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Credit Constrained Firms and Government Subsidies: Evidence from a European Union Program*

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Abstract

We assess the effects of non-repayable subsidies on financially constrained and unconstrained Hungarian SMEs. Using rejected subsidy applicants as control group and bank queries to the credit-registry to identify firms that applied for but did not receive a loan, we show that subsidies generate a sizeable incremental impact on asset growth of constrained firms relative to unconstrained businesses. This effect, however, is transitory and does not translate into higher sales, profitability or productivity. Financing, therefore, may not be the primary hurdle for these SMEs, and credit constraints may reflect other shortcomings, such as lack of good management or viable projects.

JEL codes: G38, G21, E58

Keywords: SMEs; Subsidies; Credit Constraints; Emerging Market Economies; Difference-in-differences; Credit Registry micro-data.

1 Introduction

Small and medium-sized enterprises (SMEs) play an important economic role – generating between half and two-thirds of employment and value added in major advanced as well as emerging market economies (e.g. Stein et al. [2013]). Yet globally they tend to have patchy access to finance (e.g. Beck et al. [2008], Rajan and Zingales [1996], Demirgüç-Kunt and

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Maksimovic [1999]). Governments, including the European Commission, spend substantial resources to subsidise SMEs with the goal to improve both firm-specific as well as aggregate outcomes. Our current understanding is that on average subsidies do accelerate SME growth (e.g. Kersten et al. [2017]).[1] However, there are two questions that are less well understood in the literature.

First, it is not obvious whether subsidies work by improving availability of credit or by reducing the cost of borrowing (by lowering the interest rate or by being non-repayable).[2] We attempt to separate these channels with the help of a unique combination of detailed micro-data that enables identifying credit constrained firms directly.[3] The data have records of bank queries regarding firms' outstanding loans that allow us to identify enterprises which applied for but did not receive a loan. By classifying firms as credit constrained (CC) and not constrained (NC), we then disentangle the two channels through which subsidies work.

We show that subsidies lead to a significant incremental impact on asset growth of CC firms relative to NC firms. Since subsidies constitute a source of cheap capital for *both* groups, the incremental impact points to an easing of the financial constraint for CC firms. This impact, however, fades after a few years. Relatedly, we find that subsidised CC firms are able to get more loans compared to the control group only temporarily. This seems to follow from the fact that the subsidy program requires co-financing the project, which means that firms may have pledged the subsidized project as collateral for a bank loan.

A second less well understood issue is whether subsidies improve CC firms' outcomes more broadly. A priori the answer is not obvious. If a firm has good investment ideas but cannot obtain financing due to a short credit history (e.g. young firms) or the lack of collateral (e.g. small firms), then a subsidy can accelerate growth by alleviating a relevant

[1]The literature has estimated the effect of subsidized investment on firm growth using the most up-to-date econometric methods, such as instrumental variable techniques (Bach [2013]; Brown and Earle [2017]; Criscuolo et al. [2019]; Zia [2008]) and randomized trials (Banerjee et al. [2015]; De Mel et al. [2008]). More specifically, Criscuolo et al. [2019] study EU-funded grants in the United Kingdom, Cerqua and Pellegrini [2014] investigate a similar investment subsidy in Southern Italy. Bach [2013] and Brown and Earle [2017] analyze subsidized loans in France and the United States, respectively. Rao [2016]; Agrawal et al. [2020] look into the effects of targeted tax credits.

[2]We found only one study where only the access to credit changed and not the cost of capital: Banerjee and Duflo [2014] study a program when banks expanded their lending with respect to a group of firms but the interest rate did not change.

[3]Most studies typically rely on indirect measurement of credit constraints. For example, Bach [2013]; De Mel et al. [2008] estimate that the implied marginal rate of return of capital is much higher than its marginal cost and conclude that this is possible only in the presence of credit constraints. Mateut [2018] study the link between firm-level innovation and subsidies where credit constraints are measured with a survey. Chiappini et al. [2020] assess whether winning subsidies help relax credit constraints, which are measured with balance sheet information.

constraint – as most papers that study the effectiveness of subsidy programs would predict. By contrast, if the firm does not have good projects or is poorly managed, the credit constraint may simply reflect these other shortcomings. In such cases, financing *per se* is not the hurdle. Providing a subsidy may thus only lead to a transitory impact on a CC firm's outcomes, and perhaps no incremental impact relative to NC firms.

We find that the higher asset growth of CC firms on the back of subsidies does not translate into improved outcomes in terms of sales, profitability or productivity. This finding suggests that banks may be less willing to lend to these firms for valid concerns around their future prospects and credit risks. In other words, a lack of funding does not seem to be keeping these SMEs behind, at least in the case of Hungary.

That said, our findings do not imply that subsidies have no effect on firm outcomes. The results accord with the literature that shows that EU subsidies improve a broad range of treated firms' outcomes (e.g. Dvouletý et al. [2021]).[4] Complementing this literature, we show that subsidies improve growth outcomes for *both* CC and NC firms that receive a subsidy – i.e. the impact is not driven by any one of these firm subgroups. This is an important finding in its own right, as it underscores that CC firms did not do *worse* than NC firms in terms of making use of the subsidies.

A key factor that made our analysis possible is the access to three datasets relevant for assessing the EU subsidy program in Hungary: (1) Hungarian Credit Registry with information on the credit history of corporations; (2) the European Union's Structural and Cohesion Fund's data which contains information on all subsidy applications, both successful and rejected; and (3) the Tax Return Dataset from the National Tax and Customs Administration that includes firm balance sheet and income statements. In contrast to previous studies that have utilized one or more of these data (or comparable data in other jurisdictions), to the best of our knowledge, ours is the first study to unify them all.

These data have two important features. First, they provide information on unsuccessful subsidy applicants. These firms constitute a better control group for successful applicants as compared to a control group that consists of Hungarian firms in general. Indeed, applicant firms constitute a more homogeneous group as they reveal their genuine need for financing by paying the fixed cost of a subsidy application.[5]

Second, the data allow for a *direct indicator* of CC firms: we consider a firm to be credit constrained if it has been credit checked by a bank not having any contact with the firm –

[4]Also see Banai et al. [2020], Benkovskis et al. [2018], Dvouletý and Blažková [2019], Muraközy and Telegdy [2020].

[5]Muraközy and Telegdy [2020] use similar data to show that using unsuccessful subsidy applicants as controls (instead of all Hungarian SMEs) helps diminish any systematic pre-treatment differences between control and treated firms.

akin to a loan application – but has not received any loan subsequently.[6] Typically, banks check the credit worthiness of new, unrelated clients prior to a credit decision. Therefore, firms that we consider credit constrained have applied for bank credit but failed to get one. Our approach is related to Jiménez et al. [2012] who also use credit checks by unrelated firms to identify loan applications (their goal is to study the bank-lending channel of monetary policy transmission). Among the remaining firms, those that received bank credit in the year prior to applying for a subsidy, are considered unconstrained. We categorize the rest of the firms as uncertain in this respect and drop them from the analysis.

Methodology wise, we use a triple difference-in-differences approach in panel regression. We compare firm outcomes (1) before and after the subsidy (2) across firms that received a subsidy with those which had applied but were rejected and (3) across CC and NC firms. We test the robustness of our findings by applying a combination of exact and propensity score matching between control and treated firms, using alternative definitions of the credit constraint indicator (i.e. using a different time horizon over which we define the CC indicator), or focusing on firms that win a subsidy only once. These alternative estimations reinforce our baseline findings.

The findings in this paper place fresh spotlight on the question of whether financial assistance policies should be targeted towards CC firms. It suggests caution about excessive optimism on the potential impact of subsidies on easing credit constraints. Funding CC firms may not have a large multiplier effect if lack of managerial abilities or opportunities instead of financing are the more relevant hurdles for these firms. Further research on why some SMEs are unable to get the desired level of funding is thus warranted. This can help policymakers design programs that are focused on solving the issues that underpin financing constraints. This is even more important now given that the Covid-19 pandemic led several economies globally to extend or adopt subsidy programs.

The rest of the paper proceeds as follows. In Section 2, we describe the institutional framework of the EU-funded subsidy program. In Section 3 we describe the three datasets used in the study. We then describe our approach to identifying financially constrained firms in Section 4. We present the empirical methodology and detail the results in Section 5. In Section 6, we show results of the robustness analysis and finally, Section 7 concludes.

[6] The literature uses a number of methods to indirectly identify credit constrained firms, including ex-post identification following financial assistance [Banerjee and Duflo, 2014], structural models separating credit supply from demand (e.g. Kremp and Sevestre [2013]; Atanasova and Wilson [2004]), information on listed firms (e.g. Fazzari et al. [1988]; Kaplan and Zingales [1997]), balance sheet information (Mulier et al. [2016]), direct information on financial constraints of companies through survey data (Gómez [2019]; Gorodnichenko and Schnitzer [2013]; Ferrando et al. [2017]). Other researchers used information on bank liquidity and firm-bank relations (Bentolila et al. [2018]; Chodorow-Reich [2014]; Huber [2018]; Paravisini et al. [2015]).

