right] = (R'\boldsymbol{V}^{*\prime}W\boldsymbol{V}^*R)^{-1}\Big[R'\boldsymbol{V}^{*\prime}W\mathbb{V}\left[c_n(\widehat{\boldsymbol{a}^*} - \boldsymbol{a}^*)\right](R'\boldsymbol{V}^{*\prime}W)' +&\\ ((W\boldsymbol{a}^*)' \otimes R')\mathbb{V}\left[c_n(\text{vec}(\widehat{\boldsymbol{V}^*}) - \text{vec}(\boldsymbol{V}^*))\right]((W\boldsymbol{a}^*)' \otimes R')' +&\\ S + S'\Big](R'\boldsymbol{V}^{*\prime}W\boldsymbol{V}^*R)^{-1\prime}&\end{aligned} \tag{21}$$

where

$$S = R'\boldsymbol{V}^{*\prime} W \operatorname{cov}(c_n(\widehat{\boldsymbol{a}^*} - \boldsymbol{a}^*), c_n(\operatorname{vec}(\widehat{\boldsymbol{V}^*}) - \operatorname{vec}(\boldsymbol{V}^*)))((W\boldsymbol{a}^*)' \otimes R')'$$

B.3.3 J-test

Under the null of correct-model specification, and, when using the plug-in estimator of the optimal weights W^*, we have:

$$(\widehat{\boldsymbol{a}^*} - \widehat{\boldsymbol{V}^*} R\hat{b})' \widehat{W^*} (\widehat{\boldsymbol{a}^*} - \widehat{\boldsymbol{V}^*} R\hat{b}) \sim \chi^2_{T(T+1)/2-D} \,,$$

which may be used in specification testing.

B.3.4 Placebo Test

To assess the robustness of our results, we conduct a placebo test by applying the same dynamic regression discontinuity design (RDD) to a pre-policy period, during which the Liquidity Facility Lines (LFL) policy had not yet been implemented. Specifically, we shift the timeline backward and consider a placebo treatment assignment based on the same running variable but prior to the actual policy rollout.

Let $\{Y_1, Y_2, \ldots, Y_T\}$ denote the observed outcomes in the post-policy period. We construct a placebo dataset by using pre-policy outcomes $\{Y_0, Y_{-1}, \ldots, Y_{-T+1}\}$ and the same running variable Z_t, while maintaining the original threshold for treatment eligibility. The placebo regression discontinuity (RD) estimand is defined as:

$$\tau_{t|l}^{\text{placebo}} = \lim_{s \downarrow 0} \mathbb{E}[Y_t^{\text{pre}} | Z_l = s] - \lim_{s \uparrow 0} \mathbb{E}[Y_t^{\text{pre}} | Z_l = s] \tag{22}$$

Under the null hypothesis of no treatment effect before the policy implementation, we expect that $\tau_{t|l}^{\text{placebo}} = 0$ for all t. If the estimated placebo effects are statistically indistinguishable from zero, this would support the validity of our identifying assumptions and confirm that the observed effects in the main analysis are not driven by pre-existing trends or spurious correlations.

For each pre-policy period t, we estimate the placebo treatment effect using the same specification as in the main analysis, controlling for the same covariates. We then test whether the placebo effects differ from zero by constructing confidence intervals around the estimates and checking whether zero lies within these intervals. In particular that implies that all cumulative treatment effects should be 0.

Formally, we test the hypothesis:

$$H_0 : \psi_t^{\text{placebo}} = 0 \quad \text{for all } t \in \{1, 2, \ldots, T\} \tag{23}$$

Failure to reject the null hypothesis is consistent with policy effects observed in the post-policy period not being confounded with pre-existing dynamics, thus making it more credible to attribute estimated effects to the actual policy intervention.

C Additional Empirical Results

C.1 Density Manipulation Tests

C.1.1 Table

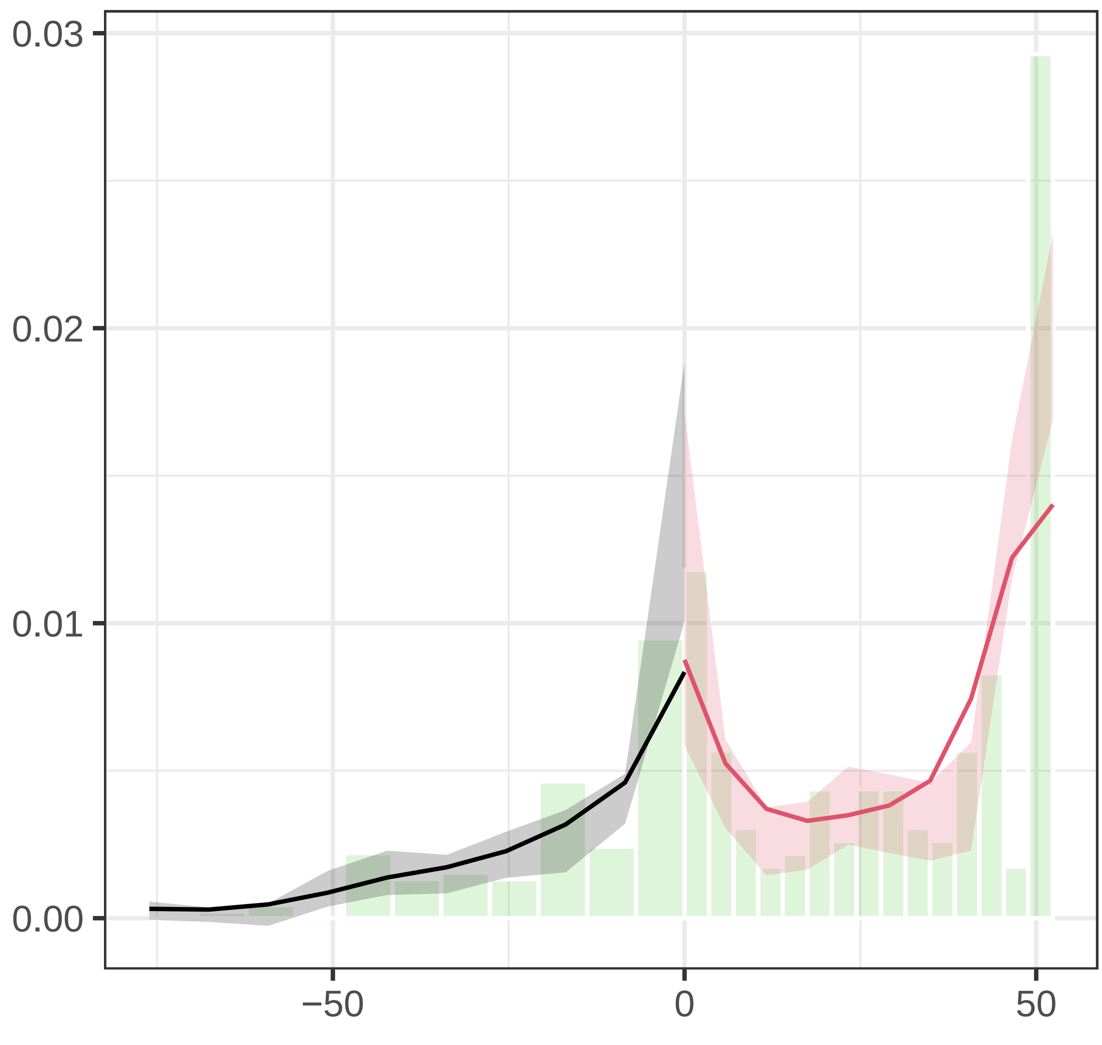
Table 3: Manipulation Test for Z_{it}

Time	Test Statistic	p-value	Eff. N Left	Eff. N Right	h_L	h_R
Nov 2021	-0.5825	0.56	115	112	25.3640	17.2677
Dec 2021	0.0910	0.93	108	126	20.8068	17.8393
Jan 2022	-0.0023	0.99	102	134	21.8986	20.4582
Feb 2022	-0.1634	0.87	106	138	22.2285	21.4362
Mar 2022	-0.4009	0.69	108	128	22.9810	19.8630
Apr 2022	-0.2123	0.83	103	130	22.3442	19.7005
May 2022	-0.4902	0.62	107	130	20.9249	20.2777
Jun 2022	-0.6521	0.51	107	128	22.5062	20.9442
Jul 2022	-0.5157	0.61	107	122	19.2556	18.2163
Aug 2022	-1.6286	0.10	120	122	20.7579	19.4576
Sep 2022	-0.3994	0.69	115	116	20.2911	17.7385
Oct 2022	-2.1302	**0.03**	115	126	18.9888	17.8679
Nov 2022	-1.6646	0.10	112	130	21.4372	20.2427
Dec 2022	-0.6888	0.49	103	126	21.4340	19.5926

Note: This table presents the manipulation test results following Cattaneo et al. (2020). Eff. N denotes the effective number of observations (those with positive weights) on each side of the threshold. Bandwidth (h) refers to the optimally chosen bandwidth for each side of the threshold. h_L and h_R are, respectively, the left and right bandwidths.

