Do IFIs make a difference? The impact of EIB lending support for SMEs in Central and Eastern Europe during the global financial crisis.

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Abstract

Does IFI funding provide support to SMEs receiving such funding? We assess the impact of funding by the European Investment Bank (EIB) on the performance of 5,074 SMEs in eight countries of Central and Eastern Europe (CEE) during 2008-2014. Our results derived from a propensity score matching and difference-in-difference estimation exercises indicate that EIB lending has a positive effect on employment, revenues and profitability. We also find that the positive impact of EIB funding on employment and revenues is significantly higher when it is provided in a crisis year and firms face a prolonged crisis. Treated firms also record an even larger advantage in terms of profitability. Overall, our results provide support to the view that IFI funding makes a difference in a period characterized by financial and economic turmoil.

JEL classification: G01, H81, L25
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1 Introduction

Development and promotional banks are thought to play an important catalytical role in supporting development in specific sectors. Small and medium-sized enterprises (SMEs) are important beneficiaries, reflecting the view that they play a key role for growth and employment (Anginer et al. (2011), Griffith-Jones et al. (2017), de la Torre et al. (2017)), but are facing credit constraints due to information asymmetries larger companies are less subject to (Beck and Demirguc-Kunt, 2006). During the global financial crisis, development banks have also assumed new roles such as explicitly aiming for counter-cyclical investment to prevent large-scale deleveraging with possible negative consequences on SMEs, a key transmission mechanism of financial crises as evidenced in the Great Depression (Bernanke, 1983). As a result, loan portfolios of promotional and development banks recorded much stronger growth in the aftermath of the Lehman default than portfolios held by private commercial banks (de Luna-Martinez and Vicente, 2012).

In many cases these activities have been supported and/or complemented by supra- and international financial institutions (IFIs) which adopted a variety of countercyclical financial measures to support SME finance. An example of such a support is the Joint International Financial Institutions Plan for Growth (JIAP) funded by EIB, EBRD and the World Bank (Final Report on the Joint IFI action plan for Growth in Central and South Eastern Europe, 2015).

Given the quantitative dimensions involved comprehensive and reliable impact assessments of IFI support to SMEs are scarce (Bah et al. (2011), Cassano et al. (2013), Asdrubali and Signore (2015)). This partly reflects the fact that in the developing world starting with the 1980s promotional and development banks were seen increasingly critically as many of them regularly recorded losses or failed to reach the beneficiaries they were supposed to reach (Hellman (1996), Caprio and Demirguc-Kunt (1998)). In mature economies, notably in continental Europe, established promotional banks continued to operate smoothly, significantly expanding the range of activities and balance sheet volumes (Harries (1998)). However, in a world turning towards bank privatization, financial liberalization and globalization (Porta et al. (2002), Clarke et al. (2005)), these institutions were widely neglected as a research topic (Robinson (2009), Hanley et al. (2016)).

Perspectives changed somewhat after the global financial crisis. While government ownership in banking and direct states interventions into the financial sector via development banks still meet substantial scepticism, mainly due to the governance challenges involved (World Bank 2013), the crisis has raised questions on the role of private sector finance Zingales (2015). In the developing world, the rise of China and India, featuring largely government-owned banks and heavily regulated financial sectors, triggered new research on the role of government-owned banks and state interventions into the financial sector (Xiao and Zhao (2012), Shen and Lin (2012), Andrianova et al. (2012)). Moreover, counter-cyclical finance received greater attention and most research found that government-owned banks (Bertay et al., 2015) and development banks ((Torres and Zeidan, 2016)) contributed to less severe decline in funding in the immediate post-crisis years.

Against this background, we assess the impact of EIB-supported funding on SME performance in Central and Eastern Europe (CEE), with a particular focus on the effectiveness in crisis times. We do so as SMEs represent one of EIBs five key operational priorities (EIB 2013, 3) and the CEE region was hit hard by the global financial crisis, also in comparison to other emerging markets regions (Goldstein and Xie (2009), Gallego et al. (2010), Bakker and Klingen (2012)). Moreover, there is evidence that EIB lending in the region made a larger difference for beneficiary leverage than in other EU countries (EIB 2013, 18).

Concretely, we exploit EIB lending data and blend it with publicly available data on individual SMEs financial and economic performance from the Bureau van Dijks Orbis / Amadeus dataset. By merging both datasets, and applying propensity score matching we construct a treatment and a control group. This allows us to run difference-in-difference (DiD) regressions testing whether SMEs receiving EIB-supported loans provided via local banks perform differently with respect to outcome variables, such as employment, revenues, profits, profitability and solvency compared to non-receiving SMEs. Furthermore, to estimate the effect of the crisis on the effectiveness of EIB lending, we address the issue of differences in the performance of firms receiving EIB funding during the crisis and during normal times. Concretely, we analyse firms receiving funding during the crisis, and compare the outcomes of firms located in countries where the crisis is still on-going with the outcome of firms located in countries where the crisis has ended.

Our results show firms receiving EIB lending record significantly higher employment and profitability (measured as EBITDA ratio) than the control group of firms established by propensity score matching, i.e. firms with similar observable characteristics as the receiving firms. Moreover, EIB lending has a negative effect on liquidity and solvency. We interpret the latter effect as an accounting effect: firms receiving EIB funds by implication become less liquid and solvent compared to the control group as any investment funded by the EIB loan reduces liquidity and the funding itself raises leverage, i.e. is associated with a decline in the equity ratio and hence in firm solvency.

We also find that in crisis times the positive effects on employment are more pronounced. We interpret this result as indicating that the comparative advantage that the receiving firms attain against non-receiving firms in a long-lasting crisis prevail over the dampening effect the crisis has on the demand. This is supported by the results that show a positive and significant effect of EIB lending on profits and revenues during the crisis over and above the effect measure in normal times. Together, these results indicate that in a crisis, when financial constraints intensify, treated firms gain a larger advantage compared to firms which do not receive funding compared to the effect measured during normal times.

The paper is organized as follows. Section 2 provides a rationale for public sector intervention into SME financing. In Section 3 we explain our data sources. Section 4 provides details concerning our empirical framework, including the propensity score matching and the difference in difference estimation. In Section 5 we discuss the results. In section 6 we present the results of the effect of the crisis on the effectiveness of EIB lending. In section 7 we conclude.

2 Rationale for public sector intervention into SME financing

Public-sector banks in the form of promotional and development banks have a long history, in a national and in an international or supranational setting. Moreover, in some countries and in certain periods these banks account for a substantial share of lending to the private and public sector in the given economy.

