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Making a difference:
Assessing the impact of the EIB's funding to SMEs

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Abstract

We look at the impact of intermediated funding provided by the European Investment Bank (EIB) on the performance of small and medium-sized enterprises (SMEs) in the 28 member countries of the European Union between 2008 and 2014. We use a combination of propensity score matching and difference-in-differences to evaluate the impact of EIB lending on corporate performance using firm-level data. We find that EIB lending had a positive effect on employment, firm size, investment and innovation capacity, and it also increased firms' leverage. We also find that the positive impact of EIB funding is higher in the countries of Central and East Europe and also in South Europe, while somewhat smaller, yet still significant, in West and North Europe. All in all, our results indicate that EIB-supported funding made a significant and positive difference to the economic and financial performance of the beneficiary SMEs.

1 Introduction

Public financial institutions, both national and international, often provide support to small- and medium-sized enterprises (SMEs) in the form of lending (Alem and Madeira, 2015; Gutierrez et al., 2011). Such a support usually offers some benefits over purely market-based borrowing options. For instance, the advantages may include better credit availability, lower borrowing costs, longer maturities, or the lending may be accompanied by additional technical assistance.

The rationale for supporting lending to SMEs by public sector entities can be both structural and cyclical. SMEs play a key role in economic growth and in the provision of employment (Anginer et al., 2011; Griffith-Jones et al., 2017; de la Torre et al., 2017). In the EU28, SMEs

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account for almost all EU28 non-financial business sector enterprises (99.8 per cent), two-thirds of total EU28 employment (66.4 per cent) and slightly less than three-fifths (56.8 per cent) of the value added generated by the non-financial business sector (European Commission, 2018). Meanwhile, SMEs are more often subject to credit rationing than large companies due to asymmetric information and the lack of sufficient collateral to back up the loan book (Beck and Demirguc-Kunt, 2006). Such credit rationing may be present at any times, but it can be exacerbated by cyclical downturns. Bank capital is usually scarce during recessions, which has a negative impact on corporate lending activities (Gambacorta and Shin, 2016). Furthermore, large downturns are usually associated with increasing consolidation and concentration in banking markets, and stronger market power is associated with increased finance constraints for SMEs (Ryan et al., 2014).

Addressing the access to finance problem faced by SMEs is also an important policy objective of the European Investment Bank (EIB). Supporting SMEs and mid-caps is one of the four main priorities of the EIB Group.[1] It has also been the largest in terms of lending flows in the recent years. Only in 2018, 36 per cent of EIB's new lending was dedicated to SMEs and mid-caps (EIB, 2019).

In this paper we ask if publicly supported lending make a difference for the beneficiary firms. We put under the microscope the firms which received EIB-support through the intermediated funding, and we assess their corporate performance against otherwise identical firms which did not receive such benefits.

Our empirical strategy is as follows. We take the firm-level data that is reported back to the EIB by intermediary banks as our starting point. We merge it with publicly available data on individual SMEs' financial and economic performance, which are collected and standardized by Bureau van Dijk in the Orbis / Amadeus dataset. Data merging allows us to track financial performance of firms with (and without) EIB support, both in the years before and after the EIB loans were signed.

In the next step we apply the propensity score matching technique to find for each of the EIB beneficiaries a firm with similar observable characteristics but not being reported to have received the EIB support. In such a way we construct a treatment and a control group.

Then we run difference-in-differences (DID) regressions on the matched sample to test whether SMEs receiving EIB-supported loans provided via local intermediary banks perform differently with respect to outcome variables, such as employment, investment, firm growth, profitability, innovative capacity and leverage compared to other firms that did not receive EIB funding. In the estimation we control for a broad set of fixed effects, including firm-level fixed effects and country-sector-year interactions, addressing potential omitted variable problems.

[1]The other three priority areas include innovation and skills, climate and environment and strategic infrastructure.

This study is a continuation and an extension of the pilot exercise documented by Gereben et al. (2019), who carry out a similar analysis for a sample of firms in Central, East and South-East Europe (CESEE). More precisely, the contribution of this paper is threefold. Firstly, we extend the geographical scope of the pilot analysis to a full EU sample. Secondly, we address some of the initial methodological caveats in a rigorous and consistent manner. Thirdly, we shed new light on the EIB impact on innovation activities, by extending the set of key output variables to intellectual property indicators.

Results indicate that firms receiving EIB lending are characterized by higher post-treatment employment, investment (measured as change in fixed assets) and balance sheet growth than firms with otherwise similar observable characteristics. We do not find significant difference in profitability. However, EIB beneficiaries appear to be more likely to engage in innovative activities (measured by patent applications) than otherwise identical firms without the EIB support. We also find that EIB lending has a significant effect on leverage. We interpret the latter as an accounting effect, as firms receiving EIB funds by implication become more indebted than the control group.

Differences between beneficiaries and the control group show heterogeneity across geographical areas. We find that the impact of EIB lending on employment, investment, firm growth lending is higher for the firms in the Central and East Europe, and in the South Europe, compared to the West and North European Member States.

The paper is organised as follows. Section 2 discusses the theoretical consdierations behind a public intervention in the SME market. Section 3 outlines our analytcal strategy and discusses its main caveats. We describe our data in Section 4. Section 5 proposes our main empirical models. The results are put forward in Section 6 and finally Section 7 concludes.

2 Public sector intervention on the SME financing market

In many countries, public sector financial institutions have dedicated lending products for smaller firms. Alem and Madeira (2015) look at the scope of operations of 8 public development finance institutions from different countries, and find that all of them are engaged in operations with SMEs.[2] Gutierrez et al. (2011) find, on the basis of a much broader global survey of 373 public development banks from 92 countries, that the most common target for public development finance institutions around the world is the SME market: about 60 per cent of the studied institutions have targeted products to SMEs.

Justification for public sector involvement in the financial sector supporting SMEs is generally derived from market failures. Information asymmetries can lead to moral hazard and adverse selection of low quality borrowers. This can make private sector financial institutions unwilling to extend credit, especially uncollateralised credit, to SMEs and mid-caps even at high interest

[2] The institutions surveyed were CDB (China), KfW (Germany), BNDES (Brazil), CDP (Italy), CDC (France), JFC (Japan), ICO (Spain), and KDB (South Korea).

rates (Jaffee and Russell, 1976; Stiglitz and Weiss, 1981). The result is credit rationing, i.e. an equilibrium where banks decide to keep the supply of credit below demand, rather than to tap the extra loan demand at higher interest rates.

SMEs are more affected by credit rationing than larger companies because decision making processes, transparency rules and dividing lines between company and personal assets are less defined for SMEs than for larger companies. Thus, information asymmetries are more pronounced for small firms and the corresponding monitoring and screening costs are higher. As a consequence, SMEs with potentially viable investment projects are financially constrained, i.e. they often cannot obtain the necessary financing from financial intermediation on a pure market basis(Beck and Demirguc-Kunt, 2006). In other words, credit constraints may prevent SMEs from implementing investments with potentially high marginal returns, which would lead to better economic performance mainifesting in higher growth or job creation. As a result, the "SME financing gap" (OECD, 2006) is often considered as a general economic policy concern. It signals a loss of aggregate output, employment and productivity compared to a solution that would emerge without information asymmetries.

This SME financing gap is thought to widen in economic downturns and financial crises. in such periods private sector banks become more risk averse, given that crisis-related losses can make bank capital scarce. Empirical evidence shows that low and declining bank capital has a negative impact on corporate lending activities by banks (Gambacorta and Shin, 2016). Also, a number of studies have put forward the conclusion that credit constraint issues are exacerbated by increasing market concentration in banking sector. Ryan et al. (2014) show how bank market power is associated with an increase in financing constraints, and thus leads to lower SME investment levels.

The above considerations provide the basis for the theory of change in our impact assessment, which underlies the SME-related financing activities of public (national or international) financial institutions, including the EIB. In particular, we focus on intermediated lending, i.e. when public banks reach the beneficiary SMEs through intermediaries. In this case public institutions provide loans to commercial banks under favourable conditions. In exchange the banks take the obligation of on-lending the acquired funds to SMEs, and passing on, partially or fully, the financial advantage of the loan to the final beneficiaries.

In a stylised framework, public sector support through intermediated lending might facilitate access to credit, and thus impact the economic and financial performance of the final SME beneficiaries, through two distinct channels:

1. Firstly, the contracts under the public intervention mandate financial intermediaries receiving funding with public support to pass on some of the funding advantage they benefit from to borrowing SMEs. The advantages to the final beneficiary SMEs over a purely market-based loan can take various forms, i.e. lower financing cost, longer maturity or technical assistance, for instance. This extra advantage can contribute to better economic performance. We call this the *funding advantage channel*.

2. Secondly, public sector support might alleviate credit constraints on the intermediary banks' funding side. Especially in times of economic or financial downturns, when capital, liquidity or both are scarce, a public line of credit expands the funding base of the intermediary bank. By doing so, it makes possible for the intermediary to lend to firms that have viable investment projects, but that would otherwise have been rejected or only partially served due to lack of funding sources. We will refer to this as the *credit easing channel.*

It may well be that the two channels operate in tandem, or that one channel dominates or mitigates the other. While differentiation between the two channels offers an interesting avenue to further research, in this study we look only at the joint effects of the two channels. Our objective in this study is therefore to test if this skeletal theory of change works in practice, and if the beneficiaries of public support, in the form of an EIB-funded intermediated loans, are doing better than other firms.

3 Analytical framework

In empirical analyses of cause and effect it is critical to measure the impact relative to the appropriate counterfactual. In our case, what we would like to measure is the difference between the mean performance of the EIB-funded firms, and the mean performance of the same firms, had they not been beneficiaries of an EIB loan.

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 $Y_i^1 - Y_i^0$.

The fundamental difficulty in measuring the causal effect is that potential outcomes are unobservable. In other words, we do not know how an EIB beneficiary would have developed in terms of the outcome variables if it had not received an EIB loan. However, under suitable conditions we can link the observed outcomes to their potential values. Firstly, we require that the observed outcomes are realized as

$$Y_i = Y_i^1 T_i + Y_i^0 (1 - T_i). \tag{1}$$

Eq. (1) is called the stable unit treatment value assumption (SUTVA) and it implies that the potential outcome of one firm is not affected by the treatment assignment of other firms. Given the large multinational sample of the EIB beneficiaries, we would argue that the potential cross-border bias resulting from violation of SUTVA is rather limited. We cannot eliminate the possibility of the domestic cross-sector spillovers, however, which are a result of unobservable direct and indirect effects. We believe, however, that the due diligence process and the rule book of the EIB-support work in the advantage of the SUTVA principle.