C.1.2 Manipulation Test Plot

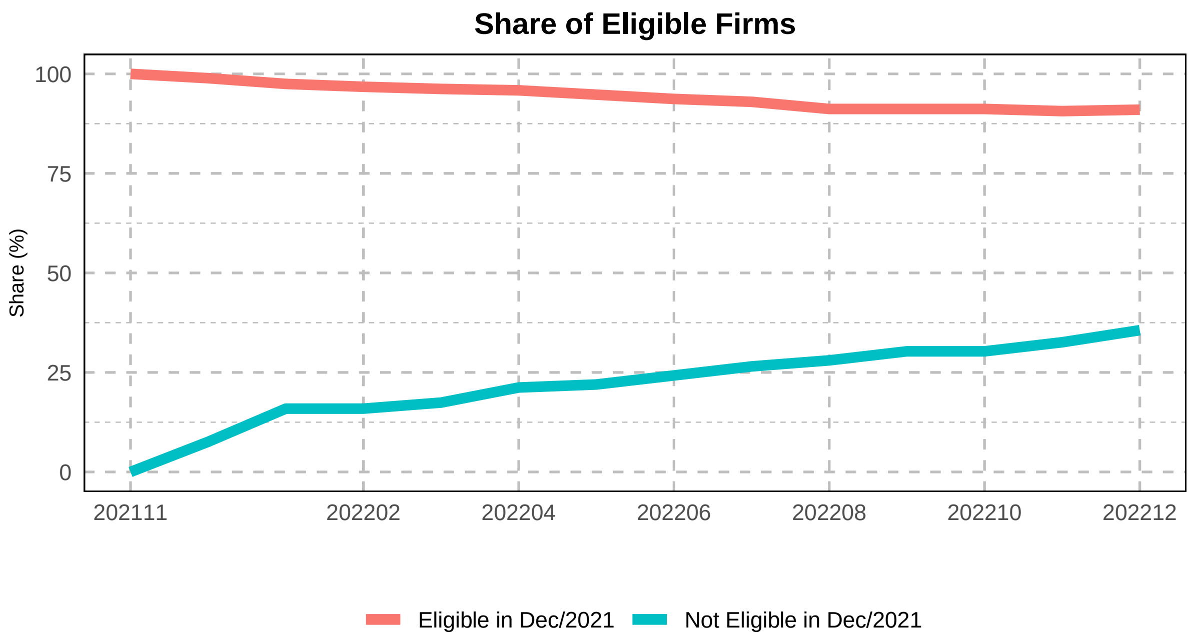
Figure 11: Time Series of Safe Asset Holdings



Note: The figure presents presents the manipulation test results following Cattaneo et al. (2020) for November 2021, when the policy started.

C.2 Evolution of eligibility over time

Figure 12: Time Series of Safe Asset Holdings



Note: The figure plots the evolution of eligibility status over time given the treatment status in November 2021, when the policy started.

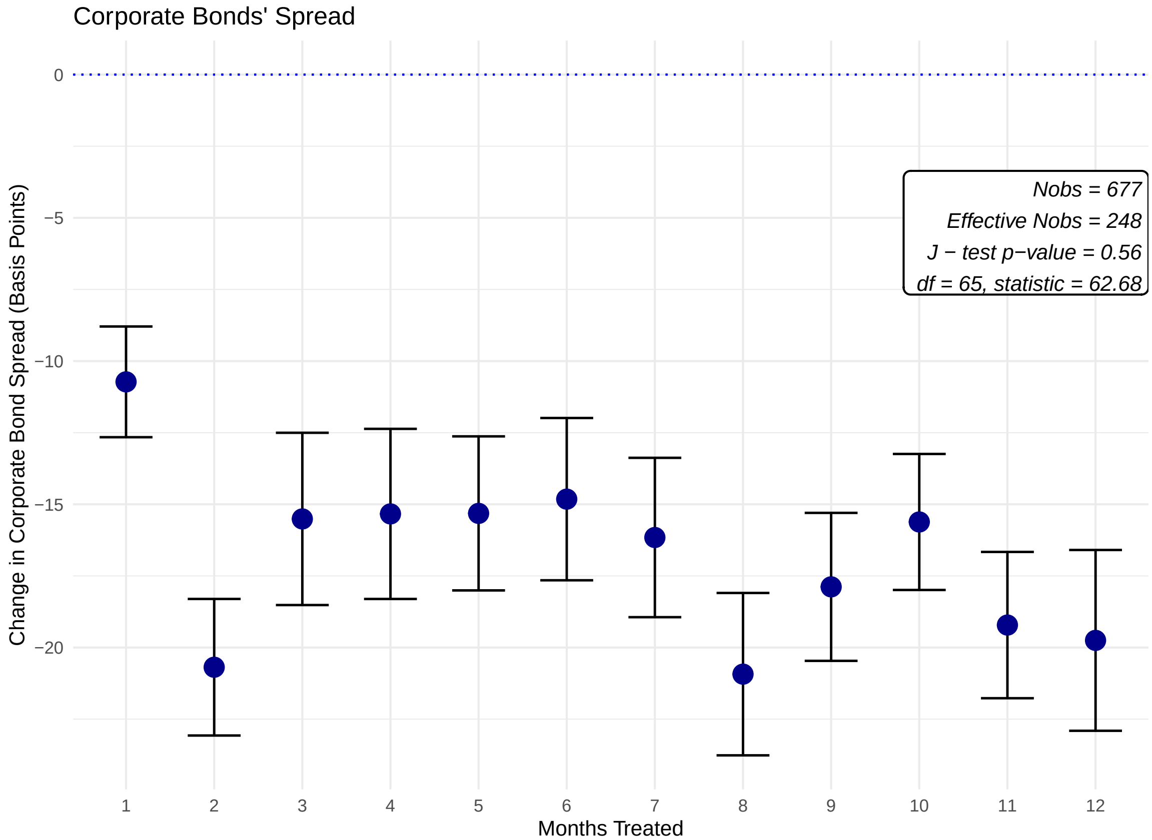
C.3 Time Series of Safe Asset Holdings

Figure 13: Time Series of Safe Asset Holdings

Note: The figure presents the total cash holdings of firms in our sample, with values normalized to 100 as of January 2020. The solid vertical line marks the month preceding the onset of Covid-19.

C.4 Effects on Cost of Capital

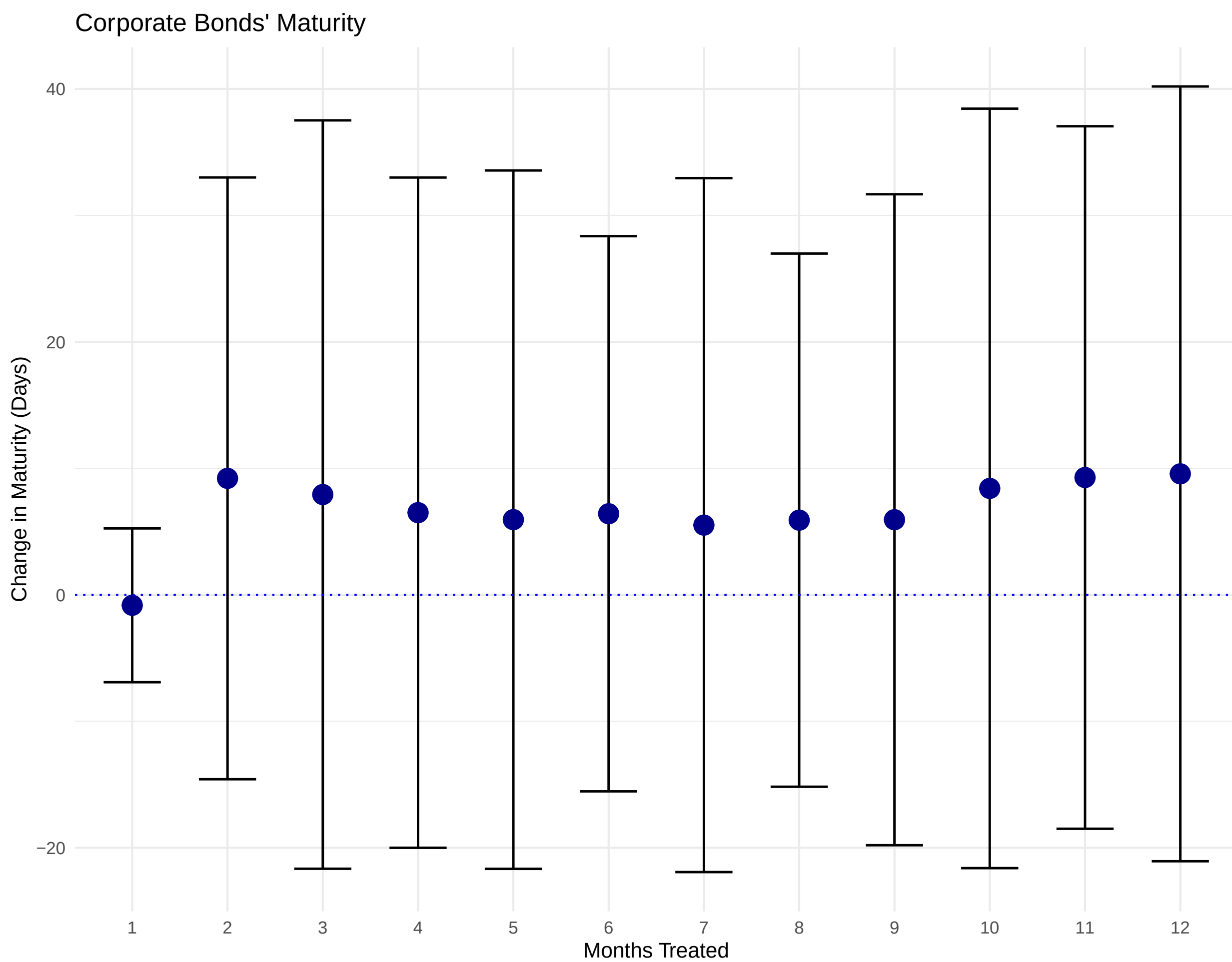
Figure 14: Dynamic Treatment Effects on Corporate Bonds Spread.