The rationale for public sector involvement in the financial sector supporting certain target groups, most importantly SMEs, is a market failure (Lazzarini et al., 2015). Information asymmetries, which can lead to both moral hazard and adverse selection of low quality borrowers, make private sector financial institutions reluctant to extend credit, especially uncollateralised credit, to SMEs and mid-cap companies, even at high interest rates (Jaffee and Russell (1976), Stiglitz and Weiss (1981))[1]. Thus, there is credit rationing, i.e. banks keep the supply of credit below demand, rather than to increase the interest rate charged on loans. As a result, many SMEs with economically viable projects are credit constraint ((Beck and Demirguc-Kunt, 2006)), i.e. they often cannot obtain funding from the regular system of financial intermediation.

Credit constraints prevent SMEs from implementing investments with high marginal returns that would lead them to a better performance with regard to outcome variables such as production, employment, profitability, liquidity or solvency. This is why the SME financing gap (OECD, 2006) is of general economic policy concern: it signals a loss of aggregate output, employment and productivity compared to a market solution that would emerge without information asymmetries.

[1]SMEs are more affected by credit rationing than larger companies because decision making processes, transparency rules, 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 cost of monitoring them is higher.

The SME financing gap usually widens in cyclical downturns and crisis periods as private sector banks become more risk averse given declining equity ratios reflecting crisis-related losses. This effect might be reinforced by the introduction of tighter regulatory standards, such as the Basel III framework (EBA 2016). Empirical evidence shows that low and declining bank capital has a negative impact on corporate lending activities by banks (Gambacorta and Shin, 2016). Factors related to the SMEs themselves also contribute to cyclical worsening of the credit rationing. For example, a financial crisis is associated with sharp drops in real estate prices. As property assets are a key source of collateral provided by SMEs (Gertler and Gilchrist, 1994), the decline in prices aggravates the funding problem of SMEs. Moreover, financial crises are associated with rising uncertainty related to the economic outlook which can exacerbate information asymmetries and result in a further decline in the banks willingness to lend to SMEs[2].

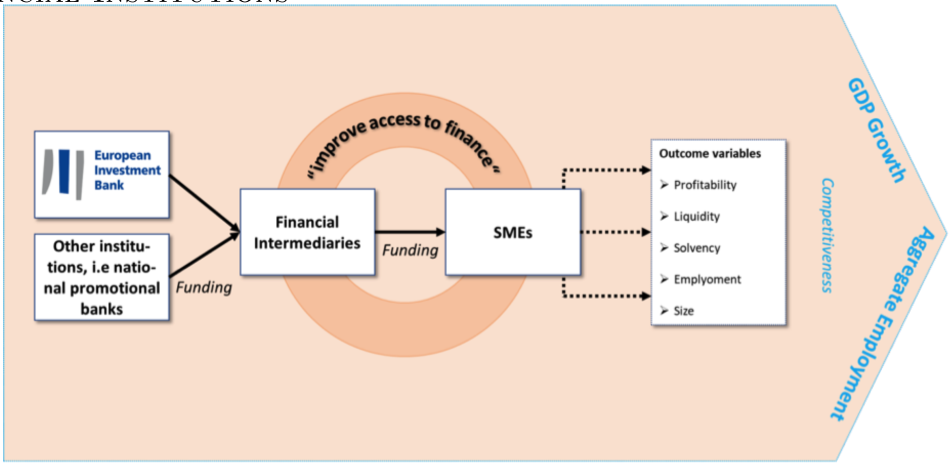
In the context of programme evaluation, these considerations provide the basis for the theory of change underlying the activities of national development and promotional banks as well as international and supranational financial institutions, such as the EIB. The theory stipulates that (access to) credit represents a treatment of dismal SME performance for outcome variables such as production, employment, profitability, liquidity and solvency (Figure 1).[3]

IFIs might facilitate access to credit in two ways. First, they mandate financial intermediaries receiving IFI loans to pass some of the funding advantage intermediaries benefit from on to borrowing SMEs (transfer-of-financial-advantage (TOFA) clause). Concretely, if the market rate for long-term funding is 4% and the IFI provides loans at 3%, the SMEs receiving funding from the IFI loan benefit if they have to pay a lower interest rate on their loan than comparable SMEs funded by resources intermediaries tap from private capital markets. Second, the IFI contribution might consist of alleviating constraints on the intermediarys funding side, i.e. the IFI line of credit allows the intermediary to expand its funding base and by doing so makes it possible for the intermediary to lend to firms that would otherwise have remained unserved, at least by the intermediaries receiving IFI funds.

[2] There is an increasing body of literature studying the impact of financial crises on SME performance. Results have been mixed. Some studies (Moscarini and Postel-Vinay, 2012) provide support for the so-called flexible view, indicating a relative growth advantage of small firms compared to large firms during the crisis. Others find evidence for the fragile view, with small businesses being identified as more vulnerable in crisis times (Kolasa et al. (2010), Ferrando et al. (2014), Bartz and Winkler (2016)).

[3] Indeed, it is the theory of change basically any financial sector involvement by the public sector is built upon. Another prominent example where this theory of change is made use of is microfinance (Banerjee et. al. 2015).

Figure 1: The Theory of Change of SME funding by International Financial Institutions



We are unable to differentiate between the above mentioned impact channels as the available data does not allow us to compare recipients of loans allocated to EIB funding with a banks other clients. Such a comparison is needed for testing the impact of TOFA separately from the impact of IFI funding as such. However, it can be assumed that both channels gain importance in crisis compared to non-crisis times. In crisis times, funding conditions deteriorate in terms of price and quantity, making it more attractive for banks to tap IFI funding in order to minimize funding costs and to see good clients through difficult times.

Until recently, the validity of this theory of change was seen as given if project evaluations, regularly conducted by IFIs (see for example Feeny and Vuong (2017)) indicate that SMEs receiving funding from IFIs show an increase in output and employment, i.e. meet the stipulated goals of the project (a credit line to SMEs). However, over the last decade it has been increasingly argued that this is not enough. The theory of change is proven only if compared to a suitable counterfactual: the impact of IFI funding is properly assessed only if the 'treated' firms do better than similar SMEs not receiving EIB funding, and this 'doing better' is caused by the treatment.

3 Empirical approach

The challenge of impact assessments is that the counterfactual cannot be observed. 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. In other words, we are after the *average treatment effect on the treated (ATT)*. However, we do not know how an SME would have developed in terms of the outcome variables if it had not received an EIB loan compared to the observable development with an EIB loan.

It can be shown that under certain assumptions Randomized Control Trials (RCTs) are able to answer the impact question By randomizing firms receiving and not receiving an EIB loan the firms which are not treated show on average the same characteristics as those which are treated. Thus, there is no selection bias into treatment, i.e. treated firms are on average in no way different from non-treated firms. This allows the researcher to take the outcome variables of the non-treated firms as evidence of the counterfactual and to measure impact by comparing the change in outcome variables of treated with the change in outcome variables of the non-treated firms.

