Precautionary Debt Capacity*

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Abstract

The determinants of debt capacity are a central theme in finance and macroeconomics, yet it remains unclear why firms leave some of their debt capacity unused and whether this affects investment and growth. Textbook theory suggests that firms should not leave their debt capacity unused (Tirole, 2006). Possible explanations include a lack of investment opportunities and cash flow volatility (Fama, 1990). An ideal experiment would expand debt capacity for firms with similar characteristic, helping to disentangle these motives. In this paper, we conduct such an experiment in the field. Despite most firms having substantial unused debt capacity before the intervention, expanding capacity leads to persistent increases in borrowing, investment, and sales. Contrary to textbook theory, these effects are strongest among firms farthest from their debt limits. The evidence points to precautionary motives, as firms appear to preserve debt capacity as insurance against negative cash-flow shocks and default risk. Our findings provide a novel explanation for why financing constrains firm growth, even for firms that appear financially sound.

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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). 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 of debt capacity by isolating the impact of capacity expansion among observationally

[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).

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 also showed lower capacity utilization rates pre-experiment. The risk of financial distress during negative cash flow events declined significantly for treated firms. These findings suggest that firms maintain financial buffers as a precautionary measure to insure against negative 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 limits 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).[2] 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 capacity increases to 2,414 firms (treated) and withheld capacity increases for the remaining 755 firms (control). All firms are subject to the same interest rate due to a nationwide cap set at 24 percent Annual Percentage Rate (APR). 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.[3] 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.

[2]There is a long tradition in macro that builds on Hart and Moore (1994) and Kiyotaki and Moore (1997). There is also an extensive finance literature incorporating debt constraints. See, for example, Hennessy and Whited (2005); Rampini and Viswanathan (2010); Falato et al. (2022).

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

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 cumulatively 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.[4] Moreover, the magnitude of the ITT on sales trans-

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

lates 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 lengthened their runway to absorb unexpected cash flow shocks by 58 percent.[5] 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 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." This self-insurance motive can arise for at least two reasons. First, uncertain cash flows and lender discretion create a strong incentive to preserve unused capacity as insurance against future borrowing constraints (Froot et al., 1993). 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.[6] Thus, concerns about facing liquidity risks due to volatile cash flows

[5]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).

[6]We classify firms using more than 75 percent of their credit capacity as being at risk of fully reaching their borrowing limits. These constraints can arise because (1) firms may need to draw down more of their credit when they face cash flow volatility or (2) due to lender discretion even in the absence of

and financing are prevalent for a majority of firms. Second, default is particularly costly for firms in Turkey due to unlimited liability. Business owners must provide personal guarantees to obtain business loans, meaning that in the event of default, lenders can seize both personal assets (e.g., homes and vehicles) and business assets (e.g., accounts receivable). In practice, lenders seize both types of assets simultaneously and immediately after default, with 75 percent of recoveries coming from personal assets and only 25 percent from business assets.[7] Thus, the significant personal financial risk owners face creates strong precautionary motives, 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. Second, debt capacity is crucial for reducing firms' default risk. Treated firms are 1.34 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 exam-

cash flow needs. While both scenarios are common, the first case is widespread– among the 75 percent of firms at risk of maxing out their limits, 60 percent do not experience any lender-initiated reductions.

[7]We obtained this statistic from the head of the recovery and litigation team at a major Turkish commercial bank specializing in retail lending. When a borrower becomes delinquent, lenders reassess borrowers' creditworthiness and decide whether to renegotiate the loan by extending its maturity. If the borrower ultimately defaults, the bank seizes owners' assets immediately. As a result, all borrowers who default on a business loan are expected to lose some or all of their personal and/or business assets.

ine whether treated firms are more likely undertake "lumpy" investment, or instances where the maximum spending for a given expense category (e.g., equipment) 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 find little supporting evidence that they are the primary drivers of our results.

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 and creditor-imposed restrictions on financial and investment decisions. We similarly document evidence of such "precautionary behavior" that EBCs lower effective debt capacity, but offer a distinct explanation for this behavior: firms maintain financial buffer to manage cash flow volatility and minimize distress risks. This finding suggests that EBCs can matter in a broader context, even in the absence of earnings-based covenants.

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, especially

during economic downturns (Ivashina and Scharfstein, 2010; Campello et al., 2010; Chodorow-Reich and Falato, 2022). However, there is much less work exploring why credit lines are pervasive but rarely drawn down to their fullest extent.[8] 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 creditor-friendly environment with harsh debt enforcement (Lerner and Schoar, 2005; Djankov et al., 2008), as optimal debt policy depends crucially on creditors' ability to seize and liquidate assets (Benmelech et al., 2005).

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.[9] This approach, combined 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. For example, Catherine et al. (2022) demonstrates how a "well-identified" reduced-form coefficient can be incorporated into the quantitative macrofinance literature.

We note that, while our experiment was conducted in Türkiye, our findings offer

[8]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, Hennessy and Whited (2005), Gamba and Triantis (2008), Nikolov et al. (2019) and Amberg et al. (2023).

[9]We provide direct evidence that our research design addresses endogeneity concerns. Specifically, covariate-controlled estimates from a pre-experimental period are only half as large as the experimental estimates, suggesting that randomization mitigates downward bias.

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 are essential for job creation, economic growth (Haltiwanger et al., 2013; Akcigit and Kerr, 2018), and the transmission of monetary policy (Caglio et al., 2021), as they are particularly responsive to monetary shocks in the presence of financing frictions. 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 and Harvey, 2001; 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, 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; Gourinchas et al., 2024). 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. The market share is comparable to that of JPMorgan Chase in the U.S. (Statista, 2023). 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. See Table A.2 for the sectoral breakdown of business credit line clients at our bank.[10]

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

[10]While our bank did not make industry classifications for our sample firms available to us, they confirmed that the sectoral distribution closely resembles that of their overall client base.

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.

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 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.

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. See Figure 1 for the experimental timeline. Due to our bank's institutional constraints, the treatment assignment is rolled out over two calendar months.