Note: The figure shows estimates of cumulative treatment effects on outstanding in the bond spread over the risk-free rate, with error bars representing 95% confidence intervals. The spread is the outstanding-volume-weighted average of credit spreads for firms' active corporate bonds.

C.5 Effects on Debt Maturity

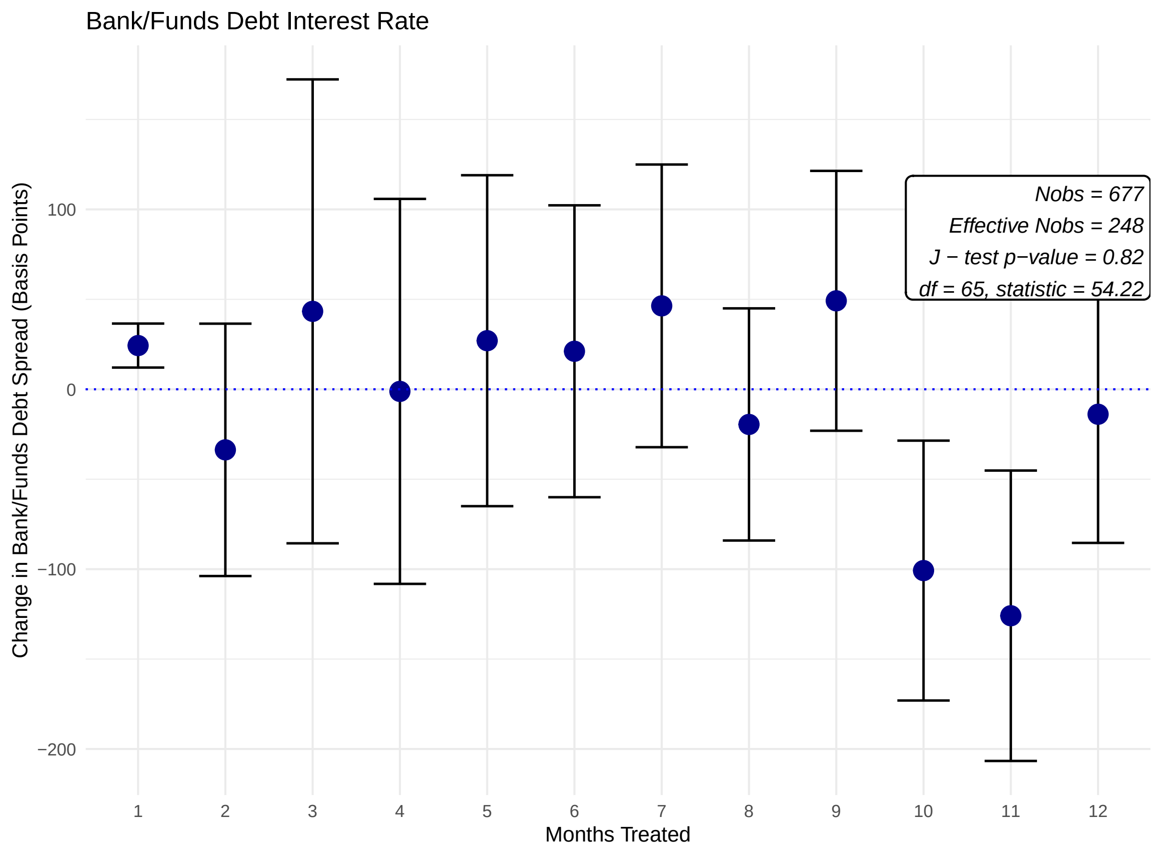
Figure 15: Dynamic Treatment Effects on Maturity



Note: The figure shows estimates of cumulative treatment effects on outstanding corporate bond debt maturity.

C.6 Effect on Interest Rates

Figure 16: Dynamic Treatment Effects on Maturity



Note: The figure shows estimates of cumulative treatment effects on outstanding bank debt (excluding corporate bonds) cost of capital.

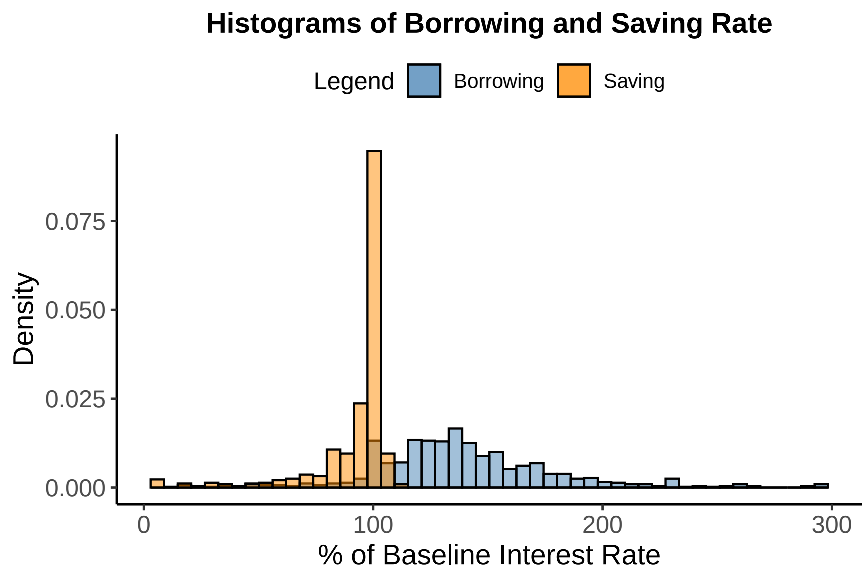


Figure 17: Dispersion of cost of capital and savings rate.

Note: The figure shows the dispersion of borrowing and savings interest rates for firms in our sample in October 2021. Borrowing rate is define as the volume-weighted average cost of capital on all intermediate debt. Savings rate is define as the total return on cash holdings. Returns are expressed in % of the (annualized) baseline interest rate.

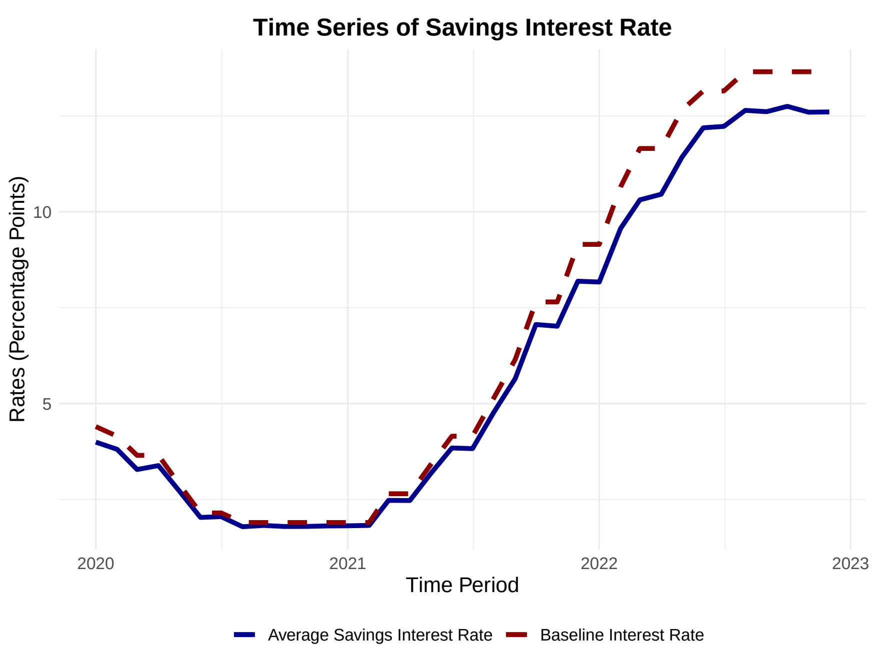


Figure 18: Time series of savings rate.

Note: The figure shows the the time series of the (volume weighted) average savings interest rates for firms in our sample. Savings rate is define as the total return on cash holdings. Returns are annualized and expressed in percentage points.

C.7 Elasticities Estimates

Table 4: Elasticity and Coefficient Estimates (Monthly Difference)

	GMM		FE-IV		FE		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β	**0.24****	0.07	0.31***	0.24*	-0.40^{***}	-0.51^{***}	0.21***	0.42***
	(0.061)	(0.13)	(0.050)	(0.118)	(0.025)	(0.084)	(0.007)	(0.033)
ε	**2.31*****	2.07***	2.46***	2.32***	1.71***	1.66***	2.27***	2.74***
	(0.117)	(0.208)	(0.121)	(0.175)	(0.012)	(0.036)	(0.011)	(0.101)
FE	YES	YES	YES	YES	YES	YES	NO	NO
N	24,204	9,405	28,201	16,872	28,201	12,875	29,083	13,626
T	ALL	PRE	ALL	PRE	ALL	PRE	ALL	PRE
J-test (p-value)	0.39	0.72	-	-	-	-	-	-

Notes: Robust standard errors in parentheses. ***p $<$ 0.001; **p $<$ 0.01; *p $<$ 0.05.

