Subsidy-Driven Firm Growth: Does Loan History Matter? Evidence from a European Union Subsidy Program*

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Abstract

Subsidies should target firms with profitable opportunities but without funding, but this is difficult due to information asymmetry between firms and the government. We study how credit history of firms can help design more efficient subsidies. To this end, we combine data on non-repayable firm subsidies and the credit registry from Hungary. Using subsidy winners and losers as treated and control groups and leveraging variation in access to loans, we identify the differential impact of subsidies. While subsidies lead to an incremental impact on assets of loan-deprived as compared to loan-acquiring firms, the impact is transitory and fades after a few years. The impact on profitability follows a similar pattern despite the higher expected marginal value of capital for loan-deprived firms. Thus, loan deprivation is likely caused by borrower shortcomings instead of credit rationing by banks. In such cases, subsidies need not privilege loan-deprived firms.

JEL codes: H25, H32, G38, G21

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1 Introduction

To add sales and LP result discussion here. More generally, any major changes in the main text to be reflected in the intro. Certainly, but let's be very brief.

Small and medium-sized enterprises (SMEs) generate between half and two-thirds of the employment and value added in major advanced and emerging market economies (e.g., Stein et al. [2013]). However, they tend to have patchy access to finance (e.g., Beck et al. [2008]; Carpenter and Petersen [2002]; Demirgüç-Kunt and Maksimovic [1999]) and governments spend substantial resources to subsidize SME investment. But what firm-types should be subsidized? It can be tempting for these programs to target firms that are unable to secure bank loans and thus are short of funds, but this may not be the optimal use of tax-payer money.

On the one hand, banks may reject a firm's loan application if its credit worthiness is low. In this case, it may not be efficient to target subsidies towards such firms. On the other hand, when dealing with asymmetric or noisy information about firm quality (e.g. a new or young firm), banks may ration credit [Stiglitz and Weiss, 1981]. In this case, loan-rejected firms could very well have profitable projects, and thus be suitable candidates for subsidies. Distinguishing between the two scenarios is not straightforward because it is difficult to observe from the outside whether a firm has profitable ventures.

In this paper, we show how credit registry data combined with estimated firm-level outcomes from a subsidy program can be used to shed light on this puzzle. Specifically, we consider a non-refundable grants program that covers a large variety of investment projects. Operationally, the program requires firms to fund a part of the project using their own funds (i.e. skin-in-the-game). Such subsidies can have both quantity and price effects on firms (firms not only get more capital, but get it for free). Quantity effects primarily impact firms that cannot secure loans while price effects impact both firm-types. If firms that are unable to get bank loans respond more strongly to such subsidies by growing faster and increasing productivity than firms that are able to secure bank loans, then *loan-rejected* firms are likely to be proficient firms for whom banks rationed credit. However, if loan-rejected firms do not respond more strongly than *loan-acquiring* firms, then the reason for rejection is probably on the borrower side, and loan-rejected firms are likely to be of poorer quality.

How can such an assessment be used to design an effective subsidy program? If loan rejections tend to reflect credit rationing in a given jurisdiction, then loan-rejected firms are the better candidates for subsidies. Indeed, because of loan deprivation and therefore a higher marginal product of capital, such firms can benefit more from subsidies than loan-acquired firms. However, if loan rejections reflect poor firm quality, targeting such firms is not efficient.[1]

To examine these issues, we study the *differential* impact of subsidies on loan-rejected (LR) and loan-acquired (LA) firms. We begin by writing a stylized model to guide the empirical analysis. Under mild assumptions, both types of firms benefit from a non-refundable grant and grow their assets, but LR firms

[1]Subsidy programs may have other objectives, such as reducing disparity in firms' access to funding within an economy or reducing inequality more generally, in which case providing subsidies to less efficient firms may be justified.

experience faster growth because subsidies alleviate a relevant constraint on credit access. At the same time, if LR and LA firms are equally productive, LR firms experience a larger increase in profitability.

We empirically test the insights of the model on a European Union (EU) subsidy programs in Hungary. We base our analysis on a unique combination of three large and confidential datasets: (1) the Hungarian Credit Registry, with information on firm credit histories, (2) the EU subsidy database, including both successful and rejected applications, and (3) information on firm balance sheets and income statements. Previous studies have used parts of similar data, but, to our knowledge, ours is the first study to link all of them together.

We first identify LR firms using the Credit Registry according to the following definition: a firm is LR if it applied for a loan but did not receive it subsequently. While we cannot implement this definition directly as we do not observe loan applications, we can indirectly deduce the identity of these firms as follows. We check the credit registry for inquiries about the firm by banks that did not have a lending relationship with the firm at the time of the query. These credit inquiries reflect loan applications by the firm for two reasons. First, while banks may check the credit history of existing clients for monitoring purposes, they typically check the history of an unrelated firm only after the firm approached the bank to ask about the possibility of getting a loan or submitted a formal loan application. Second, the Hungarian regulation forbids banks from approaching firms with loan offers, which means that they do not have an incentive to pursue a credit registry query unless the firm approached the bank for a loan.[2] Then, if the firm does not receive a loan after having applied for it during the year prior to a given date, we denote the firm as LR on that date. Among the remaining firms, those that receive a loan during this period are considered LA, while the rest of the firms (that is, those with no loan application and no loans) remain unclassified.

We then combine difference-in-difference panel regressions with matching techniques to estimate the differential effect of subsidies on the behavior of LR

[2]In the context of bank-lending channel of monetary policy transmission, a similar strategy is used by Jiménez et al. [2012] to identify loan applications.

and LA firms. To decrease the unobserved heterogeneity caused by selection into subsidy application and to improve identification, we take advantage of our data and use rejected subsidy applicants as the control group in our regressions.[3]

Consistent with the literature (e.g., Kersten et al. [2017]), first we confirm that subsidies are beneficial and are not wasted. Specifically, we find positive effects of subsidies on tangible assets of both treated LR and treated LA firms relative to control LR and control LA firms respectively and this impact persists even after five years. This result is not trivial. While subsidies obviously constitute a source of additional and cheap capital for treated firms and enable them to pursue their desired projects, control firms (especially control LA firms) may also be able to pursue the desired project using alternate sources of funding. Yet, the finding that both treated LR and LA firms benefit from subsidies underscores that subsidies matter for both firm types and also confirms that in general firms do not use subsidies for private consumption.

Next, we turn to the main issue of interest, which is the *incremental* impact of subsidies on LR firms *relative to* LA firms. As discussed above, this helps to distinguish between credit rationing as opposed to a scenario where loan rejection is based on a fair assessment of firms by banks. We find an incremental impact in terms of assets.[4] That said, the impact is not long lasting and follows an inverted U-shaped pattern: it increases and is significant for the initial three years and decreases thereafter. We uncover a similar pattern in case of profitability, while the statistical significance of the pattern is muted. Specifically, there is a positive response in firms' return on assets for two years

[3]These firms reveal their genuine need for financing by paying the fixed cost of a subsidy application, and therefore constitute a better control group than firms that did not apply. Muraközy and Telegdy [2023] use similar data and show that this control group greatly reduces pre-treatment differences between control and treated firms.

[4]This incremental impact is not mechanical. On the one hand, by construction, LA firms get a loan in the year prior to getting a subsidy, which may imply a smaller need for funding and artificially lead to an incremental impact in case of LR firms. On the other hand, however, the fact that LA firms apply for a subsidy reflects their genuine need for funding. Indeed, subsidy application costs money and also entails a skin-in-the-game requirement. Moreover, by comparing treated LA with control LA firms, any LA firm specific factors are accounted for in our triple-difference regression design.

after the subsidy is awarded, after which there is no impact. Relatedly, we do not find any incremental impact on the value of new loans taken out by LR firms, suggesting that subsidies do not improve the creditworthiness of LR firms.

These findings show that LR firms in our sample are unable to fully exploit the initial boost provided by subsidized capital. The findings thus point to poorer firm quality as the likely reason why some Hungarian SMEs were unable to get bank credit during the period under study. This implies that the soft information about firms that banks are able to observe or collect – which goes beyond standard disclosures like tax returns – and which banks embed in their loan acceptance or rejection decisions can constitute a valuable input in the design of subsidy programs.

Related literature Our article is related to two strands of literature. The first is concerned with an impact assessment of the subsidy programs. The second strand is dedicated to identifying differences in access to bank credit by SMEs.

A large literature has estimated the effect of subsidies or other types of credit supply shocks on firm growth using up-to-date econometric methods. These include instrumental variable techniques (Bach [2013]; Brown and Earle [2017]; Criscuolo et al. [2019]; Zia [2008]), randomized trials (Banerjee et al. [2015]; De Mel et al. [2008]), regression discontinuity design (Cerqua and Pellegrini [2014]), exogenous regional variation in bank credit supply (Jaume et al. [2021],Greenstone et al. [2020]), or natural experiments such as changes in eligibility rules for tax credits (Rao [2016]; Agrawal et al. [2020]).[5] On average, the evidence presented in this literature is that subsidies foster firm growth along a wide variety of outcomes (assets, employment, value of sales, and value added),[6] but they do not necessarily improve efficiency measures

[5]Several other related papers that study the impact of subsidy program in various EU economies are as follows: Dvouletý et al. [2021], Banai et al. [2020], Benkovskis et al. [2018], Dvouletý and Blažková [2019], and Muraközy and Telegdy [2023].

[6]There are, naturally, exceptions to this high-level takeaway. For example, Greenstone et al. [2020] finds no impact after a positive credit supply shock among SMEs in the US.

such as profitability.

Our paper shares this high-level takeaway in the sense that we find a significant impact of credit supply easing on asset growth but not on profitability. Our contribution is to zoom in and assess the differential impact of subsidies depending on firms' loan histories. In this sense, we complement the literature by showing how credit registry data can better understand the impact of subsidy programs and thus design more efficient programs in the future.[7]

A closely related paper which also combines credit registry data with a government subsidy program is Bellucci et al. [2023]. They show that outcomes from the subsidy program can improve credit conditions of firms vis-a-vis banks with which the firm has no existing relationship. This underscores that public signals about a firm can serve as a substitute for private information if the bank does not have such knowledge about the firm. Our paper seeks to answer a distinct question using a comparable dataset, which is whether LR firms can benefit more from subsidies than LA firms, and what that implies for where subsidies should be targeted.

Next, we turn to the large literature that aims to identify differences in firms' access to credit. Methods adopted in this literature include using firm balance sheet and profitability metrics to create an index of credit constraints (Whited and Wu [2006]; Hadlock and Pierce [2010]; Mulier et al. [2016]; Chiappini et al. [2022]); using investment-to-cash flow sensitivity to infer the ease of credit access (Kaplan and Zingales [1997]); concluding that a higher wedge between the cost of internal and external capital (Fazzari et al. [1988]) or between the marginal rate of return and the marginal cost of capital (e.g. Bach [2013]) reflects credit constraints; showing that firms that benefit from a directed lending program must have been credit constrained ex-ante ([Banerjee and Duflo, 2014]); using structural models to separate credit supply from demand (Kremp and Sevestre [2013]); demonstrating that higher informational asymmetry leads to higher interest rates [Derrien et al., 2016]; using variation

[7] Note, however, that the goal in this paper is not to pursue a cost-benefit analysis of the subsidy program, such as whether subsidies deliver greater benefits relative to the cost of the program (costs such as those that stem from the use of tax-payer money) or negative spillovers (say due to the impact on non-subsidized firms).

in credit demand when supply is fixed (Benmelech et al. [2019]); using variation in bank liquidity (Bentolila et al. [2018]; Chodorow-Reich [2014]; Huber [2018]; Paravisini et al. [2015]); using credit ratings (Caggese et al. [2019]); and using survey data (e.g. Gómez [2019]; Gorodnichenko and Schnitzer [2013]; Ferrando et al. [2017]; Mateut [2018]).

The heterogeneity of methods broadly reflects two different notions of credit constraints that researchers have in mind: a firm may have lack of access to credit even if it is willing to pay the market interest rate, or it may have access to external credit but at a price that is higher than the opportunity cost of firm's internal capital [Farre-Mensa and Ljungqvist, 2015]. This paper does not favor one or the other notion of credit constraints. Instead, it uses credit registry data to identify firms that are unable to get a bank loan (LR) and use that as a factual description of its state, contrasting it with the state of firms that are able to get a loan (LA). We stress that LR firms *may or may not be* credit constrained. Indeed, if an LR firm is of good quality (i.e. has positive net-present-value (NPV) at market interest rate) but unable to get a loan due to insufficient or noisy credit history, it would be credit constrained. However, if an LR firm were of poor quality (e.g. has negative NPV at market interest rate), then such a firm is not credit constrained as per the common definition in the literature.