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. 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 from our bank, covering three years before and after the experiment.The data includes monthly information on firms' debt capacity, drawdown balances, spending transactions, loan performance (delinquency

or restructuring), liquid asset holdings (checking, savings, or brokerage) at our bank, whether the firm has accounts at other banks, and the owner's age. We separately obtained firm-level quarterly data on total debt from the Turkish Central Bank's credit registry, Memzuç. This dataset captures all debt issued to our firms by our bank and other financial intermediaries. See Table A.3 for detailed variable definition and data sources.

Our bank collects firm-level annual profits data from tax filings for a subset of clients enrolled in one of their eight bundled "financing packages." 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. Alternatively, 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. In our experimental sample, 551 firms were enrolled in these financing products, providing firm-level profit data for this subset of clients. See Table A.4 for a comparison of firms with profits information to those without.

Outcomes. Our main outcomes of interest are debt capacity, credit line debt, and financial and real outcomes at the firm level. Debt capacity refers to the maximum limit on credit lines. 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 refer to total spending on credit lines. Monthly transactions may exceed debt since it captures all outflow expenditures, including those paid off within the billing cycle. 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.5 for more details on purchase categories.

Firm-level total debt represents the outstanding interest-accruing balances carried over from previous periods. Annual profits, defined as net income (revenues minus

expenses), are reported on a fiscal-year basis. 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.

To facilitate comparison between stock variables (e.g., debt) and flow variables (e.g., profits, sales, expenses), we analyze stock variables in terms of changes and flow variables in terms of cumulative changes relative to the onset of the experiment. This approach aligns with the standard methodology in the macroeconomic literature for estimating the marginal propensity to consume in response to credit line shocks (Gross and Souleles, 2002; Agarwal et al., 2018).

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 of 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. The median firm's debt capacity in the national dataset corresponds to 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 A.7 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.

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").[11] 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 the causal effects for compliers (Angrist and Pischke, 2009).

2.5 Pre-Experimental Borrowing and Debt Capacity

We begin by examining the pre-intervention characteristics of our experimental sample. Table 1 displays summary statistics. The average debt capacity and credit line debt were 4,662 TRY and 1,569 TRY, respectively. For context, the size of this debt capacity corresponds to roughly 4.5 months of cash runway without generating any income (i.e., a cash buffer). The unused debt capacity alone provides just under

[11] 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.

three months of runway, based on average monthly operating expenses before the experiment. Average monthly expenses was 1,050 TRY, with three-quarters allocated to operating expenses and one-quarter to financing investments. On average, firms held 1,248 TRY in liquid cash deposits at our bank. Figure 3 reports graphical illustration of pre-experimental credit line account and firm characteristics.

We establish that most firms in our study do not exhibit signs of financial constraints before the intervention, as their debt levels remained well below their available capacity. Figure 3 visually illustrates this finding. In the month prior to the intervention, fewer than 10 percent of firms were strictly financially constrained, meaning they had nearly exhausted their credit capacity (with a utilization ratio above 98 percent). On average, firms utilized 39 percent of their debt capacity, while the median firm used only 33 percent. Roughly 13 percent of firms had no outstanding debt on their credit lines at all. We next analyze the causal effects of expanding debt capacity.

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.

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. Table 2 report short-run effects using Equation 1 in Columns 2-4 and long-run effects using Equation 2 in Columns 5-7. 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 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. The second row of Table 2 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. However, Columns 5-7 show persistent borrowing effects, with treated firms borrowing 1,296 TRY more after 3 years, relative to the control group. See Panel B of Figure 4 for a graphical version of this result. Figure 4 displays visual illustration of the FS and ITT effects. Figure A.2 shows these results in real TRY, adjusting for inflation.

Treatment Effect. The third row of Table 2 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, these results suggest a large and persistent increase in drawdowns.

Robustness. Section A 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 utilization distribution of the universe of Turkish firms to assess generalizability, (3) restricting the sample to firms that only bank with our bank to address potential substitution effects, and (4) validating that firms are not substituting between cash and credit line.

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 B 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.

4 Firm-Level Financing and Real Effects

Next, we examine the impact of capacity expansion on total firm-level borrowing, investment, and growth. As outlined in Section 2.4, we employ specification 4 to analyze these firm-level effects. Section 4.1 investigates financing, Section 4.2 examines investment and spending, and Section 4.3 presents findings on sales and profits.

4.1 Total Debt and Cash Balances

We examine how debt capacity affects total debt and cash balances. The increase in borrowing among seemingly unconstrained firms could suggest that they are substituting credit line debt for other forms of debt, in which case total firm-level debt would remain unchanged. Additionally, firms may be drawing down funds to hold as cash reserves rather than using them for growth-related spending. 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).

Capacity expansion increased firm-level borrowing and had no effect on firms' cash holdings. Table 3 presents ITT (Columns 2-4) and TE (Columns 5-7) estimates for total debt and cash holdings. Firms gradually increased leverage, with total debt rising by 1.14 TRY per dollar of expanded capacity after Y1 and reaching a cumulative effect of 2.02 TRY per dollar by Y3. This pattern suggests that firms expanded overall borrowing rather than substituting credit line debt for other debt types, consistent with the view that credit line debt complements other financing sources (Rauh and Sufi, 2010; Donaldson et al., 2024). The second row of Table 3 confirms that capacity expansion had no effect on firms' cash balances, implying that firms did not draw down credit lines to accumulate liquidity. The combination of increased borrowing and stable cash balances suggests that firms deployed the additional funds toward productive capital. We next examine firms' spending patterns.

4.2 Investment and Spending

Treated firms increased their investment and operating expenses. Table 4 shows that investment spending increased by 4,402 TRY over three years, or approximately 1,467 TRY annually ($\frac{4,402}{3}$). This corresponds to a substantial 46 percent increase relative to the pre-experimental average, as shown in Column 1. The investment response is economically large but plausible given the scale of the firms we study. Figure A.3 shows examples of investment goods firms could have afforded in 2016 (3 years after the experiment) with the estimated Y3 effect. Firms could have purchased an industrial refrigerator or an industrial dishwasher paired with a jumbo teapot.

