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"Precautionary Debt Capacity"

I was a paid part-time employee of the financial institution owning the data utilized for this paper but was not paid for work related to the paper. The paper was not subject to review by the financial institution prior to release. I have no additional relevant or material financial interests that relate to the research described in this paper.

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Precautionary Debt Capacity*

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Abstract

Firms with ample financial slack are unconstrained... or are they? In a field experiment that randomly expands debt capacity on business credit lines, treated firms draw down 55 cents on the dollar of expanded debt capacity in the long-run and increase total debt, investment, and profits by $2.02, $1.83, and $3.90 on the dollar, respectively. Firms with debt levels farthest below their borrowing capacity before the intervention display sizable treatment effects. The likelihood of financial distress in negative cash flow events is reduced for treated firms. Additional analyses support the precautionary motive to preserve unused capacity as insurance against negative cash flows.

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1 Introduction

Debt capacity lies at the heart of finance and macroeconomic analyses involving financial frictions (Shleifer and Vishny, 1992). Much emphasis has been paid to identifying its determinants. Some studies argue that debt capacity is driven by the liquidation value of physical assets that firms can pledge as collateral (Hart and Moore, 1994; Kiyotaki and Moore, 1997), while others emphasize the role of cash flows generated from firms' operations (Stiglitz and Weiss, 1981; Holmström and Tirole, 1998). Although there is much debate over the relative importance of asset-based collateral constraints (Chaney et al., 2012; Catherine et al., 2022) versus earnings-based borrowing constraints (Lian and Ma, 2021; Dreschel, 2023), there is broad consensus that debt capacity plays a fundamental role in firm dynamics, business cycles, and the transmission of monetary policy (Bernanke et al., 1999).

There is much less consensus, however, on why firms leave some of their debt capacity unused and whether this matters for their investment and growth. The textbook theory of financial constraints predicts that firms' optimal investment level should equal their debt capacity, or the maximum *borrowing power* set by lenders (Tirole, 2006).[1] Yet, growing research shows that firms have substantial unused debt capacity (Chodorow-Reich et al., 2022; Donaldson et al., 2024). One possibility is that firms lack viable investment opportunities (Banerjee and Duflo, 2014). Alternatively, they may prefer to maintain a "reserve borrowing capacity" (Modigliani and Miller, 1963) because irregularities in operating cash flows may limit firms' ability to fully utilize their capacity, as financing decisions must also account for claims issued to suppliers and workers (Fama, 1990).[2] While these are core issues in finance that "rest on fundamentally different views about investment behavior and the functioning of the capital markets" (Modigliani and Miller, 1963), they remain relatively understudied.

An ideal experiment would involve providing capacity expansion to some firms while withholding it from others with similar levels of unused debt capacity, holding investment opportunities fixed. This design enables clean identification of the effects

[1] Given the tax advantage of debt, a long-standing puzzle in corporate finance is why firms do not borrow "as much as possible" (Modigliani and Miller, 1958; Myers, 1977).

[2] Other possibilities include the investment opportunity being too large to be covered by existing capacity or serving as a form of insurance against distress risks.

of debt capacity by isolating the impact of capacity expansion among observationally identical firms that differ only in how much they could borrow, thus facilitating a direct test of the traditional financial constraints channel (Tirole, 2006). According to this view, firms with limited financial slack should be the most responsive to capacity expansion, as they stand to gain the most from alleviating financial constraints. If this prediction does not hold, it may be due, for example, to precautionary behavior arising from cash flow or distress risks (Fama, 1990). Comparing treatment effects across firms with varying levels of initial financial slack offers a way to evaluate these predictions. Yet, it is rare to encounter such an experiment in a natural setting.

In this paper, we collaborate with a large European bank in Türkiye (henceforth *our bank*) and conduct this experiment in the field. We first document that most firms have debt levels far below their borrowing capacity before the experiment. Second, despite the substantial untapped borrowing power, our randomized experiment leads to large and persistent increases in firm borrowing, investment, and sales. This contrasts with Tirole (2006)'s textbook theory of financial constraints, which suggests that we would expect no change in borrowing or investment if financial constraints are absent or if firms lack viable investment opportunities. Third, a subsample heterogeneity analysis reveals that more financial slack before the experiment drive the increase in sales. Finally, we examine possible explanations for these findings. Despite ample financial slack on average, 74 percent of firms had nearly exhausted their debt capacity at least once before the experiment, indicating liquidity risk. Firms with more volatile cash flows or financing also showed lower capacity utilization rates pre-experiment. Among treated firms, the risk of financial distress during negative cash flow events declined significantly. Overall, our results suggest that firms maintain financial buffers as a precautionary measure against cash flow shocks.

This paper's contribution is threefold. First, we propose a novel "precautionary debt capacity" explanation for why financing limits firm growth, even for firms that appear financially sound. The fact that seemingly unconstrained firms grow in response to capacity expansion indicates that they may have been forgoing growth opportunities to stay below debt limits before the intervention. This finding has important policy implications, as many growth-oriented policies presuppose that only

firms facing strict financial constraints benefit from financial support. However, our results indicate that targeting seemingly unconstrained firms could unlock significant growth. Second, we contribute to the literature on credit lines by highlighting that firms' effective borrowing constraints can be lower than they appear, particularly in environments where lender discretion is pervasive and the cost of default is high. Finally, we conduct the first-ever field experiment on business credit lines that addresses critical measurement and selection concerns established in the literature. Our well-identified estimates of the sensitivity of borrowing to increases in debt capacity serve as valuable "target moments" to discipline an important class of macro-finance models that feature occasionally binding debt constraints (Nakamura and Steinsson, 2018).[3] We show that experiment-based estimates differ substantially from covariate-controlled estimates from the pre-experimental period, underscoring the importance of addressing endogeneity concerns.

The experimental setting is as follows. Our bank periodically reviews its *existing* users of business credit lines and identifies firms eligible for a debt capacity increase using proprietary underwriting criteria that trades-off a potential increase in revenues with the risk of default. In 2014, our bank identified 3,169 firms that were pre-approved for debt capacity increases. Among this pre-approved group, we randomly offered unexpected debt capacity increases to 2,414 firms (i.e., the "treated" group) and withheld capacity increases for the remaining 755 firms (i.e., the "control" group). Our experimental sample consists of established, formal businesses with up to 10 employees. These firms are small but non-trivial in size, representing the bottom half of the size distribution among incorporated Turkish firms, including the median.[4] The intervention did *not* affect the cost of borrowing due to the interest rate cap in Türkiye, which was binding for all firms. We complement this experiment with our bank's administrative credit line data, firm performance data obtained from tax filings, and total debt – including debt issued by banks beyond our partner bank– obtained from the credit registry data maintained by the Central Bank of Turkey.

[3] In macro, there's a long tradition that builds on Hart and Moore (1994) and Kiyotaki and Moore (1997). There is also a growing finance literature incorporating debt constraints. See, for example, Hennessy and Whited (2005); Rampini and Viswanathan (2010); Greenwald et al. (2023).

[4] We use various measures of firm size (capacity, total debt, and profits) to compare our sample to the universe of privately-held firms in Turkey.

We begin by examining how firms utilized their credit lines before the intervention. On average, firms utilized 39 percent of their debt capacity, with a median utilization rate of 33 percent. Fewer than 10 percent of firms had nearly exhausted their capacity (98 percent utilization), suggesting that most firms had substantial financial slack. From the perspective of the traditional financial constraints channel, these findings imply that – on average – firms should not exhibit strong responses to capacity expansion. To test this prediction, we next evaluate the causal effects of debt capacity increases. We estimate Intent-to-Treat (ITT) and Local Average Treatment Effect (TE) that uses the treatment assignment, $\mathbb{Z}_i$, as an instrument for the change in debt capacity (Angrist and Imbens, 1994). ITTs capture the effect of debt capacity expansions on financing and real outcome. TEs capture the magnitude of this change *per dollar increase* in experiment-induced debt capacity.

We report three main findings. First, the experiment generated large and positive effects on drawdowns both in the short- (i.e., 12 months) and in the long-run (i.e., 36 months after the intervention). Treated firms' debt capacity increased by 55 percent relative to the pre-experimental mean, confirming a strong first-stage effect. Following this increase, their credit line debt increased by 52 percent in the short-run and by 83 percent in the long-run compared to the control group. The Wald estimator capturing the treatment effect implies that firms used 35 cents out of a dollar increase in debt capacity in the short-run and 55 cents in the long-run. These results are robust to adjusting for inflation and using other currencies. Reweighting our sample to match the credit line utilization distribution of the universe of privately-held Turkish firms delivers qualitatively similar estimates (i.e., 0.35 in our sample vs. 0.31 reweighted), indicating that our findings are not unique to our sample.

Firms actively managed their debt structure over time. In our setting, firms can draw from the *same credit lines* in two distinct ways: as high-interest revolving debt, which offers maximum repayment flexibility, or as interest-free term debt (point-of-sale financing), which requires fixed repayments.[5] Revolving debt can be used for any expenses, whereas term debt is restricted to financing investment-like durables and

[5]The key differences between these contracts lie in cost and repayment terms. Revolving debt is expensive, with a 24 percent APR, but offers flexible repayment options. In contrast, term debt is interest-free (our bank accepts fees from participating vendors that wish to offer this in-store financing option without conducting separate credit checks) but requires fixed monthly payments over 4 to 12 months.

professional business services. Decomposing treatment effects by contract type reveals that firms relied on a mix of revolving and term debt in the short run but shifted entirely to term debt to finance investments in the long run. Interestingly, roughly a quarter of investment expenditure that could have been financed using cheaper term debt were instead financed with more expensive revolving debt in the short run. A possible interpretation is that firms initially pay a premium for the flexibility of revolving debt to address cash flow concerns; once financially stable, they shift to the cheaper, but more rigid, term debt to prioritize long-term investments.

Second, the increase in debt capacity leads to significant financing and real effects at the firm level. For each dollar of capacity expansion, firms' total debt – issued by our bank as well as other banks – increased by \$2.02 after three years. Over the same time period, profits and sales rose by \$3.90 and \$7.19 per dollar of increased debt capacity. The increase in profits represent a sizable but plausible magnitude, corresponding to the effect of moving a firm from the bottom to the top quartile of the annual profit growth distribution.[6] Moreover, the magnitude of the ITT on sales translates to firms' ability to sustain operations for 20 days without generating any income, based on their pre-experimental monthly business expenses. Given that firms, on average, held 34 days of cash buffer before the experiment, the increase in sales lengthend their runway to absorb unexpected cash flow shocks by 58 percent.[7] Finally, for each dollar of capacity expansion, treated firms increased their investment and operating expenses by \$1.83 and \$4.57, respectively, reflecting a 50 percent increase relative to the pre-experimental mean. Overall, these findings demonstrate that the additional financing was directed toward productive uses, enhancing firms' operational resilience.

Third, firms with *greater* initial financial slack exhibited the largest real effects. Specifically, firms with below-median pre-experimental utilization ratios drew down 39 cents per dollar increase in debt capacity on their credit line. These firms earned \$9 in sales and incurred \$5.5 in business expenses for each dollar of capacity expansion after three years. In contrast, firms with less initial financial slack – those with above-median utilization ratios – drew down 56 cents on their credit lines and spent \$10 on

[6]The large estimate is not surprising given the scale of firms in our sample (Davis et al., 1996).

[7]Cash buffer days is computed as pre-experimental cash balances divided by cash outflows. In the US, median firms hold 27 cash buffer days, and the cash buffer a firm needs to survive without bringing any revenues range from 16 to 47 days, depending on the industry (Farrell and Wheat, 2016).

business expenses for each dollar of expanded debt capacity. Despite these significant borrowing and spending effects, these firms did not experience any change in sales. Overall, contrary to the predictions of the traditional financial constraints channel, *seemingly* unconstrained firms with ample financial slack appear to generate the most gains from capacity expansion.

Why, then, hadn't firms with financial slack utilized their existing credit lines to grow even before the intervention? According to Modigliani and Miller (1963), firms maintain substantial untapped borrowing power to "to preserve flexibility." In my setting, the demand for flexibility can arise for at least two reasons. First, uncertain cash flows and frequent liquidity risks create a strong incentive to preserve unused capacity as insurance against future borrowing constraints. Even though firms, on average, utilized only 40 percent of their debt capacity before the intervention, 74 percent of all firms faced the risk of fully exhausting their capacity at some point prior to the intervention.[8] Thus, concerns about facing liquidity risks due to volatile cash flows and financing are prevalent for a majority of firms. Second, default is particularly costly for firms in Turkey due to unlimited liability and lender discretion. Business owners' personal wealth is directly tied to their firms, meaning a default on business loans can result in personal bankruptcy and the loss of their homes, a dynamic highlighted by Myers (1977): "perhaps managers avoid high debt ratios in an attempt to stabilize their personal wealth ... bankruptcy costs probably discourage borrowing, as costs of financial distress are incurred when the firm comes under the threat of bankruptcy, even if bankruptcy is ultimately avoided."

We test several implications of this "precautionary motives" channel. First, firms with more unpredictable cash flow or financing volatility should exhibit lower utilization ratios. Our findings align with this prediction: cross-sectional analyses show that a one standard deviation increase in unpredictable expenses, sales, and financing volatility decreases utilization rates by 1.04, 1.56, and 5.57 percent, respectively.[9] Second, debt capacity is crucial for reducing firms' default risk. Treated firms are 1.34

[8]We classify firms using over 75 percent of their capacity as being at risk of fully maxing out their limits. Such borrowing constraints can arise for two reasons: (1) firms with volatile cash flows may draw down more at times, or (2) they may face lender discretion even without any cash flow risk. While both cases are common, cash flow risk is particularly pervasive: among the 74 percent of firms at risk of maxing out, 60 percent experience binding constraints without any lender-initiated limit reductions.

[9]These estimates hold up to controlling for differences in firm age or size.

percentage points less likely to fall in distress (i.e., delinquency or renegotiation), representing a 50 percent reduction in their distress probability. Capacity expansion is particularly beneficial during negative cash flow shocks, as it lowers the probability of distress by 5.03 percentage points, representing a 66 percent reduction in distress likelihood. Thus, even if firms initially had ample financial slack, they may be willing to invest more if capacity expansion provided insurance against this downside risk.

While these findings support the precautionary channel, there are other potential explanations. One possibility is that capacity expansion enables firms to undertake "lumpy investments" that were previously infeasible with existing capacity. We examine whether treated firms are more likely undertake "lumpy" investment, or instances where the maximum spending using term debt for a given category exceeds pre-experimental unused capacity. We find that only 4 percent of firms engage in lumpy investments during the first year. Another explanation is that firms borrow and grow more after the intervention because lender-initiated capacity increases "encourage" them by signaling greater profitability. To test this, we examine treatment effect heterogeneity based on the number of capacity increases firms experienced before the intervention. Intuitively, firms experiencing capacity expansion for the first time should be most "encouraged" due to the higher signaling value. However, we find little heterogeneity in borrowing across firms based on prior capacity expansions. While these findings do not entirely rule out other potential explanations, we do not find strong supporting evidence for these alternatives.

This paper contributes to the literature on the determinants of debt capacity. Firms' cash flows from operating earnings has been highlighted as an important factor shaping firms' debt capacity (Stiglitz and Weiss, 1981; Holmström and Tirole, 1998). Consistent with this, Lian and Ma (2021) shows that earnings-based borrowing constraints (EBCs) are pervasive in the U.S., and Dreschel (2023) illustrates that firms with EBCs are more sensitive to investment shocks relative to firms with collateral constraints (see, also, Greenwald (2019) for EBCs in mortgage markets). An important insight from these studies is that EBCs impose effective borrowing limits, as firms seek to avoid costly covenant violations. Because violating earnings-based covenants triggers creditor-imposed restrictions on financial and investment decisions, firms limit bor-

rowing to stay in compliance. This paper provides complementary evidence that effective borrowing power is substantially lower than the observed debt limits because firms have precautionary motive to maintain a financial buffer as insurance against negative cash flow shocks and distress risk.

This paper also connects to the literature on committed bank credit. Credit lines constitute the majority of committed bank credit (Shockley and Thakor, 1997) and serve as a central liquidity management tool (Sufi, 2009; Acharya et al., 2013). Much of the literature focuses on the funding risks of credit lines, particularly the inability of borrowers to access liquidity in bad times due to lender discretion (Chodorow-Reich et al., 2022), especially during economic downturns (Ivashina and Scharfstein, 2010; Campello et al., 2010). However, there is much less work exploring why credit lines are pervasive but rarely drawn down to their fullest extent.[10] Recent exception is Donaldson et al. (2024), which highlights the role of credit lines as a debt dilution mechanism that protects existing debt. Intuitively, credit lines can serve as a (lender's) commitment device deterring (borrower's) new debt, which explains why only a small amount of committed credit is actually utilized. We highlight a complementary, borrower-driven explanation for why firms do not fully utilize their credit lines. This perspective is particularly important in a setting where lender discretion is pervasive and weak debtor protection (Djankov et al., 2008).

