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Estimating conditional treatment effects of EIB lending to SMEs in Europe

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Abstract

We estimate heterogeneous treatment effects of the EIB financial support on European firms between 2008 and 2015. The relevant control groups are created with propensity score matching and the effects are estimated in a difference-in-differences framework, controlling for firm-level and country-sector-year fixed effects. We find that the positive effects of EIB-supported lending on job creation and investments were larger for smaller and younger firms. Moreover, we find evidence that longer maturities and more advantageous loan pricing are associated with larger employment and investment effects, while no larger impact is observed for larger loan volumes. Overall, the results suggest that benefits of the EIB support are rather observed on an intensive, rather than on an extensive, margin.

1 Introduction

Small and medium-sized Enterprises (SMEs) play a key role in the EU economy (Anginer et al., 2011; Griffith-Jones et al., 2017; de la Torre et al., 2017). They represent 99.8% of EU non-financial companies, 66.6% of total EU employment, and 56.4% of the value added generated by the non-financial business sector (European Commission, 2018).

Yet, SMEs often find it more challenging to access external finance than their larger peers, mostly due to information asymmetries and lack of sufficient collateral to back up the loan (Beck and Demirguc-Kunt, 2006). In financial markets, firms are generally better informed about their prospects than investors, which creates a wedge between the rate of return offered by an entrepreneur and that required by external investor. In order to better account for that difference, external investors, like banks or financial institutions, usually carry out screening

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and/or demand collateral before they agree on a credit contract. However, since SMEs are mostly composed of firms with no credit or track records, uncertain cash-flows and insufficient collateral, they often experience some sort of credit rationing. This can lead to a sub-optimal allocation of financial resources, whereby even the best performing SMEs may become credit constrained.

Firm-specific finance constraints can be suboptimal on an aggregate level, as even viable investment projects may find it difficult to attract sufficient funding (Beck and Demirguc-Kunt, 2006). The gap between the demand for credit from financially viable SMEs and the actual credit supply, known as "SME financing gap", has been one of the key indicators for policy makers to inform about this drawback (OECD, 2006).

The SME financing gap can exacerbate the business cycle. Bank capital is usually scarce during downturns, putting an additional constraint on corporate lending (Gambacorta and Shin, 2016). Furthermore, large downturns are typically followed by consolidation and concentration of banking assets. This, in turn, can make banks even more selective in their credit decisions (Ryan et al., 2014), being a bottleneck for economic recovery. Funding shocks also contribute to the decline in corporate credit supply during the downturns of the financial cycle. For instance, Iyer et al. (2013) show that banks that relied more heavily on interbank borrowing before the Global Financial Crisis (GFC), decreased their credit supply in the post-crisis period significantly more than others, and the credit supply reduction was stronger for smaller firms. Bremus and Neugebauer (2018) find that the decline in cross-border banking flows via the interbank lending channel led to a deterioration in the borrowing conditions of small firms in the EU after 2010.

Banks with a public mandate, such as international financial institutions (IFIs), thus provide SME support in various forms, ranging from grants to more nuanced financial instruments, including direct lending, credit guarantee and counter-guarantee schemes, equity financing and blended instruments (Alem and Madeira, 2015; Gutierrez et al., 2011). Among the IFIs, the European Investment Bank (EIB) stands out in terms of annual volumes. The support to SMEs and mid-caps[1] is also one of its main policy objectives, with more than 35% of all the EIB Group's new lending dedicated to this objective in 2019 only (EIB, 2020).

Policymakers particularly recognized the importance of the SME financing gap in their efforts to mitigate the economic impacts of the COVID-19 pandemic. As part of the crisis management toolkit, many countries, in the EU and beyond, launched initiatives dedicated to supporting businesses' access to finance to cushion the negative impact of the pandemic on economic activity and employment (Gurría, 2020). While our sample stops in 2015, and hence it does not allow us to put under the microscope the pandemic period, our results point to several conclusions which could support the efficiency of the crisis-fighting efforts targeting the corporate sector.

[1] Following the EU recommendation 2003/361/EC, and the eligibility criteria behind the intermediated loans of the EIB Group, throughout the paper by SMEs we refer to firms with up to 249 employees, and by mid-caps to firms with between 250 and 3000 employees.

Impact assessment studies have been used in the past to provide empirical evidence of the efficacy of public guarantee and lending schemes supporting SMEs. When it comes to guarantee schemes, in the United States (US), Brown and Earle (2017) analyse the employment performance of firms that benefited from loan guarantee programmes under the US Small Business Administration (SBA), showing that SBA support did bring an increase of jobs. In Europe, a handful of similar studies have been published in the recent years. Asdrubali and Signore (2015) and Brault and Signore (2019) investigate the effects of guaranteed loans under the EU Multi-Annual Programme and the Competitiveness and Innovation Framework Programme, finding that the public support positively affected growth of firms' assets, share of intangible assets, sales and employment.

Besides guarantees, public support can also take the form of preferential funding channeled to commercial banks. Endresz et al. (2015) evaluate the impact of the National Bank of Hungary's "Funding for Growth" programme on the performance of Hungarian SMEs during the crisis. Using a modified difference-in-differences (DID) framework they find that the program succeeded in generating extra investment in the SME sector that would not have taken place otherwise. Gereben et al. (2019) study the impact of EIB-supported funding on the final beneficiary firms in Central and Eastern Europe. They find that EIB-supported lending has a positive effect on employment, revenues and profitability, and that this impact is also significantly higher when it is provided in a crisis year and firms face a prolonged crisis. Extending the framework to the EU28 sample, Amamou et al. (2020) confirm that EIB lending had a positive effect on employment, firm size, investment and innovation capacity.

While the vast majority of evidence corresponds to binary treatment effects, there are fewer that asks whether the impact varies along the different types of beneficiaries, or by the parameters of the financial support, such as cost of a loan, its maturity or volume. In this respect, we contribute to the literature in three ways.

First, we bring an updated novel data set to the table. While it capitalizes on the original data collected by Amamou et al. (2020), it adds one extra year of observations, hence covering the range from 2008 until 2015. The extra year 2015 is valuable not only because of larger and more recent sample composition. Most importantly, it allows to address some of the concerns that previous studies focused narrowly on the cyclical downturns following the GFC. As in 2015 the EU economy was growing at a solid 2.4 per cent of GDP, this better balances the expansion against the contraction years in the sample.

Second, this study explores in detail the heterogeneity of the treatment effects. In fact, there are both theoretical and empirical arguments that public interventions may not impact all the beneficiaries equally. As the financial constraints are mostly binding for smaller and younger firms, one can expect them to also benefit disproportionately more from public support (Fort et al., 2013; Levenson and Willard, 2000; Canton et al., 2012). The recent evidence from the US suggest that the effectiveness of public support increases with firm size and decreases with age (Brown et al., 2015). We explore this avenue further but estimating conditional treatment effects in treatment-by-covariate specifications.

Third, we go beyond the dummy treatment specification. To this end, we look at the term sheets of individual allocations and estimate the treatment-by-treatment effects of the EIB support. As we are able to distinguish between pricing and volumes, we shed more light on the transmission channels of public interventions. Importantly, under the assumption of conditional exogeneity, these estimates have causal interpretation.

We find that particularly smaller and younger firms exhibit larger post-treatment employment, investment and balance sheet growth compared to bigger or older peers. Our results also show that the terms of the EIB loan agreement do play an important role. To begin with, the benefits of EIB support appear to be linked to longer maturities, when looking at the impact on firm size and investment growth. Our findings show also that better pricing conditions - measured by levels of Transfer of Financial Advantage (ToFA) - are associated with a stronger effect on employment and both total and fixed assets growth. Yet, the results point to no visible impact of (relative) loan size on job creation. We do observe though substantial impact of the loan volumes on fixed asset growth. However, due to possible interactions among this variable and our measure of relative loan size, we cannot clearly identify the causal direction.

Our findings provide several interesting policy conclusions. In particular, we confirm the relevance of public support schemes to alleviate the access to finance constraints among the SMEs. This support appears particularly important among the smaller and younger firms, which are often more vulnerable in terms of job security or performance sustainability. Such support can be perceived as bringing more stability in such an uncertain market environment.

Our results suggest also that the benefits of public support are rather linked to the qualitative rather than quantitative aspects. As the EIB support is directed through the financial intermediaries, the possible conclusion is that such support results in unlocking SME financing to new clients, who are in need of longer maturities or lower pricing, rather than allowing banks to scale up their standard loan books only. This argument needs to be understood, however, in the context of special contractual agreements fostered by the EIB, like for instance the one concerning the requirement to transfer the financial advantage to the final beneficiaries.

The paper is organised as follows. Section 2 outlines our methodology and the econometric framework at the base of our analysis. In Section 3 we describe our data set. The results are displayed and commented in Section 4. We conclude in Section 5.

2 Methodology

The purpose of this section is to expand the original methodology described by Amamou et al. (2020) to explore treatment heterogeneity for given subgroups of subjects. More specifically, following the definition proposed by Abrevaya et al. (2015), we estimate the Conditional Average Treatment Effects on the Treated (CATETs)[2] along the distributions of the pre-selected variables, including both non-treatment (e.g. company's age) and treatment (e.g. size of the loan received in the treatment). We will call them Treatment-by-Covariate (TbC) and Treatment-by-Treatment (TbT), respectively.