Our main quantity of interest is the average treatment effect on the treated (ATET), defined as

$$\text{ATET} = \text{E}\left[Y^1 - Y^0 | T = 1\right], \tag{2}$$

where E denotes the expectations operator taken with respect to all firms. In other words, ATET measures the average difference in potential outcome variables for treated firms. As the potential outcomes are unobservable, the ATET is typically estimated in relation to the average treatment effect given by

$$\text{ATE} = \text{E}[Y|T = 1] - \text{E}[Y|T = 0], \tag{3}$$

which is based on observed outcomes. However, the setup creates an identification challenge, as due to a bias component the ATET and ATE do not always match. More specifically, one can write that

$$\text{ATE} = \text{ATET} + \text{E}\left[Y^0|T = 1\right] - \text{E}\left[Y^0|T = 0\right]. \tag{4}$$

The bias term reflects the problem that selection into treatment may depend on potential outcomes. 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, for instance. 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 $\text{E}[Y^0|T = 1] > \text{E}[Y^0|T = 0]$.

Having pointed this out, the second assumption in this study 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. In other words, correctly designed randomised trials impose that the potential outcome variables are independent of the treatment assignment, such that $(Y^1, Y^0) \perp\!\!\!\perp T$. As bank loans are not allocated in the form of randomised trials, we control the selection bias by selection on observables.

Let us denote by X_i a set of observable characteristics of firm i, which are predetermined with respect to the treatment T such that $X_i^1 = X_i^0$ for each i. Under the condition that $(Y^1, Y^0) \perp\!\!\!\perp T|X$ it holds that

$$\text{E}\left[Y^1 - Y^0|X\right] = \text{E}[Y|X, T = 1] - \text{E}[Y|X, T = 0]. \tag{5}$$

Furthermore, under the requirement of common support, i.e. $0 < \mathrm{P}(T = 1|X) < 1$ with probability one,[3] it follows that

$$\mathrm{ATET} = \int \left(\mathrm{E}[Y|X, T = 1] - \mathrm{E}[Y|X, T = 0]\right) \mathrm{dP}(X|T = 1), \tag{6}$$

where P stands for probability distribution. Eqs (5) and (6) imply that the ATET can be estimated by comparing the sample of treated firms to the sub-sample of non-treated ones with the same characteristics X, which can be achieved by matching techniques. Rosenbaum and Rubin (1983) extend the result from Eq. (5) and show that under selection on observables assumption, it holds that

$$\mathrm{E}\left[Y^1 - Y^0|p(X)\right] = \mathrm{E}\left[Y|p(X), T = 1\right] - \mathrm{E}\left[Y|p(X), T = 0\right], \tag{7}$$

where $p(X) = \mathrm{P}(T = 1|X)$ is the propensity score. As a consequence, ATET can be estimated by matching on the fitted propensity scores $\hat{p}(X)$, which in fact improves the performance of the procedure for larger sets of characteristics.

Propensity score matching (PSM) creates a control group among non-treated firms which at the time of the treatment are identical to treated firms with respect to observable characteristics.[4] Thus for a given set of observable characteristics, receiving an EIB-backed loan should be "as good as random".

PSM is only able to account for observable characteristics when addressing the selection bias of the treatment group. However, treated and non-treated firms might differ with regard to unobservable confounders which (i) are not perfectly correlated with observables, (ii) are correlated with observables which are unbalanced between the treated and non-treated firms, and (iii) are important for testing the proposed theory of change. To address these issues, firstly we verify the matching validity by checking the corresponding balancing properties, and secondly we exploit the time dimension of our dataset to control for certain unobserved factors.

More specifically, let us standardize the treatment year for each EIB beneficiary to $t = 0$.[5] 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. $X_i \equiv X_i(0)$. It follows that the post-treatment ATET becomes

$$\mathrm{ATET}(1) = \int \mathrm{E}\left[Y^1(1) - Y^0(1)|p\left((X(0))\right), T = 1\right] \mathrm{dP}(X|T = 1). \tag{8}$$

[3]The common support requirement, or the overlap assumption, requires that for each realization of X_i there is non-zero probability of being treated and non-treated.

[4]Starting with the seminal work of Rubin (1974) and Rosenbaum and Rubin (1983), the PSM methodology has become an industry benchmark in applied impact evaluation. An extensive introduction to the topic is provided by Caliendo and Kopeinig (2005).

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

The core identifying assumption to estimate ATET(1) is that the treated and non-treated firms exhibit the same trend in the absence of the treatment, such that

$$\mathrm{E}\left[Y^0(1) - Y^0(0)|p\left((X(0)\right), T = 1\right] = \mathrm{E}\left[Y^0(1) - Y^0(0)|p\left((X(0)\right), T = 0\right]. \tag{9}$$

Under the common trend assumption, one may derive ATET(1) as

$$\begin{aligned} \mathrm{ATET}(1) = &\left[\mathrm{E}[Y(1)|p\left((X(0)\right), T = 1] - \mathrm{E}[Y(1)|p\left((X(0)\right), T = 0]\right] \\ &- \left[\mathrm{E}[Y(0)|p\left((X(0)\right), T = 1] - \mathrm{E}[Y(0)|p\left((X(0)\right), T = 0]\right]. \end{aligned} \tag{10}$$

In fact, Eq. (10) can be estimated in a two-step approach. In the first step, we construct the matched (treated and non-treated) sample by PSM, as described above. In the second step, conditional on the validity of the propensity scores, we estimate Eq. (10) 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.

However, in case there are potential unobserved time-varying confounders, the ATET estimates from the DID approach may, in fact, be biased. One of such confounders may be the firm-specific credit demand. This issue is generally present across the impact assessment literature on publicly supported lending. Treated firms obviously exhibit credit demand at the time of the treatment. Among the firms in the control group, however, some firms may not have demand for credit at that time. For instance, some non-treated firms might lack a profitable investment opportunity. As firm-level credit demand is generally unobservable, our identification strategy may not fully account for this type of unobserved heterogeneity, and consequently the ATET may be overestimated. Brown and Earle (2017) discuss this identification issue in detail, and provide a possible way to overcome it using geographical variation in treatment availability as instruments.

In general, however, we believe that the observables $X(0)$ we use in the PSM show a strong correlation with credit demand, suggesting that the DID analysis provides us with a proper assessment of the impact of EIB funding. In addition, we utilize a wide set of fixed effects, including not only firm-level factors, but also the interaction of country, sector and year levels. The latter, in fact, absorbs any shock to demand or to technology, which happens in a particular sector, in a particular country during a particular year. Furthermore, we cover this topic as one of the robustness checks, and provide evidence that our results hold even if we add a proxy for credit demand in our PSM specification.

Similar empirical frameworks using PSM, DID or a combination of the two have been used before in the literature to assess the impact of financial support to SMEs by public institutions. Bah et al. (2011) find that USAID's technical and financial assistance for Macedonian SMEs raised employment growth rates by 16-20 percentage points. Cassano et al. (2013) analyse the impact of European Bank for Reconstruction and Development (EBRD) programs for Micro, Small and Medium Sized Enterprises (MSMEs) in selected CEE countries (Bulgaria, Georgia, Russia and Ukraine) by applying standard regression estimations after a PSM approach. They find a significant positive effect of cash flow-based and collateral based loans on most performance indicators (i.e. fixed assets, revenues and employment). 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 DID framework they find that the program succeeded in generating extra investment in the SME sector that would not have taken place otherwise. Brown and Earle (2017) analyse the impact of loans provided by the US Small Business Administration (SBA) on employment. Their results indicate an increase of 3-3.5 jobs for each million dollars of loans, and the impact is stronger for younger and larger firms. Banai et al. (2017) investigate the impact of EU-funded direct subsidies to SMEs in Hungary using PSM and fixed effects panel regression, and find a significant positive impact on the number of employees, sales revenue and gross value added.

Studies in a similar vein have also been conducted within the EIB Group to assess the impact of financial operations targeting SMEs. Asdrubali and Signore (2015) show that SMEs in the Central and South Eastern Europe (CESEE) region, which received funding guaranteed by the EU SME Guarantee Facility managed by the European investment Fund (EIF), recorded an increase in the number of employees and in sales compared to a respective control group of SMEs. The results were estimated on a sample of firms receiviving EIF-supported loans between 2005 and 2007. Bertoni et al. (2018) show that, on average, French SMEs benefitting from guaranteed loans created more jobs and grew more in terms of assets and sales . Bertoni et al. (2019) repeat the exercise on guaranteed loans granted under the EU programmes MAP and CIP[6] on SMEs' growth in Italy, the Benelux and the Nordic countries from 2002 to 2016. Guaranteed loans are found to positively affect the growth in assets, sales, employment and the share of intangible assets.

Our study is a direct follow-up and extension of Gereben et al. (2019), who look at the impact of EIB funding on the performance of 5,074 SMEs in eight countries of CESEE region during 2008-2014. They find that EIB lending has a positive effect on employment, revenues and profitability.

We contribute to the literature by assessing the impact of EIB funding to SMEs on a large, multi-country sample. Our sample is larger than the one used in previous studies both in the number of individual beneficiaries or volumes, and also in terms of geographical coverage, as our data cover 27 out of 28 EU countries (no loan allocations happened in Malta in the period we analyse). Furthermore, we provide further insights into the impact on innovation by exploiting the indicators from the patent database, and we verify the robustness of our results against several new dimensions.

[6]By abbreviations we refer to the Multi-Annual Programme for enterprises and entrepreneurship for SMEs (MAP) and Competitiveness and Innovation Framework Programme (CIP).

4 Data

EIB funding products targeting SMEs typically take the form of a Multiple Beneficiary Intermediated Loan (MBIL). To allocate funding to SMEs, the EIB leverages on the local expertise of financial institutions across the EU, using an intermediated lending model involving private financial service providers.[7] The EIB provides funding to these intermediaries directly (or indirectly, via public promotional institutions) at conditions that are somewhat better than those available on the market at a given moment. In exchange, they commit to use the funds to extend loans to SMEs, and to (partially) transfer the financial benefit to the final beneficiaries in the form of an interest rate reduction and/or the provision of longer tenors. This is a scalable mechanism that makes EIB financing available to SMEs and mid-caps in a swift and efficient manner.

The intermediated lending agreement requires the banks and financial institutions to report on the final beneficiary SMEs and mid-caps. EIB records and monitors individual allocations reported against the contractual commitments agreed with financial intermediaries. The records include basic terms sheets of the loan and while they also cover some basic information about the firms, like the sectoral classification or address, they do not contain a detailed history of their financial or economic performance. To assess the impact of the EIB loans on the beneficiaries, we first need to study their financial characteristics in the years before and after they received the loan. Therefore we merge the allocation data with financial statements and patent data from Bureau van Dijk's Orbis/Amadeus dataset. We also use Orbis/Amadeus to generate a large pool of potential counterfactual companies, which we use as the final control group.

In the remainder of this section we first describe the EIB allocation dataset and its key summary statistics. We then explain the data merging process, and the loss in data due to imperfect merging and low coverage of Orbis in certain countries and years. We describe in detail how the resulting data attrition affect our analysis. Finally, we present our strategy to generate a sample of potential counterfactual companies, which serves as an input to the PSM model.

