Temporary Liquidity and Firm Investment: The Large Effects of Corporate Loan Moratorium in a Crisis[1]

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Abstract

We study how firms respond to temporary liquidity relief and which policies can support investment during crises. We exploit a large-scale debt moratorium introduced in Austria in 2020, which allowed firms to temporarily suspend loan repayments. Exploiting size-based eligibility in a difference-in-differences design, we estimate a marginal propensity to invest of 76 cents per euro of postponed loan repayments among moratorium takers. Of the eligible firms, 44% participate. The moratorium increases investment and profitability without raising defaults once repayments resume. These empirical findings allow us to discipline a dynamic investment model with financing frictions. We compare moratoria with grants, which provide liquidity that firms do not need to repay. Unlike grants, moratoria induce virtuous self-selection by firms with profitable investment opportunities. In the model, moratoria generate investment responses comparable to those of grants, but at a substantially lower fiscal cost. Our findings suggest that moratoria offer fiscally constrained governments a cost-effective tool to stimulate investment during crises.

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1 Introduction

In the frictionless world of Modigliani and Miller 1958, liquidity plays no role in investment decisions. However, financial frictions may lead liquidity to impact investment. A long-standing corporate finance literature documents that investment responds positively to liquidity injections, exploiting cash windfalls or losses (Blanchard, Lopez-de-Silanes, and Shleifer 1994, Lamont 1997, Rauh 2006, Chodorow-Reich et al. 2026). Yet these cash shocks affect both firms' liquidity and wealth, leaving open the question of whether temporary lending alone can stimulate investment.

This paper exploits a large-scale debt moratorium implemented in Austria during the 2020 downturn to study how temporary liquidity affects corporate investment. Moratoria, a widely used crisis-relief tool, allow firms to suspend loan repayments upon request. While banks are required to grant the suspension, participation is voluntary for firms. Deferred payments continue to accrue interest and must eventually be repaid, providing liquidity without a corresponding wealth transfer. This setting allows us to estimate firms' marginal propensity to invest out of temporary liquidity and test whether relaxing financing constraints stimulates real investment.

By the end of the policy, participating firms receive an average of EUR 74,000 in liquidity relief over an 8-month horizon, equivalent to 5% of their pre-policy cost of goods sold. Of the eligible firms, 44% participate. Exploiting an eligibility cutoff based on pre-policy firm size, we find that each euro of postponed repayments increases investment by 76 cents. This effect reflects higher investment rather than reduced divestment. Eligible firms also become more profitable, without an increase in defaults after repayments resume.

Our empirical estimates of the marginal propensity to invest and policy take-up discipline a dynamic investment model, which we use to compare moratoria with grants. Unlike moratoria, which create future repayment obligations, grants increase both firms' liquidity and wealth. The model delivers a central insight: while grants are untargeted, moratoria induce virtuous self-selection among firms with profitable investment opportunities. However, this targeting advantage comes with a trade-off between selecting firms with profitable investment opportunities and maximizing the investment response. As a result, moratoria can generate a comparable increase in investment at a significantly lower fiscal cost.

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Turning to the details of our paper, we focus on the Austrian moratorium because of its sharp empirical setting: to the best of our knowledge, it was the only policy specifically targeting firms with pre-policy assets smaller than EUR 2 million. This cutoff offers three advantages. First, eligibility based on pre-policy size creates a quasi-experimental setting in which treatment around the threshold is as good as randomly assigned, allowing us to estimate the causal effects of the policy. Second, it allows us to study an important yet historically understudied segment of the economy. In Austria, firms with assets below EUR 2 million accounted for 87% of businesses, 25% of employment, and 19% of aggregate investment in 2019. Similarly, in the United States, firms with fewer than ten employees account for nearly 80% of employer firms and roughly 10% of employment. Third, while several other credit relief policies were available to Austrian firms during the same period, none shared the moratorium's eligibility cutoff, alleviating concerns about confounding effects from other relief programs.

We assemble a novel database combining three complementary sources: balance-sheet data from Orbis, administrative firm data from the Austrian Microdata Center (AMDC), and loan-level information from AnaCredit, the Eurosystem credit registry covering firm-bank credit exposures above EUR 25,000. While Orbis can be linked separately to both AMDC and AnaCredit, the latter two datasets cannot be directly merged. Together, these complementary sources allow us to study firms' financing, investment, employment, and survival, providing a comprehensive picture of their responses to the policy.

Our empirical analysis proceeds in two parts. First, we select a narrow window around the eligibility cutoff and estimate intent-to-treat effects using a difference-in-differences (DiD) framework. Second, we build on these estimates to quantify how firms allocate each euro of freed-up liquidity. To this end, we construct a Wald estimator that scales the reduced-form investment effect by the first-stage effect on our proxy for liquidity freed up by the moratorium. We find that, by the end of the policy, eligible firms postpone repayments amounting to 2% of their pre-policy assets, corresponding to approximately EUR 74,000 of freed-up liquidity per firm that took up the policy. Each euro of postponed repayments increases investment by 76 cents, suggesting that small firms faced substantial financing constraints during the crisis. This finding is also consistent with firms having profitable investment opportunities, as eligible firms experience a larger increase in profits than ineligible firms. Rather than distributing the additional liquidity to owners or shareholders, eligible firms reduce payouts relative to comparable ineligible firms, in contrast to evidence from loan forgiveness programs (Autor et al. (2022b), Autor et al. (2022a)). This decline may reflect investment indivisibilities, a mechanism highlighted by Zwick and Mahon (2017) in the context of

bonus depreciation for firms and by Kaboski and Townsend (2011) in the context of a microcredit intervention for households. Debt increases mechanically as repayments are postponed, but we find no evidence of higher defaults once repayments resume.

One may wonder why not all firms use the moratorium since it appears to be an NPV-neutral source of liquidity. First, under financing constraints, postponed payments reduce future borrowing capacity. Second, we propose a non-pecuniary reason. Firms may fear that participation signals financial distress to lenders, generating a reputational cost. We find no evidence that moratorium use impairs subsequent access to credit, but banks are more likely to classify eligible firms as "unlikely to pay" during the policy. Anecdotal evidence also corroborates concerns about stigma. By contrast, limited awareness is unlikely to explain the incomplete take-up: banks actively informed eligible borrowers about the program, and participation does not increase with firm size. Red tape is also unlikely to explain participation, as applying required only a short online form.

While policies other than moratoria were discussed and implemented during the recent crisis, the lack of clean identification strategies for these policies limits their evaluation. Hence, we exploit the empirical findings from moratoria to discipline a model of investment with financial frictions, which we then use to study grants, an alternative policy widely used in times of distress. In particular, we use the empirical moments on the take-up rate and the marginal propensity to invest to calibrate the model internally.

Comparing moratoria with counterfactual grants that provide the same amount of liquidity reveals a trade-off: while moratoria induce virtuous self-selection, grants generate greater investment. The key difference is that grants provide liquidity at no cost to firms, inducing full take-up. Since moratoria are costly, they induce self-selection: firms with positive-NPV investment opportunities opt in when the credit crunch prevents them from accessing traditional bank financing. In other words, moratoria generate a separating equilibrium that grants cannot replicate. At the same time, the obligation to repay limits the liquidity available for future investment and reduces firm value relative to grants. The trade-off is therefore between targeting liquidity toward firms with profitable investment opportunities and maximizing the investment response. To investigate this tension, we consider two additional counterfactuals. First, we impose moratoria on all firms that would receive grants, eliminating voluntary selection. Second, we give grants to the firms that took up the moratorium, holding take-up fixed - an exercise we call *targeted grants*. The first exercise generates the smallest MPI, showing that the strength of moratoria lies in self-selection. By contrast, *targeted grants* generate, per euro of relief, the highest marginal propensity to invest.

This comparison, however, abstracts from policy costs. Grants impose a fiscal cost equal to the amount disbursed, whereas moratoria initially shift the financing burden onto banks. In a setting without defaults, as approximated by our Austrian evidence, interest-bearing moratoria are approximately NPV-neutral and impose no ex-post financial costs on banks. Consistent with this, our DiD estimates show that eligible firms do not experience higher default rates than comparable ineligible firms. In other settings, however, widespread defaults following the resumption of repayments could impose substantial losses on banks and, in extreme cases, require government intervention. Hence, the ex-ante expected fiscal cost of moratoria may be higher than the ex-post cost observed in our setting. An additional potential cost is the crowding out of new lending: banks required to grant moratoria during periods of financial distress may respond by reducing credit to other firms. However, we find no empirical evidence of this mechanism in our setting.

At a time when governments face increasingly tight fiscal constraints, understanding the cost-effectiveness of credit relief policies is essential. Through this analysis, we aim to offer a framework for evaluating policies designed to support firms during periods of distress. Our findings suggest that moratoria can sustain investment at a limited fiscal cost, making them a valuable policy tool for governments to consider when confronting future crises.

Related literature. This paper contributes to the corporate finance literature studying how firms respond to cash windfalls and changes in internal funds (Fazzari, Hubbard, and Petersen 1987, Blanchard, Lopez-de-Silanes, and Shleifer 1994, Kaplan and Zingales 1997, Lamont 1997, Rauh 2006, Chodorow-Reich et al. 2026). Our paper is one of the first that causally estimates a marginal propensity to invest from a broad sample of firms. Importantly, our estimate captures the investment response to temporary liquidity to small firms during a crisis, precisely when government intervention may be most needed.

This paper also contributes to the literature studying Covid-era policy interventions targeting firms. Existing work has largely focused on grants, loan guarantees, and subsidized lending.[5] By contrast, there is little evidence on debt moratoria, particularly on firms' real responses to these policies. Existing work studies moratoria in the context of student debt (Dinerstein, Yannelis, and Chen 2024) and examines corporate debt

5. In the United States, much attention has been devoted to the Paycheck Protection Program, which offered forgivable loans—effectively grants—to eligible businesses (Hubbard and Strain 2020, Granja et al. 2022, Autor et al. 2022b, Dalton 2023). Other work studies the Federal Reserve's corporate credit facilities (Gilchrist et al. 2024) and government loan guarantees and subsidies in the United States and Europe (Bachas, Kim, and Yannelis 2021, Demmou and Franco 2021, Altavilla et al. 2023).

moratoria primarily through their effects on the judicial system and fiscal costs (Greenwood, Iverson, and Thesmar 2020, Hong and Lucas 2023a, Hong and Lucas 2023b). To our knowledge, we are the first to estimate the real effects of corporate debt moratoria. Unlike grants and subsidized credit, moratoria provide firms with temporary liquidity without direct fiscal outlays, making them a distinct policy instrument for supporting firms during crises.

The increase in investment is accompanied by higher profitability and is not driven by differential firm exit or default. This finding relates to the literature on the cleansing effect of recessions and the risk of "zombification" associated with government support (Caballero and Hammour 1991, 2000; Caballero, Hoshi, and Kashyap 2008; Schivardi, Sette, and Tabellini 2022). Our evidence suggests that the moratorium supported firms that subsequently became more profitable rather than simply delaying the exit of distressed firms. More broadly, the results show that temporary liquidity relief can preserve productive investment without necessarily preventing the reallocation associated with recessions.

2 Economics of Moratoria

This section provides a conceptual framework for understanding the costs and benefits of moratoria from the perspectives of banks and firms.

Through moratoria, governments require banks to accept firms' requests to postpone loan repayments. Firms must eventually repay the postponed amounts and accrued interest. Loans under moratoria are not considered in forbearance, thus providing countercyclical regulatory relief to banks during periods of distress. For banks, the main benefit is that moratoria may prevent borrowers from defaulting, allowing them to eventually repay their loans. However, banks are forced to extend maturities for borrowers they might otherwise be unwilling to finance, exposing them to ex-ante default risk.

From the firms' perspective, moratoria can be understood as economically equivalent to new borrowing: they provide liquidity today that must be repaid in the future. Like new loans, moratoria may entail pecuniary costs, as postponed repayments can accrue interest. Assuming that deferred repayments and new borrowing carry the same interest rate, moratoria are NPV-neutral instruments that allow firms to shift resources across time. For firms with unconstrained access to credit, moratoria and new borrowing should therefore be interchangeable.

However, moratoria may entail an additional cost that distinguishes them from conventional borrowing. Requesting a moratorium may signal financial distress to

lenders, generating reputational costs and potentially impairing future access to credit. This concern may be particularly relevant when firms maintain long-term relationships with their lenders, as is the case for the microenterprises in our sample, which borrow, on average, from a single bank.

Hence, firms should opt into the policy when financing constraints are sufficiently severe that they cannot meet their liquidity needs through alternative sources of financing, and the value of the liquidity provided by the moratorium exceeds its pecuniary and non-pecuniary costs. This trade-off becomes particularly relevant during a credit crunch: firms with profitable investment opportunities but limited access to new borrowing can use the moratorium to free up otherwise unavailable liquidity and sustain investment. By contrast, firms without positive-NPV investment opportunities have little reason to seek additional financing and therefore little incentive to take up the moratorium. Like new borrowing, postponing existing debt payments shifts repayment obligations into the future, generating an erosion of borrowing capacity that firms are willing to incur only when the additional liquidity has sufficiently valuable uses.

As we discuss in more detail in Section 7.2, grants differ from moratoria along two key dimensions. First, grants are NPV-positive, as they represent a direct transfer to firms rather than a reallocation of payments over time. Second, they are less likely to entail non-pecuniary costs, as they are provided by the government rather than by a lender that plays a central role in the firm's financing relationship and survival.

Our empirical strategy performs a dynamic difference-in-differences in the context of the 2020 Austrian moratoria. The eligibility cutoff set at EUR 2 million of pre-policy assets creates a quasi-experiment to study the economics of moratoria.

3 The Austrian moratoria on corporate debt

In this section, we provide a detailed description of the Austrian moratoria, focusing on the institutional features that are central to our analysis.

On 1 April 2020, Austria announced moratoria on micro-enterprises' loan repayments with the scope of mitigating firms' liquidity shortages during the Covid crisis (4th Covid Act). Borrowers could apply between 1 April 2020 and 31 January 2021, and payments could be postponed until November 2021. The bulk of applications took place at the start of the policy. Banks were required to grant firms' requests to postpone repayments on loans originated before 15 March 2020, allowing for a maximum maturity extension of up to 10 months. On average, firms used the policy for 8 months, not fully exploiting the maximum extension of 10 months. The policy intervention was sizable: as of December 2020, the total volume of moratoria granted to non-financial corporations amounted

to EUR 22.5 billion (Fidesser et al. 2021), corresponding to approximately 6% of 2019 Austrian GDP.

According to "§2 2nd Covid-JuBG" and according to the Austrian Financial Market Authority (FMA)'s guidelines, firms had to inform the bank by email that they wanted to take advantage of the moratorium and had to demonstrate a loss of income induced by Covid if asked. In practice, rejections were unusual with banks rarely asking for proof on income loss and declining the requests only when firms did not meet the size criterion. Borrowers and creditors were free to agree on the postponement of either principal repayments, interest expenses, or both. Interest was generally charged on suspended payments.[6]

The Austrian policy was embedded within a broader European regulatory framework. In April 2020, the European Banking Authority (EBA) issued its "Guidelines on legislative and non-legislative payment moratoria on loan repayments applied in the light of the Covid crisis" (EBA/GL/2020/02). These guidelines clarified that payment moratoria would not trigger a forbearance classification, provided certain conditions were met. In particular, EBA-compliant moratoria could only modify the timing of payments, while leaving other contractual terms - most notably interest rates - unchanged. Although moratoria suspended the counting of past-due days for default classification, the guidelines also required banks to continue identifying borrowers whose short-term liquidity difficulties may evolve into longer-term financial distress. Consistent with this, Fidesser et al. 2021 document that Austrian banks adopted a relatively conservative approach, classifying more than 40% of loans under active EBA-compliant moratoria as Stage 2, which under the International Financial Reporting Standard 9 indicates a significant increase in credit risk since initial recognition.

The wide range of other COVID-related interventions does not interfere with our identification strategy. As summarized in Figure C.1, the moratoria were the only policy specifically targeting microenterprises and thereby provide a sharp quasi-experimental setting. Appendix A provides a comprehensive overview of other COVID-era credit relief policies for firms.

4 Data and Summary Statistics

This section describes the three datasets used in our analysis and how we combine them. We rely on balance-sheet data from Bureau van Dijk's Orbis, administrative data from

6. Although most Austrian banks initially charged interest during the statutory moratorium, subsequent judicial rulings established that such interest charges were not permissible. Following these rulings, major Austrian banks agreed to reimburse affected borrowers. See https://vki.at/vki-sollzinsen-refundierung-kreditmoratorium-nur-die-volksbanken-verweigern/66821.

the Austrian Microdata Center (AMDC) covering income statements, employment, and firm exit, and loan-level information from AnaCredit. While Orbis and AMDC allow us to examine how firms allocate the liquidity freed up by the moratorium, AnaCredit is essential for identifying moratorium participation and measuring take-up.

To ensure comparability between firms on either side of the eligibility cutoff, from Orbis, we select firms with 2019 total assets between EUR 0.8 and 3.2 million. The selection of this bandwidth follows the MSE-optimal criterion proposed by Calonico, Cattaneo, and Titiunik 2014, and ensures a sizable sample on both sides of the cutoff. It is important to stress that our sample covers a meaningful share of the Austrian business sector. This feature extends to most European countries, where the vast majority of firms are micro or small, and only a limited number reach medium or large size. The Orbis data is available from 2017, which hence constitutes the starting year of our sample.

4.1 AMDC-Orbis merge

While Orbis provides detailed balance sheet information, for our selected sample it lacks income statements, employment, and exits data. We obtain such variables using administrative data from the Austrian Micro Data Center (AMDC) which is available from 2018. We could not have used AMDC without Orbis, since it does not contain balance sheet information. Hence, by merging AMDC to Orbis, we are able to obtain a full view of Austrian firms' accounts. In 2019, our merged sample comprises 18,336 firms, of which 13,344 are eligible for the moratorium and 4,992 are ineligible and form the control group. These numbers remain in a similar range throughout the sample period. Exact annual counts are reported in Figure C.2. Wholesale and retail trade is the largest sector, followed by construction, professional services, and manufacturing (Figure C.3).

From AMDC, we obtain three datasets: (i) Performance and Structural Statistics data, (ii) General Business Demographics, and (iii) Insolvency. The first dataset includes employees and personnel expenses as well as income statements - specifically, we employ the following variables: depreciation, sales, total revenues, total costs, financial income and expenses. We construct Earnings Before Tax (EBT) as the difference between total revenues and total expenses, Earnings Before Interest and Tax (EBIT) as the sum of EBT and net financial expenses, and net income as EBT minus taxes. The tax rate in Austria was 25% during the period of analysis, hence $NetIncome = EBT * (1 - 0.25)$. These variables are crucial for understanding the equity increase among treated firms, and evaluating profitability. Furthermore, we construct CAPEX as difference in Orbis fixed assets (`fias`), and add AMDC depreciation (`aabschr`).

The General Business Demographics (UDEMO) contains entry and exit information.

From the Insolvency (INS) dataset, we use a flag for whether a company applied for bankruptcy. These two databases start in 2019. From the UDEMO file we keep only the variable containing the year of exits (SLJ), while INS already only contains the year of bankruptcy filing. Importantly, when a firm closes down or files for bankruptcy, it exits the Orbis sample. To get data on exits and bankruptcy, we recur to the Insolvency and the Demographics datasets in AMDC.

4.2 AnaCredit-Orbis merge

To enrich our setting with loan-level variables, we merge Orbis with AnaCredit, a harmonized credit registry of the Euro Area, containing detailed information on bank credit to enterprises, to which we obtain access through the ECB. AnaCredit provides rich information on loan characteristics (e.g., type, maturity, interest rate, collateral, and guarantees) as well as on credit quality (e.g., default status). It is relevant to stress that a credit instrument is subject to reporting if the total committed amount at the creditor-debtor level is greater than or equal to EUR 25.000 at any point within the reference period. We clean the database following Kosekova et al. 2025. In particular, we exclude firms with active syndicated loans and loans associated with multiple creditors. At the instrument level, we include only term loans, credit lines, and revolver loans as the only instruments eligible for moratoria.

AnaCredit allows us to construct a moratorium uptake indicator. We define a loan as being under moratorium if three conditions are jointly satisfied during the application period: (i) the loan's maturity is extended, (ii) the forbearance flag is not activated, and (iii) the loan was originated before March 15, 2020.

Furthermore, we can build a measure of freed liquidity at the loan level. Such construction is not straightforward, as the specific terms of the moratorium are negotiated between the firm and the bank. For instance, repayment deferrals may apply to both principal and interest, to only one of the two, or involve partial repayments. We proceed by constructing a benchmark measure of scheduled monthly repayments. During periods in which the moratorium is active, we define - for each loan in a given month - freed-up liquidity as the difference between scheduled repayments and actual repayments. In practice, we proxy actual repayments using changes in the outstanding amount: decreases in outstanding balances correspond to repayments. Accordingly, freed-up liquidity is computed as scheduled repayments minus any observed decrease in the outstanding amount, which we call $ActualPayments_{im}$. Outside the moratorium period, freed-up liquidity is set to zero. We then aggregate the difference at the year

level and obtain the firm-year measure as follows:

$$FreedLiq_{it} = \sum_{m} \left(DuePayments_{i,m} - ActualPayments_{i,m} \right)$$

where $t \in [2020, 2021]$.

For annuity loans, we compute the implied monthly payment from the outstanding balance, monthly interest rate, and remaining maturity. For loans close to maturity, we set the scheduled payment equal to the remaining balance plus final-period interest, while zero-interest loans are assigned equal principal installments. For German-amortizing loans, scheduled payments are defined as the fixed principal installment, based on the original commitment and maturity, plus interest on the lagged outstanding balance. For fixed-installment, bullet, other, and unclassified amortization types, we use observed pre-moratorium repayments as a model-free counterfactual for scheduled payments, relying on the closest available lagged repayment prior to moratorium take-up. We then combine these type-specific measures into a unified variable of the monthly scheduled payments.

$$ScheduledP_{ilm} = \begin{cases} B_{i,l,t-1} \dfrac{r_{ilm}(1+r_{ilm})^{T_{ilm}}}{(1+r_{ilm})^{T_{ilm}} - 1} & \text{if } annuity,\ r_{ilm} > 0,\ T_{ilm} > 3 \\ B_{i,l,t-1}(1+r_{ilm}) & \text{if } annuity,\ r_{ilm} > 0,\ 1 \leq T_{ilm} \leq 3 \\ \dfrac{B_{i,l,t-1}}{T_{ilm}} & \text{if } annuity,\ r_{ilm} = 0 \\ \dfrac{C_{il}}{T^0_{il}} + r_{ilm} B_{i,l,t-1} & \text{if } German \\ Repayments_{i,l,t-3} & \text{if } unscheduled \end{cases}$$

where $B_{i,l,t-1}$ denotes the outstanding nominal amount of loan l at the beginning of month m held by firm i, r_{ilm} is the monthly interest rate, T_{ilm} is the remaining contractual maturity (in months), C_{il} is the original loan commitment, T^0_{il} is the original contractual maturity (in months), and $Repayments_{i,l,t-3}$ denotes the observed repayment 3 months prior to month m, which is chosen as the closest available pre-moratorium repayment. We choose a three-month lag because repayments exhibit a pronounced quarterly pattern. For unscheduled or unknown amortization[7], this measure serves as the counterfactual scheduled payment during the moratorium period.

7. In AnaCredit, $typ_amrtstn \in [3, 4, 5, -99]$.

We estimate that, on average, between 2020 and 2021 firms obtained liquidity through postponed repayments equal to EUR 38,600, which corresponds to 3% of their 2019 total assets from postponing loan repayments (Figure C.4).

Our measure has one important limitation. Because of the EUR 25,000 reporting threshold in AnaCredit, our sample excludes loans with small amounts at origination as well as loans close to maturity whose outstanding balance falls below the threshold. To assess the magnitude of this coverage issue, we focus on long-term debt, which, unlike short-term debt, is well populated in Orbis. We find that long-term loans observed in AnaCredit account for only about half of the long-term debt reported in Orbis. Over 2018–2023, end-of-year long-term loans average 30.18% of 2019 assets, compared with 62% for long-term debt. These ratios remain broadly stable across years. Thus, under the assumption that loans not observed in AnaCredit have the same propensity to enter moratoria as observed loans, our measure of postponed payments captures only about half of the actual liquidity freed by the policy. This implies that the total amount of liquidity freed by the moratorium was approximately EUR 76,000 per firm.

However, the ultimate goal of our analysis is to obtain a euro-for-euro interpretation of how firms use freed liquidity. To do so, we rely on cash flow statement variables from AMDC. Since AMDC and AnaCredit cannot be merged, we need to construct a proxy for freed-up liquidity using the change in long-term debt from AMDC-Orbis as follows:

$$ProxyFreedLiq_{it} = (LtDebt_{i,t}^{AMDC-Orbis} - LtDebt_{i,t-1}^{AMDC-Orbis})$$

The intuition behind this measure is that moratoria drive higher long-term debt, mechanically due to the repayments' postponement and interest accrual. We focus on long-term debt, rather than total debt, since the majority of instruments under moratoria has maturity greater than 10 years and since the variable for current loans (`loan`) is not well-populated for our sample in Orbis. As a robustness check, we also employ as proxy $\Delta TotalDebt_{i,t,t-1} = \Delta LtDebt_{i,t,t-1} + \Delta CurrentLoans_{i,t,t-1}$. Results are robust.