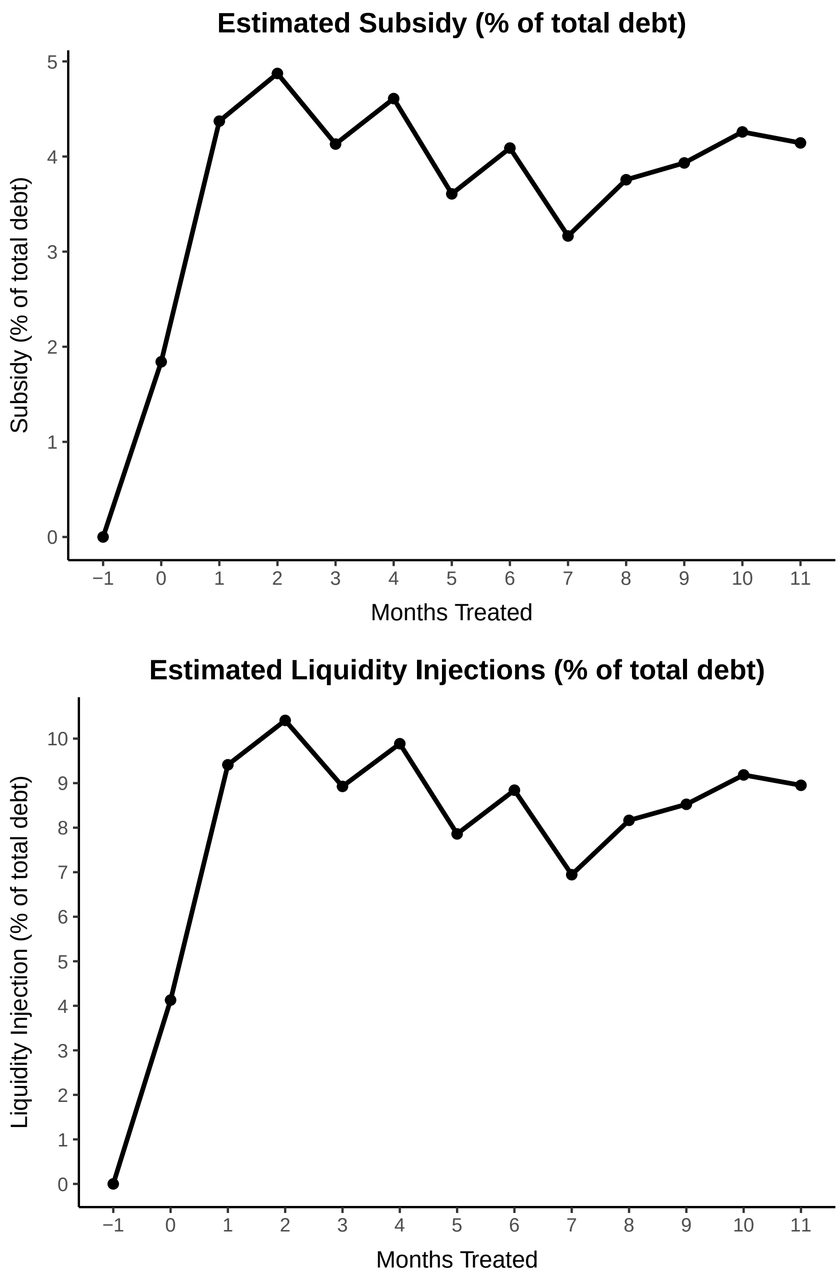
Table 5: Elasticity and Coefficient Estimates (Quarterly Difference)

	GMM	IV-FE	FE	OLS
	(1)	(2)	(3)	(4)
β	**0.23*****	0.29***	-0.39^{***}	0.18***
	(0.061)	(0.06)	(0.02)	(0.007)
ε	**2.30*****	2.41***	1.71***	2.22***
	(0.105)	(0.124)	(0.010)	(0.010)
FE	YES	YES	YES	NO
N	21,469	25,289	25,289	27,588
T	ALL	ALL	ALL	ALL
J-test (p-value)	0.33	-	-	-

Notes: Robust standard errors in parentheses. ***p $<$ 0.001; **p $<$ 0.01; *p $<$ 0.05.

C.8 Implicit Transfers Estimates

Figure 19



D Proofs and derivation - Model Section

D.1 Derivation of equation (11)

We can write the AGU problem as:

$$\max_{B_t,\{\omega_{it}\}} \mathbb{E}_t \left[\eta_b^{\frac{1}{\varepsilon}} \frac{\tilde{B}_{t+1}^{1-\gamma}}{1-\gamma} + \tilde{\tau_{c,t}} B_{t+1}^{LFL} - \tilde{\tau_{l,t}} B_{t+1}^{NLFL} - \mu(B_t) B_t \right].$$

subject to

$$\int \omega_{it} di = 1, \tag{24}$$

$$\tilde{B}_{t+1} = \left(\int \eta_i^{\frac{1}{\varepsilon}} \left(R_{i,t} \omega_{it} B_t \right)^{\frac{\varepsilon-1}{\varepsilon}} di \right)^{\frac{\varepsilon}{\varepsilon-1}}, \tag{25}$$

$$B_{t+1}^{LFL} = \left(I(i) \int \eta_i^{\frac{1}{\varepsilon}} \left(R_{i,t} \omega_{it} B_t \right)^{\frac{\varepsilon-1}{\varepsilon}} di \right)^{\frac{\varepsilon}{\varepsilon-1}}, \tag{26}$$

$$B_{t+1}^{NLFL} = \left((1 - I(i)) \int \eta_i^{\frac{1}{\varepsilon}} \left(R_{i,t} \omega_{it} B_t \right)^{\frac{\varepsilon-1}{\varepsilon}} di \right)^{\frac{\varepsilon}{\varepsilon-1}}, \tag{27}$$

where $I(i)$ is an indicator function, with value 1 if i is eligible to the policy. We can write the first-order condition with respect to ω_{it}.

$$\mathbb{E}_t \left[\left(\frac{\omega_{it}}{\eta_i} \right)^{-\frac{1}{\varepsilon}} R_{i,t}^{\frac{\varepsilon-1}{\varepsilon}} \left\{ \tilde{B}_{t+1}^{\frac{1}{\varepsilon}-\gamma} + I(i) \tilde{\tau_{c,t}} \left(\frac{B_{t+1}^{LFL}}{\eta_b} \right)^{\frac{1}{\varepsilon}} - (1 - I(i)) \tilde{\tau_{l,t}} \left(\frac{B_{t+1}^{NLFL}}{\eta_b} \right)^{\frac{1}{\varepsilon}} \right\} \right] = \frac{\lambda_t}{\eta_b^{\frac{1}{\varepsilon}} B_t^{\frac{\varepsilon-1}{\varepsilon}}}$$

Now we use the following definitions,

1. $\mathbf{1}\{ND_{i,t}\}$ in an indicator whether firm i did not default.
2. $\mathbb{P}_{i,t}$ is the default probability of firm i,
3. $S_{i,t}$ as the nominal (gross) spread over the risk-free rate R_t^f,
4. $V_{i,t}^W := \mathbb{E}_t \left[\tilde{B}_{t+1}^{\frac{1}{\varepsilon}-\gamma} \middle| ND_{i,t} \right]$,

5. $\bar{\tau_{c,t}} := \tilde{\tau_{c,t}}\mathbb{E}_t\left[\left(\frac{B_{t+1}^{LFL}}{\eta_b}\right)^{\frac{1}{\varepsilon}}\middle| ND_{i,t}\right]$, analogously defining $\bar{\tau_{l,t}}$,

combining with the property that if Y is a binary random variable, $\mathbb{E}[XY] = \mathbb{E}[X|Y=1]\mathbb{P}(Y=1)$, we can simplify the first order condition to:

$$\left(\frac{\omega_{it}}{\eta_i}\right)^{-\frac{1}{\varepsilon}} S_{i,t}^{\frac{\varepsilon-1}{\varepsilon}}\left(1-\mathbb{P}_{i,t}\right)\left(V_{i,t}^W + I(i)\bar{\tau_{c,t}} - (1-I(i))\bar{\tau_{l,t}}\right) = \frac{\lambda_t}{R_t^f \eta_b^{\frac{1}{\varepsilon}} B_t^{\frac{\varepsilon-1}{\varepsilon}}}$$

Comparing the choice between investing in an eligible versus ineligible firm, and using the definition $\omega_{i,t} = \frac{b_{i,t}}{B_t}$:

$$\frac{b_{i,t}^{LFL}}{b_{j,t}^{NLFL}} = \left(\frac{\eta_i}{\eta_j}\right)\left(\frac{S_{i,t}}{S_{j,t}}\right)^{\varepsilon-1}\left\{\left(\frac{1-\mathbb{P}_{i,t}}{1-\mathbb{P}_{j,t}}\right)\left(\frac{V_{i,t}^W + \bar{\tau}_{c,t}}{V_{j,t}^W - \bar{\tau}_{l,t}}\right)\right\}^{\varepsilon}$$