4.1 EIB allocation data

The EIB allocation data are gathered annually in a standardized format, starting in year 2008. The data are delivered by financial intermediaries which have a direct relation to the SME. They contain information on the size of the company and the main sector of operations (using the NACE Rev. 2 four-digits classification), as well as loan-specific information, such as the date of loan disbursement, loan volume and maturity. For the purpose of this analysis, we consider allocations up until end-2014 only.[8] The aggregate statistics of the allocation dataset,

[7] Potential financial intermediaries typically include commercial banks and leasing companies, and in some cases public entities such as national promotional banks.

[8] Allocation data are readily available until 2018. However, in line with the literature, for practical purposes we consider only those allocations that can be monitored for a sufficiently long period after the loan has been disbursed. This allows us to take possible lags in the impact of the loan disbursement into account. To guarantee at least 3 years of follow-up, we cut the allocation sample in 2014. This allows us tracking the financial

including the number of allocations, total amount allocated and average allocation sizes, by country, year and employment size, are presented in Table 1.

[Table 1 about here.]

The dataset includes 520,746 individual allocations to 403,788 different firms.[9] Total EIB lending to European SMEs and mid-caps through these allocations amounted to EUR 72,4bn over the seven years between 2008 and 2014. The data show significant heterogeneity across countries. The largest recipients were Spain, Poland and Italy. In addition, the average allocated amounts show large variations from country to country. Looking at the data by year, we see a gradual increase in the number of allocations and the amounts over time. The largest share of loan recipients are companies with 2 to 10 employees. However, measured in EUR, the largest share of the total funding (29 per cent) was allocated to firms with 51 to 250 employees. Overall, the data structure is visibly skewed towards the SMEs, which is also the reason why we rather consider the following analysis as representative of lending to SMEs, rather than to mid-caps.

4.2 Merging with Orbis and the resulting data attrition

We use the Orbis dataset to obtain information on the financial and economic performance of EIB loan beneficiaries. The database (compiled by the Bureau van Dijk, BvD) is a commercial dataset and it contains firm-level financial statements and ownership data, gathered and standardized to the so-called 'global format', being comparable across jurisdictions. The financial and balance-sheet information in Orbis comes from business registers collected by the local chambers of commerce to fulfill legal and administrative requirements. Our database is updated semi-annually in vintages, where each vintage is cleaned up from companies which have not reported any information for 10 years or more. Therefore, to correct for the survivorship bias, we aggregate the data for all the vintages to obtain a sample covering years until 2017, which is also the last available year at the time we updated the files. In June 2019, the database contains administrative data on 310 million firms worldwide with about 82.5 million firms in EU28. The database is widely used in economic research as a source of firm-level data for microeconometric analysis.

We work with firms' unconsolidated accounts with all monetary values expressed in euro. We clean up the data by excluding observations with odd or inconsistent values in the spirit of Kalemli-Ozcan et al. (2015). In particular, we drop firm-year 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. On top of that we check for the reporting consistency and drop the firm-year financial statements which violate

performance of the beneficiaries up to 3 years after, as 2017 is the last available year for the financial data in Orbis.

[9] It is possible that a company received multiple EIB-supported loans in the same year, or across the years. For the purpose of this analysis, we treat only the first occasion as treatment. We treat multiple allocations the following way. For the same year, we choose the loan with the largest volume. If there are still multiple loans with the same volume in the same year we pick the one with the earliest date. Otherwise, if a company received two or more identical loans at the same date, we consider such reporting as a typo and do not include such records in the analysis. For firms which received multiple loans across years we take the earliest loan.

the basic balance-sheet equivalences by more than 10%. Specifically, 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. We also deflate variables using the country-specific Harmonised Index of Consumer Prices (HICP) deflators. Finally, we winsorize the series by years at 1% levels.

Out of our 403,788 unique beneficiaries, we find corresponding entries in Orbis in 152,381 cases. Short of unique numerical company identifiers, we use company details as matching variables,[10] and use string-based matching algorithms to pair EIB beneficiaries with corresponding company records in Orbis. Given the presence of typos, different spelling conventions and often non-consistent use of accents and special characters in the two datasets, we could not rely only on perfect matches. Our main tool is BvD's own string matching algorithm, which gives a score based on matching probabilities. Matches with very high probabilities are accepted automatically, while pairings with less reliable scores were double-checked manually. When evaluating potential matches, we remain on the conservative side, as we want to absolutely avoid erroneous pairs entering into the dataset, even at the expense of potentially excluding some 'true' pairings. For instance, if there are multiple potential pairing candidates for one firm with no clear-cut indication of which is the correct one, we consider such a firm as unpaired.

Once we identify the firms in Orbis, we check if the data coverage is sufficient. Many firms have incomplete corporate records in Orbis. For our exercise we needed at least a basic set of balance sheet and income statement data, together with the number of employees, for 3 years before and after the allocations.[11] We also download the number of patent applications and patent registrations for each firm, whenever available. As the BvD's patent database uses nearly all available patent sources, if data on patents are missing, we assume that the given firm did not file any patents in the given year.

Table 2 shows the success of the matching and the data extraction from Orbis by country, year and employment class, and it illustrates the resulting loss of observations. We successfully paired 44.6 per cent of the EIB allocations with a record in Orbis. However, only 13.25 per cent of the original allocations had sufficient data coverage in Orbis to be included in the propensity score matching. This does seem to fall within the attrition range reported in the other studies. For instance, Gereben et al. (2019) work only with 4.8% of the original number of treated firms, whereas Asdrubali and Signore (2015) with 18%.

[Table 2 about here.]

When grouping the data by observable categorical variables, it is visible that the share of missing data are not balanced across these categories.[12] Data attrition is particularly unevenly

[10]In particular, the matching is carried out on company name, physical address and reported primary sector of activity.

[11]See section 5.1 for the list of variables used in the PSM. The econometric framework allows for some data gaps, therefore not all control firms need to have the full 7-year dataset.

[12]The allocation dataset allows us to create categories by country, allocation year, employment class and industry classification (according to NACE Rev. 2).

distributed across the countries. Lack of Orbis coverage in some key variables results in certain countries dropping out of the sample altogether, like in the case of Cyprus, Denmark, Estonia, Ireland, Lithuania or the UK. Most of the the largest beneficiary countries have good Orbis coverage, however. The only exception is Poland, where only less then 1 per cent of the allocations are successfully matched and populated with data. Table 2 also indicates that data availability varies somewhat less by allocation year. This is also generally true for firm size, although individual entrepreneurs are less likely to have a complete Orbis record than larger firms.

Having pointed this out, we cannot assume that the data are missing completely at random (MCAR). As a consequence of the uneven nature of data attrition, 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).

While we cannot fully eliminate the effect of the resulting bias, we are partially correcting for it as part of our robustness checks. We use inverse probability weights (IPW) for strata based on country, allocation year, industry class and employment class. The strata weights are proportional to the share of lost data in a given stratum. The aim of the procedure is to bring the dataset closer to the original statistical properties of the population with respect to a range of observed variables. The IPW-weighted results show close similarity to our baseline results, suggesting that they can be generalised beyond the actual sample.

4.3 Potential controls

In the next step, we construct a pool of potential counterfactual firms. In principle, all EU SMEs and mid-caps that have been active between 2008 and 2014 could have been eligible for an EIB-supported loan. However, for practical considerations, we populate our potential control pool with a unified number of firms per stratum, which show broad similarity to the treated firms, and thus have a high chance to serve as a reasonable comparison pair. Such a data standardization improves the asymptotic properties of the PSM setting, and therefore of our impact estimates, as the proportions between the treated and non-treated firms in each stratum become more homogenous.

For this purpose, we use stratified sampling approach. Strata are defined along 4 dimensions: country (28 categories), allocation year (7 categories for years 2008-2014), size groups by number of employees (4 categories for 0-9, 10-49, 50-250 and 250+ employees) and industry groups by NACE codes (6 categories for Agriculture (section A), General Industry (B, C, D, E), Construction and Real Estate (F, L), Trade (G), Transportation and Accommodation (H and I), and Other (other sections). These strata dimensions generate 4704 actual clusters, from which 3055 actually contain at least one firm from the treated sample. We then randomly sample firms for each cluster from the Orbis. For each cluster we attempt to sample 15 times the number of firms in the treated group. We sample only from those firms that had 3 years of financial and employment data available in Orbis both before and after the (presumed) treatment. As Orbis is not uniformly well-populated in some countries and company categories, we have not always found 15 suitable firms for each cluster. Finally, our pool of potential controls consists of 820,162 individual firms with a complete data record. The summary statistics for the potential control firms are given in Table 4, however, for brevity reasons, we discuss them together with the PSM results in Section 5.

5 Empirical strategy

This section aims at outlining the details behind our empirical approach. As emphasized in Section 3, we follow a two-step approach. Firstly, we describe the method to construct the counterfactual control sample by the PSM method. We then turn to the DID methodology to control for the potential confounding variables.

5.1 Propensity score matching

The goal of the PSM is to pair beneficiary firms (treated group) with otherwise identical firms that were not receiving EIB-supported loans (control group). As a first step, we estimate a probit regression, where we explain the probability of being selected into treatment with a set of variables that are likely to influence both the selection, and the outcome variables that we are interested in. As a vector of covariates we take a set of financial characteristics observed before three years before the treatment year, which is standardized at $t = 0$, such that $X_i(0) \equiv \{X_{it-1}, X_{it-2}, X_{it-3}\}$. As a result, the probit model takes the form

$$\Pr(T_{it} = 1 | X_i(0)) = \Phi(\beta_0 + \beta_1 X_{it-1} + \beta_2 X_{it-2} + \beta_3 X_{it-3} + \mu), \tag{11}$$

where Φ is the cumulative normal distribution, variable T is a dummy determining if a firm i was treated or not in year t, matrices X contain a set of firm-specific controls which and matrix μ contains a set of fixed effects including additively age class and employment, industry, country and year strata.[13]

Our aim is to include all important variables that affect both the selection into the treatment and the outcome of the treatment in our model.[14] We begin with a set of indicators of corporate performance describing size, sales, profitability, leverage, liquidity, asset tangibility, innovativeness, which we deem as possible inputs into the loan assignment decision. They include leverage ratio (defined as a share of current and non-current liabilities as a share of total assets), employment (in logs), total assets (in logs), cash ratio (cash and cash equivalents as a share of total assets), current ratio (current assets as a share of current liabilities), turnover ratio (operating revenues as a share of total assets) and sales growth. Regarding the measures of innovativeness, we consider two generic dummy indicators, i.e. if a company filled at least one patent application or it published at least one patent in a given year.

[13] The stratification groups are taken as described in Section 4.3. For the age classification we use 5 groups: [0,2), [2,5), [5,10), [10,20) and 20 and more years after the date of incorporation of a firm.

[14] See Caliendo and Kopeinig (2005) for a detailed discussion of variable selection for PSM.