However, the RCT methodology cannot be applied to SME credit lines as it is basically impossible to randomize among firms. Many firms should not get a loan due to a lack of creditworthiness. Indeed, it is one of the key functions of financial institutions to select borrowers, i.e. to act in a non-random way with regard to potential borrowers (Bodie and Merton, 1998). Thus, there is a selection bias problem, and it can exist in various forms. For example, the banks on-lending IFI funds might select the best and most promising companies only, as they aim to avoid the reputation risk via the IFI of not showing good results in terms of outcomes the IFI cares for. Alternatively, the selection bias might lead to a selection of risky and low-growth businesses as other firms get access to funding via traditional channels. In both cases, the performance of the non-treated firms in terms of the outcome variables does not represent the values the treated firms would have achieved if they had not been treated.

Propensity Score Matching (PSM) addresses the selection bias the treatment group is subject to by creating a control group among non-treated firms which at times of treatment are identical to treated firms with respect to observable characteristics[4]. Thus, after controlling for observable characteristics, receiving an IFI loan should be as good as random, i.e. should meet the conditional independence assumption (CIA) which requires that covariates (like firm characteristics) that may impact the probability of receiving an IFI loan can be observed and that these are the basis for the selection into treatment.

Besides CIA, there has to be a positive probability of belonging to the IFI loan receiving firms (the treatment group) as well as to the firms that do not receive an IFI loan, i.e. receiving an IFI loan is not perfectly predictable ex-ante (common support condition (CSC)). In other words, there is a sufficient overlap in the characteristics of firms receiving IFI funding and those that do not in order to identify adequate matches (i.e. otherwise comparable firms). If these assumptions

[4]The PSM methodology goes back to Rubin (1974) and Rosenbaum and Rubin (1983). An introduction is provided by Caliendo and Kopeinig (2005).

are fulfilled it is possible to create out of the group of firms not receiving an IFI loan a control group representing an unbiased counterfactual for the firms receiving an IFI loan.

We complement the PSM with estimating the effect of treatment using a difference-in-differences (DID) estimator. 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 confounderss that a) are not perfectly correlated with observables and b) are important for testing the theory of change.

The DID estimator allows us to control for such unobserved confounders, as long as they remain constant over time. Furthermore, the DID technique relies on the assumption that in absence of the treatment, the average outcomes for treated and controls would have followed parallel trends over time. The parallel trends assumption can be ensured by the appropriate specification of the propensity score model, and can be tested.

The combination of PSM and DID has been used before in the literature of impact assessments of SME credit lines. Combining a propensity score matching approach with difference-in-difference estimations Bah et al. (2011) find that US-AIDs technical and financial assistance for Macedonian SMEs raised employment growth rates in the analysed 58 assisted firms (with 764 firms in the control group) 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 propensity score matching 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 Hungarys Funding for Growth programme on the performance of Hungarian SMEs during the crisis. Using a modified difference-in-difference framework they find that the program succeeded in generating extra investment in the SME sector that would not have taken place otherwise. Banai et al. (2017) investigate the impact of EU-funded direct subsidies to SMEs in Hungary using propensity score matching and fixed effects panel regression, and find a significant positive impact on the number of employees, sales revenue and gross value added. Finally, and closest to our approach, Asdrubali and Signore (2015) show that SMEs in the Central and South Eastern Europe (CESEE) region which received funding guaranteed by the EU SME Guaranty Facility mainly between 2005 and 2007 recorded an increase in the number of employees and in sales compared to a respective control group of SMEs, with the largest impact being observed for micro and young SMEs. Their results are based on observations of 2,923 firms (treatment and control group).

We contribute to this literature by assessing the impact of EIB funding to SMEs covering a substantially larger sample of beneficiaries over a substantially longer observation period that includes the financial crisis, allowing us to test for the impact of the crisis on the impact of EIB funding.

There is a potential unobserved, time-varying confounder that the empirical approach described above may not fully account for. This issue is not unique to our study: it is a feature that is also present in most of the papers cited above. Concretely, by construction, treated firms exhibited credit demand at the time of the treatment, whereas among the firms in the control group, some firms may not have credit demand at that time. For example, some firms might lacked of a profitable investment opportunity. Thus, the identification strategy cannot account for this type of unobserved heterogeneity as we do not know which of the control firms have a demand for credit and when. Furthermore, credit demand is not the only relevant unobservable we are unable to control for; we also do not know whether firms in the control group with credit demand got a loan or not, i.e. to what degree firms in the control group are credit constrained firms and when.

In general, however, we believe that the observables we make use of in the PSM show a strong correlation with these unobservables suggesting that the difference-in-difference analysis provides us with a proper assessment of the impact of EIB funding.

4 Data

4.1 EIB Data

EIB funding products targeting SMEs typically take the form of a Multiple Beneficiary Intermediated Loan (MBIL). With MBILs, EIB provides a loan to a financial intermediary. The intermediary is then required to on-lend the amount to smaller-scale projects and investments, promoted by multiple beneficiaries such as SMEs, or possibly mid-caps. Potential financial intermediaries include commercial banks, leasing companies and other financial institutions, and in some cases public entities such as national promotional banks.

MBILs target improved access to finance and improved financing conditions to SMEs and possibly mid-caps. As such, they contribute to the EIB public policy goal of supporting SME and midcap finance. Based on specific eligibility criteria for final beneficiaries and underlying projects, MBIL operations can also contribute to other EIB public policy goals and objectives (e.g. innovation and skills, environment, infrastructure, climate action, youth employment, agriculture). Projects eligible for MBILs can include investment in tangible and intangible assets, including purchase, leasing or renovation of assets, working capital, etc.

During an agreed allocation period, which is typically 18 or 24 months, the financial intermediary is required to allocate the EIB loan amount to specific sub-loans to eligible SMEs. Data on allocation is reported back to the EIB. The reports include the names of the beneficiaries, the size of the loan and further information on the companies.

EIB funding provides financial advantages to the intermediary financial institutions which can take the form of lower financing costs, longer maturity etc. In exchange, the financial intermediary is contractually required to transfer part of the financial advantage to the final beneficiaries. The standard requirement is to transfer one third of the EIB financial benefit in the form of lower financing costs. Alternatively, the EIB financial advantage can be transferred through longer tenors of sub-loans and/or a one-off payment to final beneficiaries. In addition, the financial institutions are usually required to provide additional, complementary lending to SMEs so that their total lending to SMEs is at least the double of the EIBs participation.

The EIB allocation tables provide the data on the firms receiving EIB funding, the intermediaries involved in lending, the characteristics of the contract with the intermediaries and the characteristics of the final loans. Firms are located in the following CESEE countries: Bulgaria, Croatia, Czech Republic, Hungary, Poland, Romania, Slovakia and Slovenia.