The main criticism of this proxy is a possible violation of the exclusion restriction. Indeed, moratoria may facilitate access to borrowing, hence increasing the long-term debt above what merely implied by postponed payments. In Figure C.5, we show that this is not the case. If anything, moratoria induced a slight decrease in new issuances of loans. However, the effect of 0.02% is too small to be a reason for concern. The use of revolving loans and credit lines as well as new issuance are not impacted by the policy.

As we discuss in detail in Section 6.1, a back-of-the-envelope calculation yields an estimated EUR 73,600 in liquidity freed by the moratorium, remarkably close to the EUR 76,000 estimate obtained from AnaCredit.

4.3 Summary Statistics

Table D.1 reports summary statistics of the variables from AMDC-Orbis merge (panels A-G), and AnaCredit-Orbis merge (Panel H).

5 Empirical Specification

In this section, we present evidence supporting our identifying assumptions and describe the empirical strategy in detail. Our approach proceeds in two steps. First, we exploit the moratorium eligibility cutoff in a dynamic difference-in-differences (DiD) framework to estimate intent-to-treat (ITT) effects. Second, we perform an instrument variable two-stage least square (IV-2SLS) analysis where a proxy of the liquidity freed up by the moratorium is used as instrument, allowing for a euro-for-euro interpretation of firms' responses.

5.1 Take-up

For our identification strategy to be valid, we first establish that eligibility affects policy take-up. Properly defining the set of eligible firms requires accounting for alternative forms of debt relief available at the onset of the pandemic, before the moratoria were announced on 1 April 2020. When lockdown measures were introduced in March 2020, anecdotal evidence suggests that approximately 50% of borrowers experiencing repayment difficulties renegotiated their loans with banks. News articles confirm the steep increase in renegotiation by the end of March 2020[8]. Consistent with this evidence, AnaCredit data indicate that around half of otherwise eligible firms received payment extensions in March 2020. Firms that obtained such payment extensions were subsequently less likely to apply for the EBA-compliant moratoria. We therefore exclude from the sample firms that obtained a payment extension but did not subsequently apply for the moratorium.

Similarly to other major Covid support programs, private loan renegotiations were available to firms on both sides of the eligibility cutoff, leaving our identification strategy unaffected. Nevertheless, it is informative to compare firms that used EBA-compliant moratoria with those that instead obtained discretionary payment extensions from their banks at the onset of the lockdown. Figure C.6 shows that, as of 2019, firms that renegotiated their loans in March 2020 were financially stronger, exhibiting lower leverage and higher liquidity. This pattern is unlikely to reflect firms' self-selection. Rather, it is consistent with banks using their discretion to grant early payment relief to their healthier borrowers before the introduction of the mandatory moratorium, under

8. See for example: https://orf.at/stories/3159956/

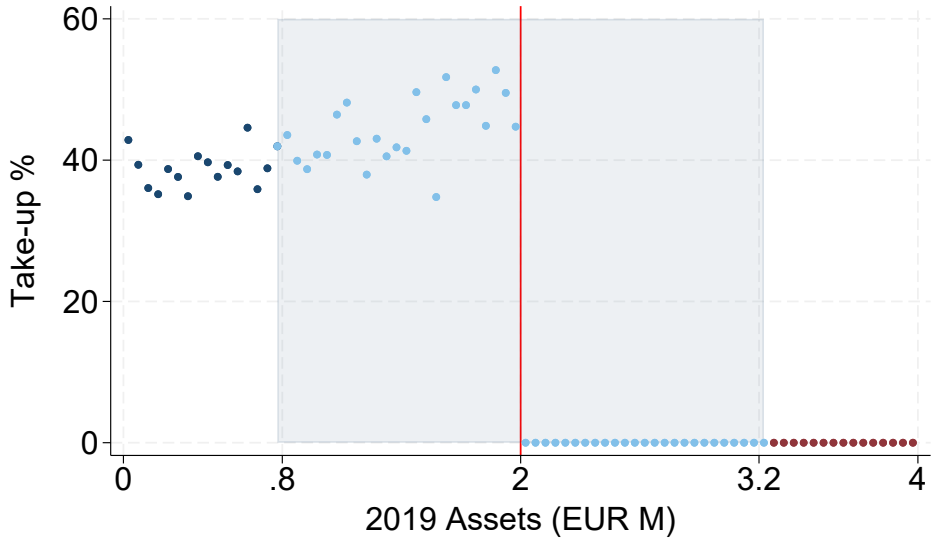

Figure 1: Take-up

Notes: The figure presents the percentage of firms that used the moratorium over eligible firms across buckets of 2019 total assets, expressed in million euros. Eligible firms are micro-enterprises with at least one loan originated before 15^{th} March 2020. Loans in default as of March 2020 and firms that privately recontracted their loans in the same month are excluded from the sample.

which banks were legally required to grant payment deferrals to all eligible applicants.

Figure 1 plots the share of firms that used the policy on at least one loan over bins of 2019 assets. The red line marks the eligibility threshold at EUR 2 million. Among eligible firms, the average take-up was 44%, substantially below the nearly 100% participation of other opt-in support measures available at the time, such as the grant-like *Umsatzersatz*. However, this aggregate figure masks substantial heterogeneity across sectors, and firms' characteristics.

As is well known, the economic impact of Covid varied markedly across industries depending on their reliance on face-to-face interactions, creating both winners and losers. Firms operating in sectors more exposed to lockdown measures were therefore likely to have stronger incentives to use the moratorium. To explore the use of the moratorium across sectors, we employ the measure of supply-side disruption - "Covid work exposure" - developed by Papanikolaou and Schmidt 2022, defined as 1 minus the fraction of workers who have ever worked full days from home. Figure C.7 shows substantial heterogeneity. In "Professional Services" - the sector least reliant on in-person work - only 30% of eligible firms used the policy. At the other end of the spectrum, heavily affected sectors such as "Transportation and Accommodation" exhibit the highest take-up, reaching up to more than 60%. Taken together, these results suggest that moratorium take-up lies between 30% and 60% - in all cases substantially lower than that of grant-based policies implemented during the pandemic, which approached full

participation.

What pre-policy loan characteristics predict moratorium take-up? To answer this question, we estimate a penalized logistic regression using the LASSO estimator. The specification includes loan pricing (interest rate and spread), accrued interest, scheduled repayments, residual maturity, loan amortization structure, interest rate type, and an indicator for whether the loan was scheduled to expire during the moratorium period. We estimate the model separately by instrument type - term loans, credit lines, revolvers - and select the penalty parameter via 10-fold cross-validation. Because the shrinkage induced by LASSO biases coefficient estimates toward zero, we use the selected covariates to estimate an unpenalized logistic regression with heteroskedasticity-robust standard errors. This post-LASSO procedure preserves the data-driven model selection while yielding coefficients that are directly interpretable as associations with moratorium take-up. Figure C.8 shows that interest rates, either in the form of annual rate or spread, predict positive take-up across all three instruments. The same is true for scheduled repayments which we are able to compute only for term loans and credit lines, and hence are not included as a variable in the LASSO selection for revolving loans. Variable interest rates, in opposition to the fixed rates, induce higher take-up for term loans and credit lines.

Are moratoria used to delay recontracting during a crisis or to obtain liquidity? Figure C.8 shows that a loan's scheduled expiration during the moratorium is not a significant predictor of take-up except for revolving loans, which however constitute a minority of our sample (Figure C.24). Moreover, although revolving loans using the policy were often scheduled to mature during the moratorium period, the vast majority of loans under moratorium were term loans and credit lines with residual maturities extending well beyond the end of the policy. This pattern is difficult to reconcile with a refinancing or recontracting motive in the spirit of Almeida et al. 2012.

Pre-policy firm leverage is also a strong predictor of moratorium take-up. Figure C.9 shows that the take-up increases monotonically in pre-policy leverage. We also consider net current assets, computed as the difference between current assets and current liabilities, as a proxy for liquidity. A U-shaped relationship exists with liquidity: firms with either the highest or the lowest levels of liquidity are the most likely to use the policy.

5.2 Dynamic Difference-in-Differences

We examine the effect of moratorium on firms' decisions by running the following dynamic difference-in-difference regression

$$\frac{y_{i,t}}{at_{i,2019}} = \iota_i + \sigma_{st} + \sum_{h \neq 2019} \gamma_h \mathbf{1}(Elig_i) \times \mathbf{1}(t = h) + \eta_{it} \tag{1}$$

where i indexes firms, t indexes time, s indexes sector, y_{it} is the dependent variable of interest (capex e.g.) scaled by 2019 total assets, $at_{i,2019}$, and winsorized at the 1-99% level, $\mathbf{1}(t = h)$ is a year dummy, $\mathbf{1}(Elig_{i,2019})$ is a dummy variable that equals one if the firm is eligible to the moratorium (i.e. its assets in 2019 are smaller than 2 million euros), and η_{it} is an error term. The inclusion of firm fixed effects fully controls for fixed differences between the two groups, while the sector-by-time fixed effects control for sectoral fluctuations. Our coefficients of interest, which capture an intent-to-treat (ITT), are γ_h. We include 3 years - or 2 years when data starts in 2018 - prior to the policy change and 3 years after the policy change to explore possible pre-trends. We normalize the coefficient for 2019 to zero such that all treatment effects are relative to levels the year before the policy took place. Standard errors are clustered at the σ_{st} level. Sectors for which either the eligible or the ineligible groups contain less than 100 observations are replaced with missing.

No Manipulation

As explained in detail in Section 3, only firms with 2019 total assets smaller than 2 million were eligible for the policy. We select firms in a narrow window around the asset cutoff defined through an MSE-optimal bandwidth based on Calonico, Cattaneo, and Titiunik 2014. We use the same bandwidth of 1.2 million assets for estimation across all outcomes. We exclude firms operating in the financial and public administration sectors.

Our identification strategy is based on the idea that firms "barely" below the cutoff are, on average, identical to the firms "barely" above the cutoff. Firms differ only by having access to the policy. Figure 2 validates the random treatment assignment within the window.

Importantly, for the treatment to be exogenously assigned, firms must not be able to manipulate the variable determining the eligibility. Anecdotally, we are able to rule out such possibility since firms could not anticipate Covid and the related policies. Figure C.10 shows that the amount of total assets in 2019 is not discontinuous around the 2 million cutoff. We furthermore validate this intuitive explanation by performing a no-manipulation test in the vein of McCrary 2008 which employs the test for density discontinuity introduced by Cattaneo, Jansson, and Ma 2018. With a p-value of 0.2078,

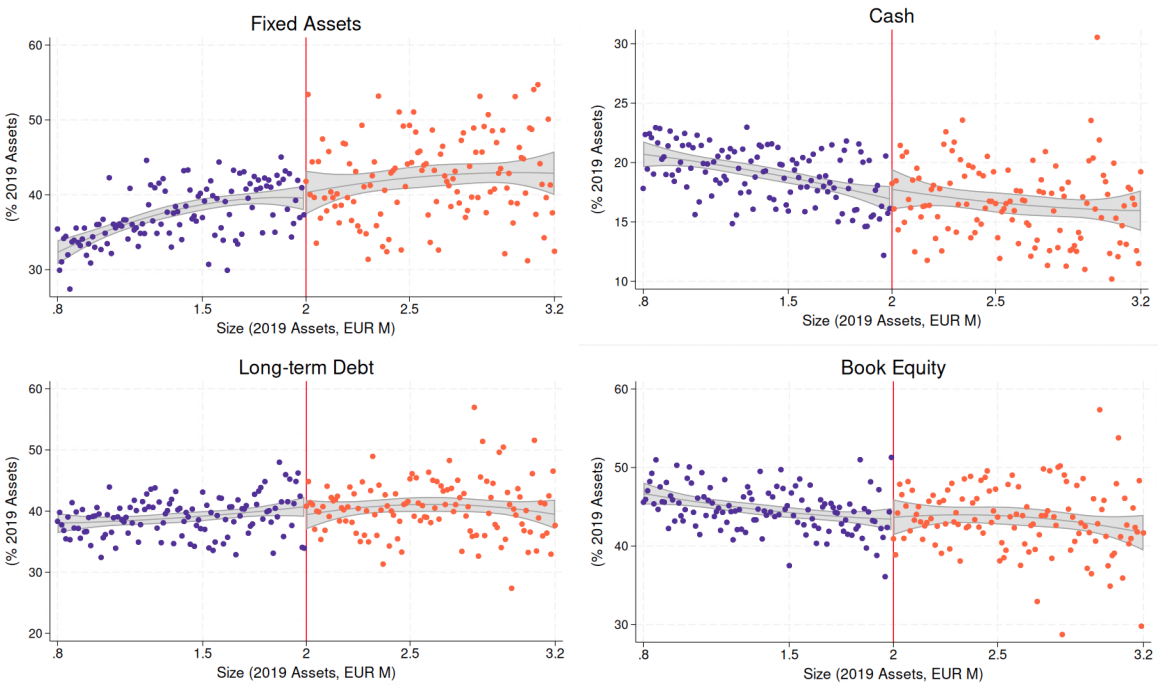

Figure 2: Covariates Balance Analysis

Notes: The figure assesses the continuity of predetermined firm characteristics around the EUR 2 million eligibility cutoff. The panels report 2019 values of fixed assets, cash, long-term debt, and book equity, all expressed as a percentage of total assets. Each dot represents the average value within a 2019 asset-size bin. The solid lines report local polynomial fits estimated separately on each side of the cutoff, and the shaded areas denote 95% confidence intervals. The vertical red line marks the EUR 2 million eligibility cutoff. All the figures employ the AMDC-Orbis dataset.

we cannot reject the null of no discontinuity in the 2019 assets. Placebo tests in Appendix E provide additional support for our identifying assumption.

Supportive evidence from a Regression Discontinuity Design

As an alternative to our DiD specification within a selected window around the eligibility cutoff, we could employ a regression discontinuity design (RDD), exploiting the same cutoff and bandwidth. This approach yields estimates with limited statistical power, a challenge also encountered by Ganong and Noel 2020. Nevertheless, Figure 3 shows a visible discontinuity in investment around the eligibility cutoff in 2021. The presence of this discontinuity, combined with the limited precision of the RDD estimates, motivates our choice of a difference-in-differences specification, which provides greater statistical power to investigate the effects of the policy.

5.3 Instrument Variable two-stage least square

The DiD estimates capture the intent-to-treat effects of eligibility. In this section, we use an instrumental variables (IV) approach to estimate the effect of liquidity freed by the

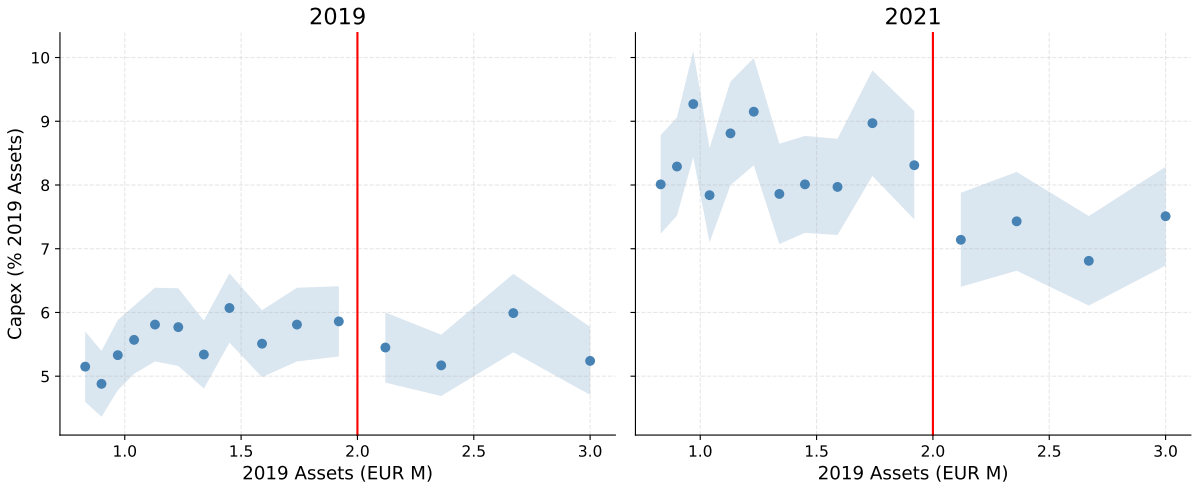

Figure 3: Binscatter of 2019 and 2021 Capex over 2019 asset

Notes: The figures present capex in 2019 (left panel) and in 2021 (right panel) over buckets on 2019 assets. Each dot represents the average value within a 2019 asset-size bin, and the shaded areas denote 90% confidence intervals. The red line shows the EUR 2M eligibility threshold.

moratorium, obtaining a euro-for-euro interpretation. We focus on 2021, the year in which the moratorium ended.

We proxy for freed-up liquidity using the change in long-term debt ($\Delta LTDebt$), introduced in Section 6.1. Moratoria mechanically increase outstanding loan balances by postponing principal repayments. We focus on long-term debt because the vast majority of loans subject to the moratorium had a residual maturity exceeding 10 years. Moreover, while long-term debt is well populated in our AMDC-Orbis sample, its short-term counterpart (`loan`) is not. Our results are robust to using the change in total debt, defined as the sum of current loans and long-term debt (`loan+ltdb`).

We instrument the change in long-term debt with eligibility interacted with the 2021 indicator. The first stage follows Equation (1), replacing the dependent variable with the change in long-term debt:

$$\frac{\Delta LTDebt_{it}}{at_{i,2019}} = \iota_i + \sigma_{st} + \gamma^{FS}_{2021}\, 1(Elig_i) \times 1(t = 2021) + \sum_{h \notin \{2019,2021\}} \gamma_h\, 1(Elig_i) \times 1(t = h) + v_{it} \tag{2}$$

where $\Delta LTDebt_{it} = LTDebt_{it} - LTDebt_{i,t-1}$, and γ^{FS}_{2021} is the first-stage coefficient for 2021. All other terms are defined as in Equation (1).

The second stage estimates the effect of freed-up liquidity on firm outcomes:

$$\frac{y_{it}}{at_{i,2019}} = \upsilon_i + \zeta_{st} + \xi_{2021} \frac{\widehat{\Delta LTDebt_{it}}}{a_{i,2019}} + \sum_{h \notin \{2019,2021\}} \delta_h\, 1(Elig_i) \times 1(t = h) + u_{it} \tag{3}$$

where y_{it} denotes the outcome of interest (e.g., capital expenditure), scaled by 2019 total assets and winsorized at the 1st and 99th percentiles. The term $\Delta\widehat{LTDebt}_{it}/a_{i,2019}$ denotes the fitted values from the first stage. Our coefficient of interest, ξ_{2021}, measures the effect of an additional euro of liquidity, as proxied by the change in long-term debt, on firm outcomes. Since both variables are scaled by the same pre-policy total assets, the coefficient admits a euro-for-euro interpretation.

With a single excluded instrument, the 2SLS coefficient is equivalent to the Wald estimator, given by the ratio of the reduced-form and first-stage coefficients (Angrist and Pischke 2009, Chapter 4):

$$\widehat{\xi}_{2021} = \frac{\widehat{\gamma}^{y}_{2021}}{\widehat{\gamma}^{FS}_{2021}} \tag{4}$$

where $\widehat{\gamma}^{y}_{2021}$ is the reduced-form DiD coefficient for the outcome of interest. With heterogeneous effects of a continuous treatment, the IV estimator can be interpreted as a weighted average of causal responses under the assumptions of Angrist and Imbens 1995b.

The validity of our IV strategy rests on instrument relevance and the exclusion restriction. The DiD estimates for $\Delta LTDebt_{it}$ support instrument relevance (Figure 4): eligibility significantly increases long-term debt in 2021, the final year of the policy. The first-stage F-statistic is well above the benchmark value of 100 (Lee et al. 2022) when we cluster at the year level, while it decreases to 23 and 13 when we cluster at the sector-year and sector levels, respectively (Table 2).

The exclusion restriction requires that eligibility affects firm outcomes only through the liquidity freed by the moratorium. Restricting the sample to firms near the pre-policy eligibility cutoff strengthens the plausibility of this assumption by improving comparability between eligible and ineligible firms. Moreover, we find no evidence that eligibility affects new lending (Section 4.2), mitigating concerns about alternative credit-supply channels.

6 Effects of Temporary Liquidity on Firms' Decisions

Whether liquidity that firms must eventually repay can affect their investment decisions remains an open question. In this section, we provide evidence that it does. We first present a quantification of the amount freed by the policy. We then move to explore the impact on investment, profitability, as well as balance sheet and income variables. An important *caveat* is that our results specifically pertain to firms' decisions during crises, shedding light on how policy can intervene in such times.

6.1 Magnitude of Temporary Liquidity

To measure the liquidity freed up by postponed repayments, we estimate Equation 2. The change in long-term debt identifies the flow of liquidity caused by the moratoria.

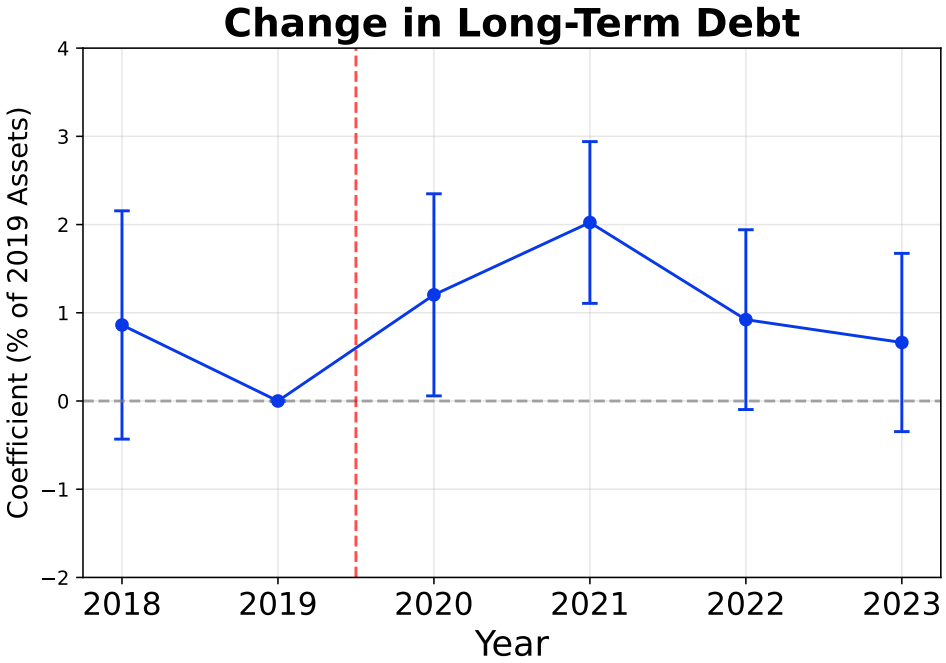

Figure 4: Freed-up Liquidity

Notes: The plot shows the coefficient resulting from the dynamic difference-in-differences

$$\frac{LTDebt_{i,t} - LTDebt_{i,t-1}}{at_{i,2019}} = \iota_i + \sigma_{st} + \sum_{h \neq 2019} \gamma_h \mathbf{1}(Elig_i) \times \mathbf{1}(t = h) + \eta_{it}$$

where i indexes firms, t indexes time, s indexes sector. The dependent variable is scaled by firms' 2019 total assets, $at_{i,2019}$, and winsorized at the 1st and 99th percentiles. $Elig_i$ is an indicator equal to one for firms with 2019 assets below EUR 2 million. Firm and sector-by-year fixed effects are included. Dots report the estimated coefficients, γ_h, and bars denote 95% confidence intervals. The analysis uses the AMDC-Orbis dataset.

In 2021, eligible firms experienced a 2 percentage points larger increase in long-term debt than ineligible firms (Figure 4). Since long-term debt is scaled by 2019 assets, and average 2019 assets were EUR 1.62 million, a back-of-the-envelope calculation implies EUR 32,400 in additional liquidity per eligible firm. Given a 44% take-up rate, this corresponds to approximately EUR 73,600 in additional long-term debt for firms taking up the moratorium. We find little heterogeneity by pre-policy leverage: firms with 2019 long-term debt both above and below the sample average exhibit an increase of approximately 2 percentage points in 2021, although the effect is slightly smaller for the latter group (Figure C.11).

One concern may however arise: a movement in long-term debt may be endogenously driven by the moratoria. Indeed, one may think that banks have an incentive to

guarantee better access to firms that received the moratoria in order to avoid defaults and make sure to obtain due repayments. We explicitly test this hypothesis, by running the dynamic DiD (Equation 1) on monthly newly originated loans. Figure C.5 shows that new credit lines are unaffected, and that new loans exhibit a negligible decline after the first half of 2021, with a peak response of -0.02% of 2019 assets. This result supports the assumption that changes in long-term debt are indeed mechanically driven by the moratorium and can serve as an appropriate measure of freed-up liquidity.