To ensure unbiased estimation of CATETs and their corresponding statistics, the covariates used to partition subjects into subgroups must be pre-treatment covariates, and they must be measured using the same procedure for all subjects across groups. Importantly, TbC effects should be rather viewed as a descriptive measure of association between the covariate and the treatment effect. On the other hand, as long as the treatment is assigned randomly, TbT measures can be interpreted causally.

To formalize the framework, let us denote the observed outcome variable for a company i by Y_i, and the treatment variable by $T_i \in \{0, 1\}$. In our case the treatment is determined by the fact that a firm has been reported as a beneficiary of the EIB-funded program, in which case $T_i = 1$ (and otherwise $T_i = 0$). Furthermore, we denote the potential outcome for a treated firm by Y_i^1, and for a non-treated firm by Y_i^0. In terms of potential outcomes, the causal effect of a treatment may be measured as $Y_i^1 - Y_i^0$. For each firm, treated and untreated, we also observe a vector of covariates X_i.

In practice, Y_i^1 and Y_i^0 cannot be observed simultaneously for the same entities. Finding a proper counterfactual is often not easy as selection into treatment may depend on potential outcomes. For instance, looking at the problem through a prism of the EIB support, it could be that firms receive EIB-backed loans simply because they happen to be on a faster growth path than other firms. In that case, comparing the treated and non-treated group averages would likely overestimate the causal effect of the EIB support as even without the EIB support these firms would display better performance $\mathrm{E}[Y^0|T = 1] > \mathrm{E}[Y^0|T = 0]$.

Proper identification of the causal effect requires that the selection bias is negligible (it is often called the unconfoundedness or exogeneity assumption). Even though the bias term is non-zero in most applications, the problem can be addressed by studying and controlling the assignment mechanism. Randomised controlled trials offer a natural solution to the selection bias, as the under the random treatment assignment, the treated and non-treated units will be similar across all the characteristics, including the unobservable Y^0. As bank loans are not allocated in the form of randomised trials, we control the selection bias by selection on observables. In other words, controlling for the observable characteristics X we argue that the potential outcome variables are independent of the treatment assignment, such that $(Y^1, Y^0) \perp\!\!\!\perp T|X$. This allows to minimize the selection bias term and claim causality over the treatment effects and, by extension, over the TbT effects.

[2]Our data properties do not allow to rigorously track the untreated entities, hence we limit our study to the evolution of treatment over the treated subjects only.

To tackle the time dimension, we standardize the treatment year for each EIB beneficiary to $t = 0$.[3] Consequently, the pre- and post-treatment periods we separate by an indicator function $I_{t>0}$, which takes value 1 if $t > 0$ and 0 otherwise. We also define the potential outcomes under treatment and no-treatment for the pre- and post-treatment periods as $Y_i^1(I_{t>0})$ and $Y_i^0(I_{t>0})$, respectively, and we note that the pre-determined observed characteristics are valid only for the pre-treatment period, i.e. $X_i \equiv X_i(0)$.

As emphasized by Amamou et al. (2020), under the assumptions of (i) stable unit treatment value (SUTVA) and (ii) conditional error exogeneity, i.e. $(Y^1, Y^0) \perp\!\!\!\perp T|X$, the Average Treatment Effect on the Treated (ATET) can be estimated as

$$\mathrm{ATET}(1) = \int \mathrm{E}\left[Y^1(1) - Y^0(1)|p\left(X\right), T = 1\right] \mathrm{dP}(X|T = 1). \tag{1}$$

We can introduce the TbC and TbT by conditioning Eq. (1) on additional variables. Under TbC scenario, the extra conditioning factor Z belongs to X, such that $Z \in X$ or $X = (X', Z)$, and under the TbT scenario the extra variable Q is co-dependent on T, and $Q \notin X$. It follows that for TbC

$$\begin{aligned} \mathrm{ATET}(1|Z = z) &= \int \mathrm{E}\left[Y^1(1) - Y^0(1)|p\left(X', Z\right), Z = z, T = 1\right] \mathrm{dP}(X'|Z = z, T = 1) \\ &= \int \mathrm{E}\left[Y^1(1) - Y^0(1)|p\left(X', z\right), T = 1\right] \mathrm{dP}(X'|Z = z, T = 1). \end{aligned} \tag{2}$$

As ATET(1) is identified from a joint distribution of (Y, T, X), identification of $\mathrm{ATET}(1|Z = z)$ follows as $Z \in X$. In particular, under parallel trend assumption, one may derive $\mathrm{ATET}(1|Z = z)$ as

$$\begin{aligned} \mathrm{ATET}(1|Z = z) = &\left[\mathrm{E}[Y(1)|p\left(X', z\right), T = 1] - \mathrm{E}[Y(1)|p\left(X', z\right), T = 0]\right] \\ &- \left[\mathrm{E}[Y(0)|p\left(X', z\right), T = 1] - \mathrm{E}[Y(0)|p\left(X', z\right), T = 0]\right]. \end{aligned} \tag{3}$$

Eq. (3) can be estimated in a two-step approach. In the first step, we construct the matched (treated and non-treated) sample by Propensity Score Matching (PSM).[4] More specifically, for each treated firm with a given characteristic $Z = z$, we look for a firm with the closest fitted propensity score $\hat{p}(X', z)$ which was not treated. (As Eq. (2) is defined over specific values $Z = z$, it can also be aggregated over intervals $Z \in (z_1, z_2)$, with $z_2 > z_1$.) In the second step, conditional on the validity of the propensity scores, we estimate Eq. (3) by linear regression in a difference-in-differences (DID) framework on the matched sample. As our data are longitudinal, the DID estimator allows us to control for unobserved confounders, as long as they remain constant over time.

[3] For instance, if a firm received a loan in 2005, for this firm year 2004 will be represented as $t = -1$.

[4] Specification in Eq. (2) allows for a possibility to match control firms for which z values are unrealistic or simply odd. While it should remain at the discretion of the Researcher, we recommend to match the controls by data cuts within the range of the Z variable.

In a similar way we proceed with the TbT scenario

$$\begin{aligned}\mathrm{ATET}(1|Q=q) &= \int \mathrm{E}\left[Y^1(1) - Y^0(1)|p(X), Q=q, T=1\right] \mathrm{dP}(X|Q=q, T=1) \\ &= \int \mathrm{E}\left[Y^1(1) - Y^0(1)|p(X), Q=q\right] \mathrm{dP}(X|Q=q),\end{aligned} \tag{4}$$

where we exploited the fact that Q is defined only for $T=1$, and we expand the conditional exogeneity assumption to variable $(Y^1, Y^0) \perp\!\!\!\perp Q|X$. For identification purposes, we would need to make an extra assumption that for control firms $Q=0$ and $Q>0$ (strictly) for treated ones. Then

$$\begin{aligned}\mathrm{ATET}(1|Q=q) = &\left[\mathrm{E}[Y(1)|p(X), Q=q] - \mathrm{E}[Y(1)|p(X), Q=0]\right] \\ &- \left[\mathrm{E}[Y(0)|p(X), Q=q] - \mathrm{E}[Y(0)|p(X), Q=0]\right].\end{aligned} \tag{5}$$

The identification then follows the same PSM strategy as in Amamou et al. (2020) but with the control firms matched at the levels $Q=q$. Similarly to the TbC case, the TbT specification can be aggregated over intervals $Q \in (q_1, q_2)$, with $q_2 > q_1$.

There are two practical remarks to be pointed out before we turn to the analysis. Firstly, by the law of total expectation it holds that

$$\mathrm{ATET}(1) = \int \mathrm{ATET}(1|Z)\mathrm{dP}(Z|T=1) = \int \mathrm{ATET}(1|Q)\mathrm{dP}(Q). \tag{6}$$

This feature may be useful to cross-check the magnitude of conditional against unconditional ATETs.

Secondly, the inference from the above framework can be drawn by directly comparing ATETs, estimated separately for the respective subgroups of units. An alternative approach is to use triple DID interaction terms, where the grouping is represented by a factor variable, classified over the distribution of the treated units and assigned to respective controls. In this paper, we apply the latter approach as it turns out to be more efficient and elegant.

3 Data

The EIB supports access to finance and SME sector development mainly through Multiple Beneficiary Intermediated Loans (MBILs). Under the MBIL scheme, the EIB provides loans to financial intermediaries under more favourable conditions compared to the market, either directly or indirectly (through public promotional institutions). The financial intermediaries are then mandated to use the funds to extend loans to SMEs, and to partially transfer to them the financial advantage they benefit from in the form of an interest rate reduction and/or provision of longer tenors. While the final beneficiaries can be subject to pre-specified eligibility criteria, such as the nature of their business and the projects underlying the loan, which are usually linked to key EIB public policy goals and objectives (e.g. innovation and skills, environment, infrastructure, climate action, youth employment, agriculture), in this study we focus on the plain vanilla loans which target solely access to finance problems among the SMEs. That should make the beneficiary firms virtually indistinguishable from the firms that did not recevie the EIB support.

Data on final beneficiaries come directly from financial intermediaries. Contractual provisions against the EIB require the corresponding banks to report on each EIB-funded allocation they make. These data are then stored in tables and reported to EIB on a yearly basis. The allocation tables include basic term sheets of the loan, including the loan size, maturity, and financial advantage, and rather incomplete information about the firms balance sheets and P&L account for the year of allocation.

Before turning to the analysis, we enrich the allocation tables with detailed financial profiles of final beneficiaries, covering the years before and after they received the treatment. More specifically, in a preliminary step we merge the allocation data with financial statements and patent data from Bureau van Dijk's Orbis dataset. To ensure consistency, Orbis is also used to generate a sample of potential counterfactual firms, against which the treatment effects are assessed.