Using the assumption $\gamma = \frac{1}{\varepsilon}$, we get the desired result by defining the risk-adjusted transfers

$$\left(\frac{1+\tau_{c,i,t}}{1-\tau_{l,i,t}}\right) := \left(\frac{1-\mathbb{P}_{i,t}}{1-\mathbb{P}_{j,t}}\right)\left(\frac{1+\bar{\tau}_{c,t}}{1-\bar{\tau}_{l,t}}\right)$$

D.2 Derivation of bound (19)

First, define

$$\mathbb{P}_{l,t} := \mathbb{E}\left[\mathbb{P}_{i,t}\left(A_t^1, S_{it}, \{\bar{\tau}_{c,j}\}_{j=1}^t\right)|\text{score}_{i,1}=0\right],$$

$$\mathbb{P}_{c,t} := \mathbb{E}\left[\mathbb{P}_{i,t}(A_t^1, S_{it}, \{-\bar{\tau}_{l,j}\}_{j=1}^t)|\text{score}_{i,1}=0\right],$$

where the first is the (expected) probability of default of a marginal firm that is always eligible, and the second term is the counterfactual probability of default in case this firm were not to be eligible.

Consider the RDD estimand $\psi_t(t)$ in our empirical application, which captures the effect of being continuously exposed to the policy since its introduction in period $t = 1$. Through the lenses of our model, this estimand identifies:

$$\psi_t(t) = \mathbb{E}[P_{it}(A_t^1, S_{it}, \{\bar\tau_{c,j}\}_{j=1}^t) - P_{it}(A_t^1, S_{it}, \{-\bar\tau_{c,j}\}_{j=1}^t)|\text{score}_{i,1} = 0] \approx$$
$$\sum_{j=1}^t \mathbb{E}\left[\frac{\partial P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1}))}{\partial \bar\tau_j}\middle| \text{score}_{i,1} = 0\right](\bar\tau_{c,j} + \bar\tau_{l,j}) =$$
$$\sum_{j=1}^t \mathbb{E}\left[\frac{\partial P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1}))}{\partial \bar\tau_j}\middle| \text{score}_{i,1} = 0\right](\bar\tau_{c,j} - \mathbb{P}_{c,t} + \bar\tau_{l,j} + \mathbb{P}_{l,t} + \mathbb{P}_{c,t} - \mathbb{P}_{l,t}) \approx \tag{28}$$
$$\left(\sum_{j=1}^t \mathbb{E}\left[\frac{\partial P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1}))}{\partial \bar\tau_j}\middle| \text{score}_{i,1} = 0\right]\psi_j^\tau(j)\right) + \mathbb{R}_t$$

where we rely on first-order Taylor expansions combined with the definition of $\psi_t^\tau(t)$ to achieve the expressions, and the remainder term $\mathbb{R}_t$ can be written as:

$$\mathbb{R}_t := \sum_{j=1}^t \mathbb{E}\left[\frac{\partial P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1}))}{\partial \bar\tau_j}\middle| \text{score}_{i,1} = 0\right](\mathbb{P}_{c,t} - \mathbb{P}_{l,t}) \approx$$
$$\sum_{j=1}^t\sum_{k=1}^t \mathbb{E}\left[\frac{\partial P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1}))}{\partial \bar\tau_j}\middle| \text{score}_{i,1} = 0\right]\mathbb{E}\left[\frac{\partial \mathbb{P}_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1}))}{\partial \bar\tau_k}\middle| \text{score}_{i,1} = 0\right](\bar\tau_{c,k} + \bar\tau_{l,k}) \approx 0.$$

Where we used the fact that the probability of default is itself a endogenous choice for the firm, and that for all $1 \leq j, k \leq t$, $\mathbb{E}\left[\frac{\partial P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1}))}{\partial \bar\tau_j}\middle| \text{score}_{i,1} = 0\right]\mathbb{E}\left[\frac{\partial \mathbb{P}_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1}))}{\partial \bar\tau_k}\middle| \text{score}_{i,1} = 0\right] \approx 0$, i.e., these terms are second order. We then conclude:

$$\psi_t(t) \approx \sum_{j=1}^t \underbrace{\mathbb{E}\left[\frac{\partial P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1}))}{\partial \bar\tau_j}\middle| \text{score}_{i,1} = 0\right]}_{\Delta_j^*}\psi_j^\tau(j). \tag{29}$$

The expression above shows that, in a first-order approximation, the estimand $\psi_t(t)$ recovers the effect, for those units around the threshold at the first period, of being continuously exposed to a sequence of "effective subsidies" $\{\bar\tau_{c,j} + \bar\tau_{l,j}\}_{j=1}^t$, in an aggregate equilibrium where the policy is in place. In other words, the RD estimand captures the effects of subsidizing bond issuance in the new aggregate equilibrium induced by the policy – general equilibrium effects are not captured by the estimator since units marginally below and above the threshold are equally subject to aggregate state variables. Recall that we can identify the sequence of $\psi_t^S(t)$.

As a consequence, for an alternative path of subsidies $\{\tilde{\tau}_j\}_{j=1}^t$, the expected effect:

$$\mathbb{E}[P_{it}(A_t^1, S_{it}, \{\tilde{\tau}_j\}_{j=1}^t) - P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1})|\text{score}_{i,1} = 0] \approx \sum_{j=1}^{t} \Delta_{j,t}^* \tilde{\tau}_j \,. \tag{30}$$

The above expression shows that, to compute multipliers, it is necessary, in a first-order approximation, to identify the partial derivatives $\left\{\Delta_{j,t}^*\right\}_{t=1}^t$. While it would be possible to leverage the homogeneity assumption underlying our RDD estimator to identify these partial derivatives by relying on the path of $\psi_t^\tau(t)$ and the path of estimands $\{\psi_t(j)\}_{j=1}^t$,[21] this approach will result in noisy estimates if some of the $\psi_t(j)$ are noisily estimated.[22]As an alternative, we thus propose an approach based on bounding the treatment path by solely relying on $\psi_t(t)$. Suppose that the policy function is monotone increasing in the subsidy rate τ_j, for every $j = 1, \dots, t$. Consider any alternative policy path of transfers, $\{\tilde{\tau}_j\}_{j=1}^t$. In this case, the effect of this alternative path can be bounded above, in a first-order approximation, by:

$$\begin{aligned} &\mathbb{E}[P_{it}(A_t^1, S_{it}, \{\tilde{\tau}_j\}_{j=1}^t) - P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1})|\text{score}_{i,1} = 0] \leq \\ &\sup_{\Delta \in \mathbb{R}_+^t : \psi_t(t) = \sum_{j=1}^t \Delta_j \psi_j^\tau(j)} \sum_{j=1}^t \Delta_j \tilde{\tau}_j = \max_{j=1,\dots,t} \frac{\psi_t(t)\tilde{\tau}_j}{\psi_j^\tau(j)} \,, \end{aligned} \tag{31}$$

and similarly, we may bound this quantity below by:

$$\begin{aligned} &\mathbb{E}[P_{it}(A_t^1, S_{it}, \{\tilde{\tau}_j\}_{j=1}^t) - P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1})|\text{score}_{i,1} = 0] \geq \\ &\inf_{\Delta \in \mathbb{R}_+^t : \psi_t(t) = \sum_{j=1}^t \Delta_j \psi_j^\tau(j)} \sum_{j=1}^t \Delta_j \tilde{\tau}_j = \min_{j=1,\dots,t} \frac{\psi_t(t)\tilde{\tau}_j}{\psi_j^\tau(j)} \,. \end{aligned} \tag{32}$$

[21]Indeed, consider the estimand $\psi_t(s)$ that captures the effect of being exposed to the policy for the s last periods, by period t. Under the cross-threshold homogeneity condition underlying our RD strategy, and through the lenses of our model, this estimand identifies:

$$\begin{aligned} \psi_t(s) = \mathbb{E}[P_{it}(A_t^1, S_{it}, \{-\bar{\tau}_{l,j}\}_{j=1}^{t-s} \cup \{\bar{\tau}_{c,j}\}_{j=t-s+1}^t) - P_{it}(A_t^1, S_{it}, \{-\tau_{l,j}\}_{j=1}^t)|\text{score}_{i,1} = 0] \approx \\ \sum_{j=t-s+1}^{t} \mathbb{E}\left[\frac{\partial P_{it}(A_t^1, S_{it}, \mathbf{0}_{t\times 1})}{\partial \tau_j}\Big| \text{score}_{i,1} = 0\right] \log\left(\frac{1+\tau_{c,j}}{1-\tau_{l,j}}\right) \,. \end{aligned}$$

By computing this formula for $s = 1, \dots t$, and using the fact that the path of implied subsidies $\left\{\log\left(\frac{1+\tau_{c,j}}{1-\tau_{l,j}}\right)\right\}_{j=1}^t$ is identified, we can recover the partial derivatives recursively $\Delta_{j,t}^*$.

[22]This will be the case if the subpopulation (cohort) that starts treatment at period $t-j+1$ are small.