Furthermore, we establish the validity of our measure with respect to the amount of postponed payments that we can compute in AnaCredit. As detailed in Section 4.2, we are able to compute this variable only for half of the long-term debt in our sample. In 2021, the average freed-up liquidity per firm amounts to EUR 38,600 which is fairly close to half of the proxy of freed liquidity from AMDC-Orbis.

6.2 Investment and Profitability

In a frictionless world à la Modigliani and Miller 1958, given unaltered investment opportunities, firms should not increase investment, and should be indifferent between redistribution to investors and raising new debt. Figure 5 shows that eligible firms actually do increase investment, hence not being indifferent between internal and external funds. The results is not driven by divestment: we find that both eligible and ineligible firms increase investment, but the increase is larger among eligible firms (Figure C.12). One year after the start of the policy, eligible firms invest almost 1.5 percentage points of 2019 assets more than their ineligible counterparts. The trend reverts in the aftermath of the policy, consistent with a narrative that the moratorium induces a temporary relaxation of financing constraints.

The rise in investment is followed by an increase in profitability, attested by an increase in EBIT driven by revenues (Figure C.13). While starting in 2021, the increasing trend becomes significant only in 2022, showing a lag in the transmission of enhanced productive capacity to higher profitability.

6.3 Marginal Propensity to Invest

Previous literature has long investigated the investment sensitivity to cash flows (Blanchard et al. 1990, Fazzari, Hubbard, and Petersen 1987, Hoshi, Kashyap, and Scharfstein 1991, Kaplan and Zingales 1997, Rauh 2006) without reaching a final agreement on a state-of-the-art estimate. The debate has been recently reignited by Covid-era credit policies that could provide sharp quasi-experiments to estimate the marginal propensity to invest (Brunnermeier and Krishnamurthy 2020, Winberry et al. 2025). In this section, we propose a euro-amount interpretation of the investment results, and compare our

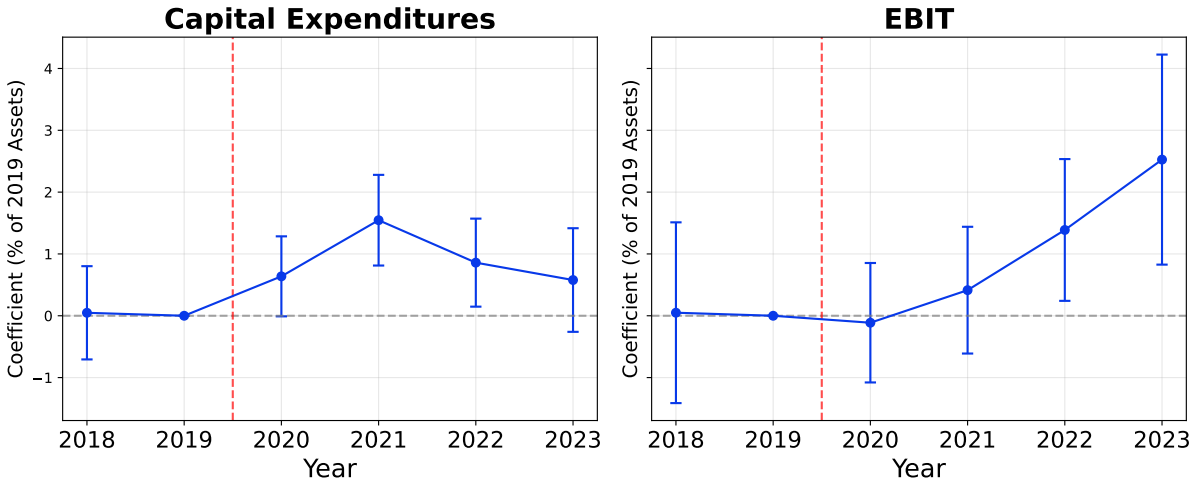

Figure 5: Effect of Moratorium on Investment and Profitability

Notes: The plots show the coefficient resulting from the dynamic difference-in-differences

$$\frac{y_{i,t}}{at_{i,2019}} = \iota_i + \sigma_{st} + \sum_{h \neq 2019} \gamma_h \mathbf{1}(Elig_i) \times \mathbf{1}(t = h) + \eta_{it}$$

where i indexes firms, t indexes time, s indexes sector. The dependent variable is scaled by firms' 2019 total assets, $at_{i,2019}$, and winsorized at the 1st and 99th percentiles. $Elig_i$ is an indicator equal to one for firms with 2019 assets below EUR 2 million. Firm and sector-by-year fixed effects are included. Dots report the estimated coefficients, γ_h, and bars denote 95% confidence intervals. The analysis uses the AMDC-Orbis dataset.

estimates to the aforementioned corporate finance literature.

The first important step is to construct cash flow statements starting from our administrative data. We exclude from the sample used in this section the handful of firms for which the change in cash did not sum to cash from operating and financing activities minus cash from investing. Table 1 contains details on the construction of cash flow statements.

A natural way to estimate the treatment effect is through the Wald estimator, which scales the reduced-form by the first stage, and the resulting Wald estimator. Under the standard IV assumptions, including monotonicity, this ratio identifies the local average treatment effect for compliers (Angrist and Imbens 1995a).

Table 2 reports reduced-form, first-stage, and Wald estimators for the cash flow statement variables. The third column in particular allows to track the flow of funds induced by the moratoria. First, we can check that the following accounting equality holds:

$$\Delta Cash = NetIncome + Depreciation - \Delta NWC - Capex + \Delta LTDebt - Payouts$$

The first important result is that, out of EUR 1 of postponed payments, 76 cents were

+ Net income
+ Depreciation
- Δ NWC
Cash from operating activities
- Capital expenditures
Cash from investing activities
+ Δ Long-term debt
- Payouts
Cash from financing activities
Change in cash

Table 1: Cash Flow Statement

Notes: The table shows the variables used to construct the cash flow statement using AMDC-Orbis data. Cash from operating activities are the sum of net income and depreciation minus the change in net working capital. Cash from investing activities coincides with the negative of capital expenditures ($\texttt{fias}_t\texttt{-fias}_{t-1}+\texttt{aabschr}_t$). Cash from investing is the difference between the change in long-term debt ($\texttt{ltdb}_t\texttt{-ltdb}_{t-1}$) and dividends ($\texttt{netinc}_t\texttt{-(osfd}_t\texttt{-osfd}_{t-1}\texttt{)}$). We excluded the handful of firms for which the sum of these three variables did not sum up to the change in cash ($\texttt{cash}_t\texttt{-cash}_{t-1}$).

Outcome variable	Reduced-form (1)	First-stage (2)	IV (3)	IV (4)	IV (5)
Net Income					
	0.14	2.03***	0.07	0.07***	0.07
	(0.36)	(0.42)	(0.173)	(0.026)	(0.228)
Depreciation					
	0.17**	2.03***	0.09***	0.09***	0.09***
	(0.08)	(0.42)	(0.033)	(0.005)	(0.028)
ΔNWC					
	1.09**	2.03***	0.54***	0.54***	0.54***
	(0.45)	(0.42)	(0.149)	(0.013)	(0.204)
Capex					
	1.54***	2.03***	0.76***	0.76***	0.76***
	(0.34)	(0.42)	(0.158)	(0.038)	(0.203)
Payouts					
	-0.83***	2.03***	-0.41	-0.41***	-0.41*
	(0.09)	(0.42)	(0.263)	(0.031)	(0.236)
$\Delta Cash$					
	0.53**	2.03***	0.26*	0.26***	0.26
	(0.26)	(0.42)	(0.141)	(0.024)	(0.171)
Number of observations	93,384	93,384	93,384	93,384	93,384
First-stage partial F	22.99	22.99	22.99	584.21	12.74
Firm FE	Yes	Yes	Yes	Yes	Yes
Sector-Year FE	Yes	Yes	Yes	Yes	Yes
Clustering	Sector-year	Sector-year	Sector-year	Year	Sector

Table 2: Effect of moratoria on cash flows and marginal propensity to invest

Notes: Columns (1)–(2) report reduced-form and first-stage estimates; columns (3)–(5) report IV estimates under sector-year, year, and sector clustering, respectively. All specifications retain firm and sector-by-year fixed effects.

used for investment and 26 cents flowed to cash buffer. An attentive reader would notice that these sum to slightly more than EUR 1. The small mismatch is due to the fact that moratoria release internal funds in addition to the increase in long-term debt. In particular, we see a significant increase in depreciation equal to 9 cents. Second, the decrease in payouts and increase in net income provide extra sources of internal funds, despite the coefficients not being significant. Finally, it is important to stress that net working capital is a proxy constructed as current assets minus current liabilities and cash (`cuas-culi-cash`). The positive and significant coefficient on ΔNWC is primarily driven by other current assets (`ocas`). Under Austrian accounting rules, this category combines receivables with a broad set of other current assets, making it difficult to determine whether the increase reflects greater trade credit extended to customers or changes in other components of current assets.[9]

A possible concern is whether the MPI reported in Table 2 depends on the choice of bandwidth around the EUR 2 million cutoff. To limit the arbitrariness of this decision, we employ an MSE-optimal bandwidth following Calonico, Cattaneo, and Titiunik 2014. Furthermore, we report a sensitivity analysis of the results for different bandwidths in Figure C.14.

Examining heterogeneity in the marginal propensity to invest (MPI) by pre-policy firm characteristics, we find that the point estimates increase with leverage (Figure C.15). Firms with below-average leverage in 2019 invest 71 cents per euro of relief, 22 cents less than firms with above-average leverage. Interpreting pre-policy leverage as a proxy for financing constraints, these estimates suggest that more financially constrained firms may exhibit a higher MPI out of temporary liquidity. Importantly, however, the differences across groups are not statistically significant, limiting our ability to draw firm conclusions about heterogeneity in investment responses.

6.4 Marginal Effects on Balance Sheet Components

The increase in equity observed in Figure C.16 deserves special attention. The following accounting equivalence points out the possible drivers:

$$\underbrace{\Delta BookEquity_{t,t-1} - \Delta PaidCapital_{t,t-1}}_{\Delta RetainedEarnings_{t,t-1}} = NetIncome_t - Payouts_t \tag{5}$$

9. `OCAS` includes other receivables; securities and shares; cash and balances with banks, central banks, and post office banks; public-sector debt securities and bills eligible for central-bank refinancing; receivables from banks and customers; bonds and other fixed-interest securities; shares and other non-fixed-interest securities; assets from investment and risk associations; accrued interest and rent; other assets; offsetting items between departments; and accrued expenses.

For each component of equation 5 scaled by 2019 assets, we run the dynamic DiD regression (Specification 1) and report the results in Figure C.17. During the policy, the increase in book equity is explained by decrease in payouts, with a coefficient of -0.83, significant at the 10% confidence level. Both eligible and ineligible firms cut payouts in 2020 and 2021, but moratoria induced eligible firms to perform larger cuts. Once investment is implemented, the hiatus in payouts between eligible and ineligible firms disappears. In 2022, the increase in equity is explained by higher net income. Two years after the policy, in 2023, equity plateaus, with higher payouts eroding the increased profits. The decline in payouts coinciding with the increase in investment may be explained by investment indivisibilities. Similarly, Kaboski and Townsend 2011 rationalize the decline in consumption among some households receiving microcredit through investment indivisibilities. In the corporate finance literature, Zwick and Mahon 2017 interpret the decline in dividends in response to bonus depreciation as consistent with non-convex adjustment costs (Caballero and Engel 1999; Cooper and Haltiwanger 2006). Figure C.17 rules out a role of capital injections since paid-in capital does not move in reaction to the moratorium.

Is the decline in dividends sizable relative to normal times? Before the policy, dividends averaged 4.92% of total assets. We estimate a decline of about 1% of assets in 2021, corresponding to a roughly 20% reduction relative to the pre-policy average. Given average 2019 assets of EUR 1.62 million, this implies an annual dividend cut of slightly more than EUR 16,000. For some of the small firms in our sample, dividend distributions may represent an important component of owner-managers' compensation. The magnitude therefore appears economically meaningful, yet plausible: EUR 16,000 corresponds to roughly 18–23% of the EUR 70,000–90,000 annual compensation benchmark for small-firm managing directors[10].

6.5 Explaining Partial Take-up: the Fear of Stigma

A major concern among policymakers (European Central Bank 2020) was that, once moratoria expired, firms might be unable to resume their debt payments, leading to a sharp increase in defaults — the so-called "cliff effect." In this section, we examine whether eligibility to the moratorium affected likelihood of defaults, credit status, and firms' subsequent access to financing.

To this scope, we run our preferred specification (Equation 1) using AnaCredit

10. The Austrian *Lohnsteuerstatistik* (wage tax statistics) from Statistics Austria reports the distribution of employed manager compensation. For small-firm managing directors (*Geschäftsführer*), the median gross annual compensation is the one we report.

monthly data:

$$y_{i,m} = \iota_i + \sigma_{sm} + \sum_{h \neq 2020M3} \gamma_h \mathbf{1}(Elig_i) \times \mathbf{1}(m = h) + \eta_{im} \tag{6}$$

where m indexes month, and all other terms are defined as above. We include 15 months prior to the policy change and 33 months after the policy change to explore possible pre-trends. We normalize the coefficient for March 2020 to zero such that all treatment effects are relative to levels the year before the policy took place.

Figure C.18 reports estimates from Equation 6, using as the dependent variable an indicator equal to one if a firm is more than 90 days past due on at least one loan at a given bank. We find no evidence that the moratorium increased defaults among eligible firms. If anything, the estimates suggest a 25-basis-point decline in the probability of default in mid-2021, although the effect is not statistically significant. This finding contrasts with evidence on subsidized lending (Collier, Howell, and Rendell 2024), which documents a decline in defaults among firms benefiting from the policy.

The absence of an effect on defaults should be interpreted in light of the broader government support provided to firms during the COVID-19 crisis. Elsinger et al. 2022 find that government support programs kept corporate insolvency rates below pre-crisis levels in Austria in 2020 and 2021. Against this backdrop, the moratorium may have played a limited additional role in preventing bankruptcies.

We next examine whether being classified as "unlikely to pay" adversely affected firms' access to financing. As discussed in Section 6.1, we find no effect on the origination of new loans or credit lines. However, even in the absence of changes in new borrowing, financing conditions may deteriorate along other dimensions, such as higher interest rates, wider credit spreads, or shorter loan maturities. To investigate this possibility, we conduct an event study tracking moratorium participants after repayments resume. We find no evidence of higher interest rates, wider spreads, or shorter maturities on newly originated loans to these firms (Figure C.19).

While, in practice and ex post, neither access to financing nor the probability of default was affected by the policy, firms may nevertheless have feared that opting into the moratorium would negatively affect banks' perceptions of their creditworthiness. To investigate this possibility, we examine banks' internal assessments of borrowers' credit risk.

Under IFRS 9, banks recognize expected credit losses (ECL) according to the stage of a loan's credit risk. A migration from Stage 1 to Stage 2 indicates that the loan remains performing but has experienced a significant increase in credit risk since origination. This classification is particularly relevant in the Austrian context, where banks appear

to have taken a more precautionary approach toward loans under moratoria. Fidesser et al. 2021 document that, among loans with active payment moratoria, 42.4% were classified as Stage 2 by Austrian banks, compared with 26.4% across European banks overall.

The European Capital Requirement Regulation (CRR)[11] imposes banks to report the default status of their borrowers. The default shall be considered to have occurred with regard to a particular obligor when either or both of the following have taken place: (a) the obligor is more than 90 days past due on any credit obligation to the institution, (b) the institution considers that the obligor is unlikely to pay. While the EBA guidelines specified that moratoria did not trigger the forbearance classification, Austrian banks associated the use of moratorium with the unlikely-to-pay status. Figure C.18 shows that, while actual defaults are unchanged, eligible firms have a peak of 0.9 percentage points higher probability to be considered unlikely to pay.

The unlikely-to-pay classification has important consequences. As clarified by the CRR, a borrower classified as unlikely-to-pay is considered to be in default. In normal times, firms in default rarely obtain new borrowing. In our setting, the stigma associated with requesting a moratorium therefore translates into higher expected costs of bankruptcy: should a firm fail to meet the postponed repayments, it risks losing access to funding from its relationship lender.

Figure C.18 provides evidence consistent with the anecdotal accounts that firms were reluctant to use the policy because of fear of stigmatization. We find that banks were more likely to classify loans under moratorium as unlikely-to-pay, despite the absence of any increase in actual defaults. Importantly, this classification quickly reverted to its pre-policy level once the moratorium expired. This temporary nature is consistent with our finding that eligible firms did not experience a deterioration in their access to financing.

The temporary increase in the unlikely-to-pay classification also indicates that, in the absence of government guarantees, banks retained strong incentives to screen and monitor borrowers throughout the policy.

6.6 Additional Results

Figure C.20 shows that employment is not impacted by the moratoria. This finding should not be surprising given the rigidity of European labor market and the availability of policies, such as the *Kurzarbeit*, that have been historically at the disposal of Austrian firms and that are specifically targeted at employment.

A valid concern is whether attrition plays a role in explaining our results. We address

11. Regulation (EU) No 575/2013. See Section B for more details.

it by performing a mean-difference test of exits between eligible and ineligible firms. We chose to perform a mean-difference test rather than the usual DiD regression due to the lack of data before 2019, preventing an evaluation of pre-trends and hence of the validity of our analysis. Table D.3 shows that differences in death rates are not significantly different from zero. This evidence shows that our results are not driven by firms in the control group disproportionately exiting the sample with respect to the treatment group.

6.7 Discussion of Results

To our knowledge, this is the first paper to study the investment response to a truly temporary liquidity injection.

A related literature studies how shocks to lending institutions are transmitted to firms' investment. Gan 2007 and Almeida and Campello 2007 document contractions in investment among affected borrowers, but their analyses are limited to large firms. Moreover, their settings do not allow for the estimation of a marginal propensity to invest.

Another strand of the literature, dating back to the late 1980s, studies how cash windfalls to firms translate into investment. While our paper differs from this literature by studying a temporary liquidity provision that must ultimately be repaid, our findings share some similarities with these studies.

Consistent with Blanchard, Lopez-de-Silanes, and Shleifer 1994, firms retain the additional liquidity internally, increasing investment and cash holdings rather than payouts. Indeed, payouts decline during the moratorium, consistent with evidence from bonus depreciation policies (Zwick and Mahon 2017). In contrast to Blanchard, Lopez-de-Silanes, and Shleifer 1994, however, we find no persistent increase in leverage: debt increases only temporarily, mechanically reflecting postponed repayments. This result also differs from Dinerstein, Yannelis, and Chen 2024, who show that households respond to student-loan moratoria by increasing borrowing.

Table D.4 compares our marginal propensity to invest (MPI) with estimates from the corporate finance literature. As shown in Figure C.21, MPI estimates range from 0.06 in Blanchard, Lopez-de-Silanes, and Shleifer 1994 to 0.7 for unconstrained firms in Kaplan and Zingales 1997. Our estimate of 0.76 lies at the upper end of this range. Beyond the temporary nature of the liquidity provision, our setting differs along two important dimensions. Previous studies primarily examine large, publicly listed firms during normal times, whereas we study microenterprises during a severe crisis. In addition, our estimate is based on a substantially larger sample than those used in previous studies.

More recently, Chodorow-Reich et al. 2026 study the Minnesota Small Business Relief Grants (SBRG), allocated by lottery to businesses with 50 or fewer full-time equivalent employees, and document positive effects. Their setting differs from ours in two important respects. First, while they study small businesses, we focus on microenterprises with an average of 13 employees, which may face tighter financing constraints. Second, the SBRG program provided grants rather than temporary liquidity. Their findings are consistent with the mechanism highlighted by our model in Section 7: firms without profitable investment opportunities may still accept grants and distribute the funds to shareholders, who, in their setting, use them to purchase automobiles or strengthen their personal finances. In line with our paper, their evidence stresses the importance of targeting relief toward firms with productive investment opportunities, a mechanism that grants, unlike moratoria, do not intrinsically provide.

7 Studying policies to support firms during crises

The empirical analysis shows that firms use the liquidity freed by the moratorium to invest and increase profitability. Once repayments resume, firms are able to meet their obligations and defaults do not increase. Yet average take-up is 44%, substantially lower than that of other opt-in support measures available at the time, such as grants.

In this section, we use the model to compare moratoria and grants, examining selection into each policy and their cost-effectiveness per euro of liquidity provided and per euro of fiscal cost.

7.1 Explaining the modeling assumptions

We make three important modeling assumptions: (1) the crisis is temporary, (2) the decline in revenues is accompanied by a tightening of new credit, and (3) firms face an exogenous non-pecuniary cost of opting into the moratorium. These assumptions not only closely mirror the empirical setting, but have also a theoretical motivation. Figure C.22 shows the role of each assumptions in driving the moratorium take-up.

First, modeling COVID as a temporary shock reflects our objective of studying temporary liquidity support. The shock should be interpreted as an MIT shock: agents do not anticipate it at $t = 0$, and it lasts for one period, matching the duration of the debt relief. Allowing the shock to persist beyond the relief period would introduce a separate question about the optimal duration of the policy relative to the duration of the shock, which is beyond the scope of this paper.

Second, the decline in revenues alone generates only modest demand for the moratorium. In the absence of a credit crunch and stigma costs, take-up is only 6%. The

tightening of new credit generates most of the demand for deferred debt, which substitutes for scarce new lending. In the benchmark specification, introducing the credit crunch raises zero-cost take-up from 6% to 76%.

Finally, the non-pecuniary participation cost is needed in our benchmark calibration to match the 44% take-up observed in the data. One may ask whether a less severe tightening of credit could reproduce the observed take-up without this wedge. The value of the credit-crunch parameter, ζ, that generates approximately 44% take-up in the absence of stigma costs is 3%, considerably smaller than the 16% average decline in new lending estimated in our AnaCredit.

7.2 Setup

Baseline. The economy consists of a representative consumer and a continuum of firms. Time is discrete and indexed by t. Each of the firms uses capital and labour in a Cobb-Douglas production function to generate output, y, according to:

$$y_{it} = Ae^{z_{it}}(k_{it}^{\alpha} l_{it}^{1-\alpha})^{\theta} \tag{7}$$

where k is the stock of capital, l is labour, z is a productivity shock, α is capital's share, and θ governs the degree of returns to scale. Since labor is fully flexible, before-tax profits net of labor cost, can be expressed as:

$$\pi(k_{it}; z_{it}) = \max_{l_{it}} \{p_{it} y_{it} - w l_{it}\} = \Omega e^{\frac{\phi}{\alpha\theta} z_{it}} k_{it}^{\phi} \tag{8}$$

with $\phi \equiv \frac{\alpha\theta}{1-(1-\alpha)\theta}$ and $\Omega \equiv (1-(1-\alpha)\theta)\left(\frac{(1-\alpha)\theta}{w}\right)^{\frac{1-\alpha}{\alpha}} \phi A^{\frac{\phi}{\alpha\theta}}$.

Revenue-based log-productivity, z_{it}, follows an AR(1) process given by:

$$z_{it} = \rho z_{it-1} + \omega_{it}, \quad \omega_{it} \sim N(0, \sigma_{\omega}^2) \tag{9}$$

Firms invest, I, and capital accumulates following:

$$k_{it+1} = (1-\delta)k_{it} + I_{it}$$

Investment comes with quadratic adjustment costs that take the form:

$$c(k_{it}, k_{it+1}) = \frac{\Gamma(k_{it+1} - (1-\delta)k_{it})^2}{k_{it}}$$

The firm can finance its optimal investment program with external debt or internal equity in the form of retained earnings. We express b as the stock of net debt, so $b > 0$

represents debt, and $b \leq 0$ represents cash. Debt is a one-period risk-free bond. The interest on the debt is r, while the interest that the firm earns on the cash is $r - \xi$.

$$r_b(b) = \begin{cases} r & \text{if } b_t > 0 \\ r - \xi & \text{if } b_t \leq 0 \end{cases} \tag{10}$$

Cash flows to shareholders, $cf(k_t, b_t; z_t)$, are the after-tax net profits to net debt issuance minus net expenditure on investment, plus tax shield and net borrowing, as follows:

$$\begin{aligned} cf_{it}(k_{it}, k_{it+1}; z_{it}) = \pi(k_{it}; z_{it})(1-\tau) \\ - \big(k_{it+1} - (1-\delta)k_{it}\big) - c(k_{it}, k_{it+1}) + \tau(\delta k_{it} + r b_{it}) \\ + b_{it+1} - (1 + r_b(b))b_{it} \end{aligned} \tag{11}$$

As our sample consists of microenterprises without access to financial markets, we assume that firms cannot issue equity, that is we impose the following no-equity issuance constraint:

$$cf_{it}(k_{it}, k_{it+1}; z_{it}) \geq 0 \tag{12}$$

The Bellman equation for the problem is as follows:

$$V(k_{it}, b_{it}; z_{it}) = \max_{k_{it+1}, b_{it+1}} \left\{ cf_{it}(k_{it}, k_{it+1}; z_{it}) + \frac{1}{1+r} \mathbb{E}_t \Big[V(k_{it+1}, b_{it+1}; z_{it+1}) \Big] \right\} \tag{13}$$

$$\text{s.t.} \qquad b_{it+1} \leq \gamma k_{it+1}, \qquad cf_{it}(k_{it}, k_{it+1}; z_{it}) \geq 0.$$

Shock, Credit Crunch, and Moratoria. We introduce the Covid shock at $t = 1$. Firms experience a one-period revenue shock that scales revenues by $(1 - s)$. The shock is accompanied by a credit crunch on the new loans. We model credit crunch as in Buera and Moll 2015

$$b_{i2} \leq (1 - \zeta)\gamma k_{i2},$$

where ζ is the tightening on the usual collateral haircut, γ.