The resulting data set represents an update of that of Amamou et al. (2020). More precisely, we extend the panel of EIB-supported loan allocations by one additional year, covering years from 2008 until 2015. In the remainder of this section, we describe all the steps of the data cleaning process and the key summary statistics, illustrating the main differences between the old and the new data set.

3.1 EIB allocation data

The EIB allocation tables include information on the size of the company and the main sector of operations (following the NACE Rev. 2 four-digits classification), as well as basic terms sheets of the loan, such as the date of allocation processing,[5] loan volume and maturity, and the financial advantage transferred to the final beneficiaries (often referred to as ToFA).[6] The tables at our disposal span a range of 10 years, from 2008 and 2018, however we consider only those allocations that can be monitored for a sufficiently long period after the loan has been disbursed. With 3 years of pre- and post-treatment windows, it leaves us with an effective

[5]EIB collects the allocation reports from financial intermediaries on a regular basis. The frequency of reporting varies from every 6 weeks to a couple of months, keeping in mind that the signature of the sub-loan cannot exceed 6 months as of presentation to the EIB. While we cannot exclude the possibility that some sub-loans slip into subsequent year, for the majority of operations the allocation processing year corresponds to the year of the sub-loan signature. Furthermore, the credit decision from a credit committee at a commercial bank is typically valid for 3 months, within which the sub-loan should be disbursed.

[6]One of the key features of the EIB's intermediated SME financing concerns the quantification of the contractual Transfer of Financial Advantage (ToFA). EIB passes on its funding advantage to financial intermediaries in the form of either more attractive pricing or longer tenors (or both) than available on the markets. These benefits are called financial value added and they are expressed in basis points. The financial intermediary is contractually obliged to pass on a part of this advantage to the final beneficiaries, who sign up for the relevant loan products. The benefits received by the final firms are measured as ToFA. Due to sensitivity of these data, we cannot reveal the concrete ToFA numbers in the summary statistics table.

time range of between 2008 and 2015.[7]

Table 1 exhibits the core summary statistics, including the number of allocations, total amount allocated and average allocation size, with breakdowns by country, year and firm employment level.

[Table 1 about here.]

As compared to Amamou et al. (2020), the extra year of data brings the number of allocations up from 520,746 to 672,222. The reader should be aware that it is possible that a company received multiple EIB-supported loans in the same year, or across the years. We apply the following rules to ensure that the treatment is properly identified. For firms which received multiple loans across years, we consider only the first year as the treatment year. For firms which received multiple loans in the same year, we choose the loan with the largest volume. If a firm received multiple loans with the same volume in the same year, we pick the one with the earliest calendar date. This ensures unique identification of treatment for each treated firm.

At this point, we are left with 514,410 unique beneficiaries with a total of EUR 91.1bn allocated over the years between 2008 and 2015. The data exhibit significant heterogeneity. To begin with, we observe that both the number of loans and their amounts gradually increase over the years, ranging respectively from 39,129 and EUR 6.1bn in 2008 to 151,476 and EUR 18.7bn in 2015. Looking at the data by country, we also see a notable variability, with Spain, Poland and Italy being (in this order) the three largest beneficiaries in terms of average loan amounts. When total loan amounts are considered instead, Poland is replaced by France on the podium. Finally, while companies with 2 to 10 employees constitute the largest chunk of data, firms with 51 to 250 employees received the largest share of allocated loan volumes.

3.2 Financial accounts and data attrition

We merge the allocation data with financial statements and patent data from Bureau van Dijk's Orbis database. Orbis is widely used for microeconometric analysis as it contains firm-level financial statements and ownership data, gathered and standardized to the so-called 'global format', being comparable across jurisdictions. The database receives constant updates in the form of semi-annual vintages, which we aggregate to obtain a sample covering years from 2005 until 2018. New data vintages are also used to update earlier years for possible missing or revised observations. To get the full historical perspective of the accounts, we combine all the available vintages as of October 2019 (the latest available vintage used by Gereben et al. (2019) was March 2019). All nominal variables are reported in EUR, with non-EUR values converted to EUR using the official exchange rate prevailing at the date of each financial report.

[7] For reference, Amamou et al. (2020) consider allocations of up until end-2014, as at the time the study was conducted, 2017 was the latest available year.

The financial data are adjusted to correct for possible inconsistencies, according to the rules proposed by Gopinath et al. (2017) and Barbiero et al. (2020). In particular, observations in which total assets, fixed assets, intangible fixed assets, sales, long-term debt, loans, creditors, debtors, other current liabilities, or total shareholder funds and liabilities have negative values are excluded. We also check whether the basic accounting identities are satisfied. We impose that (i) total asset match total liabilities, (ii) total assets match the sum of fixed assets and current assets, and (iii) current liabilities match the sum of loans, trade credit and other current liabilities. Specifically, we allow a tolerance of 10%, above which the firm-year observation is dropped. Finally, we deflate the nominal variables using the country-specific Harmonised Index of Consumer Prices (HICP) deflators and we winsorize the series by years at 1% level. At this point, the Orbis database contains administrative and financial data on 19,516,283 firms in EU28 with a total of 123,639,465 observations.

The matching process between Orbis and the EIB allocation data set relies on Bureau Van Dijk's own string matching algorithm, which is used to pair entries based on company details, such as company name, address and sector of activity. It can be viewed as similar to the probabilistic matching procedure, proposed by Geurts (2016). The matching algorithm is based upon comparing string similarity, after standardizing the relevant strings for possible trailing characters, typos, different spelling conventions, non-consistent use of accents and special characters. Matching accuracy is reflected in matching accuracy score, which falls between 0 (no similarity) to 1 (perfect similarity). Matches with the joint accuracy above 0.85 are accepted automatically. Less reliable matches are double-checked manually. In case there is no precise and unique assignment between the two data sets, we leave such entries as unpaired and do not consider them in the analysis.

Out of the 514,410 unique beneficiaries from the EIB allocation data set, we find a correspondence in Orbis for 204,792 of them (the matching rate of 39.8 per cent). Nonetheless, even paired beneficiaries may have incomplete corporate records or not sufficient data to cover the entire period of interest (i.e. 3 years before and after the allocation). Taking this into account, we include in the analysis 67,008 treated firms, which is 33 per cent of the matched allocations, or 13 per cent of the original allocations. The final data set we put under the microscope is consistent with previous studies. For instance, Amamou et al. (2020) work with 13.25 per cent of the original number of treated firms, Gereben et al. (2019) only with 4.8 per cent, whereas Asdrubali and Signore (2015) with 18 per cent.[8]

Table 2 shows the success of the matching procedure and the data extraction from Orbis by country, year and employment class, and it illustrates the resulting loss of observations along the process. It can be clearly seen that attrition is not balanced across the data cuts, and especially so across countries and allocation years. For instance, countries like Cyprus, Denmark, Estonia, Ireland, Lithuania or the UK entirely drop out of the sample, whereas for Poland only less

[8]It must be pointed out that even with an access to the full business register, and in the absence of spelling inconsistencies, we would not be able to reach 100 per cent matching score. This is because many firms share the same name which makes them virtually indistinguishable for string matching algorithms.

then 1 per cent of the allocations are successfully matched and populated with data. However, despite these few exceptions, most of the beneficiary countries have good Orbis coverage. Since data attrition is unevenly distributed, we cannot assume that the data are Missing Completely at Random (MCaR). It follows that the treatment effects calculated based our final sample can be considered as Sample Average Treatment Effects on the Treated (SATET), which cannot necessarily be generalised as Population Average Treatment Effects on the Treated (PATET).

[Table 2 about here.]

3.3 Potential controls

In theory, the pool of potential counterfactuals should contain all EU SMEs and mid-caps that have been active between 2008 and 2015. Almost 20 million firms to draw from offers a good starting point in the process. However, to better reflect the characteristics of the treated firms, we create a control group by a stratified sampling approach.

To begin with, we take into consideration the composition of the pool of EIB beneficiaries with regard to country (28 strata), year of allocation (8 strata for years 2008-2015), size groups by number of employees (4 strata for 0-9, 10-49, 50-250 and 250+ employee) and industry groups by sector codes (6 strata for Agriculture (NACE Section A), General Industry (B, C, D, E), Con-struction and Real Estate (F, L), Trade (G), Transportation and Accommodation (H and I),and Other (other sections)). These dimensions are used to define 5376 actual strata, from which 3492 actually contain at least one firm from the treated sample.

We populate the potential control pool by drawing for each stratum a random sample from the same stratum in the full Orbis data set, which is 15 times bigger than the number of treated firms in that data cut. A pre-condition for a firm to be drawn into a sample of potential counterfactuals is that it has not received funding and that it has data for seven consecutive years, centered around a given stratum year, which also defines its cohort. This procedure serves two main purposes. First, it assures that after the matching procedure each treated firm has a sufficient probability to have a counterfactual from its own strata. Second, it better balances the data, as the proportions between the treated and non-treated firms in each stratum become more homogeneous. At this point, the pool of potential controls consists of 1,439,352 individual firms with complete data record.[9]

3.4 Propensity score matching

We construct the final counterfactual control sample using Propensity Score Matching (PSM), with an aim to pair each beneficiary firm with an otherwise identical company that did not receive an EIB-supported loan. Our strategy closely follows the one proposed by Amamou

[9] As Orbis data are not uniformly distributed across strata, we could not always find 15 suitable firms for each treated firm per cluster. This is why we have less than exactly 15 times the number of treated firms. The impact of this discrepancy is at most marginal. To ensure the robustness of the procedure, we repeated the main estimations on a set of potential controls taken as a fixed number of firms per stratum. The results remain unchanged.

et al. (2020), but with some key modifications needed for the estimation conditional treatment effects.