D.3 Firm Model

Here, we specify a firm model where the condition (12) holds.

D.3.1 Setup

Intermediary goods producers and liquidity risk Intermediary firms are indexed by $i \in [0, 1]$, and are owned by a representative household. Each firm consists of a production function given by

$$y_{it} = a_{it} k_{it}^{\alpha} n_{it}^{1-\alpha},$$

where a_{it}, k_{it} and n_{it} are productivity, capital stock and labor, respectively. For simplicity, we assume that labor is hired at wage w_t. Firms can access a risk-free asset with (gross) return R_{it}^{f}, and m_{it} is the firms' holdings of the risk-free asset. Firms can issue debt $q_{it} b_{it}$, and firms distribute (real) dividends to the representative household d_{it}. For simplicity, we assume firms can commit to repay debt.

Firms are subject to liquidity risk in the following sense. At period $t-1$, firms hire both next period labor $l_{i,t}$ at wage w_t, invest in capital $k_{i,t}$, and have to repay b_{it}. At t, firms are subject to a productivity shock, i.e., $a_{i,t}$ is stochastic. We define the firm working capital as

$$W_{it} = p_{it} y_{it} + m_{it} - w_t l_{it} - b_{it}.$$

We model liquidity risk in analogy to the "buffer-stock" idea, where if firms do not have enough cash on hand to pay obligations, they have to liquidate capital stock. We model that in reduced form by assuming after the productivity shock is realized, production takes place and firm decides on borrowing, the remaining capital is given by $\gamma\left(W_{it} + q_{it} b_{it+1}\right) k_{it}$, where $\gamma(x)$ is a continuously differentiable function such that:

$$\gamma'(x) > 0, \quad \gamma(0) = (1-\delta), \quad \lim_{x \to -\infty} \gamma(x) = 0, \quad \lim_{x \to +\infty} \gamma(x) = (1-\bar{\delta}), \quad 0 < \delta < \bar{\delta} < 1.$$

Final Goods Producer Final goods producer operates in a competitive market with technology given by:

$$Y_t = \int \left(y_{it}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}} di.$$

Intermediate goods demand is given by,

$$y_{it} = p_{it}^{-\sigma},$$

where the price index $P_t := \left(\int p_{it}^{1-\sigma} di\right)^{\frac{1}{1-\sigma}}$, is normalized to 1.

Asset Demand We follow the financial block from Section 5.1.1

$$q_{i,t} = b_{it+1}^{-\frac{1}{\varepsilon-1}} (1+\bar{\tau_{i,t}})^{\frac{\varepsilon}{\varepsilon-1}} \frac{B_t^{\frac{1}{\varepsilon-1}}}{R_t^B},$$

where B_t is a measure of aggregate corporate debt, and R_t^B is an aggregate (gross) return of corporate debt.

Representative Household From the household block, we assume only the existence of a well-defined stochastic discount factor, Λ_{t+1}, which the firm takes as given in its valuation.

D.3.2 Firm Maximization Problem

The firm maximization problem is given by (where we drop the i subscript),

$$v_t(k_t, a_t, l_t, W_{it}, X_t) = \max_{l_{t+1}, k_{t+1}, b_{t+1}, m_{t+1}} d_t + \mathbb{E}_t \left[\Lambda_{t+1} v_{t+1}(k_{t+1}, l_{t+1}, a_{t+1}, W_{it+1}, X_{t+1}) \right]$$

subject to

$$W_{t+1} = p_{t+1} y_{it+1} + m_{t+1} - w_{t+1} l_{t+1} - b_{t+1}$$

$$\frac{y_t}{Y_t} = p_t^{-\sigma},$$

$$y_t = a_t k_t^{\alpha} l_t^{1-\alpha},$$

$$q_t = b_{t+1}^{-\frac{1}{\varepsilon-1}} (1+\tau_t)^{\left(\frac{\varepsilon}{\varepsilon-1}\right)} \frac{B_t^{\frac{1}{\varepsilon-1}}}{R_t^B},$$

$$d_t \leq W_t + q_t b_{t+1} + \gamma \left(W_t + q_t b_{t+1} \right) k_t - k_{t+1} - \frac{m_{t+1}}{R_t^f},$$

$$b_{t+1} \geq 0, \quad m_{t+1} \geq 0, \quad k_{t+1} \geq 0$$

where X_t captures the exogenous (from the perspective of the firms) aggregate state variables, $X_t := (B_t, R_t^B, R_t^f, w_t)$. Condition (12) can then be derived by combining the first order conditions with respect to debt b_{t+1}, cash-holdings m_{t+1} and the envelope theorem and will hold with equality when firms choose an interior solution for both variables, i.e., they have positive debt and savings at a given point in time.

E Simple Model - Borrowing constraints and Cash Holdings

This section presents a simple model exploring how firms manage liquidity risk under borrowing constraints. Firms risk default if they fail to produce enough intermediate output to cover costs. A reduction in borrowing constraints enables capacity expansion while decreasing cash holdings. The model suggests that empirical results are consistent with easing these constraints, with real effects amplified by the endogenous decision of firms to reduce cash buffers due to a lower need for precautionary savings.

E.1 Setup

We consider a three-period model, $t = 0, 1, 2$. At period $t = 0$ firms decide on a project consisting of inputs k that produces a total amount of output $y = k^{\alpha}$, $0 < \alpha < 1$. There is a risk-free asset (cash). Firms start without cash, $m_0 = 0$, and cannot issue equity. Input unit cost is q and requires a fraction δ of payment upfront, rest must be paid at period 1.

Firms need external finance to conduct the project, all credit is due at period 2. At period 0 firms can issue "long-term" corporate bonds, at a price q^b, subject to the following borrowing constraint:

$$b \leq \bar{b},$$

where $\bar{b} > 0$ is an exogenous borrowing limit.

Firms face liquidity risk in the sense that receive only θy of the project total output in period 1, where $\theta \sim \mathcal{U}[0, 1]$ is realized in period 1. The firm receives the remaining $R(1-\theta)y$ in period 2, where we assume the return is interest rate adjusted to make the shock a pure liquidity shock. After the realization of θ firms can borrow to cover working capital (wages) expenses and are subject to a borrowing limit:

$$Rl \leq \lambda(1-\theta)Ry_2,$$

where $\lambda \in [0, 1]$. If the firm does not have enough funds to pay inputs, $y_2 = 0$ and $y_2 = y$ otherwise.[23] Firms can hold cash, and the rate of return is $R > 1$.

Debt is priced competitively by risk-neutral lenders, such that $q^b = \mathbb{P}(b, k)$ and $q^l = 1$, where $\mathbb{P}(\cdot)$ is the probability of repaying debt at period 2. The firm is risk-neutral and maximizes expected returns in period 2.

E.2 Solution

We start by analyzing firm behavior in period 1 to solve the model. It is enough to restrict to the case where the borrowing constraint binds at period 1 since the borrowing and saving interest rates are the same. Conditional on the choices from period 0 and the realization of θ, firm default if and only if:

$$\theta k^\alpha + m_1 + \lambda(1-\theta)k^\alpha - q(1-\delta)k \leq 0,$$

thus we can define the default threshold, firm default if and only if

$$\theta \leq \bar{\theta}(k, m_1) := \frac{q(1-\delta)k - \lambda k^\alpha - m_1}{(1-\lambda)k^\alpha}.$$

With this characterization, we can simplify the firm's maximization problem, summarized in Lemma 1. Cash holdings after period 1 are given by $m_1 = \mathbb{P}(\theta \geq \bar{\theta})b - \delta qk$.

Lemma 1. *1. Firm's choice is characterized by the solution to the optimization problem:*

$$\begin{aligned}
&\max_{b,k,\bar{\theta}} A(\bar{\theta})k^\alpha - qk - S(\bar{\theta})b \\
&s.t. \;\; b \leq \bar{b}, \\
&A(\bar{\theta}) = \mathbb{P}(\theta \geq \bar{\theta}) + (1 - \mathbb{P}(\theta \geq \bar{\theta}))\mathbb{E}[\theta | \theta \leq \bar{\theta}], \\
&S(\bar{\theta}) = 1 - \mathbb{P}(\theta \geq \bar{\theta}), \\
&\bar{\theta} = \frac{qk - \lambda k^\alpha - \mathbb{P}(\theta \geq \bar{\theta})b}{(1-\lambda)k^\alpha}.
\end{aligned}$$

[23] In this model, y_2 is potentially a function of l, which may lead to multiple equilibria. When this is the case we assume the best equilibrium is played (the maximum possible y_2).