The remainder of the paper proceeds as follows. In Section 2, we describe the institutional framework of the EU subsidy program. Section 3 presents a stylized model to guide the empirical approach. In Section 4 we describe the three datasets used in the study. We then describe our approach to identifying firms with poorer access to bank credit in Section 5. We present the empirical methodology in Section 6. In Section 7, we present the results of our analysis, and Section 8 concludes.

2 The European Union's subsidy program in Hungary

The European Union's (EU) Structural Cohesion Fund programs aims to reduce disparities within the Union. These programs provide subsidized funding to regions in the EU where per capita GDP is below a certain threshold. The expectation of these programs is that subsidized funding can increase employment and foster sustainable growth in these economies.[8] In turn, the programs can help to close the gap with more advanced regions. During the 2007-2013 programming period, almost 25 billion euros (HUF 7000 billion, or almost a quarter of the annual GDP in 2013) were allocated to Hungary through these programs [Boldizsár et al., 2016]. Most of the available budget was allocated to public projects (such as infrastructure development, energy grid mobilization, or improving education), but about a quarter (6 billion euros or 1800 billion HUF) were given directly to Hungarian firms. In this paper we focus on these firm-level subsidies.

While the stated purpose of the EU subsidy program was to decrease regional disparities across the EU, the goal of national governments was to allocate all the available funds and to target firms that will not waste them. As such, national governments strove to attract better firms. Indeed, Muraközy and Telegdy [2023] demonstrate that there was a positive selection into application, that is, applying firms were relatively bigger and more productive than those that did not apply.[9]

The distribution of the EU funds followed a hierarchical structure. Funds were allocated to several Operational Programs, which were divided into Measures and Sub-Measures. A Sub-Measure typically issued several *calls* for proposals, which informed the interested parties about the available funds, the

[8] More specifically, the programs are based on three pillars, namely (i) capacity building and support for the transition to sustainability and digitalization; (ii) reducing regulatory burden and improving market access; and (iii) improving access to financing. See Commision [2020] for more details.

[9] Corruption in the allocation of the subsidies was unlikely to be systemic since there were many such transfers and usually involved small sums.

activity to be funded, the eligibility criteria, the minimum and maximum grant size, the proportion of the project that can be subsidized, and the evaluation process.

While each call had a fairly specific purpose for which subsidies were given, there were many calls, which means that firms were likely to find a call that was suitable for their needs. This also meant that subsidies were a reasonable substitute for loans. For example, the calls had purposes such as the purchase of new machinery or IT equipment, construction of new production facilities, R&D expenditures, insulation of buildings, and worker training. Among these, we focus on those calls which directly financed activities that affect the scale of production.

Funding was provided in the form of a non-refundable grant (i.e. money which need not be paid back). To ensure that firms applied for subsidies for genuine use cases and to avoid waste, subsidies typically only covered between 30 and 50 percent of the cost of investment while the remaining funding requirement had to be covered by the firm applying for the subsidy. In other words, firms had to commit skin-in-the-game. The exact share required to be paid by the firm was specified in the calls – the firms could not alter this.

In general, the program targeted small investments. The vast majority (95 percent) of the subsidy requests were below EUR 0.45 million (HUF 160 million), and the median was about EUR 0.03 million (10 million HUF). That said, because the target firms were generally small and medium-sized enterprises, these subsidies still represented a significant source of funding for them (see Figure A.1 in the Appendix).

About 31 thousand firms applied for these subsidies during the 2007-13 period, some applying multiple times. More than two-thirds of the applicants received a subsidy.[10]

The evaluation process for the majority of the subsidy applications was streamlined. The decision-making process for small subsidy applications (which

[10]For the yearly distribution of successful and rejected applicant firms, as well as the distribution of number of winnings per firm, see Tables A.2 and A.3 in the Appendix. Note that the typical annual subsidy amount was close to 2% of annual SME credit during this period – therefore, it is unlikely that subsidies crowded out credit to SMEs.

is the typical subsidy in our sample) was essentially automated. As such, each application that satisfied the eligibility criteria received funding until the allocated funds were depleted. The median time for making a decision after the submission of an application was less than 4 months, while the time needed to sign the contract after a positive decision was approximately one month.

These operational modalities of the program, wherein subsidies were not aimed at certain groups of firms, helped limit the scope for selection bias in subsidy awards. In particular, there was no preference given to the so-called LR or LA firms, nor was there a preference to grant specific projects to some but not other firms. Indeed, as we show later in Table 1, the fraction of firms that received or did not receive bank loans in the year before the subsidy application is similar between the winners and losers of the subsidy. As we elaborate in Section 6 on the empirical design, this helps strengthen our identification.

Business environment. Non-repayable subsidies are always an attractive financing means for firms, but their effectiveness in terms of improving firm-level outcomes depends on the general business environment. If credit is abundant and interest rates are low, most firms can secure financing without subsidies. If, on the contrary, it is hard to get credit or interest rates are high, subsidies can be more useful, especially for the credit restricted firms. While the assessment of how credit constrained Hungarian firms were during the period under study is beyond the scope of this paper, we review several macroeconomic indicators to describe the business environment surrounding the firms we study.

Appendix Table A.1 presents the growth rate of GDP, the inflation rate, corporate debt relative to GDP, the yearly change of corporate debt, and interest rates for the years when the firms in our sample applied for subsidies (2011-2014). Hungary was hit hard by the great financial crisis and also by the European debt crisis. It resulted low GDP growth (well below its potential level) for years and initially high inflation relative to the central bank target of 3 percent. Inflation eased eventually, but interest rate conditions remained relatively strict for most of the period. Lending activity of the banking sector stagnated for years after the great financial crisis. In part, this was because of a lack of ability to lend: the banking sector suffered huge losses, it relied strongly on foreign funding, and a big adjustment was needed both on capital and liquidity side. At the same time, an uncertain economic environment decreased the willingness of banks to lend. As a result, corporate debt was scarce during most of the years studied in the paper and firms likely struggled to get funding.

3 Impact of subsidy on firms: A stylized model

A key issue when assessing the impact of non-repayable subsidies on firm-level outcomes is whether subsidies improve by more the outcomes for firms that previously had poorer access to credit. As discussed in the introduction, the answer to this question is not obvious. In this section, we present a stylized model to develop an understanding of the potentially heterogeneous impact that subsidies may have on different types of firms. We use the model to draw empirically testable implications.

Let us consider a continuum of firms. They have the same capital endowment k and have access to an investment opportunity that requires a fixed cost c and exhibits linear returns to scale otherwise. The firms differ, however, in terms of their productivity θ.

Each firm approaches a bank for a loan b. Bank loans are subject to a leverage or loan-to-value (LTV) limit. That is, if a loan is granted, the amount is an exogenously given multiple x of the own funding the firm puts in (skin-in-the-game).[11]

The bank cannot observe firms' productivity but can obtain a noisy normally distributed signal $\hat{\theta}$ with mean μ and standard deviation σ. These noise parameters μ and σ are firm-specific. For instance, in case of young-firms and those with poor credit history or corporate governance, banks may be less optimistic and more uncertain about firm quality. For such firms, the bank

[11] For instance, an LTV ratio of 90% would translate into a value of x = 10. While ideally x could depend on θ, but for the sake of brevity of exposition, we abstract away from this possibility, not least because θ is unobservable to the bank.

may associate a lower μ and/or a higher σ.[12]

The bank issues a loan b to the firm at interest rate r if the risk-adjusted return for the bank is above a threshold.[13] We compute the risk-adjusted return as follows. First we compute the bank's marginal return, which depends on whether the borrowing firm is able to cover its liabilities or not:

$$\begin{aligned} R^b &= r && \text{if the firm remains solvent: } \hat{\theta}(b+k-c) \geq rb \\ &= \frac{\hat{\theta}(b+k-c)}{b} && \text{if the firm becomes insolvent: } \hat{\theta}(b+k-c) < rb \end{aligned}$$

Note that while the firm's true productivity parameter θ is fixed, the marginal return R^b from the bank's perspective is a function of $\hat{\theta}$ and is stochastic. In particular, from the bank's point of view, the firm can fail with a probability $\mathcal{F}(\theta^*; \mu, \sigma)$ where θ^* determines whether a firm is solvent or not: $\theta^*(b+k-c) = rb$ and $\mathcal{F}$ is the cumulative distribution function for a normal random variable. It is useful to note that θ^* is decreasing in k, that is, firms with more capital endowment (or skin-in-the-game) are less likely to fail from the bank's perspective, which makes them more creditworthy. This mechanism is key for having a role of subsidies in the model.

Having defined the marginal return R^b, the risk-adjusted excess return for the bank from giving a loan to the firm whose perceived productivity is characterized by parameters μ and σ is:

$$\tau(\mu, \sigma) = \frac{\mathbb{E}[R^b] - R^f}{Std[R^b]}.$$

Here $\mathbb{E}[R^b]$ is the expected value of R^b, $Std[R^b]$ is its standard deviation, and R^f is a reference or hurdle rate. As such, τ is akin to a Sharpe ratio. We assume that the bank grants the loan if the risk-return perception of the firm

[12]We do not specify an explicit mapping from θ to μ and σ as this is not needed for the discussion of the model. That said, it is reasonable to assume that a higher θ is generally associated with a higher μ and a lower σ.

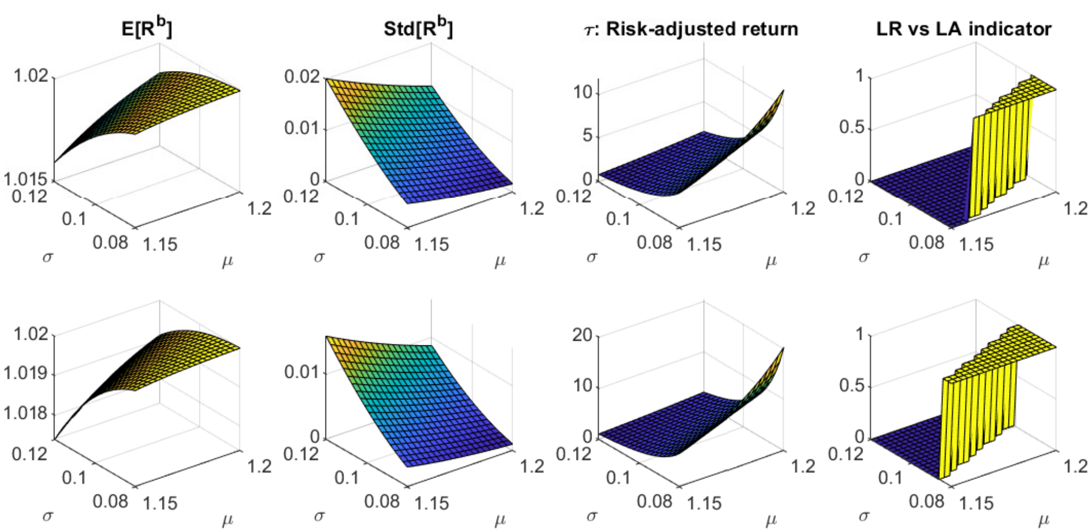
[13]In this simple stylized model, we take this threshold as given. In a competitive economy, this threshold would typically be market determined.

is sufficiently favorable, as determined by a cutoff τ^*:

$$\tau(\mu,\sigma) \geq \tau^* \quad \text{Loan application accepted (LA)}$$
$$\tau(\mu,\sigma) < \tau^* \quad \text{Loan application rejected (LR)}$$

The model captures the idea that as σ increases, a higher μ is needed to compensate for the risk and induce the bank to grant a loan to the firm (as illustrated numerically in Figure 1, last column panels). The model also covers the possibility that two firms with the same productivity can have different credit statuses due to different perceived riskiness by the bank. For example, despite the same θ, a firm with lower μ (or higher σ) can be LR while another one with higher μ (or lower σ) can be LA.

Figure 1: **Numerical illustration of model outcomes: With and without subsidies**



Notes: The top row panels illustrate how the perceived riskiness of the firm, as captured by μ and σ (and irrespective of the true productivity θ, which is not shown) translates into its ability to get bank loans. As σ increases, a higher μ is needed to compensate the risk and induce the bank to grant a loan to the firm. The bottom row panels show that a subsidy improves the creditworthiness of firms on the margin, and enables more firms to get loans. The illustration is based on the following numerical values assigned to the parameters: $k = 1, r = 1.02, R^f = 1, \tau^* = 5, c = 0.5, x = 10, q = 1$.

In case a firm's loan application is rejected (LR), it simply pursues the investment using the capital endowment k, while the loan accepted (LA) firm operates at a scale $k + b$, where, recall that the loan amount is equal to the multiple x of the own funding provided by the firm: $b = kx$.

Now consider that some firms, irrespective of their LR versus LA status can win a subsidy of a fixed amount q.[14] How does this affect outcomes for LA and LR firms differently?