We continue with a data set consisting of treated firms and a random collection of non-treated corporates per strata (see Section 3.3). We estimate the probability of being selected into treatment, conditional on a set of observed financial and non-financial characteristics via a probit regression as

$$\Pr(T_{it} = 1 | X_i(0)) = \Phi(\beta_0 + \beta_1 X_{it-1} + \beta_2 X_{it-2} + \beta_3 X_{it-3} + \mu) \tag{7}$$

where Φ is the cumulative normal distribution and $X_i(0) \equiv \{X_{it-1}, X_{it-2}, X_{it-3}\}$ is a matrix of conditional covariates pre-determined at the time of treatment $t = 0$ for a firm i. It includes a set of firm-specific controls, capturing size, sales, profitability, leverage, liquidity, asset tangibility, and the use of patents. More specifically, we control for total assets (in log), number of employees (in log), total debt (over total assets), cash and cash equivalents (over total assets), tangible assets (over total assets), current assets (over current liabilities), turnover (over total assets), sales (year-on-year growth rate) and a dummy patent variable if a company filled at least one patent application or publication in a given year. For better fit, beyond the variables in levels we also include squared and cubic terms. Finally, we add a vector of fixed effects μ, which includes additively age class and employment, industry, country and year strata.[10]

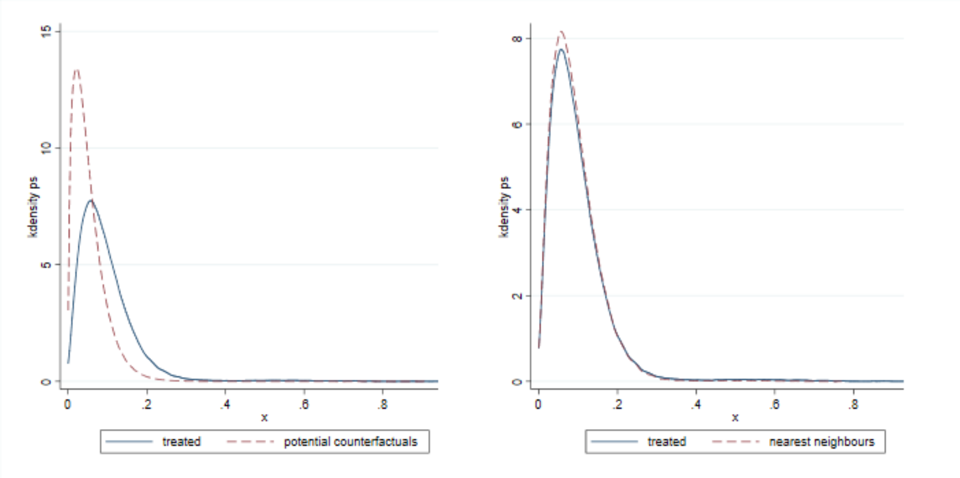
In the next step, the relevant control group is constructed by matching on predicted propensity score for specific TbC subgroups. More specifically, the set of variables $Z \in X$ for which we estimate CATET includes the number of employees and age class. We break down both of them into 4 intervals, i.e. [0-10), [10-50), [50-250) and 250+ for the former, and [0-10), [10-15), [15-20) and 20+ for the latter. For each treated firm in each combination of intervals, we find a non-treated firm from the same combination which has the closest fitted propensity score $\hat{p}(X', z)$. To correct for possible inconsistencies, we further restrict the matching to the firms in the same country and in the same cohort.

The success of the matching process is illustrated in Figure 1. The left panel plots the density curves of the estimated propensity score of the treated (blue line) and the non-treated firms (red line). The PSM model is able to discriminate between the two groups in the sample with the non-treated firms evidently more skewed towards lower fitted probabilities of being treated. The right panel plots the distribution of the estimated propensity score of the treated and the matched control groups. The two lines overlap almost perfectly, which is the desired outcome of the procedure.

Beyond the close similarity of the propensity score distributions, we also track the changes in the balancing properties of the key variables of interest. Table 3 summarizes the average characteristics of the data set for the pre-treatment period, $t < 0$. Figure 2 further visualizes

[10] The detailed probit estimates are available upon request.

Figure 1: Density plots of propensity scores before and after the matching.



Notes: Fitted propensity scores from a probit model for the EIB loan beneficiaries ('Treated'), a full pool of potential controls ('Potential counterfactuals') and the matched controls ('Nearest neighbours').

the results in terms of the bias reduction. We consider the set of the most common financial variables used in the corporate literature.

[Table 3 about here.]

The improvement in the aggregate statistics is evident. The matched controls show a much closer similarity to the treated firms with respect to all variables of interest.

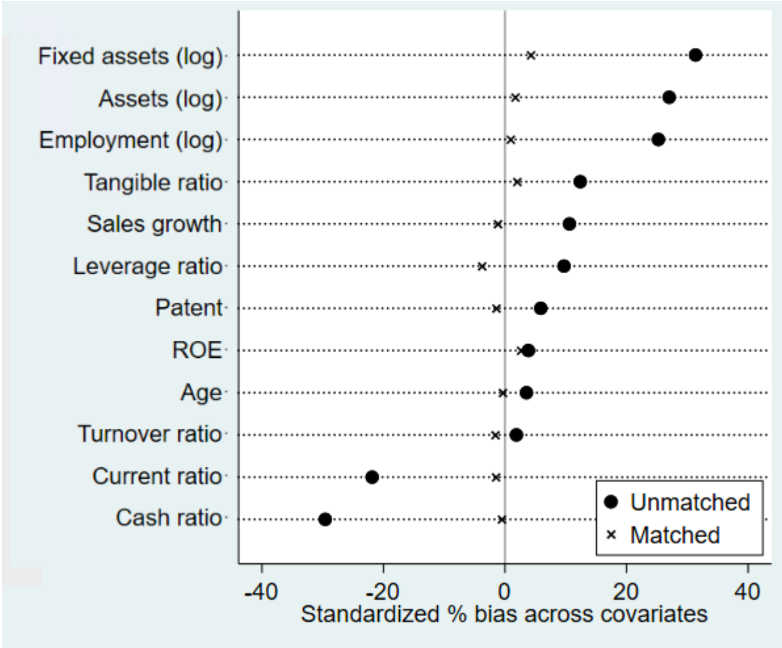
4 Results

While the average treatment effects of EIB support turn out to be largely positive on a variety of outcome variables (Gereben et al., 2019; Amamou et al., 2020), we take a closer look at the effects by subgroups of treated firms. In particular, Sections 4.1 and 4.2 study the TbC effects with breakdowns by firm age and size, respectively. Sections 4.3, 4.4 and 4.5 look at the TbT effects by loan maturity, amount and transfered financial advantage, respectively. For reference, TbC effects should be rather viewed as a descriptive measure of association between the covariate and the treatment effect. On the other hand, as long as the underlying conditional exogeneity assumption holds, TbT measures can be interpreted causally.

We take the unconditional DID estimates of the ATET as our benchmark for comparison. Under the assumption of parallel trends and assuming that the error term is conditionally mean-centered (or more precisely $\mathrm{E}[\varepsilon|I_{t>0}, T] = 0$), it can be verified that in the presence of unobserved country-sector-year time-invariant heterogeneity, the plug-in estimator of ATET matches the estimate of α_2 from the following panel regression

$$Y_{it} = \alpha_1 I_{t>0} + \alpha_2 (T_i \times I_{t>0}) + \nu_{cst} + \xi_i + \varepsilon_{it}, \tag{8}$$

Figure 2: Balancing properties between the unmatched and matched firms.



Notes: The graph depicts the bias, as % of the respective variable level, across the main financial characteristics between unmatched and matched firms.

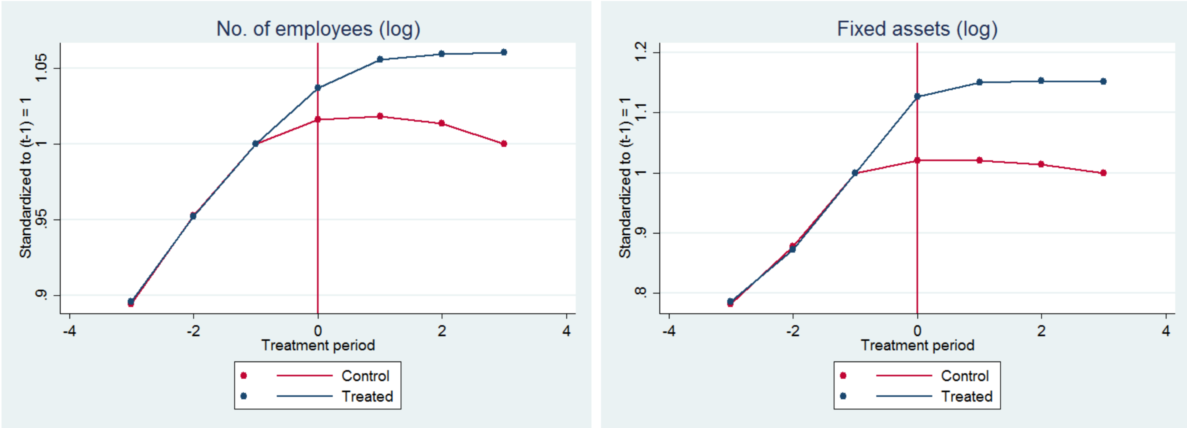
where ν_{cst} is a vector of country-sector-year fixed effects and ξ_i are the firm-level fixed effects (note that firm-level fixed effects ξ_i span over the T_i variable, which is why we do not include it explicitly in the specification). In fact, our data structure allows us to expand the sector dimension to a higher granularity level than in the stratification strategy. We take NACE Rev. 2 classification at 4-digit level as our sectoral fixed-effects cut, absorbing unobserved shocks occurring in each sector in each country and in each year.