At the same time, the moratorium becomes available to firms with outstanding debt, $b_{i1} > 0$, that can postpone a fraction μ of the payment due at $t = 1$. The moratorium provides immediate liquidity by reducing the debt payment due at $t = 1$ by $\mu(1 + r)b_{i1}$. Postponed payments accrue interest and must be repaid in the following period, hence entering the current financing constraint. The limited-enforcement framework of Rampini and Viswanathan 2010, in which different financial promises compete for a

common stock of debt capacity, provides a theoretical rationale for including deferred payments in the financing constraint. Indeed, If limited enforcement restricts the total promises that can be made against the firm's collateral or future assets, the deferred claim should continue to consume debt capacity. In addition, firms incur a non-pecuniary cost κ, capturing the fear of stigma which we showed anecdotal and empirical evidence of (Section 6.5). We discipline κ using the observed take-up rate of the moratorium.

Conditional on taking up the moratorium, firm i's cash flow at $t = 1$ is

$$\begin{aligned} cf_{i1}^{M}(k_{i1}, k_{i2}; z_{i1}) = (1-s)\pi(k_{i1}; z_{i1})(1-\tau) \\ - [k_{i2} - (1-\delta)k_{i1}] - c(k_{i1}, k_{i2}) + \tau\left(\delta k_{i1} + r b_{i1}\right) \\ + b_{i2} - (1-\mu)(1+r)b_{i1}. \end{aligned} \tag{14}$$

The corresponding Bellman equation is

$$V_{i1}^{M}(k_{i1}, b_{i1}; z_{i1}) = \max_{k_{i2}, b_{i2}} \left\{ cf_{i1}^{M} + \frac{1}{1+r}\mathbb{E}_1\left[V_{i2}^{M}(k_{i2}, b_{i2} + \mu(1+r)b_{i1}; z_{i2})\right] \right\}, \tag{15}$$

subject to

$$b_{i2} + \mu(1+r)b_{i1} \leq \gamma k_{i2} \tag{16}$$

$$b_{i2} \leq (1-\zeta)\gamma k_{i2} \tag{17}$$

$$cf_{i1}^{M} \geq 0 \tag{18}$$

The first constraint accounts for the firm's total debt obligations, while the second captures the temporary credit crunch on new borrowing. The third constraint is the usual no equity issuance constraint.

A firm takes up the moratorium whenever the value of doing so, net of the non-pecuniary cost, exceeds the value of remaining under the Covid shock without the moratorium:

$$\mathbf{1}(\text{Take-up}_i) = \mathbf{1}\left[V_{i1}^{M}(1-\kappa) > V_{i1}\right]$$

Grant counterfactual. We compare the moratorium with a grant that provides the same amount of liquidity at $t = 1$:

$$G_i = \mu(1+r)b_{i1}$$

The grant differs from the moratorium in two dimensions. First, the transfer does not generate a repayment obligation in $t = 2$. Second, receiving the grant entails no non-pecuniary cost, implying full take-up among eligible firms. Cash flow under the

grant at $t = 1$ is:

$$\begin{aligned} cf_{i1}^{G}(k_{i1}, k_{i2}; z_{i1}) = (1-s)\pi(k_{i1}; z_{i1})(1-\tau) \\ - [k_{i2} - (1-\delta)k_{i1}] - c(k_{i1}, k_{i2}) + \tau\left(\delta k_{i1} + r b_{i1}\right) \\ + b_{i2} - (1-\mu)(1+r)b_{i1} \end{aligned} \tag{19}$$

Its Bellman equation is:

$$V^{G}(k_{i1}, b_{i1}; z_{i1}) = \max_{k_{i2}, b_{i2}} \left\{ cf_{i1}^{G} + \frac{1}{1+r}\mathbb{E}_1\left[V(k_{i2}, b_{i2}; z_{i2})\right] \right\}, \tag{20}$$

subject to

$$b_{i2} \leq (1-\zeta)\gamma k_{i2}, \tag{21}$$

$$cf_{i1}^{G} \geq 0 \tag{22}$$

7.3 Estimation

The model is yearly. It has 14 parameters, all of them externally calibrated with the exception of κ which is internally calibrated. Importantly, when non-pecuniary costs are set to zero, take-up is 76%, substantially above the 44% observed in the data. This gap points to a role for non-pecuniary costs in explaining partial take-up. As shown in Table 3, the stigma cost is only 0.04% of the firm value, yet it lowers the take-up by 32 percentage points, among mildly constrained firms.

Model reproduces empirical findings. Figure 6 reports take-up rates and impulse response functions (IRFs) for moratoria-eligible firms (solid blue line), grant-eligible (solid orange line) and the control group (dashed gray line). The plots have to be interpreted as the model analog of the intention-to-treat results obtained in the empirical section.

Moratorium take-up is by construction equal to 44%, while for grants it reaches 100%. The moratorium allows eligible firms to cut investment less than the control group during the shock. For grants, the divestment is even smaller. The impact on net income of both forgiveness and moratorium is small but positive, and indistinguishable between the two groups. Considering that only 44% of the eligible firms use the moratoria, this result already hints at the higher profitability of moratoria-takers. Mechanically, outstanding debt is higher for moratoria during the policy due to postponed repayments. Interestingly, grant-takers delever during the shock, but they correct the trend at time t=2 and remain consistently higher levered that the other two groups. Dividends are

Parameter	Symbol	Value	Target/Source
Externally calibrated			
Adjustment cost	Γ	0.1106	Ivanov et al. (2025)
Capital share	α	0.33	Bartelsman et al. (2013)
Depreciation rate	δ	0.06	Midrigan and Xu (2014)
Interest rate on cash	$r - \xi$	0.0014	Risk-free rate
Interest rate on debt	r	0.02	Corporate loan rate
Leverage cap	γ	0.393	Average leverage in pre-policy period
Moratorium intensity	μ	0.381	Average postponed payment over due payments
Productivity persistence	ρ	0.851	Catherine et al. (2022)
Productivity volatility	σ	0.131	Catherine et al. (2022)
Returns to scale	θ	0.851	Catherine et al. (2022)
Revenue shock (COVID)	s	0.542	95th percentile of 2019–2020 decrease in sales
Tax rate	τ	0.25	Statutory corporate tax rate
Internally calibrated			
Stigma	κ	0.0002	Match 44% take-up
Credit-crunch parameter	ζ	0.1165	Match 16% average 2019–2020 decrease in new lending

Table 3: Parameters

Notes: This table summarizes the parameters of our models that are calibrated externally and internally.

permanently higher for grants. This evidence is consistent with Autor et al. (2022a) and Autor et al. (2022b) which find that grant funds were diverted to shareholders.

One limitation of the model is its inability to generate, at $t = 1$, the larger decline in payouts among eligible firms relative to the control group that we observe empirically during the policy period (Figure C.17). This behavior is not new in the literature. Kaboski and Townsend (2011) rationalize the decline in consumption among some households receiving funds from a microcredit program through investment indivisibilities. In the corporate finance literature, Zwick and Mahon (2017) interpret the decline in dividends in response to bonus depreciation as consistent with non-convex adjustment costs (Caballero and Engel 1999; Cooper and Haltiwanger 2006). For simplicity, we do not incorporate this mechanism into the model, but lumpy investment may similarly rationalize the decline in payouts observed in our setting.

7.4 Selection and Cost-benefit analysis

In this section, we present the two main results emerging from the comparison of moratoria to grants. First, moratoria come with virtuous self-selection of firms that dispose of profitable projects. Second, moratoria stimulate investment at substantially lower fiscal cost. We conclude the section with a *caveat*: defaults and crowding out may downsize the positive evaluation of moratoria.

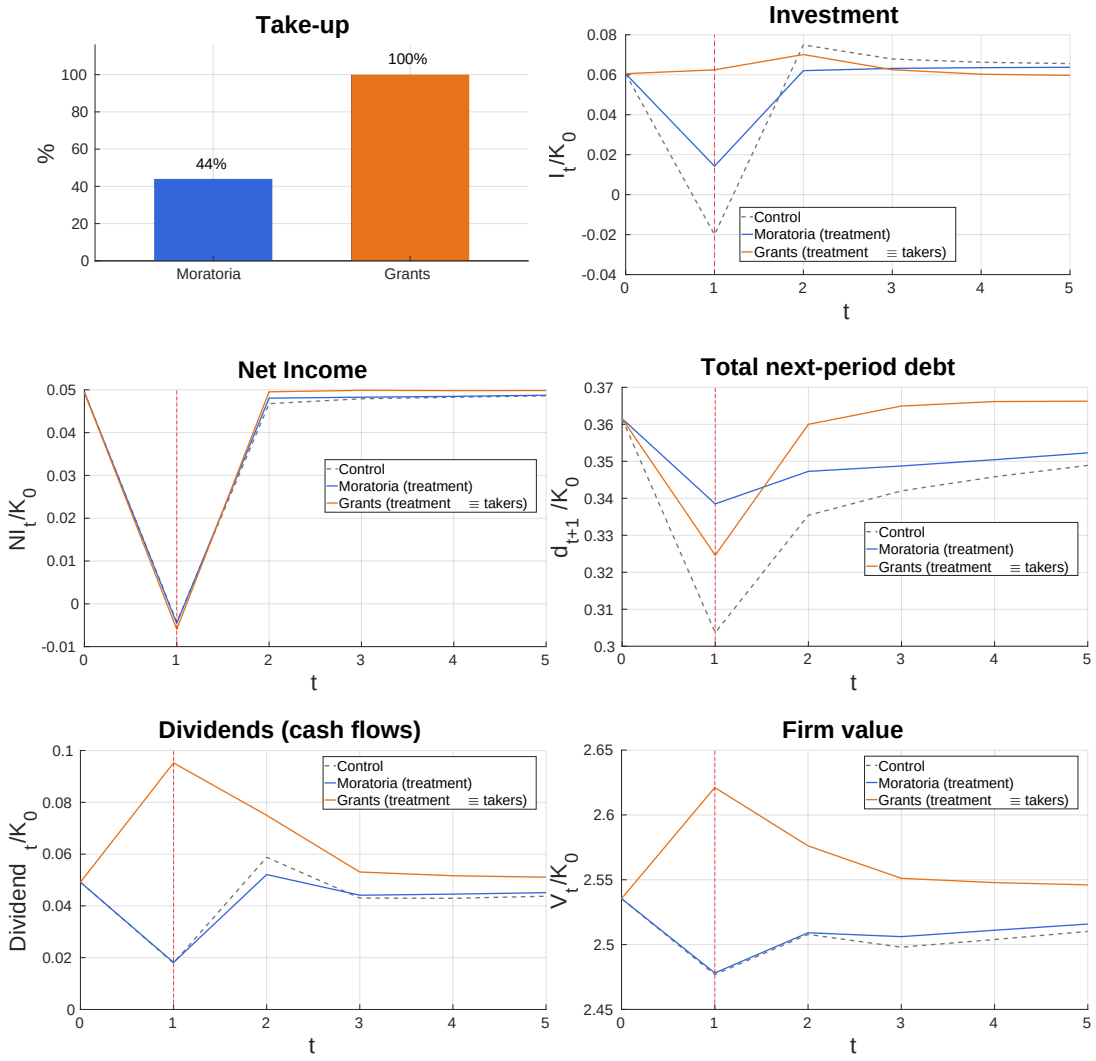

Figure 6: Impulse Response Functions from Model: Moratoria vs Grants

Notes: The figure compares model-implied take-up and firm outcomes under moratoria and grants. The COVID shock and policy interventions occur at $t = 1$, with $t = 0$ denoting the pre-shock period. Blue and orange solid lines represent the moratoria and grants scenarios, respectively; the dashed line represents the counterfactual with the same COVID shock and credit conditions but no policy support. Moratoria outcomes include both takers and non-takers. Monetary outcomes are scaled by the common pre-shock capital stock, K_0. Profitability is measured by net income, and next-period debt includes postponed payments under the moratoria.

Virtuous self-selection of moratoria. To evaluate how effectively the two policies channel liquidity, it is important to understand which firms each policy reaches. The empirical findings are not able to evaluate how take-up depends on ex-ante projects' profitability. Indeed, this is usually a private information of the firms and hard to measure in the data. The model fills this gap.

Figure 7 shows that moratoria come with virtuous self-selection. The take-up indeed increases with the intensity of the credit crunch and firm productivity. Grants, instead, are a free lunch and therefore generate full take-up regardless of financial constraints

or the profitability of investment opportunities. As a result, grants also provide funds to firms without positive-NPV investment opportunities and to firms that could have obtained financing through traditional bank lending. The shaded area between the two lines highlights the cost-inefficient allocation of public funds.

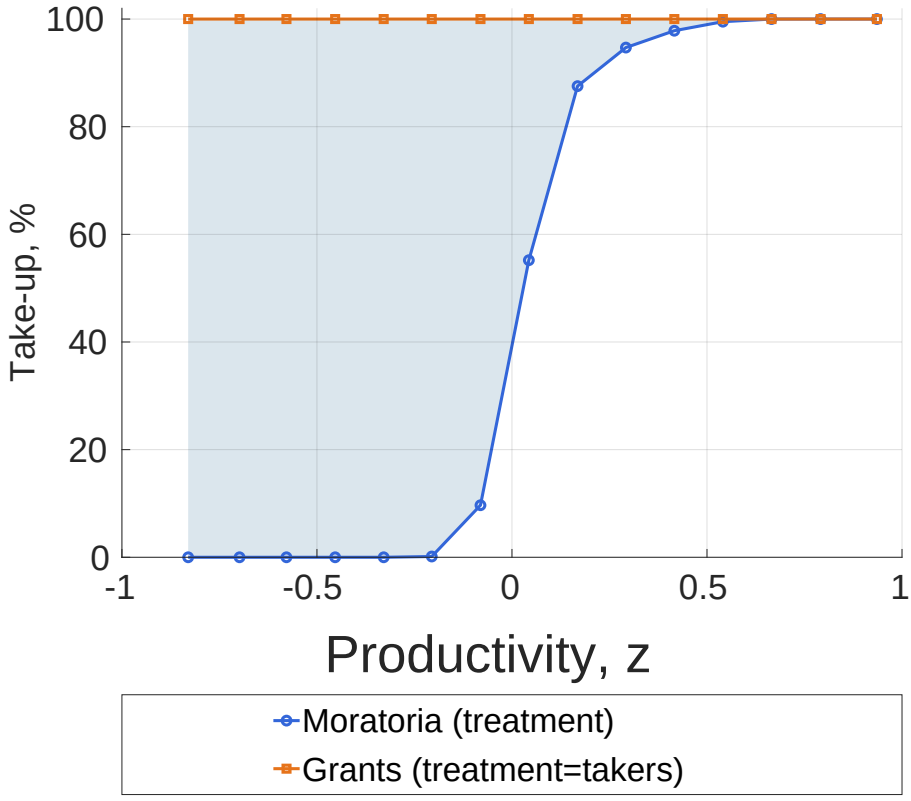

Figure 7: Take-up across productivity

Notes: The figure shows take-up among eligible firms under moratoria (blue) and grants (orange) across fifteen equally spaced productivity bins under the baseline specification; each point is plotted at the bin's mean productivity. The shaded area highlights the difference in take-up between the two policies.

In other words, moratoria generate a separating equilibrium in which only firms that need liquidity to undertake profitable investments select into the policy. This mechanism is particularly relevant for policy design. Eligibility criteria based on firm size are often used to target support toward firms perceived to be most in need. However, as recently shown by Brown et al. 2026, changes in program eligibility thresholds can generate unintended consequences. Moratoria reduce the need for such targeting: rather than relying on eligibility criteria to identify firms in need of support, the policy induces firms with valuable investment opportunities and scarce external financing to self-select into the program.

Cost-benefit analysis. We compare moratoria and grants by examining their investment effects, recipient selection, and fiscal costs. We first show how policy-provided

liquidity affects investment, new borrowing, and shareholder payouts. We then use counterfactual assignments to distinguish the effects of policy design from those of self-selection into moratoria. Finally, we compare the investment responses with the fiscal costs of providing relief.

We evaluate policy effects using the same initial population of firms and identical productivity innovations across scenarios. The policy economy includes both recipients and non-recipients. Our comparison therefore measures the effect of introducing the policy, rather than comparing firms that participate with those that do not.

For each policy or counterfactual assignment P, we define the marginal propensity to invest as

$$\text{mpi}^P = \frac{\overline{I}_1^P - \overline{I}_1^0}{L_P/N} = \frac{\sum_{i=1}^N (I_{i1}^P - I_{i1}^0)}{L_P},$$

where L_P is total liquidity provided, N is the initial number of firms, and superscript 0 denotes the no-policy counterfactual. Average liquidity includes zero relief for non-recipients. This measure corresponds to the investment IRF gap at $t = 1$, divided by average policy liquidity, with both quantities normalized by average pre-policy capital.

In our empirical setting, banks provide moratorium relief without a direct government disbursement. To assign a comparable fiscal cost, we consider a counterfactual interest-free government bridge loan to banks. The government advances the postponed repayments at $t = 1$ and recovers the principal in full at $t = 2$. Discounting at rate r, the present-value fiscal costs are

$$C_M = L_M - \frac{L_M}{1+r} = \frac{r}{1+r} L_M, \qquad C_G = L_G.$$

Figure 8 illustrates how moratorium and grant recipients allocate the liquidity received and the total funds available under each policy. For moratoria, 43 cents of each euro of relief are invested, while almost nothing is distributed to shareholders. Since postponed repayments enter the financing constraint, moratoria partly substitute for new borrowing, resulting in fewer total funds available than under grants. This contrasts with our empirical finding that moratoria do not affect the origination of new loans (Figure C.5). One possible explanation is the presence of other relief programs during the COVID-19 crisis, which may have mitigated the reduction in credit availability for moratorium recipients. This discrepancy also highlights the differences between the stylized model and the broader policy environment captured by the empirical analysis. By contrast, grant recipients allocate approximately equal shares of each euro received to investment and shareholder payouts. New borrowing also increases slightly, as grants raise cash flows without tightening the financing constraint.

When investment is measured relative to total available funds, moratorium recipients

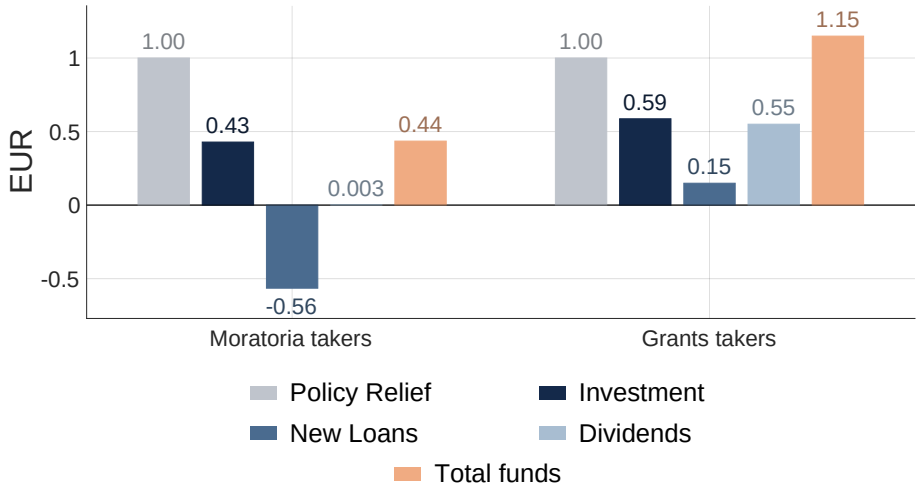

Figure 8: Use of Relief

Notes: The figure shows how moratorium and grant takers use each euro of liquidity provided at $t = 1$. Investment, new loans, and dividends are measured relative to the same firms' outcomes without policy intervention and normalized by total liquidity received under each policy. Total funds equal policy relief plus the change in new loans.

invest 98%, compared with only 51% among grant recipients. This finding is consistent with the beneficial self-selection illustrated in Figure 7: the 54% of grant recipients with negative productivity do not undertake negative-NPV projects, instead distributing the funds to shareholders.

We give more structure to this observation by performing two counterfactual exercises that hold the recipient set fixed while changing the policy instrument. In the first exercise, *moratoria without selection*, we assign moratoria to all firms that choose grants in the baseline model. Each recipient postpones $\mu(1+r)d_{i1}$, and the deferred amount enters its next-period debt. Assignment is mandatory: firms cannot opt out, even if they would prefer the no-policy alternative. The proportional stigma cost κ continues to apply, so firm-value effects can be negative for some recipients. In the second exercise, *targeted grants*, we give grants to the firms that choose moratoria in the baseline model. Each recipient receives a grant equal to the payment it would otherwise postpone, $\mu(1+r)d_{i1}$. This exercise compares temporary liquidity relief with a permanent transfer to the same firms, holding total liquidity provided constant.

Figure 9 highlights that the value of moratoria lies in their beneficial self-selection: when this mechanism is removed, *moratoria without selection* generate the lowest marginal propensity to invest. By contrast, *targeted grants*, which can be interpreted as an "oracular" policy that directs funds exclusively to firms with profitable investment opportunities, generate the highest marginal propensity to invest, with 97 cents invested per euro of relief. These results reinforce the findings in Figure 8: moratorium

recipients exhibit a higher marginal propensity to invest than grant recipients because, unlike grants, moratoria induce self-selection among firms with profitable investment opportunities.

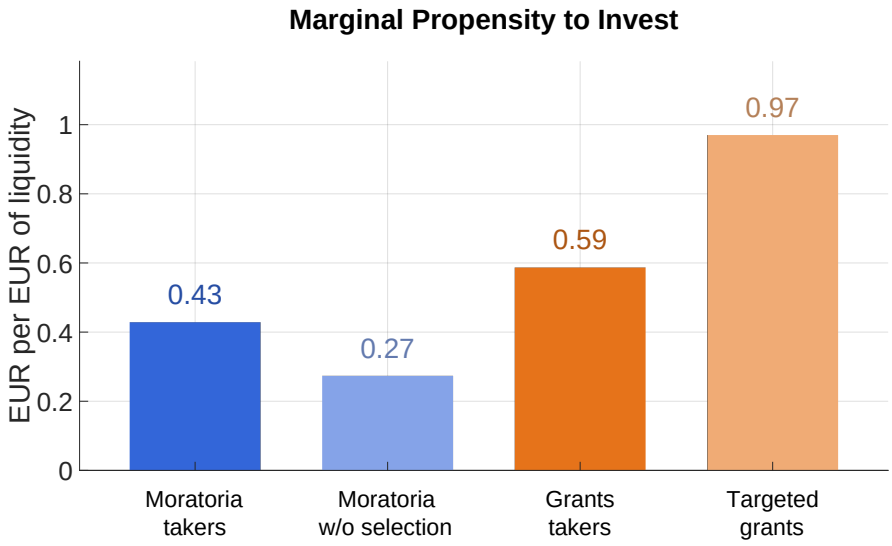

Figure 9: Marginal Propensity to Invest across Policy Counterfactuals

Notes: The plot shows the marginal propensity to invest out of the relief across different policy counterfactuals. Moratoria without selection assigns moratoria to all baseline grant takers, without allowing them to opt out; Targeted grants assigns grants to baseline moratorium takers.

While *targeted grants* would be an ideal policy, implementing such targeting is challenging due to asymmetric information about projects' ex-ante profitability. Firms have private information about their investment opportunities and may lack incentives to disclose it truthfully to governments. Beyond these implementation challenges, *targeted grants* also entail fiscal costs, as illustrated in Figure C.23. Grants impose the highest fiscal cost, while moratoria entail the lowest fiscal burden under the assumption of no defaults.

A more comprehensive evaluation of the fiscal costs of moratoria would also account for the expected costs of government bailouts in the event of widespread defaults. Although such an analysis lies beyond the scope of this paper, we acknowledge it as an avenue for future research.

7.5 A Word of Caution: the Omitted Costs of Moratoria

Our empirical and theoretical analysis points to an overall positive evaluation of moratoria as a fiscally cost-effective policy to support firms' investment. Because they require no direct fiscal outlay, moratoria may be especially appealing to highly indebted governments.