The tables list in total 142.263 allocations to 103.735 SMEs (i.e. beneficiaries) between 2008 and 2014. The bulk of these allocations, 97.205 in total (68%), are allocated to firms in Poland. The total volume of funding amounts to 11.3 billion EUR. As the average funding of Polish firms is substantially below the CESEE average, the share of the amount allocated to Polish SMEs is 32%, followed by the shares of firms in the Czech Republic and Hungary (see figure 2). The median amount allocated to firms is 19.764 EUR, mainly driven by Poland, where the median allocated amount is around 17.000 EUR. The median allocated amount in the remaining countries is substantially higher. The median beneficiary employs 9 employees. Firms received these funds from 126 intermediaries based on 210 contracts between the EIB and local intermediaries.

The number of allocations grew steadily over the years, from 2.299 in 2008 to 40.243 in 2014, which resulted in an increase in the annual allocated amount from 0.5 billion EUR in 2008 to roughly 2.4 billion EUR in 2014.

Some beneficiaries have received funding through more than one intermediary and more than one installment per year. We define treatment as the first installment of a loan to a beneficiary through any intermediary under any contract between the EIB and an intermediary. According to this definition, treatment in a certain year to a certain beneficiary can cover several allocations over several

Figure 2: (i.) Number of allocations and (ii) Total loan amount by country

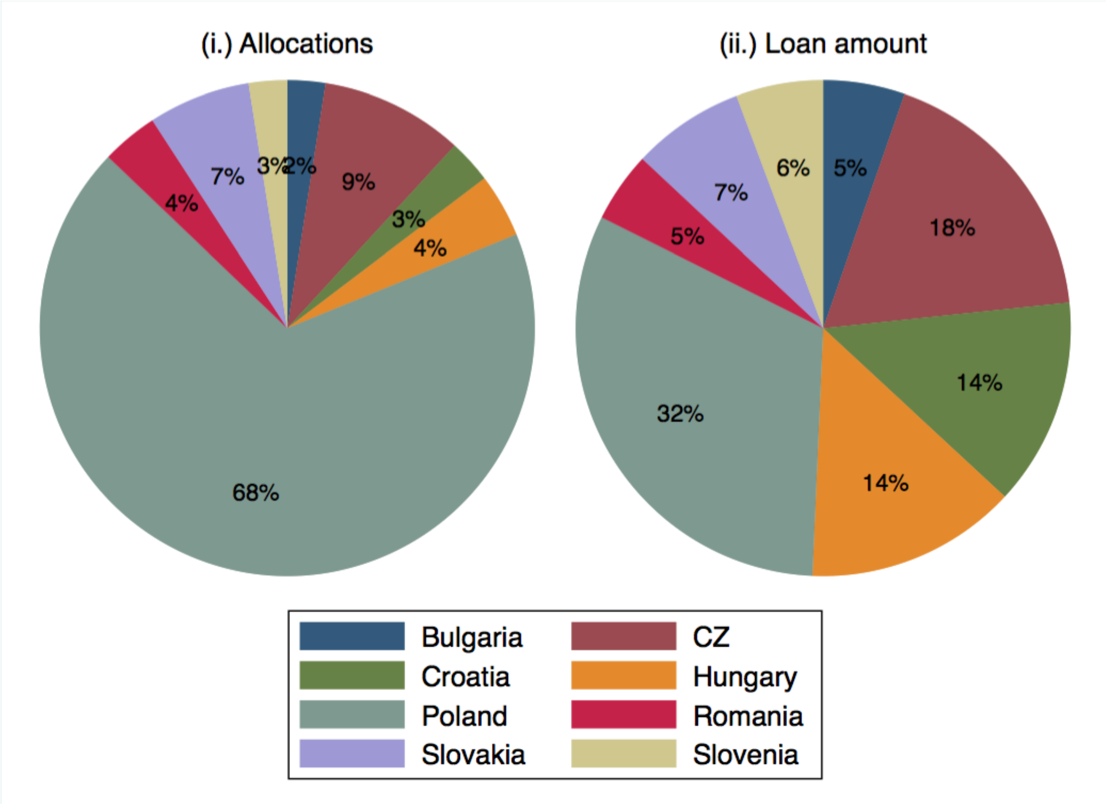
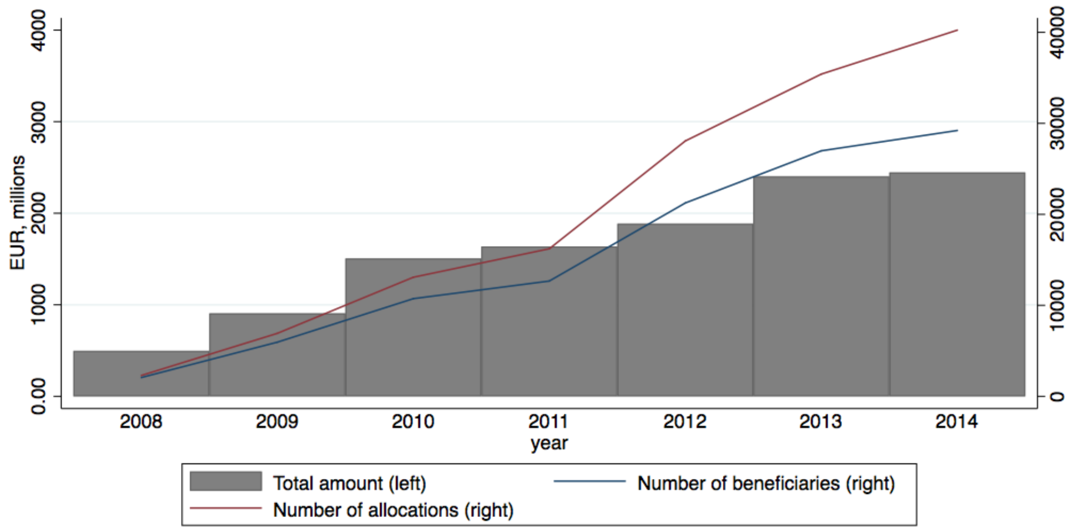


Figure 3: Total loan amount, number of allocations and number beneficiaries by year



years through one or more intermediary. The alternative, namely considering every allocation as a separate treatment would inflate the number of treatments. Furthermore, if allocations to a beneficiary span several years, pre-treatment periods for the later allocations would overlap with the post treatment periods for the earlier allocations. This would violate the condition for propensity score matching where variables explaining selection into treatment should not be affected by treatment.