Figure 3 illustrates the baseline results with the detailed estimation results presented in the Table A1.[11] We view these estimates as a basis for comparison for our conditional results presented in the next sub-sections. On average, EIB-supported beneficiaries experienced 4.2 per cent higher employment and 11.6 per cent higher levels of fixed assets relative to the control group. While moderately smaller for both variables, the results in fact confirm the findings of Amamou et al. (2020).

[11] To generate the figures we use a richer specification compared to Eq. (8). Instead of splitting the sample to two periods (pre- and post-treatment), we estimate the impact separately for each year before and after the treatment

$$Y_{it} = \sum_{k=0}^{6} \gamma_k I_{t=k-3} + \sum_{k=0}^{6} \eta_k (T_i \times I_{t=k-3}) + \xi_i + \varepsilon_{it}, \tag{9}$$

where η_4, η_5 and η_6 correspond to ATET for years $t = 1$, $t = 2$ and $t = 3$, respectively. We similarly rearrange Eq. (10) to display the CATET impact graphs in the following sub-sections.



(a) Impact on employment.

(b) Impact on fixed assets.

Figure 3: Average impact of the EIB support in the 3 years before and after the loan allocations. The treatment year is set at $t = 0$ with standardized scale $t - 1 \equiv 0$.

Looking at the CATETs, Eqs. (3) and Eqs. (5) can be estimated by the triple DID specification, whereby we consider variables Z and Q to be split in j mutually exclusive intervals as $Z_1 \in [z_1, z_2)$, $Z_2 \in [z_2, z_3)$, ... with $z_1 < z_2 < z_3 <$ ..., and $Q_1 \in [q_1, q_2)$, $Q_2 \in [q_2, q_3)$, ... with $q_1 < q_2 < q_3 <$ .... It follows that under the parallel trend assumption and mean-centered errors for each interval j, CATET for TbC can be estimated as

$$Y_{it} = \beta_0 + \beta_1 I_{t>0} + \beta_2(I_{t>0} \times T_i) + \sum_{j \neq b} \delta_j(I_{t>0} \times Z_j) + \sum_{j \neq b} \gamma_j(I_{t>0} \times T_i \times Z_j) + \nu_{cst} + \xi_i + \varepsilon_{it}, \quad (10)$$

where the fixed effects are specified as in Eq. (8) and b represents the base group for comparison. CATET estimates are captured by referencing γ_j for each of the specified intervals $j \neq b$ against the base group.[12] CATET for TbT are estimated analogously for variable Q_j.

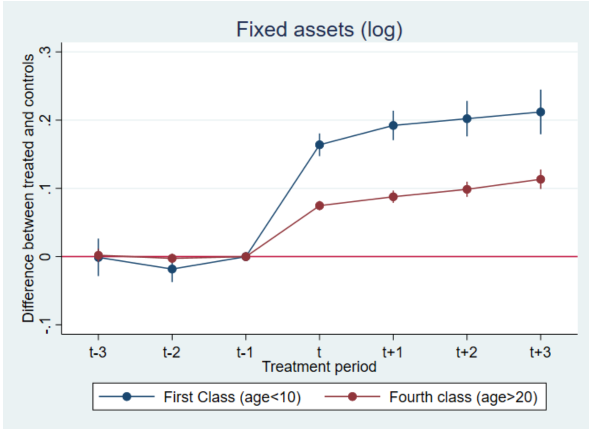
For transparency reasons, in the main body of the paper we present stylized impact graphs per year for the two most peripheral subgroups of the relevant variables (see Footnote 11 for the richer model mechanics). The detailed quantitative results are presented in Appendix.

4.1 Impact by firm age

We split the range of the age variable into 4 mutually exclusive baskets, based on the distribution of the treated firms: 0-9 years, 10-14 years, 15-19 years and 20+ years. The baskets represent 19%, 19%, 19% and 43% of total number of treated firms, respectively. The results are depicted in Figure 4, and the supporting detailed quantitative results are presented in Table A2.

[12] In our specifications the base group is set at $b = 1$.

(a) Impact on employment.



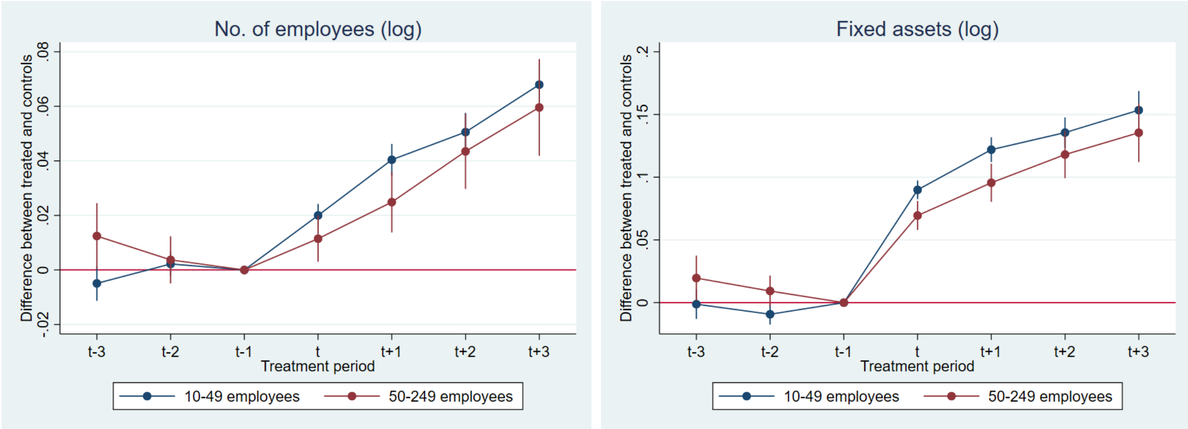
(b) Impact on fixed assets.

Figure 4: Performance difference between the EIB support beneficiaries and control firms in the 3 years before and after the loan allocation for two peripheral firm age baskets. The treatment year is set at $t = 0$ with standardized scale $t - 1 \equiv 0$. Error bars correspond to 95% confidence intervals.

Figure 4 shows that the positive effect of loan reception on job creation decreases as firms get older, halving from 6.2 per cent for firms younger than 10 years (Age Basket 1), to 3.1 per cent for firms with 20 or more years (Age Basket 4). The same pattern applies to fixed assets growth rate. The strongest effect is observed in the first Age Basket, 17.4 per cent. It gradually decreases towards older firms in the last Age Basket, where the impact shrinks to 8.1 per cent. The results are consistent with existing evidence on access to finance and closely mirror the findings of Brown et al. (2015), who also report that employment effects are highest for young firms and decline as treatment firms get older.

4.2 Impact by firm size class

We categorize the firms into 4 mutually exclusive size categories, based on the distribution of the treated firms, i.e. 0-9, 10-49, 50-250 and 250+ employees. The baskets represent 38%, 45%, 15% and 2% of total number of treated firms, respectively. For transparency, we choose the two middle baskets to compare in Figure 5, but the quantitative results for all baskets are presented in Table A3.



(a) Impact on employment. (b) Impact on fixed assets.

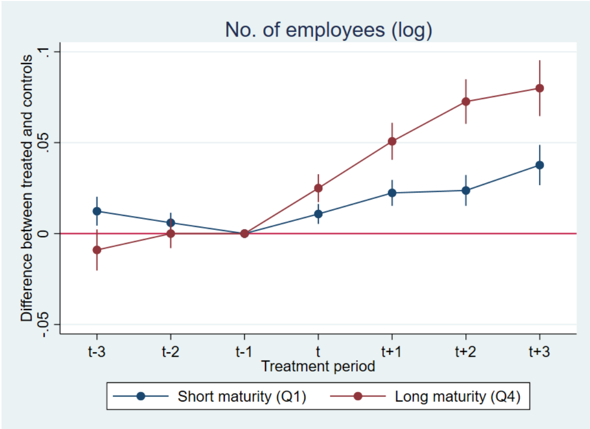
Figure 5: Performance difference between the EIB support beneficiaries and control firms in the 3 years before and after the loan allocation for two firm size baskets. The treatment year is set at $t = 0$ with standardized scale $t - 1 \equiv 0$. Error bars correspond to 95% confidence intervals.

The employment and asset gains from the EIB-support are stronger for smaller firms, and in particular for the second class (10-49 employees). The difference is particularly visible in the first years after the treatment after which the statistical significance fades away (the overall difference is however significant at 0.001 level, see Table A3).