2 EU subsidy program in Hungary

The European Union's Structural Funds and the Cohesion Fund programs operate with the aim of reducing disparities in the region. During the 2007-2013 programming period, almost EUR 25 billion (HUF 7000 billion, ie close to a quarter of the annual GDP in 2013) was allocated to Hungary through these programs [Boldizsár et al., 2016].

The main objective of these funds for this period has been increasing employment and fostering sustainable growth. While a majority of the available budget was allocated to public projects that aimed to achieve this goal indirectly (such as through infrastructure development, energy grid mobilization or improving education), a significant portion was handed out directly to Hungarian companies in the form of non-refundable investment grants in order to directly stimulate the growth of these firms. Most of these grants were concentrated in the Economic Development Operational Program (EDOP) and the Regional Development Operational Program (RDOP). This paper focuses on subsidies disbursed as part of these two programs, totaling almost HUF 1800 billion, or EUR 6 billion. During the 2007-2013 programming window, about 31 thousand firms applied for these subsidies (some applied multiple times). More than two-thirds of the applicants received a subsidy. For a year-wise distribution of applicant firms and wins, see Table 12 in Appendix A.

The calls for proposals for these subsidies generally covered specific, relatively small investments, such as the purchase of machinery or IT equipment. Accordingly, the vast majority of the subsidy requested amounts are well below HUF 160 million (EUR 0.45 million) – the 95$^{\text{th}}$ percentile – while the median is about HUF 10 million (EUR 0.03 million). The typical subsidy covers 50 percent of the total cost of the investment, but in some cases this amount can be as low as 30 percent. In either case, these subsidies represent a significant source of funding investment for many firms in our study (see Figure 7 in Appendix D).

The evaluation process of subsidy applications was rather streamlined. In many cases, the decision-making was essentially automated, with each eligible proposal receiving funding until the allocated sum of money was depleted. And while the assessing authority might have looked at the financials of the applicant, or some details of the project, thorough screening was not the norm. The operational modality of the program limits selection bias and strengthens our identification because subsidies were not targeted towards CC firms per se (see also Table 1 which shows that the fraction of CC firms among subsidy winners and losers is similar). The average/median time for a subsidy decision following an application was less than 4 months, while the time needed to sign the contract following a positive decision was about a month.

3 Data

We build a novel firm-level micro database by combining three administrative sources. First is the Unified Monitoring Information System of the Prime Minister's Office, giving us access to all applications falling under the Economic Development Operational Programme and the Regional Development Operational Programmes in the 2007–2013 programming period. We observe the date of submission, requested amount, date of evaluation, payment date(s), and outcome for each subsidy application. In addition, the data contain supplementary information such as the sub-program under which the application was submitted and the own contribution of the applicant.[7]

Second, we obtain loan histories of companies from the Central Credit Information System (CCIS). This dataset contains information on all outstanding loans disbursed by banks and non-bank financial institutions to businesses. Available variables include an identifier for the lending institution, loan origination date, and the duration and size of the loan.[8]

A special feature of this dataset is that since 2012 it also provides information on the queries initiated by banks to the CCIS. While the purpose of a query is not available in the dataset, the main goal of the CCIS is to provide a unified way for banks to vet their clients' credit histories before disbursing a loan.[9] Yet, banks can also use the CCIS to monitor their current clients. In either case, the fact that each query entails a (small, but non-zero) cost for the financial institutions suggests that these queries are made with a definitive purpose.[10] As described in the next section, data on loan queries is at the core of our approach to identifying credit constrained firms.

Finally, we obtain data on companies' financials – ie balance sheet and income statements – from their annual tax returns to the National Tax and Customs Authority. These data include all enterprises subject to double-entry book-keeping, thereby covering a large majority of Hungarian SMEs.

Each of the above datasets contain the tax number of the companies as a unique identifier that we use to link the datasets. To summarise, two key features of the database are that we observe both successful and failed subsidy applicants, and that we can directly identify credit constrained firms instead of relying on indirect measures or estimates. These features

[7] For more information on the subsidy programs and the related dataset, see for instance Banai et al. [2020].

[8] See appendix I for a detailed description of the dataset, and Banai et al. [2016] for some of the caveats around its use.

[9] In fact it is mandatory in Hungary for banks to query the credit history of firms before issuing a loan.

[10] The cost of a query is usually around 2-3 euros.

provide a solid basis to study the differential impact of subsidies across CC and NC firms.

3.1 Data preparation

The starting point of our analysis is the set of firms that applied for the subsidy program at least once. We restrict attention to applicant firms (both winners and losers) rather than the universe of SMEs in Hungary. This is because applicant firms – by revealing their funding needs – constitute a more homogenous group than Hungarian SMEs in general. Relatedly, applicant firms that lost constitute a better control group for applicant firms that won as compared to non-applicant firms. This approach ensures that we eliminate important unobserved heterogeneity that has to with with applying for a subsidy.

Among applicant firms, we restrict attention to for-profit non-financial firms for which financial information is available. We also only consider micro, small, or medium-sized (SME) firms with at least 5 employees. This is because in case of very small firms data quality is less reliable.[11]

To keep our analysis focused, we focus on non-refundable subsidies that are classified as having the purpose of economic development. We exclude from our analysis those successful applications where the subsidy received by the firm was eventually paid back to the authorities (say because the planned project was not undertaken eventually). And we only look at applications between 2012 and 2015. This is due to the fact that while subsidy applications were submitted throughout the period 2007–2015 (the official period of the program we are assessing), credit query data is only available 2012 onward. Given that a substantial number of applications happen after 2012 (recall Table 12 in Appendix A), we do not expect this data limitation to introduce a systematic bias in our analysis. Finally, we exclude applications where the subsidy amount is larger than HUF 500 million.[12]

The final sample consists of HUF 790 billion worth of investment subsidies. For a detailed description of the number of applicant firms and observations at each step of the data preparation process, we refer the reader to Appendix C.

[11]Firm-year observations with seemingly erroneous data, namely negative leverage or sales revenue, pre-tax ROA of more than 200% in absolute value are also dropped. Moreover, to avoid selection biases, we drop firms from sectors with very few firms, namely 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 defence, compulsory social security; activities of households as employers; activities of extraterritorial organisations and bodies.

[12]This is to avoid biases given that idiosyncratic factors may play a role in the success of applications with very large subsidy requested amounts. Also, large firms usually have to present a business plan so decision making is not automated and political considerations may also play a role.

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

	Never won a subsidy	Won a subsidy at least once	Total
CC	184 (9%)	433 (21%)	617 (30%)
NC	434 (22%)	974 (48%)	1,408 (70%)
Total	618 (31%)	1,407 (69%)	2,025

Notes: The table shows the distribution of firms in our sample by outcome of subsidy application and credit constrained status. The percentages are expressed relative to the total number of applicant firms in our analysis sample. Note that the analysis sample excludes firms whose credit constrained status is 'unknown' at the time of application. There are 847 such firms.

4 Identifying credit constrained firms

Directly identifying credit constrained firms is one of the key contributions of this paper. We consider a firm to be credit constrained (CC) if it had applied for a loan but did not receive one. While this definition of a CC firm is not directly implementable as we do not observe loan applications, the data on loan queries allows us to get around this limitation and construct a firm-level indicator of the credit constrained status based on the following idea.

To determine whether a firm F is credit-constrained at the time of its subsidy application, say date t, we assess its credit history in the one year prior to date t.[13] We first identify loan applications. To this end, we consider credit queries made by banks on firm F during this period. Our identifying assumption is that a credit query on firm F by a bank B which did not have an existing lending relationship with firm F must be because of a new loan application by firm F at bank B. The non-existence of the lending relationship between firm F and bank B is key here because a loan query does not necessarily imply a loan application as banks may periodically check the credit history of their existing borrowers – as also emphasized in Jiménez et al. [2012].[14] Second, we argue that if firm F has a credit query initiated by an *unrelated* bank (which is akin to a loan application) but no new loans from any bank in the one year prior to date t, then firm F must be credit-constrained on date t. On the contrary, if a firm has successfully taken out one or more loans in the one year before date t, we consider it to be unconstrained (NC) on date t.[15]

[13] The choice of the 1-year window to determine the credit constrained status is guided by the average number of new loans per year by firms in Hungary, which is about 0.82. This implies that the mean duration between loans is about 15 months. See Figure 8 in Appendix E for the loan frequency distribution.

[14] In theory, the credit registry could also function as a channel for banks to find suitable firms for offering new loans. This is, however, legally forbidden in Hungary.

[15] A potential issue with this classification is that despite getting a loan, a firm may still be *somewhat* constrained if it did not have receive as much funding as it wanted. While this channel can introduce a bias, we expect the bias to be conservative ie provide a lower bound on the incremental effect of subsidies

Table 2: **Comparison of characteristics of CC and NC firms.**

	CC		NC	
	Mean	St. dev.	Mean	St. dev.
Total assets (HUF million)	373.44	702.24	544.04	1960.14
Firm age (years)	12.77	6.23	11.93	6.48
Employee number	24.16	36.84	24.23	33.35
Real tangible assets (HUF million)	138.72	299.60	198.42	696.01
Real sales revenue (HUF million)	633.77	1879.99	782.74	1768.78
Real pre-tax profit (EBITDA) (HUF million)	29.79	82.43	37.58	304.49
Return on Assets (ROA)	0.11	0.16	0.11	0.15
Requested subsidy amount per total assets	0.24	0.53	0.26	1.07
Average no. of new loans pre-treatment	0.92	2.04	2.83	7.33
Labor prod. based on sales revenue	30.29	66.93	36.62	127.70

Notes: Summary statistics of firm characteristics as of the pre-treatment year. ROA is calculated by dividing pre-tax profit by the average value of total assets in the current and the previous year. Profits and net sales revenue is deflated with the product price index, while tangible assets with the investment price index.