(Something on SUTVA)

4.2 Bureau van Dijk Orbis database

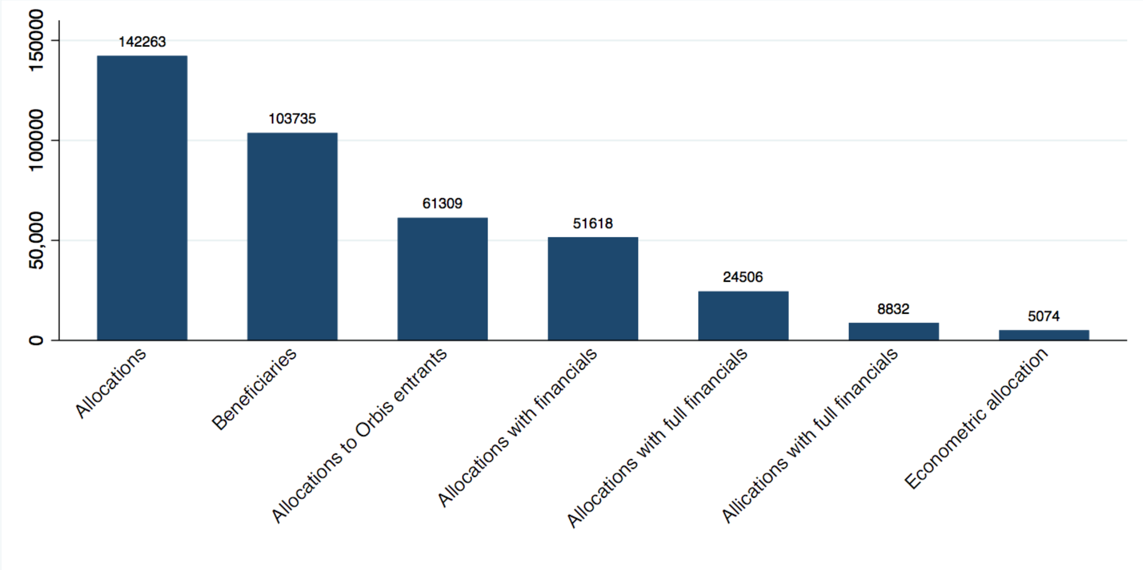
The EIB allocation tables do not contain sufficient information on the firms economic and financial performance. In particular, it does not contain any information on the firms performance after the loan was signed and disbursed. Auxiliary information is therefore necessary to measure and evaluate the SMEs performance after the disbursement of the loan.

For this purpose, we merge the allocation tables with the Orbis database in order to obtain the financial and other firm level data on the beneficiaries. This is a necessary step for measuring the performance of the beneficiaries, but it is also required to create a proper control group of similar companies against which the performance is measured.[5]

Out of 142.263 allocations SMEs from CESEE, 61.309 allocations have an entry in Orbis database. Of these, 51.618 provide some information on the financials and

[5]To assure a high quality of the merge, the process was first conducted for the year 2014 for all countries other than Poland, due to an early merging exercise conducted by the EIB in cooperation with the Bvd on the 2014 data. When the quality was deemed sufficient, the procedure was repeated for the years prior to 2014. The merging was done using the BvD online batch search tool. The search tool provides an option to upload details of up to 1000 beneficiaries at a time. In this merging exercise, the details provided were the beneficiary name and country. Other information would contribute excessive noise into the search procedure and decrease the number of successful matches. The batch search results in a match if the name/country proximity of the allocation tables to an Orbis entry is of a quality labeled A. Furthermore, for non A matches, suggestions of lower proximity are provided. All matches labeled A were kept. For the non-successful merges for all countries other than Poland a manual merging exercise was performed among the provided suggestions. For 2014 out of 10.723 allocations 9.006 were found to have an entry in Orbis. In 6.858 cases the same entry was identified in the EIB/BvD exercise. In two cases the entry in the Orbis database was different to the one identified in the the EIB/BvD exercise. Finally, for 2.150 cases, EIB/BvD exercise did not find a match. Partially this is due to the fact that the EIB/BvD exercise excluded all sole entrepreneurs. For the cases where EIB/BvD did not find a match it cannot be judged on the quality of the merge. The explained procedure for 2014 was deemed appropriate and thus applied for the years from 2008 to 2014.

Figure 4: Creating the final sample of allocations



24.506 have information for total assets, turnover, the current ratio and net income in the year of allocation. However, only 8.832 have that information three years prior and three years after the year of the allocation. Finally, when redefining an allocation (treatment) to satisfy prerequisites of our methodology, our final sample is further diminished to 5.074 observations.

The attrition of data is non-negligible, furthermore we cannot assume that data is missing completely at random (MCAR). Indeed, when considering observable categorical variables such as country, year, employment and industry classification, data attrition is not balanced across the categories defined by them. As a consequence, treatment effects calculated based our final sample can be considered as *sample average treatment effects on the treated* (SATT), which cannot necessarily be generalised as *population average treatment effects on the treated* (PATT).

We attempt to partially account for the missing data bias using three different techniques as part our robustness checks (see Appendix A, Section 1). First, we use inverse probability weights - a technique widely used to correct for survey non-response - to approximate the statistical properties of the original population with respect to some observed variables, and re-estimate our model on a weighted data set. Second, we re-calculate our results on a sub-sample consisting on a single country, Romania, where the missing data problem is the least prevalent. Finally, we again re-estimate the the treatment effects, this time by controlling for country, cohort and country-cohort fixed effects not only at the propensity score estimation, but also in the estimation of the impact of EIB lending.

The key results do not change using these alternative specifications, suggesting that the missing data do not substantially affect the key conclusions of the analysis.

5 Empirical framework

Assessing the impact of EIB funding on SME performance demands an econometric approach to establish a causal relationship between EIB allocations and the performance of SME beneficiaries following an allocation. In establishing a causal relationship we resort to the Rubin's causal model. In doing so we have to overcome the fundamental problem of causal inference, that is that the outcome for the firms which have received funding in the case in which they have would not have received it, is unobservable. Due to non-randomness in the allocation process, any firm which has not received EIB funding does not serve as a good substitute for the unobserved. Firms which receive EIB funding may differ from other firms in characteristics which correlate with their performance after receiving an allocation. In absence of a natural experiment setting and since random allocation of funds to asses the impact of funding on performance is unfeasible, we resort to the established methodology of propensity score matching to obtain the counterfactuals. These are firms which, if certain assumptions, are met, serve as observations of the firms which have received funding as if they had not. The most important assumption is that of the conditional independence, which in our case states that conditioning on observable characteristics, the assignment of an allocation to a firm is "as good as random".

Upon obtaining the counterfactuals we perform difference in difference estimation of the causal effect of EIB funding on SME performance. Difference in difference estimation compares the difference in conditional means of performance after receiving an allocation and before of the firms which have received an allocation and those which serve as counterfactuals, thus providing an estimate for the causal effect of EIB funding on SME performance.

5.1 Sampling and stratification

Based on the merging process just explained, we construct a sample of treated firms which have received EIB funding. Since several beneficiaries have received funding through more than one intermediary and more than one installment per year, we redefine an allocation as a loan or an installment of a loan to a beneficiary through a single contract between the EIB and an intermediary. Moreover, we define treatment as the first installment of a loan to a beneficiary through any intermediary under any contract between the EIB and an intermediary.