In terms of treatment effects, Column 7 show that spending rose across all three categories, with firms allocating \$1.50 to professional business services (advertising, marketing, etc), \$1.83 to investment (machinery, equipment, etc), and \$4.57 to operating expenses per dollar increase in debt capacity over the long run. Overall, firms utilized expanded debt capacity to invest in productive capital and scale up operations. We next examine whether this increase in investment translated into higher sales and profits.

4.3 Sales and Profits

The experiment led to significant increases in sales and profits. Columns 2-4 of Table 5 show that treated firms cumulatively earned 25,482 TRY more than the control group after three years. This increase is 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, Column 7 indicates that firms cumulatively generated \$7.19 in sales and \$3.90 in profits per dollar of increased debt capacity after three years.

Our estimated effects are sizable but plausible given the scale of firms in our sample. First, the 26 percent annual profit increase we document aligns closely with the 24 percent annual profit effect reported in Cai and Szeidl (2023), which examines the impact of randomly increasing credit on privately held small firms similar to those in

our study. In terms of economic magnitude, given that the average pre-experimental profit growth rate was 12 percent, our estimated increase in profits is roughly twice the mean growth rate. Put differently, the effect is comparable to one standard deviation of the annual profit growth rate prior to the experiment (34 percent) and is of the same order of magnitude as moving a firm from the bottom to the top quartile (18 percent). Second, the 8,494 TRY annual effect 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 lengthened their runway to absorb unexpected cash flow shocks by 58 percent.

5 Precautionary Debt Capacity

Despite most firms having substantial unused debt capacity before the intervention, expanding capacity leads to persistent increases in borrowing, investment, and sales. This section explores potential explanations. Section 5.1 documents subsample heterogeneity by pre-experimental utilization rates. Section 5.2 explores the precautionary debt capacity channel, and Section 5.3 explores alternative mechanisms.

5.1 Firms with Financial Slack

The textbook *static* theory of financial constraints predicts that only firms facing strict financial constraints should respond to capacity expansion. Consequently, firms with debt levels well below their capacity before the experiment should not exhibit high sensitivity to capacity expansion. To test this hypothesis, we restrict our sample to firms with below-median utilization rates before the experiment or those with the lowest debt utilization relative to their expanded capacity.

Capacity expansion led to a moderate increase in credit line drawdowns and a significant rise in sales and expenses for these firms. Table 6 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). 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.

The sales effects are significantly larger for the financially unconstrained subsample than the average effect observed in the overall sample, suggesting that average firm growth is driven by firms with greater financial slack before the experiment. To further illustrate this point, Table A.10 reports estimates for firms with above-median utilization rates before the experiment. Financially constrained firms drew heavily on credit lines, borrowing 0.56 cents per dollar of expanded capacity after three years. Despite increased business spending, treated firms did not experience sales gains. Overall, these findings suggest that capacity expansions had larger real effects on firms with greater financial slack, despite their moderate borrowing response.

5.2 Drivers of Precautionary Debt Capacity

The finding that capacity expansions had the largest real effects on firms with greater pre-experimental financial slack raises an important question: Why had these firms left their debt capacity unused despite having viable investment opportunities? One possibility, as posited by Modigliani and Miller (1963), is that firms prefer to maintain a 'reserve borrowing capacity.' This self-insurance motive may stem from cash flow and financing volatilities that necessitate holding cash reserves (Fama, 1990) or from the substantial personal financial risks that owners face in the event of unexpected revenue declines (Myers, 1977). We test the relevance of these hypotheses.

Volatility. We confirm that unexpected volatilities in cash flows and financing is negatively correlated with utilization behavior, consistent with the precautionary motive of holding a cash buffer in uncertain economic environment. Specifically, we conduct a cross-sectional analysis correlating various measures of volatility with utilization rates. As a first step, we construct proxies of expense, cash flow, and financing volatil-

ity, 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. We restrict this analysis to control firms unaffected by capacity expansions to minimize confounding effects.

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. Table 7 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.

These findings remain robust even after controlling for alternative explanations for low utilization. We control for various measures of firm size (e.g., log of expenses, sales, and total debt) to account for the possibility that volatility scales with size. We also consider the potential influence of frequent lender-initiated limit increases, which might prompt firms to manage liquidity more actively. To capture this, we control for whether a firm received limit increases more frequently before the experiment. Additionally, we account for the possibility that older firms or more experienced own-

ers 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. Columns 4 through 6 show that even after accounting for firm size, frequent limit increases, and experience, higher volatility remains negatively correlated with utilization rates.

Overall, concerns about liquidity risks stemming from volatile cash flows and financing are prevalent for the majority of firms. While our experimental sample held substantial financial buffers on average, most had faced the possibility of exhausting their debt capacity at least once before the intervention. Figure 3 shows the distribution of how frequently firms encountered binding constraints prior to the experiment.[12]

Financial Distress. Owners' personal financial risk is another highly plausible reason for firms to preserve debt capacity. In Turkey, business owners must provide personal guarantees to obtain business loans, effectively exposing them to unlimited liability. In the event of default, lenders can immediately seize both personal and business assets. According to our discussion with the head of the recovery and litigation team at a major commercial bank in Turkey, approximately 75 percent of default recoveries come from personal assets, while only 25 percent are recovered from business assets. This creditor-friendly financial system can generate significant risk aversion and motivate firms to maintain substantial buffers to avoid distress.

To corroborate the value of capacity expansion for avoiding financial distress, we quantify the extent to which firms can minimize distress risk with a higher debt capacity. Column 1 of Table 8 shows that higher debt capacity 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. Figure 4 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

[12]Figure A.4 shows a monotonic increase in drawdown response based on the number of times firms faced binding constraints before the experiment. Treatment effects are 140 percent larger (0.21 vs. 0.51) in the short run and 265 percent larger (0.2 vs. 0.73) in the long run for firms that encountered binding constraints more than eight times relative to those that had no prior borrowing before the intervention.