LA firms are sufficiently creditworthy and can already get a bank loan, which means that if they do not win a subsidy, they can still pursue the investment at scale $k + b = k(1 + x)$. If they do win a subsidy – which adds to their capital endowment – it enables them to expand their investment to $k + q + b = (k + q)(1 + x)$.

In case of LR firms without a subsidy, the firm would simply operate at scale k. With a subsidy, some but not all previously LR firms can become LA, as illustrated in Figure 1. Indeed, on the one hand, the subsidy may not improve the LR firm's creditworthiness in a material way (either because the subsidy amount is small or the firm's creditworthiness was quite poor to begin with). In this case, the LR firm would still be unable to get a loan, and the subsidy would only lead to a one-to-one increase in its investment scale, i.e. from k to $(k+q)$. On the other hand, the subsidy may sufficiently improve the creditworthiness of the LR firm and enable it to get a loan. As discussed above, in this case, the subsidy adds to the capital endowment of the firm, reduces θ^*, and makes it more loan worthy from the bank's point of view). In this case, the firm is able to pursue the project at a larger scale $(k + q)(1 + x)$.

Taken together, the model delivers the following two takeaways which we test empirically in the following part of the paper.

First, subsidies enable both LR and LA firms, irrespective of their true productivity, to increase their assets. This is because while LA firms increase their assets from $k(1 + x)$ to $(k + q)(1 + x)$, LR firms increase their assets from

[14]That is, we assume that the criteria for winning a subsidy is different from the criteria for getting a bank loan. Indeed, banks (who tend to maximize profits) and governments (who tend to give subsidies for benevolent reasons) have different objectives.

k to $k+q$ or to $(k+q)(1+x)$ depending on whether subsidies ease credit access and enables some LR firms to get a loan. Assuming that for a fraction $\alpha > 0$ of LR firms subsidies do enable getting a bank loan, then the relative increases in assets for the two groups of firms is given as:

$$\underbrace{\frac{(k+q)(1+x)}{k(1+x)}}_{\text{LA firms}} < \underbrace{\frac{\alpha(k+q)(1+x)+(1-\alpha)(k+q)}{k}}_{\text{LR firms}}$$

Second, consider two firms with the same true productivity θ, but one is LR because its perceived risk-adjusted return by the bank is low while the other is LA. Then, the increase in profitability for treated LR firms relative to control LR firms is larger than that in case of LA firms. The change in return on assets (ROA) of LR and LA firms due to a subsidy is respectively given as follows:[15]

$$\Delta ROA^{LR} = \frac{\theta^{LR}(k+q-c)}{k+q} - \frac{\theta^{LR}(k-c)}{k} = \theta^{LR}c\left(\frac{1}{k} - \frac{1}{k+q}\right)$$

$$\Delta ROA^{LA} = \frac{\theta^{LA}((k+q)(1+x)-c) - r(k+q)x}{(k+q)(1+x)} - \frac{\theta^{LA}(k(1+x)-c) - rkx}{k(1+x)}$$
$$= \frac{\theta^{LA}c}{1+x}\left(\frac{1}{k} - \frac{1}{k+q}\right)$$

It is immediately clear that

$$\theta^{LR} = \theta^{LA} \implies \Delta ROA^{LR} > \Delta ROA^{LA}.$$

A corollary of the above result is that if subsidies do not increase the profitability of LR firms by more than that of LA firms, then LR firms must have been less productive (i.e. have a lower θ) to begin with.

[15]For the simplicity of exposition, we consider the case where the subsidy *does not* sufficiently improve the credit worthiness of LR firms and thus does not enable the LR firm to get a loan. The increase in ROA for LR firms is larger if the subsidy enables them to get loans, which reinforces the key takeaway.

4 Data

Data sources and variables We build a novel firm-level database by linking three administrative sources via a firm-specific tax identifier. The first database is the Unified Monitoring Information System of the Prime Minister's Office in Hungary, giving us access to all subsidy applications falling under the Economic Development and the Regional Development Operational Programs during the 2007–2013 programming period. We observe the date of submission, the requested amount of the subsidy, the date of evaluation, the outcome for each subsidy application (successful or rejected), and the payment date(s). Furthermore, the database contains additional information such as the subprogram under which the application was submitted and the own contribution of the applicant.[16] We use the subprograms to select grants that directly affect production activities, such as the purchase of machinery, building construction, or investment in information and telecommunication technology.

The second database is the Central Credit Information System (CCIS), which contains loan histories of firms. The aim of the CCIS is to have a repository that banks can query to learn about the debts and payment delinquencies of their existing or potential clients. For this purpose, each bank and financial institution is required by law to enter their loans and loan-like contracts into the CCIS. As such, the database contains information on all outstanding loans disbursed by banks and non-bank financial institutions in Hungary. The available variables include an identifier for the lending institution and the borrower, loan origination date, and the duration and size of the loan.[17]

[16]For more information on the subsidy programs and the Unified Monitoring Information System dataset, see Banai et al. [2020].

[17]The database, and in particular, the version used in this paper, has gone through a number of changes over the years. Between 2010 and 2012, the data contain the identity of the borrower. Between 2012 and 2015, the identity of the borrower became anonymized, making it no longer possible to link firms' lending contracts with other databases. However, there was a law change in 2015 that allowed the Central Bank to see the identity of the borrowers in the data, which allowed it to link this information with other databases, even retrospectively. Moreover, since information entered into the CCIS is retained for a period of 5 years after the end of the contract, we are able to know each borrower's ongoing contracts at any time after 2010. For further information on this dataset, see Banai et al. [2016].

A special feature of this data set is that since 2012, it has also provided information on the queries initiated by banks to the CCIS. The query may serve two purposes: checking on other credits taken by an existing client or vetting future clients' credit histories before disbursing a loan (in fact, it is mandatory in Hungary for banks to query the credit history of firms before issuing a loan). Each query entails a cost of 2-3 euros for the financial institution, so these queries are made with a definitive purpose. As described in the next section, the data on credit queries are at the core of our approach to identifying firms that applied for but did not receive a loan.

We focus on loans that are likely intended for investment purposes. Our source data do not provide information on the purpose of the loans. However, by focusing on longer-term loans (original term more than a year), we are able to rule out a majority of those that are likely meant for working capital or to solve imminent liquidity issues.

The third database that we use is maintained by the National Tax and Customs Administration and covers all enterprises subject to double-entry bookkeeping, thereby covering almost all Hungarian SMEs. The data contain information on the balance sheet, income statement, and other information such as the number of employees and industrial classification.

Sample construction The starting point of our sample creation is the set of firms that applied for the subsidy program at least once. By restricting our attention to applicant firms (both winners and losers) rather than considering the universe of firms in Hungary, we ensure that rejected applicants – by revealing their funding needs – constitute a better comparator group for winning applicants. We use only those subsidy applications that are meant to fund production activities, and focus on firms that made subsidy applications between 2012 and 2015. This is because credit query data, which is key for the construction of LR and LA groups (described in Section 5), are only available 2012 onward. Among the set of applicant firms, we focus on for-profit non-financial firms with available financial information. In addition, we only consider firms that are classified as SMEs.[18] This is because SMEs are the main target of the subsidy program, and also because very large firms can access alternative sources of finance. We also exclude very small firms (those with less than 5 employees) as data quality is less reliable for these firms. Firm-year observations with seemingly erroneous data, namely negative leverage or sales revenue, or pretax ROA of more than 200 percent in absolute value, are also dropped (there were very few such observations). We also exclude firms from sectors with very few firms that applied.[19] Given that idiosyncratic factors may play a role in the success of applications with very large requested subsidies, we exclude applications where the subsidy amount is greater than HUF 500 million.[20] Finally, we deflate sales and profits using the producer price index and the value of debt and tangible assets using the investment price index.

Appendix Table A.4 provides a detailed description of the number of applicant firms and observations at each step of the data preparation process.

5 Identifying loan-rejected firms

We use the Credit Registry to construct a binary variable that classifies firms as either loan-rejected (LR) or loan-acquired (LA). Firm-level access to credit can vary over time (for example, due to changes in macroeconomic conditions), which means that we need to classify firms as LR or LA on a particular date of interest. We focus on the date when firms apply for a subsidy, as this is the time when differences in access to bank credit are relevant to how valuable a subsidy is to them. We define a firm as an LR if it applied for a bank loan but did not receive it during the year before the date of the subsidy application.

[18]We use the European Commission definition of an SME, that is, enterprises which employ fewer than 250 persons and which have an annual turnover not exceeding EUR 50 million, and/or an annual balance sheet total not exceeding EUR 43 million.

[19]These sector are the following: mining and quarrying; electricity and gas, steam and air conditioning supply; water supply, sewerage, waste management and remediation activities; financial and insurance activities; public administration and defense, compulsory social security; activities of households as employers; activities of extraterritorial organizations.

[20]Also, in the case of applications with a large requested amount decision-making is not automated, and political considerations may also play a role.

We define a firm as an LA if it obtained one or more loans during the same period.

We choose a one-year window to focus on the recent (and thus more relevant) credit access status of a firm. Indeed, we are interested in assessing the impact of subsidies on firms that had expressed their need for a bank loan in the recent (not so distant) past but were unable to secure one.[21]

Although the definition of an LR firm is intuitive, we do not have data on loan applications *per se*. That said, queries made by banks in the Credit Registry to assess firms' credit profiles allow us to get around this limitation. These credit queries are likely to reflect a loan application from the firm for the following reasons. First, while banks may check the credit history of existing clients for monitoring purposes, they have a strong incentive to check the credit history of a potential *new* client firm after receiving a loan application (as also emphasized in Jiménez et al. [2012]). Indeed, for new clients, it is standard practice for banks to collect as much hard and soft information as possible to reduce information asymmetry and thus reduce credit risk. Second, Hungarian banks are forbidden to use the credit registry to select firms to approach with loan offers. Thus, banks have no reason to conduct credit inquiries (which cost banks) for *unrelated* firms unless the firm files a loan application or approaches the bank for information about the possibility of a loan.[22] Therefore, a credit query by a bank on an *unaffiliated* firm is highly likely to mean that the firm applied for a loan at the bank. For these reasons, we classify a firm as LR if an unaffiliated bank issued a credit query on their behalf in the one-year period prior to the subsidy application, but this firm received no new loans from any bank during this period. Firms that did receive a loan during period are classified as LA.

[21]The choice of a one-year window is also guided by the average number of new loans per year by firms in our sample, which is 0.82. This implies that the mean duration between loans is about 15 months. See Figure A.2 in the Appendix for the loan frequency distribution. In any case, we test our results with alternative time windows in the robustness analysis.

[22]It is possible that banks initiate a query on major partners or clients of firms that apply for a loan, which would result in a measurement error in our variable. However, we expect such instances to be uncommon in our sample, as most firms are small and unlikely to be key trade partners of other firms.

Table 1: **Distribution of firms in our sample**

	Never won a subsidy	Won a subsidy at least once	Total
LR	184 (30%)	433 (31%)	617 (30%)
LA	434 (70%)	974 (69%)	1,408 (70%)
Total	618 (100%)	1,407 (100%)	2,025 (100%)

Notes: The table shows the number and percent of firms in our sample by outcome of subsidy application and credit access status. The sample excludes firms whose credit access status is unknown at the time of application (847 firms).

Table 1 shows the number and proportion of firms by application status and credit access. We are able to classify about two-thirds of the applying firms as LR or LA. Among these firms, 30 percent are LR and 70 percent LA at the time of applying for a subsidy. The proportion of LR and LA firms is approximately the same among successful and rejected subsidy applicants.

The comparison of financial information of the LR and LA firms shows that LR firms have a 42 percent lower value of tangible assets (see Table 2). They also have lower sales and lower labor productivity. That said, they have comparable return on assets, age, number of employees, and also request for similar subsidy amounts. Also, by construction, LR firms did not take out any loans in the pre-treatment year while LA firms did. Relatedly, LR firms have a somewhat lower debt to assets ratio.

To shed light on how firm characteristics relate to loan rejection more formally, we run a linear probability model with the dependent variable being a dummy that takes the value 1 if the firm is LR and 0 otherwise. The estimated coefficients presented in Table 3 suggest that larger firms (as measured by the value of tangible assets) and firms with higher ROA are less likely to face loan rejections, which makes intuitive sense. LR firms also tend to have significantly less debt (relative to assets) as compared to LA firms.[23] At the same time,

[23]Higher ex-ante leverage – especially beyond a threshold – can deter a firm's ability to get more loans. However, the higher indebtedness of LA firms reflects their ability to take more loans in general, as also shown in Figure 2 below.