In the probit model we include multiple lags of variables that later serve as outcome variables in our DID specification. By this, we are matching pre-treatment trends in outcomes, and thus enforce the parallel trend assumption of the DID. Some recent research by Daw and Hatfield (2018), O'Neill et al. (2016) and Chabé-Ferret (2017) highlight that combining DID with matching with past outcomes can introduce bias by the possibility of matching on noise, which may lead to mean reversion. However, Chabé-Ferret (2017) and Ryan et al. (2018) also point out that the risk of such a bias is significantly reduced when the matching is performed using at least three pre-intervention periods on past observations of the outcome variables, as in our case.

While developing the PSM model, we try to keep significant regressors together with their corresponding higher order (squared and cubic) terms if they improve the goodness of fit of the model. As the data availability, especially regarding the income statements, is missing for many of the treated firms, in the process we try to keep the balance between the number of regressors and the final number of matched firms. Overall, however, the final ATET(1) results turn out to be robust to various PSM specifications. Table 3 shows the results for our final specification, which we consider as an optimal compromise between inputs and outputs. We follow the approach of Asdrubali and Signore (2015) and consider the probit model as instrumental to the estimation of ATET(1), and instead of focusing on specific model coefficients, we rather verify the properties of the fitted propensity scores.

[Table 3 about here.]

The matching itself is done by pairing each treated firm with a potential control firm that has the closest fitted propensity score. In our baseline specification we use matching with replacement, i.e. a control firm could be matched potentially with several treated firms, if that particular control firm was the closest neighbour of several treated firms. The matching results in 49,703 firms in the control group, matched with 53,491 firms in the treated group. About 96.7 per cent of the control firms are matched with only one treated, and 3 per cent are matched against two treated. Our resulting control sample is therefore only slightly different from the one that we would have obtained with one-to-one matching (without replacement). The remaining 0.3 per cent had higher number of matches, five matches being the maximum in the case of 3 control firms.

Figure 1 illustrates the success of the matching procedure. The panel on the left represents the density curves of the estimated propensity scores on the sample of treated firms (blue line), and the complete pool of potential control firms (red line). The model has discriminatory power in a stochastic sense between the two groups, as the distribution of the potential controls are evidently more skewed towards zero. The right panel plots the distribution of the estimated propensity score of the treated and the matched control group. The two lines overlap almost perfectly, indicating that the distributions of the propensity scores are quasi-identical for the two groups.

[Figure 1 about here.]

Beyond the close similarity of the propensity scores, we also verify the impact of the matching on the key variables of interest, related to treatment assignment and outcome. Table 4 summarizes the aggregate characteristics of the dataset for the pre-treatment period. In particular, we look at the original pool of potential controls, matched controls and the treated firms. The improvement in the aggregate statistics is striking. The PSM-matched controls show a very close similarity to the treated firms with respect to all variables of interest. This is not restricted only to the mean aggregates, but also to higher moments of the data distribution.

[Table 4 about here.]

As the last step, we look at the balancing properties of the PSM in more detail. Specifically, for each variable we calculate the standardised percentage bias, before and after the matching, defined as a percentage difference of the sample means in the treated and control groups (either potential or matched) as a percentage of the square root of the average of the corresponding sample variances (Rosenbaum and Rubin, 1985). Table 5 shows the balancing properties before and after the matching with respect to the variables used in the probit model. We can see that the matching resulted in a very significant reduction of the bias. With the exception of the cash ratio and the current ratio, the remaining differences between the two samples are statistically not significant.

[Table 5 about here.]

Overall, the result of the matching exercise gives us sufficient comfort to claim that the control group shows sufficient similarity to the treated group to serve as a fair basis of comparison.

5.2 Difference-in-differences

Given the PSM-matched sample of treated and control firms, according to Eq. (10), ATET(1) can be estimated by measuring the difference in the performance of the observed characteristics Y_i over time. The underlying assumption of the DID framework requires, however, that both treated and control firms would share the same trend in the absence of the treatment. In the first step, therefore, we verify if there is enough evidence in our data to support this claim.

As it is impossible to observe enough data points before and after treatment (as $Y_i^0|T_i = 1$ are unobservable for $t >= 0$), we carry out the experiment on the pre-treatment period only.[15] Specifically, we estimate the following OLS model on the $t < 0$ sample for each outcome variable Y

$$Y_{it} = \alpha_0 + \alpha_1 t + \alpha_2 (t \times T_i) + \xi_i + \varepsilon_{it}, \tag{12}$$

where ξ_i is a vector of firm-specific fixed effects.

[15] Even though the conclusions from the exercise cannot be directly extended to the post-treatment phase, we view this procedure as sufficient for a wide range of possible scenarios. In fact, without a structural break in trend in $t = 0$, one can expect that pre-treatment trend can be extrapolated onto the post-treatment period.

The results are depicted in Table 6. It can be readily observed that the coefficients capturing the interaction between time trend and treatment group are not significant for any of the proposed outcome variables. The evidence suggests therefore that the trends between treated and matched controls are parallel in the pre-treatment phase.

[Table 6 about here.]

We calculate ATET(1) in a linear regression framework. Under the assumption 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 Eq. (10) matches the estimate of β_2 from the following panel regression

$$Y_{it} = \beta_1 I_{t>0} + \beta_2(T_i \times I_{t>0}) + \nu_{cst} + \xi_i + \varepsilon_{it}, \tag{13}$$

where ν_{cst} is a vector of country-sector-year 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.

We further assess the magnitude of the treatment impact for individual post-treatment years by estimating the extended DID specification

$$\begin{aligned} Y_{it} = {} & \gamma_1 I_{t=1} + \gamma_2 I_{t=2} + \gamma_3 I_{t=3} \\ & + \gamma_4(T_i \times I_{t=1}) + \gamma_5(T_i \times I_{t=2}) + \gamma_6(T_i \times I_{t=3}) + \nu_{cst} + \xi_i + \varepsilon_{it}, \end{aligned} \tag{14}$$

where γ_4, γ_5 and γ_6 correspond to ATET for years $t = 1$, $t = 2$ and $t = 3$, respectively.

To ensure the robustness of the results, we expand the DID model into several directions, addressing the commonly known shortcomings of the methodology. Firstly, we consider a fully balanced panel of the treated and control firms, addressing possible selection bias inconsistencies. Secondly, we apply the standard error correction proposed by Bertrand et al. (2003), to alleviate the potential bias introduced by serial correlation. Thirdly, we carry out a simple placebo test by shifting the treatment period to year $t-2$ and estimating the differences between $t = -3$ and $t = -1$. The rationale aims at checking if we would find an impact at a time when no treatment was applied. Fourthly, we address some of the concerns brought up by the data attrition and possible inconsistencies between the initial set of EIB loans and the ones that actually enter the regressions. We do that by adding weights into the DID model, trying to re-establish the initial composition of allocations by stratas. Lastly, we address the concerns that the treatment effects may be blurred by the project availability at the firm level. We expand the PSM specification by controling for indebntedness in $t = 0$, following the intuition that firms elevate their leverage ratios having in mind the relevant investment project to be financed.

6 Results

We split the results into two categories of outcome variables. Firstly, we look at the basic performance metrics such as number of employees, total assets and propensity to fill patent applications, which broadly correspond to commonly reported impact indicators (Gertler et al., 2011). As a second step, we expand the set of outcome variables to additional metrics including return on equity, fixed assets and leverage ratio.

In brief, our results indicate a significant and positive effect of EIB funding on employment, firm size, investment levels, and propensity to innovate. We have not found any significant impact on profitability. We also detect a deterioration in leverage as a result of borrowing, consistent with booking procedures of extra loans. The mean difference between treated and control firms is illustrated by Figure 2, which plots the pre- and post-allocation dynamics for the treated and control firms with respect to our main variables of interest. Tables 7, 8 and 9 further present the estimation results of models given in Eqs. 13 and 14.

[Figure 2 about here.]

[Table 7 about here.]

[Table 8 about here.]

[Table 9 about here.]

The analysis suggests a significant positive effect on employment, expressed in the number of employees. Beneficiary firms increase their staff numbers on average by 5.5 per cent relative to the controls (Column 1 in Table 7). Looking at the year-by-year impact, the increase in employment occurres gradually over the post-treatment years, as indicated by the estimated interaction coefficients in Column 1 of Table 9. The positive pre-treatment trend suggests that firms in our sample grow in employment in the pre-treatment period already. Upon receiving a loan, the positive effect on employment assures the possibility of adding new employees to the firm, and thereby continuing their growth. However, had these firms not received EIB funding, they would need to resort to stop hiring new staff, or even scale down their operation and decrease their employment levels.

When it comes to firm size, expressed in total assets, we see a similar positive effect. Total assets are 7 per cent higher in the treatment group after the allocation, compared to the corresponding value for the firms in the control group (Column 2 in Table 7). The difference in total assets also grows over the years (Column 2 in Table 9).

We also find that beneficiary firms have somewhat higher propensity to submit patent applications, implying a potentially higher level of innovative activity (Column 3 in Table 7). However, these results need to be treated with a caveat, as the overall proportion of companies filing patents in the sample is very low. Therefore, we consider this finding as an interesting avenue to explore more in depth in the future.

The data do not suggest any persistent and significant impact on firm profitability measured by returns on equity. While we experimented with various measures of firm-level returns, including return on assets and various income ratios, it seems that EIB-supported lending does not increase the returns on firms' activities.

On the other hand, fixed assets show a significant increase relative to the control group. This is a variable that we consider as a good proxy for investment at the firm level. Column 2 of Table 8 indicate that the level of fixed assets are close to 15 per cent higher for EIB beneficiaries. It indicates that the increase in level of firm activity happens along with a proportionally higher accumulation of productive assets.

The negative effect of EIB funding on the leverage of treated firms, seen in Column 3 of Table 8 and Column 6 in Table 9, is largely driven by accounting mechanics. Extra financing in the form of a EIB-supported loan increases the level of debt relative to equity financing which has a negative impact on the leverage ratio, compared to firms that do not receive such funding. The fact that we can directly observe the impact of the treatment in the accounting data supports the validity of our empirical framework.

6.1 Breaking down the impact results by country groups

While the above analysis informs on the overall magnitude of the effects in the sample countries, it is possible that the effects vary by different data cuts. Not only the geographical coverage of the EIB loans is scattered (see Table 1), but also the market environment across the EU is vastly heterogeneous. As the advantages of the EIB support are always measured against the relevant market metrics, we break down the impact results between three broader groups of countries: Central and East (CE) Europe, South (S) Europe and West and North (WN) Europe.[16]

We extend the model given in Eq. 13 to a triple DID framework, including the interactions with specific country groups. As a result the specification becomes

$$\begin{aligned} Y_{it} = {} & \delta_1(T_i \times I_{t>0}) + \delta_2(I_{c\in CE} \times I_{t>0}) + \delta_3(I_{c\in S} \times I_{t>0}) \\ & + \delta_4(T_i \times I_{c\in CE} \times I_{t>0}) + \delta_5(T_i \times I_{c\in S} \times I_{t>0}) + \nu_{cst} + \xi_i + \varepsilon_{it}, \end{aligned} \tag{15}$$

where as the base group we take the WN countries, such that $I_{c\in CE}$ is an indicator function taking value 1 if the country belongs to the CE group, and 0 otherwise. By analogy, $I_{c\in S}$ represents the Sout Europe. The regional breakdown of the results is presented in Figure 3, which is further supported by regression results in Table 10.