A word of caution is however warranted. Our results come from an episode in which firms received multiple forms of government support and default rates remained exceptionally low. The absence of defaults is central to the favorable cost-benefit assessment of moratoria. When firms repay the postponed amounts, the policy imposes neither a direct fiscal cost on the government nor significant losses on banks. In other settings, however, a wave of defaults after payments resume could shift the cost of the policy onto the banking sector. This "cliff effect" was a central concern of policymakers during the pandemic. In an extreme scenario, sufficiently large bank losses could ultimately require government intervention, turning an apparently costless policy into a fiscal liability.

Default risk also helps explain why government intervention may be necessary to implement moratoria. Austrian banks were already offering payment suspensions at the onset of the pandemic, but extended them to healthier borrowers. While our anecdotal evidence suggests that banks benefited from the policy *ex post*, when defaults turned out to be limited, *ex ante* the policy required them to extend relief to riskier borrowers during a period of exceptional uncertainty. This creates a coordination problem: an individual bank may be unwilling to bear the additional risk associated with supporting distressed borrowers, even when broad participation in the policy can improve outcomes for firms and the economy as a whole.

Regulators partly addressed this problem by preventing moratoria from mechanically triggering adverse loan classifications. In Europe, the EBA clarified that participation in an eligible general moratorium did not by itself trigger classification as forbearance or default through distressed restructuring. The United States adopted a similar, though non-mandatory, approach: regulators encouraged banks to modify loans and allowed qualifying COVID-related modifications to avoid automatic classification as troubled debt restructurings. These measures reduced the risk that providing temporary payment relief would itself generate an immediate increase in provisions and pressure on bank capital.

Importantly, this regulatory relief did not suspend the recognition of genuine credit deterioration. Under the European prudential framework, default could still occur if a bank judged a borrower "unlikely to pay," independently of the 90-days-past-due criterion. The EBA explicitly required banks to continue assessing borrowers' ability to repay and identifying exposures facing longer-term financial difficulties. This distinction is visible in our data: despite the favorable regulatory treatment of moratoria, Austrian banks became more likely to classify eligible firms as "unlikely to pay" during the policy. Thus, banks used the discretion preserved by the regulatory framework to recognize credit risk even though participation in the moratorium did not mechanically trigger

default.

This behavior helps rationalize both the regulatory intervention and the mandatory nature of the Austrian policy. Regulators sought to prevent a temporary, economy-wide liquidity shock from mechanically generating higher provisions and tighter bank capital constraints, while leaving banks responsible for recognizing genuine deterioration in borrowers' creditworthiness. At the same time, banks' willingness to classify moratorium-eligible borrowers as unlikely to pay suggests that they remained concerned about the credit risk associated with the policy. Government intervention therefore helped separate the provision of temporary liquidity relief from banks' individual willingness to bear the associated risk.

Thus, while moratoria may be particularly appealing to highly indebted governments because they require no direct fiscal outlay, their ultimate cost depends critically on firms' ability to repay once the policy expires. The fiscal advantage of moratoria comes with an important qualification: default risk can shift the cost of supporting firms from the government to the banking sector and, in sufficiently severe crises, potentially back to the government.

8 Conclusion

Understanding how firms' investment responds to liquidity is a central question in corporate finance. This paper studies the response to temporary liquidity that firms must eventually repay. Loan moratoria provide a suitable quasi-experiment because they temporarily postpone debt service without forgiving firms' obligations. Furthermore, they allow us to study the response during a crisis, exactly the moment when a government may want to intervene to support investment.

We study the 2020 Austrian moratorium on corporate debt, which provided participating microenterprises with an average of EUR 74,000 over 8 months in freed-up liquidity. We estimate that firms invest EUR 0.76 out of each euro of postponed payments, and that the increase in investment is accompanied by higher profitability. Yet only 44% of eligible firms participate in the policy. A dynamic investment model rationalizes the investment response and incomplete take-up and allows us to compare moratoria with equivalent grants. Moratoria induce virtuous self-selection: financially constrained firms with profitable investment opportunities choose to participate, while grants generate full take-up. Although the two policies generate similar gains in firm value per euro of fiscal cost, moratoria generate substantially more investment per euro of fiscal cost.

These findings have several policy implications. First, temporary relief programs providing liquidity that must be eventually repaid can induce firms to self-select into

support. This mechanism can help target liquidity toward firms that value it most, reducing the burden on policymakers of identifying these firms *ex ante*. Second, because moratoria require no direct fiscal outlay when firms repay, they may be particularly attractive to highly indebted governments.

The favorable evaluation of moratoria comes with important limitations. Most importantly, our results come from an episode in which firms received multiple forms of government support and defaults remained exceptionally low. The absence of defaults is central to the fiscal advantage of moratoria: if firms fail to repay once payments resume, losses shift to the banking sector and may ultimately generate fiscal costs. Our analysis is also partial equilibrium and therefore does not capture potential spillovers onto ineligible firms or aggregate credit supply.

More broadly, moratoria demonstrate that the timing of liquidity matters when firms face financing constraints. Temporary liquidity provision can therefore support investment during crises at a low fiscal cost.

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A Other COVID policies

Concerns may arise that our identification strategy is biased by concurring policies implemented during the same period and targeted to the same period. While many other policies supported firms in the hardship of COVID, none specifically targeted microenterprises. In this section, we list alternative policies and show that they do not interfere with our identification strategy.

Corona-Kurzarbeit *Corona-Kurzarbeit* is an emergency short-time work scheme that subsidizes wages so firms can sharply reduce hours without laying off workers. The public employment service (AMS) reimburses most of the cost of non-worked hours, so that employees typically receive about 80–90% of their previous net income despite large hour reductions. Employees of all private and some public Austrian firms were eligible to the *Kurzarbeit*.

Credit guarantees. For SMEs, one-person businesses, liberal professions and some primary sector firms, the Austrian government provided 100% guarantees for new loans up to EUR 0.5 million EUR, 90% for larger loans (with caps linked to turnover/payroll), and an 80%, for loans typically with maturities up to 5 years and fast-track approval via the house bank. Large enterprises facing COVID-related liquidity shortages were provided up to 90% guarantees for new investment and working-capital loans. Maximum loan sizes were tied to turnover or payroll and guarantee maturities were up to 6 years, in line with the EU Temporary Framework.

Degressive depreciation for investment. Since July 1, 2020, firms may use declining-balance depreciation for movable assets, allowing up to 30% of an asset's residual value to be depreciated annually and front-loading deductions relative to straight-line depreciation.

Fixkostenzuschuss. The *Fixkostenzuschuss* consists in grants provided by the Austrian government to cover firms' operating costs. Companies that suffered a loss in sales of at least 40% due to COVID-19 were eligible. The measure amounts to EUR 12 billion.

Härtefallfonds. The *Härtefallfonds* consists in transfers from a hardship fund to self-employed individuals, one-person businesses, micro-enterprises, and certain freelancers who were severely affected by COVID-19 and unable to benefit from other main support instruments such as loan guarantees or short-time work. The fund is intended to compensate for losses in self-employment income. Firms could apply to obtain monthly

subsidies from March 2020 to March 2021. The total disbursement was around EUR 900 million and the majority of take-up was from one-person enterprises. This is the only policy, among the ones available to enterprises, which may constitute a concern for our identification strategy. However, it is important to notice the sizably smaller uptake of the Härtefallfonds in comparison to the EUR 23 billion provided the moratorium. The fact that the main beneficiaries were one-person firms, while we focus around the 2 M cutoff which corresponds to firms with around 10 employees, is an evidence in support of our strategy. Furthermore, *Härtefallfonds* was meant to compensate for losses in self-employment income rather than bridging liquidity shortages.

Loss carryback. Since July 2020, firms may carry back up to EUR 5 million of 2020 losses to offset profits from 2019—and, with restrictions, from 2018—triggering refunds of previously paid taxes. Before this reform, losses could only be carried forward, delaying their tax benefit and preventing use when no future profits were expected.

Tax deferral and tax pre-payments. Firms could request interest-free deferrals or installment plans for assessed taxes (notably income tax, corporate tax and VAT) until January 2021. Furthermore, advance payments of income and corporate tax for 2020 can be reduced in line with expected lower profits, including setting prepayments to zero if no 2020 tax liability was anticipated. Where prepayments were reduced and a positive final 2020 tax bill later arose, no interest was charged on the additional amount.

Wirtshauspaket. The *Wirtshauspaket* consists in a support package, combining tax reliefs with demand *stimuli*, for hospitality venues. The measure amounts to EUR 600 million.

B Defaults

One major concern surrounding the moratorium is the potential for a cliff effect[12]: when firms resume repayments on suspended loans, they may be unable to do so and default. We analyze the evolution of defaults induced by the moratorium.

AnaCredit disposes of a debtor's default status which is defined following the Capital Requirements Regulation[13]. The default flag is positive if either one of the two conditions is met: (i) the institution considers that the obligor is unlikely-to-pay its credit obligations, (ii) the obligor is more than 90 days past due on any material credit obligation to

12. https://www.ecb.europa.eu/press/financial-stability-publications/fsr/special/html/ecb.fsrart202011_01~47160f35a4.en.html
13. Article 178 - Regulation 575/2013 (CRR).

the institution. For the purposes of point (i), elements to be taken as indications of unlikeliness to pay include:

(a) the institution puts the credit obligation on non-accrued status

(b) the institution recognises a specific credit adjustment due to a significant perceived decline in credit quality subsequent to the institution taking on the exposure

(c) the institution sells the credit obligation at a material credit-related economic loss

(d) the institution consents to a forbearance measure where that measure is likely to result in a diminished financial obligation (e.g., forgiveness or postponement of principal, interest or fees)

(e) the institution has filed for the obligor's bankruptcy or a similar order in respect of the obligor's credit obligation to the institution, the parent or any subsidiaries

It is important to note that condition (d) cannot be triggered by moratorium usage, as the EBA guidelines[14] explicitly state that "payment moratoria do not trigger forbearance classification or the assessment of distressed restructuring".

AnaCredit further identifies whether the default flag is activated through conditions (i) or (ii). This distinction is crucial for disentangling whether observed default dynamics reflect firms' inability to repay or banks' risk reassessments.

14. EBA/GL/2020/02.

C Additional Figures

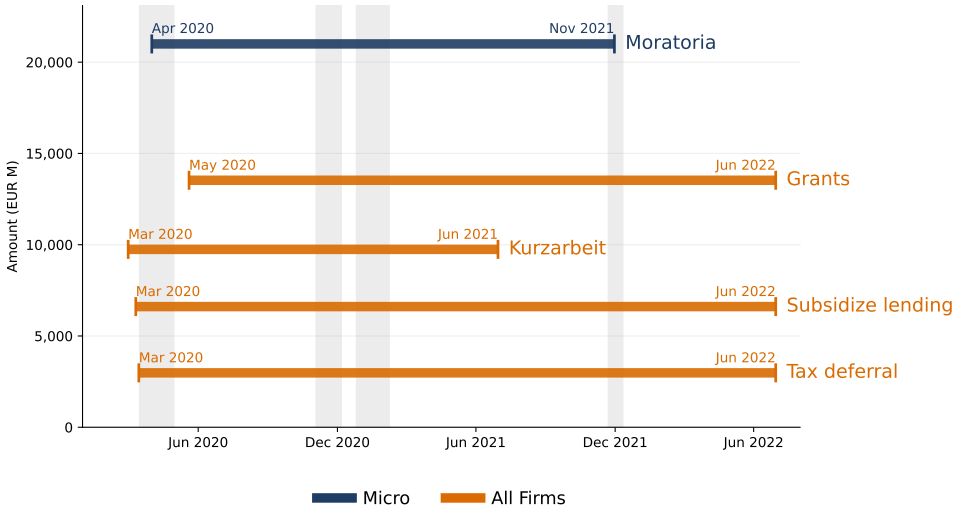


Figure C.1: Financial Support Policies for Firms during Covid

Notes: This figure summarizes the timing and magnitude of the main Covid credit and liquidity support policies in Austria. Horizontal bars indicate the period during which each policy was in place, while their vertical position reports the total amount of support (EUR million). The moratorium, shown in blue, was the only policy specifically targeted at microenterprises; all other interventions, shown in orange, were available more broadly to firms. Gray shaded areas indicate lockdown periods. Data are from OeNB and *Bundesministerium für Finanzen* (Austria's Federal Ministry of Finance).

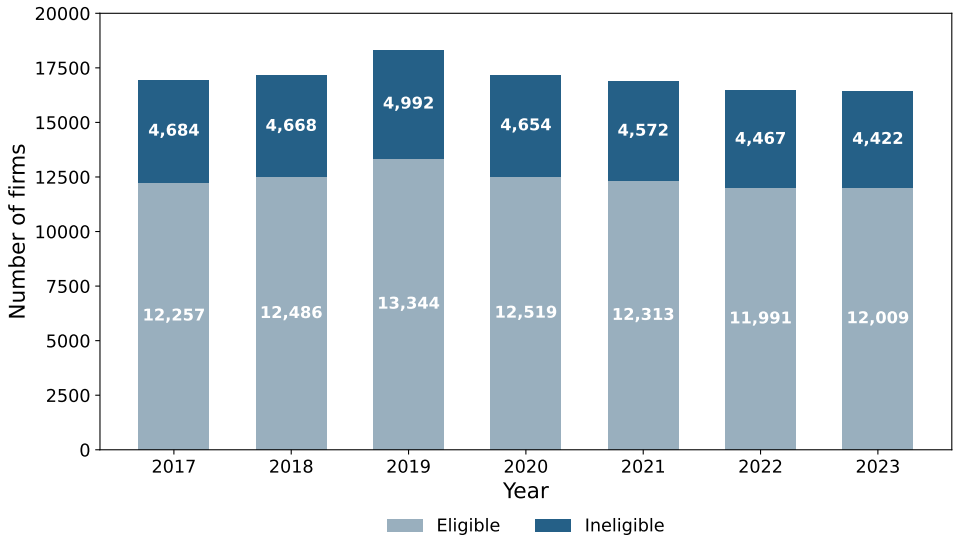


Figure C.2: Number of Eligible and Ineligible Firms by Year

Notes: The plot shows the number of eligible (gray) and ineligible (blue) firms for each year. The analysis uses the AMDC-Orbis dataset.

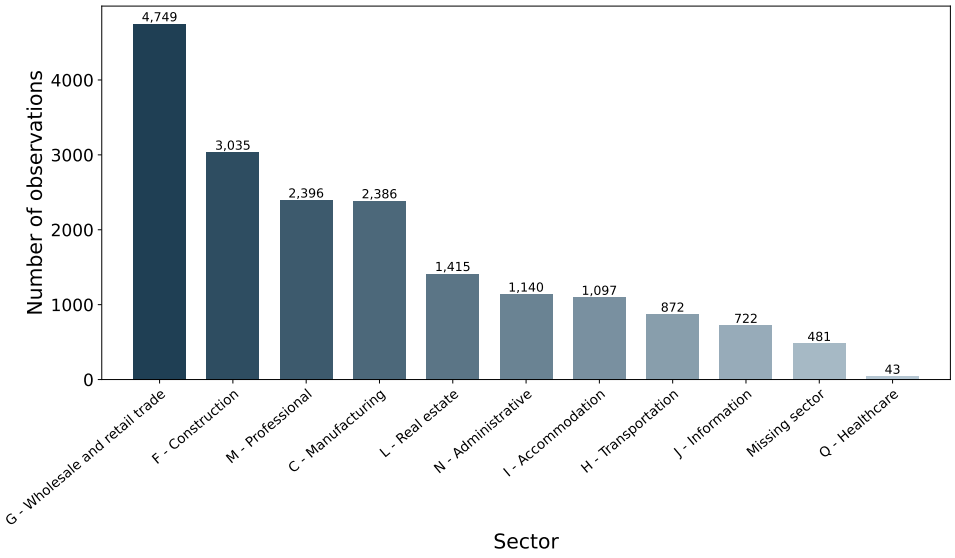


Figure C.3: Number of Firms by Sector

Notes: The plot shows the number of firms for each sector in 2019. The analysis uses the AMDC-Orbis dataset.

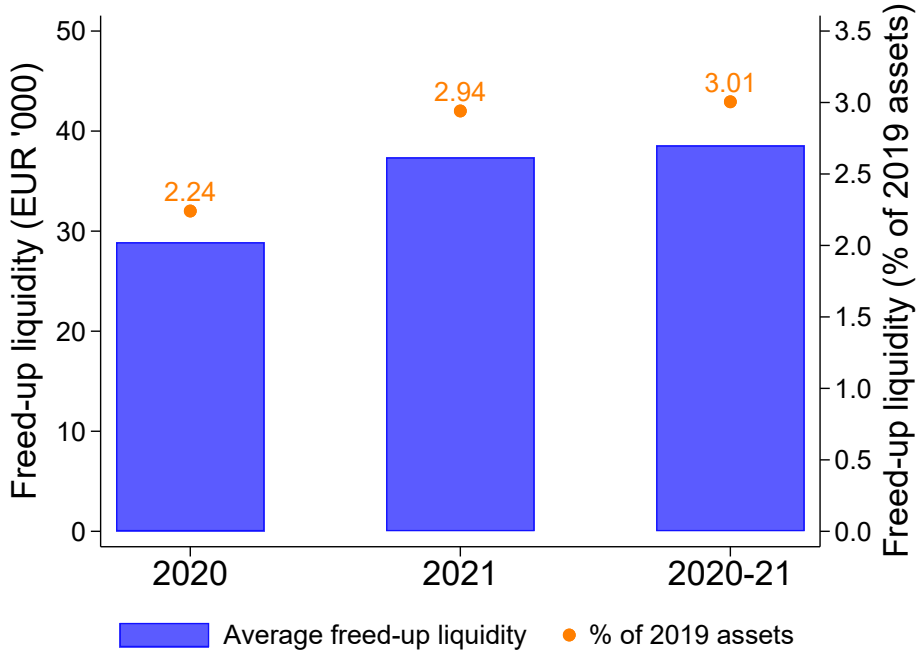


Figure C.4: Robustness measure of freed-up liquidity

Notes: The plot shows the average amount of liquidity per firm freed by the moratoria in 2020, 2021, and over the two years, as EUR amount (blue bars), and as percentage of 2019 assets (orange dots). The analysis uses the AnaCredit-Orbis dataset. For each firm i in a year t, freed-up liquidity is obtained as $FreedLiq_{it} = \sum_m \left(DuePayments_{i,m} - ActualPayments_{i,m}\right)$.

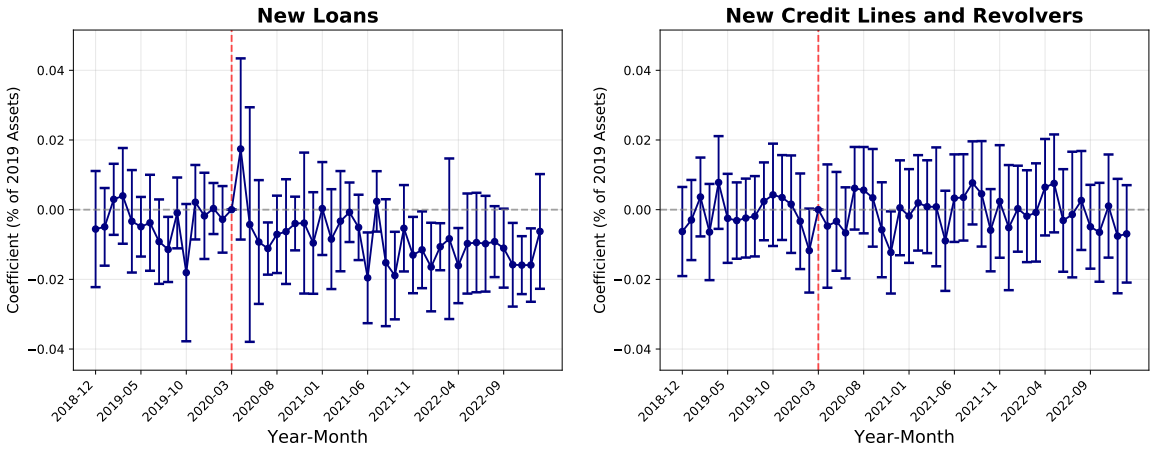


Figure C.5: Effect of Moratorium on New Loans

Notes: The plots show the coefficient resulting from the dynamic difference-in-differences

$$y_{i,m} = \iota_i + \sigma_{sm} + \sum_{h \neq 2020M3} \gamma_h \mathbf{1}(Elig_i) \times \mathbf{1}(m = h) + \eta_{im}$$

where i indexes firms, m indexes time in months, s indexes sector. The dependent variables are the firm's total amount outstanding of loans newly originated in that month. $Elig_i$ is an indicator equal to one for firms with 2019 assets below EUR 2 million. Firm and sector-by-month fixed effects are included. Dots report the estimated coefficients, γ_h, and error bars denote 95% confidence intervals. The omitted month is March 2020. The analysis uses the matched AnaCredit-Orbis dataset.

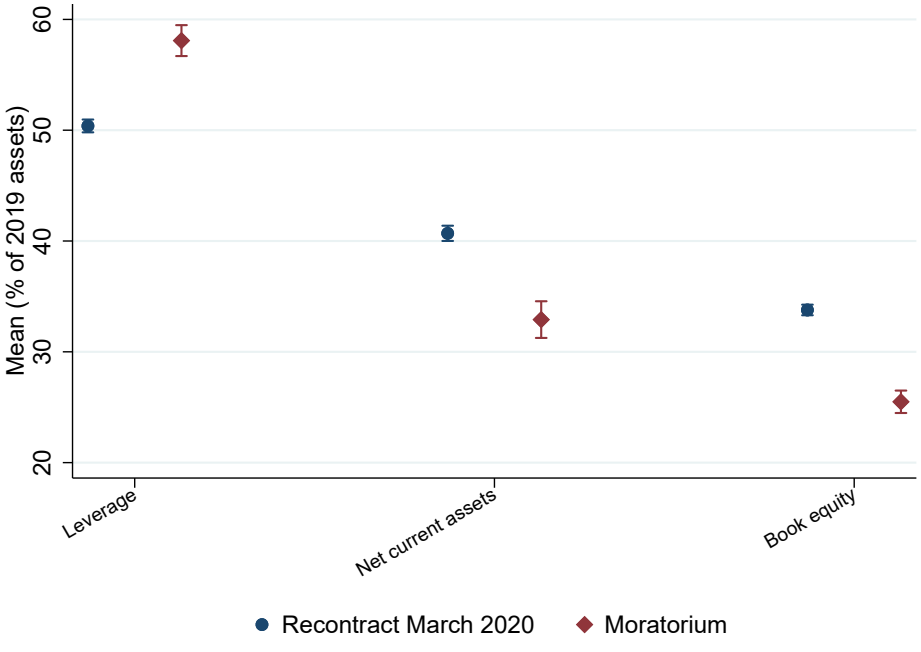


Figure C.6: Firm's characteristics: Recontracting in March 2020 vs Moratoria

Notes: The plots show the average and 95% confidence intervals of selected loans' characteristics as of December 2019 for firms that recontracted at least one of their loans in March 2020 before moratoria were announced (blue) and for firms that used moratoria on at least one of their loans (red). The selected variables are: leverage, net current assets, and equity. The analysis uses the AnaCredit-Orbis dataset.

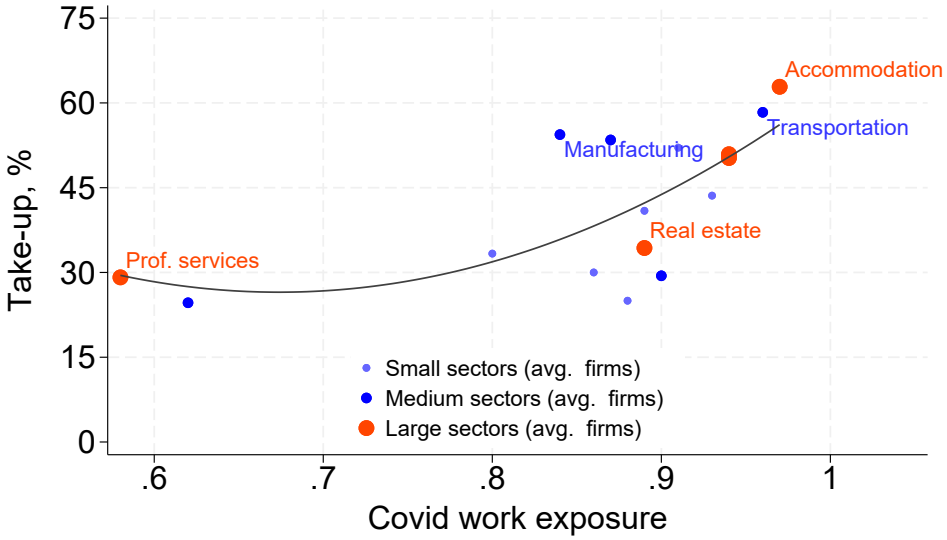


Figure C.7: Take-up across Sectors

Notes: The figure presents, out of eligible firms, the percentage of firms that used the moratorium on at least one of their loans across different work-exposure as computed by Papanikolaou and Schmidt 2022. Each dot represents a sector.