We continue by constructing a pool of potential counterfactuals. In doing so we take into consideration the composition of the pool of treated with regard to country, year of allocation, size and industry. Accordingly, we define several strata across these dimensions. To keep granularity at a reasonable level, we define size groups according to the number of employees and industry groups according to their primary NACE code. Thus, in total we have

- 8 countries: Bulgaria, Croatia, Czech Republic, Hungary, Poland, Romania, Slovakia, Slovenia,
- 7 years: from 2008 to 2014,
- 5 size groups: 1 employee, from 2 to 10 employees, from 10 to 50 employees, from 51 to 250 employees, from 251 to 500 employees,
- 6 industry groups.

This adds up to a total of 1680 strata. To ensure that all the strata are represented we draw a random sample of 10 firms from Orbis financials database for each strata. This also assures that after the matching procedure each treated firm has a sufficient probability to have a counterfactual from its own strata. A precondition for a firm to be drawn into a sample of potential counterfactuals is that it has not received funding and that it has data on key financials for seven consecutive years. The financials data on every potential counterfactual is centered around a year which also defines its cohort. The sampling procedure assures that firms which have data for more than seven consecutive years do not appear in the sample as potential counterfactuals more than once.

5.2 Propensity score estimation

In the propensity score model we pool all cohorts, countries, size and industry groups together. This implies that the data is collapsed in a way that ensures that every treatment, as defined in the previous section, is considered period $t = 0$. This implies that a total of 5074 treated firms, i.e. firms which have received EIB funding, are centered around their treatment year (which defines their cohort), and that all potential counterfactuals are centered around the year which defines their cohort. To assure the condition that variables that explain selection into treatment are not affected by the treatment, we estimate the model on pretreatment data. Thus, we compute three pretreatment years averages for all key financials which are to be included in explaining the selection into treatment.

Following the literature on credit scoring models[6], the following end of year financial and business data are obtained from Orbis database: the number of employees, total assets, fixed total assets, tangible fixed assets, intangible fixed assets, current assets, r&d expenditures, total operating revenues, total export revenues, ebitda, net income, the solvency ratio, the current ratio and the liquidity ratio. For variables measured in levels, the growth rates and some relevant ratios are computed. Among the latter are the share of intangible fixed assets in total fixed assets, the ebitda margin, computed as earnings before interest, tax, depreciation

[6] See Volk (2014)

and amortization over revenues and return on assets, measured as net income over total assets. Furthermore, all variables expressed in Euro amounts, are adjusted for cross country price levels, exchange rate movements and inflation[7].

The propensity score model is a probit model explaining selection into treatment, i.e. obtaining EIB funding, using firm financial and demographic data. We use the following set of financial characteristics to explain the selection process: size, funding structure, liquidity, revenue generation, profitability, innovativeness and growth. For each of the characteristics at least one variable is used. If adding additional ratios or variables to a group with a particular significant information raises the predictive power of the model, the variable is kept. Moreover, we control for cohort, country, size, industry and cohort-country specific effects. Higher order terms are included if they prove to be statistically significant and add to the predictive power of the model.

Table 1: Propensity score estimation

	(1) TREATED		
TREATED			
REAL_REV	-9.63E-10	EBITDA_R	0.0143*
	(5.58E-10)		(0.0013
HTTPS://WWW.OVERLEAF.COM/PROJECT/5BED66AB21F7045434F19AB6 REAL_EBITDA	6.02E-08***	SQ_EBITDA_R	-0.00005
	(1.11E-08)		(0.00003
SQ_REAL_EBITDA	-1.93E-15***	CUB_EBITDA_R	-0.0000013
	(3.93E-16)		(0.000000
CUB_REAL_EBITDA	1.59E-24***	EMPLOY_GR	0.116*
	(3.28E-25)		(0.025
S_R	0.00681***	SQ_EMPLOY_GR	-0.0101
	(0.000769)		(0.0028
SQ_S_R	-0.0000639***	CUB_EMPLOY_GR	0.0000015
	(0.0000106)		(0.000000
CUB_S_R	-0.00000103***	D_EBITDA_R	0.00784
	(0.000000155)		(0.0017
C_R	-0.0167***	D_C_R	-0.0536
	(0.00446)		(0.0073
SQ_D_EBITDA_R	-0.0000878*	CUB_D_EBITDA_R	-0.000001
	(0.0000366)		(0.000000
N	28679		

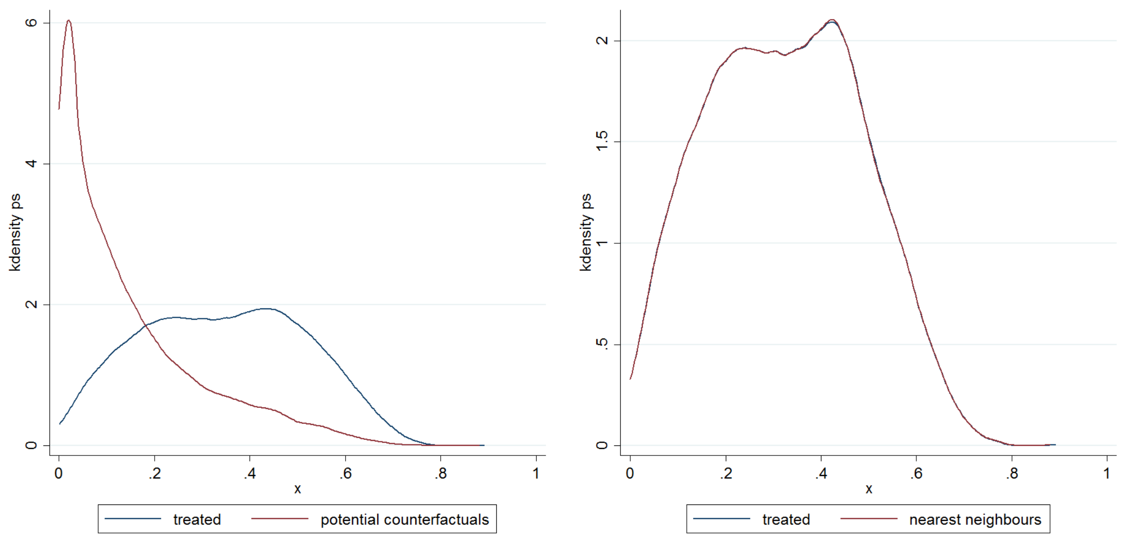
Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[7] Every Euro denominated data point was converted into local currency, adjusted by local currency gdp deflator to 2010 local prices and then converted to 2010 Euros. To adjust for the differences across countries these amounts were then multiplied by the 2010 cross country price index.

Table 1 presents the results of the propensity score estimation. The estimated model provides us with a propensity score that represents the estimated probabilities of being treated conditional on observed characteristics of firms in the sample. To obtain the list of counterfactuals we need to pair every treated firm with a counterfactual. We do so using the nearest neighbour technique.[8] Each firm can serve as a counterfactual for only one treated firm. If two treated firms share the same nearest neighbour, we keep that nearest neighbour for the firm with the closer propensity score and find the next nearest neighbour excluding the firm already used.