[Figure 3 about here.]

[Table 10 about here.]

[16] Central and East Europe countries include Bulgaria, Croatia, Czech Republic, Hungary, Latvia, Poland, Romania, Slovakia and Slovenia; South Europe covers Greece, Italy, Portugal and Spain; and West and North Europe spans over Austria, Belgium, Finland, France, Germany, Luxembourg, Netherlands, Sweden and the United Kingdom.

It can be readily observed, that whereas the employment and asset gains from the EIB-support are positive and significant in all three groups of countries, there are interesting differences between them. The results suggest that the impact in CE countries is higher by 6 per cent and 10 per cent, respectively, than in the WN Member States. Similarly, the effects for the South Europe are higher by 1.7 per cent, and 1.5 per cent, respectively, vis-a-vis the WN Europe. The patent regression does not indicate any geographical differences.[17]

6.2 Robustness

To confirm the validity of the results against a range of different modelling specifications, we turn to several robustness checks. Altogether, these tests are supportive of our earlier findings and they confirm the stability of our results with respect to various modelling assumptions.

Balanced panel of firms. The properties of the fixed-effects estimators may be affected by the data composition. For instance, if the missing observations in an unbalanced panel are a result of a non-random effect, the data structure may contain a selection bias. Alternatively, if the sample excludes the firms which do not report for all the years, the data may include a survivorship bias. To better benchmark the results, we complement the results presented in Table 7 by the results obtained from the same model but on a balanced sample of firms in Table 11.

[Table 11 about here.]

It can be readily observed that the main results fully hold. The magnitude of the coefficients varies marginally and statistical significance is fully preserved.

Cluster-robust estimation. Bertrand et al. (2003) point out that the firm-level DID estimator uses observations of the same entity over multiple time periods. Traditional DID estimators do not necessarily account for the resulting serial correlation of the error term, and as a consequence, regression standard errors may be underestimated. To overcome this difficulty, instead of using multiple yearly observations for both the pre- and the post-treatment periods for the same firm, Bertrand et al. (2003) propose to use the average of the outcome variables before and after the treatment and run the DID regressions on these averaged outcome variables. We follow this advice and the results with the serial-correlation-robust standard errors are given in Table 12.

[Table 12 about here.]

It appears that the correction for the serial correlation of the errors does not affect the statistical significance of our results.

[17]This may be again a result of patent data structure.

Placebo treatment. One of possible verifications of the correct specification of the treatment effects models is a so-called placebo test. The rationale assumes that a correctly designed statistical procedure should not pick up any evidence in favour of the treatment if the treatment is not applied. Otherwise, there is a risk that the observed impact results could be an artifact of a statistical model rather than data-driven.

We design a simple placebo scenario, where we assume that the treatment happens in period $t = -2$. To exclude the actual treatment period, we run the core model on the pre-treatment sample only ($t < 0$), effectively comparing the levels of the outcome variables in periods $t = -3$ and $t = -1$. The results of this experiment are given in Table 13.

[Table 13 about here.]

The findings suggest that the model design passes the placebo test. With the exception of of the ROE, which picks up treatment effects at a time of no treatment at the 10% significance level only, for none of the outcomes we observe statistically significant treatment effects in period $t = -2$.

Data attrition. We show in Section 4.2 that a significant proportion of our initial observations drops out while we merge various datasets. The reasons include unsuccessful matching of beneficiary company names with Orbis records, and missing data in Orbis for already matched companies. To correct for the data attrition bias, we use inverse probability weights (IPWs). IPW is a technique widely used to correct for non-response in surveys, which re-establishes the statistical properties of the original population with respect to some observed variables.

To generate the weights, we stratify our allocation dataset along the same dimensions as used for the construction of the pool of potential controls for the PSM model (see Section 4.3). They include country, allocation year, number of employees and industry classification. We then calculate the number of firms in each cluster before and after data attrition, and measure the shares of firms that survive the procedure. The IPWs are the inverse of these shares. To demonstrate the properties of the re-weighted sample, we compare it against the original data, as reported in the EIB allocation tables, and the sample of firms we use in the PSM model and subsequent regressions. The numbers are reported in Table 14.

[Table 14 about here.]

It can be readily observed that re-weighting brings back the properties of the original sample. The EU-wide figures indicate that in terms of number of firms the coverage increases from 13.25% to 90.42%, and in terms of average allocated amounts the ratio drops down from 135.27% to 99.81%, being nearly identical to the raw numbers. The gains are visible for each country group.

By including the IPWs as estimation weights in Eq. (13), we re-weight the evidence of ATET(1) contained in each observation to match the strata distribution of the original allocation database. While the findings fully hold for the whole range of weights, to improve the properties of the OLS estimators, we focus on strata for which the weights are below 50, i.e. there were more than 2% of initial firms that survived the data tuning procedures. The IPW-weighted estimation results are given in Table 15.

[Table 15 about here.]

The main findings remain statistically significant under this specification, too. Interestingly, the magnitude of the coefficients marginally decreases for the employment and total assets, while it increases for the patent regression.

6.3 Idiosyncratic loan demand and identification

One potential criticism of the analysis performed in this paper is that we cannot fully ensure that the companies in our control group exhibit demand for external financing the same way the treated companies do. In principle it is possible that at least some control group firms did not have project ideas to finance at hand, whereas the treated firms did. In that case the former would not have applied for external financing, whereas the latter would have. If such idiosyncratic differences in loan demand were present, we would not be able to determine if the results are driven by the fact that a firm received the EIB support, or they are stemming from the fact that treated companies were more likely to have a project to be financed in the first place. While the propensity score matching, which ensures a high level of similarity between treated and control firms, mitigates this problem to some extent, a more careful approach to address this problem is desired.

Similar concerns have been flagged by, for instance, Brown and Earle (2017). They point out that treated firms may have experienced idiosyncratic demand, productivity, or cost shocks, precisely during the treatment year. If positive, such shocks can raise demand or productivity, and if negative they can increase cost burdens. In any case, they may motivate firms to seek external financing either to increase production or to stay alive. While Brown and Earle (2017) develop an identification method based on instrumental variables, where instruments correspond to some of the observed characteristics of loan-granting banks, linking control firms with relevant banks in our setup drastically reduces the sample size.[18] Hence, we propose an alternative strategy.

We address this drawback at the selection into treatment stage. Specifically, in a follow-up exercise, we select only those firms from the pool of all potential counterfactuals that adjusted their balance sheet structure in the treatment year in a manner consistent with taking up a loan. For instance, such a signal can be inferred from deteriorating indebtedness metrics. By

[18] For instance, in the study of Ferrando and Wolski (2018), the number of firms with appropriately identified banking relation is less than 10% of the number of firms in the original sample.

imposing this additional constraint in the propensity score model, we can ensure that firms in the control group exhibited loan demand, and obtained external finance.[19]

We need to point out that this approach may bias the estimated impact of the EIB lending downwards. As our counterfactual now contains only firms that actually managed to access external finance, in this setup the impact of an EIB-suppoarted loan can manifest solely from the preferential conditions of an EIB loan (the *funding advantage channel*), and we rule out any potential impact from the other mechanism highlighted in our theory of change, which works through easing the banks' funding constraints and allowing access to finance to firms that would have suffered from credit rationing otherwise (the *credit easing channel*). In this sense, the results of this alternative specification can be considered as a lower bound of the overall impact.

Thus we adjust the PSM model, in Eq. 11, by controlling for the leverage ratio at time $t = 0$, with the corresponding squared and cubic terms. To control for possibly various liquidity preferences in project funding, we differentiate between corporate financing options. Financial leverage may well approximate other-than-EIB-backed loans coming from the banking sector. Trade credit might provide a clue on the unmet loan demand, as there is evidence that companies substituted bank financing with trade credit in the heights of the credit crunch (Ferrando and Wolski, 2018). Consequently, we use three alternative definitions of leverage, i.e. (i) total debt, (ii) total debt excluding trade credit and other liabilities (financial leverage)[20] and (iii) total debt excluding trade credit and other liabilities as well as cash and cash equivalents (net financial leverage). All the ratios are calculated as a share of total assets. The matching technique and the following DID regressions remain the same as in Section 5. The estimation results corrected for the demand effects are presented in Tables 16, 17 and 18.

[Table 16 about here.]

[Table 17 about here.]

[Table 18 about here.]

Again, the effects on the main variables of interest remain virtually unchanged against the results in Table 7, at the same significance level. Moreover, the magnitude of the effect remains unchanged when using different definitions of leverage ratio. These results suggest that the estimated impact is not a result of idiosyncratic differences between available project ideas, and consequently of loan demand, but it is causally linked to the treatment itself.

[19] The literature usually warrants against the use of observations in the propensity score model that are potentially influenced by the treatment itself (see for example Imbens (2004)). The reason behind that is such observations can bias the selection of the control group towards units that match the post-treatment dynamics of the treated. This may lead the model to under-estimate the treatment effect. In our case, however, this alternative specification is used to confirm the validity of our baseline results, despite the possibility of such a bias.

[20] Other liabilities are related to pensions, personnel costs, provisions and deferred taxes. Overall, financial leverage includes the sum of company's loans and long-term debt.

7 Conclusions

In this paper we ask whether, and to what extent, the EIB intermediated lending can provide tangible and meaasurable economic and financial benefits to the beneficiary SMEs. We tackle this question empirically by looking at the impact of EIB funding on SME performance in 28 EU Member States between 2008 and 2014.

Our results indicate that EIB lending has a positive effect on employment, firm growth, investment and the propensity to innovate. At the same time, we are unable to detect any persistent and significant impact on profitability. Moreover, EIB-funded firms record a deterioration in leverage, which, we believe, is largely driven by accounting mechanics. These results appear to be robust against a wide range of alternative model specifications and data tuning.

We also find that the positive impact of EIB funding is larger in certain geographic regions. In this respect, EIB lending in Central and East Europe has the highest impact, followed by South Europe. Beneficiaries in West and North Europe still enjoy a significant positive impact, however, the improvement over the control group is somewhat smaller than elsewhere.

Overall, we conclude that EIB lending, during the period in question, makes a difference. Conditional on data and methodological constraints, which we try to address in the best possible manner, our results provide support to the view that EIB funding supports employment, firm growth, investment and possibly also the innovative capacity of SMEs across the EU countries.

Looking forward, the study offers several avenues to explore in more details. Firstly, given the positive impact overall, one may try to understand which of the two channels outlined in Section 2 dominates. Secondly, it would be interesting to investigate to what extent the magnitude of the transferred financial advantage affects the results. In this respect, a natural extension to our setup would include a dose-response framework.