We note that this approach leaves firms with no query and no loans in the past year as unclassified. Their status is indeed ambiguous. It may be that a firm had enough resources and chose to not apply for a loan, in which case it is NC, or that a firm did apply for a loan but was rejected without the bank querying its credit history in CCIS (say because the bank already had past information about the firm), in which case it is CC. About one-third of all firms remain 'unclassified' in our approach. While in our main analysis we exclude these firms, we ensure the robustness of our findings by extending the definition of being credit constrained to these firms via a logit model (see Section 6).

Among the 'classified' firms – ie whose credit constraint status is known – about 30% are CC at the time of applying for a subsidy (see Table 1). Crucially for our analysis, this proportion does not differ substantially between the group of firms that won a subsidy and those that did not, thus ensuring that there is no selection bias into winning along the credit constraint dimension.

While intuitive and more direct than estimates of credit constraints based on firm balance sheet and profitability metrics, it is informative to assess how our CC categorization conforms with common intuition regarding firm characteristics that underpin credit constraints. To this end, we compare balance sheet and profitability metrics across CC and NC firms. We show that CC firms are generally smaller in terms of total or tangible assets (see Table 2). Notably, tangible assets typically require lumpy investment funding that CC firms may be unable to procure. In addition, we find that while on average CC and NC firms are of comparable age and employ similar number of employees, CC firms have lower sales

on CC firms.

revenue, and value added and profits pointing to lower labor productivity. A lower labor productivity may reflect the potential scarcity of tangible assets in CC firms.

5 Empirical analysis

Our goal is to investigate the differential effect of subsidized investment on CC and NC firms. We do this in a difference-in-differences (DD) framework. We first define the treatment and control groups, and then present the regression results. As left-hand-side variables, we focus on tangible assets – which is a key target of the subsidy program – and also assess broader firm-level outcomes such as access to credit, growth (in terms of sales and employment) and performance (measured by profitability and labor productivity). At the end of the section, we augment our baseline analysis with matching techniques to show that the results are robust to the use of a control group that is more similar to the treated companies.

5.1 Construction of treatment and control groups

We define a firm as 'treated' if it applied for and won a subsidy at least once during the sample period, and consider year of the first successful application as the reference (ie treatment) year. Firms that applied but *never* won a subsidy are considered as 'controls', and their first application date is taken as the reference year.[16] We consider four years of firm history before and after the reference year to assess pre-treatment trends and impact in the post-treatment period.

Unlike other studies that tend to use non-applicant firms as controls, our approach ensures that treated and control groups are ex-ante more similar to each other. 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 they win, the own contribution in the investment (which is around 50 percent). Table 3 shows that during the pre-treatment period, while control firms are larger (and relatedly requested larger subsidies), they are comparable to treated firms in terms of age, productivity, and number of loans obtained.

Our main outcome variable is the value of tangible assets.[17] The evolution of its

[16] Despite the possibility that a firm can apply for a subsidy multiple times, our approach clearly organizes firms into treated and controls and each firm's history is used only once (See Table 13 in Appendix B for the number of successful and rejected applications). 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 wins from the sample in Section 6.2. [Muraközy and Telegdy, 2020] 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.

[17] Even though subsidies are designed to have a direct impact on a firm's tangible asset growth, it is not

Table 3: **Comparison of characteristics of treated and control firms.**

	Treated		Control		T-test (C - T)
	Mean	St. dev.	Mean	St. dev.	
Total assets (HUF mln.)	367.21	781.20	610.75	2508.61	243.54*
Firm age (days)	4119.72	2217.40	4063.93	2501.65	-55.78
Employee number	22.47	30.66	24.93	35.92	2.46
Real tangible assets (HUF mln.)	145.84	341.25	238.90	1023.69	93.06*
Real sales revenue (HUF mln.)	629.16	1508.73	792.18	1839.03	163.01*
Real pre-tax profit (HUF mln.)	23.80	71.64	23.58	78.33	-0.22
Operational ROA	0.11	0.13	0.10	0.13	-0.01
Requested subsidy/total assets	0.23	0.79	0.47	1.99	0.24**
Yearly average no. of new loans	2.22	6.56	2.24	5.46	0.03
Labor productivity	31.51	66.91	35.64	77.13	4.13

*Notes: Summary statistics of firm characteristics as of the pre-treatment year. Operational ROA is calculated by dividing operational profit by the average value of total assets in the current and the previous year. Net sales revenue is deflated with the product price index, while tangible assets with the investment price index. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.*

unconditional mean is presented in Figure 1. On the left panel, we show the evolution of assets of treated and control firms separately, while on the right panel we further divide the sample by CC and NC. The unconditional means clearly show that assets of firms with rejected and successful applications align firms well – ie evolve similarly – in the pre-treatment period. The lack of pre-trends is also maintained when zooming into the CC and NC subgroup of firms. In the post-treatment period, the effect of the subsidy, in both CC and NC groups, can be inferred from the fact that treated firms increase the value of their assets much faster than control firms. We investigate these (unconditional) observations formally via regressions below.

5.2 Empirical specification: Assessing the impact on assets

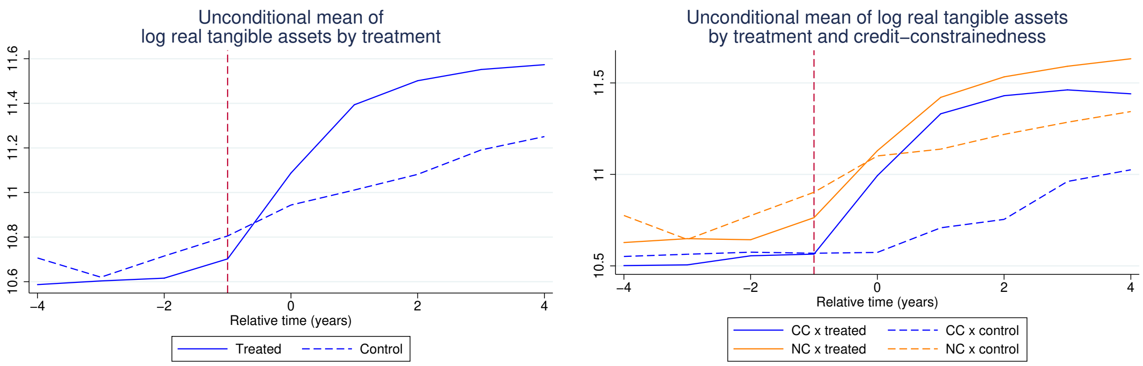
To assess the effect of subsidies on the value of tangible assets, we run the following difference-in-differences (DD) regression:

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

Here i indexes firms, s denotes the two-digit industry NACE code, T indexes years relative to the reference year (which is set to 0) and goes from -4 to 4, and t indexes

obvious that there will be an incremental impact on CC firms. As discussed earlier, a potential incremental impact can help distinguish between the two channels through which subsidies can work – ease credit access versus lower cost of borrowing.

Figure 1: **Evolution of log tangible assets of firms in the sample.**



Notes: Unconditional mean of log tangible assets of firms in the analysis sample by treatment (left-hand panel) and by credit constrained status (right-hand panel). The dotted red vertical line indicates the year immediately before treatment.

calendar years.[18] $treat_i$ is an indicator dummy variable which equals 1 during the entire sample period if firm i received a subsidy, and $post_T$ is set to 1 for the post-treatment years. The effect of the subsidy is measured by β. To attenuate the potential bias of selection into winning, we control for τ_T, a set of year dummies relative to the reference year, for α_i firm-fixed effects, and $\delta_{s,t}$ sector–calendar year fixed effects to control for macroeconomic changes, such as various industry-level price changes. Standard errors are clustered at the firm level.

To assess the overall treatment effect, we start by estimating Equation 1 on the whole sample. Next, in order to assess treatment effects on CC and on NC firms separately, we run Equation 1 individually on the two sub-samples.[19] Finally, in order to assess the statistical significance of the treatment effect on CC firms "relative" to NC businesses – ie the incremental impact on CC firms – we adopt a regression where we include interactions between *treat*, *post* and *CC* (see Equation 2). The coefficient of interest from this regression – which we run on the entire sample – is γ i.e. the treatment effect.

$$y_{i,s,T,t} = \gamma \left[post_T \times treat_i \times CC_i\right] + post_T \times treat_i + post_T \times CC_i + \tau_T + \alpha_i + \delta_{s,t} + \varepsilon_{i,T} \quad (2)$$

The estimation result from the various regressions are shown in Table 4. In the first

[18] Recall that reference year is the year of first win in case of treated firms and the year of first application in case of control firms.