Table 2: **Comparison of LR and LA firms**

	LR		LA	
	Mean	St. dev.	Mean	St. dev.
Tangible assets	138.7	299.6	198.4	696.1
Employment	24.16	36.84	24.23	33.35
Firm age	12.77	6.23	11.93	6.48
Return on assets	0.11	0.16	0.11	0.15
Debt to assets	0.51	0.26	0.56	0.23
Requested subsidy to tangible assets	0.47	0.38	0.51	0.38
Sales	632.8	186.1	788.1	177.3
Labor productivity	30.3	66.9	36.6	127.8
Number of new loans	0	0	2.83	7.33

Notes: The table presents summary statistics of LR and LA firms' characteristics in the pre-treatment year. Return on assets is the pre-tax profit relative to average total assets in the pre-treatment and it's previous year. Profits and sales are deflated with producer price index, and debt and tangible assets are deflated with the investment price index. Labor productivity is the ratio of sales and employment. Tangible assets, sales, and labor productivity are in HUF millions, while firm age is in years.

firm age does not seem to matter.[24]

There are several potential reasons for why LR and LA firms look less dissimilar than one might expect. The firms in our sample are similar to each other by construction in the sense that all of them have revealed their need for funding – by paying the cost of applying for a subsidy – while also seeking a loan. Thus, they tend to be the 'better' SMEs in Hungary. Indeed, Appendix Table A.5 shows that firms outside our sample (that is, they are from the same size and industrial distribution but did not apply for a subsidy, nor did we place them in the LR or LA groups), tend to be smaller in terms of employment as well as assets and have substantially lower sales, profitability, and productivity than firms in our sample. In addition, banks assess credit worthiness and approve/reject loans not only on the basis of obvious observable features such

[24]Note that the firms in our sample are typically old, with the average age being 12 years. This is because very young firms (with less then 2-3 years of operation) are not eligible for subsidies.

Table 3: **Characteristics of loan rejected firms**

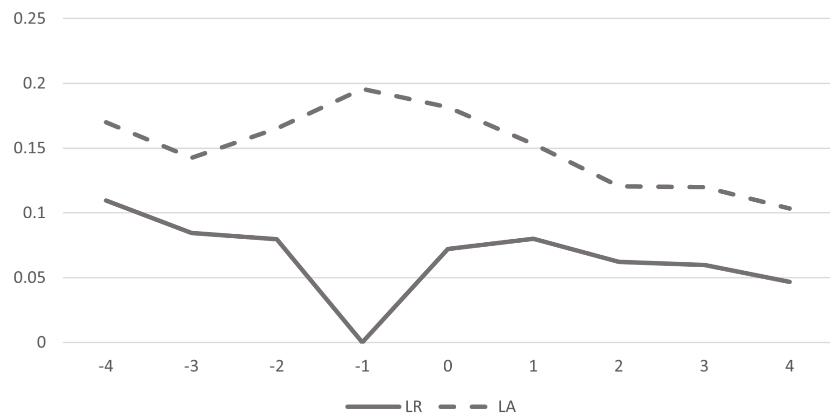
Variable	Coefficient
Tangible assets	-0.013***
	(0.004)
Employment	-0.002
	(0.007)
Firm age	1.85e-6
	(2.61e-6)
Return on assets	-0.067**
	(0.032)
Debt to assets	-0.122***
	(0.023)
N	7,706
Adjusted R^2	0.064

Notes: The table presents the results of a linear probability regression with dependent variable equaling 1 if the firm's loan application was rejected by the bank, and equaling 0 if the firm received a loan. The sample consists of all observations prior to the year in which firms apply for the subsidy. Tangible assets and employment are in logs. Sector and year fixed effects are included. The mean value of the dependent variable is 0.44. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

as those reported in Table 2, but also other features such as collateral and credit scores (that we cannot observe and where starker differences are to be expected) and intangibles such as project quality and governance (which are also something we do not observe but banks do).

In what follows, we comment on three crucial aspects of our LR versus LA classification strategy. First, a potential issue with our classification is that it can pick up cyclical patterns, such as temporary or intermittently poor access to bank credit. This is because we rely on loan status only in the one-year period before the subsidy application date. It is possible, however, that firms change status over time. If so, this would suggest that our classification did not

Figure 2: **Value of new loans (relative to assets) of LR and LA firms**



Notes: The figure presents the value of new loans relative to firm assets for LR and LA firms. Event time is measured relative to subsidy application year.

capture something intrinsic about the firms, and rather a transitory feature.[25] To scrutinize our classification, we assess the evolution of new loans (relative to assets) taken out by the LR and LA firms, respectively. The results of this exercise are presented in Figure 2, where year 0 is the subsidy application year. The figure demonstrates that throughout the sample period LR firms receive much less credit than LA firms. The magnitude of this difference varies between 5 and 10 percentage points. Furthermore, it is reassuring that even in the post-application period, un-subsidized LR firms continue to get less credit than un-subsidized LA firms. All of this suggests that our LR definition captures poor access to credit more generally, and not only in the year before the subsidy application.

Second, a firm that we classify as LA may not have received as much funding as it requested. Although this can bias downward the difference in estimated incremental impact on LR relative to LA firms, the fact that we still find a significant impact in terms of the level of assets (in Section 7) means that the

[25]The worst-case scenario for our identification would be that firms get loans randomly, so those which did not get one in the year before application were simply unlucky. This is, however, highly unlikely in practice.

bias is likely to be limited. Moreover, we expect this effect to be small, as the loan amount – in practice – does not deviate much from the requested sum.[26]

Third, roughly 30 percent of the firms in our sample are unclassified. Most of these are firms with no queries and no loans in the year before application. Their status is ambiguous. They could be effectively LA firms because they did not need a loan and hence did not apply for one. Or they could be effectively LR firms if they needed a loan, but discussions with bank officials made clear that they will not get a loan, and therefore they did not submit a formal loan application. Some firms may not even contact the bank because they anticipate rejection. Some of the firms in the unclassified group will also be such that they have queries by banks with existing ties, meaning that they might have applied for an additional loan at their existing bank, but received no loans afterwards. As discussed before, while this scenario would effectively mean a loan rejection, if such firms approach other (new) banks for loans, they would get classified as per our baseline strategy.

In either case, while in our main analysis we exclude unclassified firms, in Section 7.1, we construct a proxy of whether they are LR or LA firms using a Logit model. The idea of the model is to use firm level characteristics to infer for each firm in the non-classified group whether it is more likely to be an LR or LA firm. This allows us to run the baseline analysis on the whole sample and examine the robustness of the baseline results.

6 Estimation procedures

To estimate the effect of subsidized investment on LR and LA firms, we employ a difference-in-differences framework and run panel data regressions. Our main interest lies in the effect of the subsidy on tangible assets (which is directly affected by the subsidy), but also on firm performance (i.e., return on assets) and the value of new loans.

[26]Banks may offer a lower amount based on credit worthiness of firms but they also consider the needs of the company as it is essential for the payback. As a result, there cannot be a large deviation between the amount applied for and the one received.

We define a firm as treated if it applied for and won a subsidy at least once during the sample period, and we consider the year of the first successful application as the reference (i.e., treatment) year.[27] Firms that applied but never won a subsidy form the control group, and the first application date is taken as their reference year.[28] Unlike other studies that tend to use non-applicant firms as controls, our approach ensures that the treated and control groups are *ex-ante* as similar as possible. This is because firms that apply for a subsidy reveal their need for funding by paying both the fixed cost of subsidy application and, in case of winning, the own contribution in the investment (which is typically at least 50 percent).

Table 4: **Comparison of characteristics of treated and control firms**

	Treated		Control	
	Mean	St. dev.	Mean	St. dev.
Tangible assets	10.63	1.73	10.71	1.96
Employment	2.63	0.92	2.67	0.99
Firm age	11.28	6.07	11.13	6.85
Return on assets	0.11	0.13	0.10	0.13
Debt to assets	0.54	0.24	0.57	0.24
Requested subsidy to tangible assets	0.46	0.38	0.54	0.40
Sales	12.5	1.3	12.6	8.4
Labor productivity	9.80	0.98	9.85	1.01
Number of new loans	2.22	6.56	2.24	5.46

Notes: The table presents summary statistics of treated and control firms' characteristics in the pre-treatment year. Return on assets is the pre-tax profit relative to average total assets in the pre-treatment and it's previous year. Profits and sales are deflated with producer price index, while tangible assets and debt are deflated with the investment price index. Labor productivity is the ratio of sales and employment. Tangible assets, employment, sales, and labor productivity are logged, while firm age is in years.

[27] We consider the year of the application as the reference instead of the year of the decision, contract-signing or the first payment because beneficiaries were allowed to start subsidized projects from the application onward. As such, choosing the application as a reference eliminates the possibility of observing the impact of treatment before the reference date.

[28] Additional successful applications may appear in the post-treatment period of a treated firm and reinforce the treatment effect. We test whether this matters for our results by dropping firms with multiple subsidies from the sample in Section 7.1. Muraközy and Telegdy [2023] use longer time series of the same data on subsidies and compute the effect of single and multiple subsidies on a large set of firm outcomes.

We consider four years of firm history before and after the reference year (9 years in total) to assess pre-treatment trends and the subsidy impact in the post-treatment period. Table 4 shows that during the pre-treatment year, the two groups are quite similar in terms of various firm level characteristics. The only somewhat material difference is in the requested subsidy amount to tangible assets ratio – the same is higher for control firms and may reflect that firms which asked for much higher subsidies relative to their assets tended to be turned down.

To further investigate potential selection of firms into winning a subsidy, we run a linear probability model with a dependent variable being a dummy that takes the value of 1 if the firm received the subsidy. Several firm characteristics serve as left-hand side variables. The regression result presented in Table 5 reinforces the comparison of means in Table 4, which implies that the control and treated are not very different from each other, except in terms of their debt levels. Also, the weakly significant difference in employment and return on assets suggests that the subsidy program favors smaller and more profitable firms.[29] A key reason for the similarity of the control and treated groups is that firms that apply for subsidies constitute a more uniform group of treated and control firms (which is the approach we follow) than having non-applicants included in the control group.

To assess the effect of subsidies on the value of tangible assets, we run the following regression:

$$y_{i,s,T,t} = \beta \left[post_T \times treat_i \right] + \tau_T + \alpha_i + \delta_{s,t} + \varepsilon_{i,T} \tag{1}$$

Here, i indexes firms, s denotes the two-digit industry NACE code, t indexes calendar years, and $-4 \leq T \leq 4$ indexes years *relative* to the reference year (which is set to 0).[30] $treat_i$ is a binary variable that equals 1 throughout the

[29]The mean value of the dependent variable equals 0.59, so the estimated selection effects are not very large. For example, a difference of 10 percent in the debt level decreases the probability of winning by 1.4 percent.

[30]Firms with fewer than 4 pre-treatment or post-treatment years would naturally have a smaller weight in the regression estimate and may bias our result. However, less than 10 percent of the firms belong to this category. Moreover, our results remain robust to excluding

Table 5: **Characteristics of subsidy winners**

Variable	Coefficient
Tangible assets	0.002
	(0.004)
Employment	-0.013*
	(0.007)
Firm age	1.99e-7
	(2.62e-6)
Return on assets	0.057*
	(0.032)
Debt to assets	-0.086***
	(0.023)
N	7,706
Adjusted R^2	0.041

Notes: The table presents the results of a linear probability regression with dependent variable equaling 1 if the firm won a subsidy and equaling 0 if the firm did not win a subsidy. The sample consists of all observations prior to the year in which firms apply for the subsidy. Tangible assets and employment variables are in logs. Sector and year fixed effects are included. The mean value of the dependent variable is 0.59. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

sample period if the firm i received a subsidy (0 otherwise) and $post_T$ is set to 1 for the post-treatment years, that is, $T \geq 0$ (0 otherwise). The effect of the subsidy is measured by β. We control for τ_T (a set of dummies relative to the reference year) to attenuate the potential bias of selection towards winning, for α_i (firm-fixed effects) to address systematic differences across firms, and $\delta_{s,t}$ (sector-calendar year fixed effects) to account for macroeconomic changes including the variation of industry-level prices.[31] We cluster standard errors at

these firms.

[31] A recent literature (De Chaisemartin and d'Haultfoeuille [2020]) demonstrates that the two-way fixed effect model may produce biased estimates if the timing and intensity of treatment vary across units. We do not expect this to cause a serious problem in our analysis because 98 percent of treatment events occur very close to each other, that is in 2012 and

the firm level.[32]

To assess the effects of the subsidy on LR and on LA firms separately, we follow two approaches. First, we run Equation (1) individually on the subsamples of LR and LA firms. The advantage of this approach is its full flexibility in the sense that relative-time and sector-year fixed effects are estimated separately for the two groups. Second, to assess the statistical significance of the incremental effect on LR relative to LA firms, we adopt a triple interaction specification, where we include interactions between $treat_i$, $post_T$, and a binary LR_i that takes the value 1 for LR firms:

$$\begin{aligned} y_{i,s,T,t} = \gamma\, [post_T \times treat_i \times LR_i] + \gamma_1 [post_T \times treat_i] + \\ \gamma_2 [post_T \times LR_i] + \tau_T + \alpha_i + \delta_{s,t} + \varepsilon_{i,T} \end{aligned} \tag{2}$$

To maintain the flexibility of the controls, we include the interaction between the treatment dummy and post dummy, and also the post dummy and LR dummy. The coefficient of interest in this regression is γ, which shows the incremental effect of the subsidy on LR relative to LA firms.