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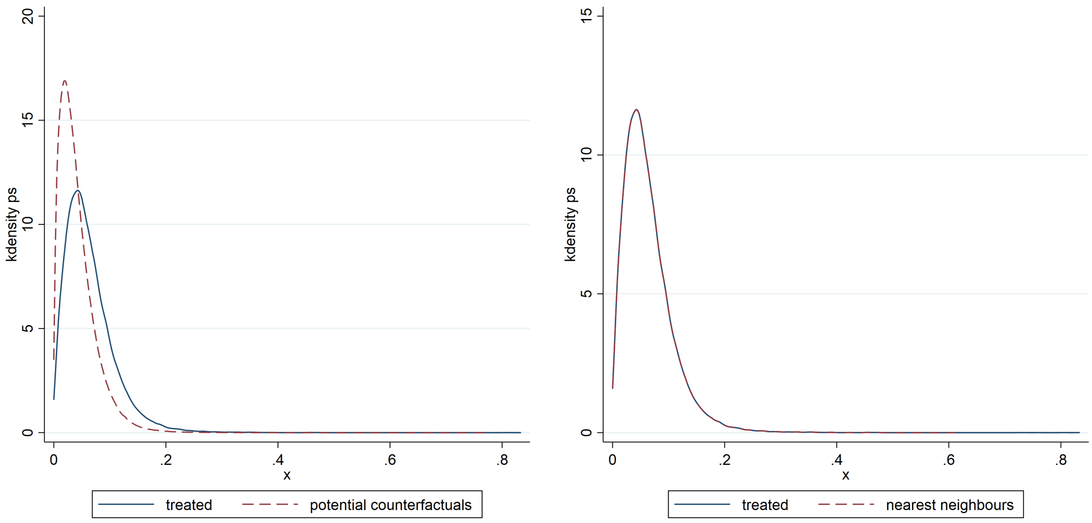
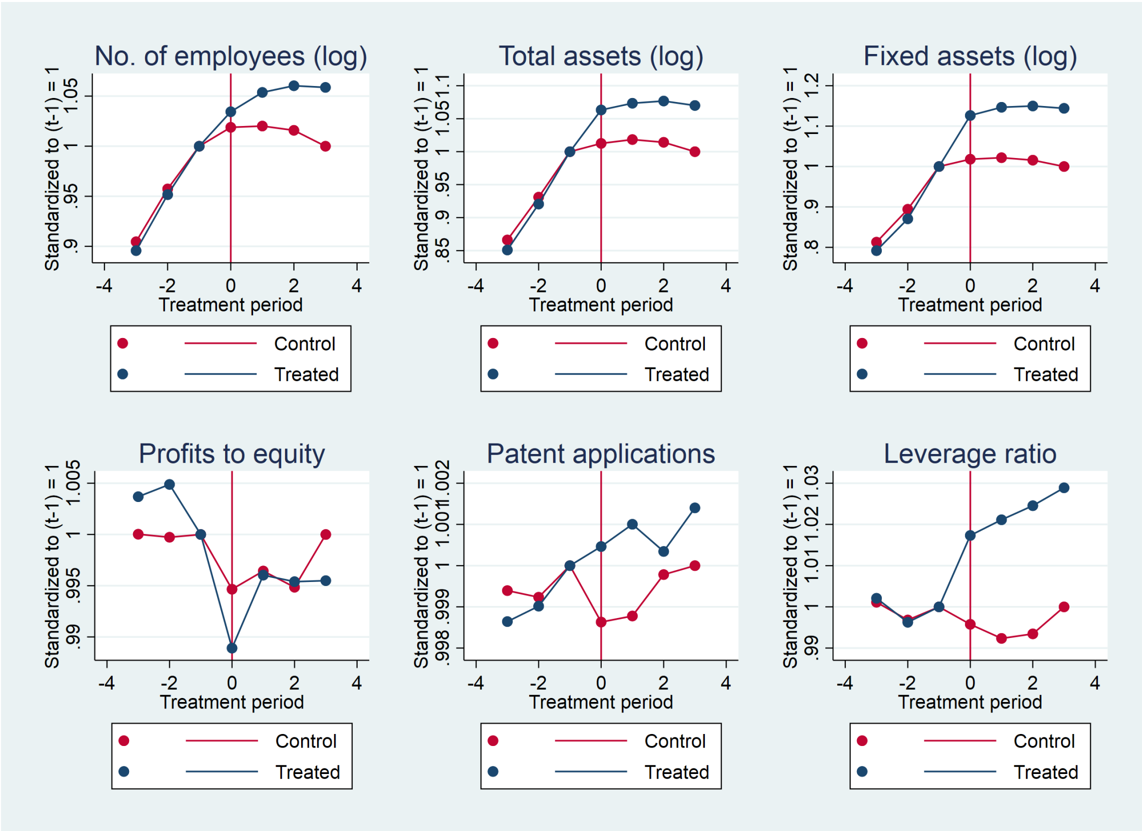
Figure 1: Density plots of propensity scores before and after the matching.



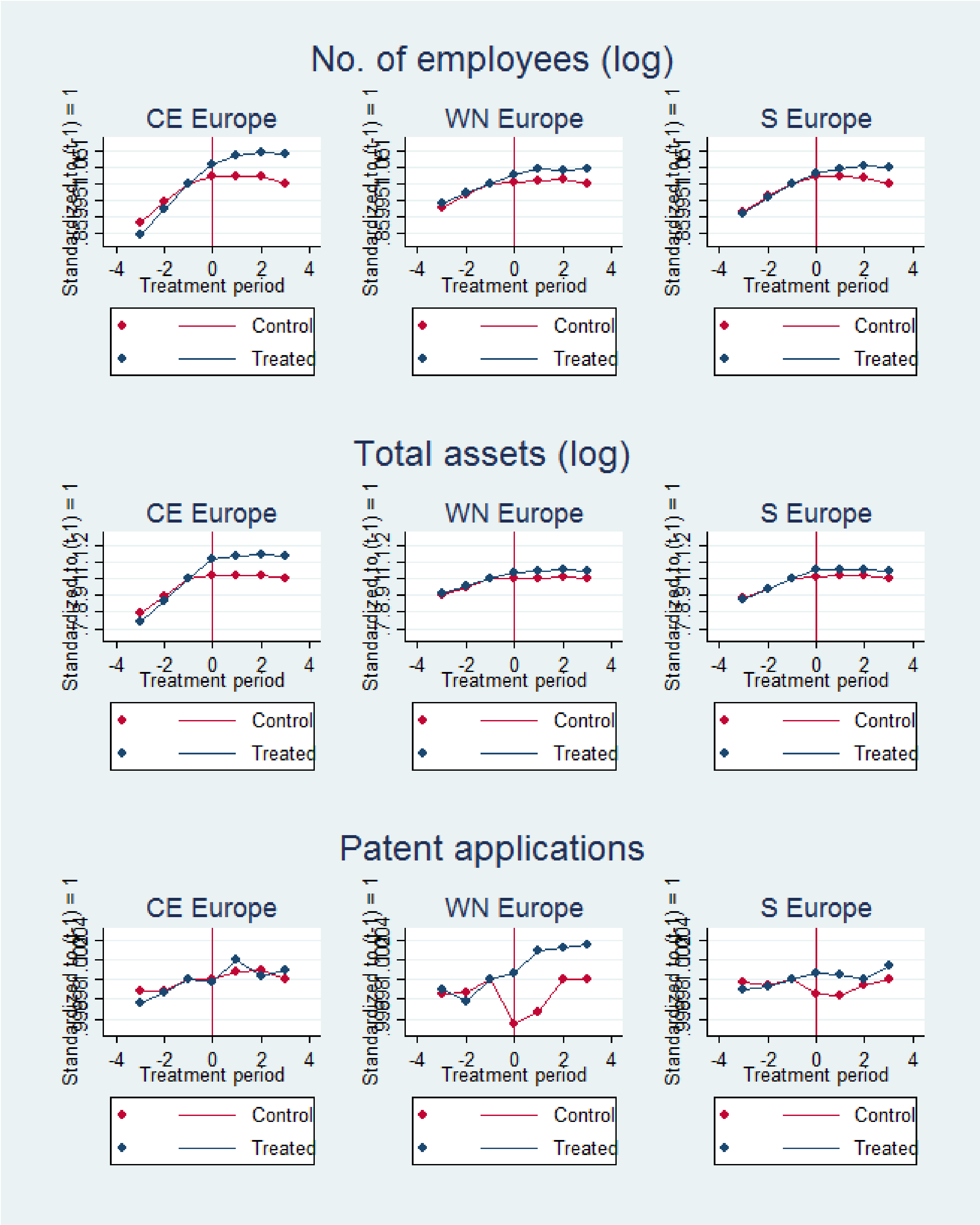
Notes: Fitted propensity scores from a probit model for the EIB loan beneficiaries ('Treated'), a full pool of potential controls ('Potential counterfactuals') and the matched controls ('Nearest neighbours').

Figure 2: Impact of EIB-supported lending to SMEs.



Notes: Performance of EIB loan beneficiaries ('Treated') against the comparison group ('Control') in the 3 years before and after the loan allocation. The treatment year at $t = 0$ *with standardized scale* $t - 1 \equiv 1$

Figure 3: Impact of EIB-supported lending to SMEs by country groups.



Notes: Performance of EIB loan beneficiaries ('Treated') against the comparison group ('Control') in the 3 years before and after the loan allocation in three country groups. CE Europe includes Bulgaria, Croatia, Czech Republic, Hungary, Latvia, Poland, Romania, Slovakia and Slovenia; S Europe covers Greece, Italy, Portugal and Spain; and WN Europe spans over Austria, Belgium, Finland, France, Germany, Luxembourg, Netherlands, Sweden and the United Kingdom. The treatment year at $t = 0$ *with standardized scale* $t - 1 \equiv 1$.