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.

5.3 Alternative Explanations

There are additional explanations beyond precautionary debt capacity that may account for why seemingly unconstrained firms exhibit greater sensitivity to capacity expansion. First, this pattern can be explained by lumpiness in investment. Unconstrained firms may have borrowed more in response to debt capacity expansion because higher capacity enabled them to undertake large investments that were previously unaffordable within their existing debt limits. Since investments tend to be lumpy (Cooper et al., 1999), prior research suggests that external financing can influence both the timing and size of investment projects (Whited, 2006).

To explore this possibility, we examine the probability of firms making lumpy investments – defined as spending on a specific category (e.g., equipment or electronics) within the first 12 months of the experiment that exceeds their unused debt capacity at the onset. Table A.11 shows that while higher debt capacity increases the likelihood of firms making lumpy investments by 4.3 percentage points, 74 percent of the ITT effect (585/795) is driven by non-lumpy spending. In terms of the share of firms, only 4 percent of firms engage in lumpy investments, suggesting that the higher spending effects in response to capacity expansion are not merely driven by firms using expanded debt capacity to finance capital they previously could not afford.

Another possibility is that lender-initiated capacity expansions "encourage" firms 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. Figure A.4 shows little gradient in treatment effect heterogeneity by the number of capacity increases. While these findings do not entirely rule out other potential explanations, we find little supporting evidence for these alternatives.

6 Conclusion

We investigate why firms leave some of their debt capacity unused and whether this matters for their investment and growth. Through a field experiment that randomly increased debt capacity for some firms but not for others with similar characteristics, we find a significant and persistent increase in borrowing, investment, and sales – despite most firms having substantial unused capacity before the experiment. Sales growth is largest among firms with the greatest unused debt capacity before the experiment. Firms appear to preserve debt capacity as an insurance against negative cash flow shocks and default risk: unexpected cash flow and financing volatilities are negatively correlated with utilization rates, and expanded capacity materially reduces distress risk in negative cash flow events.

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 bolstering firm investment and growth could target 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*."

Although we provide 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, while our long-run estimates have a causal interpretation in that these effects would not have materialized without the experiment, we cannot isolate the specific factors driving improved firm performance over

time. Long-run effects likely reflect various margins firms adjusted over time, such as investment and other forms of borrowing. Second, our study focuses on established firms in good financial standing, well-positioned for growth. Whether precautionary debt capacity similarly affects firms already in financial distress or new firms seeking initial credit access remains an open question.

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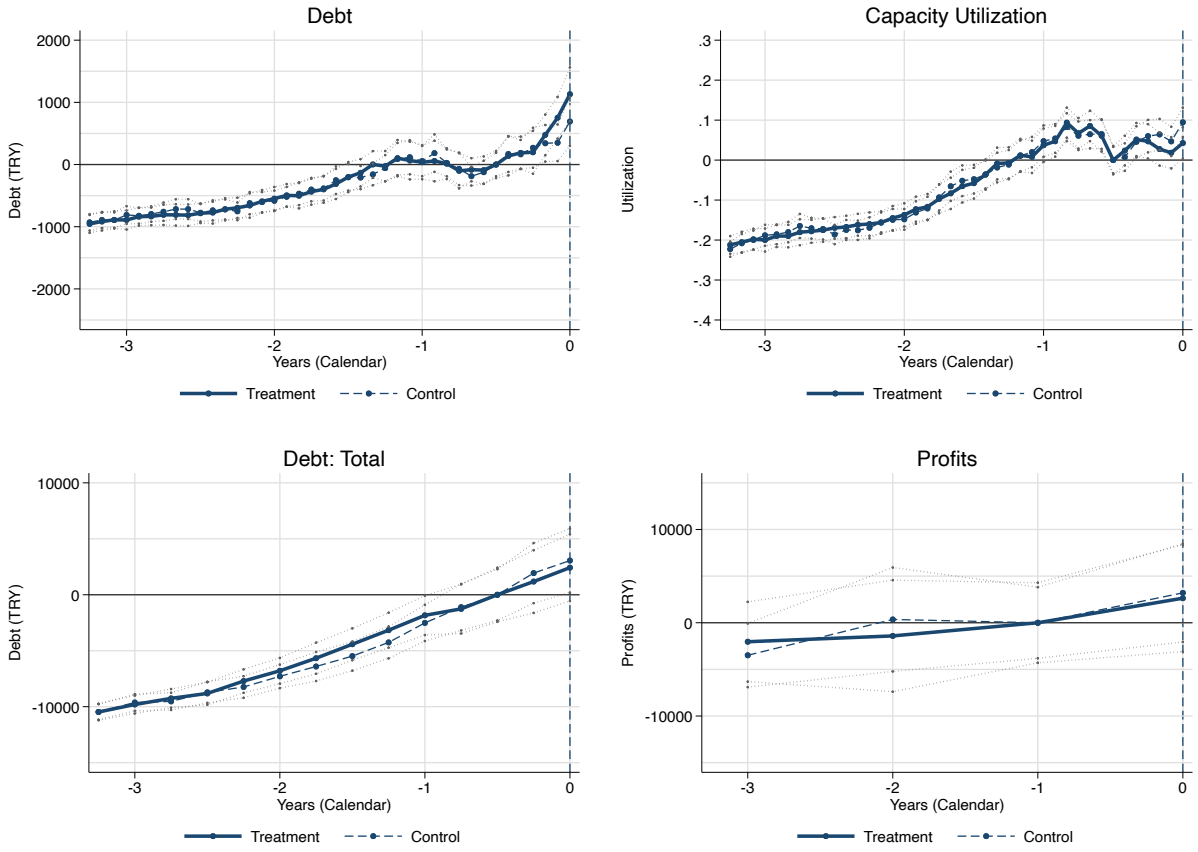
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Figure 1: Experimental Timeline