Matching To enforce a common support of the variables between treated and control groups and to further diminish any pre-treatment differences, we also run the main specification on a matched sample. We perform a combination of exact and propensity score matching. First, we impose exact matching by LR and LA categories, 2-digit NACE sectors, three firm-size categories, and terciles of tangible asset growth (we use the mode of each variable during the pre-treatment period). Then we estimate a logistic regression to produce a propensity score for each firm based on its pre-treatment characteristics. As explanatory variables, we use the pre-treatment mean of a variety of firm observables, as well as the ratio of the requested amount of subsidy to the value of tangible assets, the treatment year, and a dummy indicating that the

2013.

[32] The estimation may affect control firms if winners increase their market share at the expense of the control firms. Because only a small group of Hungarian SMEs was treated in our analysis, it is unlikely that such a spillover effect would be substantial.

firm has rejected loans.[33] The estimated coefficients and the corresponding standard errors are presented in Appendix Table A.6. In the groups created by exact matching, we use the propensity score to match treated and control firms using kernel matching with a caliper of 0.05 and imposing a common support. For details on the effectiveness of the matching exercise in terms of aligning the covariates of the treatment and control firms, see Tables A.7 and A.8 in the Appendix.

In addition to matching, the following institutional features help mitigate potential contamination of our estimation. First, loan histories are not used as a criteria for granting subsidies. This means that the *LR* and *treat* dummies capture distinct variations in the data (recall Table 3). Second, subsidies are granted for a large variety of purposes that SMEs typically seek bank loans for, which means that loans and subsidies are substitutes in the financing means for firms. This also underscores that variation in loan access across firms is a relevant aspect of how subsidies impact firms.

7 Results

7.1 The effect of subsidies on assets

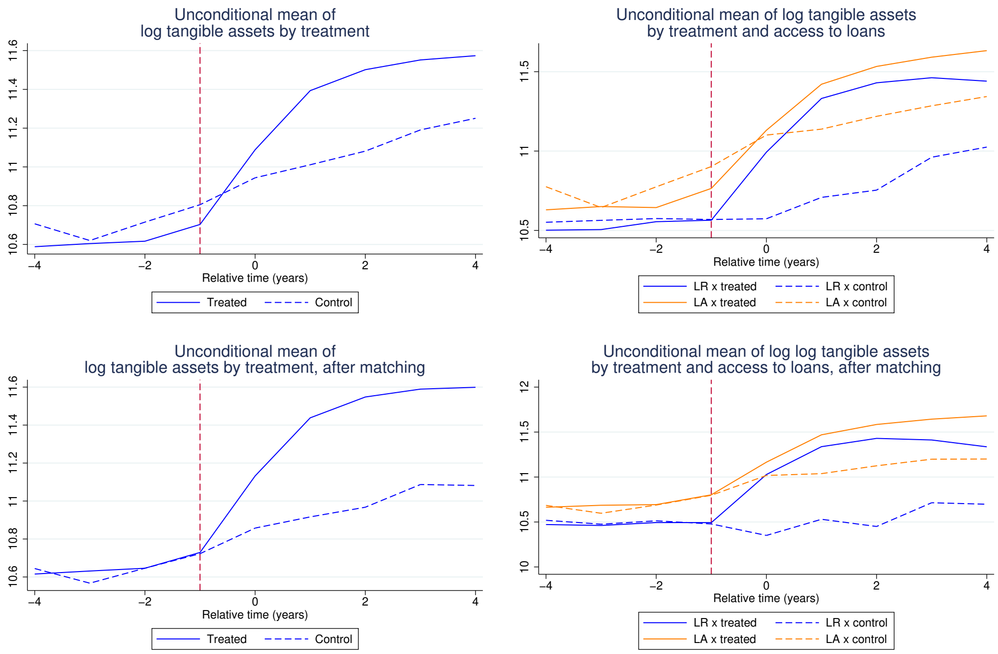
We start presenting our results by documenting the unconditional evolution of tangible assets around the reference year in the full (that is, unmatched) and matched samples (Figure 3). During the pre-treatment period, the treated and control firms behave similarly. The difference in the evolution of assets of the two groups is small, and matching practically eliminates any remaining wedge. This observation is largely maintained when zooming into the LR and LA subgroups (right panels). All of this confirms that firms with rejected subsidy applications are indeed good controls for firms with winning ones.

Turning to the post-treatment period, subsidies provide a major boost to treated firms' assets in both the full and matched samples (left panels),

[33] The variables are firm age, employment, real tangible assets, real value added, real sales revenue, real profit before tax, operational return on assets, real personnel cost, export share and a dummy variable indicating whether foreign ownership exceeds 50 percent.

and separately for both LR and LA subgroup of firms. By contrast, control firms increase their assets to a much smaller extent, largely in line with the pre-treatment trend.[34]

Figure 3: **Evolution of assets**



Notes: N = 17643/11269 (full/matched sample). The table presents the unconditional mean of log tangible assets of firms by treatment status (left panels) and also by credit access status (right panels). The dotted red vertical line indicates the year before treatment.

To investigate the evolution of assets more formally, we estimate the regression equations (1) and (2) and present the results in Table 6. The top panel of the table is based on the full sample, and the bottom panel is based on the matched sample.

The first column shows that subsidies have an economically large and significant impact on tangible assets of all treated firms, to the tune of 50 log

[34]Indeed, control firms, especially LA firms, are likely to have alternate sources of funding (including retained earnings and owner's funds) which can help them grow even without subsidies.

points in both the full and the matched-sample estimations.[35] Zooming into the LR and LA subgroups in the second and third columns of the table, we uncover a significant impact in each case. These findings are reaffirmed in the matched sample, which underscores that subsidies constitute a source of additional and cheap capital for LR and LA firms.

That said, the impact is larger for LR firms: they increase their assets by 64 log points compared to LA firms where the increase is around 40 log points.[36] In the matched sample, the estimated effect is similar in case of LA firms and is larger in case of LR firms (as compared the effects estimated on the full sample).

To gauge whether the greater impact of subsidies on LR firms relative to LA firms is significant in a statistical sense, we move away from subsample regressions and focus on the triple-difference regression in Column 4 of the table. The triple-interaction coefficient showing the incremental impact of subsidies on LR firms equals 23 log points, and it is significant at the 10 percent level in the full sample and almost twice as large (43 log points) and statistically significant at the 5 percent level in the matched sample. Consistent with the predictions of the stylized model, the subsidy has a larger effect on the assets of the LR firms, although the two groups of firms received roughly homogeneous treatment, that is, similar amounts of subsidies relative to their assets (recall Table 2). To test the robustness of our results, we consider several variations in the definition of our LR indicator and data sampling. First, to assess how sensitive the results are to our definition of loan status, we change the time horizon of one year in our baseline definition used to identify LR firms and instead consider a 6-month and a 2-year window. These alternative

[35] This result is broadly consistent with the findings in the literature and for Hungary as well (Banai et al. [2020] and Muraközy and Telegdy [2023]).

[36] This incremental impact is not mechanical. On the one hand, by construction, LA firms get a loan in the year prior to getting a subsidy, which may imply a smaller need for funding and artificially lead to an incremental impact in case of LR firms. On the other hand, however, the fact that LA firms apply for a subsidy reflects their genuine need for funding. Indeed, subsidy application costs money and also entails a skin-in-the-game requirement. Moreover, by comparing treated LA with control LA firms, any LA firm specific factors are accounted for in our triple-difference empirical design.

Table 6: **Impact of subsidy: tangible assets**

	All	LR	LA	LR vs LA
	(1)	(2)	(3)	(4)
Full Sample				
treat=1 × post=1	0.471***	0.639***	0.396***	0.402***
	(0.055)	(0.102)	(0.065)	(0.065)
treat=1 × post=1 × LR=1				0.227*
				(0.119)
N	17643	5396	12234	17643
R2	0.824	0.822	0.827	0.824
Matched Sample				
treat=1 × post=1	0.476***	0.809***	0.371***	0.374***
	(0.097)	(0.146)	(0.116)	(0.116)
treat=1 × post=1 × LR=1				0.434**
				(0.187)
N	11269	2724	8540	11269
R2	0.813	0.806	0.820	0.815
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

Notes: The table presents the estimated coefficients and corresponding standard errors associated with receiving a subsidy (Equation 1 in Columns 1-3) and its interaction with an indicator of being loan deprived the year before winning the subsidy (Equation 2 in Column 4). Dependent variable: log tangible assets. The sample in Columns 1 and 4 is LR and LA firms combined; in Column 2 it is LR firms; in Column 3 it is LA firms. The top panel is based on the full (= unmatched) sample, the bottom panel on the matched sample. Standard errors clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

LR definitions lead to estimation results that are comparable to benchmark estimates in terms of economic and statistical significance, as shown in the first four columns of Table 7.

Next, we check if the fact that multiple subsidies are received by some firms drives our results.[37] We drop firms with more than one successful application, which is about 10 percent of the firms in our sample. This sample manipulation does not affect the main takeaway (Table 7, fifth and sixth columns).

[37] Muraközy and Telegdy [2023] show that a successful application increases the probability of a second successful application and that the second grant leads to larger effects.

Table 7: **Impact of the subsidy: tangible assets (robustness checks)**

	(1) LR(6m)	(2) LR(6m)	(3) LR(2y)	(4) LR(2y)	(5) Singlewin	(6) Singlewin	(7) LR(logit)	(8) LR(logit)	(9) Margin
treat=1×post=1	0.382***	0.290*	0.422***	0.411***	0.348***	0.312***	0.402***	0.376***	0.409***
	(0.082)	(0.162)	(0.06)	(0.108)	(0.067)	(0.116)	(0.065)	(0.116)	(0.113)
treat=1×post=1×LR=1	0.297**	0.519**	0.251*	0.472**	0.268**	0.472**	0.228*	0.433**	0.479**
	(0.129)	(0.218)	(0.144)	(0.224)	(0.122)	(0.190)	(0.108)	(0.187)	(0.200)
N	13725	8865	17634	11260	15578	10040	17634	11260	9089
R2	0.821	0.802	0.824	0.815	0.825	0.815	0.824	0.815	0.819
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector×Calendar Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matching	No	Yes	No	Yes	No	Yes	No	Yes	Yes

Notes: The table presents the estimated coefficients and corresponding standard errors associated with receiving a subsidy and its interaction with an indicator of being loan deprived the year before winning the subsidy (Equation 2). Dependent variable: log tangible assets. Columns 1-4: LR definition based on a time window of 6 months/2 years; Columns 5-6: firms with multiple subsidies dropped from the sample; Columns 7-8: the definition of LR is extended to all firms using a logistic regression model. Column 9 is based on the sub-sample of firms that have predicted probabilities of being an LR in the range of 0.2 to 0.4 (using the same logistic model as in columns 7-8). Standard errors clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Next, recall that the baseline LR indicator leaves some firms as unclassified (and these firms were dropped from the analysis sample). To expand our analysis to the set of unclassified firms (and to test the external validity of our method), we use a logistic regression to classify unknown firms as LR or LA, based on their observable characteristics.[38] The use of this extended definition leaves the estimated coefficients to be similar, as shown in columns 7 and 8 of the table.[39]

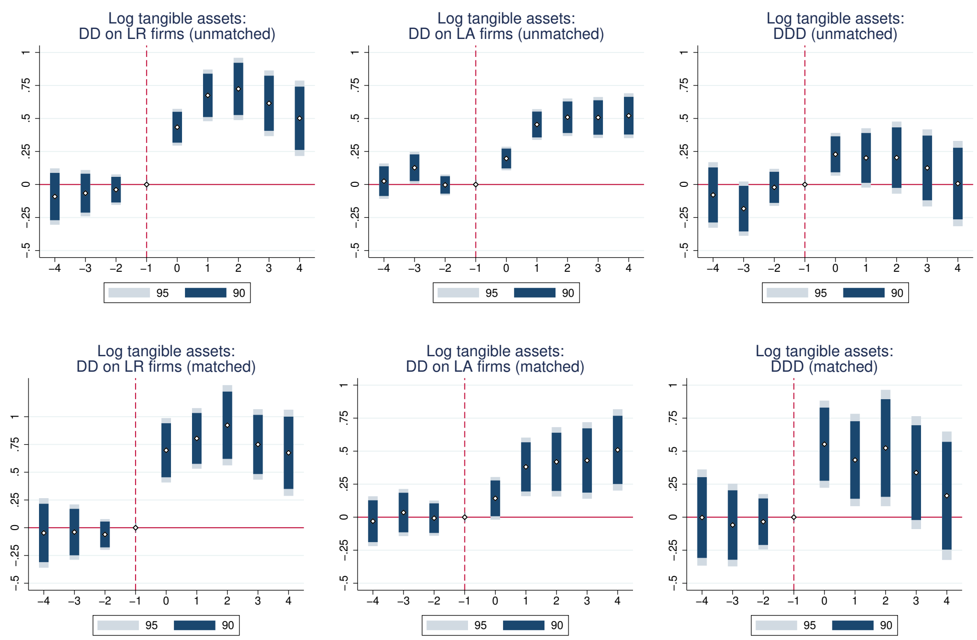
Finally, we focus on firms that likely had their loans accepted or rejected

[38]Guided by firm level characteristics that are likely to have a bearing on loan rejections and acceptances, the logistic regression model is based on the following factors: age, employment, tangible assets, value added, sales, profits and costs to assets, leverage, cost-to-income ratio, export sales to total sales, and foreign ownership. The model has an Area Under Receiver Operating Curve (AUROC) of 61 percent.