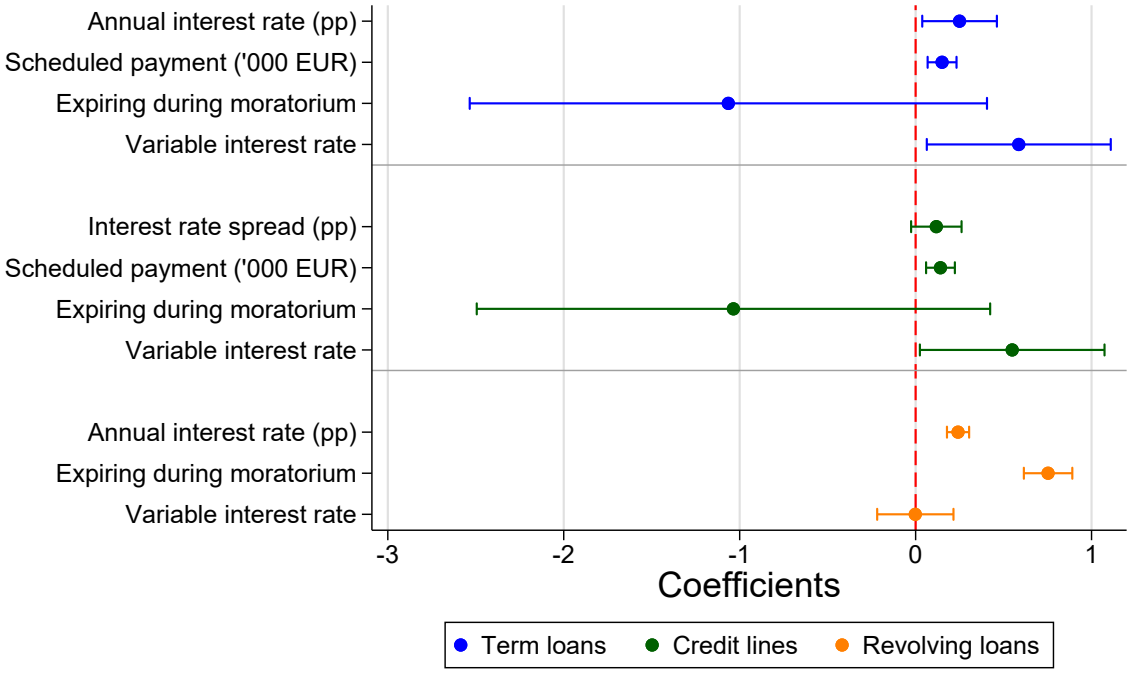


Figure C.8: Loan characteristics predicting moratoria take-up

Notes: This figure shows the coefficients obtained through a penalized logistic regression using the LASSO estimator. The pre-policy variables are defined as of December 2019. Each panel reports for each type of instrument - (a) term loan, (b) credit lines, (c) revolving loans - the selected variables. The analysis uses the AnaCredit-Orbis dataset.

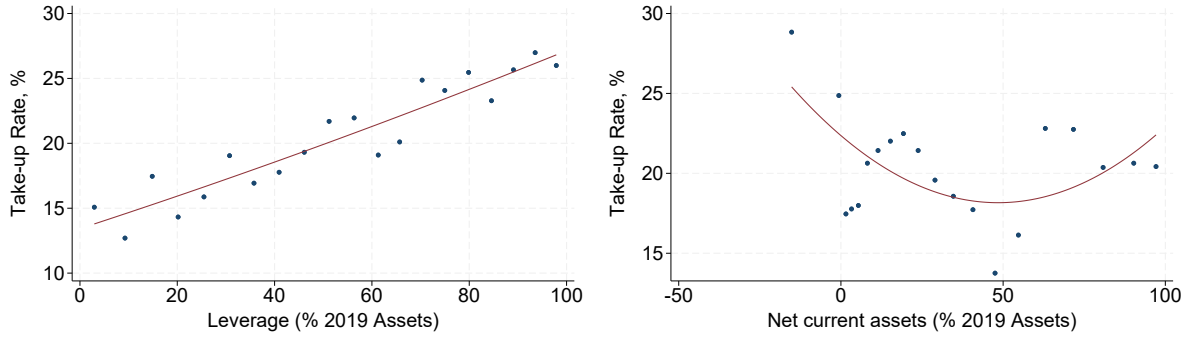


Figure C.9: Take-up by Financial Ratios

Notes: This figure presents binscatter plots of take-up by bins of pre-policy leverage (LHS) and pre-policy net current assets (RHS), both as percentage of total assets. Pre-policy variables are defined as of 2019. The dataset used is Orbis restricted to the selected of 0.8-3.2 million of 2019 assets. The analysis uses the AnaCredit-Orbis dataset.

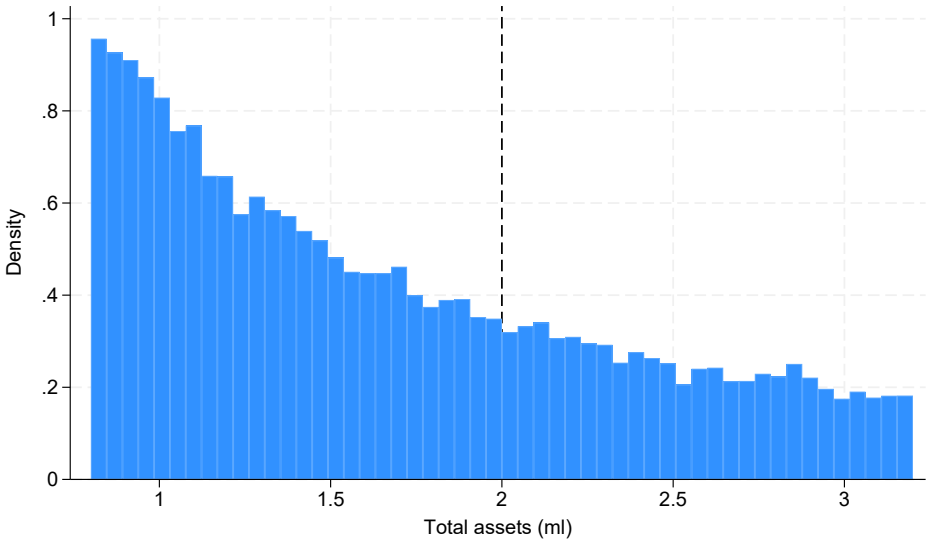


Figure C.10: Total Assets in 2019

Notes: This figure shows the density distribution of firms over 2019's total assets in million euros. The vertical line at 2 indicates the eligibility cutoff.

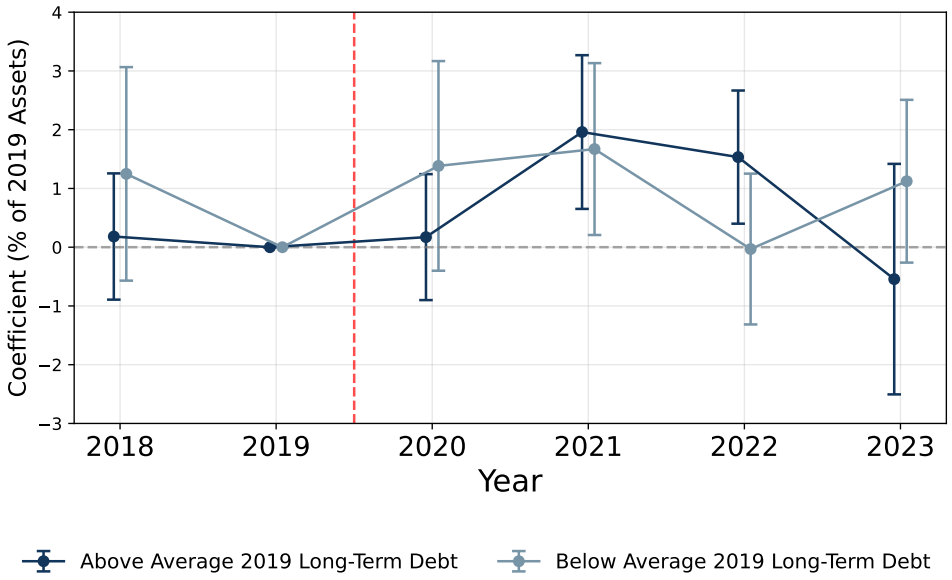


Figure C.11: Change in Long-term Debt by Pre-policy Leverage

Notes: The plots show the coefficient resulting from the dynamic difference-in-differences

$$\frac{y_{i,t}}{at_{i,2019}} = \iota_i + \sigma_{st} + \sum_{h \neq 2019} \gamma_h \mathbf{1}(Elig_i) \times \mathbf{1}(t = h) + \eta_{it}$$

where i indexes firms, t indexes time, s indexes sector. The blue (gray) line shows the results for firms that in 2019 have long-term above (below) the average. The dependent variable is scaled by firms' 2019 total assets, $at_{i,2019}$, and winsorized at the 1st and 99th percentiles. $Elig_i$ is an indicator equal to one for firms with 2019 assets below EUR 2 million. Firm and sector-by-year fixed effects are included. Dots report the estimated coefficients, γ_h, and bars denote 95% confidence intervals. The analysis uses the AMDC-Orbis dataset.

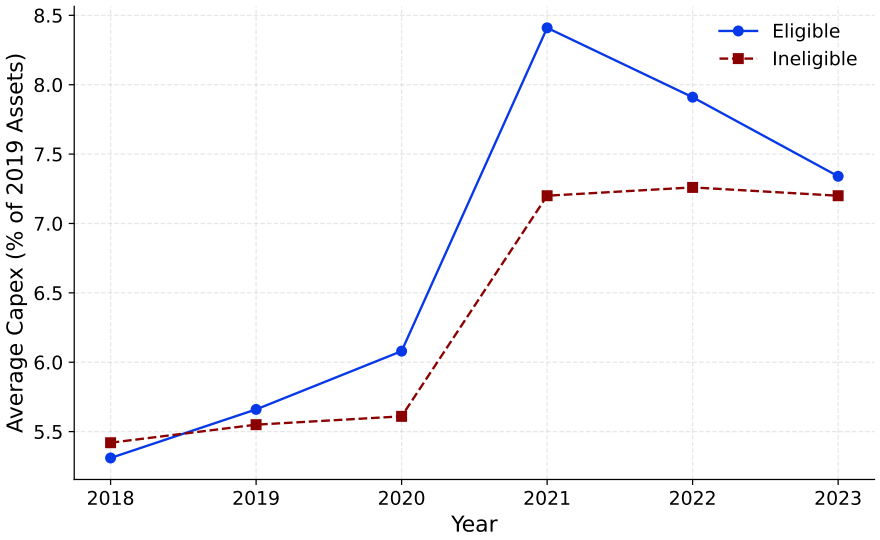


Figure C.12: Time series of capex for eligible and ineligible firms

Notes: The plot shows the time series of average capex as percentage of 2019 assets for eligible (blue solid line) and ineligible (red dashed line) firms.

Figure C.13: Effect of Moratorium on Investment and Profitability

Notes: The plots show the coefficient resulting from the dynamic difference-in-differences

$$\frac{y_{i,t}}{at_{i,2019}} = \iota_i + \sigma_{st} + \sum_{h \neq 2019} \gamma_h \mathbf{1}(Elig_i) \times \mathbf{1}(t = h) + \eta_{it}$$

where i indexes firms, t indexes time, s indexes sector. The dependent variable is scaled by firms' 2019 total assets, $at_{i,2019}$, and winsorized at the 1st and 99th percentiles. $Elig_i$ is an indicator equal to one for firms with 2019 assets below EUR 2 million. Firm and sector-by-year fixed effects are included. Dots report the estimated coefficients, γ_h, and bars denote 95% confidence intervals. The analysis uses the AMDC-Orbis dataset.

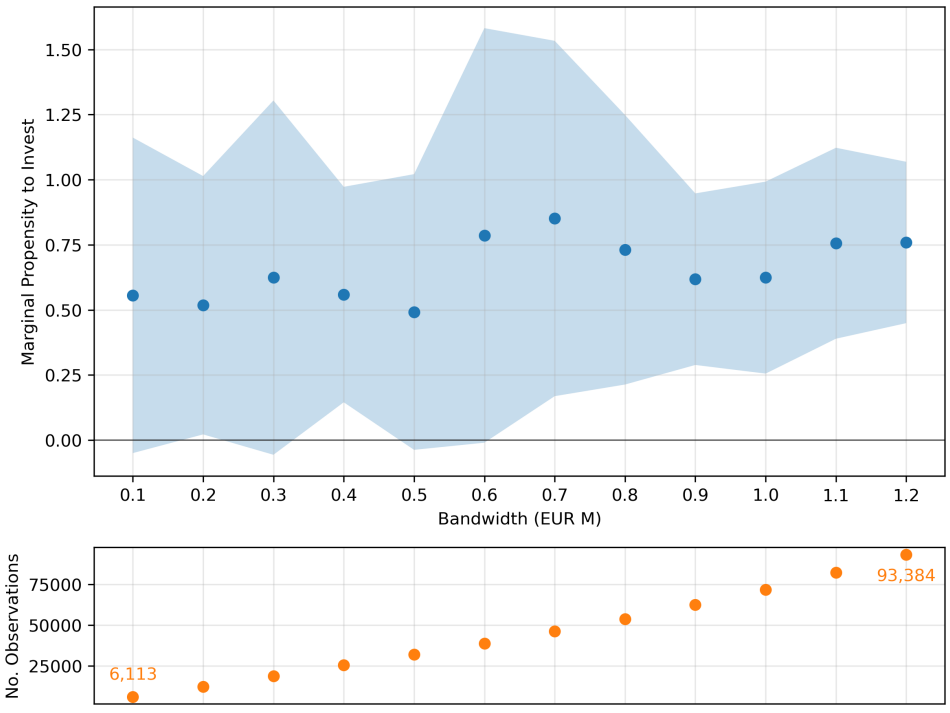


Figure C.14: Sensitivity of the MPI to bandwidth choice

Notes: This figure reports the estimated marginal propensity to invest (MPI) across different bandwidths around the eligibility cutoff. The dots represent point estimates, while the shaded area indicates 95% confidence intervals. The lower panel reports the number of observations used in each estimation.

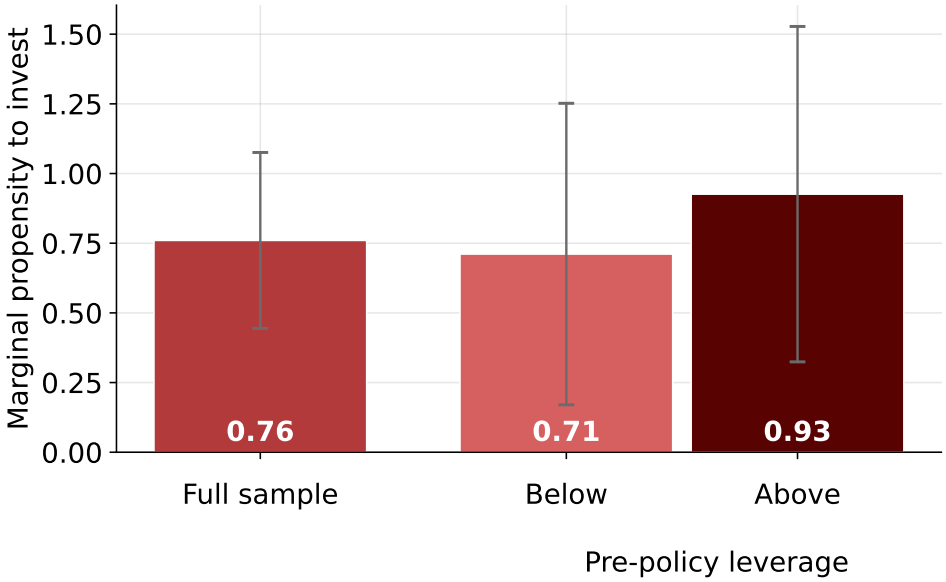


Figure C.15: Heterogeneity in MPI

Notes: This figure reports the estimated marginal propensity to invest (MPI) for the full sample and across subsamples defined by pre-policy leverage. Firms are classified as above or below average based on their respective pre-policy values. The bars represent point estimates, while the vertical lines indicate 95% confidence intervals.

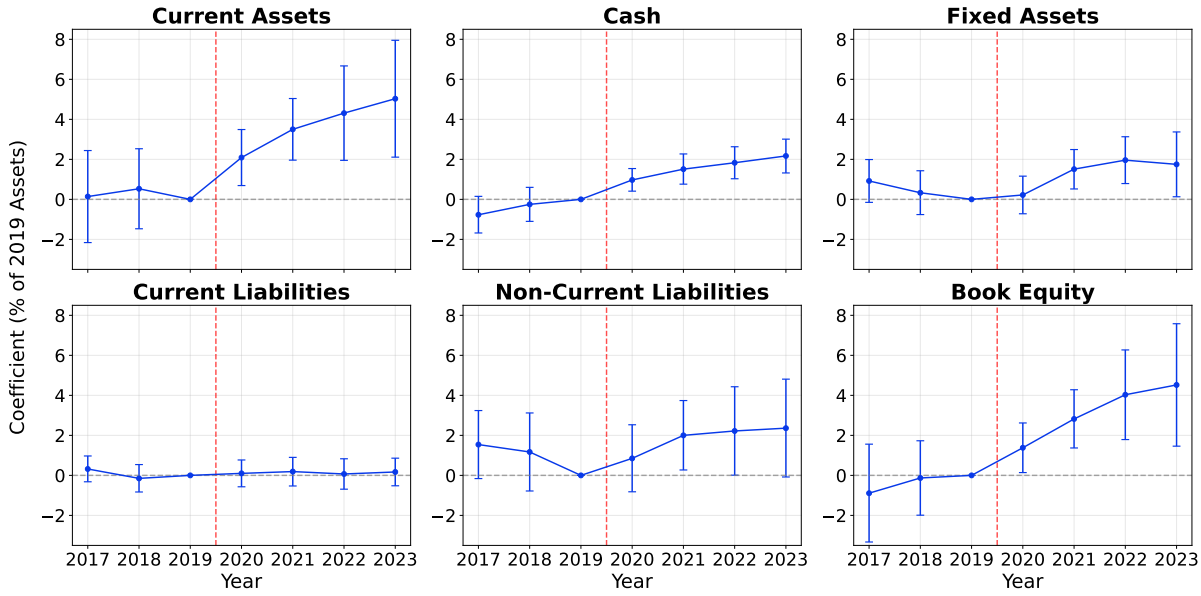


Figure C.16: Effect of Moratorium on Balance Sheet Variables

Notes: The plots show the coefficient resulting from the dynamic difference-in-differences

$$\frac{y_{i,t}}{at_{i,2019}} = \iota_i + \sigma_{st} + \sum_{h \neq 2019} \gamma_h \mathbf{1}(Elig_i) \times \mathbf{1}(t = h) + \eta_{it}$$

where i indexes firms, t indexes time, s indexes sector. The dependent variable is scaled by firms' 2019 total assets, $at_{i,2019}$, and winsorized at the 1st and 99th percentiles. $Elig_i$ is an indicator equal to one for firms with 2019 assets below EUR 2 million. Firm and sector-by-year fixed effects are included. Dots report the estimated coefficients, γ_h, and bars denote 95% confidence intervals. The analysis uses the AMDC-Orbis dataset.

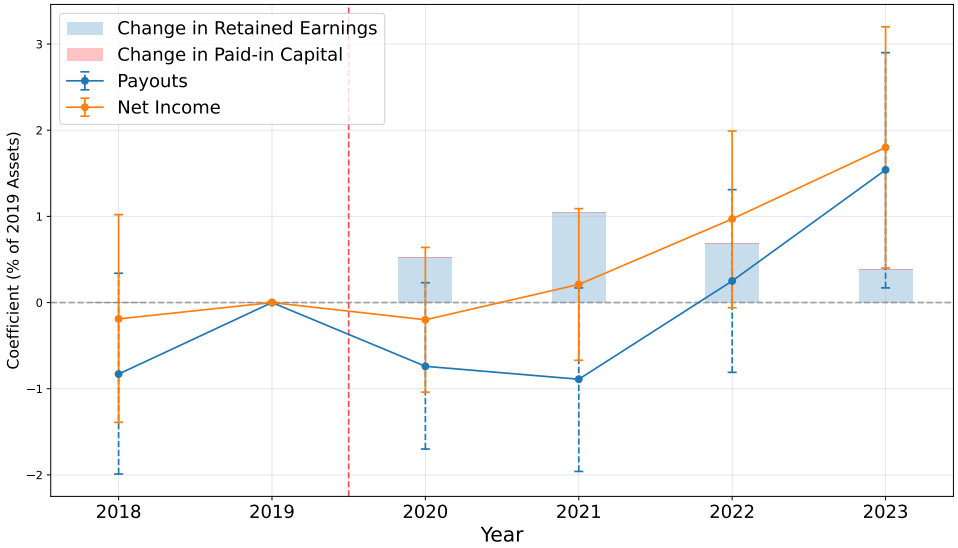


Figure C.17: Effect of Moratorium on Net income and Dividends

Notes: The plot shows the coefficient resulting from the dynamic difference-in-differences

$$\frac{y_{i,t}}{at_{i,2019}} = \iota_i + \sigma_{st} + \sum_{h \neq 2019} \gamma_h \mathbf{1}(Elig_i) \times \mathbf{1}(t = h) + \eta_{it}$$

where i indexes firms, t indexes time, s indexes sector. The dependent variable is scaled by firms' 2019 total assets, $at_{i,2019}$, and winsorized at the 1st and 99th percentiles. $Elig_i$ is an indicator equal to one for firms with 2019 assets below EUR 2 million. Firm and sector-by-year fixed effects are included. The blue and red bars report the estimated coefficients, γ_h, for year-on-year changes in retained earnings and paid-in capital, respectively. The blue and orange series report the corresponding estimates for payouts and net income, respectively. Error bars denote 95% confidence intervals. The analysis uses the matched AMDC-Orbis dataset.

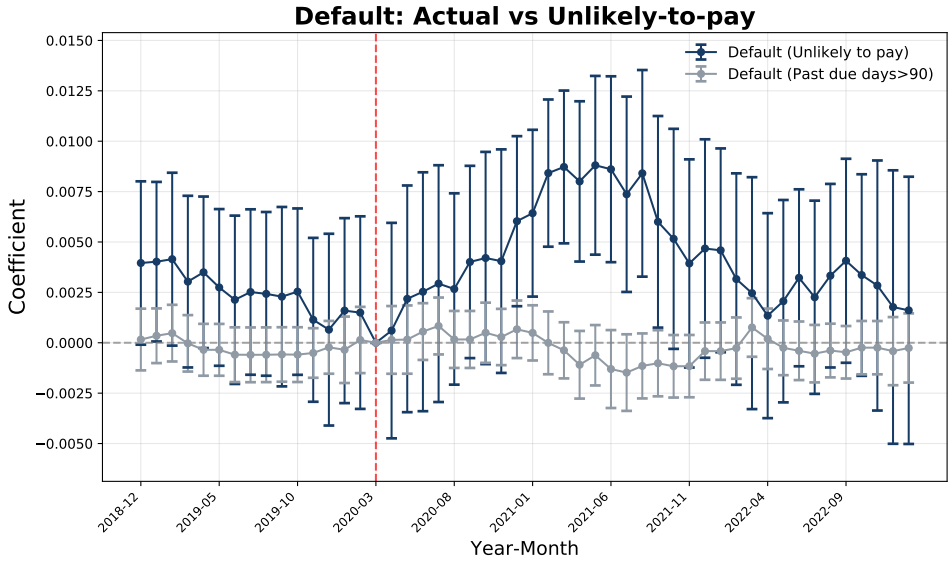


Figure C.18: Effect of Moratorium on Actual Defaults and Unlikely-to-Pay

Notes: The plot shows the coefficient resulting from the dynamic difference-in-differences

$$y_{i,m} = \iota_i + \sigma_{sm} + \sum_{h \neq 2020M3} \gamma_h \mathbf{1}(Elig_i) \times \mathbf{1}(m = h) + \eta_{im}$$

where i indexes firms, m indexes time in months, s indexes sector. The dependent variables are an indicator equal to one if the firm has at least one loan with more than 90 days past due (light blue) and an indicator equal to one if the firm has at least one loan classified by its bank as unlikely to pay (red). $Elig_i$ is an indicator equal to one for firms with 2019 assets below EUR 2 million. Firm and sector-by-month fixed effects are included. Dots report the estimated coefficients, γ_h, and error bars denote 95% confidence intervals. The omitted month is March 2020. The analysis uses the matched AnaCredit-Orbis dataset.