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

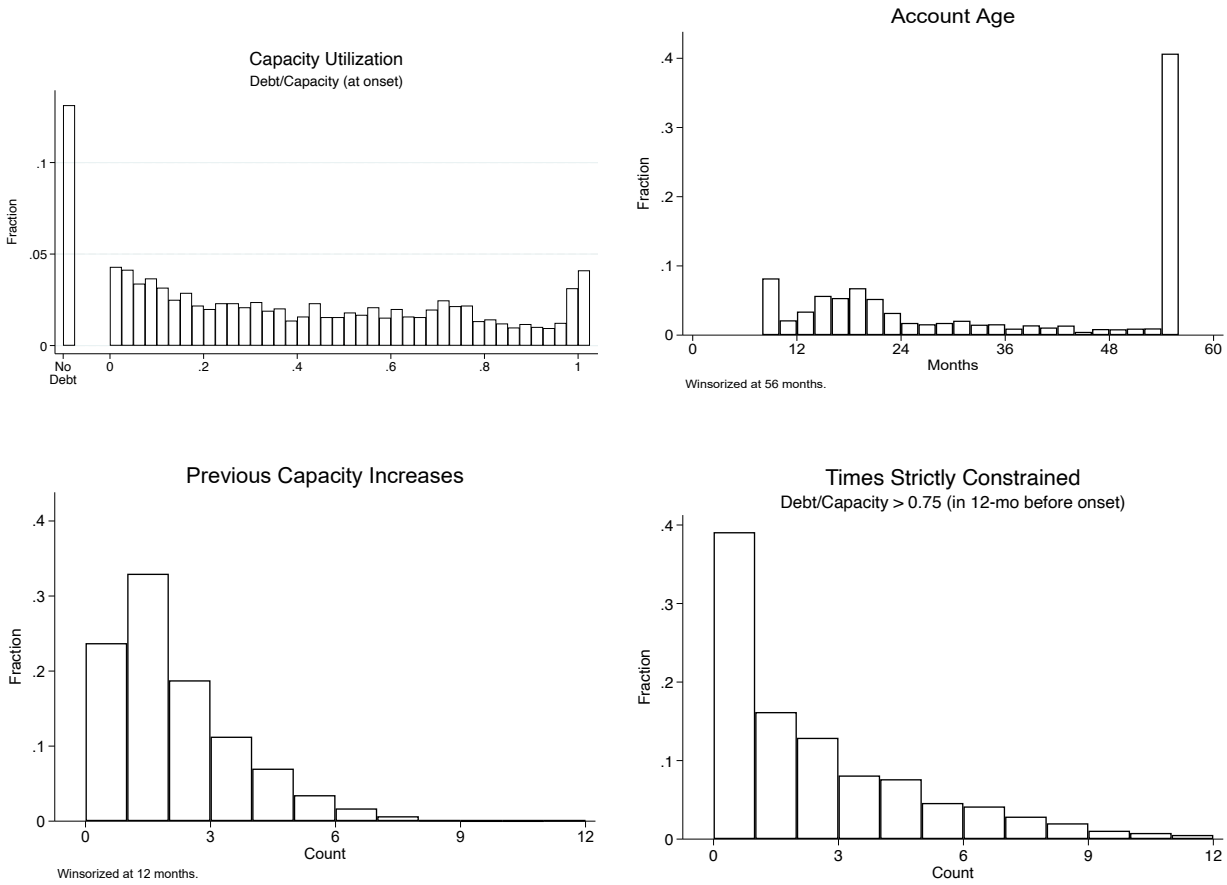
Note. This figure summarizes our randomization timeline.

Figure 2: Covariate Balance Pre-trends



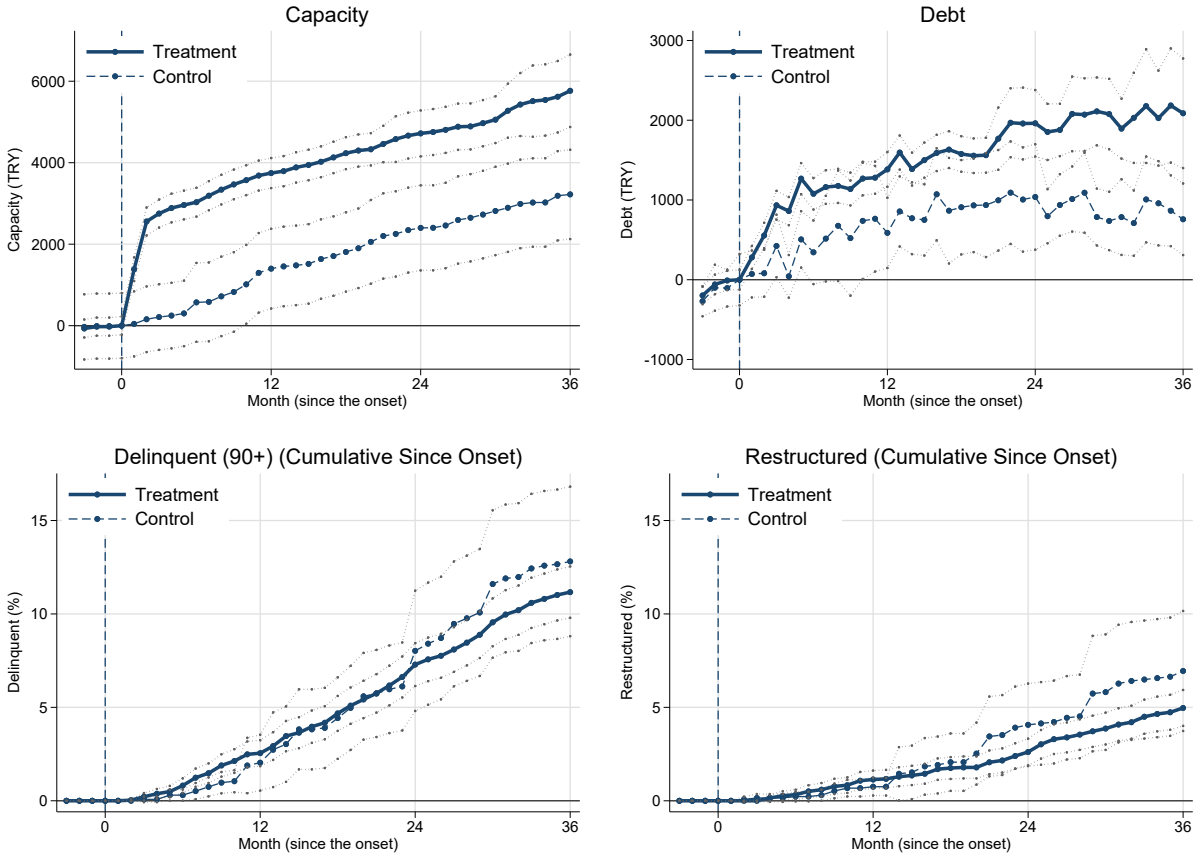
Note. Figures provide visual assessment of pre-trends by plotting covariates for treated and control group $\mathbb{Z}_i$ over three-years preceding the experiment. The blue dashed line denotes the start date of the experiment. The y-axis is normalized to have levels equal to zero at the onset of the experiment. Dashed lines indicate 95% confidence intervals for the estimate of the mean. See Table A.7 for a more comprehensive set of balance tests.