Table 1: EIB allocation data.

by country					
	Allocations (in #)	Share (in %)	Amount (in mEUR)	Share (in %)	Mean size (in kEUR)
Austria	2,377	0.46	1,539	2.13	648
Belgium	9,784	1.88	1,577	2.18	161
Bulgaria	3,525	0.68	601	0.83	170
Croatia	4,008	0.77	1,458	2.01	364
Cyprus	782	0.15	274	0.38	350
Czech Republic	12,577	2.42	1,908	2.64	152
Denmark	3,494	0.67	397	0.55	114
Estonia	5	0.00	12	0.02	2,429
Finland	1,524	0.29	359	0.50	236
France	37,009	7.11	6,210	8.58	168
Germany	11,149	2.14	3,770	5.21	338
Greece	4,227	0.81	1,962	2.71	464
Hungary	5,876	1.13	1,526	2.11	260
Ireland	3,313	0.64	512	0.71	155
Italy	77,173	14.82	17,560	24.25	228
Latvia	1,856	0.36	197	0.27	106
Lithuania	27	0.01	48	0.07	1,767
Luxembourg	1,451	0.28	724	1.00	499
Netherlands	7,071	1.36	2,278	3.15	322
Poland	97,137	18.65	3,547	4.90	37
Portugal	12,034	2.31	3,208	4.43	267
Romania	5,239	1.01	504	0.70	96
Slovakia	11,375	2.18	1,543	2.13	136
Slovenia	3,558	0.68	640	0.88	180
Spain	193,451	37.15	17,809	24.60	92
Sweden	4,642	0.89	88	0.12	19
United Kingdom	6,082	1.17	2,149	2.97	353
Total	520,746	100	72,401	100	

by year					
	Allocations (in #)	Share (in %)	Amount (in mEUR)	Share (in %)	Mean size (in kEUR)
2008	39,129	7.51	6,142	8.48	157
2009	42,722	8.20	7,259	10.03	170
2010	63,865	12.26	10,082	13.92	158
2011	63,849	12.26	13,148	18.16	206
2012	75,796	14.56	9,326	12.88	123
2013	101,185	19.43	11,311	15.62	112
2014	134,200	25.77	15,134	20.90	113
Total	520,746	100	72,401	100	

by employment class					
	Allocations (in #)	Share (in %)	Amount (in mEUR)	Share (in %)	Mean size (in kEUR)
0-1	115,774	22.23	10,209	14.10	88
2-10	186,913	35.89	14,781	20.42	79
11-50	135,184	25.96	20,034	27.67	148
51-250	76,443	14.68	20,913	28.88	274
250-500	3,404	0.65	2,732	3.77	803
501 or missing	3,028	0.58	3,733	5.16	1,236
Total	520,746	100	72,401	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 #)	(in %)	with useful data (in #)	(in %)
Austria	1,861	699	37.56	1	0.05
Belgium	7,673	2,797	36.45	361	4.70
Bulgaria	2,633	1,955	74.25	963	36.57
Croatia	3,394	1,741	51.30	1159	34.15
Cyprus	765	303	39.61	0	-
Czech Republic	9,092	7,126	78.38	2659	29.25
Denmark	2,097	1,412	67.33	0	-
Estonia	3	1	33.33	0	-
Finland	1,257	924	73.51	221	17.58
France	24,991	12,081	48.34	2722	10.89
Germany	8,134	3,144	38.65	246	3.02
Greece	3,688	298	8.08	66	1.79
Hungary	3,953	2,631	66.56	1233	31.19
Ireland	3,021	334	11.06	0	-
Italy	56,918	22,124	38.87	8911	15.66
Latvia	1,219	368	30.19	94	7.71
Lithuania	26	12	46.15	0	-
Luxembourg	1,011	537	53.12	52	5.14
Netherlands	5,825	1,851	31.78	96	1.65
Poland	70,761	22,630	31.98	605	0.85
Portugal	10,681	4,156	38.91	2887	27.03
Romania	3,855	3,558	92.30	2789	72.35
Slovakia	7,950	4,897	61.60	1751	22.03
Slovenia	2,668	1,986	74.44	975	36.54
Spain	161,054	49,543	30.76	24425	15.17
Sweden	4,058	3,217	79.28	1273	31.37
United Kingdom	5,200	1,741	33.48	2	0.04
Total	403,788	152,066	37.66	53491	13.25

by year

	Total EIB (in #)	with BvDID (in #)	(in %)	with useful data (in #)	(in %)
2008	29,354	8,847	30.14	3,055	10.41
2009	34,433	12,710	36.91	4,506	13.09
2010	49,591	17,402	35.09	5,886	11.87
2011	47,975	19,312	40.25	6,808	14.19
2012	57,126	17,269	30.23	5,160	9.03
2013	79,869	22,088	27.66	7,344	9.20
2014	105,440	54,438	51.63	20,732	19.66
Total	403,788	152,066	37.66	53,491	13.25

by employment class

	Total EIB (in #)	with BvDID (in #)	(in %)	with useful data (in #)	(in %)
0-1	101,893	16,945	16.63	2,280	2.24
2-10	156,875	66,469	42.37	20,069	12.79
11-50	95,261	46,041	48.33	20,941	21.98
51-250	46,408	20,748	44.71	9,470	20.41
250-500	1,813	1,032	56.92	488	26.92
501 or missing	1,538	831	54.03	243	15.80
Total	403,788	152,066	37.66	53,491	13.25

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: Probit model results.

	Lag 1	Lag 2	Lag 3	Lag 1 (sq)	Lag 2 (sq)	Lag 3 (sq)	Lag 1 (cub)	Lag 2 (cub)	Lag 3 (cub)
Leverage ratio	2.458***	-0.418***	0.600***	-1.898***	0.133	-0.372***	0.445***	-0.022	0.064*
	(0.115)	(0.141)	(0.117)	(0.121)	(0.146)	(0.121)	(0.035)	(0.043)	(0.035)
Employment (log)	-0.054	0.140*	-0.246***	0.051**	-0.047	0.102***	-0.006**	0.004	-0.012***
	(0.063)	(0.074)	(0.055)	(0.024)	(0.029)	(0.021)	(0.003)	(0.003)	(0.002)
Total assets (log)	4.836***	-2.426***	-3.666***	-0.319***	0.162***	0.277***	0.008***	-0.004**	-0.007***
	(0.660)	(0.819)	(0.570)	(0.048)	(0.060)	(0.042)	(0.001)	(0.001)	(0.001)
Cash ratio	-0.937***	-0.505***	-0.598***	2.295***	1.136**	1.165**	-1.625***	-0.832	-0.823*
	(0.125)	(0.131)	(0.120)	(0.494)	(0.509)	(0.465)	(0.508)	(0.514)	(0.467)
Tangible assets ratio	2.753***	-1.001***	0.243*	-3.361***	1.169**	-0.789**	1.501***	-0.674*	0.476*
	(0.142)	(0.176)	(0.139)	(0.376)	(0.460)	(0.371)	(0.284)	(0.345)	(0.280)
Current ratio	0.087***	-0.004	0.004	-0.007***	-0.000	-0.001	0.000***	0.000	0.000
	(0.007)	(0.008)	(0.008)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Turnover ratio	0.207***	0.343***	0.103***	-0.057***	-0.088***	-0.049***	0.005***	0.007***	0.005***
	(0.037)	(0.039)	(0.025)	(0.010)	(0.011)	(0.008)	(0.001)	(0.001)	(0.001)
Sales growth	0.165***	-0.045**	-0.003						
	(0.019)	(0.018)	(0.005)						
Patents	0.115***	0.052*	0.072***						
	(0.027)	(0.030)	(0.028)						
Size class FE	Yes								
Age class FE	Yes								
Allocation year FE	Yes								
Sector FE	Yes								
Country FE	Yes								
Observations	737,162								
R2	0.069								

*Notes: Estimation results of the probit model. Employment is measured as number of employees. Patent variable is a dummy depending if a company filled at least one patent application or publication in a given year. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 4: Summary statistics.

Unmatched controls						
	Obs.	Mean	Median	St. dev.	Min.	Max.
Leverage ratio	2,460,476	0.64	0.65	0.34	0.03	2.33
Employment (log)	2,460,486	2.59	2.48	1.24	0.69	6.04
Assets (log)	2,460,486	14.04	13.98	1.74	10.00	17.75
Cash ratio	2,460,476	0.14	0.07	0.17	0.00	0.78
Tangible ratio	2,460,476	0.28	0.21	0.25	0.00	0.94
Current ratio	2,453,597	2.43	1.37	3.87	0.09	31.02
Turnover ratio	2,367,320	1.63	1.31	1.29	0.02	7.34
Sales growth	2,290,178	0.10	0.02	0.50	-0.78	3.25
Patent (app)	2,460,486	0.01	0.00	0.10	0.00	1.00
Patent (pub)	2,460,486	0.01	0.00	0.11	0.00	1.00

Matched controls						
	Obs.	Mean	Median	St. dev.	Min.	Max.
Leverage ratio	149,109	0.68	0.69	0.29	0.03	2.33
Employment (log)	149,109	2.80	2.77	1.19	0.69	6.04
Assets (log)	149,109	14.36	14.36	1.63	10.00	17.75
Cash ratio	149,109	0.10	0.05	0.14	0.00	0.78
Tangible ratio	149,109	0.31	0.26	0.24	0.00	0.94
Current ratio	149,109	1.94	1.27	2.86	0.09	31.02
Turnover ratio	149,109	1.62	1.36	1.18	0.02	7.34
Sales growth	146,206	0.13	0.03	0.52	-0.78	3.25
Patent (app)	149,109	0.01	0.00	0.12	0.00	1.00
Patent (pub)	149,109	0.02	0.00	0.13	0.00	1.00

Matched treated						
	Obs.	Mean	Median	St. dev.	Min.	Max.
Leverage ratio	157,547	0.68	0.70	0.27	0.03	2.33
Employment (log)	157,547	2.81	2.71	1.19	0.69	6.04
Assets (log)	157,547	14.36	14.36	1.59	10.00	17.75
Cash ratio	157,547	0.10	0.04	0.13	0.00	0.78
Tangible ratio	157,547	0.31	0.26	0.24	0.00	0.94
Current ratio	157,547	1.82	1.28	2.45	0.09	31.02
Turnover ratio	157,547	1.62	1.35	1.16	0.02	7.34
Sales growth	154,992	0.13	0.04	0.50	-0.78	3.25
Patent (app)	157,547	0.01	0.00	0.12	0.00	1.00
Patent (pub)	157,547	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 5: Balancing properties.

	Unmatched bias	p-value	Matched bias	p-value
Leverage ratio	0.063	0.000	0.005	0.043
Employment (log)	0.084	0.000	0.002	0.489
Assets (log)	0.023	0.000	0.000	0.853
Cash ratio	-0.309	0.000	-0.089	0.000
Tangible ratio	0.092	0.000	0.007	0.117
Current ratio	-0.249	0.000	-0.059	0.000
Turnover ratio	-0.002	0.000	0.000	0.985
Sales growth	0.324	0.000	0.010	0.542
Patent (app)	0.319	0.000	0.021	0.634
Patent (pub)	0.328	0.000	0.052	0.217

Notes: Standardized percentage bias before and after matching in the 3-year pre-treatment period. 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. P-values correspond to the test of equivalence in means between the treated and control groups for a given variable.

Table 6: Assessment of common trends.

	(1) Employment (log)	(2) Total assets (log)	(3) Patent app.	(4) ROE	(5) Fixed assets (log)	(6) Leverage ratio
Time trend x Treated	-0.002	0.000	0.000	0.004	-0.002	-0.000
	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.001)
Time trend	0.033***	0.068***	-0.000	-0.022***	0.095***	-0.007***
	(0.001)	(0.001)	(0.000)	(0.002)	(0.002)	(0.001)
Const.	2.869***	14.499***	0.014***	0.077***	13.222***	0.669***
	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.001)
Firm-level FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	306,656	306,656	306,656	306,302	305,756	306,656
R2	0.972	0.984	0.663	0.427	0.964	0.906

*Notes: Estimation of differences in trends between treated and control groups in 3-year pre-treatment period. Employment is measured as number of employees. Patent variable is a dummy depending if a company filled at least one patent application or publication in a given year. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 7: Impact of the EIB lending - main results.