[19] An advantage of this approach is full flexibility: ie all the effects of the control variables are estimated exclusively for CC or NC firms.

Table 4: **Regression results: log tangible assets**

	Log tangible assets DD (1)	Log tangible assets DD if CC (2)	Log tangible assets DD if NC (3)	Log tangible assets DDD (4)
treat=1 × post=1	0.471***	0.639***	0.396***	0.402***
	(8.56)	(6.27)	(6.09)	(6.21)
treat=1 × post=1 × CC=1				0.227*
				(1.90)
N	17643	5396	12234	17643
R2	0.824	0.822	0.827	0.824
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

*Notes: As assessment of the impact of subsidies on log tangible assets of treated firms. Columns 1, 2 and 3 present results from a DD analysis on all, CC, and NC firms respectively. Column 4 presents results from the triple interaction analysis. In all the specifications, a firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.*

column the results refer to DD based on the whole sample. We find that subsidies have an economically large and significant impact on the growth of tangible assets of all treated firms, to the tune of 0.50 log points.[20]

Zooming into the CC and NC subgroups of firms in the second and third columns of the table, we uncover a significant impact in each case. This confirms that the overall finding in column (1) is not driven in particular by one of these subgroups. Moreover, the impact of the subsidy on CC firms is larger than on NC firms (0.64 and 0.40 log points, respectively), despite comparable subsidy amounts received by winning CC and NC firms.[21]

To gauge whether the *seemingly* greater impact of subsidies on CC firms is significant in a statistical sense, we move away from sub-sample DD regressions and focus on the triple-difference regression (see column four of the table). The triple-interaction coefficient – which shows the incremental impact of subsidy on CC firms – is, expectedly, roughly equal to the difference between the coefficients of interest in the subsample DDs for CC and NC firms. Moreover, it is significant at the 10-percent level. This finding suggests that subsidies drive up the tangible assets of CC firms by 0.23 log point more than of NC firms during the post-treatment period.

The results so far shed light on the average impact of the subsidy program on treated

[20]Our finding is broadly consistent with that in [Banai et al., 2020] and [Muraközy and Telegdy, 2020], who run similar regressions for Hungary, although the focus, samples, and methodology used are different compared to those in this paper.

[21]Recall Table 2: the mean subsidy amount received by CC and NC firms is 24 and 26 percent respectively. These amounts are not statistically different at 1% confidence level.

firms in the post-treatment period, but they are silent about how that impact evolves over time. To quantify this, we replace the *post* dummy with a *relative-year* dummy in the DD regressions. The profile over time of the coefficient on the interaction term shows how the two groups evolve in the pre- and post-treatment years after controlling for firm and sector-time fixed effects as well as common trends.

We do not find significant differences in the evolution of the treated and control firms before treatment in all three regressions of interest, namely sub-sample DD on CC firms (first panel of Figure 2), sub-sample DD on NC firms (second panel), and the triple-interaction regression (third panel).[22] The post-treatment impact on CC firms follows an inverted U-shaped profile, i.e. increasing initially and declining afterwards (first panel). By contrast, the impact on treated NC firms stabilises by the third year. Relatedly, the last panel shows that the incremental impact on CC firms relative to NC firms in significant initially but fades away over time, becoming insignificant by the third year post treatment. Yet, recall from Table 4 that the average incremental impact on CC firms during the post-treatment period is significant at the 10 percent level.

A significant impact on NC firms underscores that subsidies reduce the cost of borrowing and makes viable those projects of NC firms that were not viable at the bank loan rate. A significant *incremental* impact on CC firms (relative to NC firms) suggests that in addition to reducing the cost funding, subsidies also ease credit access for CC firms.

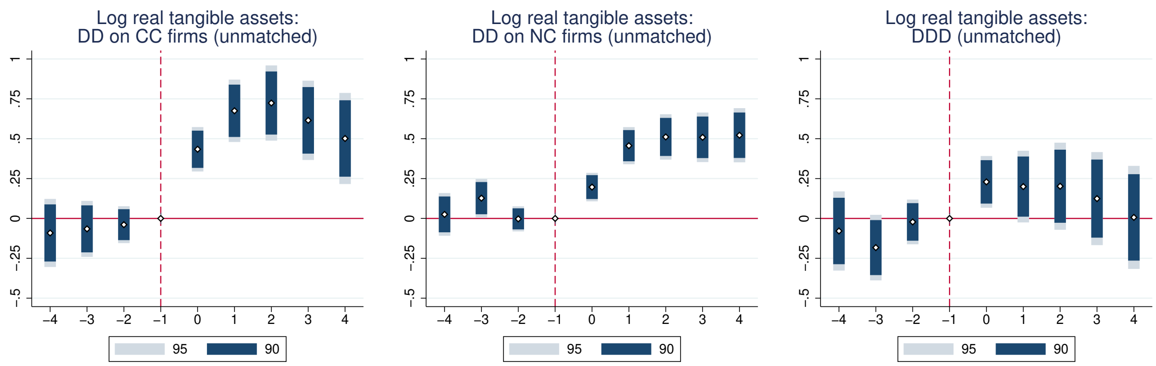
5.3 Digging deeper: do subsidies help CC firms get credit?

We now turn to investigating whether subsidies help CC firms get loans more easily? One reason for why subsidies can induce borrowing is the collateral channel: assets bought with subsidies may be used as collateral for bank loans. Subsidies might also relax credit constraints in the longer run if they help CC firms improve their earnings outcomes, gain more assets, and improve creditworthiness.

To test this hypothesis, we assess whether the value of loans taken out by CC firms differs significantly from that by NC firms post-treatment. To this end, we replace the dependent variable in our regressions with the log of the total amount of loans taken by the firm each year. Table 5 shows that both CC and NC firms take out significantly more loans after receiving a subsidy (the increase in the loan amounts is 2.3 and 1.2 log points respectively). However, the incremental impact of subsidies on CC firms is not significant at any conventional level.

[22]While this confirms the parallel trend assumption, we discuss in Section 5.5 a matching strategy aimed at making the pre-treatment period evolution of treated and control firms even more similar.

Figure 2: **Evolution of the impact of subsidy over time: log tangible assets**



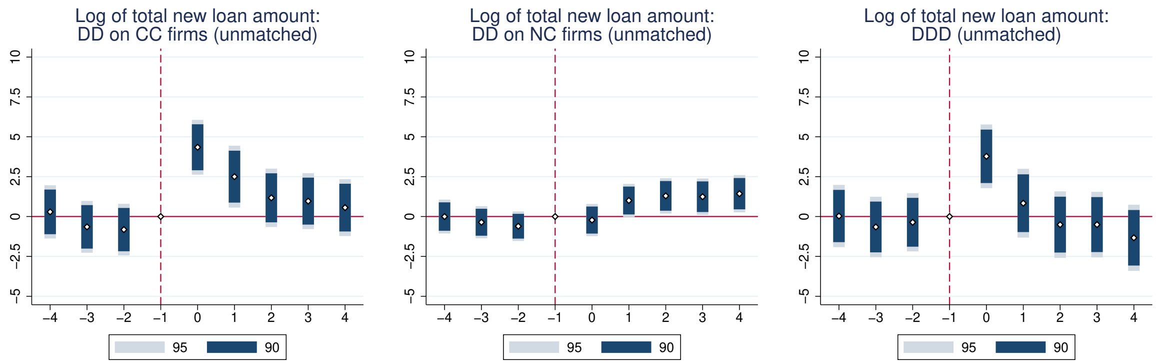
Notes: As assessment of the relative-year wise impact of subsidies on log tangible assets of treated firms. Left and centre panels plot the coefficients on the main interaction term in the DD analysis on CC and NC firms respectively. Right panel plots the triple interaction coefficients. In all the regressions, firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. For the full set of coefficients, see Table 17 in Appendix G.

Table 5: **Regression results: log new loans**

	Log new loan am. DD (1)	Log new loan am. DD if CC (2)	Log new loan am. DD if NC (3)	Log new loan am. DDD (4)
treat=1 × post=1	1.572***	2.307***	1.164***	1.306***
	(5.00)	(3.92)	(3.11)	(3.51)
treat=1 × post=1 × CC=1				0.877
				(1.27)
N	17905	5473	12418	17905
R2	0.400	0.338	0.376	0.400
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

*Notes: As assessment of the impact of subsidies on log new loan amount of treated firms. Columns 1, 2 and 3 present results from a DD analysis on all, CC, and NC firms respectively. Column 4 presents results from the triple interaction analysis. In all the specifications, a firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.*

Figure 3: **Evolution of the impact of subsidy over time: log new loans**



*Notes: As assessment of the relative-year wise impact of subsidies on log of the value of new loans taken out by treated firms. Left and centre panels plot the coefficients on the main interaction term in the DD analysis on CC and NC firms respectively. Right panel plots the triple interaction coefficients. In all the regressions, firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level. For the full set of coefficients, see Table 19 in Appendix G.*

Moreover, the event-time regressions presented in Figure 3 reveal that the impact on the loans taken by CC firms is rather temporary – there is a major increase in loan amount in year 0, followed by a sharp decline. This observations suggests that CC firms may be obtaining more loans in the first years after winning a subsidy to co-finance the subsidised investment. Indeed, subsidy may have served as loan collateral to temporarily ease the credit constraint of a CC firm. Yet, if the collateral value of newly purchased tangible assets declines quickly, or if these assets are not used efficiently so as to generate higher profits in the next few years, the lack of collateral or poor creditworthiness problem of a CC firm would remain unresolved. This would then manifest in the form non-difference in loan pattern of subsidized CC firms relative to non-subsidized ones. For NC firms, the loan value increases somewhat after the subsidy is received and remains stable thereafter.