Figure 3: Pre-Experimental Credit Line Characteristics



Note. This figure presents histograms of pre-experimental credit line account and firm characteristics. The top-left panel shows the distribution of utilization rates, defined as outstanding debt balances divided by debt capacity. The top-right panel displays the histogram of account age, measured as the number of months an account had positive debt capacity (top-truncated at 60 months). The bottom-left panel illustrates the frequency of (non-experimental) debt capacity increases, capturing both lender- and borrower-initiated limit adjustments. The bottom-right panel depicts the histogram of the number of times a firm faced binding constraints, defined as having a utilization ratio above 75 percent.

Figure 4: Event Study: Effects of Debt Capacity



Note. The top left figure plots the evolution of debt capacity since the onset (i.e., first-stage) and the top right figure plots that of credit line debt (i.e., intent-to-treat). The bottom left figure plots delinquency rates and the bottom right plots restructuring rates. 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	Mean	SD	p10	p25	p50	p75	p90
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(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
Debt-to-Capacity	3,169	0.39	0.33	0.00	0.07	0.33	0.69	0.91
Unused Capacity (TRY)	3,169	3,093	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
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: Effects on Capacity and Debt

	Baseline		Short-Run			Long-Run		
	Level		*3m*	*6m*	*12m*	*Y1*	*Y2*	*Y3*
	(1)		(2)	(3)	(4)	(5)	(6)	(7)
Capacity (TRY)	4,662	First Stage	2,562	2,480	2,351	2,346	2,318	2,543
			(122)	(206)	(240)	(244)	(387)	(546)
Debt (TRY)	1,569	Intent-to-treat	538	770	820	785	902	1,296
			(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)

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. 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. Robust standard errors clustered at the firm level and reported in parentheses.

Table 3: 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	2,994	4,615	5,252	1.14	1.75	2.02
		(878)	(1,263)	(1,325)	(0.36)	(0.55)	(0.61)
Cash Balances	1,248	-56	-216	–	-0.02	-0.08	–
		(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 4: 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 5: 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 6: 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 2, 4, and 5. Table A.10 reports estimates for high utilization sample. Robust standard errors clustered at the firm level and reported in parentheses.

Table 7: 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	0.089	0.131	0.074	0.209	0.172	0.137

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.

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"). Our bank classifies accounts as non-performing if payments are overdue by 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.

A 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.

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. Overall, our estimates do not appear to reflect balance shifting across accounts.

B 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 $\delta_x = E[Y_1 - Y_0|x] + \text{bias}$

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).

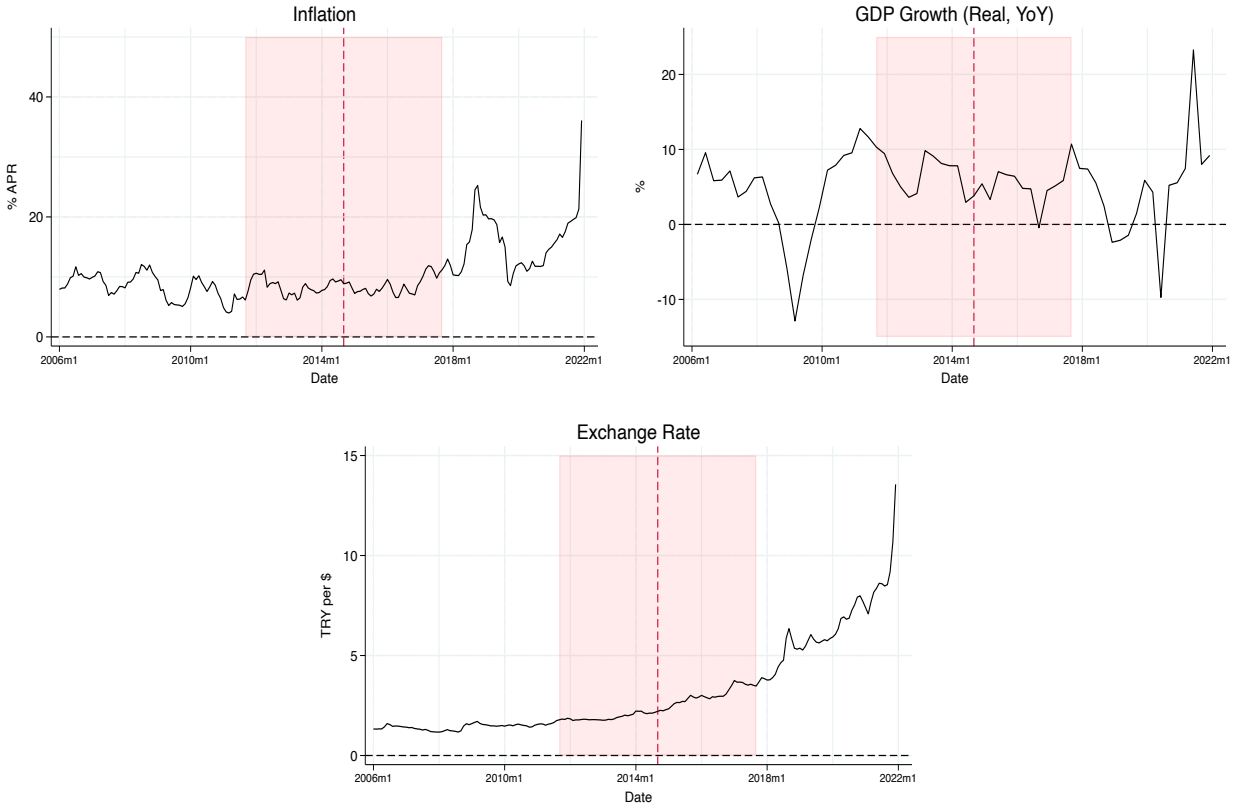
$$\rho = (\beta_1 - \beta_0)' E[\mathbf{X}] + bias$$