Figure 5 provides an illustration of the success of the matching process. The left panel plots the distribution of the estimated propensity score of the treated and the non-treated. The model is able to discriminate between the two groups in the sample with the non-treated evidently more skewed towards zero. The right panel plots the distribution of the estimated propensity score of the treated and their nearest neighbours, the counterfactuals. The overlaying graphs provide evidence that the estimated propensity scores are balanced across the two groups.

Figure 5: Distribution of the estimated propensity score for the: (i.) treated and non treated and (ii.) treated and counterfactuals



5.3 Difference in difference estimation of the causal effects

We now test if the common trend condition for pretreatment outcome variables holds, i.e. whether in the pre-treatment period those firms receiving EIB funding

[8]For the benefits and details of nearest neighbour matching see Caliendo and Kopeinig (2005)

behave in a similar way as the chosen counterfactuals. It is an important test of the success of our matching strategy before applying the difference in difference estimation.

A violation of the common trend assumption would indicate potential unobserved characteristics, which influence the selection into treatment (i.e. successfully applying for an EIB-funded loan), and which are not taken into account in our propensity score model. In this case divergences in outcome variables after treatment could not be interpreted as the treatment effect, i.e. as caused by EIB funding, as they would differ already before treatment with respect to those outcome variables.

$$y_{i,t} = \beta_0 + \beta_1 trend_{i,t} \beta_2 treat_i + \beta_3 trend_{i,t} * treat_i + \epsilon_{i,t} \tag{1}$$

The difference between the trend of the treated and the non-treated is implied by the coefficient of the interaction term of the treated and the trend, β_3. Tables **??** list provides the result of the test for the list of variables where the common trend is confirmed.

Table 2: Common trend tests: log levels

	(1) log(EBITDA)	(2) log(revenues)	(3) log(employment)
treated	0.378***	0.474***	0.144*
	(0.0592)	(0.0553)	(0.0469)
trend	0.0148	0.0314	0.0567***
	(0.0213)	(0.0195)	(0.0160)
treat˙trend	0.0176	0.0415	0.0000794
	(0.0277)	(0.0261)	(0.0220)
_cons	12.12***	14.36***	2.921***
	(0.0458)	(0.0416)	(0.0343)
N	25015	27835	27683
R^2	0.009	0.013	0.003
adj. R^2	0.009	0.013	0.003

Standard errors in parentheses

* $p < 0.5$, * $p < 0.01$, *** $p < 0.001$

Estimation of the average treatment effect on the treated (ATT) relies on the difference in difference estimation. We estimate the following model.

$$y_{i,t} = \beta_0 + \gamma post_t + \delta treat_i + \tau(post_t * treat_i) + \eta Controls_{i,t} + \epsilon_{i,t} \tag{2}$$

where $y_{i,t}$ denotes the outcome variable of interest. Thus with this model we test whether firms receiving EIB funding ($treat_i$) on average behave differently in post treatment periods ($post_t$) than their respective counterfactuals with regards to asset growth, employment growth, liquidity, solvency and profitability. Thus, the $post_t$, a dummy variable which takes the value 1 if period t is a post-treatment period, $treat_i$, a dummy variable which takes the value 1 if firm i is a treated

Table 3: Common trend tests: performance ratios

	(1)	(2)	(3)
	ebitda ratio	current ratio	solvency ratio
treated	0.456	0.0150	-1.169
	(0.454)	(0.102)	(0.883)
trend	-0.399*	-0.0318	1.288***
	(0.155)	(0.0380)	(0.294)
treat˙trend	0.272	0.0158	-0.399
	(0.214)	(0.0533)	(0.406)
_cons	11.22***	1.902***	36.56***
	(0.331)	(0.0720)	(0.643)
N	26978	27895	27735
R^2	0.000	0.000	0.001
adj. R^2	0.000	-0.000	0.001

Standard errors in parentheses

* $p < 0.5$, * $p < 0.01$, *** $p < 0.001$

firm (i.e. received EIB funding) are included as separate variables. Moreover, we employ controls in the form of strata dimensions specific dummy variables with the interaction terms. Some size groups, countries, cohorts and industries are overrepresented in the final sample. As long as within those groups the probability of entering the final sample is exogenous to the outcome variable, controlling for strata specific effects assures no bias due to attrition of the data[9]

The coefficient of interest in equation 2 is the τ, which measures the difference between the treated and non-treated in terms of the outcome variable between the pretreatment and post-treatment periods, the ATT. The coefficient τ gives us the average effect across all post-treatment periods.

To disentangle the effect between the three post-treatment periods separately we define three post-treatment dummies, $post\_1_t$,$post\_2_t$ and $post\_3_t$, which take value 1 if the period t is 1,2 or 3 years after treatment. Thus we transform equation 2 into:

$$\begin{aligned} y_{i,t} = \beta_0 &+ \gamma_1 post\_1_t + \gamma_2 post\_2_t + \gamma_3 post\_3_t + \delta treat_i \\ &+ \tau_1(post\_1_t * treat_i) + \tau_2(post\_2_t * treat_i) + \tau_3(post\_3_t * treat_i) \\ &+ \eta Controls_{i,t} + \epsilon_{i,t} \end{aligned} \tag{3}$$

where the interaction between $post\_1_t$ to $post\_3_t$ with $treat_i$ inform about the direction and the significance of treatment effects in the individual years.

[9]For a more detailed treatise of addressing biases stemming from sampling see Solon et al. (2013).

6 Results

Tables 4 and 5 present the results of the estimations of models 2 and 3. For every outcome variable, both models are estimated. In brief, our results indicate a significant and positive effect of EIB funding on profits, revenues, employment and profitability and a significant negative effect of EIB funding on liquidity and solvency.

Table 4: Regression results table: log levels

	(1)	(2)	(3)	(4)	(5)	(6)
	log(EBITDA)		log(revenues)		log(employment)	
treated	0.367***	0.367***	0.414***	0.414***	0.150***	0.150***
	(0.0195)	(0.0195)	(0.0183)	(0.0183)	(0.0155)	(0.0155)
post	0.0149		-0.0700***		0.0225	
	(0.0234)		(0.0218)		(0.0174)	
treat_post	0.0753**		0.137***		0.110***	
	(0.0301)		(0.0287)		(0.0236)	
post_1		-0.0185		-0.0278		0.0391
		(0.0342)		(0.0315)		(0.0254)
post_2		0.0318		-0.0670**		0.0308
		(0.0347)		(0.0323)		(0.0256)
post_3		0.0322		-0.116***		-0.00275
		(0.0346)		(0.0334)		(0.0260)
treat_post_1		0.0703		0.102**		0.0788**
		(0.0439)		(0.0413)		(0.0343)
treat_post_2		0.0652		0.136***		0.112***
		(0.0443)		(0.0424)		(0.0345)
treat_post_3		0.0905**		0.175***		0.139***
		(0.0447)		(0.0437)		(0.0349)
_cons	12.09***	12.09***	14.30***	14.30***	2.829***	2.829***
	(0.0150)	(0.0150)	(0.0137)	(0.0137)	(0.0113)	(0.0113)
N	57430	57430	64978	64978	64558	64558
R^2	0.013	0.013	0.017	0.017	0.005	0.005
adj. R^2	0.013	0.013	0.017	0.017	0.005	0.005

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

There is a significant positive overall effect on both profits, measured by EBITDA, and revenues (see columns (1) and (3) in table 4). Whereas the positive effect on revenues is significant over all separate post-treatment year (column (4)), the positive effect on profits is driven by the positive effect in the third post-treatment year (column (2)).