[39]If the logit regression had no explanatory power and randomly placed unclassified firms into the LR and LA groups, the estimated incremental effect of the subsidy on LR firms should have fallen.

by a small margin. We use the same logistic regression that is presented above to obtain for each firm the probability that it is an LR firm. We then focus on firms that are near the probability cutoff that optimally classifies whether a firm is LR or LA.[40]. In the end, we subset our sample to firms whose probability score is in a small radius of 0.1 around the cutoff. The incremental impact of subsidies on the LR firms in this sample, however, is very similar as before.

Figure 4: **Evolution of the impact of subsidy over time: tangible assets**



Notes: The figure presents the estimated coefficients and corresponding confidence intervals associated with interactions between event time dummies and receiving a subsidy (Equation 1 in Columns 1-2) and its interaction with an indicator of being loan deprived the year before winning the subsidy (Equation 2 in Column 3). Dependent variable: log tangible assets. The sample in Column 1 consists of LR firms; in Column 2 LA firms; in Column 3 LR and LA firms. The top panel is based on the full (= unmatched) sample, the bottom panel on the matched sample. Standard errors clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

The results so far show that there is an impact of the subsidy program on

[40]The predictive power of this model is low, which is to be somewhat expected given that LR and LA firms are not too different in terms of their observable features

the assets of the treated firms in the post treatment period on average, but these results are silent about how the impact evolves *during* that period. To quantify this pattern, we run event-time regressions where each year (relative to the reference year) gets its own coefficient. We consider sub-sample regressions on LR and LA firms, as well as triple-interaction regression on all firms.

In the pre-treatment period, there are some trends in the full sample (see the upper panels in Figure 4), but the coefficients are small relative to the post-treatment effect and generally not different from zero in statistical terms. Any pre-trends disappear in the matched sample (bottom panels in Figure 4).

In the post-treatment period, we find an inverted U-shaped impact of subsidies in the case of LR firms. That is, treated LR firms significantly increase their assets more than control LR firms for three years. However, after that, this difference declines, both in full and matched samples (figures in the first column). Despite the decline, the impact on treated firms remains highly significant even after five years. The impact on LA firms, by contrast, is smaller on average but more persistent and is even increasing in the matched sample throughout the post-treatment period (second column).

The last column in Figure 4 shows the incremental impact on LR relative to LA firms.[41] We find that this impact fades over time in both the full and the matched samples. In theory, the fading incremental impact can be due to the inverted U-shaped impact on LR firms, the more persistent impact on LA firms, or a combination of the two effects. Figure 4 suggests that both factors contribute. A closer look, especially on the matched sample results, shows that between $T = 2$ and $T = 4$ the estimated effect on LA firms increases by 9 percent while for LR firms it declines by 24 percent. Therefore, the reason for the fading incremental effect on LR firms is *primarily* the LR firms themselves in the sense that LR firms are unable to sustain the boost provided by subsidies in the same way as LA firms.

[41] The estimated coefficient associated with the triple interaction term in Equation 2 is $\hat{\gamma} = E[\Delta(y_{Treated} - y_{Control})|X]^{LR} - E[\Delta(y_{Treated} - y_{Control})|X]^{LA}$, where y represents assets, X represents all control variables, and Δ represents the difference between the pre- and post-treatment period. In the event time specification, Δ is computed for each year relative to the year before the application year ($T = -1$).

What is, then, the main reason for an inverted U-shaped impact on LR firms' assets? We can only give a partial answer to this question by revisiting the unconditional evolution of the assets of treated and control LR firms in the right-hand panels in Figure 3. Two observations are notable. For one, after an initial boost, the growth in assets of treated LR firms largely stalls. In contrast, control LR firms are able to increase their assets in the latter part of the post-treatment period. This pattern is clearly present in the full sample and less pronounced, but still visible in the matched sample.[42] Taken together, this reduces the gap between treated and control LR firms by the end of the treatment period, which underpins the inverted U-shaped impact of subsidies on LR firms.

To summarize, the evidence presented in this section leads to two main takeaways. First, a positive and significant impact on both LR and LA firms' assets suggests that subsidies ease credit conditions for both sets of firms, prompting them to undertake fresh investments. Second, in line with the prediction from the stylized model in Section 3, there is an incremental impact on LR firms' assets relative to LA firms. However, this effect is temporary and fades over time. In the next two subsections, we pursue additional investigations to better interpret these findings.

7.2 Impact of subsidies on firm performance: ROA and sales

As suggested by our model, profitability can be impacted by subsidies. If LR firms have projects that are equally productive or are equally well managed as LA firms, then a subsidy should lead to a larger boost in the profitability of LR firms. Indeed, a subsidy received by a firm with good investment ideas

[42] One reason for this increase is the general improvement in economic conditions in Hungary after 2012, which coincides with the post-treatment period for the majority of firms in our sample. However, easing credit conditions should help both treated and control LR firms, which means that the easing itself cannot explain the inverted U-shaped impact of the subsidy on treated LR firms. However, it is possible that improved economic conditions raised the profits of firms, and control LR firms used this money to pursue the investment, while the treated LR firms did not make any additional investment.

but poor access to bank credit should lead to an accelerated improvement in firm-level prospects by alleviating a relevant constraint. By contrast, if the firm does not have good quality projects or is poorly managed to start with, providing a subsidy may not help improve these firms' longer-term prospects and only lead to a temporary impact on firm performance. This could also be a potential explanation for why treated LR firms (unlike LA firms) are not able to maintain the subsidy-led boost in assets.

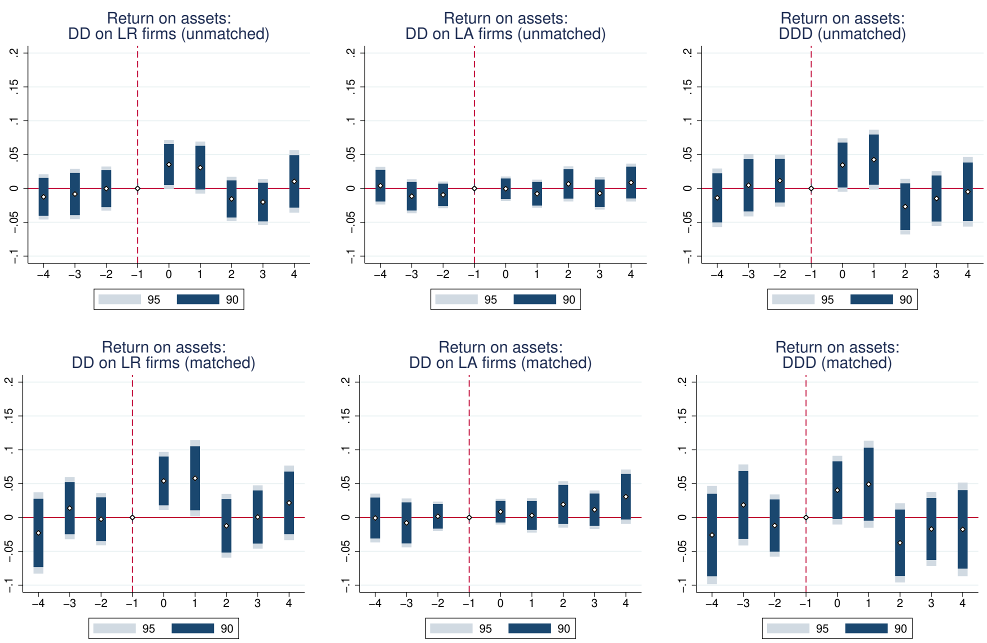
Table 8: **Impact of subsidy: return on assets**

	All firms (1)	LR firms (2)	LA firms (3)	LR vs LA firms (4)
Full Sample				
treat=1 × post=1	0.006	0.014	0.004	0.004
	(0.006)	(0.131)	(0.075)	(0.07)
treat=1 × post=1 × LR=1				0.007
				(0.014)
N	17618	5419	12186	17618
R2	0.353	0.396	0.342	0.353
Matched Sample				
treat=1 × post=1	0.018**	0.027	0.016*	0.016
	(0.008)	(0.018)	(0.010)	(0.010)
treat=1 × post=1 × LR=1				0.010
				(0.02)
N	11247	2738	8504	11247
R2	0.344	0.396	0.340	0.344
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

Notes: The table presents the estimated coefficients and corresponding standard errors associated with receiving a subsidy (Equation 1 in Columns 1-3) and its interaction with an indicator of being loan deprived the year before winning the subsidy (Equation 2 in Column 4). Dependent variable: return on assets. The sample in Columns 1 and 4 is LR and LA firms combined; in Column 2 it is LR firms; in Column 3 it is LA firms. The top panel is based on the full (= unmatched) sample, the bottom panel on the matched sample. Standard errors clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

To test this hypothesis empirically, we assess the impact of subsidies on profitability. We summarize the empirical findings in Table 8. In the full sample, ROA is not affected by the subsidies: the estimated coefficients are economically small and statistically indifferent from zero in every specification (top panel). In the matched sample (bottom panel) we do find an overall positive impact as large as 1.8 percentage points (column 1), but the incremental effect on LR firms is only 1 percentage point and has a large standard error, making it insignificant (column 4).

Figure 5: **Evolution of the impact of subsidy over time: return on assets**



Notes: The figure presents the estimated coefficients and corresponding confidence intervals associated with interactions between event time dummies and receiving a subsidy (Equation 1 in Columns 1-2) and its interaction with an indicator of being loan deprived the year before winning the subsidy (Equation 2 in Column 3). Dependent variable: return on assets. The sample in Column 1 consists of LR firms; in Column 2 LA firms; in Column 3 LR and LA firms. The top panel is based on the full (= unmatched) sample, the bottom panel on the matched sample. Standard errors clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Digging deeper, the evolution in time of the subsidy effect is similar to the pattern we found for assets (see Figure 5). In both unmatched and matched samples, ROA of treated LR firms improves relative to their controls during the two years after the subsidy is received (although the statistical significance is marginal) but declines thereafter (first column panels).The incremental impact on LR firms also follows a similar pattern (third column panels).

Table 9: **Impact of subsidy: sales**

	All firms (1)	LR firms (2)	LA firms (3)	LR vs LA firms (4)
Full Sample				
treat=1 × post=1	0.134***	0.189***	0.111**	0.115**
	(0.038)	(0.068)	(0.045)	(0.045)
treat=1 × post=1 × LR=1				0.063
				(0.079)
N	17761	5428	12319	17761
R2	0.847	0.869	0.840	0.848
Matched Sample				
treat=1 × post=1	0.149**	0.285***	0.108	0.109
	(0.069)	(0.096)	(0.084)	(0.084)
treat=1 × post=1 × LR=1				0.172
				(0.125)
N	11335	2734	8594	11335
R2	0.788	0.855	0.770	0.789
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

Notes: The table presents the estimated coefficients and corresponding standard errors associated with receiving a subsidy (Equation 1 in Columns 1-3) and its interaction with an indicator of being loan deprived the year before winning the subsidy (Equation 2 in Column 4). Dependent variable: log of sales revenue. The sample in Columns 1 and 4 is LR and LA firms combined; in Column 2 it is LR firms; in Column 3 it is LA firms. The top panel is based on the full (= unmatched) sample, the bottom panel on the matched sample. Standard errors clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

While ROA is a natural way to measure firm performance and it is also used in our model, its use has several empirical drawbacks. First, the impact of the subsidy on its denominator and numerator may have different time horizon. While assets increase right after the investment was executed, profits may take longer to build up.[43] In addition, despite deflating both assets and

[43]A related measurement issue in the case of ROA is that the denominator, i.e. assets, is

profits with proper deflators, inflation may bias the evolution of ROA due to its differential impact on the numerator and the denominator. Finally, ROA is prone to accounting manipulation for tax reasons, especially among small private firms.