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.

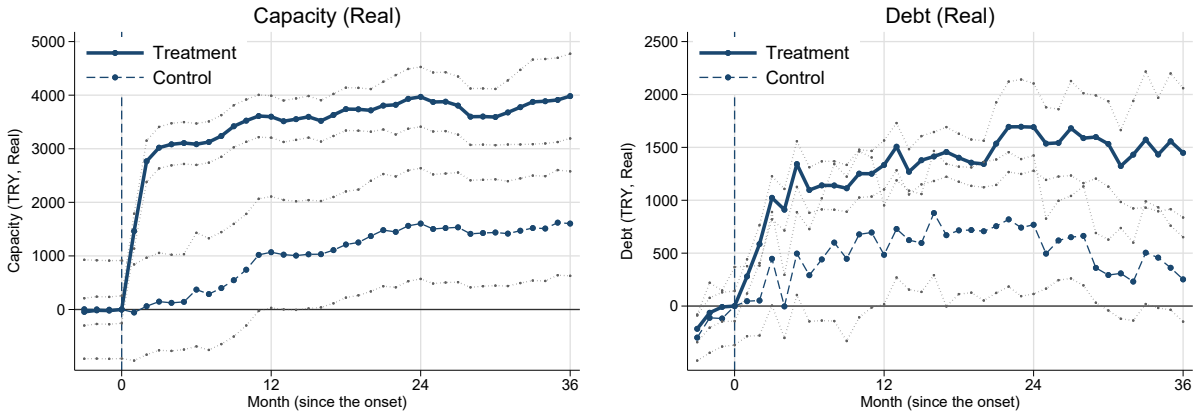
Overall, this exercise illustrates the importance of having a credible, exogenous instrument for financing. The fact that we get a substantially different estimate *for the same set of firms* illustrates that randomization effectively addresses selection bias in estimating financing effects from observational 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.

Figure A.1: Macroeconomic Environment in Türkiye



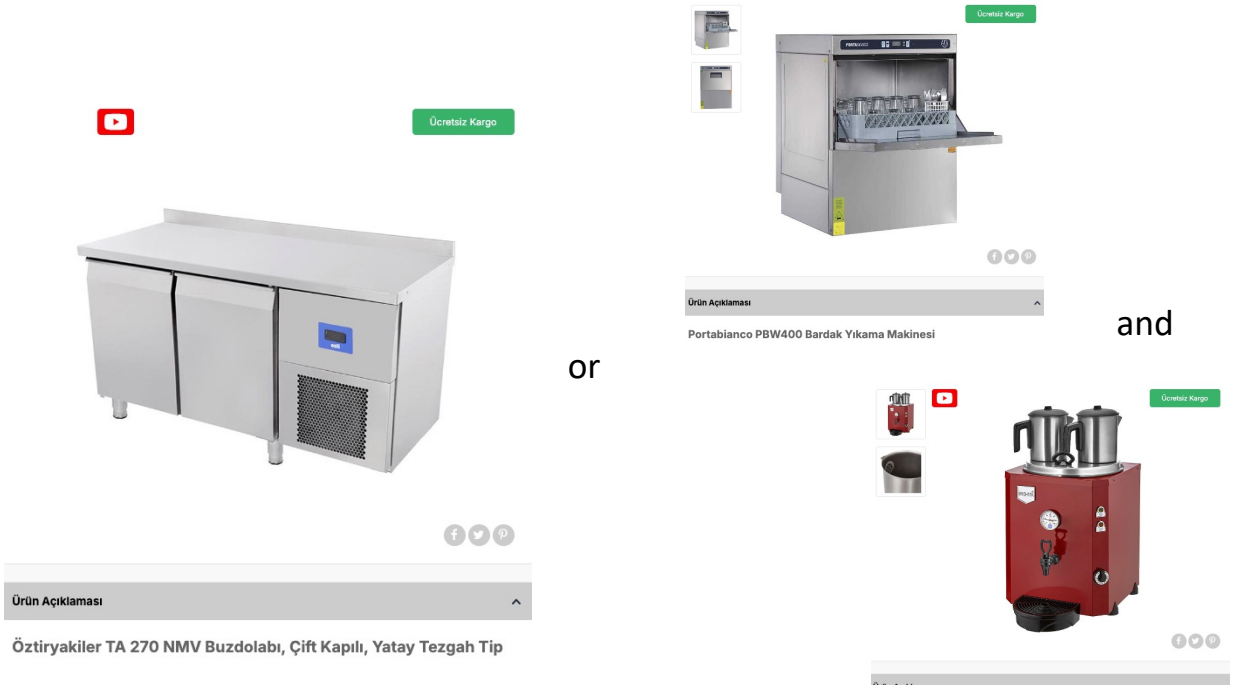
Note. Figures plot annual inflation, GDP growth, and USD/TRY exchange rate. In each panel, the dotted line denotes the onset of our experiment (2014m9), and the highlighted area in pink denotes our sample period. Source: Turkstat via Türkiye Data Monitor.