	(1) Employment (log)	(2) Total assets (log)	(3) Patent app.
Post x Treated	0.055***	0.072***	0.001**
	(0.003)	(0.003)	(0.001)
Post	-0.058***	-0.086***	-0.001
	(0.002)	(0.002)	(0.001)
Firm-level FE	Yes	Yes	Yes
Country x sector x year FE	Yes	Yes	Yes
Observations	665,630	668,266	669,087
R2	0.95	0.973	0.56

*Notes: Estimation results of the main treatment effects model. Employment is measured as number of employees. Patent applications are measured as a dummy depending if a company filled at least one patent application in a given year. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 8: Impact of the EIB lending - additional variables.

	(1) ROE	(2) Fixed assets (log)	(3) Leverage ratio
Post x Treated	-0.003	0.142***	0.024***
	(0.003)	(0.005)	(0.001)
Post	0.005	-0.138***	-0.011***
	(0.003)	(0.003)	(0.001)
Firm-level FE	Yes	Yes	Yes
Country x sector x year FE	Yes	Yes	Yes
Observations	667,389	665,997	667,723
R2	0.291	0.941	0.826

*Notes: Estimation results of the main treatment effects model. ROE is measured as net profit to shareholders' funds ratio. Leverage ratio is measured as a share of current and non-current liabilities to total assets. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 9: Impact of the EIB lending - yearly decomposition.

	(1) Employment (log)	(2) Total assets (log)	(3) Patent app.	(4) ROE	(5) Fixed assets (log)	(6) Leverage ratio
Post (1st year) x Treated	0.033***	0.049***	0.002***	-0.001	0.110***	0.024***
	(0.003)	(0.003)	(0.001)	(0.004)	(0.004)	(0.001)
Post (2nd year) x Treated	0.044***	0.056***	0.000	-0.000	0.119***	0.026***
	(0.003)	(0.003)	(0.001)	(0.004)	(0.005)	(0.002)
Post (3rd year) x Treated	0.058***	0.064***	0.001	-0.006	0.129***	0.024***
	(0.004)	(0.004)	(0.001)	(0.005)	(0.007)	(0.002)
Post (1st year)	-0.055***	-0.084***	-0.001*	0.006	-0.137***	-0.010***
	(0.002)	(0.002)	(0.001)	(0.004)	(0.003)	(0.001)
Post (2nd year)	-0.101***	-0.149***	-0.000	0.007	-0.233***	-0.010***
	(0.003)	(0.002)	(0.001)	(0.005)	(0.004)	(0.001)
Post (3rd year)	-0.160***	-0.223***	-0.000	0.015***	-0.340***	-0.005***
	(0.003)	(0.003)	(0.001)	(0.006)	(0.006)	(0.002)
Firm-level FE	Yes	Yes	Yes	Yes	Yes	Yes
Country x sector x year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	665,630	668,266	669,087	667,389	665,997	667,723
R2	0.95	0.973	0.561	0.291	0.942	0.826

*Notes: Estimation results of the main treatment effects model by post-treatment years. Employment is measured as number of employees. Patent applications are measured as a dummy depending if a company filled at least one patent application in a given year. ROE is measured as net profit to shareholders' funds ratio. Leverage ratio is measured as a share of current and non-current liabilities to total assets. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 10: Impact of the EIB lending - breakdown by country groups.

	(1) Employment (log)	(2) Total assets (log)	(3) Patent app.
Post x Treated	0.030***	0.037***	0.019
	(0.007)	(0.007)	(0.018)
Post x CE Europe	-0.054***	-0.091***	0.001
	(0.007)	(0.006)	(0.016)
Post x S Europe	-0.026***	-0.024***	-0.004
	(0.006)	(0.005)	(0.017)
Post x Treated x CE Europe	0.061***	0.102***	-0.005
	(0.010)	(0.010)	(0.019)
Post x Treated x S Europe	0.017**	0.015*	-0.017
	(0.008)	(0.008)	(0.019)
Firm-level FE	Yes	Yes	Yes
Country x sector x year FE	Yes	Yes	Yes
Observations	665,630	668,266	669,087
R2	0.95	0.973	0.77

*Notes: Estimation results of the main treatment effects model by country groups. Base category are the West and North Europe countries as specified in the text. Employment is measured as number of employees. Patent applications are measured as a dummy depending if a company filled at least one patent application in a given year. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 11: Impact of the EIB lending - balanced panel of firms.

	(1) Employment (log)	(2) Total assets (log)	(3) Patent app.
Post x Treated	0.053***	0.068***	0.002**
	(0.003)	(0.003)	(0.001)
Post	-0.050***	-0.070***	-0.001
	(0.002)	(0.002)	(0.001)
Firm-level FE	Yes	Yes	Yes
Country x sector x year FE	Yes	Yes	Yes
Observations	529,675	531,982	532,457
R2	0.949	0.973	0.566

*Notes: Estimation results of the main treatment effects on the balanced panel of firms. Employment is measured as number of employees. Patent applications are measured as a dummy depending if a company filled at least one patent application in a given year. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 12: Impact of the EIB lending - standard errors' correction.

	(1) Employment (log)	(2) Total assets (log)	(3) Patent app.
Post x Treated	0.071***	0.088***	0.001**
	(0.003)	(0.003)	(0.001)
Post	-0.007***	0.061***	-0.004***
	(0.002)	(0.002)	(0.000)
Firm-level FE	Yes	Yes	Yes
Observations	201608	202774	202962
R2	0.963	0.979	0.776

*Notes: Estimation results of the main treatment effects model corrected for the serial correlation of error terms. Employment is measured as number of employees. Patent applications are measured as a dummy depending if a company filled at least one patent application in a given year. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 13: Placebo treatment period.

	(1) Employment (log)	(2) Total assets (log)	(3) Patent app.	(4) ROE	(5) Fixed assets (log)	(6) Leverage ratio
Post x Treated	-0.001	0.002	0.001	0.008*	0.001	-0.001
	(0.002)	(0.002)	(0.001)	(0.004)	(0.004)	(0.001)
Post	0.045***	0.101***	-0.000	-0.034***	0.147***	-0.008***
	(0.002)	(0.002)	(0.000)	(0.003)	(0.003)	(0.001)
Firm-level FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	306,280	306,280	306,280	305,885	305,248	306,280
R2	0.971	0.984	0.663	0.425	0.964	0.906

*Notes: Estimation results of the placebo treatment effects model, with simulated treatment in period $t-2$. Employment is measured as number of employees. Patent applications are measured as a dummy depending if a company filled at least one patent application in a given year. ROE is measured as net profit to shareholders' funds ratio. Leverage ratio is measured as a share of current and non-current liabilities to total assets. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 14: The properties of re-weighting.

Number of firms					
	Total EIB (in #)	with useful data (in #)	(in %)	re-weighted (in #)	(in %)
Central and East Europe	105,540	12,228	11.59	90,285	85.55
South Europe	233,093	36,289	15.57	228,190	97.90
West and North Europe	65,155	4,974	7.63	46,631	71.57
EU-wide	403,788	53,491	13.25	365,106	90.42

Amounts					
	Total EIB (in kEUR)	with useful data (in kEUR)	(in %)	re-weighted (in kEUR)	(in %)
Central and East Europe	91.42	206.69	226.08	100.63	110.08
South Europe	146.13	192.97	132.06	152.24	104.19
West and North Europe	225.39	187.76	83.30	190.32	84.44
EU-wide	144.62	195.62	135.27	144.35	99.81

Notes: 'Total EIB' represents number of firms and average amounts as reported in the EIB allocation database, 'with useful data' describes number of firms and average amounts with sufficient data coverage to be included in the PSM, and 're-weighted' shows number of firms and average amounts re-weighted using the IPW weights. Shares represent the proportion of Total EIB. Country groups are specified in the text.

Table 15: Impact of the EIB lending - IPW-weighted results.

	(1) Employment (log)	(2) Total assets (log)	(3) Patent app.
Post x Treated	0.040***	0.042***	0.005***
	(0.009)	(0.009)	(0.002)
Post	-0.056***	-0.087***	-0.005***
	(0.008)	(0.008)	(0.002)
Firm-level FE	Yes	Yes	Yes
Country x sector x year FE	Yes	Yes	Yes
Observations	340046	341823	342637
R2	0.955	0.975	0.595

*Notes: Estimation results of the main treatment effects model with allocation IPW weights. Observations with weights above 50 are excluded. Employment is measured as number of employees. Patent applications are measured as a dummy depending if a company filled at least one patent application in a given year. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 16: Impact of the EIB lending - loan demand correction.

	(1) Employment (log)	(2) Total assets (log)	(3) Patent app.
Post x Treated	0.057*** (0.003)	0.076*** (0.003)	0.001** (0.001)
Post	-0.058*** (0.002)	-0.090*** (0.002)	-0.001** (0.001)
Firm-level FE	Yes	Yes	Yes
Country x sector x year FE	Yes	Yes	Yes
Observations	662,787	665,550	666,213
R2	0.949	0.972	0.566

*Notes: Estimation results of the main treatment effects on the sample matched on an extended PSM model with leverage ratio in $t = 0$. The leverage ratio is calculated as a share of total debt to total assets. Employment is measured as number of employees. Patent applications are measured as a dummy depending if a company filled at least one patent application in a given year. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 17: Impact of the EIB lending - loan demand correction (alternative leverage definition).

	(1) Employment (log)	(2) Total assets (log)	(3) Patent app.
Post x Treated	0.059*** (0.003)	0.067*** (0.003)	0.002*** (0.001)
Post	-0.058*** (0.002)	-0.076*** (0.002)	-0.001** (0.001)
Firm-level FE	Yes	Yes	Yes
Country x sector x year FE	Yes	Yes	Yes
Observations	543,353	545,621	546,283
R2	0.952	0.975	0.561

*Notes: Estimation results of the main treatment effects on the sample matched on an extended PSM model with leverage ratio in $t = 0$. The leverage ratio is calculated as a share of loans and long-term debt to total assets. Employment is measured as number of employees. Patent applications are measured as a dummy depending if a company filled at least one patent application in a given year. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

Table 18: Impact of the EIB lending - loan demand correction (alternative leverage definition).

	(1) Employment (log)	(2) Total assets (log)	(3) Patent app.
Post x Treated	0.058***	0.066***	0.001
	(0.003)	(0.003)	(0.001)
Post	-0.056***	-0.073***	-0.000
	(0.002)	(0.002)	(0.001)
Firm-level FE	Yes	Yes	Yes
Country x sector x year FE	Yes	Yes	Yes
Observations	535,892	538,103	538,753
R2	0.952	0.975	0.568

*Notes: Estimation results of the main treatment effects on the sample matched on an extended PSM model with leverage ratio in $t = 0$. The leverage ratio is calculated as a share of loans and long-term debt minus cash and cash equivalents to total assets. Employment is measured as number of employees. Patent applications are measured as a dummy depending if a company filled at least one patent application in a given year. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels.*

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