To the best of our knowledge, this is the first experimental evidence on business credit lines. Our empirical approach addresses critical measurement and selection concerns. First, we directly observe firms' debt and utilization ratio (debt-to-limit) before the intervention, which allows us to estimate heterogeneous effects of capacity expansion by initial financial slack. Second, the lender-initiated capacity expansion was applied *automatically* and communicated to the firm via phone or text, with no prior announcement. This "surprise" treatment minimizes selection concerns that arise when firms seek financing in anticipation of future needs. Thus, we can estimate the effect of capacity expansion while holding expectations about future debt capacity and information about investment opportunities fixed.[11] This approach, combined

[10]This question received attention in a dynamic structural literature that incorporates firms' trade-off between borrowing more today versus facing liquidity risks in the future. See, for example, Gamba and Triantis (2008), Nikolov et al. (2019) and Amberg et al. (2023).

[11]We provide direct evidence that our research design addresses endogeneity concerns. Specifically,

with administrative data offers a rare opportunity to provide well-identified, reliable estimates that can provide a useful target moment for any macro-finance models that feature occasionally binding debt constraints.

We note that, while our experiment was conducted in Türkiye, our findings offer general insights that are applicable to broader contexts like the U.S. and other developed economies. First, bank-dependent small firms, like those we study in this paper, are highly prevalent, accounting for over 80 percent of firms in the U.S. and 90 percent of businesses globally (SBA, 2019; The World Bank, 2023). These firms play a crucial role in understanding how monetary policy transmits to the real economy, as they are particularly responsive to monetary shocks in the presence of financing frictions (Caglio et al., 2021). Second, the credit utilization behaviors we document – where only a small amount of committed credit is actually utilized – closely mirror patterns observed among U.S. firms (Chodorow-Reich et al., 2022; Greenwald et al., 2023), suggesting that precautionary debt capacity is likely widespread, even in the U.S. Finally, survey evidence reinforces firms' desire to maintain financial buffers: more than 50 percent of firms report actively preserving credit lines to maintain flexibility and avoid financial distress during downturns (Graham, 2022). Thus, the nature of frictions identified in our study likely extend to a broader set of firms.

2 Experimental Context and Design

This section describes the institutional setting and our experimental design.

2.1 Institutional Background

Firm Size Distribution in Türkiye. Privately-held small firms form the backbone of the Turkish economy, providing a major source of income for low-income urban households and accounting for half of the country's total value added (Turkish Statistical Institute, 2021). Starting in the 2000s, Türkiye pursued ambitious reforms that fueled dramatic economic growth and halved the poverty rate between 2006 and 2020 (The World Bank, 2023). Small and medium-sized firms were central to this agenda,

covariate-controlled estimates from a pre-experimental period are only half as large as the experimental estimates, suggesting that randomization mitigates downward bias.

leading the Turkish government to implement one of the world's largest credit guarantee programs (7.6% of GDP) in 2017 to foster their growth (OECD, 2019; Akcigit et al., 2021). As of 2023, Türkiye is the world's 19^{th} largest economy, ranking just below the Netherlands (18^{th}) and above Switzerland (20^{th}). Table A.1 compares Turkey to the U.S., Europe, and other emerging markets. Turkey's GDP per capital is comparable to that of Bulgaria, Greece, and Romania, and its poverty rate is similar to that of the U.S.

Despite privately-held small firms' central role in the Turkish economy, financial access remains a major impediment for Turkish businesses. In 2019, access to finance was the biggest business obstacle in Türkiye across all firm sizes. Whereas only 15 percent of small-and-medium sized firms in Europe and Central Asia stated access to finance was the most important constraint (The World Bank, 2019), a third of Turkish businesses indicated financing was the most important hurdle, more than other challenges such as tax rates and political instability. Small firms rely exclusively on bank lending and face greater costs in raising external capital, making them disproportionately sensitive to macroeconomic fluctuations and credit cycles (Davis et al., 1996; Berger and Udell, 2002). Given the importance of financing, the experiment generates high-stakes opportunities for participating businesses.

Our bank. We collaborate with one of the largest commercial banks in Türkiye, which offers retail banking products to privately-held firms. Our bank holds a 17 percent market share in Turkey's retail banking, serving over 13 million businesses representative of the population that participates in the local banking system.[12] The bank underwrites covenant-free, unsecured lines of credit in exchange for an upfront fee. Firms can draw any amount up to the committed debt capacity at any time. These contracts typically last 3 to 5 years and are renewed upon expiration. Our bank's clients operate in a wide range of sectors including services (e.g., health, education, restaurants, etc), construction (e.g., builders, construction materials, manufacturer, etc), food (e.g., bakery, butcher, grocery, etc), among others.[13] See Table A.5 for the sectoral breakdown of business credit line clients at our bank.

[12]The market share is comparable to that of JPMorgan Chase in the US (Statista, 2023).

[13]We do not directly observe sectoral breakdown for our sample of firms. However, our bank confirmed that the sector distribution for our sample looks similar to the universe of our bank's clients. We further corroborate this by inferring sector based on firms' spending patterns.

Firms have two ways of drawing on the *same* credit line: as flexible-payment revolving debt or fixed-payment term debt. With revolving debt, firms can borrow up to their credit limit during a billing cycle and choose to pay off the debt partially or in full, or carry it over by paying a preset interest rate on the unpaid balance. The regulator caps the interest rate (APR) at 24 percent, and this cap is binding for all firms. Therefore, all firms in our experiment face the same interest rate, which does not vary over time or across firms. Alternatively, firms can draw term debts with fixed payments until the principal and interest are paid off. In our setting, all term debts are point-of-sale (POS), allowing businesses to purchase from participating vendors and pay over 3-12 months, typically interest-free.[14] Our bank accepts fees from vendors and originates these loans without a separate credit check. Term debts are issued in-store at the time of purchase and cannot be refinanced, so any increase in term debt reflects new spending, not refinancing. Their use is limited to financing durables (e.g., machinery, auto parts) or professional business services (e.g., advertising/marketing, etc). See Table A.2 for detailed categories.

2.2 Research Design

Sample. The experiment's participants include 3,169 businesses approved for a capacity increase in the summer of 2014. These firms were identified by the bank and they are *not* applicants that sought out more credit. Our bank periodically assesses existing clients' debt limits to identify firms that may need higher debt capacity in real terms. Firms that recently opened accounts or received a capacity increase within the last six months were excluded. The bank then selects potential candidates based on proprietary underwriting criteria, balancing predicted revenue and costs based on the firms' credit line usage history and other characteristics. This process resulted in 3,169 businesses being eligible for automatic credit increases.

[14]There are several reasons why term debts are less expensive than revolving debt. First, from our bank's perspective, term debts are subsidized by participating vendors, who pay a transaction fee each time businesses draw down term debt on lines of credit to finance purchases. While term debts aren't technically collateralized, they can be considered secured debt since the bank can seize the durable asset purchased with the term debt in the event of bankruptcy. Additionally, term debts typically have lower expected losses compared to revolving debt.

Randomization The eligible firms are stratified into equal-width bins based on their end-of-billing cycle balances relative to their debt capacity. A random subsample is drawn from each stratum using a random number generator. This group, denoted as $\mathbb{Z}_i = 1$, receives an automatic capacity increase, while while the control group ($\mathbb{Z}_i = 0$) is excluded from the lender-initiated capacity increase. Although assignment to the treatment group ($\mathbb{Z}_i$), is random, the magnitude of the capacity increase is not randomized. See Figure 1 for the experimental timeline. Due to our bank's institutional constraints, the treatment assignment is rolled out over two calendar months.

The capacity expansion was applied automatically and communicated to treated firms via email or text, depending on their preferred method of notification they selected when they first opened a credit line account. Since the approval process was part of the bank's regular business operations, the experimental sample was unaware they were participating in an experiment.

Macroeconomic Environment. Macroeconomic conditions in Türkiye were relatively stable during our experimental period. Figure A.1 plots annual inflation, GDP growth rate, and the exchange rate with U.S. dollars. In our analysis, we conduct robustness tests by measuring key outcomes in real Turkish Lira and US dollars to confirm that our results are not driven by inflation or fluctuations in exchange rates.

2.3 Data

We obtained administrative credit line data covering three years before and after the experiment. The data includes monthly information on firms' debt capacity, drawdown balances, spending transactions for each type of debt (e.g., revolving vs. term), loan performance (delinquency / restructuring), the presence and amount of liquid assets (checking, savings, or brokerage) at our bank, whether the firm has accounts at other banks, and the owner's age. We supplemented this with quarterly data on the firm's total debt, including both non-credit line debt issued by our bank and debt from other banks. This data is obtained from credit bureau. Annual sales and profits data are available for a subset of 551 firms. See Table A.3 for detailed variable definition and data sources.

Outcomes. Our main outcomes of interest are debt capacity, credit line debt, and financial and real outcomes at the firm level. Debt refers to the stock of interest-accruing credit-line debt carried across periods, net of balances paid off at the end of the billing cycle. Transactions on revolving and term debt can be categorized into eight spending categories, as shown in Table A.2. Monthly spending may exceed drawdowns (i.e., debt) since it captures all transaction flows, including those paid off within the billing cycle. Firm-level total debt and other debt refer to the outstanding interest-accruing balances carried over across periods. Total debt captures all liabilities issued by our bank or other financial institutions and is obtained from Memzuç, the credit registry maintained by the Central Bank of Turkey. Investment is defined as total spending on capital expenditures (e.g., appliances, electronics, machinery).

Annual profits, defined as net income (revenues minus expenses), are reported on a fiscal-year basis and obtained from tax filings for a subset of 551 firms. These firms represent our bank's core client base with access to multiple financing products. Specifically, our bank requires clients enrolled in one of their eight bundled financing "packages" to submit their tax filings annually. These packages are tailored to address a variety of business needs. For instance, the "Promissory Note Package" is tailored to firms with supplier relationships that necessitate frequent wire transfers and includes benefits such as waived wire transaction fees and a complimentary checkbook. Similarly, the "Foreign Trade Package" is designed for firms engaged in international trade, providing advantages such as foreign currency transaction fee waivers, complimentary transfers via the bank's ATMs, and a checkbook. See Table A.4 for a comparison of how firms with profits information to those without. We infer sales by adding business expenses to net income. Using expenses on credit lines to estimate total firm-level expenses is a reasonable proxy, as 60 percent of firms with profits data exclusively bank with our bank and have no other banking relationships. However, sales may be underestimated if firms maintain additional business credit line accounts at other banks that we cannot observe. We conduct robustness using a sample of firms that have an exclusive banking relationship with our bank to address this concern.

Representativeness. How do firms in our sample compare to the national distribution? To assess the representativeness of our sample, we obtained statistics on the size

distribution for the universe of *incorporated* Turkish firms from the Central Bank of Türkiye. Table A.6 reports these statistics. Compared to the national distribution, our firms cover the bottom half of the capacity distribution within the universe of Turkish firms, including the median. Specifically, the debt capacity of a median firm in the national dataset aligns with the debt capacity of the top 5 percent of firms in our sample. Total debt for firms in our sample is comparable of that for the firms below the median in the universe. According to statistics from the Central Bank of Türkiye, firms with credit lines of similar size to those in our sample typically employ five people. Overall, based on different measures of firm size (capacity, total debt, and profits), our sample is comprised of firms that are small, but non-trivial in terms of size.

2.4 Randomization Assessment and Estimation Framework

Balance. The random assignment successfully balanced the characteristics of treated and control firms. Table 2 reports covariate balancing in levels for all sample and for the subsample of firms with sales and profits information. Figure 2 helps to visualize the pre-trends for a set of key outcomes by $\mathbb{Z}_i$. Treated and control firms' trends in credit usage and debt levels are similar as far back as three years preceding the intervention. We further report statistical tests to assess the validity of randomization for the four quarters preceding the intervention in Table A.7.

Estimation Framework. We first focus on the short run, the first year of the experimental period. We estimate first-stage (FS) and intent-to-treat (ITT) using simple regressions of the form

$$Y_i = \beta X_i + f_s + \epsilon_i \tag{1}$$

where i denotes a business and f_s stands for randomization strata fixed effects. The FS and ITT specifications compare the average change in capacity and debt between the treatment group and the control group over a period τ using ordinary least squares (OLS). In these specifications, X_i is set to $\mathbb{Z}_i$, which is orthogonal to measurement error and omitted variables, such as shocks to cash flows and investment opportunities.

The treatment effect is calculated as the ratio of the ITT and FS effects:

$$\beta_{\tau}^{\text{Wald}} = \frac{\beta_{\tau}^{\text{ITT}}}{\beta_{\tau}^{\text{FS}}} = \frac{\mathbb{E}\left[\Delta^{\tau} \text{ Debt}_i | \mathbb{Z}_i = 1\right] - \mathbb{E}\left[\Delta^{\tau} \text{ Debt}_i | \mathbb{Z}_i = 0\right]}{\mathbb{E}\left[\Delta^{\tau} \text{ Capacity}_i | \mathbb{Z}_i = 1\right] - \mathbb{E}\left[\Delta^{\tau} \text{ Capacity}_i | \mathbb{Z}_i = 0\right]}$$

and captures the local average treatment effect (LATE) of increased debt capacity. Since the model is exactly identified, the Wald estimate aligns with the treatment effect obtained through a two-stage least-squares (2SLS) approach, where changes in capacity and debt are used as the endogenous and outcome variables, respectively, with $\mathbb{Z}_i$ as an instrument for the change in debt capacity.

We examine long-run dynamics using the following distributed lag specification:

$$Y_{it} = \sum_{j=1}^{T} \gamma_j X_{ij} + f_t + f_s + \epsilon_{it} \tag{2}$$

where f_t and f_s denote time and strata fixed effects, respectively. For FS and ITT specifications, the instruments (i.e., X_{ij}) are the treatment assignment $\mathbb{Z}_i$ interacted with time dummies $\times f_{t=j}$. We derive long-run cumulative first-stage and intent-to-treat coefficients, $\Gamma_{\tau}^{\text{FS}} = \sum_{j=1}^{\tau} \gamma_j^{\text{FS}}$ and $\Gamma_{\tau}^{\text{iTT}} = \sum_{j=1}^{\tau} \gamma_j^{\text{ITT}}$, by summing the point-in-time coefficients. Cumulative treatment effects, $\Gamma_{\tau}^{\text{TE}} = \sum_{j=1}^{\tau} \gamma_j^{\text{TE}}$, measure the change in the outcome $Y_{i,t}$ per unit change in capacity over τ quarters, with γ_j^{TE} estimated using 2SLS . For the long-run analysis, data is collapsed into quarterly intervals and effects are estimated on NxT = 3,169 x 12 participant-quarter observations, with robust standard errors clustered at the firm level.

We examine how our bank's debt capacity increases transmit into firm-level financing and real outcomes by following an approach similar to Khwaja and Mian (2008) and Greenwald et al. (2024):

$$\widetilde{\mathbb{Z}_i} = \mathbb{Z}_i \times \left(\frac{DebtCapacity_i^{pre}}{TotalDebt^{pre}{}_i}\right) \tag{3}$$

Specifically, we compute each firm's overall exposure to our bank's debt capacity changes, as the treatment assignment weighted by the share of our bank's debt capacity relative to total debt in the pre-experimental period. Using this definition, we

estimate the regression:

$$Y_i = \beta \widetilde{\mathbb{Z}}_i + f_s + \epsilon_i \tag{4}$$

The idea is that firm-level effect should depend on the extent to which the firm relied on our bank's credit line relative to their total liabilities before the experiment.

Note that the experiment features partial compliance because our bank did not prevent control firms from requesting and obtaining higher debt capacity (i.e., "always-takers") nor did they prevent treated firms from opting out (i.e., "never-takers").[15] Specifically, the never-takers represent firms that opted out of automatically receiving promotional offers when they first opened a credit line. Partial compliance is a common feature of RCTs, and 2SLS-IV addresses this by identifying effects on compliers (Angrist and Pischke, 2009).

2.5 Pre-Experimental Borrowing and Debt Capacity

Before we examine the causal effect debt capacity expansions, we first assess how our experimental sample looks before the intervention and establish that most firms we study do not appear to be financially constrained. Table 1 displays summary statistics. Before the experiment, the average line of credit capacity was 4,662 TRY, with an average debt (stock) of 1,569 TRY. Of this debt, 44 percent of represented high-cost revolving debt, while 56 percent were interest-free term debts. Average monthly spending (flow) using credit lines was 823 TRY, with three-quarters allocated to operating expenses and one-quarter to financing investments via term debts. On average, firms held 1,248 TRY in liquid cash deposits at our bank. Figure A.2 reports other pre-experimental firm characteristics, including liquidity structure, account age distribution, and the frequency of capacity increases and facing binding constraints.