5.4 Impact on other growth and efficiency characteristics

A somewhat significant incremental impact of subsidies on tangible assets of CC firms, and no such impact on loans of CC firms raises the natural question of whether subsidies improve other aspects of CC firms' growth or efficiency. To this end, we consider the following outcome variables: size of the firm (in terms of employment and sales) financial efficiency (return on assets) and operative efficiency (value of sales over employment).

Table 6: **Impact of subsidies on broader measures of firm growth and efficiency**

Category	Outcome	Impact on All (1)	CC (2)	NC (3)	CC vs NC (4)
Growth	Log Employees	0.130***	0.157***	0.110***	0.065
	Log Sales	0.137**	0.189***	0.115**	0.059
Efficiency	Profitability	0.006	0.014	0.004	0.007
	Log labor productivity	0.004	0.028	-0.001	-0.002

*Notes: Impact of subsidy on firms' growth and efficiency outcomes. Columns 1, 2 and 3 present results from a DD analysis on all, CC, and NC firms respectively. Column 4 presents results from the triple interaction analysis. In all the specifications, a firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level. The corresponding the full set of coefficients are documented in Tables 21, 22, 23 and 24 in the Appendix. *,**,*** indicate significance at the 10,5 and 1 percent levels respectively.*

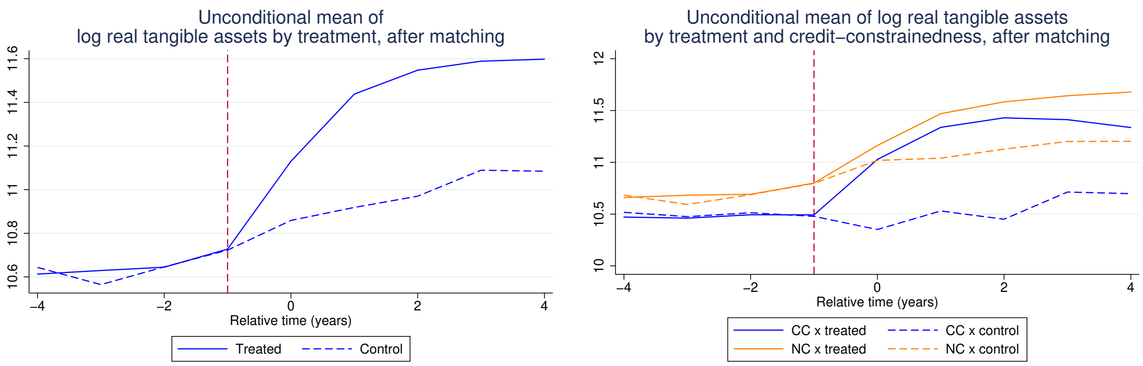
We summarise our findings in Table 6. Subsidies have a significant impact on growth outcomes for all firms as well as separately for CC and NC firms (see first three columns). And while the impact is of a higher magnitude in case of CC firms, the difference between CC and NC firms is not statistically significant (see fourth column). Moreover, we find no effect of subsidies on either financial or operative efficiency. These results underscore that despite the subsidy led larger increase in assets of CC firms relative to NC businesses, there is no attendant impact on sales or efficiency of CC firms.

5.5 Matching

We noted visually in Figure 1, and formally in Figure 2 that average log tangible assets of treated and control groups evolved in parallel during the pre-treatment period – that is, the coefficients on pre-treatment years were not different from zero at any conventional level of significance. These observations support the Parallel Trends Assumption (PTA) – that is the control group offers a valid approximation of the counterfactual post-treatment dynamics of the treated group – and supports a causal interpretation of our main finding. To further test this interpretation, we match the treated and control groups on the basis of their pre-treatment characteristics to further reduce any systematic differences between them.

We begin by estimating a logistic regression to produce propensity scores for each firm in the period before application. As explanatory variables, we use the pre-treatment mean

Figure 4: **Evolution of log tangible assets of firms in the sample after matching**



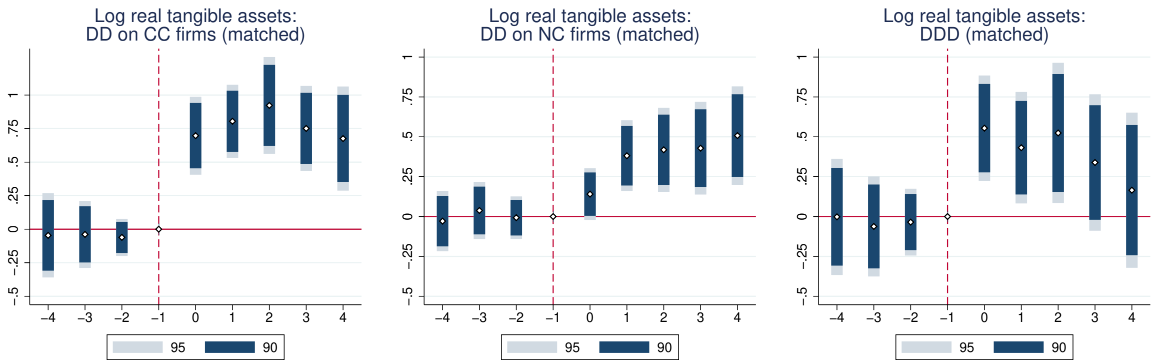
Notes: Unconditional mean of log tangible assets of firms in the analysis sample by treatment (left-hand panel) and by credit constrained status (right-hand panel) after matching. The dotted red vertical line indicates the year immediately before treatment.

of a variety of firm characteristics[23] as well as the ratio of the requested amount of subsidy and value of tangible assets, the treatment year, and whether the firm is credit-constrained. We then use the propensity score to match treated and control firms (using kernel matching with a caliper of 0.05 and imposing common support) while restricting to exact matches within CC and NC categories, 2-digit NACE sectors, three firm-size categories, and tertiles of tangible asset growth. Regarding exact matching, we use the mode of each categorical variable over the four pre-treatment years. For more details about the matching weights and its effectiveness, see Table 15 and Table 16 in Appendix F, respectively.

Figure 4 depicts the mean evolution of log tangible assets for treated and control groups by the CC indicator in the matched samples. Compared to Figure 1, which shows the same statistics for the unmatched sample, visual inspection suggests that matching improves the comparability of evolution of the four groups of firms in the pre-treatment period. More formally (ie using the DD and DDD time-dummy regressions based on Equations 1 and 2), Figure 5 shows that after we control for firm and sector-time fixed effects, there are no significant differences nor pre-trends in the evolution of the treated and control firms before treatment in any of the three cases of interest, namely sub-sample DD on CC firms (first panel), sub-sample DD on NC firms (second panel) and the triple-interaction regression (third panel). At the same time, the significance of post-treatment patterns is further reinforced.

[23]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.

Figure 5: **Evolution of the impact of subsidy over time after matching: log tangible assets**



Notes: As assessment of the relative-year wise impact of subsidies on log tangible assets of treated firms after matching. Left and centre panels plot the coefficients on the main interaction term in the DD analysis on CC and NC firms respectively. Right panel plots the triple interaction coefficients. In all the regressions, firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. For the full set of coefficients, see Table 18 in Appendix G.

Re-estimating the post-dummy regressions (see Table 7) on the matched sample underscores the robustness of our baseline results (recall Table 4). That is, the economic magnitude and significance of the various coefficients of interest are broadly stable across the matched and unmatched regressions. In particular, matching reinforces the conclusion that there is an incremental impact on CC firms' tangible assets that becomes insignificant only in the fourth year after treatment.

Looking at the impact of subsidies on loans, we again note that the results are not sensitive to the use of matched or unmatched samples. As before, loans increase for both CC and NC firms, but this increase is not significantly different across the two group and is driven by the year when the firm received the subsidy (see Table 8 and Figure 6). These results reinforce the view that CC firms do not secure materially more loans in the post-treatment period.

Even in terms of the incremental impact of subsidies on other growth and efficiency outcomes of CC firms, we find that the regression results based on the matched and unmatched samples are generally consistent with each other (see Table 9).