To get around these caveats, we study the impact of subsidies on sales. An advantage of looking at sales is that it is a more transparent measure of business activity than ROA, especially among small firms. Moreover, assessing sales can shed light on whether the subsidy is used for productive purposes (as opposed to private consumption).

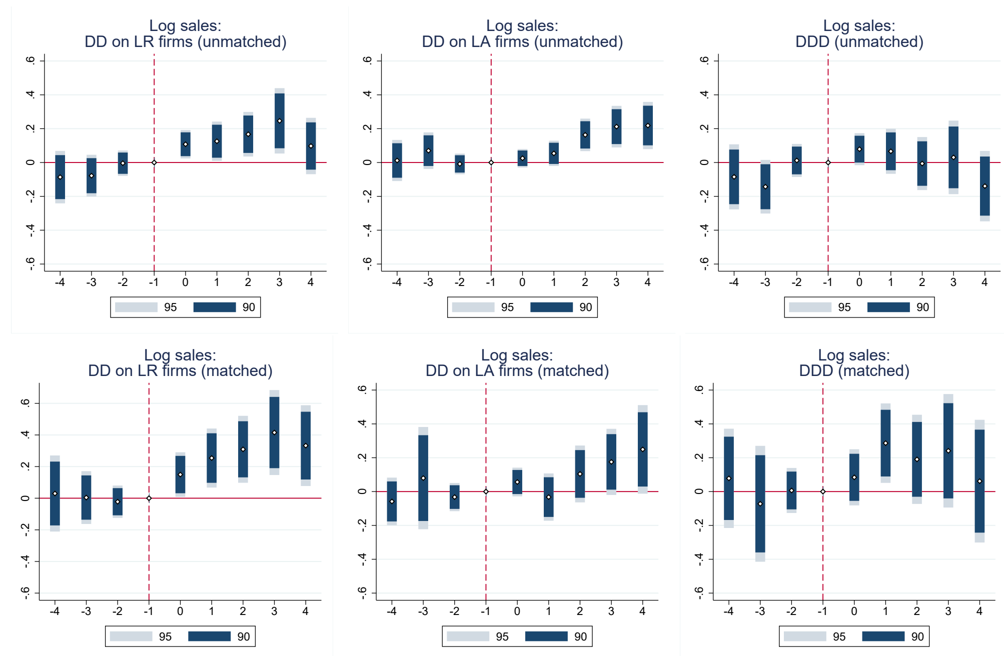
We summarize the empirical findings in Table 9. In the full sample, we find that subsidies improve the sales revenues of both LR and LA firms, but the incremental effect of the subsidy on LR firms is not significant. In the matched sample (bottom panel) we estimate similar effects.

The evolution in time of the subsidy effect on sales (presented in Figure 6) shows that subsidies had an inverted U-shape impact on the sales of LR firms. This finding resonates with what we found in the case of assets. The incremental impact of LR firms is also weak and fades over time.

We also look into the effects of the subsidy on labor productivity, another variable that is correlated with firm performance. Labor productivity is expected to be less prone to differences in accounting practices across firms and sales and employment are likely to be measured more accurately than profits. Moreover, a subsidy led improvement in sales may be realized sooner than a potential improvement in profits. In Appendix Table A.9, we report the main findings on labor productivity. Like in the case of ROA, subsidies do not have any material impact on the productivity of both LR and LA firms, and no incremental impact on LR firms either. The time evolution of the subsidy effect also reveals no impact throughout the post-treatment period (see Appendix

affected by the subsidy to different degrees in the case of LR and LA firms. This can bias the measurement of the impact of subsidies on ROA of LR firms relative to LA firms. To help get around this issue, we study the impact of subsidies on profits in levels *per se*. We find that subsidies have no material impact on LR firms relative to LA firms. This applies both in the case of unmatched and matched sample. These findings are similar to those in the case of ROA and thus serve as a robustness to the ROA results.

Figure 6: **Evolution of the impact of subsidy over time: sales**



Notes: The figure presents the estimated coefficients and corresponding confidence intervals associated with interactions between event time dummies and receiving a subsidy (Equation 1 in Columns 1-2) and its interaction with an indicator of being loan deprived the year before winning the subsidy (Equation 2 in Column 3). Dependent variable: log of sales revenue. The sample in Column 1 consists of LR firms; in Column 2 LA firms; in Column 3 LR and LA firms. The top panel is based on the full (= unmatched) sample, the bottom panel on the matched sample. Standard errors clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Figure A.3).

To summarize, the results suggest that while subsidies translate into a material improvement in LR firms' assets and bolster their sales, albeit one that fades over time, there is no impact on their profitability or productivity. In other words, in spite of starting from sub-optimal levels of assets – that is, levels at which the marginal value of assets should be higher than the marginal cost of funding at the market rate – subsidies *do not* lead to sustainably improved profitability for LR firms while our theoretical model predicts that profitability should have increased if the initial productivity of the two types of firms were similar. Taken at a face value, this means that LR firms are not as productive as LA firms to begin with. As such, LR firms are loan deprived probably because of fair reasons such as lack of good quality projects or poor management in the past (as reflected in their loan history) and that these shortcomings prevent them from outperforming their peers even when they get access to subsidized capital.

7.3 Do subsidies help LR firms get more loans?

A complementary approach to assessing the impact of subsidies on firm performance is to study whether subsidies help LR firms get more loans in the medium to long run. Indeed, if the investment carried out with the subsidy turns out to be sufficiently profitable, it can improve the firm's future credit worthiness and enable it to obtain more loans (or obtain loans more easily) over time. However, if LR firms are inherently less well managed or less productive than LA firms, the subsidy may not ease their access to credit. To study this question, we replace the dependent variable in our regressions with the log amount of new loans taken by the firm during the year. Table 10 shows that both treated LR and treated LA firms take out more loans after receiving a subsidy relative to their respective control groups (second and third columns). And while the coefficient estimated on the LR subsample is twice as large as that estimated on the LA subsample, the incremental effect is not significant at any conventional level (fourth column).

Turning to event-time regressions (Figure 7), we find that the impact on loans obtained by LR firms is rather temporary: there is a major increase in the loan amount in the treatment year, followed by a quick reversal to no impact (panels of the first column). The impact on LA firms is somewhat more persistent in the full sample but disappears in the matched sample (panels of the second column). Not surprisingly, any incremental impact on loans taken by LR firms relative to LA firm is only noted in the treatment year and ceases to exist thereafter (third column panels). The short-term positive impact of LR firms is likely to be mechanical. Treated LR firms may have used the loans to

Table 10: **Impact of subsidy: new loans**

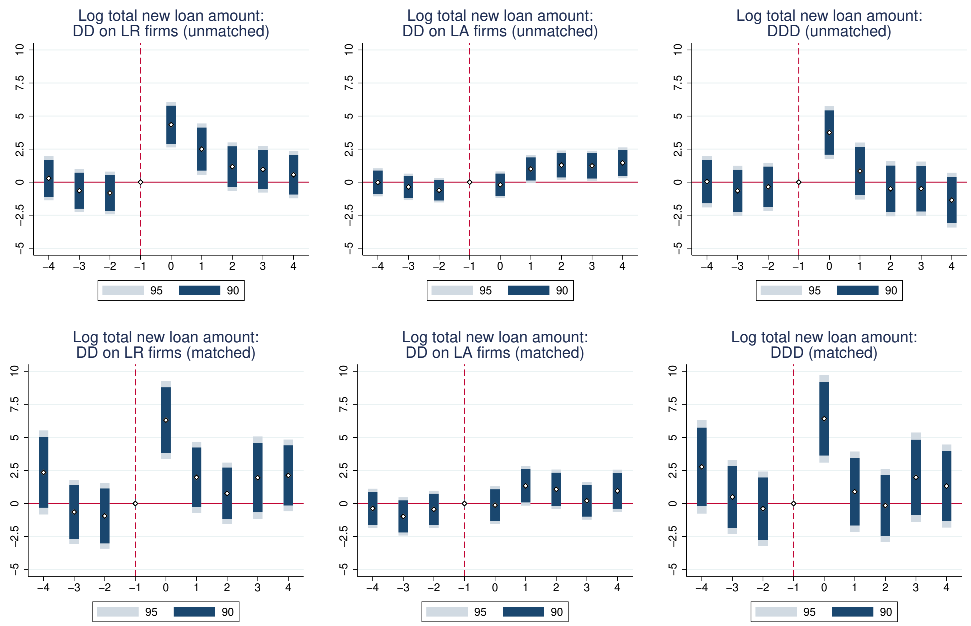
	All	LR	LA	LR vs LA
	(1)	(2)	(3)	(4)
Full Sample				
treat=1 × post=1	1.572***	2.307***	1.164***	1.306***
	(0.314)	(0.589)	(0.374)	(0.372)
treat=1 × post=1 × LR=1				0.877
				(0.691)
N	17905	5473	12418	17905
R2	0.400	0.338	0.376	0.400
Matched Sample				
treat=1 × post=1	1.473***	2.488***	1.117**	1.106**
	(0.452)	(0.782)	(0.529)	(0.534)
treat=1 × post=1 × LR=1				1.546
				(0.960)
N	11424	2761	8659	11424
R2	0.385	0.332	0.359	0.385
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

Notes: The table presents the estimated coefficients and corresponding standard errors associated with receiving a subsidy (Equation 1 in Columns 1-3) and its interaction with an indicator of being loan rejected the year before winning the subsidy (Equation 2 in Column 4). Dependent variable: log value of new loans. The sample in Columns 1 and 4 is LR and LA firms; in Column 2 LR firms; in Column 3 LA firms. The top panel is based on the full (= unmatched) sample, the bottom panel on the matched sample. Standard errors clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

fund the un-subsidised (i.e., own contribution) portion of the investment (which is typically 50-70 percent of the total project cost). Indeed, banks may have been willing to provide these loans to LR firms as the credit risk (valuation) of a subsidized project is likely to have been smaller (better). This hypothesis is supported by the fact that when we drop the treatment date (that is T = 0) from the regression sample, the coefficients in Table 10 Column 2 become close to zero and statistically insignificant.[44]

[44] In theory, the absence of an enduring impact on loans obtained by subsidized LR firm can also reflect that they do not need additional investment for a few years since they already carried out their desired investment project. However, in practice, Hungarian companies

Figure 7: **Evolution of the impact of subsidy over time: log new loans**



Notes: The figure presents the estimated coefficients and corresponding confidence intervals associated with interactions between event time dummies and receiving a subsidy (Equation 1 in Columns 1-2) and its interaction with an indicator of being loan deprived the year before winning the subsidy (Equation 2 in Column 3). Dependent variable: log value of new loans. The sample in Column 1 consists of LR firms; in Column 2 LA firms; in Column 3 LR and LA firms. The top panel is based on the full (= unmatched) sample, the bottom panel on the matched sample. Standard errors clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

8 Conclusion

To add sales and LP result discussion here We do not write about separate results, so there is no need to add sales and LP.

Policymakers in both advanced and emerging market economies employ subsidy programs to improve the outcomes of small and medium-sized enterprises. Ideally, subsidies should target firms with profitable opportunities but without funding, but such firms are hard to identify. In this paper, we show

seek an investment loan every 15 months on average, which means that lower demand for loans is an unlikely explanation of the finding.

how administrative data can help design more effective subsidy programs.

We use data from Hungary to assess if loan deprived firms – identified using credit registry data – benefited more from subsidies. The answer helps identify the source of loan deprivation. If loan deprived firms benefited more from subsidies, it is likely that banks rationed credit to these firms. However, if loan deprived firms benefited less from subsidies, then poor firm quality is the more likely explanation for loan deprivation.

Knowing what underpins loan deprivation can help decide whether or not subsidies should target loan deprived firms. In the case of Hungary, we find that loan deprivation tends to reflect poor firm quality rather than credit rationing by banks. In this case, the implication in Hungary is that future subsidies may not necessarily be targeted towards loan deprived firms.

What underpins loan deprivation can also help policymakers decide whether subsidies should be channeled through banks or directly by the government. In an economy where banks have demonstrated effective assessment of firms' credit worthiness in the past, banks may be better positioned than the government to channel subsidized funding to the most deserving firms.[45] That said, who should distribute subsidies also hinges on the ultimate policy objective. If the goal is to simply improve outcomes for *all* firms that cannot get bank credit – irrespective of their productivity – then the optimal policy choice may very well be that the government targets loan-deprived firms. This is also because banks may have different objectives (e.g. profit maximization) or credit risk assessment criteria as compared to the state (i.e. welfare maximization).