2. If $\bar{\theta}^* = 0$, then the optimal choice of capital and cash holdings are defined implicitly by:

$$qk^* - \lambda k^{*\alpha} = \bar{b}.$$

$$m_1^* = (1-\delta)qk^* - \lambda k^{*\alpha}$$

E.3 Comparative Statics

From Lemma 1 we can derive the following comparative statics:

Lemma 2. *1.*

$$\frac{dk^*}{d\lambda} > 0, \quad \frac{dm_1^*}{d\lambda} < 0, \quad \frac{d\bar{\theta}}{d\lambda} = 0$$

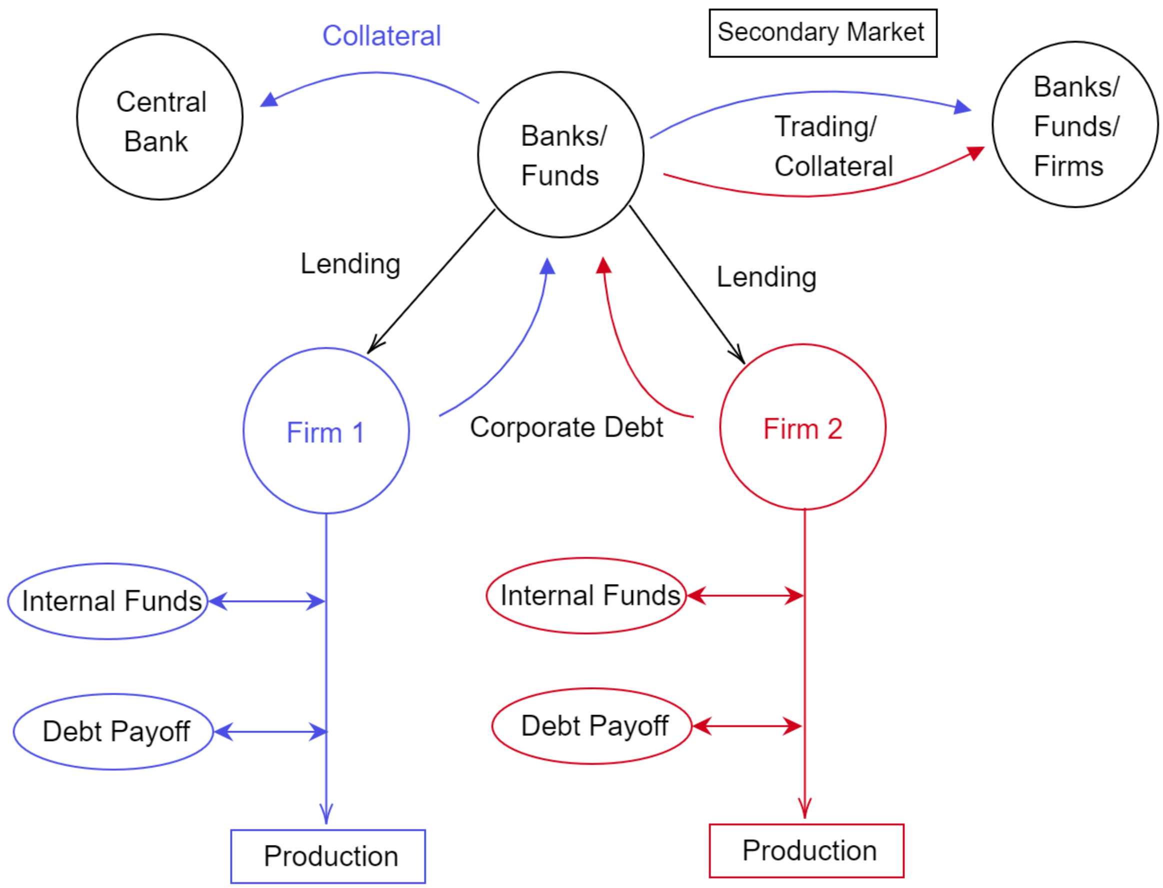
2.

$$\frac{dk^*}{d\bar{b}} > 0, \quad \frac{dm_1^*}{d\bar{b}} > 0, \quad \frac{d\bar{\theta}}{d\bar{b}} = 0$$

The model illustrates how firms manage liquidity risk under borrowing constraints. Firms face default if they lack sufficient intermediate output to cover costs. Higher liquidity allows for increased capital investment but reduces cash holdings.

F Diagram of the empirical analysis

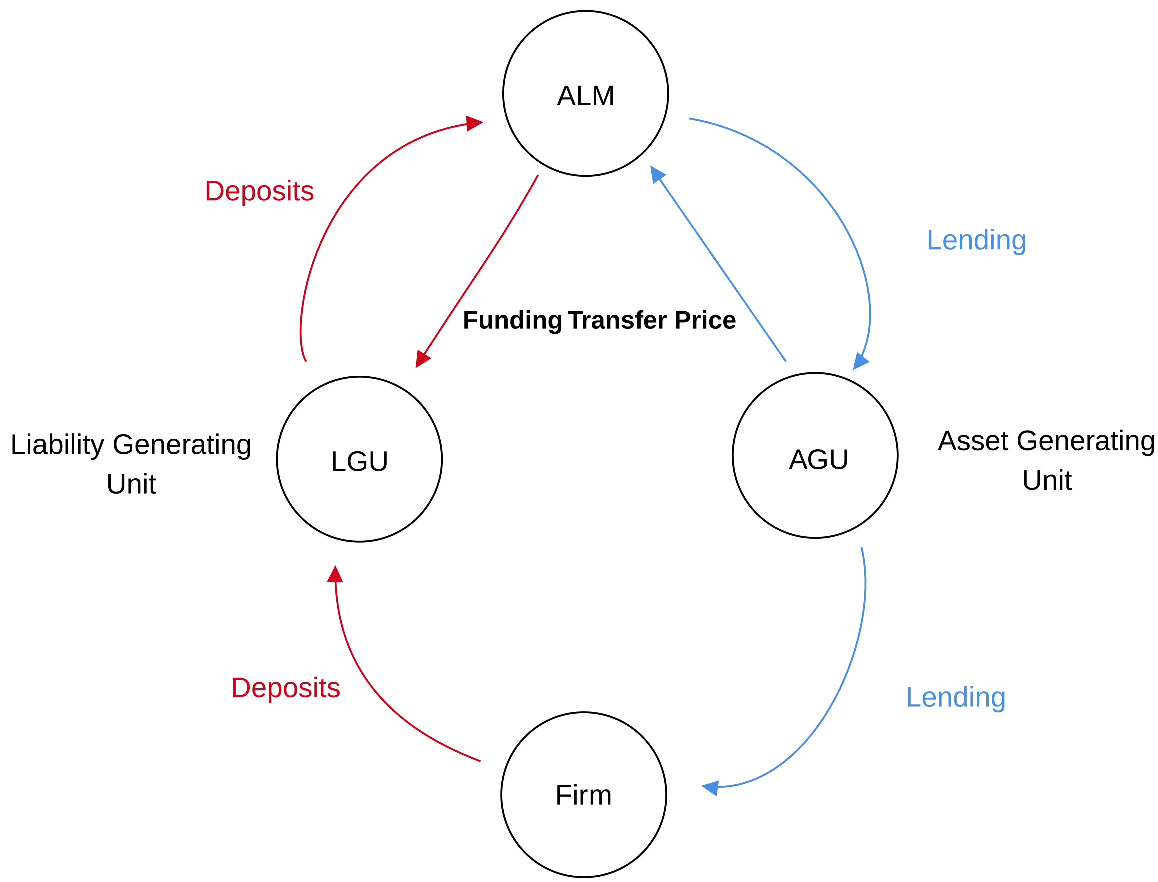
Figure 20: Diagram Empirical Analysis



Note: This diagram illustrates in a simplified way the identification and empirical strategy, as well as the connection between the empirical results.

G Diagram of theoretical model

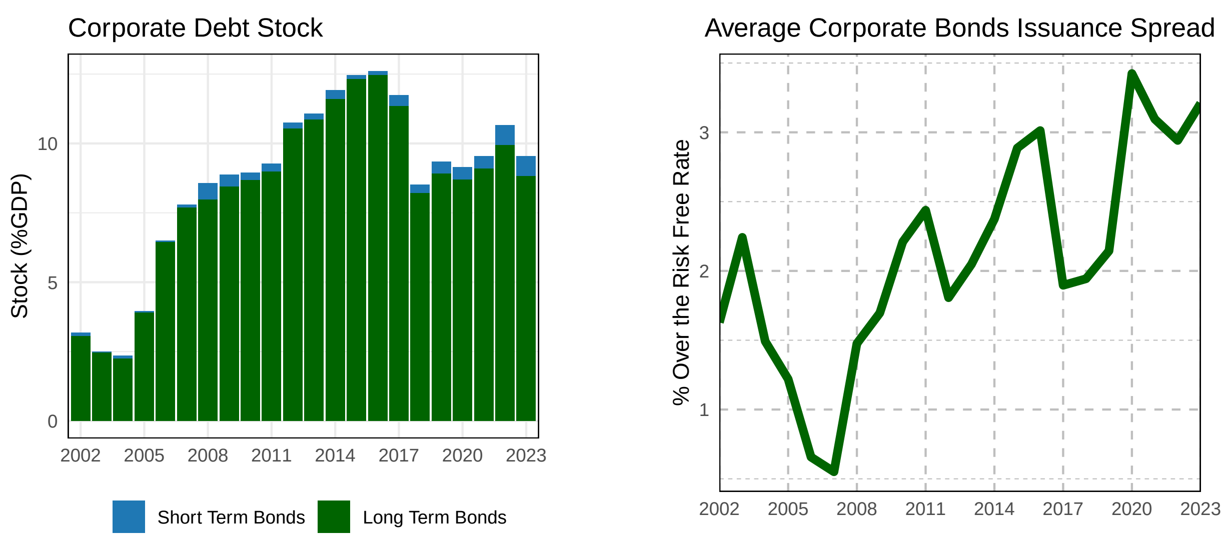
Figure 21: Model Diagram



H Corporate Bonds Market In Brazil

As of December 2023, Brazil's corporate bonds accounted for about 10% of GDP (20% of total credit to nonfinancial corporations). In comparison, the US corporate bonds market accounted for approximately 40% of GDP and 42% of total credit. In Brazil, there are two types of corporate bonds: long-term (*Debêntures*) and short-term (*Notas Comerciais*). The left panel on Figure 22 shows the time series evolution of the outstanding corporate debt (as a share of GDP). The debt stock is primarily in long-term bonds, but the past two years have shown an increase in short-term bonds following a regulatory reform in late 2021. The low share of GDP for Brazil's corporate bonds market is due to the overall low share of credit to GDP, yet the market remains relatively important, especially for larger firms.