Table 7: **Regression results after matching: log tangible assets**

	Log tangible assets DD (1)	Log tangible assets DD if CC (2)	Log tangible assets DD if NC (3)	Log tangible assets DDD (4)
treat=1 × post=1	0.476***	0.809***	0.371***	0.374***
	(4.89)	(5.53)	(3.21)	(3.22)
treat=1 × post=1 × CC=1				0.434**
				(2.32)
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

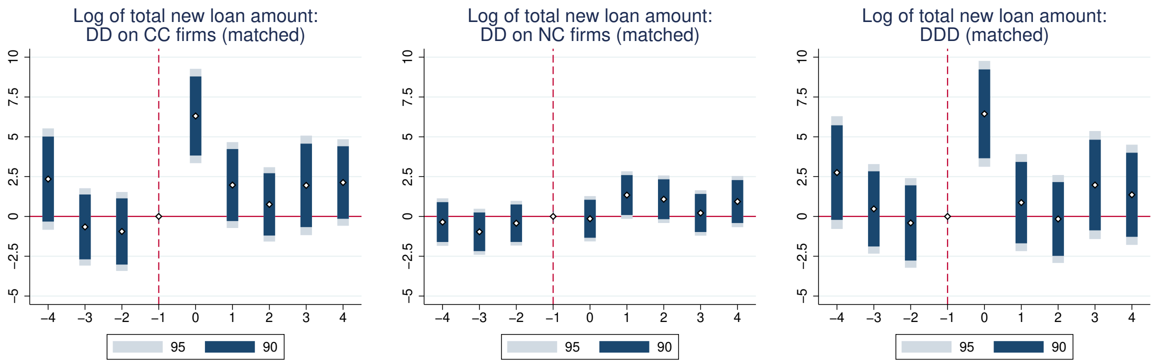
*Notes: As assessment of the impact of subsidies on log tangible assets of treated firms after matching. Columns 1, 2 and 3 present results from a DD analysis on all, CC, and NC firms respectively. Column 4 presents results from the triple interaction analysis. In all the specifications, a firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.*

Table 8: **Regression results after matching: log new loans**

	Log new loan am. DD (1)	Log new loan am. DD if CC (2)	Log new loan am. DD if NC (3)	Log new loan am. DDD (4)
treat=1 × post=1	1.473***	2.488***	1.117**	1.106**
	(3.26)	(3.18)	(2.11)	(2.07)
treat=1 × post=1 × CC=1				1.546
				(1.61)
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: As assessment of the impact of subsidies on log new loan amount of treated firms after matching. Columns 1, 2 and 3 present results from a DD analysis on all, CC, and NC firms respectively. Column 4 presents results from the triple interaction analysis. In all the specifications, a firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.*

Figure 6: **Evolution of the impact of subsidy over time after matching: log new loans**



*Notes: As assessment of the relative-year wise impact of subsidies on log of the value of new loans taken out by treated firms after matching. Left and centre panels plot the coefficients on the main interaction term in the DD analysis on CC and NC firms respectively. Right panel plots the triple interaction coefficients. In all the regressions, firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level. For the full set of coefficients, see Table 20 in Appendix G.*

Table 9: **Impact of subsidies on broader measures of firm growth and efficiency: Matched sample**

Category	Outcome	Impact on All (1)	CC (2)	NC (3)	CC vs NC (4)
Growth	Log Employees	0.164***	0.188***	0.154***	0.038
	Log Sales	0.154**	0.294***	0.112	0.177
Efficiency	Profitability	0.018**	0.027	0.016*	0.010
	Log labor productivity	-0.003	0.103	-0.033	0.128

*Notes: Impact of subsidy on firms' growth and efficiency outcomes in the matched sample. Columns 1, 2 and 3 present results from a DD analysis on all, CC, and NC firms respectively. Column 4 presents results from the triple interaction analysis. In all the specifications, a firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level. The corresponding the full set of coefficients are documented in Tables 25, 26, 27 and 28 in the Appendix. *,**,*** indicate significance at the 10, 5 and 1 percent levels respectively.*

6 Robustness

In this section, we test the robustness of our results by examining alternative approaches to defining the credit constrained status and data sampling.

6.1 Alternative CC identification

To assess how our baseline definition of credit-constraints compares to alternative definitions, we check the correlation of our CC indicator – i.e. CC where the time horizon over which a firm does not get a loan is one year – with two alternative indicators where the time horizon is 6 months (CC_{6m}) or 2 years (CC_{2y}). We find a high and significant correlation of 0.736 and 0.730 between the baseline and these alternative indicators, respectively.

Next, we test whether our main results hold when we use these alternative CC indicators. Our findings – based on the unmatched sample – are reported in the first two columns of Table 10. Broadly, the incremental impact of the subsidy program on log tangible assets of CC firms in case of using CC_{6m} or CC_{2y} is comparable (both in terms of economic and statistical significance) to that of using CC.

Finally, recall that the baseline CC indicator does not provide each firm a credit-constraint status. To expand our analysis to the set of 'unclassified' firms, we use a logistic model of credit constraints based on observable firm characteristics to then classfiy these firms as either CC or NC.[24] We denote the resulting indicator as CC_{logit}. The use of this extended definition in our baseline regression supports our main conclusion (see column three in Table 10).

6.2 Firms with one successful application

Some firms win a subsidy multiple times. This may bias upwards the treatment impact we find in our analysis. This is because by design the impact is attributed to the first successful application, whereas it may actually be the cumulative impact of several subsidies during the sample period.[25] We test if our results hold once we drop firms with more than one win in our sample. As shown in Table 11, we find that all the coefficients of interest are essentially stable and comparable to our baseline findings.

[24] Guided by firm level characteristics that are likely to have a bearing on credit constraints, 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 AUROC of 61.03%.

[25] Muraközy and Telegdy [2020] show that a successful application increases the probability of a second successful application.

Table 10: **The effect of subsidies on log tangible assets with alternative CC indicators**

	Log tangible assets		
	DDD without matching		
	(1)	(2)	(3)
	CC (6m)	CC (2y)	CC (logit)
treat=1 × post=1	0.382***	0.423***	0.402***
	(4.69)	(7.01)	(6.21)
treat=1 × post=1 × CC=1	0.296**	0.250*	0.227*
	(2.31)	(1.74)	(1.90)
N	13733	17643	17643
R2	0.821	0.824	0.824
Firm FE	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes

*Notes: As a robustness check of the estimated impact of subsidies on log tangible assets of treated firms. Columns present results from the triple interaction analysis using CC definitions CC_{6m}, $CC_{2y}1$ and CC_{logit}, respectively. In all the specifications, a firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.*

Table 11: **Regression results on treated firms winning only once: log tangible assets**

	Log tangible assets	Log tangible assets	Log tangible assets	Log tangible assets
	DD	DD if CC	DD if NC	DDD
	(1)	(2)	(3)	(4)
treat=1 × post=1	0.429***	0.625***	0.342***	0.348***
	(7.63)	(6.00)	(5.15)	(5.23)
treat=1 × post=1 × CC=1				0.268**
				(2.20)
N	15578	4869	10695	15578
R2	0.824	0.823	0.827	0.825
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

*Notes: As a robustness check of the estimated impact of subsidies on log tangible assets of treated firms. Only those firms are included in the treatment group which won only once. Columns 1, 2 and 3 present results from a DD analysis on all, CC, and NC firms respectively. Column 4 presents results from the triple interaction analysis. In all the specifications, a firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.*

7 Conclusion

Do non-repayable subsidies help credit constrained firms by more relative to the unconstrained ones? We show that while on average subsidies improve treated firms' outcomes – in line with the literature – the impact on constrained versus unconstrained firms is not uniform. There is an incremental impact of subsidies on constrained firms' asset growth relative to that of unconstrained firms. However, there is no such impact on broader measures of growth and efficiency. Moreover, there is only a transient improvement in the financing situation of constrained firms that receive a subsidy.

Our analysis underscores the need to better understand why some SMEs are unable to get the credit they seek – for example, whether this is due to a short credit history combined with credit rationing by banks, or is it due to a poor credit history. The answer is crucial for several reasons. For one, it can underpin whether there is a market failure that governments want to solve. If so, should financial assistance be targeted towards CC firms. Moreover, should such transfers be made directly by governments, or via banks who may have a comparative advantage in assessing firms' prospects. Answering these questions can help policy makers design more efficient assistance programs. This is crucial especially because governments across the world continue to spend huge resources to subsidize SMEs, including during the Covid-19 crisis.

The paper also highlights that even if banks are unwilling to fund some SMEs for valid concerns around their credit-risk profile, it is not obvious that subsidies should not be targeted at such firms. The answer depends on the objective of the subsidy program. For instance, if the goal is simply to channel funding to lagging SMEs to propel their development and improve their well being – without the expectation that they would use the funding more productively and contribute to a positive multiplier affect on aggregate outcomes – then whether the recipient firm is CC or not may be less relevant. This may apply in particular when CC firms' outcomes following a subsidy are "no worse" as compared to that of NC firms, as we show in the case of Hungary.