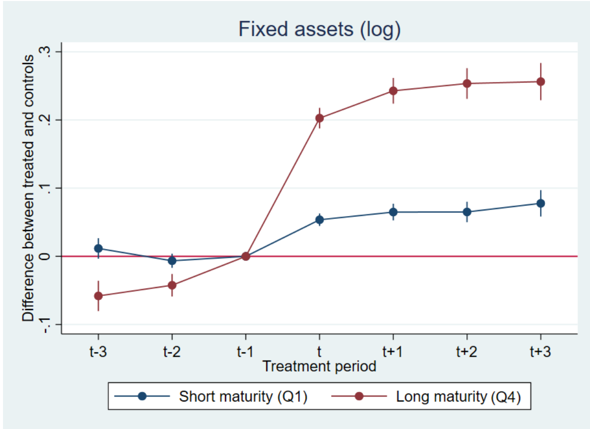
This finding is consistent with the implication that as smaller firms suffer from financial constraints more than larger peers, the benefits from public support are also greater. More specifically, firms with 10 to 49 employees increase their number of employees by 4.9 per cent, on average, in the 3 years after receiving the EIB-supported loan, as compared to firms with 50 to 249 employees, which increased by 3.6 per cent. We observe a similar pattern also for fixed asset growth: 12.3 per cent for firms with between 10 and 49 employees, and 9.3 per cent for firms with 50 to 249 employees

4.3 Impact by loan maturity

We assess the effect of loan maturity on firms' performance by breaking down the maturity distribution of treated firms by quartiles. For reference, in our sample the first quartile includes maturity lower than 3 years, the second quartile maturity between 3 and 4 years, the third quartile maturity between 4 and 5 years, and finally the fourth quartile maturity longer than 5 years. Importantly, as the loan characteristics data are unavailable for control firms, we set for them maturity at 0 years for each of the quartiles.



(a) Impact on employment.



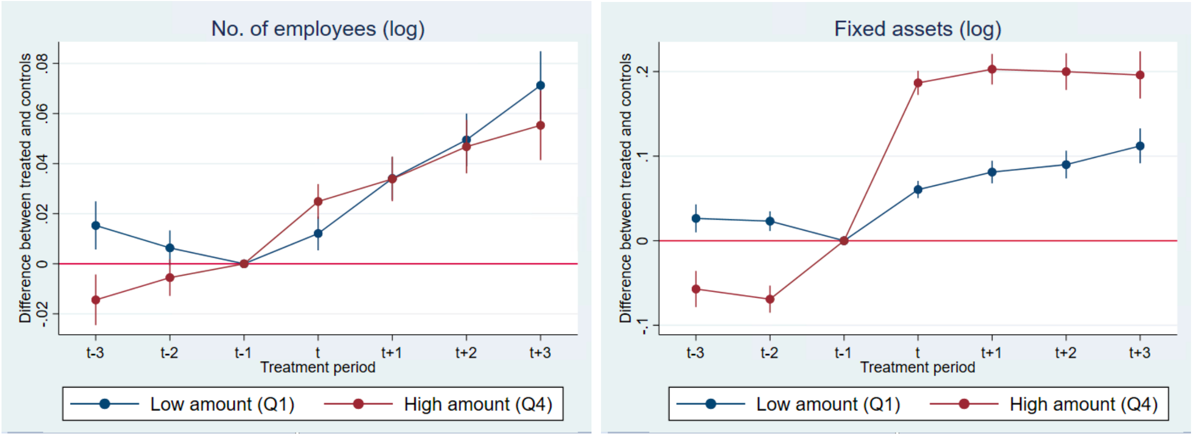
(b) Impact on fixed assets.

Figure 6: Performance difference between the EIB support beneficiaries and control firms in the 3 years before and after the loan allocation for two maturity baskets. The treatment year is set at $t = 0$ with standardized scale $t - 1 \equiv 0$. Error bars correspond to 95% confidence intervals.

Figure 6 confirms that the benefits of the EIB support are linked to longer maturities. This is particularly true with respect to fixed assets growth, with the effects being gradually larger as we move towards the fourth quartile in the distribution. Employment impact, while still positive, can be mostly attributed to the change between the first and the second quartile (see Table A4). One possible explanation is that firms match the life time of an asset with the maturity profile of how it is funded. As longer-term investments are typically larger and more costly, this can become evident in positive relation between the size of the books and loan maturity. At the same time, a firm can cut the number of employees at lower cost than to dispose its assets in case the investment project turns into red at some point in the future. From the decision-making perspective, beyond a certain point, time is less important factor when hiring an employee. This can explain why the employment impact of EIB support materializes mostly at shorter maturities and remains so for longer tenors.

4.4 Impact by loan amount

The effects of loan size are measured for the allocated amounts, normalized by firms' assets size. The data baskets correspond to the quartiles of the treated firms' distribution. As the loan characteristics data are unavailable for control firms, the loan amounts for control firms are set at 0 for each of the quartiles. The impact graphs are presented in Figure 7, and the detailed results are given in Table A5.



(a) Impact on employment.

(b) Impact on fixed assets.

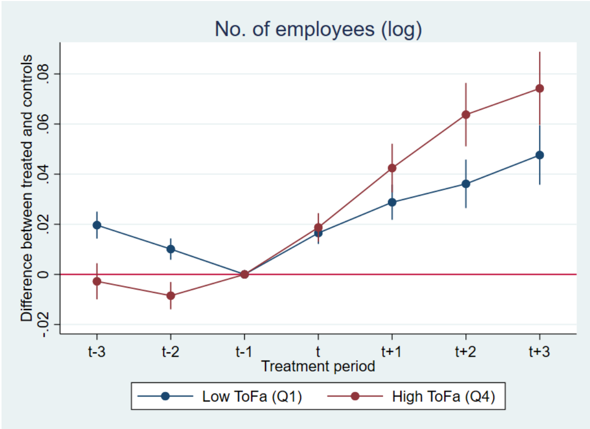
Figure 7: Performance difference between the EIB support beneficiaries and control firms in the 3 years before and after the loan allocation for two loan size baskets. The treatment year is set at $t = 0$ with standardized scale $t - 1 \equiv 0$. Error bars correspond to 95% confidence intervals.

The results point to no visible impact of relative loan size on job creation: the employment results for any loan size basket are not different from each other (see Column 1 in Table A5). We speculate that this is consistent with the fact that staff needs are directly linked to an investment project. It follows that firms plan their resource needs before applying for the loan, such that higher loan amounts do not necessarily adjust the investment project as such and, by implication, employment structure.

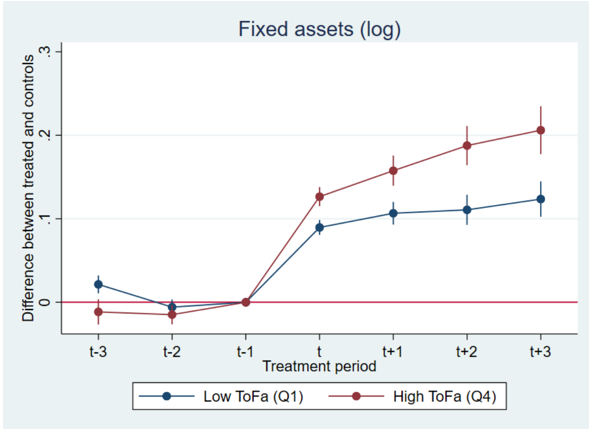
We observe however substantial impact of the loan volumes on fixed asset growth. As normalized loan amount is calculated with total assets in denominator, we cannot clearly identify which of the variables drives the relation. Even though the estimations on non-standardized loan amounts point to a clear positive effects of larger loan tickets, we cannot unambiguously control for company size. As a result, we leave this identification question to be further exploited in future studies.

4.5 Impact by Transfer of Financial advantage

The pricing effects are assessed for different levels of Transfer of Financial Advantage (ToFA). This contractual element represents the main measure of the pricing advantage, associated with the EIB tag, passed on by the intermediary to the final SME. The distribution of ToFA is split into four equally-sized baskets, corresponding to quartiles of the treated firms' distribution. The control firms are given ToFA of 0 for each of the baskets. The impact graphs are presented in Figure 8, and the detailed results are given in Table A6.



(a) Impact on employment.



(b) Impact on fixed assets.

Figure 8: Performance difference between the EIB support beneficiaries and control firms in the 3 years before and after the loan allocation for two ToFA baskets. The treatment year is set at $t = 0$ with standardized scale $t - 1 \equiv 0$. Error bars correspond to 95% confidence intervals.

We find that a higher ToFA is associated with a stronger effect on employment and fixed assets growth. In particular, the positive differential in the effect intensity for employment becomes particularly strong when the ToFA is very large. As a matter of fact, the additional impact on employment (as opposed to the first quartile) is very stable across the second and the third quartiles (hovering around 2 per cent) and then jumps by almost one percentage points (3.1 per cent) in the last quartile. It thus seems that for employment ToFA plays an important role at the tails of the distribution, with the impact being significantly lower for low ToFA levels and significantly higher for high ToFA levels, and stable in-between.

On the other hand, we find that the effect on fixed assets growth increases steadily at each of the ToFA quartiles: 8.6 per cent in Q1, 10.9 per cent in Q2, 12.6 per cent in Q3, and finally 15.4 per cent in Q4. As it can been observed, the increase is very consistent and of roughly 2 percentage points from one class to the subsequent one.

As observed in the figure above, we also find that the positive impact on employment materializes with delay. That underpins the mechanics of this channel, as the gains from lower loan pricing become visible only as the loan amortizes. Following the narrative in Section 4.4, it seems that the employment impact from a higher ToFA is not necessarily linked to the initial investment project the company applied for. While we cannot point to the exact job origination, it is evident that lower financial costs spur the beneficiary firm to create jobs throughout their structures.