We further evaluate firms' pre-experimental credit usage and financing choices, and document two key facts. First, most firms had debt levels far below their available capacity. Figure 3a visually illustrates this finding. In the month prior to the intervention, fewer than 10 percent of firms were genuinely financially constrained, meaning they had nearly exhausted their credit capacity (with a utilization ratio above 98 per-

[15] After 12 months, 77% of the treatment group and 24% of the control group received a capacity increase, while 22% of treatment did not receive a capacity increase.

cent). On average, firms utilized 39 percent of their debt capacity, while the median firm used only 33 percent. About 13 percent of firms had no outstanding debt on their credit lines at all. Second, despite revolving debt being markedly more expensive than term debt, most firms relied on revolving debt. Figure 3b plots the distribution of financing choice, or the ratio of revolving debt to the total credit line debt. Three mass points stand out 13 percent of firms do not borrow at all; 17 percent exclusively borrow on term debts; and 14 percent exclusively borrow expensive revolving debt. The remaining 56 percent of the businesses use a mix of revolving and term debts.

3 The Effect of Debt Capacity Expansions on Credit Lines

This section examines the causal effects of debt capacity expansions on credit lines. Section 3.1 presents results on capacity and debt. Section 3.2 reports results on credit line contract choice.

3.1 Capacity and Debt

The First-Stage Effect Capacity. The random assignment $\mathbb{Z}_i$ had a large and monotonic effect on debt capacity. Panel A of Figure 4 displays the evolution of debt capacity for the treated and the control group in the short- (left) and in the long-run (right). Treated firms' debt capacity increased sharply in the first two months and remained high after 12 months. The difference in debt capacity between the treatment and the control group remained wide and did not attenuate in the long-run.[16] Figure A.3 shows these results in real TRY, adjusting for inflation.

The first-stage effects are economically large and statistically significant. To assess the economic significance, Columns 2-4 of Table 3 report estimates using the short-run specification 1. Columns 4 show that the experimental assignment is associated with a first-stage effect of 2,351 TRY after 12 months, which constitutes a 50 percent increase relative to pre-experiment capacity reported in Column 1. The F-statistic for the 12-month first-stage regression is 104, indicating that $\mathbb{Z}_i$ is a strong instrument for debt capacity. Columns 5-7, which report estimates using the long-run specification 2,

[16]Control firms' capacity also increased slightly over the same period, in line with partial compliance discussed in Section 2.4.

show that these effects persist in the long-run.

The Intent-to-Treat Effect on Debt. The randomized debt capacity expansions led to a large and immediate borrowing response for treated firms. Panel B of Figure 4 illustrates that treated firms' borrowing increased sharply in the first five months of the experiment and leveled off at high levels six months into the experiment. The chart on the right shows that the increase in drawdowns remain high well beyond the first year and persist until 36 months after the intervention.

The ITT effects on drawdowns are large and economically meaningful. The second row of Table 3 shows that relative to the control group, treated firms borrowed 820 TRY more after 12 months. The magnitude of the 12-month debt response corresponds to a 52 percent increase in the pre-experiment capacity mean reported in Column 1. The estimate is statistically significant (p=0.003). The short-run effect may be temporary if firms are pulling expenditures forward. Columns 5-7 show evidence consistent with the latter explanation. Treated firms drew down 1,296 TRY more after 3 years, relative to the control group.

Treatment Effect. The third row of Table 3 reports the treatment effect, $\beta_{\tau}^{\text{Wald}}$. For a dollar of debt capacity increase, firms drew down 21 cents (i.e., $538/2,562 = 0.21$) after 3-months and 35 cents after 12-months. In the long-run, firms borrowed 44 cents after 2 years and 55 cents after 3 years out of a dollar increase in debt capacity. Overall, we document a large and persistent increase in drawdowns, suggesting that firms committed their debt capacity to financing projects in the long-run rather than temporarily pulling expenditures forward.

Magnitude Assessment. We provide direct evidence that randomization mitigates selection bias in estimating financing effects. In the spirit of Lalonde (1986), we compare treatment effects from non-experimental analysis to our experimental benchmark to gauge the size and the direction of bias one might encounter without randomization – i.e., fixing our sample, how different would our estimates be if we did not use an experimental approach? Compared to our 12-month experimental benchmark TE of 35 cents, non-experimental methods using observational data yield estimates between 18 to 21 cents on the dollar, depending on the estimation method. Section A discusses

these results in detail. This exercise underscores the importance of having a credible, exogenous instrument for financing. The fact that we get a substantially different estimate *for the same set of firms* demonstrates that randomization effectively addresses selection bias in observational data.

Robustness. Section B describes additional evidence that my findings are robust to adjusting for inflation, firm selection, and balance shifting. Some key examples include: (1) using real TRY and US dollars to ensure that our results are not driven by inflation or exchange rate fluctuations, (2) re-weighting our sample to match the distribution of the universe of Turkish firms to validate generalizability, (3) restricting the sample to firms without accounts at other banks to address potential substitution effects, and (4) confirming that firms are not substituting between cash and credit line.

3.2 Financing Choice and Spending

To better understand how firms use their expanded debt capacity, we next analyze their financing contract choice. As discussed in Section 2.1, firms can draw on the *same* credit line as high-cost, flexible-payment revolving debt or interest-free, fixed-payment term debt. We examine firms' financing choice by decomposing treatment effects – i.e., how much firms borrowed per dollar of expanded debt capacity – into its revolving and term debt subcomponents.

Firms initially used a mix of high-cost revolving debt and interest-free term debt, but over time, they gradually shifted away from revolving debt and relied exclusively on term debt in the long run. Panel B of Table 3 report short-run and long-run effects for each financing type. After 12 months, out of 35 cent increase in drawdowns per dollar of expanded debt capacity, revolving debt accounted for 43 percent (15 cents) whereas term debt accounted for the remaining 57 percent (20 cents). Given that firms, on average, held 40 percent of credit line debt in revolving and 60 percent in term debt before the experiment, treated firms simply expanded their existing financing structure in the short-run. Firms exclusively relied on term debt in the long run. Columns 5 to 7 show that term debts accounted for 58 percent of total drawdowns out of debt capacity expansion in the first year, 84 percent in the second year, then 100 percent of treatment effect after three years. Figure A.4 provides a visual display of the dynamics

of financing choice in the short- and in the long-run.

4 Firm-Level Financing and Real Effects

We next explore how changes in our bank's credit line debt capacity affect other borrowing at the firm-level beyond credit line debt, investment, and growth. As detailed in Section 2.4, we use specification 4 to analyze firm-level effects. Section 4.1 discusses firm-level financing, Section 4.2 covers investment and spending, and Section 4.3 presents results on sales and profits.

4.1 Total Debt and Cash Balances

Debt capacity expansion resulted in increased total borrowing at the firm-level. Table 4 reports estimation results on the impact of capacity expansions on firm-level total debt and cash holdings. Columns 2-4 report ITT effects and Columns 5-7 report treatment effects. Debt capacity expansions led firms to lever up gradually, increasing total debt by 1.14 TRY per dollar increase in debt capacity after Y1 then having a cumulative effect of 2.02 TRY on the dollar after Y3 of the experiment. Overall, these results suggest that firms are increasing overall borrowing rather than substituting credit line debt with other types of debt. This finding is consistent with credit lines complementing other types of debt (Donaldson et al., 2024).

Debt capacity expansion had no impact on firms' cash holdings. Prior research suggests that firms might allocate costly external finance to build liquidity when external financing is cheap or when the shadow value of future internal funds is high (Eisfeldt and Muir, 2016). However, the second row shows that cash balances remained unchanged over time, indicating that firms did not simply substitute cash with debt or draw from their credit lines to accumulate cash liquidity. The lack of change in cash balances, combined with increased borrowing, suggests that firms may be directing their funds toward productive capital. We next evaluate firms' spending patterns.

4.2 Investment and Spending

We harness our unique administrative data to track firms' spending patterns. Our bank classifies all credit line transactions into eight mutually exclusive categories, based on the transaction counterparty's point-of-sale identifier. We group these business expenses into three categories: (1) professional business services, which include advertising, consulting, marketing, etc., (2) investment spending on capital expenditures like machinery, equipment, durable expenses, etc., and (3) all other operating expenses. See Table A.2 for more details on purchase categories.

Treated firms increased their overall spending on investment and operating expenses. As shown in Table 5, spending rose across all three categories, with firms allocating $1.50 to business services, $1.83 to investment, and $4.57 to operating expenses per dollar increase in debt capacity over the long-run. The ITT coefficients indicate that investment spending increased by 4,402 TRY after three years, or 1,467 TRY annually ($\frac{4,402}{3 years}$). This represents a substantial and meaningful 46 percent increase relative to pre-experimental average investment spending, as shown in Column 1. To understand the economic magnitude, Figure A.5 shows examples of investment goods one can buy based on the Y3 response. Firms can afford an industrial refrigerator or an industrial dishwasher and jumbo teapot. Figure A.6 provides a more detailed breakdown of how firms allocated their capital by types of contract. Overall, these results suggest that firms used their expanded debt capacity to invest in productive capital and scale up their business operations.

4.3 Sales and Profits

We next turn our attention to firm-level effects on sales and profits to test whether expanding debt capacity ultimately helped firms to grow and become more profitable. Sales and profits are available for a subset of 551 firms in fiscal year intervals.

The experiment led to significant increases in sales and profits. Table 6 displays these results. Columns 2-4 show that treated firms earned 25,482 TRY more than the control group after three years. This increase is statistically significant and economically large, corresponding to 35 percent (8,494 TRY) annual increase compared to the

baseline mean. Similarly, profits increased by 13,807 TRY after three years, representing a 26 percent annual increase compared to the baseline mean, or 4,602 TRY. In terms of treatment effects, firms generated $7.19 in sales and $3.90 in profits per dollar of increased debt capacity.

Our estimated effects are sizable but plausible. For example, consider the 26 percent annual increase in profit. The average profit growth rate for our firms before the experiment was 12 percent, indicating that this estimated increase is roughly twice the mean growth rate. To put another way, the estimated effect is comparable to one standard deviation of the annual profit growth rate prior to the experiment (34 percent) and about the same order of magnitude as moving a firm from the bottom to the top quartile (18 percent). The large estimate is not surprising, given the scale of firms in our sample, and is consistent with prior research showing that small firms experience larger growth (and contraction) rates compared to large firms (Davis et al., 1996). Furthermore, the magnitude of these results is comparable to findings from other studies involving firms of similar scale. For instance, Cai and Szeidl (2023) documents a 24 percent increase in profits for firms in China that randomly receive access to credit.

5 Precautionary Debt Capacity

This section examines firms whose debt levels were farthest below their borrowing capacity prior to the intervention. Section 5.1 focuses on the impact of capacity expansions on this specific group. In Section 5.2, we further assess the factors driving precautionary debt capacity.

5.1 Firms with Financial Slack

Given that the average firm in our sample maintained a substantial capacity buffer before the experiment, the treatment effect (TE) we have documented so far is somewhat puzzling from a *static* view of financial constraints. To better understand the behavior of firms with more slack, we focus our analysis on firms with below-median utilization rates prior to the experiment. We calculate each firm's average utilization rate over the 12 months before the intervention and restrict the sample to those with below-median utilization rates. On average, these firms utilized only 23 percent of

their debt capacity before the intervention.

Capacity expansion led to a moderate increase in drawdowns and a significant rise in sales and expenses for these firms. Table 7 presents these findings. Initially, the increase in debt capacity had minimal impact on firms' credit line drawdowns during the first two years. However, three years after the experiment, firms cumulatively drew down 39 cents for every dollar increase in capacity. Despite the delayed effect on borrowing, effects on sales and business expenses materialized sooner. The second row of the table shows that treated firms earned 46,463 TRY more than the control group after three years, representing an annual increase of 58 percent relative to the baseline mean reported in Column 1 ($46,463/3 = 15,488$ vs. 26,569). Similarly, total business expenses increased by 12,366 TRY, or corresponding to an annual effect of 43 percent. In terms of treatment effects, firms cumulatively earned \$9.01 and spent \$5.46 for every dollar increase in debt capacity. Table A.11 reports estimates for firms with above-median utilization rates before the experiment. Financially constrained firms drew down heavily on credit lines, borrowing 0.56 cents on the dollar after three years.[17] Even though treated firms increased business spending, they did not see gains in sales. These results suggest that capacity expansions had the largest real effects on firms with more financial slack, even though these firms only borrowed moderately from the expanded debt capacity.

5.2 Drivers of Precautionary Debt Capacity

Why does expanding debt capacity lead to significant real effects for seemingly unconstrained firms? We conjecture that firms seek to self-insure against the uninsurable risk in their capital income and liquidity. Firms weigh the benefits of borrowing more today against the potential costs of facing liquidity risks tomorrow (Hennessy and Whited, 2005; Gamba and Triantis, 2008; DeAngelo et al., 2011; Nikolov et al., 2019). Under this *dynamic* lens of financial constraints, firms have high *shadow* constraints holding back their growth potential even if they do not appear to be financially constrained (Froot et al., 1993).

This study's setting makes the above-mentioned theoretical predictions on precau-

[17] Figure A.8 reports heterogeneous treatment effects across the utilization distribution.

tionary behavior and firm growth highly plausible. First, default is very costly in Turkey. It is common practice for banks to require a personal guarantee or collateral when issuing a business loan, and access to credit markets is severely restricted if a firm is delinquent on a credit account, suggesting that owners bear significant personal risk of business failure (i.e., limited risk-sharing opportunities). Second, firms face considerable volatility in financing needs and cash flows. Thus, Turkey's uncertain economic environment and creditor-friendly financial system can generate significant risk aversion and motivate firms to maintain substantial buffers to avoid distress.

Financial Distress. We corroborate this interpretation in two ways. First, we confirm that capacity expansion reduces financial distress, particularly when the firm faces a negative cash flow shock. Column 1 of Table 8 shows that the treatment assignment $\mathbb{Z}$ reduces the probability of distress by -0.85 percentage points or 26 percent of the control mean. Columns 2 and 3 show similar results for credit line delinquency and restructuring rates.[18] Figure A.7 presents a visual illustration of this result. Capacity expansion is particularly valuable when a firm faces a negative cash flow shock. We first define unexpected cash flow change by computing year-over-year changes in sales after adjusting for seasonality and firm-specific factors. Then, a large cash flow shock refers to years when the reduction in sales exceeds one standard deviation of the unexpected change in cash flows. Column 4 shows that a cash flow shock increases the probability of distress by 7.6 percentage points, or 2.3 times the outcome mean. However, treated firms are better able to absorb this risk, as capacity expansion reduces the distress probability by 5.03 percentage points, bringing it down to 2.53 percent. This reduction is economically meaningful, representing a 66 percent decrease in the probability of distress. These results suggest that debt capacity materially reduces default risk, consistent with the precautionary motive to maintain a financial buffer. Table A.12 further validates this interpretation by showing that higher utilization or frequency of facing binding constraints increases default risk.

Volatility. Second, we confirm that volatility in financing and cash flows create precautionary motives to maintain financial buffer by conducting a cross-sectional analysis on how various volatility measures correlate with utilization rates. This analysis is

[18] Our bank classifies accounts as non-performing if payments are overdue by 90 days or more.

restricted to control firms unaffected by capacity expansions. As a first step, we construct proxies of expense, cash flow, and financing volatility, σ_i^k, where $k \in$(expense, cash flow, financing):

$$\sigma_i^k = Std.Dev(\eta_{i,t})$$
$$\eta_{i,t} = Y_{i,t} - \widehat{Y_{i,t}}$$

where the predicted mean value, $\widehat{Y_{i,t}}$, uses coefficients from the regression

$$Y_{i,t} = \alpha_i + \gamma_t + \varepsilon_{i,t}$$

Intuitively, σ_i^k captures unexpected variation in k after residualizing predictable components of k, such as time-invariant firm characteristics (i.e., firm fixed-effects) and seasonality (i.e., time fixed-effects). We calculate σ_i^k using pre-experiment data and correlate this measure with post-experiment utilization rates to understand how past volatilities affect future drawdown behavior.

Volatility measures that create precautionary motives are negatively related to utilization rates. We examine the correlation between σ_i^k and three indicators of financial slack: utilization rate, and binary indicators for firms having utilization rates below the 25th and 50th percentiles.[19] Table 9 presents these findings. Column 1 indicates that a one standard deviation increase in unpredictable expenses, cash flow, and financing volatility reduces utilization rates by 1.04, 1.56, and 5.57 percent, respectively. Columns 2 and 3 show similar results, with higher volatility increasing the likelihood that a firm falls into the low utilization groups. Financing volatility has the strongest negative correlation with utilization rates, suggesting that uncertainty in funding can incentivize firms to hold large financial buffers.

Our findings remain robust even after controlling for firm size and other potential explanations for low utilization. In Columns 4 to 6, we include various measures of firm size (e.g., log of expenses, sales, and total debt) to account for the possibility that volatility may scale with size. We also consider the potential influence of frequent lender-initiated limit increases, which might 'encourage' firms to manage their

[19] P25 and p50 groups had average utilization rates of 13 and 24 percent, respectively.

liquidity more actively as borrowers may interpret the lender-initiated increase in borrowing limit as a positive signal on their growth potential (Inderst and Mueller, 2006). To capture this, we control for whether a firm received limit increases more frequently (i.e., above the 50th percentile in frequency). Additionally, we account for the possibility that older firms or owners, with more experience, have learned to optimize credit line usage over time by including a variable for whether a firm or its owner is above the median age.[20] Columns 4 through 6 demonstrate that, even after considering firm size, frequent limit increases, and experience, higher volatilities remain negatively correlated with utilization rates. This suggests that precautionary motives play a significant role in preserving financial flexibility.