While our specific findings obviously depend on structural aspects of the Hungarian economy, our methodological approach applies more generally. At a minimum, as the characteristics of the SME subsidy program in Hungary are derived from an EU framework, the findings in this article are relevant for other EU countries where similar programs have been implemented. This can support the development of best practices for SME subsidy programs by

[45]Such a practice would be consistent with the practice adopted in several advanced and emerging economies (e.g., US and Saudi Arabia) during the Covid-19 pandemic, wherein policymakers used banks to transmit funding support to SMEs rather than subsidizing them directly (see for example OECD [2021]).

providing a template for how policymakers can use administrative data such as the credit registry to design a more efficient program. Moreover, we show how firms' credit performance data can be used to gauge the degree of credit rationing in the economy.

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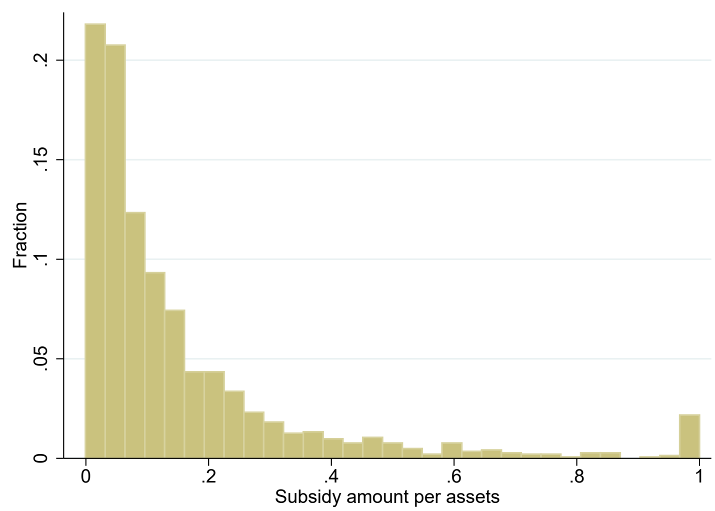
A Appendix

Table A.1: **Macroeconomic Conditions**

Year	GDP Growth	Inflation	Corporate Debt/ GDP	Corporate Debt Change	Interest HUF	Interest EUR
2011	1.9	4.1	27.1	-3.8	8.7	4.0
2012	-1.3	5.0	23.9	-4.7	9.4	3.5
2013	1.8	0.4	22.0	-1.3	6.5	3.0
2014	4.2	-0.9	20.6	2.3	4.6	2.6

Source: Hungarian Statistical Office; Central Bank of Hungary Notes: The table presents several macroeconomic indicators for Hungary for the years when firms applied for a subsidy. The interest rate figures refer to floating rate corporate loans with more than one year maturity.

Figure A.1: **Distribution of subsidy requested to assets ratio for all applicants**



Notes: Distribution is winsorized at 1.

Table A.2: **Distribution of applicants, winners, and losers by year**

Year	Never won	Won once or more	Total applicants
2007	627	1932	2559
2008	889	1630	2519
2009	1096	3050	4146
2010	1277	2172	3449
2011	1031	3889	4920
2012	2378	6661	9039
2013	1380	2651	4031
2014	148	99	247
2015	17	60	77
Total	8843	22144	30987

Table A.3: **Distribution of firms by number of applications, and by number of wins**

Number of applications/wins	Number of firms with that many wins	Number of firms with that many applications
0	8843	NA
1	16255	20274
2	3198	5661
3	1263	2252
4	646	1098
5 or more	782	1702
Total	30987	30987

Table A.4: **Number of firms and observations at the various data creation steps**

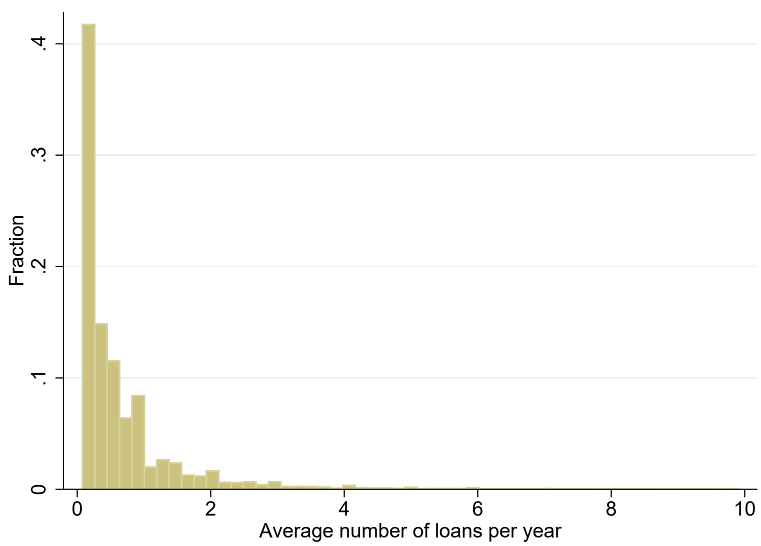
Stage	Observations	Firms
Universe of applicants: Total number of applying firms and applications in the database (note that a firm can apply multiple times)	64387	35488
Basic cleaning: Drop observations where the recipient is an intermediary and not the final beneficiary, subsidy contract terminated without a payment, applications with missing characteristics, or both positive and negative decisions	54405	30987
Restrict to applications between 2012 and 2015. This is because the loan query data – which is key for the definition of loan deprived status – is unavailable before 2012.	24442	14858
Keep first application of applicants that never win during our sample period, and first successful application of applicants that win at least once (this step results in a cross-sectional firm-level dataset)	13306	13306
Restrict attention to firms for which financial characteristics are available. Obtain characteristics for 4 years before and after the treatment year, for a total of 9 years (not all firms may have data for all years). Delete very large firms (based on SME code), very small firms (less than 5 employees), those operating in select NACE sectors, and delete observations with suspected erroneous firm data	25253	2873
Define the LR indicator and drop firms for which the LR indicator is not defined	17901	2025

Notes: This tables describes the various steps involved in preparing the final dataset that we use in our regressions.

Table A.5: **Comparison of characteristics of in-sample and out-of-sample firms.**

	Out-of-sample firms		In-sample firms	
	Mean	St. dev.	Mean	St. dev.
Tangible assets	160.6	194.7	182.1	613.3
Employment	19.0	31.1	24.2	29.5
Firm age	11.7	7.58	12.3	6.41
Return on assets	0.05	0.34	0.11	0.15
Debt to assets	0.50	0.27	0.54	0.24
Sales	566.2	519.3	727.3	173.6
Labor productivity	31.7	57.3	34.6	11.2
Number of new loans	1.57	1.64	1.97	1.90

Figure A.2: **Average number of loans per year during the sample period**



Notes: Loans with maturity of at least one year.

Table A.6: **Results of the logit regression to create propensity score for matching**

	Coef.	Std. Err.
Firm Age	0.000	0.000
Employment (log)	0.218	0.170
Tangible Assets (log)	-0.001	0.042
Value Added (log)	0.154	0.131
Sales (log)	-0.076	0.083
Profits (log)	-0.057	0.059
Return on assets	1.275**	0.553
Personnel Cost (log)	0.002***	0.137
Export Share	-0.193	0.271
Foreign	-0.706***	0.241
Subsidy request to tangible assets	-0.281***	0.085
Loan rejection dummy	0.022	0.110
Reference Year	-0.538***	0.077
Asset Growth	-0.143	0.132
Medium Firm	-0.325**	0.152
Large Firm	-0.803**	0.335

Notes: N=2025, Pseudo $R^2 = 0.046$ Log Likelihood = -1206.5. The regression controls for 2-digit industries. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.7: **Weights of the matched sample and change in sample size due to matching**

Matched firms and matching weight	Controls	Treated	Total
1	183	842	1 025
2	131	0	131
3	55	0	55
4	13	0	13
5	8	0	8
6	5	0	5
7	2	0	2
8	3	0	3
9	2	0	2
10	4	0	4
Unmatched firms	224	579	803
Without estimated propensity score	17	25	42
Total	647	1 446	2 093

Notes: Matching weights are rounded to the nearest integer.

Table A.8: **Balance of covariates in the matched sample**

	Mean treated	Std. dev.	Mean control	Std. dev.	Diff
Employment	2.59	0.85	2.61	0.85	-0.016
Tangible assets	10.65	1.65	10.64	1.64	0.004
Sales	12.64	1.23	12.54	1.21	0.10
Labor productivity	9.99	0.95	9.89	0.92	0.10
Return on assets	0.135	0.143	0.136	0.147	-0.005
Requested subsidy to tangible assets	0.46	0.37	0.49	0.39	-0.006
Growth rate of tangible assets	0.089	0.607	0.086	0.69	0.030
Value of new loans	11.21	8.23	11.15	8.22	0.005

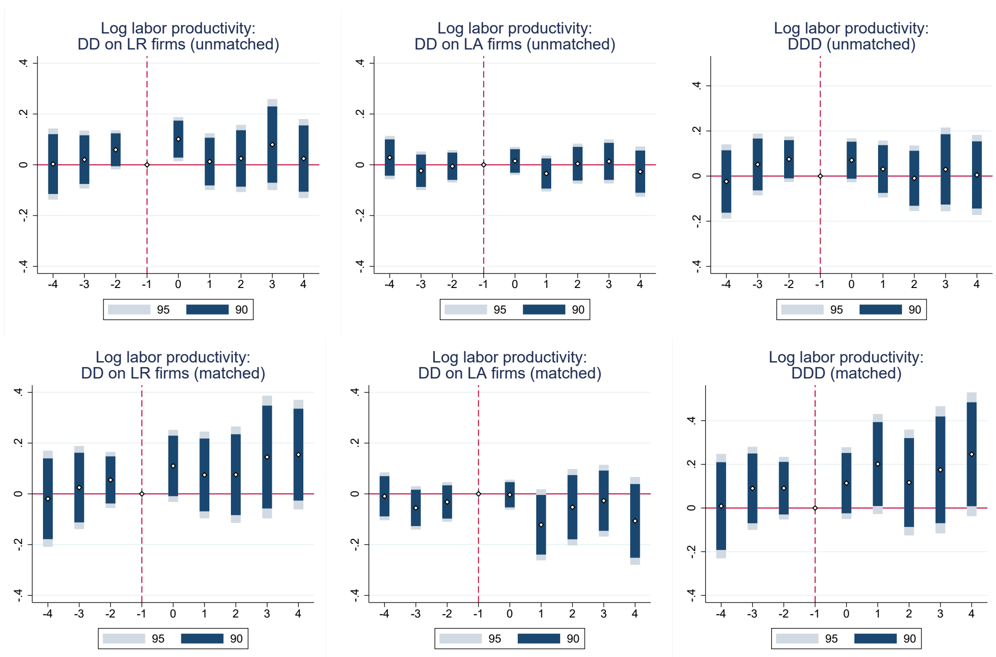
Notes: Mean of the respective variables during the pre-treatment period. Variable definitions are detailed under Table 2. For brevity, categorical variables of the propensity score model (i.e. modes of two-digit NACE categories, modes of region categories of the firm's headquarters, modes of size categories, and treatment year dummies) are omitted from the table. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.9: **Impact of subsidy: labor productivity**

	All firms (1)	LR firms (2)	LA firms (3)	LR vs LA firms (4)
Full Sample				
treat=1 × post=1	0.0004	0.028	-0.004	0.0003
	(0.027)	(0.054)	(0.031)	(0.031)
treat=1 × post=1 × LR=1				0.0009
				(0.060)
N	17698	5405	12279	17698
R2	0.813	0.811	0.816	0.813
Matched Sample				
treat=1 × post=1	-0.008	0.094	-.037	-.037
	(0.046)	(0.076)	(0.055)	(0.055)
treat=1 × post=1 × LR=1				0.123
				(0.094)
N	11295	2722	8566	11295
R2	0.786	0.816	0.779	0.787
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

Notes: The table presents the estimated coefficients and corresponding standard errors associated with receiving a subsidy (Equation 1 in Columns 1-3) and its interaction with an indicator of being loan deprived the year before winning the subsidy (Equation 2 in Column 4). Dependent variable: labor productivity (sales revenue to employment ratio). The sample in Columns 1 and 4 is LR and LA firms combined; in Column 2 it is LR firms; in Column 3 it is LA firms. The top panel is based on the full (= unmatched) sample, the bottom panel on the matched sample. Standard errors clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Figure A.3: **Evolution of the impact of subsidy over time: labor productivity**



Notes: The figure presents the estimated coefficients and corresponding confidence intervals associated with interactions between event time dummies and receiving a subsidy (Equation 1 in Columns 1-2) and its interaction with an indicator of being loan deprived the year before winning the subsidy (Equation 2 in Column 3). Dependent variable: labor productivity (sales revenue to employment ratio). The sample in Column 1 consists of LR firms; in Column 2 LA firms; in Column 3 LR and LA firms. The top panel is based on the full (= unmatched) sample, the bottom panel on the matched sample. Standard errors clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.