Overall, the results suggest that the impact of EIB financial support is rather driven by price than quantity effects. That supports the view that SMEs benefit the most from the “funding advantage channel”, which materializes in better-than-market loan conditions associated with the EIB support (e.g. interest rate reduction). As we do not find positive effects associated with the loan size (see Section 4.4), we conclude that the EIB impact is mostly visible at the intensive rather than extensive margin.

5 Conclusions

The purpose of this study is to assess the impact of the EIB support on the final beneficiary firms for different subgroups of treated firms. In particular, we split the population of EIB-linked firms in the years between 2008 and 2015 by covariates (Treatment-by-Covariate, TbC) and loan term sheet details (Treatment-by-Treatment, TbT). While the TbC effect may be rather viewed as a descriptive measure of association between the subgroup variables and treatment, the TbT can be interpreted casually as long as the treatment is assigned randomly. Our empirical design follows a combination of propensity score matching and difference-in-difference framework, aiming to satisfy the random assignment assumption conditional on a set of observed firm characteristics.

We find that that in particular smaller and younger firms exhibit higher post-treatment employment, investment (measured as change in fixed assets) and balance sheet growth than their bigger or older peers.

Besides heterogeneity by beneficiaries subgroups, our results also show that the terms of the EIB loan agreement do play a pivotal role. To begin with, the benefits of EIB support appear to be linked to longer maturities. Our findings also show that better pricing conditions - measured by levels of Transfer of Financial Advantage (ToFA) - are associated with a stronger effect on employment and fixed assets growth. We also observe substantial impact of the loan volumes on asset growth. However, due to possible interactions among this variable and our measure of relative loan size, we cannot clearly identify the causal relation among them. There is no impact of loan size on job creation though.

Our findings provide several interesting policy conclusions. We confirm the relevance of public support schemes to alleviate the access to finance constraints among the SMEs. This support appears particularly important among the smaller and younger firms, which are often more vulnerable in terms of job security or performance sustainability. At the same time, our results suggest that the impact of policy support is rather driven by qualitative (maturity or ToFA) than quantitative (size of the loan) effects.

While the study expands the previous findings of Gereben et al. (2019) and Amamou et al. (2020), several interesting research avenues remain on the horizon. Firstly, the follow-up designs can put stronger emphasis on innovation performance. In this respect, the sample can be better fine-tuned, by for instance focusing on specific sectors where patent data coverage is more representative. Secondly, while this study points to the clear relevance of the pricing advantage in transmitting the EIB impact, the hypothesis can be further tested by controlling for banks' characteristics, such as liquidity or capital ratios. Last but not least, the COVID-19 crisis offers a good opportunity to measure the resilience of the corporate sector to an unexpected systemic event, distinguishing whether the EIB-supported firms differ from others in their performance through the period of EU-wide lockdowns.

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Table 1: EIB allocation data.

by country					
	Allocations (in #)	Share (in %)	Amount (in mEUR)	Share (in %)	Mean size (in kEUR)
Austria	2,610	0.39	1,714	1.88	658
Belgium	11,650	1.73	2,121	2.33	182
Bulgaria	4,463	0.66	681	0.75	153
Croatia	4,606	0.69	1,724	1.89	375
Cyprus	853	0.13	350	0.38	410
Czech Republic	15,240	2.27	2,275	2.50	149
Denmark	7,057	1.05	698	0.77	99
Estonia	5	0.00	12	0.01	2,429
Finland	1,617	0.24	384	0.42	238
France	43,042	6.40	7,279	7.99	169
Germany	16,964	2.52	4,903	5.38	289
Greece	4,259	0.63	1,980	2.17	465
Hungary	6,620	0.98	1,648	1.81	249
Ireland	3,314	0.49	513	0.56	155
Italy	84,948	12.64	21,189	23.26	249
Latvia	1,856	0.28	197	0.22	106
Lithuania	27	0.00	48	0.05	1,767
Luxembourg	1,524	0.23	766	0.84	503
Netherlands	8,726	1.30	3,492	3.83	400
Poland	114,156	16.98	4,312	4.73	38
Portugal	17,358	2.58	4,373	4.80	252
Romania	7,761	1.15	684	0.75	88
Slovakia	13,121	1.95	1,689	1.85	129
Slovenia	4,558	0.68	736	0.81	161
Spain	282,651	42.05	24,954	27.39	88
Sweden	7,101	1.06	154	0.17	22
United Kingdom	6,135	0.91	2,232	2.45	364
Total	672,222	100	91,108	100	

by year					
	Allocations (in #)	Share (in %)	Amount (in mEUR)	Share (in %)	Mean size (in kEUR)
2008	39,129	5.82	6,142	6.74	157
2009	42,722	6.36	7,259	7.97	170
2010	63,865	9.50	10,082	11.07	158
2011	63,849	9.50	13,148	14.43	206
2012	75,796	11.28	9,326	10.24	123
2013	101,185	15.05	11,311	12.41	112
2014	134,200	19.96	15,134	16.61	113
2015	151,476	22.53	18,706	20.53	
Total	672,222	100	91,108	100	

by employment class					
	Allocations (in #)	Share (in %)	Amount (in mEUR)	Share (in %)	Mean size (in kEUR)
0-1	155,123	23.08	12,019	13.19	77
2-10	245,350	36.50	18,494	20.30	75
11-50	170,191	25.32	25,163	27.62	148
51-250	91,060	13.55	25,597	28.10	281
250-500	5,400	0.80	4,238	4.65	785
501 or missing	5,098	0.76	5,597	6.14	1,100
Total	672,222	100	91,108	100	

Notes: The numbers correspond to the raw data and therefore include multiple allocations to the same beneficiary.

Table 2: Data attrition.

by country					
	Total EIB (in #)	with BvDID (in #)	with BvDID (in %)	with useful data (in #)	with useful data (in %)
Austria	2,062	867	42.05	5	0.24
Belgium	9,008	3,940	43.74	439	4.87
Bulgaria	2,985	2,244	75.18	335	11.22
Croatia	3,925	1,957	49.86	1,188	30.27
Cyprus	828	338	40.82	0	-
Czech Republic	10,327	8,021	77.67	2,667	25.83
Denmark	3,408	2,703	79.31	52	1.53
Estonia	4	1	25.00	0	-
Finland	1,300	957	73.62	209	16.08
France	28,489	14,733	51.71	2,792	9.80
Germany	12,407	5,018	40.44	256	2.06
Greece	3,718	306	8.23	153	4.12
Hungary	4,348	2,815	64.74	1,233	28.36
Ireland	3,022	336	11.12	0	-
Italy	62,537	26,487	42.35	10,818	17.30
Latvia	1,219	369	30.27	36	2.95
Lithuania	26	12	46.15	0	-
Luxembourg	1,040	551	52.98	36	3.46
Netherlands	6,805	2,803	41.19	42	0.62
Poland	82,405	25,930	31.47	482	0.58
Portugal	14,716	6,863	46.64	4,800	32.62
Romania	5,519	4,911	88.98	3,507	63.54
Slovakia	9,036	5,514	61.02	1,786	19.77
Slovenia	3,237	2,428	75.01	1,186	36.64
Spain	230,884	78,141	33.84	33,130	14.35
Sweden	5,906	4,780	80.93	1,856	31.43
United Kingdom	5,249	1,767	33.66	0	-
Total	514,410	204,792	39.81	67,008	13.03

by year					
	Total EIB (in #)	with BvDID (in #)	with BvDID (in %)	with useful data (in #)	with useful data (in %)
2008	29,172	8,681	29.76	2,591	8.88
2009	34,195	12,497	36.55	3,784	11.07
2010	49,068	17,097	34.84	5,001	10.19
2011	47,371	18,865	39.82	5,963	12.59
2012	56,519	16,896	29.89	4,521	8.00
2013	78,357	21,174	27.02	6,450	8.23
2014	101,483	52,706	51.94	18,383	18.11
2015	118,245	56,876	48.10	20,315	17.18
Total	514,410	204,792	39.81	67,008	13.03

by employment class					
	Total EIB (in #)	with BvDID (in #)	with BvDID (in %)	with useful data (in #)	with useful data (in %)
0-1	136,534	22,142	16.22	2,463	1.80
2-10	203,179	93,723	46.13	26,657	13.12
11-50	116,432	61,104	52.48	26,387	22.66
51-250	53,181	24,901	46.82	10,531	19.80
250-500	2,758	1,594	57.80	650	23.57
501 or missing	2,326	1,328	57.09	320	13.76
Total	514,410	204,792	39.81	67,008	13.03

Notes: 'Total EIB' correspond to the figures as reported in the EIB allocation tables, 'with BvDID' describes number and percentage of firms successfully paired with Orbis, and 'with useful data' shows number and percentage of firms with sufficient data coverage to be included in the Propensity Score Matching (PSM).

Table 3: Summary statistics.