Overall, the finding that seemingly unconstrained firms exhibit meaningful real effects despite not borrowing heavily on expanded debt capacity suggests that capacity expansion provides significant insurance against financial distress and volatility in day-to-day business operations. Figure A.2d shows that more than 60 percent of firms in our sample faced the possibility of exhausting their debt limits at least once before the intervention, even though they – on average– hold a substantial financial buffer. Thus, firms' precautionary motives appear to be well-justified.[21]

6 Conclusion

Contrary to the widespread notion that small firms struggle to secure sufficient external funds at reasonable costs, most firms have substantial borrowing power. Through a field experiment that randomly increased debt capacity for some firms but not for others with otherwise similar characteristics, we document a significant and persistent

[20] See Figure A.9 for heterogeneous treatment effects by these alternatives. Another possibility is that higher capacity allowed firms to make large investments that they couldn't have afforded before with existing debt capacity. To assess this possibility, we examine the probability of firms making lumpy investments and decompose the 12-month treatment effects by lumpy and non-lumpy investments. We define "lumpy" investment as an indicator that equals one if the maximum spending using term debt (i.e., debt earmarked for capital expenses) in the first 12 months of the experiment is greater than the firm's unused debt capacity at the onset. Table A.13 shows that while the treatment assignment increases the likelihood of firms making lumpy investments by 4.3 ppt (Col 1), 74 percent of ITT (585/795) effect is driven by non-lumpy spending, indicating a limited role of the lumpy investment channel.

[21] Figure A.8 shows a monotonic increase in drawdown response by the number of times firms faced binding constraints before the experiment. Treatment effects are 140 percent larger (0.21 vs. 0.51) for firms that faced binding constraints more than eight times relative to those that did not have any borrowing before the intervention in the short run and 265 percent larger (0.2 vs. 0.73) in the long run.

increase in drawdowns. Capacity expansions had significant financing and real effects at the firm level, with treated firms increasing their total debt beyond our bank's credit lines and allocating this capital to productive investment to boost sales. The long-term sales growth is present even for firms that initially had debt levels well below their capacity.

Our findings are consistent with precautionary debt capacity shaping firm growth. We corroborate this interpretation by showing that (1) capacity expansion materially reduces distress probability when firms experience a negative cash flow shock and (2) through a cross-sectional analysis that correlates unpredictable expense, cash flow, and financing volatilities with utilization rates. Even after controlling for firm characteristics and other potential explanations for low utilization, volatility measures that proxy for precautionary motives are negatively related to utilization rates. Financing volatility has the strongest negative correlation with utilization rates, suggesting that uncertainty in funding environment can create strong precautionary motives.

The key takeaway is that precautionary debt capacity offers a new explanation for why financing limits firm growth, even for firms that appear financially sound. The fact that seemingly unconstrained firms benefit from debt capacity expansion indicates that firms may have been forgoing growth opportunities to stay below debt limits before the intervention. Because precautionary motives can inhibit firm growth, policies aimed at strengthening the firms sector could bolster economic growth by targeting seemingly unconstrained firms. Our economic interpretation aligns with Modigliani and Miller (1963, p. 442), which noted that "*the need for preserving flexibility, will normally imply the maintenance by the firm of a substantial reserve of untapped borrowing power*." It also complements recent survey findings that more than 70 percent of small firms cited the need to maintain financial flexibility to avoid financial distress during downturns (Graham, 2022).

Although we have provided clean, well-identified experimental evidence that precautionary motives to maintain a financial buffer inhibit firm growth, some caution is warranted in interpreting our results. First, even though our long-run estimates have a "causal" interpretation in the sense that these long-run responses would not have materialized in the absence of the initial exogenous capacity increase, we cannot pinpoint

specific factors that led to improved firm performance in the long run, as long-run effects reflect various margins that firms adjusted over time (e.g., investment, other debt, etc.). Second, the firms investigated in this study are established firms in good financial standing that are well-poised to grow. The extent to which precautionary debt capacity matters for firms already in financial distress or new firms establishing access to capital for the first time is an open question.

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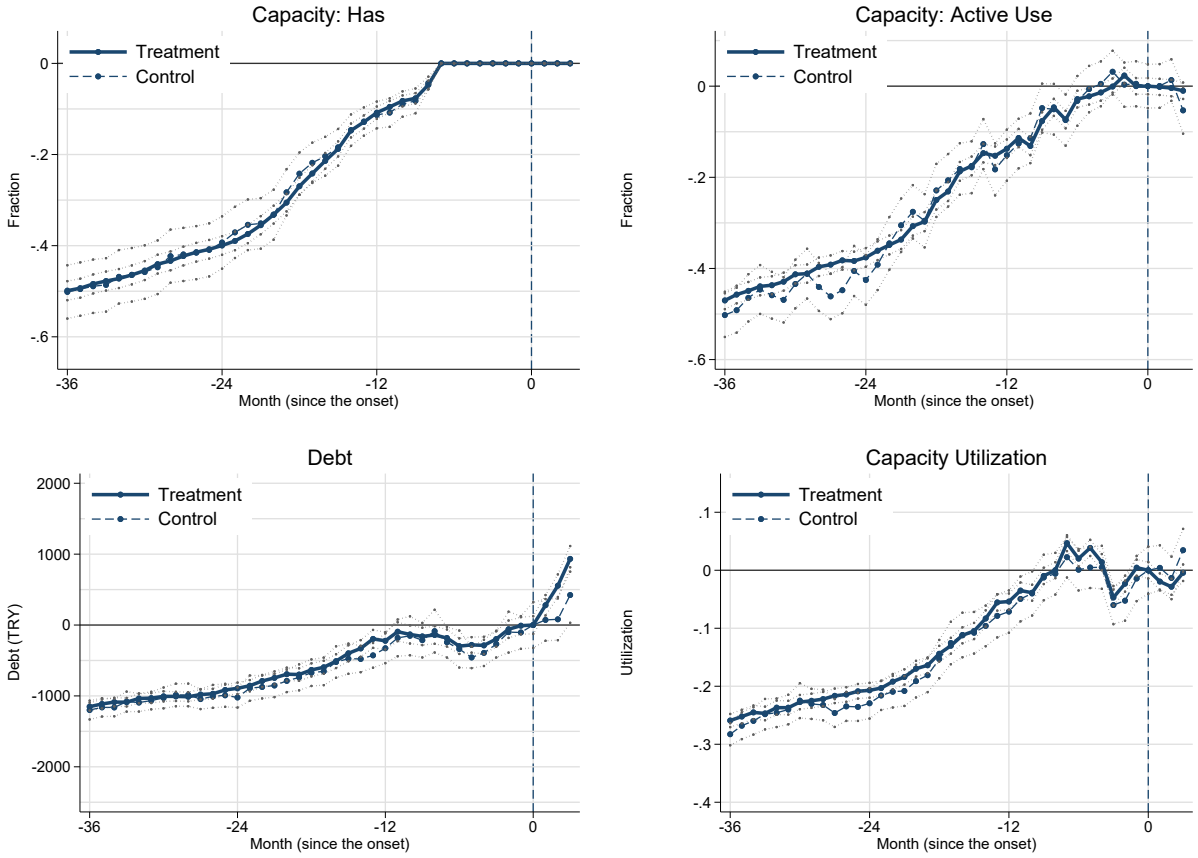
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Figure 1: Experimental Timeline

Selection	→	**Randomization**	→	**Intervention**
Participants (N=3,169) pre-approved for increase ΔC_i		$\mathbb{Z}_i \in \{0, 1\}$		$\mathbb{Z}_i = 1$: Treatment (N=2,414) pushed ΔC_i $\mathbb{Z}_i = 0$: Control (N=755) withheld from ΔC_i

Note. This figure summarizes our randomization timeline.

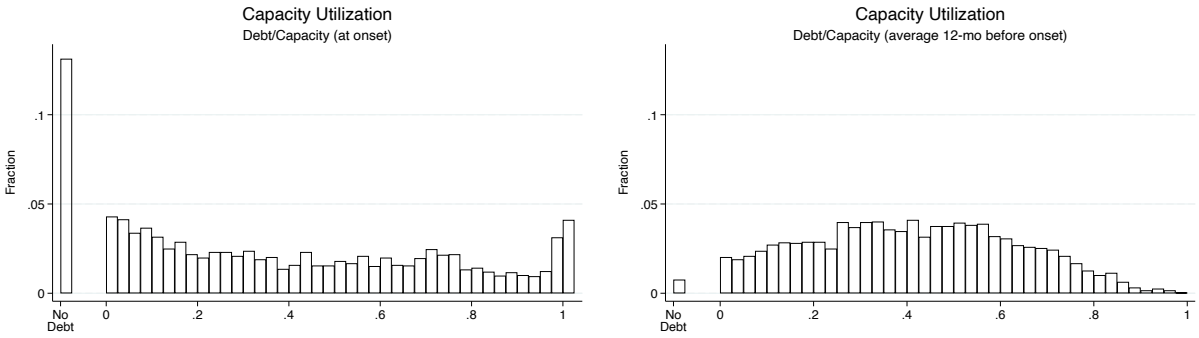
Figure 2: Covariate Balance Pre-trends



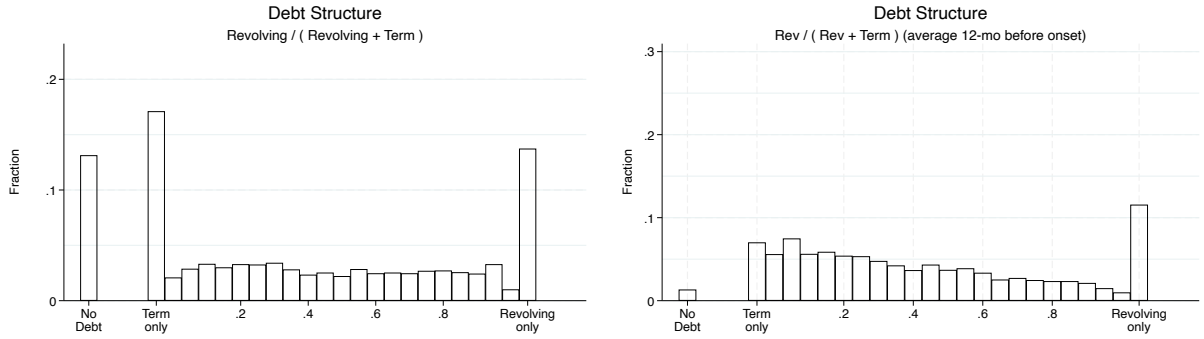
Note. Figures provide visual assessment of pre-trends by plotting covariates for treated and control group $\mathbb{Z}_i$ over 36-months preceding the experiment. The blue dashed line denotes the start date of the experiment. The y-axis is normalized to have levels equal to zero at the onset of the experiment. Dashed lines indicate 95% confidence intervals for the estimate of the mean.

Figure 3: Pre-Experimental Capacity Utilization and Financing Choice

(a) Histogram of Capacity Utilization



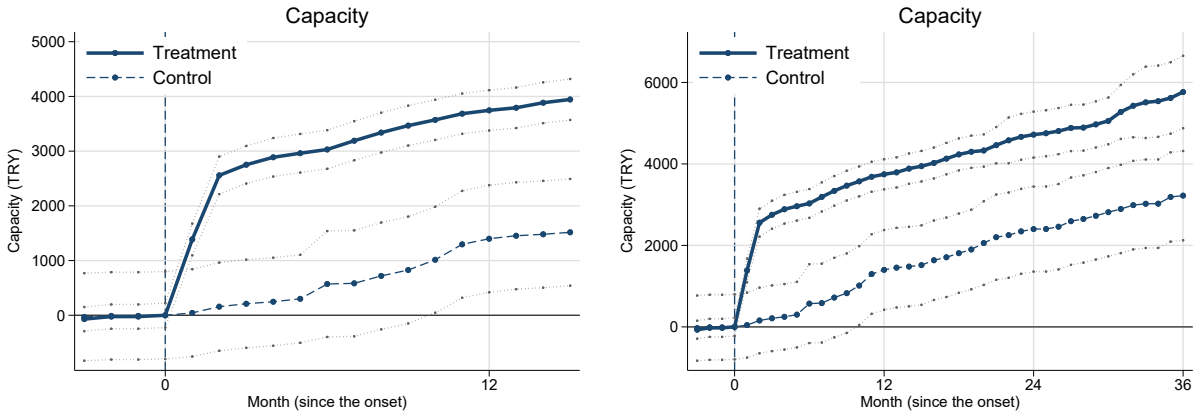
(b) Histogram of Debt Structure



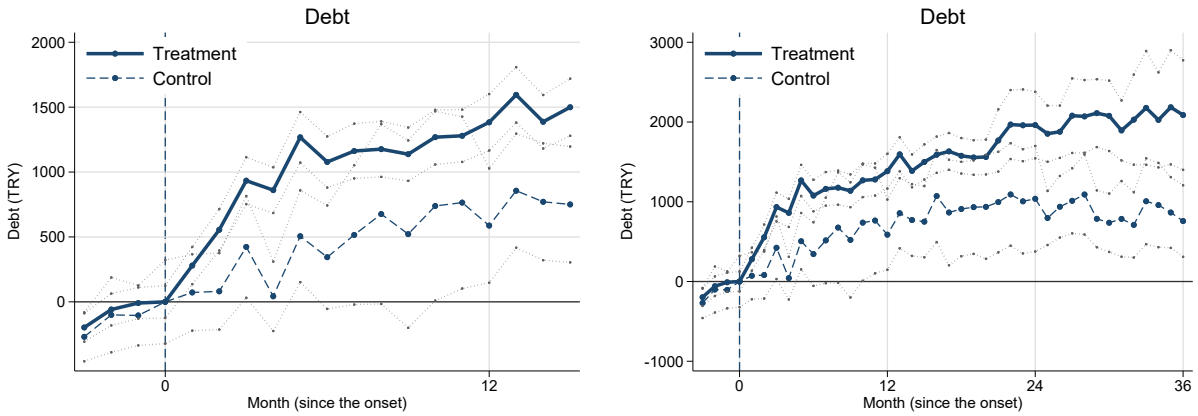
Note. This figure plots the histogram of pre-experimental capacity utilization and debt structure. In each panel, the left chart plots histogram at the onset (i.e., month before the intervention) and the right chart plots outcomes averaged over the 12-month period before the intervention. In both panels, the left-most bar shows the share of businesses with no outstanding debt. Figure 3a plots the histogram of utilization rate, measured as the outstanding debt balances divided by debt capacity. Figure 3b plots the the distribution of revolving debt as a share of the total debt among businesses with outstanding debt. If the share is 0 (1), all outstanding debt is drawn in the form of term debts (revolving debt).

Figure 4: Effects on Debt Capacity and Credit Line Debt

(a) First-stage: Short-run and Long-run



(b) Intent-to-Treat: Short-Run and Long-Run



Note. Panel 4a plots the evolution of debt capacity since the onset and Panel 4b plots that of credit line debt. The y-axis is normalized to have levels equal to zero at the onset of the experiment. Dashed lines indicate 95% confidence intervals for the estimate of the mean.

Table 1: Summary Statistics

	N (1)	Mean (2)	SD (3)	p10 (4)	p25 (5)	p50 (6)	p75 (7)	p90 (8)
				A. Credit Line				
Capacity (TRY)	3,169	4,662	5,374	800	1,250	3,000	5,700	10,700
Debt (TRY)	3,169	1,569	2,626	0	190	784	1,891	3,690
Revolving	3,169	684	1,643	0	0	86	778	1,703
Term	3,169	885	1,845	0	0	243	977	2,430
Debt-to-Capacity	3,169	0.39	0.33	0.00	0.07	0.33	0.69	0.91
Unused Capacity (TRY)	3,169	3,100	4,420	137	500	1,500	3,802	7,399
Transactions (TRY)	3,169	1,050	1,968	0	77	389	1,163	2,754
Revolving	3,169	817	1,678	0	29	276	950	2,082
Term	3,169	233	914	0	0	0	0	568
				B. Firm & Owner				
Has a Liquid Assets?	3,169	0.59	0.49	0	0	1	1	1
Cash Balances (TRY)	1,883	1,248	4,340	0	0	2	180	1,613
Has Other Debt?	3,169	0.61	0.49	0	0	1	1	1
Total Debt (TRY)†	3,169	13,329	18,400	102	828	5,924	16,652	44,352
Sales (TRY)*	551	24,416	18,356	10,742	12,750	17,724	26,950	47,737
Profits (TRY)*	551	17,565	12,910	8,412	9,678	12,675	19,269	35,196
Owner's Age	3,169	39	10	27	31	37	46	54
Firm Age (5 years+)	3,169	0.41	0.49	0	0	0	1	1

Note. Statistics based on the month before the experiment. Nominal variables expressed in Turkish Lira (TRY). Statistics represent raw distribution not adjusted by strata. All variables are measured in monthly frequency, except for †and *, which denote quarterly and annual frequency.