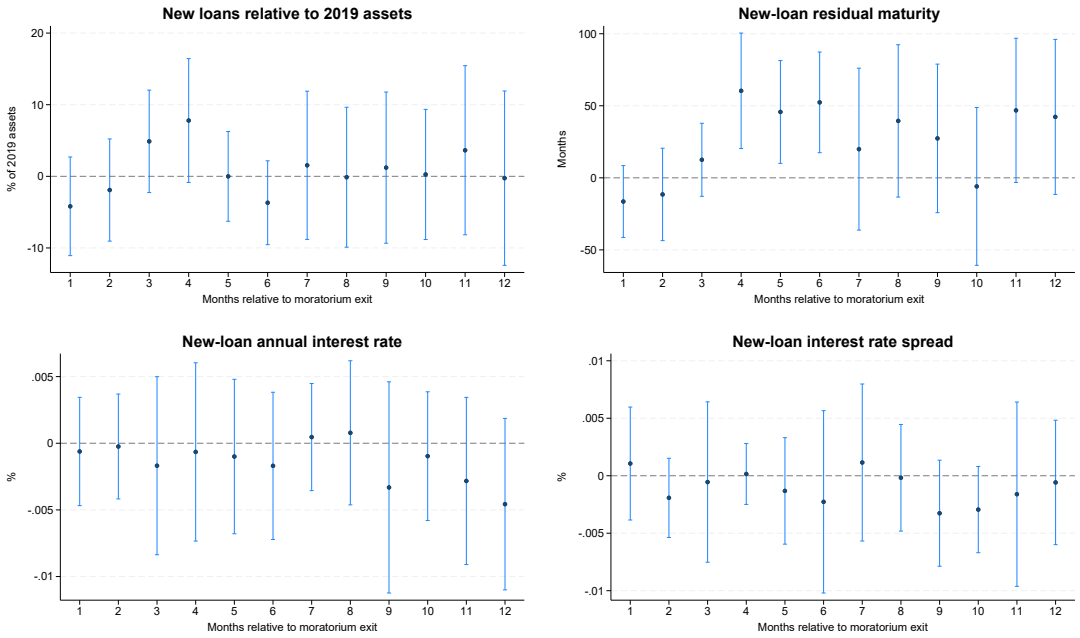


Figure C.19: Access to new financing after moratorium exit

Notes: The panels report estimated changes in new lending relative to 2019 assets (top left), residual maturity in months (top right), the annualized interest rate (bottom left), and the interest rate spread (bottom right) following moratorium exit. Estimates are obtained from the event-study specification

$$y_{i,t} = \sigma_{s(i)} + \tau_t + \sum_{h=1}^{12} \beta_h \mathbf{1}\{t - T_i = h\} + \eta_{i,t},$$

where T_i denotes the exit month for instrument i, $\sigma_{s(i)}$ denotes NACE sector fixed effects, and τ_t denotes calendar-month fixed effects. The sample covers the exit month and the following twelve months. The exit month ($h = 0$) is the omitted reference period. Points show coefficient estimates, and vertical bars indicate 95% confidence intervals based on standard errors clustered at the NACE sector level. The analysis uses the AnaCredit–Orbis dataset and restricts the sample to firms that participated in the moratorium.

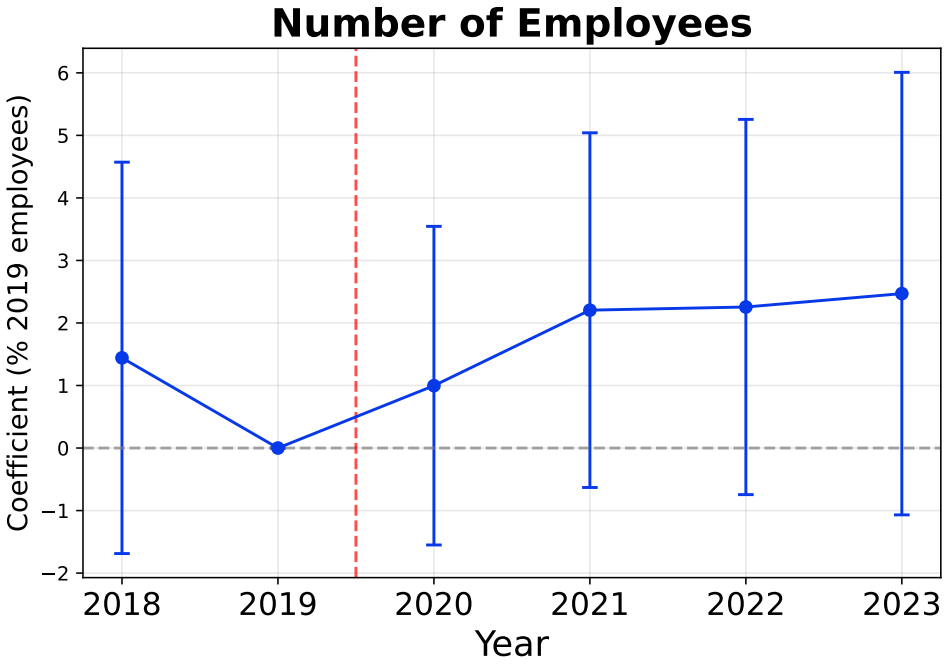


Figure C.20: Effect of Moratorium on Employment

Notes: The plots show the coefficient resulting from the dynamic difference-in-differences

$$\frac{employees_{i,t}}{employees_{i,2019}} = \iota_i + \sigma_{st} + \sum_{h \neq 2019} \gamma_h \mathbf{1}(Elig_i) \times \mathbf{1}(t = h) + \eta_{it}$$

where i indexes firms, t indexes time, s indexes sector. The dependent variable is the number of employees as % of 2019 employees. $Elig_i$ is an indicator equal to one for firms with 2019 assets below EUR 2 million. Firm and sector-by-year fixed effects are included. Dots report the estimated coefficients, γ_h, and bars denote 95% confidence intervals. The analysis uses the AMDC-Orbis dataset.

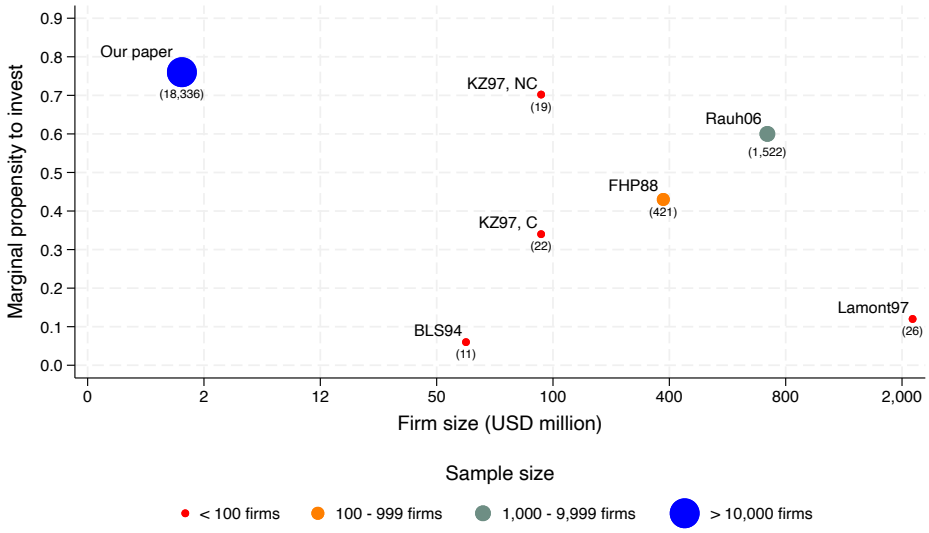
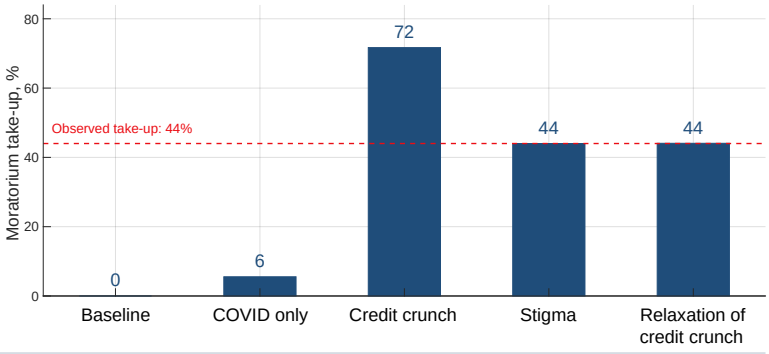


Figure C.21: Past Marginal Propensity to Invest estimates by Firm Size

Notes: This figure plots the marginal propensity to invest against median firm size across previous studies and our sample. The coefficients and details on the underlying specifications are reported in Table D.4. Dot colors indicate sample size: red for fewer than 100 firms, orange for 100–999 firms, teal for 1,000–9,999 firms, and blue for 10,000 or more firms. Numbers in parentheses below each dot report the exact number of unique firms in the corresponding study.



	Baseline	COVID only	Credit crunch	Stigma	Relaxation of credit crunch
Shock, s=0.542		✓	✓	✓	✓
Credit crunch, ζ			0.1166	0.1166	0.0286
Stigma, % of firm value				0.02%	0%

Figure C.22: Take-up under different Modeling Assumptions

Notes: This figure shows moratorium take-up under alternative modeling assumptions. "Baseline" includes neither the COVID shock nor a credit crunch. "COVID only" introduces the COVID shock, $s = 0.542$. "Observed credit crunch" additionally tightens the new-borrowing constraint by $\zeta = 0.12$, calibrated to the decline in lending to the control group between 2019 and 2020. "Relaxation of credit crunch" raises ζ to approximately 0.03 without stigma, whereas "Stigma" retains $\zeta = 0.12$ and introduces a participation cost of approximately 0.02% of firm value. The red dashed line indicates observed take-up, and the table summarizes the assumptions in each scenario.

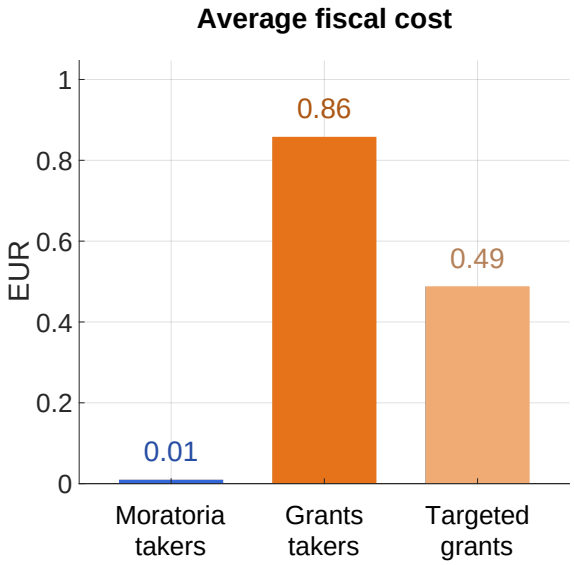


Figure C.23: Average Fiscal Cost of Policy Counterfactuals

Notes: The plot shows average fiscal cost across policy counterfactuals. Targeted grants assigns grants to baseline moratorium takers.

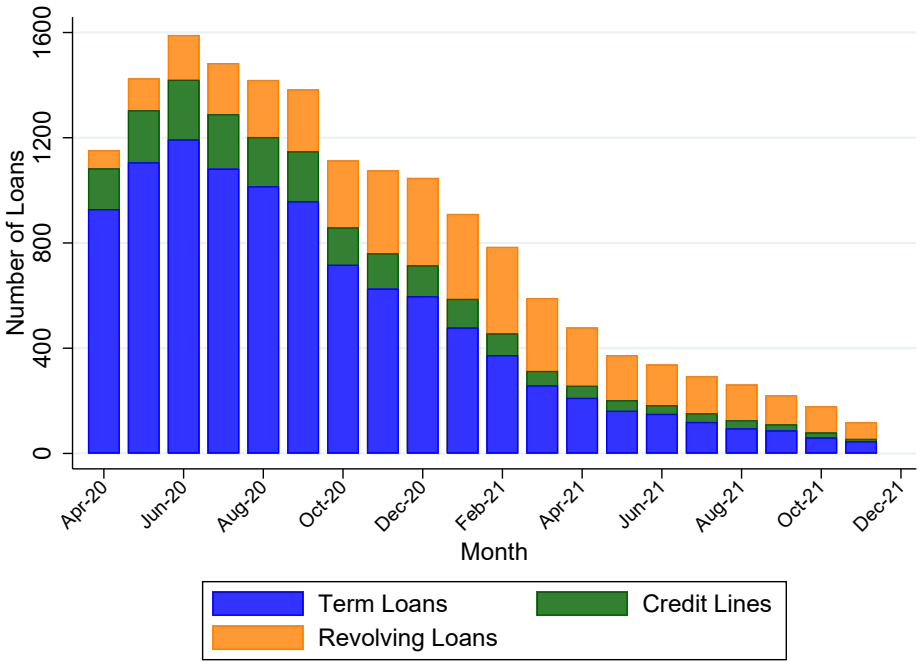


Figure C.24: Dynamics of Moratorium Take-up: Stock of Loans

Notes: This figure shows the amount of term loans (blue), credit line (green), and revolving loans (orange) in moratoria over the period April 2020-November 2021.The analysis uses the AnaCredit-Orbis dataset.

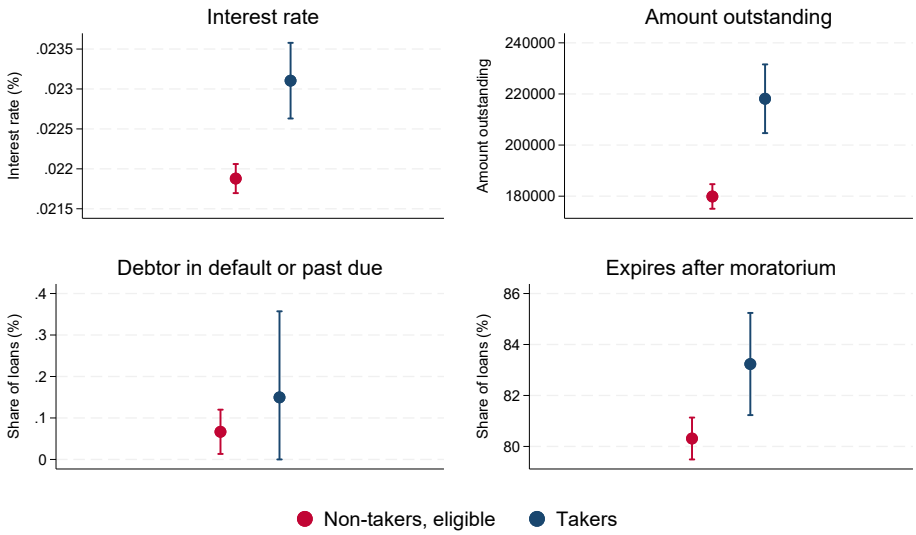


Figure C.25: Pre-policy loan characteristics of takers vs non-takers

Notes: The plots show the average and 95% confidence intervals of selected loans' characteristics as of December 2019 for eligible non-takers (red), and takers (blue). The selected variables are: interest rates, amount outstanding of loans, a flag for whether the debtor is in default, and a flag for whether the loan expires after November 2021. The analysis uses the AnaCredit-Orbis dataset.

D Additional Tables

Name	Variable	N	Mean	Std. Dev.	P25	P75
AMDC-ORBIS						
Panel A: Assets (%)						
Fixed Assets	fias	119,378	40.33	35.90	9.56	65.56
Current Assets	cuas	119,378	69.04	44.63	36.02	93.64
Cash	cash	116,004	20.81	24.55	2.00	31.39
Net Current Assets	ncas	119,376	65.08	44.90	30.46	91.54
Panel B: Equity (%)						
Shareholders' Funds	shfd	119,378	50.84	34.06	23.61	72.78
Other Shareholders' Funds	osfd	119,355	46.13	34.37	18.59	68.14
Capital	capi	119,355	4.59	8.98	1.44	3.67
Panel C: Liabilities (%)						
Total Financial Debt	tdbt	119,378	44.82	38.22	14.80	66.36
Long-Term Debt	ltdb	119,372	44.73	38.15	14.74	66.29
Change in Long-Term Debt	delta ltdb	93,386	2.13	23.05	-5.96	7.06
Non-Current Liabilities	ncli	119,378	54.74	38.66	25.92	75.96
Current Liabilities	culi	119,378	3.80	11.85	0.00	0.00
Panel D: Capital Expenditures (%)						
Capital Expenditures	capex	93,386	6.60	13.56	0.40	7.48
Panel E: Income Statement (%)						
Sales	sales	84,101	0.03	0.04	-0.09	0.18
Net Income	netinc	93,386	12.27	20.53	0.85	16.81
Earnings Before Interest and Taxes	ebit	93,386	13.24	26.11	0.36	21.80
Operating Revenues	orevn	93,386	167.99	154.27	58.64	228.88
Cost of Goods Sold	cogs	93,386	107.52	123.86	25.07	143.22
Personnel Cost	pers_cost	93,386	39.41	38.14	9.38	57.64
Depreciation	aabschr	93,386	4.50	4.66	1.47	5.74
Dividends	div	93,386	5.50	12.51	0.00	0.95
Panel F: Employment						
Number of Employees	employees	99,265	13.52	11.15	5.00	19.00
Number of Employees (Full-Time Equivalent)	vze	88,954	12.10	10.02	4.00	17.00
Panel G: Pre-policy characteristics (EUR m in 2019)						
Total Assets	toas_2019	18,336	1.62	0.65	1.06	2.07
Operating Revenues	orevn_2019	18,336	2.33	2.20	0.76	3.18
Cost of Goods Sold	cogs_2019	18,336	1.51	1.80	0.33	1.96
ANACREDIT-ORBIS						
Panel H: Loans and Credit Lines						
Amount Outstanding (EUR m)	ona	719,527	0.96	1.44	0.23	1.19
Amount Outstanding (%)	ona	719,527	54.98	57.86	15.59	74.39
Newly Originated (EUR m)	ona	18,835	0.29	0.54	0.05	0.32
Newly Originated (%)	ona	18,835	16.62	22.95	3.08	19.87
Long-Term Loans (EUR m)	ona	667,880	0.87	1.28	0.21	1.10
Long-Term Loans (%)	ona	667,880	50.33	52.03	14.44	68.85
Default: Unlikely to Pay	dflt_stts_dbtr=19	719,527	0.01	0.10	0.00	0.00
Default: Past Due Days > 90	dflt_stts_dbtr=20	719,527	0.00	0.03	0.00	0.00
Annual Rate	annlsd_agrd_rt_w_ona_cv	719,404	0.02	0.01	0.02	0.03
Interest Rate Spread	intrst_rt_sprd	601,630	0.02	0.01	0.01	0.02
Original Maturity, Years	orgnl_mtrty/360	719,374	13.82	15.56	7.44	19.50

Table D.1: Summary Statistics

Notes: The table reports summary statistics from AMDC-Orbis at the firm level, and from AnaCredit-Orbis at the loan level. All variables in (%) are normalized by 2019 total assets and winsorised at the 1–99 level.

Assets		Liabilities	
	Coef		**Coef**
Current assets	3.500^{***}	**Current liabilities**	0.190
Inventories	0.230	Loans	–
Trade receivables	-0.010	Trade payables	–
Other current assets	1.520^{***}	Other current liabilities	–
Cash	1.510^{***}	**Non-current liabilities**	2.000^{**}
		Long-term debt	1.680^{**}
Fixed assets	1.510^{***}	Other non-current liabilities	0.330^{**}
Tangible fixed assets	1.330^{***}	**Book equity**	2.820^{***}
Intangible fixed assets	-0.020	Paid-in capital	0.07
Other fixed assets	0.200^{**}	Other shareholders' funds	2.750^{***}

Table D.2: DiD results for 2021 balance sheet

Notes: The table presents the 2021 difference-in-differences estimates in balance sheet format. Each variable is in level, scaled by 2019 total assets, and winsorized at the 1st and 99th percentiles. The Coef column reports the estimated coefficients. The analysis uses the AMDC-Orbis dataset. Results from loans, trade payables, and other current liabilities are not reported since in Orbis they are poorly populated.

Year	Eligible firms N_E	Ineligible firms N_I	Coefficient (%) ($p_E - p_I$, p.p.)	P-value
2019	17,617	7,065	-0.01	0.574
			(0.00)	
2020	17,360	6,945	0.10	0.113
			(0.06)	
2021	17,553	7,061	-0.00	0.969
			(0.10)	
2022	17,375	7,044	0.11	0.343
			(0.12)	

Table D.3: Mean Difference Test for Exits

Notes: The table shows mean difference test for exits between eligible and ineligible firms.

Paper	Equation / shock	Estimate	Crisis	Estimation details	Data	Asset size ($ m)	Listed	Table
Fazzari, Hubbard, and Petersen (1988)	$\frac{I_{it}}{K_{i,t-1}} = \alpha_i + \alpha_t + \beta_1 Q_{it} + \beta_2 \frac{CF_{it}}{K_{i,t-1}} + \varepsilon_{it}$	0.35	No	Investment–cash flow regression with firm and year effects. Firms are classified according to dividend payout, with low-payout firms interpreted as relatively financially constrained.	U.S. manufacturing firms, 1970–1984. Low-dividend-payout subsample.	384.4	Yes	Table 4
Hoshi, Kashyap, and Scharfstein (1991)	FIX	0.464	No	Regresses cumulative investment from $t+1$ to $t+3$ on industrial-group membership and measures of firms' financial relationships.	125 financially distressed Japanese manufacturing firms: 45 group firms and 80 non-group firms.	–	Yes	Table 4
Blanchard, López-de-Silanes, and Shleifer (1994)	$MPI = \frac{\Delta Inv}{NPVA}$	0.06	No	Cash windfalls from won or settled lawsuits selected to be unrelated to marginal Q. ΔInv is average gross investment in t and $t+1$ minus average gross investment in $t-2$ and $t-1$. The MPI is the median $\Delta Inv/NPVA$. The median MPI is 0.00 for troubled firms and 0.12 for healthier firms.	11 U.S. firms receiving substantial lawsuit cash windfalls. Cases are selected so that the litigation does not change the firm's investment opportunity set.	62.6	Yes	Table 5
Lamont (1997)	1986 oil-price collapse $\rightarrow \Delta(I/S)_{\text{nonoil}}$	0.12	Yes	Uses the 1986 oil-price collapse as a shock to the cash flow and collateral of oil companies and examines investment by their non-oil segments. Changes in non-oil investment are industry-adjusted using matched Compustat segments.	26 oil-dependent U.S. firms with 40 nonoil segments; 39 segments in the industry-adjusted sample. Main comparison: 1985–1986.	2,109 (mean)	Yes	Tables 4–5
Kaplan and Zingales (1997), constrained	$\frac{I_{it}}{K_{i,t-1}} = \alpha_i + \alpha_t + \beta_1 Q_{i,t-1} + \beta_2 \frac{CF_{it}}{K_{i,t-1}} + \varepsilon_{it}$	0.340	No	Firm and year effects. Financing-constraint status is classified using annual reports, 10-Ks, financial statements, and quantitative indicators of financial condition. Estimate shown for firms classified as likely constrained.	49 low-dividend firms from FHP, 1970–1984. Constrained classification based on direct examination of firms' financial condition.	94.9	Yes	Table 5
Kaplan and Zingales (1997), unconstrained	same as above	0.702	No	Same specification and classification procedure. Estimate shown for firms classified as never financially constrained.	Same underlying sample of 49 low-dividend FHP firms, 1970–1984; subsample classified as never constrained.	94.9	Yes	Table 5
Rauh (2006)	$\frac{I_{it}}{A_{i,t-1}} = \alpha_i + \alpha_t + \beta_1 Q_{i,t-1} + \beta_2 \frac{CF_{it}}{A_{i,t-1}} + \beta_3 \frac{MC_{it}}{A_{i,t-1}} + X_{it}\gamma + \varepsilon_{it}$	~ 0.6–0.83	No	Uses mandatory defined-benefit pension contributions (MC) as a plausibly exogenous drain on internal funds. Firm and year effects; controls for Q, nonpension cash flow, and pension funding status.	U.S. Compustat firms matched to IRS Form 5500 defined-benefit pension filings, 1990–1998; 8,030 firm-year observations.	737	Yes	Table 2
Chodorow-Reich, Isen, Sunderam, and Zwick (2026)	$Y_{it} = \tau R_i \widehat{\times Post}_t + \text{FE}_{i,t,g\times Post}$, $R_i \times Post_t \leftarrow W_i \times Post_t$	$\simeq 0$	Yes	Uses random lotteries for Minnesota COVID relief grants. Two-stage least squares instruments the grant amount with lottery winner status, allowing for ineligible winners. Specifications include firm fixed effects, time fixed effects, and demographic-group-by-post effects.	Applicants to two Minnesota COVID relief programs.	<= $10M	No	–

Table D.4: Past Marginal Propensity to Invest Estimates

Notes: Estimates measure the response of investment to changes in internal financial resources across studies. "Crisis" indicates whether the empirical design exploits a major adverse aggregate or sectoral shock. Asset size is reported in millions of U.S. dollars and refers to the median unless otherwise indicated. For FHP, size is approximated by the firm-count-weighted average of the median capital stock across the four dividend-payout classes. For KZ, the reported size is the median capital stock of the underlying FHP Class 1 sample rather than the separate constrained and unconstrained subsamples.