Appendices

A Year-wise count of applicants

Table 12: **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

B Number of applications and wins per firm

Table 13: **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

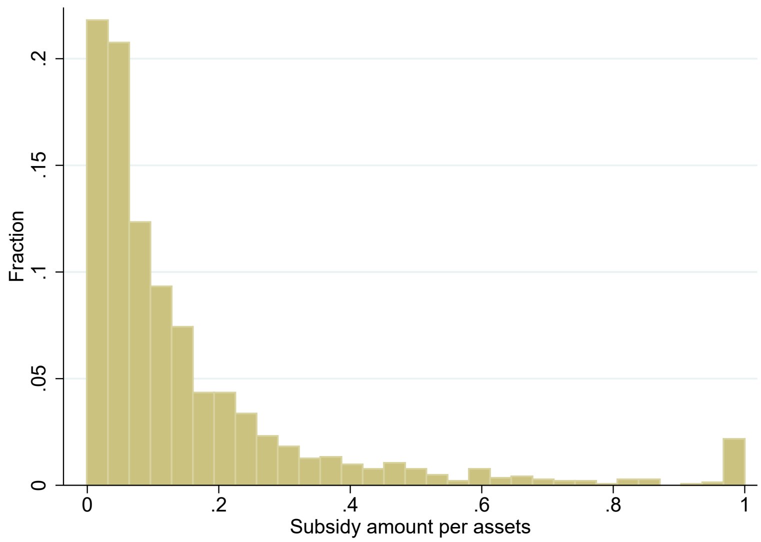
C Data waterfall

Table 14: **Number of firms / observations at the various data creation steps**

Stage	Observations	Firms
Universe of applicants: Total number of applying firms and applications in the application database (recall 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 (recall that this step is needed as loan query data – which is key for the definition of credit constrained status – are 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
Obtain firm financial characteristics for 4 years before and after the treatment year, for total of 9 years (not all firms may have data for all years), delete very large (based on SME code) and very small firms (less than 5 employees) and those operating in select NACE sectors, and delete observations with suspected erroneous firm data	25253	2873
Define the CC indicator and drop firms for which the CC indicator is not defined	17901	2025

D Subsidy requested amount

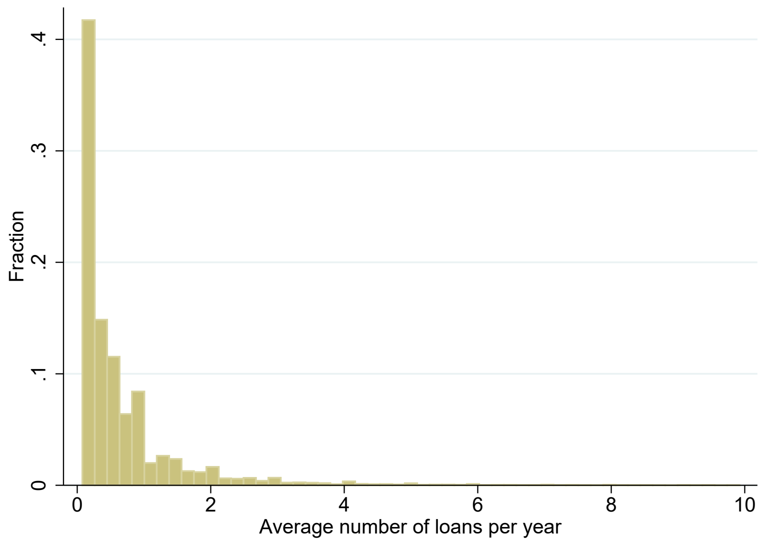
Figure 7: **Distribution of subsidy requested to assets ratio for all applicants**



Notes: Distribution is winsorized at 1.

E Frequency of new loans

Figure 8: **Average number of loans per year during the sample period**



F Matching

Given the kernel matching approach, weights assigned to control firms can be non-integers. Table 15 gives an overview of the weights (rounded to 1) assigned to the controls. Not all treated firms could be matched for three reasons. First is the imposing of a common support (dropping treatment observations with propensity score higher than the maximum or less than the minimum of that of the controls). Second is the imposing of a caliper (ie maximum distance in terms of the score). Third is the lack of a propensity score due to missing firm-level data.

Table 15: **Weights of the matched sample and change of the sample due to matching**

Matching weight, rounded to integers	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
Not matched	224	579	803
Without estimated propensity score	17	25	42
Total	647	1 446	2 093

Table 16: **Effectiveness of the matching**

Explanatory variable	Original sample			Matched sample		
	Mean of treated	Mean of control	T-test of diff	Mean of treated	Mean of control	T-test of diff
Firm age (days)	4119.7	4063.9	0.51	4216.8	4147.8	0.64
Log of employee number	2.616	2.646	-0.71	2.575	2.599	-0.63
Log of real tangible assets	10.572	10.616	-0.51	10.606	10.586	0.23
Log of real value added	10.896	10.916	-0.36	10.873	10.841	0.59
Log of real sales revenue	12.37	12.447	-1.24	12.506	12.429	1.3
Log of real pre-tax profit	8.905	8.958	-0.71	8.916	8.884	0.44
Operational ROA	0.110	0.099	1.9^{*}	0.103	0.103	-0.04
Log of real personnel cost	10.323	10.349	-0.48	10.281	10.251	0.6
Ratio of exports to sales revenue	0.083	0.107	-2.39^{**}	0.088	0.079	0.96
Foreign ownership dummy	0.046	0.078	-3.1^{***}	0.046	0.033	1.43
Req. subsidy amount/total assets	0.230	0.468	-3.92^{***}	0.187	0.251	-3.76^{***}
Yearly gr. rate of real tangible assets	0.103	0.139	-1.76^{*}	0.111	0.080	1.53
Credit-constrained dummy	0.309	0.296	0.57	0.236	0.236	0

Note: Each observation pertains to the mean of the respective variable for the pre-treatment years (but at most four years prior to the treatment). 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. ***, ** *and* * *represent significance at 99, 95 and 90%, respectively.*

G Time dummy regressions

In all regressions in this appendix, columns 1, 2 and 3 present results from a DD analysis on all, CC, and NC firms respectively. Column 4 presents results from the triple interaction analysis. In all the specifications, a firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 17: **Coefficients from unmatched time dummy regressions for log tangible Assets**

	Log tangible assets DD (1)	Log tangible assets DD if CC (2)	Log tangible assets DD if NC (3)	Log tangible assets DDD (4)
treat=1 × -4	-0.00919	-0.0911	0.0253	0.0155
	(-0.16)	(-0.84)	(0.37)	(0.23)
treat=1 × -3	0.0696	-0.0655	0.127**	0.125**
	(1.36)	(-0.73)	(2.07)	(2.03)
treat=1 × -2	-0.0136	-0.0390	-0.00281	-0.00812
	(-0.41)	(-0.66)	(-0.07)	(-0.20)
treat=1 × 0	0.271***	0.434***	0.197***	0.201***
	(7.13)	(6.12)	(4.32)	(4.43)
treat=1 × 1	0.522***	0.675***	0.456***	0.461***
	(10.28)	(6.78)	(7.68)	(7.76)
treat=1 × 2	0.579***	0.724***	0.511***	0.518***
	(9.40)	(6.03)	(7.05)	(7.23)
treat=1 × 3	0.547***	0.615***	0.509***	0.509***
	(8.19)	(4.86)	(6.43)	(6.49)
treat=1 × 4	0.518***	0.501***	0.522***	0.515***
	(6.96)	(3.45)	(6.03)	(6.00)
treat=1 × -4 × CC=1				-0.0787
				(-0.62)
treat=1 × -3 × CC=1				-0.183*
				(-1.74)
treat=1 × -2 × CC=1				-0.0219
				(-0.31)
treat=1 × 0 × CC=1				0.229***
				(2.77)
treat=1 × 1 × CC=1				0.200*
				(1.74)
treat=1 × 2 × CC=1				0.202
				(1.45)
treat=1 × 3 × CC=1				0.124
				(0.84)
treat=1 × 4 × CC=1				0.00653
				(0.04)
N	17643	5396	12234	17643
R2	0.825	0.822	0.828	0.825
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes

Table 18: **Coefficients from matched time dummy regressions for log tangible Assets**

	Log tangible assets DD (1)	Log tangible assets DD if CC (2)	Log tangible assets DD if NC (3)	Log tangible assets DDD (4)
treat=1 × -4	-0.0338	-0.0462	-0.0288	-0.0329
	(-0.40)	(-0.29)	(-0.30)	(-0.34)
treat=1 × -3	0.0213	-0.0390	0.0378	0.0376
	(0.28)	(-0.31)	(0.42)	(0.41)
treat=1 × -2	-0.0171	-0.0610	-0.00718	-0.00810
	(-0.31)	(-0.87)	(-0.11)	(-0.12)
treat=1 × 0	0.274***	0.697***	0.141*	0.142*
	(3.69)	(4.72)	(1.71)	(1.72)
treat=1 × 1	0.485***	0.804***	0.381***	0.382***
	(5.20)	(5.81)	(3.36)	(3.37)
treat=1 × 2	0.542***	0.923***	0.418***	0.420***
	(4.83)	(5.04)	(3.12)	(3.13)
treat=1 × 3	0.511***	0.751***	0.429***	0.432***
	(4.21)	(4.66)	(2.89)	(2.92)
treat=1 × 4	0.549***	0.676***	0.508***	0.511***
	(4.25)	(3.43)	(3.23)	(3.27)
treat=1 × -4 × CC=1				-0.00199
				(-0.01)
treat=1 × -3 × CC=1				-0.0623
				(-0.39)
treat=1 × -2 × CC=1				-0.0350
				(-0.33)
treat=1 × 0 × CC=1				0.554***
				(3.29)
treat=1 × 1 × CC=1				0.432**
				(2.42)
treat=1 × 2 × CC=1				0.524**
				(2.33)
treat=1 × 3 × CC=1				0.339
				(1.55)
treat=1 × 4 × CC=1				0.165
				(0.67)
N	11269	2724	8540	11269
R2	0.814	0.807	0.821	0.816
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes

Table 19: **Coefficients from unmatched time dummy regressions for log total loans**

	Log new loan am. DD (1)	Log new loan am. DD if CC (2)	Log new loan am. DD if NC (3)	Log new loan am. DDD (4)
treat=1 × -4	0.143	0.292	-0.00195	0.0472
	(0.28)	(0.34)	(-0.00)	(0.09)
treat=1 × -3	-0.403	-0.647	-0.358	-0.283
	(-0.85)	(-0.78)	(-0.70)	(-0.55)
treat=1 × -2	-0.597	-0.820	-0.608	-0.572
	(-1.37)	(-0.99)	(-1.29)	(-1.22)
treat=1 × 0	1.275***	4.344***	-0.221	0.0820
	(2.83)	(4.97)	(-0.43)	(0.16)
treat=1 × 1	1.502***	2.501**	1.005*	1.190**
	(3.03)	(2.53)	(1.89)	(2.23)
treat=1 × 2	1.399***	1.173	1.295**	1.493***
	(2.75)	(1.26)	(2.29)	(2.65)
treat=1 × 3	1.253**	0.966	1.242**	1.350**
	(2.44)	(1.08)	(2.13)	(2.33)
treat=1 × 4	1.323**	0.560	1.432**	1.668***
	(2.52)	(0.62)	(2.40)	(2.82)
treat=1 × -4 × CC=1				0.0315
				(0.03)
treat=1 × -3 × CC=1				-0.655
				(-0.68)
treat=1 × -2 × CC=1				-0.360
				(-0.39)
treat=1 × 0 × CC=1				3.776***
				(3.71)
treat=1 × 1 × CC=1				0.835
				(0.76)
treat=1 × 2 × CC=1				-0.507
				(-0.48)
treat=1 × 3 × CC=1				-0.505
				(-0.48)
treat=1 × 4 × CC=1				-1.332
				(-1.26)
N	17905	5473	12418	17905
R2	0.400	0.341	0.377	0.424
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes

Table 20: **Coefficients from matched time dummy regressions for log total loans**

	Log new loan am. DD (1)	Log new loan am. DD if CC (2)	Log new loan am. DD if NC (3)	Log new loan am. DDD (4)
treat=1 × -4	0.399	2.346	-0.354	-0.333
	(0.53)	(1.45)	(-0.47)	(-0.44)
treat=1 × -3	-0.776	-0.658	-0.965	-0.935
	(-1.14)	(-0.53)	(-1.31)	(-1.26)
treat=1 × -2	-0.475	-0.944	-0.426	-0.408
	(-0.72)	(-0.75)	(-0.60)	(-0.57)
treat=1 × 0	1.463**	6.307***	-0.146	-0.117
	(2.20)	(4.19)	(-0.20)	(-0.16)
treat=1 × 1	1.602**	1.970	1.341*	1.359*
	(2.31)	(1.44)	(1.76)	(1.79)
treat=1 × 2	1.107*	0.757	1.079	1.100
	(1.65)	(0.64)	(1.42)	(1.44)
treat=1 × 3	0.747	1.950	0.218	0.244
	(1.10)	(1.23)	(0.30)	(0.33)
treat=1 × 4	1.317*	2.130	0.934	0.957
	(1.84)	(1.54)	(1.14)	(1.17)
treat=1 × -4 × CC=1				2.754
				(1.53)
treat=1 × -3 × CC=1				0.475
				(0.33)
treat=1 × -2 × CC=1				-0.409
				(-0.28)
treat=1 × 0 × CC=1				6.443***
				(3.80)
treat=1 × 1 × CC=1				0.868
				(0.56)
treat=1 × 2 × CC=1				-0.160
				(-0.11)
treat=1 × 3 × CC=1				1.971
				(1.14)
treat=1 × 4 × CC=1				1.360
				(0.85)
N	11424	2761	8659	11424
R2	0.385	0.342	0.360	0.408
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes

H Impact on firms' growth and efficiency

In all regressions in this appendix, columns 1, 2 and 3 present results from a DD analysis on all, CC, and NC firms respectively. Column 4 presents results from the triple interaction analysis. In all the specifications, a firm, sector × calendar-year, and relative-year fixed effects (FE) are included. Standard errors are always clustered at the firm level. * indicates significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 21: **Log employee number**

	Log employee number DD (1)	Log employee number DD if CC (2)	Log employee number DD if NC (3)	Log employee number DDD (4)
treat=1 × post=1	0.130***	0.157***	0.110***	0.110***
	(4.59)	(3.22)	(3.22)	(3.24)
treat=1 × post=1 × CC=1				0.0654
				(1.12)
N	17746	5420	12312	17746
R2	0.816	0.825	0.817	0.817
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

Table 22: **Log real sales revenue**

	Log sales revenue DD (1)	Log sales revenue DD if CC (2)	Log sales revenue DD if NC (3)	Log sales revenue DDD (4)
treat=1 × post=1	0.137***	0.189***	0.115**	0.120***
	(3.59)	(2.77)	(2.50)	(2.63)
treat=1 × post=1 × CC=1				0.0595
				(0.75)
N	17770	5428	12328	17770
R2	0.848	0.870	0.840	0.848
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

Table 23: **Return on assets**

	Operational ROA DD (1)	Operational ROA DD if CC (2)	Operational ROA DD if NC (3)	Operational ROA DDD (4)
treat=1 × post=1	0.00625	0.0142	0.00397	0.00398
	(0.94)	(1.08)	(0.53)	(0.54)
treat=1 × post=1 × CC=1				0.00748
				(0.51)
N	17618	5419	12186	17618
R2	0.353	0.396	0.342	0.353
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

Table 24: **Log labor productivity (log (sales / employees))**

	Log labor prod. DD (1)	Log labor prod. DD if CC (2)	Log labor prod. DD if NC (3)	Log labor prod. DDD (4)
treat=1 × post=1	0.00360	0.0287	-0.000177	0.00461
	(0.13)	(0.53)	(-0.01)	(0.15)
treat=1 × post=1 × CC=1				-0.00284
				(-0.05)
N	17707	5405	12288	17707
R2	0.812	0.810	0.815	0.812
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

Table 25: **Log employee number in the matched sample**

	Log employee number DD (1)	Log employee number DD if CC (2)	Log employee number DD if NC (3)	Log employee number DDD (4)
treat=1 × post=1	0.164***	0.188***	0.154***	0.155***
	(4.75)	(2.87)	(3.90)	(3.93)
treat=1 × post=1 × CC=1				0.0381
				(0.49)
N	11324	2733	8585	11324
R2	0.805	0.782	0.815	0.806
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

Table 26: **Log real sales revenue in the matched sample**

	Log sales revenue DD (1)	Log sales revenue DD if CC (2)	Log sales revenue DD if NC (3)	Log sales revenue DDD (4)
treat=1 × post=1	0.154**	0.294***	0.112	0.113
	(2.20)	(3.07)	(1.32)	(1.33)
treat=1 × post=1 × CC=1				0.177
				(1.41)
N	11344	2734	8603	11344
R2	0.788	0.856	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

Table 27: **Return on assets in the matched sample**

	Operational ROA DD (1)	Operational ROA DD if CC (2)	Operational ROA DD if NC (3)	Operational ROA DDD (4)
treat=1 × post=1	0.0180**	0.0277	0.0160*	0.0156
	(2.13)	(1.57)	(1.66)	(1.62)
treat=1 × post=1 × CC=1				0.0101
				(0.50)
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

Table 28: **Log labor productivity (log (sales / employees)) in the matched sample**

	Log labor prod. DD (1)	Log labor prod. DD if CC (2)	Log labor prod. DD if NC (3)	Log labor prod. DDD (4)
treat=1 × post=1	-0.00353	0.103	-0.0331	-0.0332
	(-0.07)	(1.36)	(-0.58)	(-0.59)
treat=1 × post=1 × CC=1				0.128
				(1.35)
N	11304	2722	8575	11304
R2	0.785	0.816	0.778	0.786
Firm FE	Yes	Yes	Yes	Yes
Sector × Calendar Year FE	Yes	Yes	Yes	Yes
Relative Year FE	Yes	Yes	Yes	Yes

I The Central Credit Information System

Each bank and financial institution is required by law to enter their loans and loan-like contracts towards corporations and sole proprietors into the Central Credit Information System (CCIS). The aim is to have a central repository of data from which banks can query the indebtedness and payment delinquencies of their potential clients. Information entered into the CCIS is retained for a period of 5 years after the end of the contract.

The database, and, in particular, the way it is reported to the Central Bank of Hungary has gone through a number of changes over the years. From 2010–2012, a snapshot of the database has been provided containing various aspects of the loan contract, as well as the identity of the borrower. From 2012–2015, the set of variables being reported was expanded and the quality of the data improved significantly, but at the same time, the identity of the borrower became anonymized. That is, it was possible to link their various contracts within this database, but not to our other data sources. In 2015, there was a change in law that allowed the central bank to discover the tax numbers of the borrowers in the database, therefore allowing it to be joined to the NTCA tax reports and the subsidy data.

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