Table 2: Covariate Balance

	All Sample			Firms with Sales		
	Control Mean (1)	Z (2)	p-value (3)	Control Mean (4)	Z (5)	p-value (6)
Capacity	4,934	-452	0.29	3,353	523	0.30
		(423)			(504)	
Debt	1,650	-53	0.76	1,471	-52	0.88
		(176)			(344)	
Revolving	600	76	0.21	547	42	0.75
		(61)			(129)	
Term	1,050	-129	0.41	924	-93	0.77
		(158)			(326)	
Utilization	0.40	0.00	1.00	0.41	0.02	0.72
		(0.02)			(0.05)	
Exclusive Banking	0.74	0.00	0.93	0.59	-0.01	0.93
		(0.03)			(0.07)	
Owner's Age	38	0	0.88	33	1	0.44
		(1)			(1)	
Total Debt*	10,829	649	0.47	10,879	-474	0.86
		(898)			(2,696)	
Has Other Debt*	0.59	-0.04	0.16	0.67	-0.05	0.51
		(0.03)			(0.08)	
Sales*	23,481	1,595	0.54	23,481	1,595	0.54
		(2,582)			(2,582)	
Profit*	16,604	1,637	0.37	16,604	1,637	0.37
		(1,827)			(1,827)	
Sample Size	755	2,414	3,169	119	432	551

Note. Column 1 and 4 report sample means using data from the onset of the experiment or the last available data point before the experiment (*). Columns 2 and 5 report treatment-control difference. Columns 3 and 6 report p-values for the difference. Nominal variables are expressed in TRY. The first column uses all 3,169 participants and the last three restricts the sample to 551 firms with sales information.

Table 3: Effects on Capacity and Debt

	Baseline		Short-Run			Long-Run		
	Level		*3m*	*6m*	*12m*	*Y1*	*Y2*	*Y3*
	(1)		(2)	(3)	(4)	(5)	(6)	(7)
			Panel A. Capacity and Debt					
Capacity	4,662	First Stage	2,562	2,480	2,351	2,346	2,318	2,543
(TRY)			(122)	(206)	(240)	(244)	(387)	(546)
Debt	1,569	Intent-to-treat	538	770	820	785	902	1,296
(TRY)			(201)	(216)	(280)	(290)	(426)	(423)
		Treatment Effect	0.21	0.31	0.35	0.30	0.44	0.55
			(0.08)	(0.09)	(0.11)	(0.13)	(0.18)	(0.16)
			Panel B. Types of Debt					
Revolving	684	Intent-to-Treat	149	287	343	327	140	2
(TRY)			(131)	(97)	(118)	(124)	(345)	(193)
		Treatment Effect	0.06	0.12	0.15	0.12	0.07	-0.03
			(0.05)	(0.04)	(0.05)	(0.05)	(0.13)	(0.08)
Term	885	Intent-to-Treat	389	483	477	457	762	1,294
(TRY)			(130)	(170)	(229)	(236)	(231)	(330)
		Treatment Effect	0.15	0.19	0.20	0.18	0.37	0.57
			(0.05)	(0.07)	(0.09)	(0.10)	(0.11)	(0.16)
		Revolving Share	0.28	0.37	0.42	0.42	0.16	0.00

Note. This table presents first-stage (FS), intent-to-treat (ITT), and treatment effects (TE) over different time horizons after the onset of the experiment. Panel A reports results on capacity and debt. Panel B decomposes credit line debt into its revolving and term subcomponents. Column 1 reports the pre-experiment mean of the outcome variables. Columns 2-4 report short-run estimates obtained from Equation (1). Columns 5-7 report long-run estimates obtained from Equation (2) collapsing data to quarterly-level. FS and ITT capture cumulative effects since the onset of the experiment and are shown in Turkish Lira (TRY). The Wald estimator of TE is calculated as the ratio of FS to ITT estimates and captures the change in Y per dollar (lira) increase in debt capacity. Revolving share is defined as the ratio of revolving debt to the sum of revolving and term debt. Robust standard errors clustered at the firm level and reported in parentheses.

Table 4: Effects on Total Debt and Cash Balances

	Baseline	Intent-to-Treat			Treatment Effect		
	Level	*Y1*	*Y2*	*Y3*	*Y1*	*Y2*	*Y3*
TRY	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total Debt	13,329	2,764	4,446	5,089	1.14	1.75	2.02
		(898)	(1,278)	(1,347)	(0.36)	(0.55)	(0.61)
Cash Balances	1,248	-56	-216	–	-0.02	-0.078	–
		(586)	(334)		(0.22)	(0.12)	

Note. This table presents intent-to-treat (ITT) and treatment effects (TE) on firms' total debt and cash balances over different time horizons after the onset of the experiment. Estimates are obtained from running Equation 4, where the treatment assignment Z is weighted by each firm's exposure to the experiment, measured as the share of credit line debt capacity to total debt. Column 1 reports the pre-experiment mean of the outcome variables. Total debt is available at quarterly intervals but is not available beyond the first quarter of Y3, so Y3 refers to cumulative debt up until that point. Cash balances refer to firms' liquid cash balances on business checking account. Cash balances are available at monthly intervals and is not available beyond Y2. Robust standard errors clustered at the firm level and reported in parentheses.

Table 5: Effects on Investment and Other Business Expenses

	Baseline	Intent-to-Treat			Treatment Effect		
	Level	*Y1*	*Y2*	*Y3*	*Y1*	*Y2*	*Y3*
TRY	(1)	(2)	(3)	(4)	(5)	(6)	(6)
Business Services	673	994	2,215	3,598	0.38	0.84	1.50
		(275)	(530)	(831)	(0.09)	(0.18)	(0.34)
Investment	3,121	2,607	3,941	4,402	0.99	1.49	1.83
		(629)	(1,113)	(1,772)	(0.24)	(0.41)	(0.73)
Operating Expense	5,313	3,654	7,110	10,983	1.39	2.69	4.57
		(1,205)	(2,461)	(3,676)	(0.41)	(0.89)	(1.43)

Note. This table presents intent-to-treat (ITT) and treatment effects (TE) on firm expenses over different time horizons after the onset of the experiment. Estimates are obtained from running Equation 4, where the treatment assignment Z is weighted by each firm's exposure to the experiment, measured as the share of credit line debt capacity to total debt. Column 1 reports the pre-experiment mean of the outcome variables. Business services capture expenses on professional services such as advertising, consulting, marketing, IT, security, etc. Investment captures capital expenses on durable equipment or machinery. Operating expenses include all other expenses. Robust standard errors clustered at the firm level and reported in parentheses.

Table 6: Effects on Sales and Profits

	Baseline	Intent-to-Treat			Treatment Effect		
	Level	*Y1*	*Y2*	*Y3*	*Y1*	*Y2*	*Y3*
TRY	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sales	24,416	8,934	17,951	25,482	3.33	5.30	7.19
		(3,300)	(7,340)	(12,825)	(1.15)	(1.86)	(3.21)
Profits	17,565	4,191	8,584	13,807	1.56	2.53	3.90
		(2,209)	(4,863)	(8,510)	(0.80)	(1.32)	(2.27)

Note. This table presents intent-to-treat (ITT) and treatment effects (TE) on firm sales and profits over different time horizons after the onset of the experiment. Estimates are obtained from running Equation 4, where the treatment assignment Z is weighted by each firm's exposure to the experiment, measured as the share of credit line debt capacity to total debt. Column 1 reports the pre-experiment mean of the outcome variables. Sales and profits are available at fiscal year intervals and may not align precisely with the timing of the experiment. 'Y1' refers to the first data point available within the first year of the experiment, and subsequent 'Y' labels correspond to outcomes after each respective fiscal year. These outcomes are reported for 551 firms. Robust standard errors clustered at the firm level and reported in parentheses.

Table 7: Effects on Firms with Low Baseline Utilization

	Baseline	Intent-to-Treat			Treatment Effect		
TRY	Level	*Y1*	*Y2*	*Y3*	*Y1*	*Y2*	*Y3*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Below p50 Utilization (<0.41); Mean Util = 0.23						
Debt	1,028	567	644	824	0.21	0.28	0.39
		(514)	(682)	(448)	(0.18)	(0.29)	(0.19)
Sales	26,569	16,287	33,398	46,463	4.50	7.12	9.01
		(4,930)	(10,873)	(19,582)	(1.37)	(2.04)	(3.65)
Biz Expense	9,632	4,783	9,249	12,366	1.77	3.63	5.46
		(2,267)	(4,280)	(7,300)	(0.71)	(1.50)	(2.87)

Note. This table presents intent-to-treat (ITT) and treatment effects on credit line debt, sales, and business expenses over different time horizons after the onset of the experiment. The sample is restricted to firms with below median (0.41) utilization rate over the 12-month period before the experiment. Estimates are obtained using the same procedure as in Tables 3, 6, and 5. Table A.11 reports estimates for high utilization sample. Robust standard errors clustered at the firm level and reported in parentheses.

Table 8: Financial Distress: Non-Performing Loan and Renegotiation

	Distress (1)	*NPL* (2)	*Renegotiation* (3)	*Distress* (4)	*NPL* (5)	*Renegotiation* (6)
Z	-0.85	-0.61	-1.34	-0.36	-0.21	-0.70
	(0.25)	(0.23)	(0.23)	(0.22)	(0.21)	(0.20)
1(CF Shock)				7.60	6.51	7.25
				(1.63)	(1.61)	(1.63)
1(CF Shock) X Z				-5.03	-4.12	-6.59
				(1.70)	(1.67)	(1.67)
Outcome Mean (%)	3.22	2.59	2.71	3.22	2.59	2.71

Note. This table analyzes whether an expansion in debt capacity reduces financial distress for firms facing a negative cash flow shock. Since the cash flow shock is defined based on sales, the sample is limited to firms with available sales data. The dependent variables measure whether a firm has non-performing credit ("NPL"), undergoing renegotiation ("Renegotiation"), or both ("Distress"). A credit account is considered to be non-performing if payments have not been made for 90 days or more. All outcome variables are multiplied by 100, and the percent of firm-month in distress for the control group is reported in the row labeled "Outcome Mean." $\mathbb{Z}$ represents the treatment assignment. Cash flow shock is defined as a binary variable equal to 1 in years when a firm experiences a significant, unexpected reduction in sales. Unexpected changes in sales are measured as the year-over-year change in sales, adjusted for seasonality and firm-specific, time-invariant factors. A large cash flow shock occurs when the reduction in sales exceeds one standard deviation of the unexpected sales change. All regressions control for strata and include time fixed effects.

Table 9: Drivers of Utilization

	No Controls			Controls		
	Util	*1(Util <p25)*	*1(Util <p50)*	*Util*	*1(Util <p25)*	*1(Util <p50)*
	(1)	(2)	(3)	(4)	(5)	(6)
			Precautionary Motives			
$\sigma^{Expense}$	-1.04	0.02	0.04	-1.33	0.03	0.04
	(0.26)	(0.01)	(0.01)	(0.25)	(0.01)	(0.01)
$\sigma^{Cashflow}$	-1.56	0.06	0.01	-1.00	0.06	0.02
	(0.22)	(0.00)	(0.01)	(0.23)	(0.00)	(0.01)
$\sigma^{Financing}$	-5.57	0.14	0.12	-5.03	0.125	0.117
	(0.21)	(0.00)	(0.01)	(0.21)	(0.00)	(0.01)
			Firm & Owner Characteristics			
ln(Expense)				9.93	-0.11	-0.03
				(0.41)	(0.01)	(0.01)
ln(Sales)				-4.83	0.02	-0.01
				(0.34)	(0.01)	(0.01)
ln(Total Debt)				3.739	0.02	-0.15
				(0.30)	(0.01)	(0.01)
1(Freq Limit Ch)				-0.27	-0.03	0.02
				(0.23)	(0.00)	(0.01)
1(Firm Old)				-0.74	0.01	0.00
				(0.23)	(0.01)	(0.01)
1(Owner Old)				-0.85	0.00	0.02
				(0.21)	(0.00)	(0.01)
R2	0.088	0.123	0.073	0.204	0.164	0.136

Note. This table presents a cross-sectional analysis of the factors influencing firms' utilization rates. The analysis is limited to the control group. The dependent variables include the utilization rate (multiplied by 100) and indicators for whether a firm's utilization rate falls below the 25th and 50th percentiles in the post-experiment period. All explanatory variables are measured using data from the pre-experiment period. Precautionary motives are defined as unexpected volatility in expenses, sales, and financing, after adjusting for seasonality and firm-specific, time-invariant characteristics. σ^x represents the standard deviation of residuals from a regression of a firm's X (e.g., cash flows) on time dummies and firm fixed effects. Firm characteristics include measures of size (log of expenses, sales, and total debt), the frequency of credit line limit increases before the experiment, and whether the firm or its owner is above the median age. The explanatory variables are standardized and therefore the estimates can be interpreted as the implied change in the outcome for a one standard deviation change in the explanatory variable.

A Evaluating Non-Experimental Methods

To what extent does randomization help us document accurate financing effects? Randomized control trials (RCTs) are considered the "gold standard" for causal inference (Floyd and List, 2016), but the extent to which it helps to address identification challenges that financing is not independent of investment opportunities and their correlated confounds is not well-understood.

We demonstrate that randomization mitigates downward bias when estimating financing effects. In the spirit of Lalonde (1986) and Angrist et al. (2015), we compare treatment effects from non-experimental analysis to our experimental benchmark. Using data from three years before the intervention, we document a 12-month effect of having a higher debt capacity by comparing firms with more versus less debt capacity using observational data. Since debt capacity changes before the intervention were not randomized, naively comparing firms with more versus less debt capacity captures bias arising from correlated confounds and selection.

We report treatment effects using various covariate-controlled and selection-on-observables methods (i.e., variants of propensity score matching). Specifically, let D_i denote firms having a higher debt capacity today relative to 12 months before. We examine the effect of D_i (rather than Z_i) on debt capacity and firm drawdowns 3 years before the experiment. Non-experimental estimates should be similar to our experimental estimate of 0.35 in the absence of selection bias. We use the following methods:

1. Raw correlation

$$\rho = E[Y_1 - Y_0] + bias$$

2. Including strata fixed-effects (i.e., pre-experimental utilization bins).

$$\rho = E[Y_1 - Y_0|x] + bias$$

3. Including strata fixed-effects + firm controls

$$\rho = E[Y_1 - Y_0|\mathbf{X}] + bias$$

4. Strata matching à la Angrist (1998) that computes the average of conditional treatment

effects using the conditional probability of treatment within strata $\lambda_x = E[D|x]$:

$$\rho = E\Big\{ \frac{\lambda_x(1-\lambda_x)}{E[\lambda_x(1-\lambda_x)]}\delta_x \Big\}$$

where $\delta_x = E[Y_1 - Y_0|x]$+ bias

5. Propensity score weighting à la Hirano et al. (2003)

$$\rho = E\left[\frac{yD}{\lambda_x}|\mathbf{X}\right] - E\left[\frac{y(1-D)}{(1-\lambda_x)}|\mathbf{X}\right] + bias$$

where $\lambda_x = E[D|\mathbf{X}]$ is the conditional-on-covariates probability of treatment.

6. Hybrid weighting/reweighting procedure suggested by Kline (2011).

$$\rho = (\beta_1 - \beta_0)'E[\mathbf{X}] + bias$$

where $E[Y_0|\mathbf{X}] = E[y|\mathbf{X}, D = 0] = \mathbf{X}'\beta_0$ and $E[Y_1|\mathbf{X}] = E[y|\mathbf{X}, D = 1] = \mathbf{X}'\beta_1$.

Using non-experimental methods delivers estimates that are half as large as our experimental estimate, indicating that randomization helps to address the downward bias in estimating the financing effect. Table A.8 report the estimates. Compared to our experimental benchmark of 35 cents, non-experimental methods deliver estimates of 18 to 21 cents on the dollar. We conjecture two potential reasons for this downward bias. First, our bank might have only approved higher lines for firms that had lower marginal propensity to drawdown on expanded debt capacity and financially stable before the experiment. Second, firms that seek more financing are simply those that seek higher debt limits as a precautionary motive.

Overall, this exercise illustrates the importance of having a credible, exogenous instrument for financing. The fact that we get a substantially different estimate *for the same set of firms* illustrates that randomization effectively addresses selection bias in estimating financing effects from observational data. Instead, we illustrate that a naive comparison between firms with more versus less financing introduces a downward bias and that this bias can reduce the magnitude of the experimental benchmark by half.