E Placebo Test

To validate our identification strategy, we conduct a placebo test using only firms ineligible for the policy. We restrict our analysis to firms with 2019 assets between EUR 2M and EUR 3.2M (eg. ineligible firms) and implement a set of artificial cutoffs (2.2M, 2.4M, 2.6M, 2.8M, and 3M).

This design directly tests whether our estimation strategy generates spurious treatment effects in the absence of any policy intervention. Since no firms in either group accessed the program, significant "treatment effects" at the placebo cutoffs would indicate that our empirical approach incorrectly attributes pre-existing discontinuous differences between firm size categories to the policy intervention. The placebo test validates the identifying assumption that unobserved determinants of firm outcomes vary smoothly around asset-based cutoffs. Our main ITT estimate relies on the assumption that firms just below and above the EUR 2M eligibility threshold are comparable in all respects except policy access. Finding no discontinuous changes at the arbitrary placebo cutoffs within the ineligible population supports the validity of our cutoff-based identification strategy. Additionally, this design controls for potential confounding factors that vary with firm size but are unrelated to the policy, such as scale effects or differential access to financing. Such factors would generate similar discontinuities at both the real (EUR 2M) and placebo cutoffs.

We estimate placebo effects using identical specifications to our main analysis, replacing the true eligibility indicator with the placebo treatment indicator. Under the null hypothesis of no spurious effects, we expect to find economically and statistically insignificant coefficients across all outcome variables.

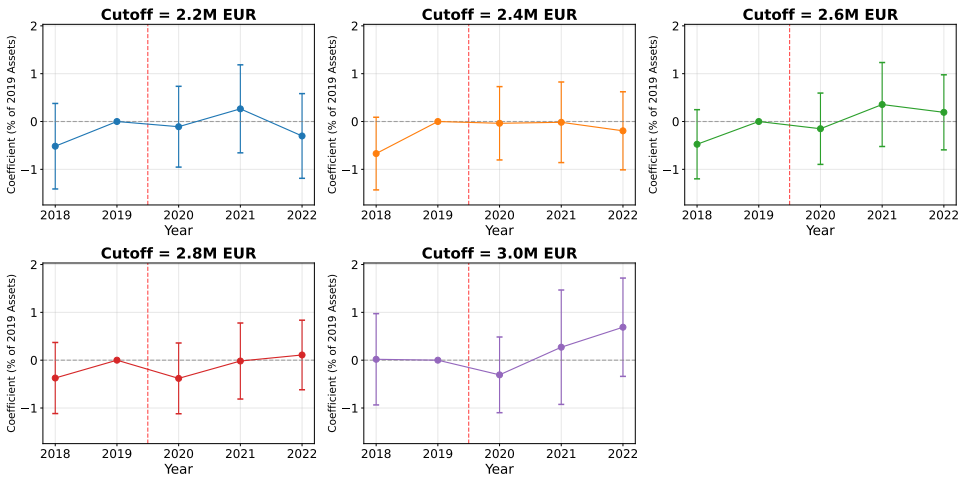


Figure E.1: Placebo Effect of Moratorium on Capex

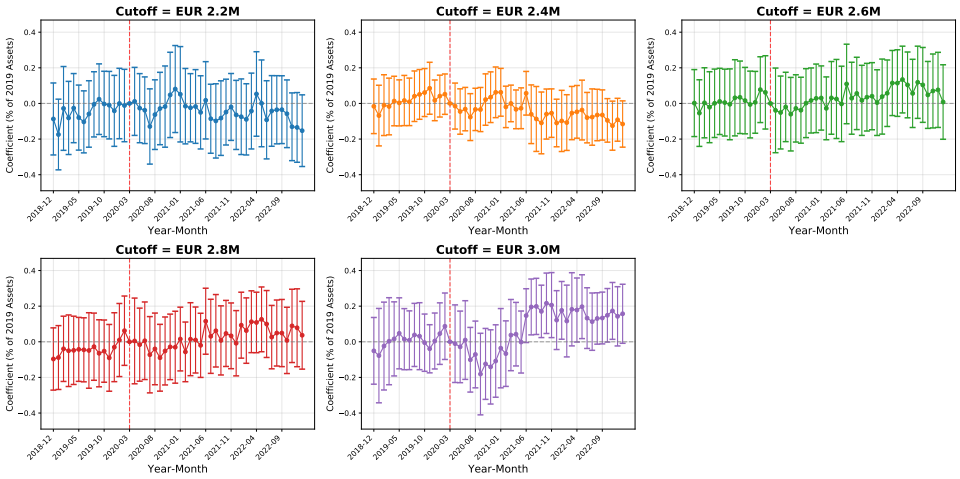


Figure E.2: Placebo Effect of Moratorium on Short-term Loans

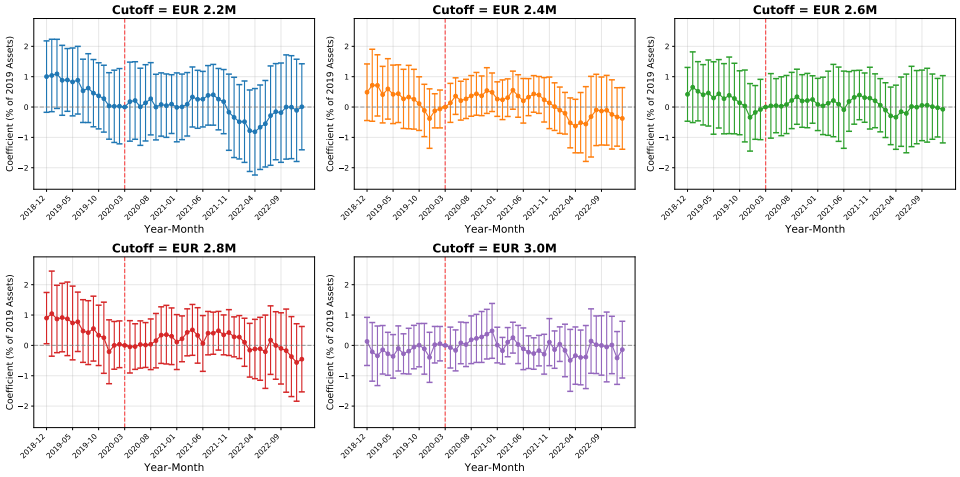


Figure E.3: Placebo Effect of Moratorium on Long-term Loans

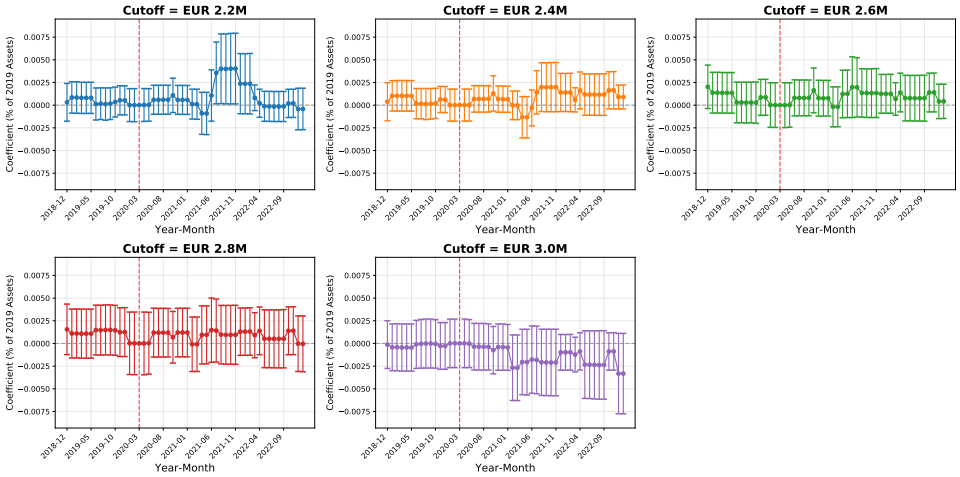


Figure E.4: Placebo Effect of Actual Defaults

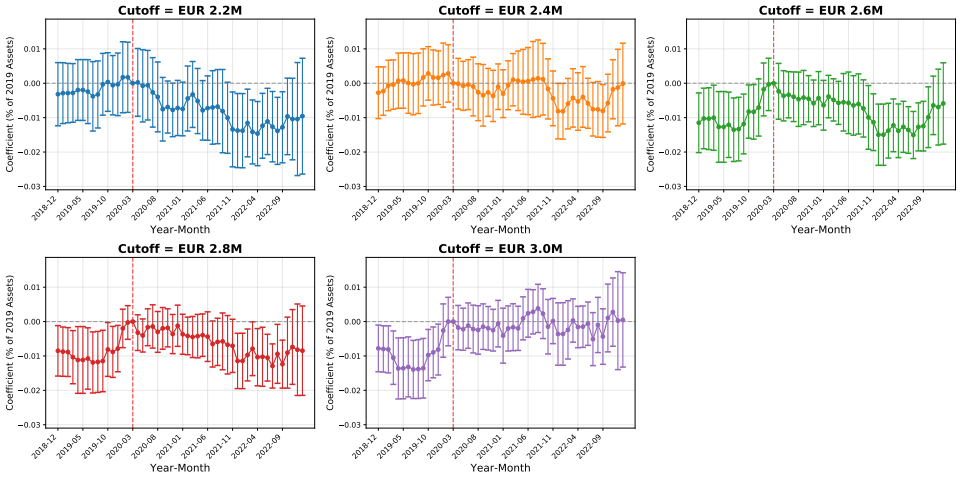


Figure E.5: Placebo Effect of Unlikely-to-Pay

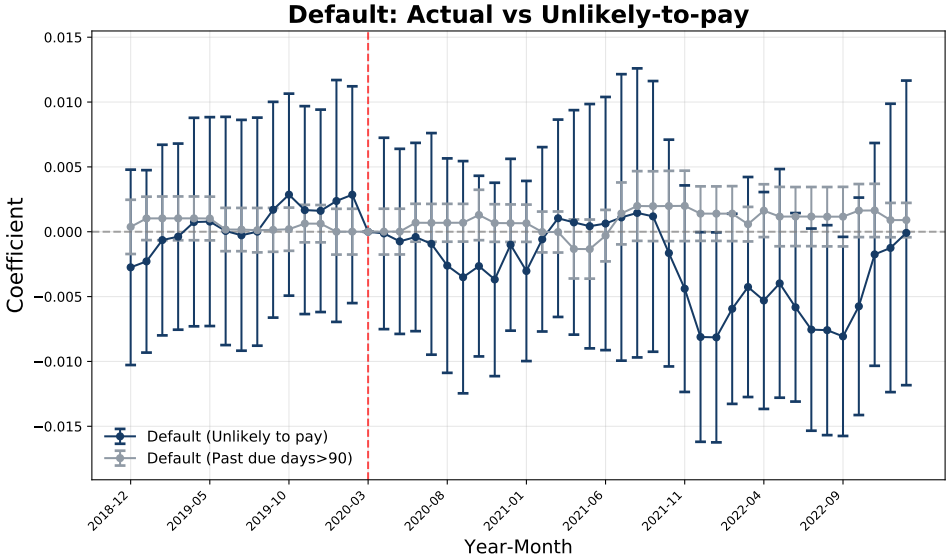


Figure E.6: Placebo Effect of Unlikely-to-Pay and Actual Defaults, cutoff of EUR 2.4 M

F Numerical resolution

F.1 Value function iteration

We solve the model using discrete dynamic programming. The firm's state is $x_t = (z_t, k_t, b_t)$, where z_t denotes productivity, k_t capital, and $b_t = B_t/k_t$ the debt-to-capital ratio. Negative values of b_t represent cash holdings. The controls are next-period capital and newly chosen leverage, (k_{t+1}, b_{t+1}). Borrowing carries the exogenous interest rate r, while cash earns $r - \xi$. We do not solve for state-dependent debt prices or default risk premia.

Productivity follows the mean-zero AR(1) process

$$z_{t+1} = \rho z_t + \varepsilon_{t+1}, \qquad \varepsilon_{t+1} \sim N(0, \sigma_\varepsilon^2).$$

We approximate this process using Tauchen's method with 30 grid points spanning

$$[-4\sigma_z,\, 4\sigma_z], \qquad \sigma_z = \frac{\sigma_\varepsilon}{\sqrt{1-\rho^2}}.$$

The resulting transition matrix, $\Pi(z'|z)$, is used to evaluate conditional expectations in the Bellman equations.

The capital grid contains 250 logarithmically spaced points. Its lower bound is one half of the minimum frictionless capital benchmark across productivity states. Its upper bound is the maximum of this benchmark multiplied by $\exp(5h)$, where $h = -\log(1-\delta)/2$. These buffers permit choices below and above the range of frictionless capital benchmarks.

The debt-to-capital grid contains 100 uniformly spaced points between -0.1 and $\gamma = 0.393$. The capital and leverage choice grids coincide with their respective state grids. Choices violating the non-negative-distribution constraint are assigned a value of $-\infty$.

The stationary value function satisfies

$$V(z,k,b) = \max_{k',b'} \left\{ CF(z,k,b;k',b') + \frac{1}{1+r} \sum_{z'} \Pi(z'|z) V(z',k',b') \right\},$$

subject to the financing and non-negative-distribution constraints. Here, CF denotes the cash flow distributed to shareholders. Since stationary choices lie on the state grids, continuation values are evaluated directly at grid points without interpolation.

We initialize the value function at one at every state and use modified policy iteration. Each policy-improvement step searches over all feasible capital and leverage choices. It

is followed by 100 Howard policy-evaluation steps, during which the policy is held fixed. Iteration stops when

$$\max_x \left| \frac{V_{n+1}(x)}{V_n(x)} - 1 \right| < 10^{-6}.$$

The implementation also imposes a safety limit of 3,000 outer iterations.

The baseline results are computed in double precision using CPU-parallel evaluation across next-period capital choices. For each capital choice, the algorithm maximizes over leverage and retains the corresponding best value and leverage index. It then maximizes across capital choices. This avoids storing the full joint state-by-action array.

F.2 Temporary shock and policy problems

After solving the stationary model, we solve the period-1 problems for the no-policy, grant, and moratorium scenarios. The unexpected COVID shock scales current operating surplus by $1 - s$, with $s = 0.542$ in the baseline. The shock and the restriction on new borrowing are temporary: from period 2 onward, firms again face the stationary environment, but enter it with policy-dependent capital and debt.

For the no-policy and grant scenarios, the period-1 Bellman problem uses $V(z_2, k_2, b_2)$ as its continuation value. During the shock period, newly chosen leverage is restricted to $b_2 \leq \gamma^S$, where $\gamma^S = 0.209$. Numerically, this restriction is imposed by excluding leverage grid points above the ceiling.

The grant provides a non-repayable transfer equal to

$$G_i = \mu(1 + r) \max\{B_{i1}, 0\},$$

where $\mu = 0.381$ in the baseline. It increases current liquidity without adding to next-period debt. In the implemented cash-flow equations, the transfer has no separate tax effect, and the interest deduction is calculated on the full scheduled interest.

The moratorium provides the same immediate liquidity relief but capitalizes the postponed payment into the firm's next-period debt. In levels, the debt carried into period 2 is

$$B_{i2}^M = b_{i2} k_{i2} + \mu(1 + r) \max\{B_{i1}, 0\}.$$

Consequently, the leverage state entering the stationary continuation value is

$$b_{i2}^M = b_{i2} + \mu(1 + r) \max\{b_{i1}, 0\} \frac{k_{i1}}{k_{i2}}.$$

The conditional moratorium problem is therefore

$$V_1^M(z_1, k_1, b_1) = \max_{k_2, b_2} \left\{ CF_1^M(z_1, k_1, b_1; k_2, b_2) + \frac{1}{1+r} \sum_{z_2} \Pi(z_2|z_1) V(z_2, k_2, b_2^M) \right\},$$

subject to

$$CF_1^M \geq 0, \qquad b_2 \leq \gamma^S, \qquad b_2^M \leq \gamma.$$

At period 1, the firm pays only the fraction $1-\mu$ of its scheduled positive-debt service, while retaining the deduction for the full scheduled interest. The deferred balance becomes part of ordinary debt at period 2 and is subsequently treated according to the stationary cash-flow equations.

There is no additional deferred-repayment state, repayment grid, or separate period-2 Bellman problem. Since b_2^M generally lies between leverage grid points, the expected stationary continuation value is evaluated using linear interpolation along the debt dimension.

If a period-1 policy problem has no feasible choice, its value is set to zero and an exit indicator is recorded. Policy participation is then determined by comparing feasible options. An eligible firm takes the moratorium when

$$(1-\kappa)V_1^M > V_1^0,$$

where V_1^0 denotes the value without policy support. Eligibility requires positive debt upon entering period 1. The proportional stigma cost affects participation but not the optimal choices conditional on participation. We calibrate κ by bisection to match a take-up rate of 44% among eligible firms. Grants have no stigma cost and use their own participation decisions.

F.3 Simulation and impulse responses

We simulate 100,000 firms following a 500-period stationary burn-in. Annual impulse responses are reported for $t = 0, \ldots, 5$, with the shock and policy intervention occurring at $t = 1$. The moratorium affects the debt state entering $t = 2$; all subsequent choices follow the stationary policy functions. For the output-based benefit-cost analysis, we extend the same simulation to $t = 20$.

Productivity is simulated using Gaussian innovations to the AR(1) process and is bounded by the productivity grid. Continuous policy and value functions are evaluated at simulated states using multilinear interpolation. Next-period capital and leverage states are bounded by their numerical grid limits.

Exit indicators are evaluated using nearest-neighbor interpolation. Moratorium take-up, however, is determined directly from the comparison of interpolated policy and no-policy values at each firm's period-1 state. The implementation requires the net value gain to exceed a numerical tolerance of $10^{-5}|V_1^0|$.

Firms that exit at $t = 1$ produce zero thereafter. The simulation does not introduce replacement entry or a separate subsequent exit hazard. All counterfactual paths use the same initial cross-section and productivity innovations. These common random numbers isolate policy effects from differences in simulation draws.

G Data Cleaning

We operate the following cleaning to Orbis:

- Drop firms if assets/liabilities/turnover is negative in any year
- Drop firms if operating revenue/shareholders' capital/total assets/fixed assets is zero in any year
- Drop firms if employment is either zero, negative or greater than 2Mln in any year
- Keep only firms that have non-missing total assets in at least one year
- Keep only firms that have non-missing total liabilities
- Drop firms for which the sum of current liabilities, non-current liabilities, and equity is smaller than 80% or greater than 120% of total assets
- Drop firms with either total liabilities, current liabilities or long-term debt greater than total assets

We perform the following cleanings to Anacredit in line with Kosekova et al. 2025:

- Exclude all firms with NACE codes 64-66
- Omit firms with syndicated loans in December 2019
- Exclude loans associated with multiple creditors
- Restrict to loans granted in EUR
- Exclude values of the outstanding amounts below or equal to 0 and above or equal to the 99.9% level in the empirical distribution
- Winsorize outstanding and commitment amounts at the 0.01% level -

We construct the joint dataset of Orbis and LSE, keeping only the matched observations. The merge is based on the AMDC internal identifier and end of fiscal year. We then merge-in the two datasets UDEMO (demographics) and INS (bankruptcy filings). From UDEMO, we keep year of entry (NGJ) and year of exit (SLJ), while from INS we keep the year of bankruptcy filing (ins_flag).

In order to ascertain the correctness of LSE variables, we perform the following checks and cleanings:

- Reconstruct total revenues and total expenses and check whether they matched with AMDC variables. Specifically, total revenues should equal EZUS until 2020, and to EGESAMNT from 2021 onward, while total expenses should match AGES until 2020, and to AGESAMNT from 2021 onward. We drop the firms with inconsistencies (less than 1% of the sample)
- Reconstruct total investment by summing up the subcategories. It should match with IGES in AMDC, here "flag_err_invest" marks observations for which the computed investment deviates by more than 5% (in absolute value) from IGES.
- Keep only observations for which 2019 total assets from Orbis are not missing.
- Drop firms with more than 50 employees in 2019 (36 firms dropped). Since our sample restricts to firms with less than 3.2 million of assets in 2019, it seems unlikely to have firms with number of employees typical of Medium enterprises according to EU definition.

Then we operate the following cleanings:

- If the "flag_err_invest" is 1, we set IGES equal to missing.
- Replace with missing employment variables - EMPLOYEES (number of employees), and VZE (hours worked) - equal to 0. We winsorize at the 1-99 level

Variable construction

- Operating expenses (OEXPN)= COGS + Personnel costs (PPERSA) + Good's taxes(ASTEU)
- Operating revenues (OREVN) = Sales (UMSATZ) + Other operating income (EUBRIG) + Own work capitalized (EEIGEN) + Subsidies (ESUBV) + Income from selling used assets (EGEBRSA)
- Financial Income = Income from financial earning assets (EZING) + Income from equity investments (EBETEIL)

- Sales growth at one and three years as log difference in sales between 1- and 3-years lags, respectively.
- CAPEX(t) = Fixed asset(t) - Fixed asset(t-1) + Depreciation(t), where Fixed asset variable is from Orbis and Depreciation from AMDC

H AMDC

Total sales revenue and earnings = OREVN		EZUS
Total sales revenue		UMS
Other operating income		EUBRIG
Income from the activation of own work		EEIGEN
Income from investments		EBETEIL
Interest, securities and similar returns		EZINS
Subsidies		ESUBV
Proceeds from the sale of used fixed assets (incl. land and buildings)		EGEBRSA
Total expenses		AGES
Advance payments (prepayments) = COGS		VORLEIST
	Procurement of goods for resale	AHW
	Procurement of services for resale or re-billed services	ADL
	Procurement of material for processing and manufacturing	AROH
	Expenses for external workers (temporary workers)	AFREMDAK
	Expenditure for contracted repairs and maintenance	AREP
	Expenses for renting buildings, machinery and means of transport	AMIET
	Expenses for operating leases of buildings, machinery and means of transport	AOPERLS
	Expenditure for finance leasing of buildings, machinery and means of transport	AFIN
	Other operating expenses	ASONST
Personnel Expenses		PERSA
Interest and similar expenses		AZINS
Depreciation of tangible and intangible assets		AABSCHR
Taxes and duties		ASTEU
Total investments		IGES
Investments in tangible assets		ISACH
	Investments in undeveloped land	IGRUND
	Investments in old buildings	ALTBAU
	Investments in the construction and conversion of buildings and structures	IBAU
	Investments in machinery and equipment, tools, operating and business equipment	IMASCH
	Investments in means of transport	ITRANSP
	Investments in used fixed assets	IGEBRSA
Investments in low-value assets (according to §13 EStG 1988)		IGWG
Investments in software		ISOFT
Investments in concessions, industrial property rights and similar rights		IKONZESS

Table H.1: Income statement, AMDC, LSE 2018-2020

Differently from 2021 onward, Proceeds from the sales of used fixed assets is not included in total sales revenues and earnings

Total Revenue = OREVN		EGESAMT
Sales Revenues		UMSATZ
Other Operating Income		EUEBRIG
Income from Capitalized Own Work		EEIGEN
Income from Equity Participations		EBETEIL
Interest, Securities, and Similar Income		EZINS
Subsidies		ESUBV
Income from the Sale of Used Fixed Assets		EGEBRSA
Total Expenses		AGESAMT
Advance Payments = COGS		AVORL
	Merchandise Intake	AHW
	Expenses for Other Related Services	ADL
	Material Use	AMATEINS
	Expenses for External Workers (Temporary)	AFREMDAK
	Expenditure for Contracted Repairs and Maintenance	AREP
	Expenses for Renting and Leasing Buildings	AMIETGE
	Expenses for Renting and Leasing Machinery and Transport	AMIETMA
	Other Operating Expenses	ASONST
Personnel Expenses		PERSA
Interest and Similar Expenses		AZINS
Depreciation of Tangible and Intangible Assets		AABSCHR
Taxes and Duties		ASTEU
Total Investments		IGES
Investments in tangible assets		ISACH
	Investments in undeveloped land	IGRUND
	Investments in old buildings	ALTBAU
	Investments in the construction and conversion of buildings and structures	IBAU
	Investments in machinery and equipment, tools, operating and business equipment	IMASCH
	Investments in office machines, data processing equipment and facilities	IBUERO
	Investments in broadcasting, television, and communications technology	IRFFN
	Investments in means of transport	ITRANSP
	Investments in used fixed assets	IGEBRSA
Investments in low-value assets		IGWG
Investments in software		ISOFT
Investments in concessions, industrial property rights and similar rights		IKONZESS

Table H.2: Income statement, AMDC, LSE 2021

EBT	**Total Revenues – Total Expenses**
Total Revenues	EGESAMT or EZUS+EGEBRSA
Total Expenses	AGESAMT or AGES
EBIT	**EBT – Financial Income + Financial Expenses**
Financial expenses	AZINS
Financial income	EBETEIL

Table H.3: Measuring profitability from AMDC variables