	Obs.	Mean	Median	St. dev.	Min.	Max.
Unmatched controls						
Leverage ratio	4,318,057	0.64	0.63	0.38	0.02	2.46
Employment (log)	4,318,062	2.44	2.30	1.25	0.69	6.09
Assets (log)	4,318,062	13.85	13.76	1.77	9.85	17.75
Cash ratio	4,318,057	0.14	0.07	0.17	0.00	0.80
Tangible ratio	4,318,057	0.28	0.21	0.25	0.00	0.94
Current ratio	4,307,715	2.68	1.41	4.60	0.08	35.60
Turnover ratio	4,175,561	1.59	1.25	1.33	0.02	7.52
Sales growth	4,043,495	0.09	0.01	0.50	-0.75	3.38
Patent (app)	4,318,062	0.00	0.00	0.00	0.00	0.00
Patent (pub)	4,318,062	0.01	0.00	0.11	0.00	1.00
Matched controls						
Leverage ratio	209,994	0.69	0.70	0.29	0.02	2.46
Employment (log)	209,994	2.70	2.64	1.17	0.69	6.09
Assets (log)	209,994	14.25	14.25	1.60	9.85	17.75
Cash ratio	209,994	0.10	0.05	0.13	0.00	0.80
Tangible ratio	209,994	0.31	0.26	0.24	0.00	0.94
Current ratio	209,994	1.92	1.27	2.89	0.08	35.60
Turnover ratio	209,994	1.61	1.36	1.17	0.02	7.52
Sales growth	206,362	0.13	0.02	0.53	-0.75	3.38
Patent (app)	209,994	0.00	0.00	0.00	0.00	0.00
Patent (pub)	209,994	0.02	0.00	0.13	0.00	1.00
Matched treated						
Leverage ratio	206,615	0.68	0.69	0.27	0.02	2.46
Employment (log)	206,615	2.74	2.64	1.17	0.69	6.09
Assets (log)	206,615	14.29	14.27	1.59	9.85	17.75
Cash ratio	206,615	0.10	0.05	0.13	0.00	0.80
Tangible ratio	206,615	0.31	0.26	0.24	0.00	0.94
Current ratio	206,615	1.88	1.30	2.61	0.08	35.60
Turnover ratio	206,615	1.61	1.34	1.17	0.02	7.52
Sales growth	203,440	0.13	0.03	0.50	-0.75	3.38
Patent (app)	206,615	0.00	0.00	0.00	0.00	0.00
Patent (pub)	206,615	0.02	0.00	0.13	0.00	1.00

Notes: Summary statistics for unmatched controls, matched controls and matched treated firms in the 3-year pre-treatment period. Firms are paired by the Propensity Score Matching (PSM) technique. Employment is measured as number of employees. Patents are measured as dummies if a company filled at least one patent application or publication in a given year.

Table A1: Impact of the EIB lending - benchmark results.

	(1) Employment (log)	(2) Fixed assets (log)
Post x Treated	0.042***	0.116***
	(0.002)	(0.004)
Post	-0.053***	-0.132***
	(0.002)	(0.003)
Firm-level FE	Yes	Yes
Country x sector x year FE	Yes	Yes
Observations	908,341	910,035
R2	0.951	0.939

*Notes: Estimation results of the benchmark treatment effects model. Employment is measured as number of employees. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table A2: Impact of the EIB lending by firm age.

	(1) Employment (log)	(2) Fixed Assets (log)
Post	0.051*** (0.004)	0.012 (0.008)
Treated x Post	0.062*** (0.006)	0.174*** (0.012)
Post x Age Basket 2	-0.080*** (0.006)	-0.132*** (0.011)
Post x Age Basket 3	-0.124*** (0.005)	-0.179*** (0.010)
Post x Age Basket 4	-0.148*** (0.005)	-0.185*** (0.009)
Treated x Post x Age Basket 2	-0.020*** (0.008)	-0.038** (0.015)
Treated x Post x Age Basket 3	-0.019** (0.008)	-0.058*** (0.014)
Treated x Post x Age Basket 4	-0.031*** (0.007)	-0.093*** (0.013)
Constant	2.773*** (0.000)	13.046*** (0.001)
Firm-level FE	Yes	Yes
Country x sector x year FE	Yes	Yes
R2	0.952	0.941
Observations	871,355	873,086

*Notes: Estimation of the post-treatment effects for various firm age baskets. Age baskets are determined by firm age at the treatment years. Base age basket is 0-9 years. The remaining baskets are 10-14 years (Basket 2), 15-19 years (Basket 3) and 20+ years (Basket 4). Employment is measured as number of employees. Standard errors are clustered at the firm level and given in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table A3: Impact of the EIB lending by firm size.

	(1) Employment (log)	(2) Fixed Assets (log)
Post	-0.070***	-0.153***
	(0.002)	(0.005)
Treated x Post	0.036***	0.123***
	(0.003)	(0.007)
Post x Size Basket 2	0.027***	0.044***
	(0.003)	(0.006)
Post x Size Basket 3	0.032***	0.036***
	(0.005)	(0.008)
Post x Size Basket 4	0.038***	0.013
	(0.015)	(0.018)
Treated x Post x Size Basket 2	0.013***	-0.005
	(0.005)	(0.009)
Treated x Post x Size Basket 3	0.002	-0.030***
	(0.007)	(0.011)
Treated x Post x Size Basket 4	-0.060***	-0.102***
	(0.018)	(0.022)
Constant	2.772***	13.046***
	(0.000)	(0.001)
Firm-level FE	Yes	Yes
Country x sector x year FE	Yes	Yes
R2	0.952	0.940
Observations	871,355	873,086

*Notes: Estimation of the post-treatment effects for various firm size baskets. Size Baskets are determined by employment size class at the treatment year. Base size is 0-9 employees. The remaining baskets are 10-49 (Basket 2), 50-250 (Basket 3) and above 250 (Basket 4). Employment is measured as number of employees. Standard errors are clustered at the firm level and given in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table A4: Impact of the EIB lending by loan maturity.

	(1) Employment (log)	(2) Fixed Assets (log)
Post	-0.055***	-0.128***
	(0.003)	(0.005)
Treated x Post	0.020***	0.054***
	(0.004)	(0.007)
Post x Maturity Q2	0.005	-0.013
	(0.005)	(0.010)
Post x Maturity Q3	0.008**	0.010
	(0.004)	(0.007)
Post x Maturity Q4	-0.002	-0.010
	(0.005)	(0.009)
Treated x Post x Maturity Q2	0.025***	0.042***
	(0.007)	(0.013)
Treated x Post x Maturity Q3	0.035***	0.066***
	(0.005)	(0.010)
Treated x Post x Maturity Q4	0.044***	0.174***
	(0.006)	(0.012)
Constant	2.811***	13.042***
	(0.023)	(0.040)
Firm-level FE	Yes	Yes
Country x sector x year FE	Yes	Yes
R2	0.954	0.944
Observations	781,700	783,321

*Notes: Estimation of the post-treatment effects for various maturity baskets. Baskets are determined by the the Quartiles (Q) of the distribution of loan maturities at the treatment year. Base quartile is Q1 (0-3 years). The other quartiles are 3-4 years (Q2), 4-5 years (Q3) and above 5 years (Q4). Employment is measured as number of employees. Standard errors are clustered at the firm level and given in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table A5: Impact of the EIB lending by normalized loan amount.

	(1) Employment (log)	(2) Fixed Assets (log)
Post	-0.019***	-0.055***
	(0.003)	(0.005)
Treated X Post	0.034***	0.053***
	(0.003)	(0.007)
Post x Rel. Amount Q2	-0.020***	-0.054***
	(0.004)	(0.007)
Post x Rel. Amount Q3	-0.030***	-0.069***
	(0.004)	(0.007)
Post x Rel. Amount Q4	-0.026***	-0.048***
	(0.004)	(0.008)
Treated x Post x Rel. Amount Q2	-0.009	0.014
	(0.006)	(0.009)
Treated x Post x Rel. Amount Q3	-0.009	0.023**
	(0.006)	(0.010)
Treated x Post x Rel. Amount Q4	0.004	0.142***
	(0.006)	(0.012)
Constant	3.068***	13.797***
	(0.004)	(0.007)
Firm-level FE	Yes	Yes
Country x sector x year FE	Yes	Yes
R2	0.957	0.951
Observations	714,190	716,078

*Notes: Estimation of the post-treatment effects for various relative loan amount baskets. The baskets are determined by the the Quartiles (Q) of the distribution of loan allocated amounts, standardized by firms' total assets at the treatment year. Base quartile is Q1 (0-0.016). The remaining baskets are 0.016-0.040 (Q2), 0.040-0.093 (Q3) and above 0.093 (Q4). Employment is measured as number of employees. Standard errors are clustered at the firm level and given in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table A6: Impact of the EIB lending by ToFA.

	(1) Employment (log)	(2) Fixed Assets (log)
Post	-0.054***	-0.132***
	(0.003)	(0.006)
Treated x Post	0.025***	0.086***
	(0.004)	(0.008)
Post x ToFa Q2	0.010**	0.033***
	(0.005)	(0.009)
Post x ToFa Q3	0.001	-0.010
	(0.004)	(0.008)
Post x ToFa Q4	-0.008	-0.029**
	(0.006)	(0.012)
Treated x Post x ToFa Q2	0.020***	0.023*
	(0.007)	(0.012)
Treated x Post x ToFa Q3	0.022***	0.040***
	(0.006)	(0.011)
Treated x Post x ToFa Q4	0.031***	0.068***
	(0.008)	(0.015)
Constant	2.748***	12.970***
	(0.039)	(0.067)
Firm-level FE	Yes	Yes
Country x sector x year FE	Yes	Yes
R2	0.954	0.944
Observations	682,031	683,422

*Notes: Estimation of the post-treatment effects for baskets of Transfer of Financial Advantage (ToFA). The baskets are determined by the the Quartiles (Q) of the ToFA distribution at the treatment year. Base quartile is Q1 (0-20 basis points). The other quartiles are 21-30 (Q2), 31-100 (Q3) and above 100 (Q4). Employment is measured as number of employees. Standard errors are clustered at the firm level and given in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

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