B Robustness

We provide a battery of robustness checks to ensure that our main TEs are not simply driven by inflation, nature of firms we study, and balance shifting. In Panel A of Table A.9, we conduct

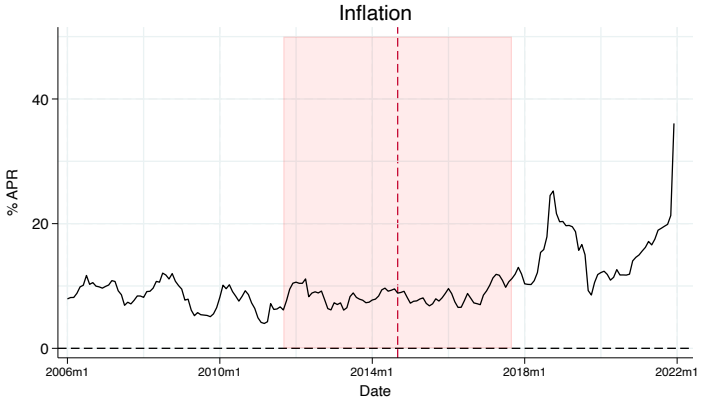
robustness tests using US dollars and real TRY to ensure that fluctuations in the exchange rate or inflation do not drive our results. The first row uses the dollar value of debt as a left-hand-side variable. The second row uses the real value of debt as a left-hand-side variable, deflated using the price deflator for personal consumption expenditures. The estimates remain quantitatively consistent compared to our main short-run estimate of 35 cents. See Table A.12 for long-run TEs in Euros and USD.

We ensure that our results are not specific to the population of firms we study and do not simply reflect a large number of very small firms exhibiting large drawdown responses. In the first row of Panel B, we reweight the sample to match the distribution of the universe of Turkish firms in terms of capacity utilization rate. In the second row, we restrict our analysis to the largest 5% of firms in our sample that look like a median firms in the economy in terms of average debt capacity. The sample re-weighting generates quantitatively similar estimates, and restricting our sample to the largest firms in the sample deliver estimates that are even larger than our baseline estimate of 35 cents.

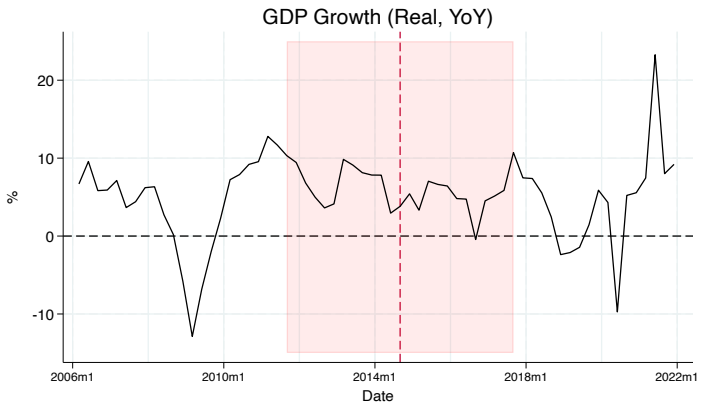
An important concern is that our estimates may capture firms' propensity to substitute financing elsewhere. In this case, the large drawdown response would reflect a substitution between financing that does not lead to real effects. However, Section 4 shows that treatment assignment leads to higher total and other debt beyond credit line debt and generate real effects. In addition, Panel C of Table A.9 further documents that our treatment effect does not simply reflect substitution across accounts. The first row reports treatment effects restricting the 74 percent of firms that do not have a bank account elsewhere, and the second row restricts the sample to 41 percent of firms that do not have a cash account at our bank. Both sample restrictions deliver estimates quantitatively similar to our baseline estimates. In the last row, we directly confirm that firms do not change their cash use using 59 percent of firms in our sample with checking accounts at our bank. Overall, our estimates do not appear to reflect balance shifting across accounts or across types of financing (cash vs. credit lines).

Figure A.1: Macroeconomic Environment in Türkiye

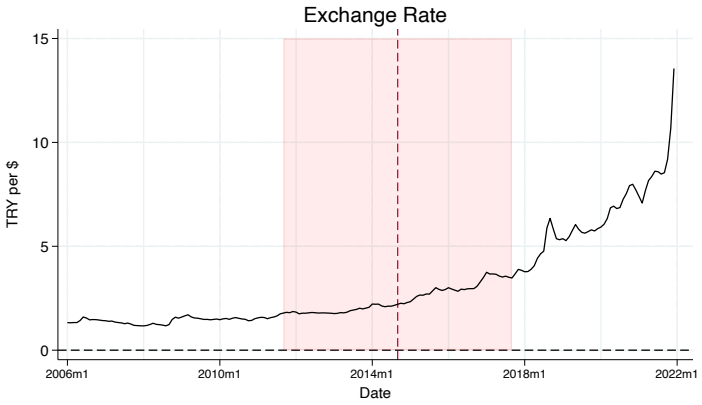
(a) Inflation



(b) Real GDP Growth (quarterly %)



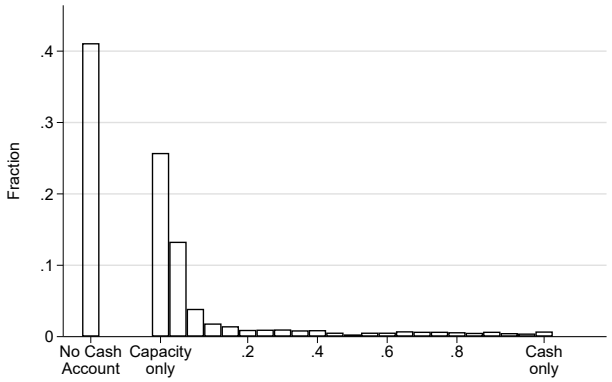
(c) USD/TRY Exchange Rate



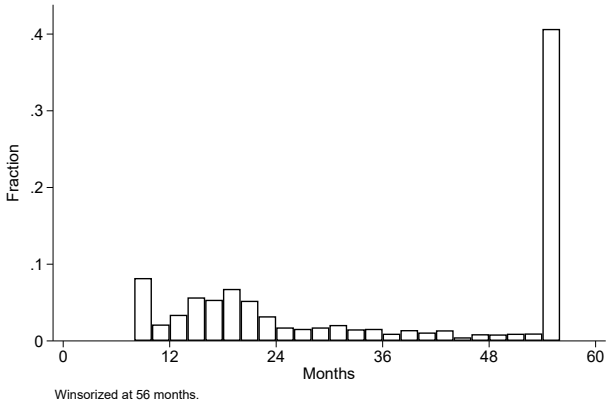
Note. Figures plot annual inflation, GDP growth, and USD/TRY exchange rate. In each panel, the dotted line denotes the onset of our experiment (2014m9), and the highlighted area in pink denotes our sample period. Source: Turkstat via Türkiye Data Monitor.

Figure A.2: Other Firm Characteristics
(Pre-Experiment)

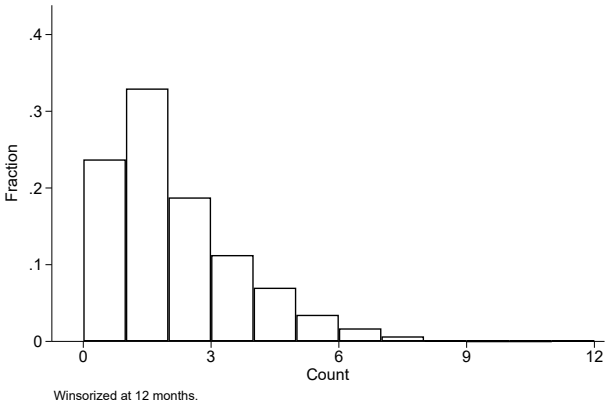
(a) Liquidity Structure:
Cash / (Cash + Unused Capacity)



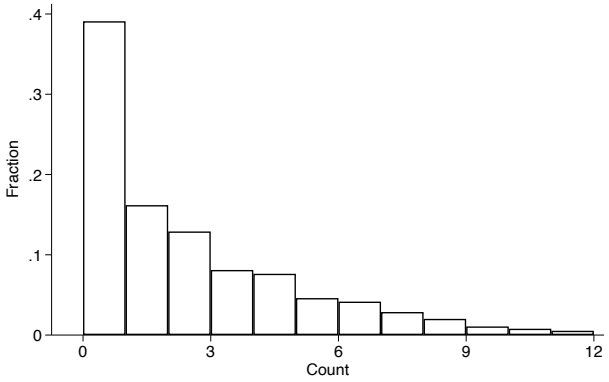
(b) Histogram of Account Age



(c) Previous Capacity Increases



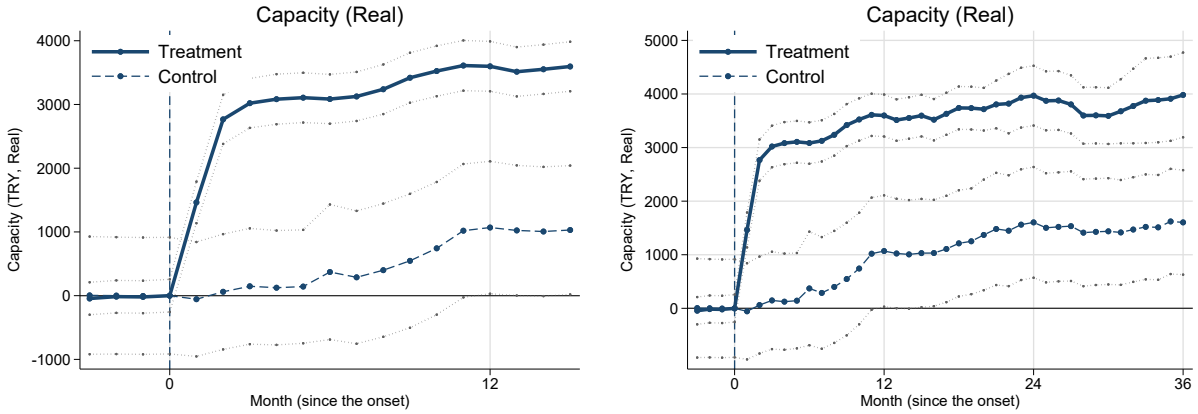
(d) Frequency of Having Utilization > 0.75



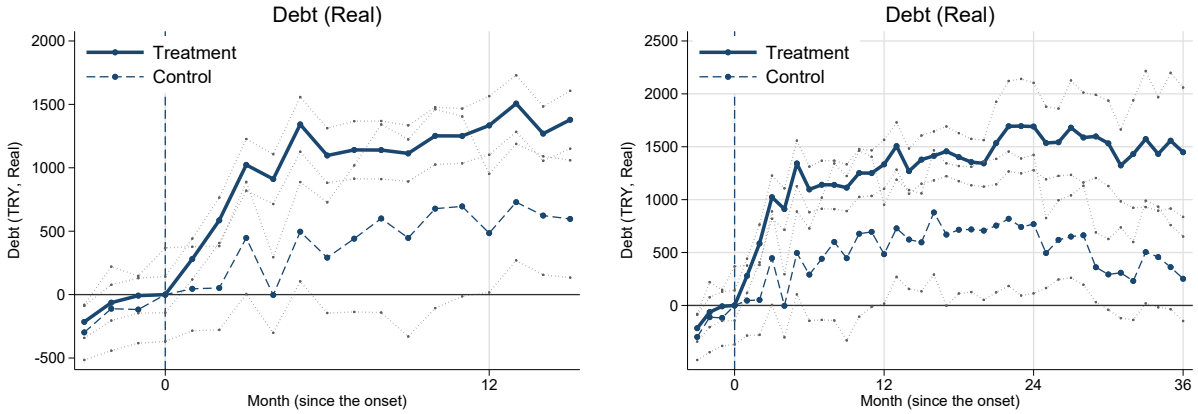
Note. Figures plot the distribution of liquidity structure, account age, frequency of capacity increases firms experienced, and number of times that firms face binding constraints before the intervention. Liquidity structure is measured as the share of cash to the sum of cash and available capacity at the onset. The left-most bar shows the share of businesses with no cash account. Account age is measured as the number of months that an account has a positive debt capacity and is truncated at the top – these firms have account age of over 60 months. Capacity increases before the experiment captures non-experimental increases in debt capacity, which captures both lender- and borrower-initiated changes. Binding constraint is measured as firms having utilization ratio greater than 75 percent.

Figure A.3: Effects on Debt Capacity and Credit Line Debt in Real TRY

(a) First-Stage: Short-Run and Long-Run



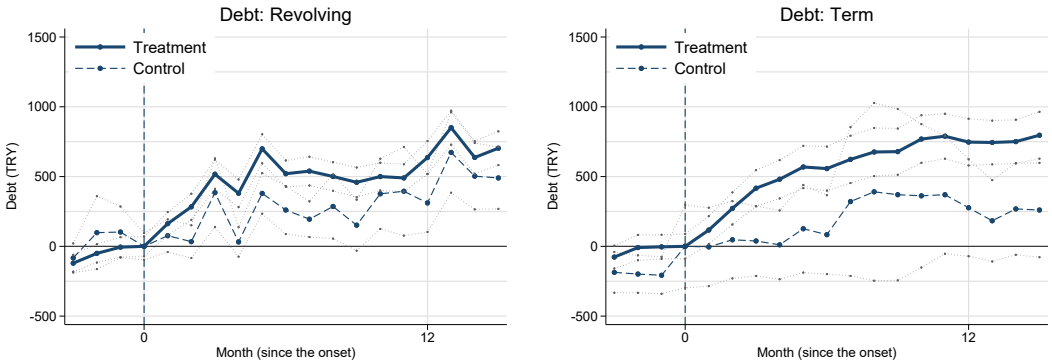
(b) Intent-to-Treat: Short-Run and Long-Run



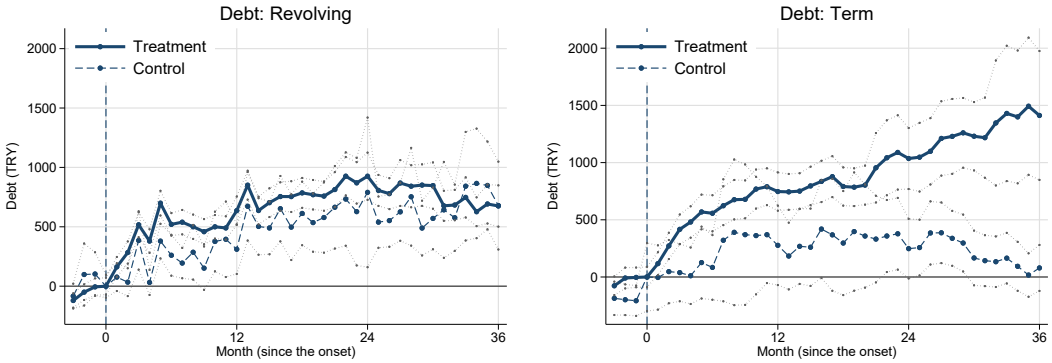
Note. In each panel, the left chart plots outcomes in nominal Turkish Lira (TRY) and the right chart plots outcomes in real TRY. Real levels are calculated using the implicit price deflator for personal consumption expenditures. Panel 4a plots short-run effects over 12 months since the onset of the experiment. Panel 4b plots long-run effects over 36 months since the onset. The y-axis is normalized to have levels equal to zero at the onset of the experiment. The blue dashed line denotes the start date of the experiment. Dashed lines indicate 95% confidence intervals for the estimate of the mean.