Figure 22: Evolution of Corporate Bonds Market.

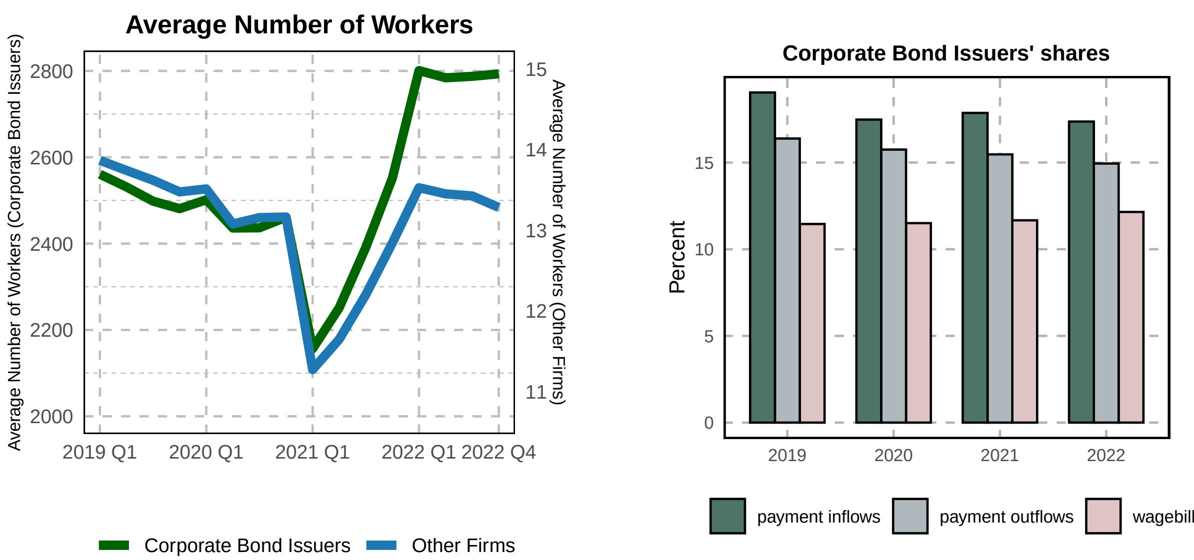


Note: The graph shows the evolution of corporate bond stock and spreads over time. Stocks are calculated on the last business day of each year. Issuance spreads are the (simple) average of spreads on assets linked to the baseline interest rate, which represents about 70% of the issuance volume and number in the sample period. Data sources: B3 (Short Term Bonds) and ANBIMA (Long Term Bonds).

The average spread over the baseline interest rate (risk-free rate in the inter-bank market) on corporate bonds is notably low, around 3.2% in 2023 for interest-rate indexed bonds, compared to an aggregate spread of 7.5% on all corporate credit. The right panel of Figure 22 shows the time series evolution of these bond spreads. Unlike the US, where most corporate bonds are nominal liabilities (see Gomes et al. (2016)), 70% of Brazil's corporate bonds are indexed to the baseline interest rate, and 23% to the inflation rate.

Corporate bond issuers are usually the largest firms in the economy, with more access to credit and significant roles in labor markets and the supply chain. By the end of 2022, these firms employed an average of 2,800 workers, compared to 13 in the rest of the economy. They represented 12% of the formal labor share and 15% of payment flows between non-financial firms, which measures their importance in intermediary goods and services transactions. Figure 23 shows the time series of firm size, labor share, and payment share. Notably, corporate bond issuers experienced a smaller decline in labor force during COVID and a significantly stronger recovery compared to other firms in the economy.

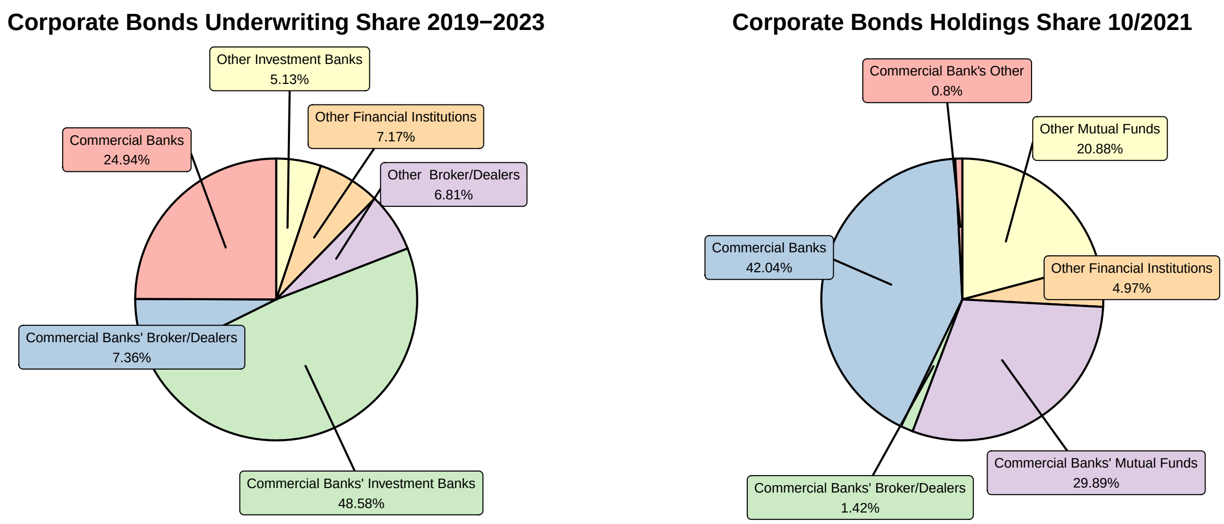
Figure 23: Corporate Bond Issuers Size.



Note: The graph shows the average number of workers, the labor share, and the payment share in inter-firm transactions by corporate bond issuers. The labor share represents the share of the wage bill for formal workers in the last quarter of the respective year. The payment share refers to the share of total electronic payments (invoices, bank transfers, and PIX) between non-financial firms in the respective year. Data Sources: B3 and ANBIMA (Corporate Bond Issuers), RAIS (wage bill and number of workers), and BCB - Department of Banking Operations and Payment Systems (electronic payments).

Corporate bonds can be traded in the secondary market, but liquidity is low, with most bonds rarely or never traded (Carvalho and Marques (2020)). The market is highly concentrated, and dominated by commercial banks. Figure 24 shows that the 7 largest commercial banks and their subsidiaries account for 81% of corporate bond underwriting and 75% of holdings (in October 2021). These banks often follow a "buy-and-hold" strategy, allocating bonds to their funds or own books, which likely contributes to the low liquidity in secondary markets. As a result of this market structure, over half of commercial bonds are not even rated by rating agencies (Carvalho and Marques (2020)).

Figure 24: Corporate Bonds Market Structure.



Note: The graph displays the underwriting and holdings share of local currency-denominated corporate bonds for Brazil's 7 largest commercial banks, their subsidiaries, and other financial institutions. Underwriting shares are based on total issuance value from January 2019 to December 2023, while holdings shares are based on the reported book value of each corporate bond on the last business day of October 2021. A fund is considered a subsidiary of a commercial bank if managed by one of its companies. Data sources: ANBIMA (Underwriting), B3 (Holdings), IRS and CVM (Ownership and Fund Management).

Commercial banks have incentives to hold corporate bonds in their portfolios because a significant portion of their deposits (68% as of December 2023) are term deposits with returns indexed to the baseline interest rate. This creates a natural hedge for their deposit franchise, similar to the US market where banks lend long-term on fixed rates as discussed by Drechsler et al. (2021). Additionally, banks can generate service flow from these bonds by using them in repo operations with noncommercial firms, which provides both liquidity benefits and tax advantages, as these transactions are exempt from the financial transaction tax that applies to term deposits.[24]

[24]Holdings microdata shows that repo operations are exclusively conducted with large companies whose deposits exceed insurance limits. Backing these operations with unliquidated long-term assets can substitute for deposit insurance by reducing the incentive for runs Payne and Weiss (2023), providing additional liquidity benefits. Moreover, repo operations are not classified as deposits and face fewer regulatory constraints. In the Brazilian market, almost all corporate bond holdings by nonfinancial institutions are short-term with resale obligations, making them essentially cash-like assets for firms. This contrasts with the practices of large firms in the U.S. market. discussed by Darmouni and Mota (2023), where cross-border tax incentives drive corporate holdings of bonds.