Figure A.2: Effects on Debt Capacity and Credit Line Debt in Real TRY



Note. This figure plots FS and ITT effects in real TRY. Real levels are calculated using the implicit price deflator for personal consumption expenditures. 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.

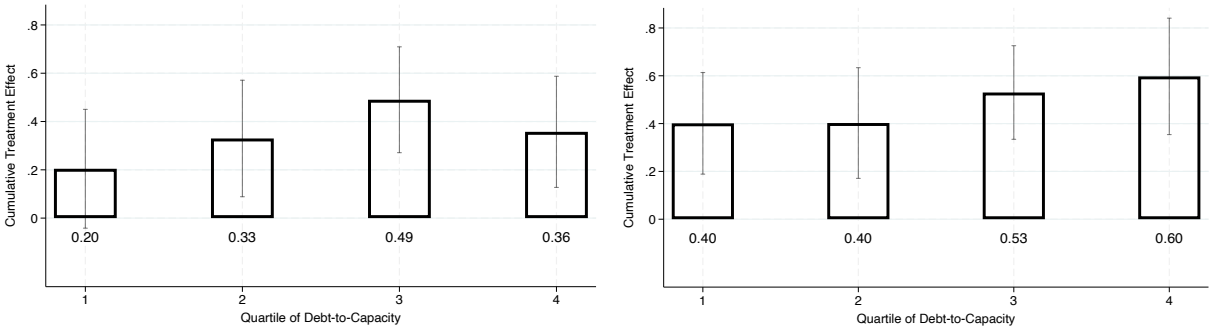
Figure A.3: Examples of Firm Investment Based on Y3 Investment ITT



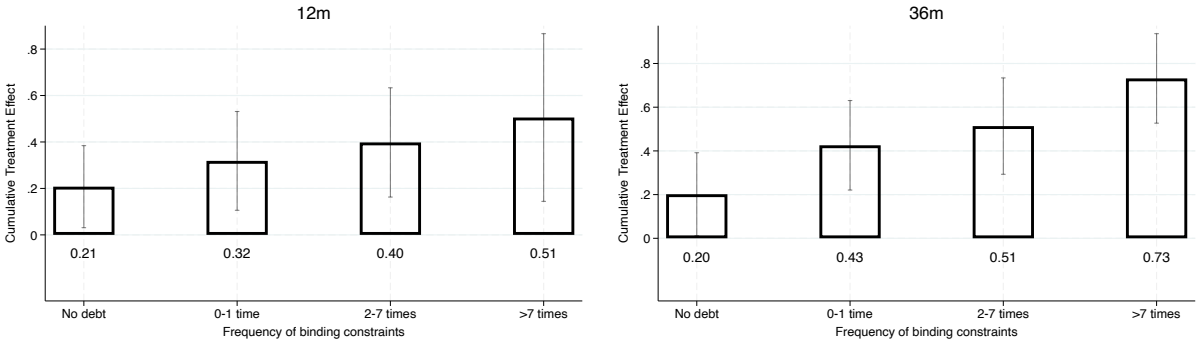
Note. This figure shows examples of durable goods one can buy with Y3 ITT investment response reported in Table 4 in 2017 TRY. The left chart shows an industrial refrigerator. The right chart shows an industrial dishwashing equipment and a jumbo tea pot.

Figure A.4: Heterogeneous Treatment Effects

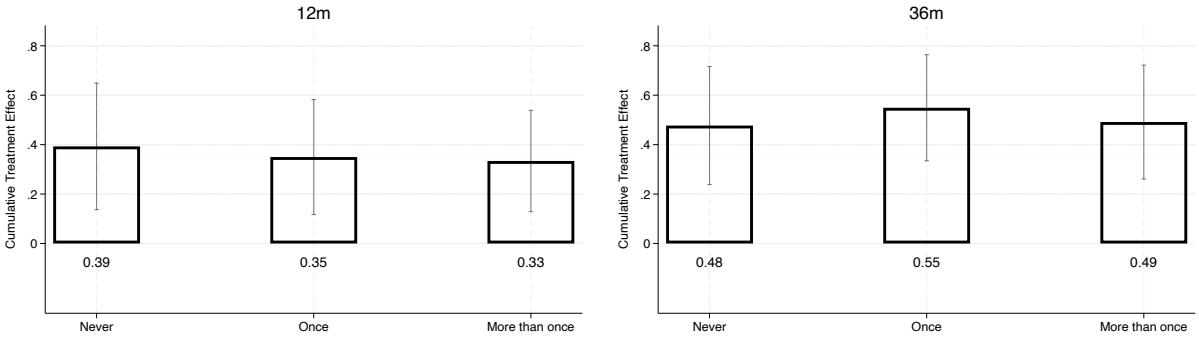
(a) By Baseline Utilization Rate



(b) By the Frequency of Firms Facing Binding Constraints



(c) By the Number of Capacity Increases



Note. This figure decomposes heterogeneous treatment effects on credit line debt 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.4a plots heterogeneity by quartiles of baseline (i.e., 12-month period before the intervention) utilization rates. Figure A.4b 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.4c plots heterogeneity by the number of capacity increases before the experiment. Firms are grouped into three bins: (1) Never experienced; (2) experienced one limit change before; and (3) more than once.

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: 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.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
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
	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)	
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	3,169

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

Table A.5: 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.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	3,169
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

	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)	
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 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)	(103)	(132)	(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 2	3,169	0.21 (0.08)	0.31 (0.09)	0.35 (0.11)
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)

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 2. 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). Robust standard errors clustered at the business-level and reported in parentheses. Month-end exchange rate converting TRY to USD was obtained from Datastream.

Table A.10: Effects on Firms with High Baseline Utilization

TRY	Baseline Level	Intent-to-Treat			Treatment Effect		
		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 2, 5, and 4. Robust standard errors clustered at the firm level and reported in parentheses.

Table A.11: Lumpy Investments

12m	Outcomes		
	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 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.