Figure A.4: Short-Run and Long-Run Effects on Financing Choice

(a) Intent-to-Treat: Short-Run

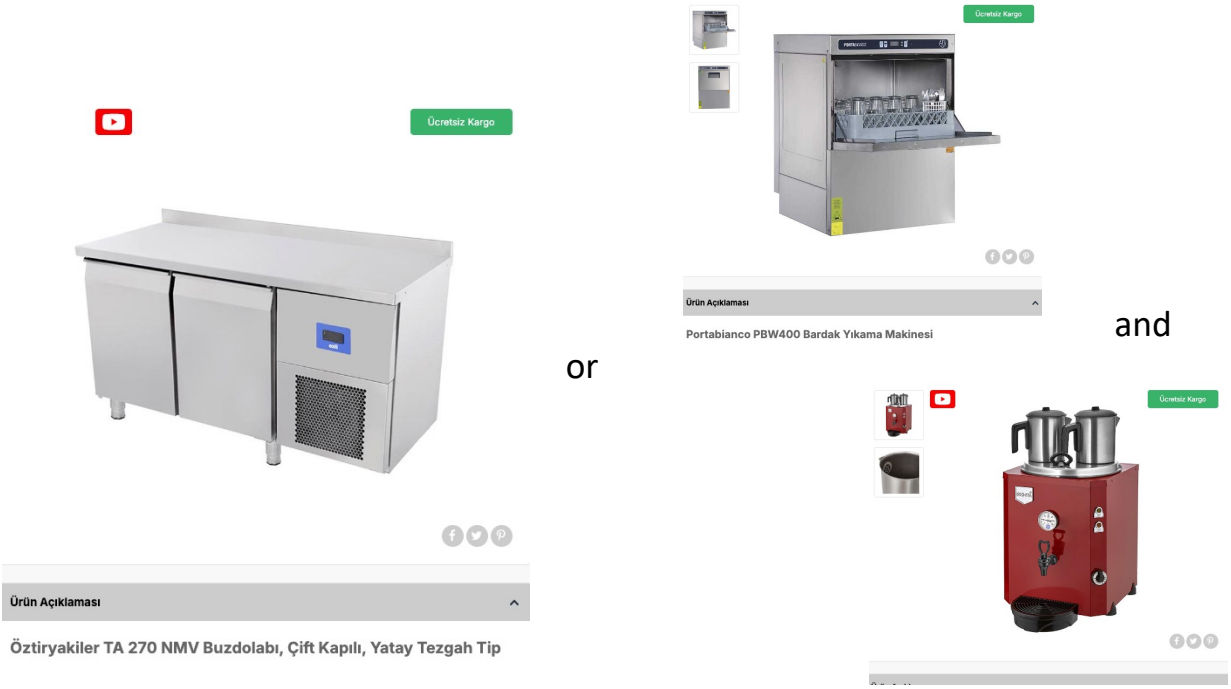


(b) Intent-to-Treat: Long-Run



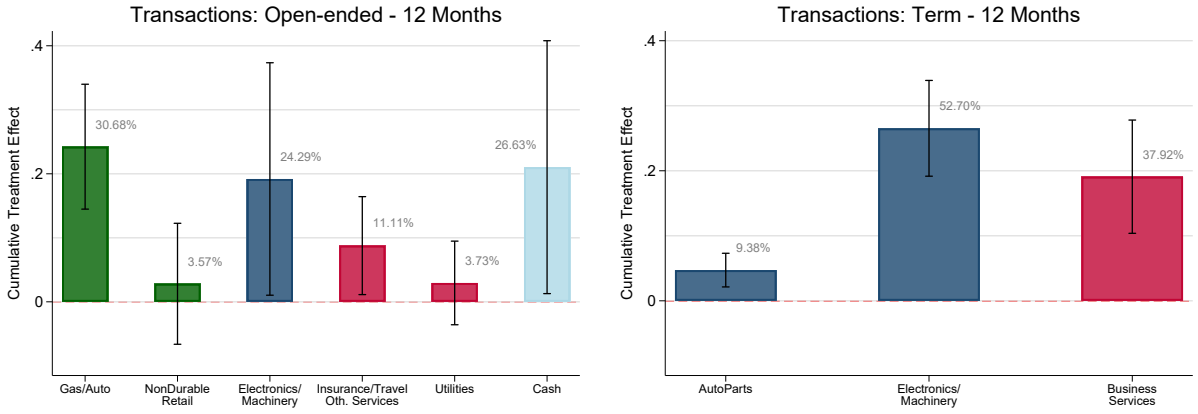
Note. Figures plot revolving and term debt by treatment status over the first 12-months (Figure A.4a) and 36-months (Figure A.4b) of the experiment. Firms can draw on the *same* credit line as revolving or term debt without additional approval process. Revolving debt is high-cost, interest-accruing debt and term debt is a point-of-sale term debt offered at 0% APR. See Section 3.2 for a detailed discussion on the types of debt. Outcomes are shown in Turkish Lira (TRY) and normalized to have levels equal to zero at the onset of the experiment. Blue dashed line denotes the onset of the experiment. Dotted lines are the 95% confidence intervals for the estimate of the mean.

Figure A.5: Examples of Firm Investment Based on Y3 Investment ITT



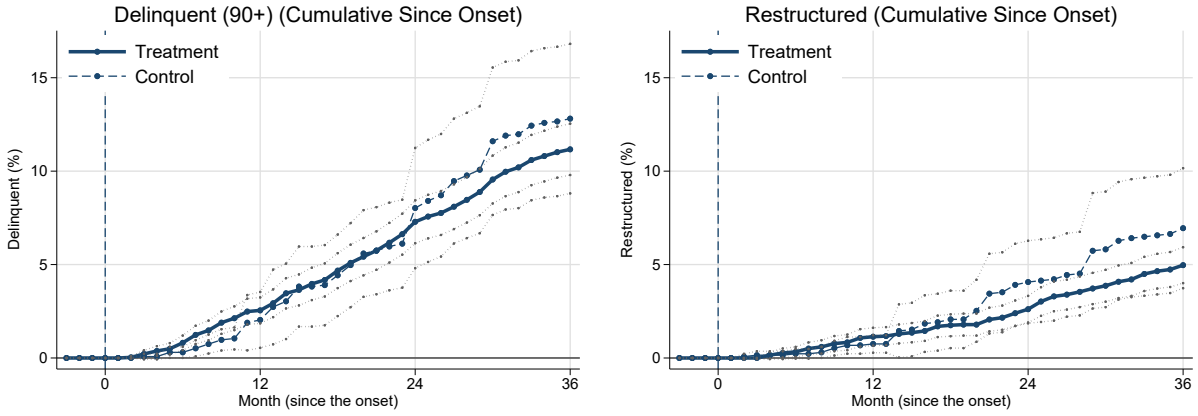
Note. This figure shows examples of durable goods one can buy with Y3 ITT investment response reported in Table 5 in 2017 TRY. The left chart shows an industrial refrigerator. The right chart shows an industrial dishwashing equipment and a jumbo tea pot.

Figure A.6: Decomposition of Spending Type by Financing Choice



Note. This figure plots 12-month treatment effects on detailed spending categories transacted using revolving and term debt. The share of spending on each spending category relative to total transactions is shown in gray. For example, the left-most bar in the left figure shows that spending on gas and auto services (e.g., auto repair, car wash, inspection, etc) represents 31 percent of total transactions incurred using revolving debt. Spending on non-durable goods are shown in green; durable goods in dark blue; services in red; and cash advance in a light blue bar. term debt only captures spending on durable goods and business services, as regulations in Türkiye prohibit the use of term debt to finance strict nondurable goods. See Table A.2 for examples of transactions included in each spending category. Whiskers show 95 percent confidence intervals.

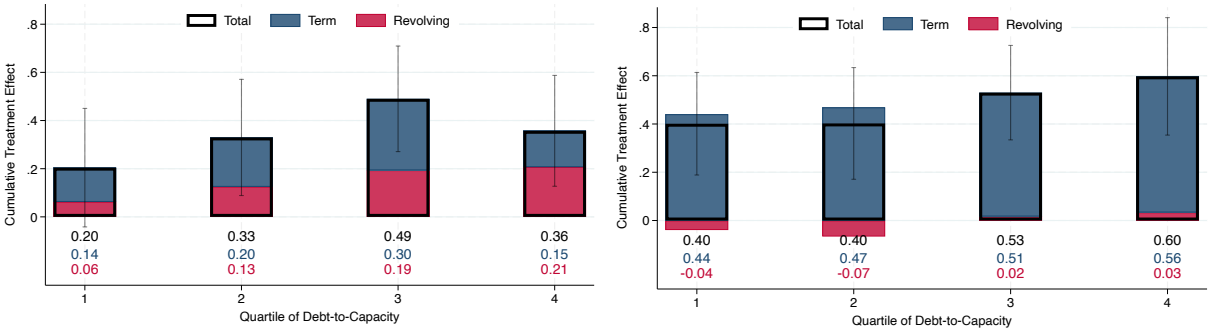
Figure A.7: Event Study: Share of Firms in Distress and Renegotiation



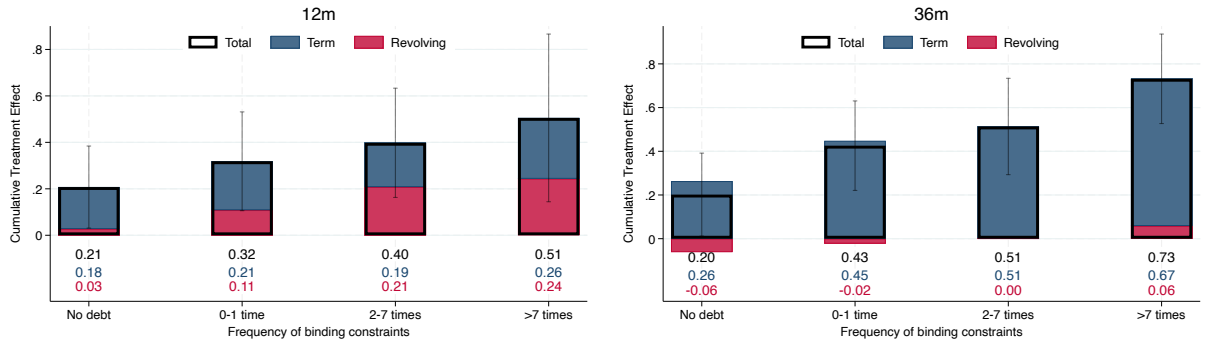
Note. Figures plot the share of businesses with non-performing (i.e., "delinquent") loans and with restructured debt by treatment status over 36 months since the onset of the experiment. A credit account is considered to be non-performing if payments have not been made for 90 days or more. Outcomes are normalized to have levels equal to zero at the onset of the experiment. Blue dashed line denotes the onset of the experiment. Dotted lines denote 95% confidence intervals.

Figure A.8: Heterogeneous Treatment Effects:
Utilization and Sector

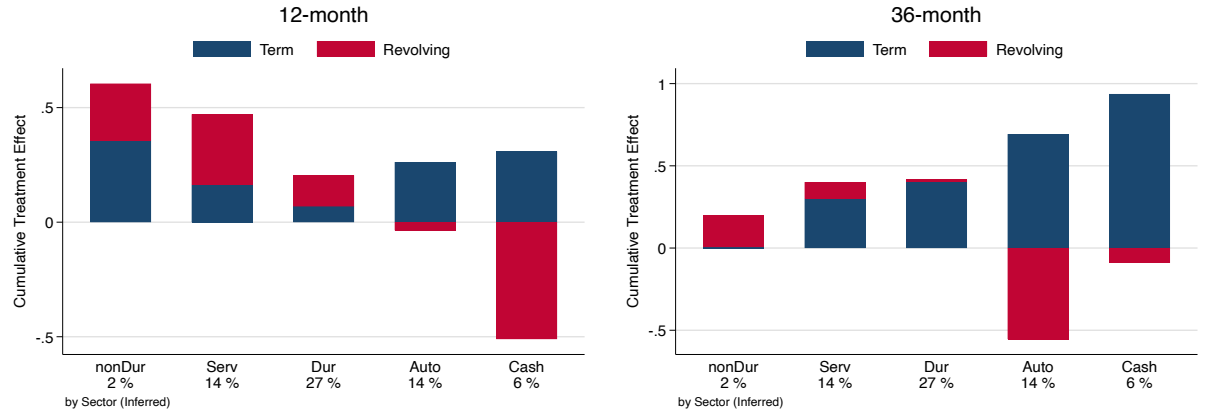
(a) By Baseline Utilization Rate



(b) By the Frequency of Firms Facing Binding Constraints



(c) By Sector



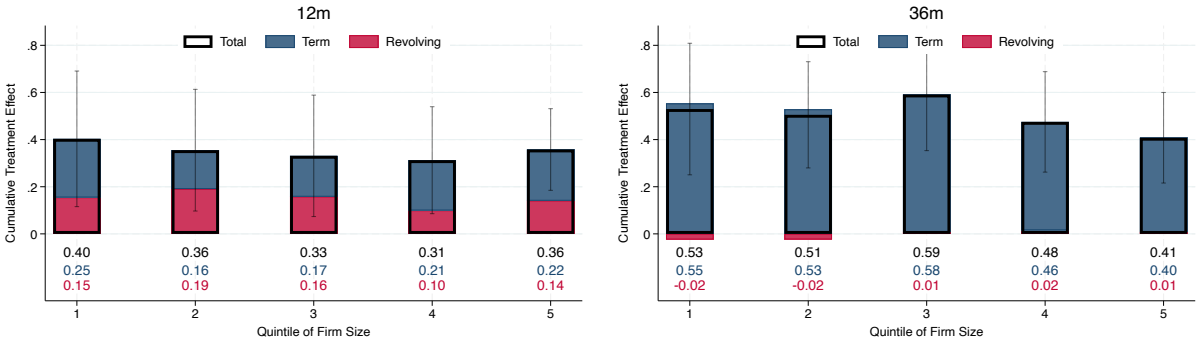
Note. This figure decomposes heterogeneous treatment effects on total credit line debt (denoted "Total") by revolving (red) and term (blue) using Equation

$$Y_i = \sum_{k=1}^{K=n} \psi_{jk} \cdot X_{ij} \times f_k \; + f_s + \epsilon_i \tag{5}$$

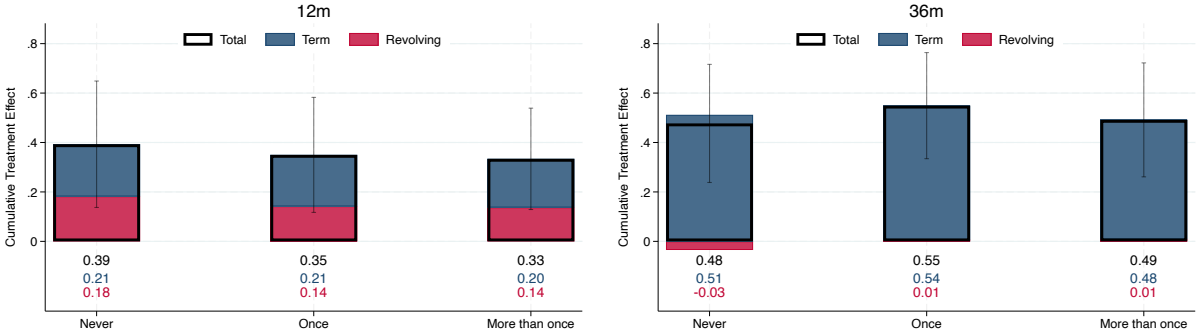
where k denotes a bin and f_k stands for bin fixed effects. Figure A.8a plots heterogeneity by quartiles of baseline (i.e., 12-month period before the intervention) utilization rates. Figure A.8b plots heterogeneity by baseline the frequency of firms face binding financial constraints (i.e., utilization rate > 0.75). These groups are firms that: (1) don't carry balance across periods; (2) hit binding constraints at least once or less; (3) hit binding constraints 2-7 times; (4) more than 8 times. Figure A.8c reports these treatment effects by *inferred* sector based on firm's spending patterns. If a firm spent more than 50 percent of their spending on a specific category on average in the year before the experiment, we assign firms to this sector. We display the fraction of firms in each category below the label.

Figure A.9: Heterogeneous Treatment Effects: Alternatives

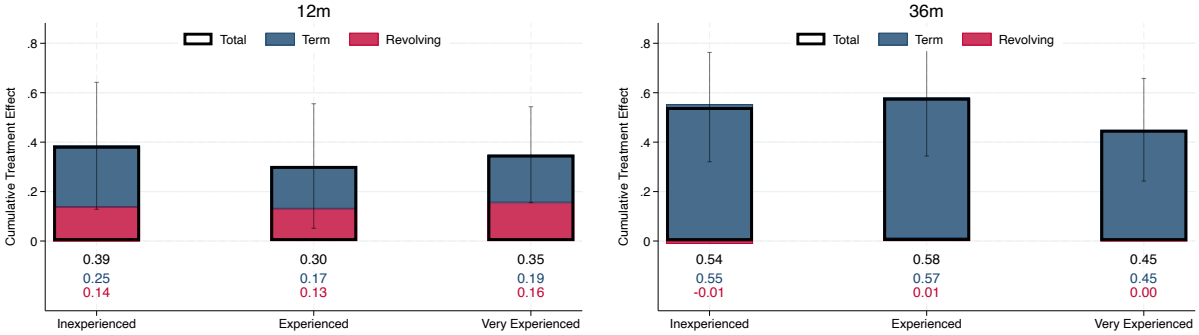
(a) By Baseline Firm Size



(b) By the Number of Capacity Increases



(c) By Account Age at Onset



Note. The figure shows heterogeneous treatment effects by firm size proxied by debt capacity, the number of capacity increases, and account age at onset. Heterogeneous treatment effects are obtained using Equation 5. Firm size bins are grouped into 5 bins based on quintiles: (1) 100-1,000; (2) 1,050 to 2,100; (3) 2,200 - 3,600; (4) 3,650 - 6,200; (5) 6,300 or over. Firms are grouped into subsample bins based on the distribution of average subsample split variable over the 12-month period before the intervention. Number of capacity increases are grouped into three bins: (1) Never experienced; (2) experienced one limit change before; and (3) more than once. Account age bins are grouped into age terciles: firms that had an active credit line account for (1) 8 to 19 months; (2) 20 to 55 months; and (3) more than 56 months. Whiskers denote 95% confidence interval. Treatment effects are annotated below each bar.

Table A.1: Macroeconomic Indicators

	Turkey (1)	SE. Europe GR, BR, RO (2)	S. Europe IT, ES, PT (3)	Lat. Am MX, BR, AR (4)	EU 28 (5)	US (6)
GDP per capita (PPP)	24.6	23.5	35.4	19.5	39.9	57.3
GDP Growth (annual %)	4.9	1.9	0.7	0.3	1.6	2.3
Poverty	2.2	6.9	1.6	7.6	–	1.5

Note. This table compares Türkiye (TR) with selected geographic and economic peer groups, as well as the European Union 28 (EU28) and the United States (US). Peer groups include Southeast Europe (Greece, Bulgaria, and Romania), Southern Europe (Italy, Spain, and Portugal), and Latin America (Mexico, Brazil, and Argentina). GDP per capita (PPP) is reported in constant 2017 international dollars. Poverty is measured as the headcount ratio at $3.65 per day (2017 PPP), expressed as a percentage of the population. Source: World Bank and OECD.

Table A.2: Transaction Categories and Aggregation

Type (1)	Group (2)	Category (3)	Subcategory Examples (4)
		A. Professional Business Services	
Services	Business Svcs	Advertising, consulting, misc biz svcs	Ads, consulting, marketing, IT, security
Auto	Auto Repair	Auto parts replacement	Replacement of tires, spare parts
		B. Investment	
Durable	Electronics/ Machinery	Appliances, electronics, furniture	Major appliances, computer, cell phones, equipment, furniture, building materials
		C. Operating Expenses	
Cash	Cash	Cash advance	–
Nondur	Gas & Auto	Fuel, auto repair	Gas, car wash, protection
Nondur	Nondur Retail	Retail, recreation, restaurants, hobbies, stationary	Department store, sports acticvities, resturant, printing, office supplies
Services	Insurance/ Travel/Other Services	Insurance, travel, health, education	Insurance, hotel, resorts, car rental, schools, hospitals, laboratories
Services	Utilities	Utilities	Natural gas, electricity, water, telecomm

Note. This table reports examples of detailed spending categories.

Table A.3: Variable Definition

Variable Name (1)	Definition (2)	Source (3)	Frequency (4)
	A. Credit Line-Level		
Capacity (TRY)	Credit limit on business credit line account	Our bank	Monthly
Debt (TRY)	Interest-accruing credit line debt carried over periods	Our bank	Monthly
Revolving	Credit line debt drawn as revolving debt	Our bank	Monthly
Term	Credit line debt drawn as term (point-of-sale) debt	Our bank	Monthly
Debt-to-Capacity	Debt divided by capacity	Our bank	Monthly
Unused Capacity (TRY)	Credit line capacity minus debt	Our bank	Monthly
Transactions (TRY)	Total outflow transactions on credit lines.	Our bank	Monthly
Revolving	Total outflow transactions drawn as revolving debt	Our bank	Monthly
Term	Total outflow transactions drawn as term debt	Our bank	Monthly
	B. Firm & Owner-Level		
Has a Liquid Assets?	Indicator for whether the firm has a checking account at our bank	Our bank	Monthly
Liquid Balances (TRY)	Amount of liquid cash held in the business checking account	Our bank	Monthly
Has Other Debt?	Indicator for whether the firm has any debt issued by any other bank	Credit Registry	Quarterly
Total Debt (TRY)	Total outstanding debt at all banks	Credit Registry	Quarterly
Profits (TRY)	Total revenue minus total costs; only non-negative values are reported	Tax Filings	Annual
Sales (TRY)	Profits plus total outflow transactions on our bank's credit line	Our Bank & Tax Filings	Annual
Owner's Age	Age of the business owner	Our bank	Monthly

Note. This table reports variable definition, data source, and frequency. Panel A reports credit-line level outcomes. Panel B reports firm-level outcomes. The credit registry data (Memzuç) is maintained by the Central Bank of Turkey and includes information on the total debt issued to firms, including those from banks other than ours.

Table A.4: Comparison of Firms with and without Sales and Profits Information

	No Sales (1)	Has Sales (2)	p-value (3)
Capacity	3,928	222	0.69
		(559)	
Debt	1,474	-249	0.15
		(174)	
Revolving	356	-55	0.20
		(43)	
Term	1,118	-194	0.24
		(165)	
Utilization	0.39	-0.04	0.17
		(0.03)	
Exclusive Banking	0.78	-0.23	0.00
		(0.04)	
Owner's Age	39	-5.2	0.00
		(0.79)	
Total Debt	11,270	-499	0.72
		(1,387)	
Has Other Debt	0.55	0.09	0.02
		(0.04)	
Sample Size	2,618	551	551

Note. This table compares pre-experiment characteristics of firms with and without sales information.

Table A.5: Sectoral Breakdown

Sector	Fraction
Services	15 %
Construction	13 %
Food	9 %
Consumer Goods Wholesale/Retail	8 %
Textile	7 %
Construction Materials	5 %
Durable Retail	4 %
Electrical and Optical	3 %
Metal	3 %
Transport	3 %
Forestry	3 %
Petroleum	3 %
Industrial Machinery	3 %
Tourism	3 %
Transportation	3 %

Note. This table presents the sectoral breakdown of our bank's business credit line customers. We report sectors that make up at least 3 percent of the universe, and these sectors account for 85 percent of the total. Construction includes builders, contractors, and renovators; and construction materials include producers and retailers of cement, bricks, ceramic and tiles, kitchen and bathroom accessories, glass and glass products, etc. Services include education, health, restaurants, insurance, cleaning, security, advertising, beauty, and financial services. Food includes bakers, butchers, fishmongers, and produce shops.

Table A.6: Benchmarking: Universe of Incorporated Firms in Turkey

	p5 (1)	p25 (2)	p50 (3)	p75 (4)	p95 (5)	Number of Firms (6)
Panel A. All Firms excluding Top 5%						
Capacity	1,480	10,000	26,500	75,418	337,414	3,439,077
Debt	0	0	588	24,200	124,200	3,439,077
Utilization	0.00	0.00	0.03	0.54	1.00	3,439,077
Total Debt	189	41,038	251,716	953,896	4,394,798	597,058
Profits	-144,968	-8,619	2,727	30,924	252,534	597,058
Panel B. All Firms below p50						
Capacity	668	4,500	10,000	20,000	25,000	1,719,477
Debt	0	0	0	1,805	15,000	1,719,477
Utilization	0.00	0.00	0.00	0.27	1.00	1,719,477
Total Debt	0	6,002	47,135	134,577	251,716	314,241
Profits	-64,937	-7,978	-41	9,942	89,948	314,241
Panel C. Our Sample						
Capacity	500	1,250	3,000	5,700	17,100	3,169
Debt	0	190	754	1,891	5,852	3,169
Utilization	0.00	0.07	0.33	0.69	1.00	3,169
Total Debt	0	828	5,924	16,652	67,329	551
Profits	7,692	9,678	12,675	19,269	50,820	551

Note. This table reports benchmarking statistics for the universe of incorporated businesses in Turkey. Statistics reported in Panels A and B are obtained from the Central Bank of Türkiye's credit registry and corporate tax datasets. Credit line outcomes (capacity, debt, utilization) are based on the month before the experiment and firm-level outcomes (total debt and profits) are based on the year of the experiment. Panel A excludes firms in the top 5% of the size distribution, or firms that are likely to be publicly-traded. Panel B reports the distribution of all firms in the bottom half of the size distribution.

Table A.7: Covariate Balance: Levels and Changes

	Capacity (1)	Debt (2)	Liq. Assets (3)	Total Debt (4)	Profits (5)	Sales (6)
			A. Levels			
γ_{-1}	-450	35	-68	77	1,564	1,274
	(405)	(105)	(397)	(1,204)	(1,623)	(2,234)
γ_{-2}	42	4	165	-73	-433	1,055
	(312)	(142)	(164)	(1,090)	(2,492)	(2,670)
γ_{-3}	206	16	82	756	2,780	3,556
	(366)	(133)	(177)	(860)	(1,625)	(2,156)
γ_{-4}	94	68	117	1,169		
	(490)	(121)	(207)	(773)		
p-value	0.15	0.97	0.70	0.50	0.21	0.32
			B. Changes			
γ_{-1}	-449	37	-235	204	2,112	675
	(212)	(119)	(310)	(663)	(1,846)	(2,126)
γ_{-2}	-122	-6	80	-775	-275	439
	(183)	(132)	(140)	(729)	(515)	(679)
γ_{-3}	156	-46	-37	-360		
	(186)	(115)	(190)	(592)		
γ_{-4}	-54	35	32	86		
	(107)	(102)	(192)	(520)		
p-value	0.23	0.99	0.90	0.83	0.40	0.77

Note. This table reports dynamic covariate balancing for key outcome variables in levels (Panel A) and changes (Panel B). Columns 1– 4 present quarterly lags using data from the four quarters preceding the experiment for $N = 3,169$ participants. Columns 5 and 6 display annual lags using data from up to three years before the experiment. The bottom row shows p-values for the null hypothesis that γ_j are jointly zero.

Table A.8: Econometric Evaluations of Non-Experimental Methods

12-month effect	Raw Correlation (1)	Strata-adj OLS (2)	Strata-adj OLS + Controls (3)	Strata Matching (4)	HIR (5)	Oaxaca-Blinder (6)
First-stage (TRY)	1,906	1,895	1,905	1,934	1,522	1,923
	(110)	(108)	(96)	(104)	(134)	(65)
Intent-to-treat (TRY)	394	391	396	423	279	344
	(73)	(72)	(102)	(62)	(50)	(92)
Treatment Effect	0.21	0.21	0.21	0.22	0.18	0.18

Note. This table reports 12-month first-stage, intent-to-treat, and treatment effects using (endogenous) debt capacity increase $\mathbb{D}_i$ – rather than (exogenous) treatment assignment, $\mathbb{Z}_i = 1$ as an instrument to demonstrate selection bias that could arise from employing non-experimental methods. The estimates are obtained using the pre-experimental period (i.e., evaluated 36 months before the intervention). Column 1 reports raw correlation between Y and D. Column 2 controls for randomization strata, f_s, which corresponds to fixed effects for 10 different utilization bins. Column 3 adds pre-experimental business characteristics to Column 2. Column 4 estimates treatment effects weighted by the conditional probability of treatment within strata à la (Angrist, 1998). Column 5 estimates average treatment effect by swapping propensity score weights for strata fixed effects à la (Hirano et al., 2003). Column 6 uses a reweighting procedure suggested by (Kline, 2011). Bootstrap iterations for columns 4 and 5 are set to 1,000.

Table A.9: Short-Run Effects on Capacity and Debt Robustness

	Y		N (1)	$3m$ (2)	$6m$ (3)	$12m$ (4)
	Δ Debt	Main IV Estimate as in Table 3	3,169	0.21 (0.08)	0.31 (0.09)	0.35 (0.09)
Panel A: Real Terms	Δ Debt	USD Terms Debt in U.S. Dollars	3,169	0.21 (0.08)	0.30 (0.09)	0.33 (0.12)
		Real Terms Debt deflated by CPI	3,169	0.21 (0.08)	0.31 (0.09)	0.34 (0.11)
Panel B: External Validity	Δ Debt	Weighted for External Validity to Match Private Firm Universe	3,169	0.18 (0.08)	0.32 (0.08)	0.31 (0.10)
		Subsample Largest 5% by Capacity to Match the Median Private Firm in Universe	156	0.39 (0.09)	0.46 (0.12)	0.49 (0.13)
Panel C: Balance Shifting	Δ Debt	Subsample No Account on Other Banks Ensure no shifting on Other Banks	2,352	0.24 (0.08)	0.35 (0.09)	0.39 (0.11)
	Δ Debt	Subsample No Cash Account Ensure no shifting on Cash	1,286	0.23 (0.09)	0.30 (0.10)	0.40 (0.12)
	Δ Cash	Cash Balances Ensure no shifting on Cash	1,883	-0.002 (0.17)	-0.01 (0.11)	-0.02 (0.25)

Note. This reports treatment effect robustness over different time horizons after the onset of the experiment. The first row reports IV estimates as shown in Table 3. Panel A reports estimates in U.S. dollars and real Turkish Liras. Real terms are deflated using the price deflator for Consumer Price Index. The first row of Panel B reports estimates reweighting the experimental sample to match the distribution of the universe of private firms in Türkiye using utilization bins reported in Table A.6. The second row of Panel B reports estimates restricting the sample to the largest 5 percent of firms using capacity utilization. The first two rows of Panel C reports estimates restricting the sample to subsample with no account at other banks (first row) and to those with no checking account at our bank (second row). The last row uses cash spending as an outcome for a sample of firms with checking accounts at our bank. Robust standard errors clustered at the business-level and reported in parentheses.

Table A.10: Capacity and Debt in EUR and USD

	Baseline Level		*6m*	*12m*	*18m*	*24m*	*30m*	*36m*
	(1)		(2)	(3)	(4)	(5)	(6)	(7)
		A. Euro (EUR)						
ΔCapacity	1,641	First-stage (OLS)	886	712	766	709	614	640
			(75)	(70)	(79)	(113)	(109)	(134)
ΔDebt	552	Intent-to-treat (OLS)	276	242	205	270	340	307
			(77)	(88)	(110)	(126)	(85)	(108)
		Treatment Effect (Wald)	0.31	0.34	0.27	0.38	0.55	0.48
			(0.09)	(0.12)	(0.14)	(0.17)	(0.11)	(0.11)
		B. U.S. Dollar (USD)						
ΔCapacity	2,154	First-stage (OLS)	972	812	885	812	676	765
			(77)	(82)	(92)	(130)	(125)	(162)
ΔDebt	725	Intent-to-treat (OLS)	296	270	233	303	362	363
			(87)	(103)	(129)	(145)	(98)	(129)
		Treatment Effect (Wald)	0.30	0.33	0.26	0.37	0.54	0.47
			(0.09)	(0.12)	(0.14)	(0.17)	(0.12)	(0.11)

Note. This table presents first-stage (FS), intent-to-treat (ITT), and treatment effects over different time horizons after the onset of the experiment in Euros and U.S. dollars. Estimates are obtained from running Equation (1), which captures the average effect of receiving a debt capacity increase, $\mathbb{Z}_i = 1$. Month-end exchange rates converting TRY to EUR and USD are obtained from Datastream and applied to transform outcome variables.

Table A.11: Effects on Firms with High Baseline Utilization

	Baseline	Intent-to-Treat			Treatment Effect		
TRY	Level	*Y1*	*Y2*	*Y3*	*Y1*	*Y2*	*Y3*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Above p50 Utilization (>=0.41); Mean Util = 0.60						
Debt	2,087	1,089	1,181	1,666	0.51	0.49	0.56
		(212)	(424)	(684)	(0.09)	(0.11)	(0.11)
Sales	22,254	-2,262	-4,431	-4,797	-1.47	-2.57	-2.79
		(3,558)	(8,708)	(14,866)	(2.55)	(5.80)	(9.53)
Business Expense	8,606	10,041	17,704	25,524	3.95	6.57	10.10
		(2,370)	(4,749)	(6,776)	(0.87)	(1.66)	(2.62)

Note. This table presents intent-to-treat (ITT) and treatment effects on credit line debt, sales, and business expenses over different time horizons after the onset of the experiment. The sample is restricted to firms with above median (0.41) utilization rate over the 12-month period before the experiment. Estimates are obtained using the same procedure as in Tables 3, 6, and 5. Robust standard errors clustered at the firm level and reported in parentheses.

Table A.12: Correlation Between Utilization and Financial Distress

	No Controls			Controls		
	Distress	*Default*	*Renegotiation*	*Distress*	*Default*	*Renegotiation*
	(1)	(2)	(3)	(4)	(5)	(6)
Utilization	4.90	4.14	1.93	2.25	1.64	1.02
	(0.07)	(0.06)	(0.04)	(0.13)	(0.11)	(0.09)
Binding Frequency	1.95	1.60	0.78	1.38	1.03	0.58
	(0.07)	(0.06)	(0.04)	(0.11)	(0.10)	(0.08)
Outcome Mean (%)	2.82	2.48	1.27	2.82	2.48	1.27
Time F.E.				X	X	X
Firm size control				X	X	X

Note. This table reports the correlation between utilization and financial distress. The sample is restricted to the control group unaffected by the experiment. Columns 1-3 report raw correlations and Columns 4-6 report controlling for time fixed effects and firm size, proxied by total debt. Utilization is expressed in percent and binding frequency refers to the number of times that a firm utilized more than 75 percent on their credit lines. The dependent variables include indicators for a firm being delinquent on a credit line ("Default"), in renegotiation ("Renegotiation"), or both ("Distress"). All outcomes are multiplied by 100 and the outcome means are reported in the row labeled "Outcome Mean".

Table A.13: Lumpy Investments

	Outcomes		
12m	1(Lumpy)	Change in Debt	
	(1)	(2)	(3)
Z	0.042	820	
	(0.01)	(280)	
Z x non-Lumpy			615
			(280)
Z x Lumpy			4,132
			(781)
N	3,169	3,169	3,169

Note. This table examines the effect of treatment assignment on lumpy investments and decomposes 12-month ITT effect into lumpy and non-lumpy investments. Column 1 uses the indicator "Lumpy" as the outcome, which equals one if the maximum spending using term debt on a specific category of goods (e.g., equipment) is greater than the unused (available) debt capacity at the onset. Since our data does not capture item-by-item investment spending, our measure of lumpy investment may overstate total lumpy investment spending. For example, this measure captures total spending on total equipment rather than spending on toaster oven vs. fridge, etc. Columns 1 and 2 decomposes ITT effects into change in debt driven by non-lumpy and lumpy investments.