There is a significant positive effect on employment, escalating over the post-treatment years (see columns (5) and (6) in the table 4). Overall the EIB funding increases employment of SMEs which have received funding by 11%, relative to those SMEs which have not received EIB funding.

The positive pre-treatment trend suggests that firms in our sample grow in employment in the pre-treatment period. Upon receiving a loan, the positive effect on employment assures the possibility of adding new employees to the firm thereby continuing their growth. However, had these firms not received EIB funding, they would need to resort to scaling down their operation and decreasing the number of employees.

Table 5: Regression results table: performance ratios

	(1)	(2)	(3)	(4)	(5)	(6)
	EBITDA ratio		current ratio		solvency ratio	
treated	0.0509	0.0509	-0.0244	-0.0244	-0.927***	-0.927***
	(0.150)	(0.150)	(0.0340)	(0.0340)	(0.291)	(0.291)
post	-1.615***		0.466***		3.606***	
	(0.183)		(0.0454)		(0.355)	
treat_post	0.958***		-0.363***		-1.958***	
	(0.245)		(0.0594)		(0.476)	
post_1		-1.762***		0.272***		2.061***
		(0.265)		(0.0661)		(0.523)
post_2		-1.542***		0.476***		3.577***
		(0.275)		(0.0697)		(0.538)
post_3		-1.537***		0.650***		5.205***
		(0.290)		(0.0780)		(0.547)
treat_post_1		1.145***		-0.236***		-1.888***
		(0.352)		(0.0856)		(0.697)
treat_post_2		0.713*		-0.394***		-1.884***
		(0.366)		(0.0908)		(0.713)
treat_post_3		1.013***		-0.459***		-2.111***
		(0.384)		(0.0958)		(0.732)
_cons	11.72***	11.72***	1.965***	1.965***	34.32***	34.32***
	(0.110)	(0.110)	(0.0233)	(0.0233)	(0.212)	(0.212)
N	62824	62824	65175	65175	64213	64213
R^2	0.002	0.002	0.003	0.003	0.003	0.004
adj. R^2	0.002	0.002	0.003	0.003	0.003	0.004

Standard errors in parentheses

* $p < 0.1$, * $p < 0.05$, *** $p < 0.01$

Although both profits and revenues increase due to EIB funding, there are still efficiency gains, evident in the positive effect of the EBITDA to revenues ratio. This suggests that firms access to funds not only increases their capacity to generate revenues sales but also contributes to reducing the average cost.

The negative effect of EIB funding on liquidity and solvency ratios of the respective beneficiaries compared to the group of counterfactuals, seen in columns (3) to (6) in table 5 is largely driven by accounting mechanics. By definition funding from EIB lowers the liquidity position of the receiving firms compared to firms that do not receive funding if they fund long term assets with short term debt. Similarly, debt financing by EIB increases the level of debt relative to equity financing which has a negative impact on the solvency ratio compared to firms that do not receive such funding.[10]

7 Difference in difference in difference estimation of the causal effect of the crisis on the effectiveness of EIB funding

The impact analysis conducted up to now did not account for the fact that during the observation period the economies in the analysis were hit by financial crises and that EIB lending - at least partly - took place during the crisis.

There are two opposite possible ways in which a crisis can affect the impact of EIB lending. On the one hand, a crisis gives firms which receive EIB lending a larger advantage compared to the counterfactuals as the crisis is likely to place stronger financing constraints on all SMEs. On the other hand, a crisis also dampens general demand in the economy making it harder for firms to reap the benefits of an easing in credit constraints provided by EIB funding. In this section we aim to estimate which of the two effects prevails.

It is important to emphasise that the methodology aims at obtaining the *causal* effect of the crisis on the impact of EIB funding. The emphasis is crucial also from a policy perspective, as our results do not answer the question whether and to what extent EIB funding during the crisis has on average a different effect than EIB funding in a normal period. Answering the latter question would require to control for the difference between the firms receiving EIB funding in crisis times and firms receiving in normal times.

This we are unable to do as the observation period starts in 2008 only, i.e. the year of the Lehman brothers default. As many CEE countries were also hit by the euro crisis the post-2011 years do not provide a basis either for compiling a sample of firms receiving EIB funding in normal times and comparing the characteristics of these firms with the characteristics of beneficiaries in a crisis period.

In order to control for the composition and characteristics of firms applying for assisted loans and estimate the causal effect of the crisis on the effectiveness of EIB lending, we apply a similar methodology as before but add a third difference term. However, to avoid convoluted matching techniques, we assure equal characteristics

[10] An alternative, negative, explanation of the results on liquidity would demand that the counterfactual firms are able to obtain market funding and that this funding is of longer maturity. This would however go against the result that the solvency of the treated decreases relative to the counterfactuals.

of firms by limiting our analysis on firms receiving an allocation in a crisis period. We then contrast the outcome of the firms for which the post treatment periods are non crisis periods to the outcome of the firms for which the crisis continued.

We follow Lo Duca et al. in defining the crisis years in the countries under review[11] and estimate the following model.

$$y_{i,t} = \beta_0 + \beta_1 treat_{i,t} + \beta_2 post_{i,t} + \beta_3 crisis_{i,t} + \tau_1 treat_{i,t} post_{i,t} \quad (4)$$
$$+ \beta_4 treat_{i,t} crisis_{i,t} + \beta_5 post_{i,t} crisis_{i,t} + \tau_2 treat_{i,t} post_{i,t} crisis_{i,t} + \epsilon_{i,t} \quad (5)$$

where $crisis_{i,t}$ is a dummy variable indicating whether the crisis continued for a beneficiary after receiving an allocation. The coefficient of interest, τ_2, estimates an effect of the crisis on the effectiveness of EIB funding. That is, it measures a difference in the effect of EIB funding (the difference-in-difference coefficients) on the outcome variables between the firms which receive funding during the crisis and for which the crisis continues and the firms which receive funding during the crisis but the loan is followed by non-crisis years.[12]

Table 6 indicates that the economic outcomes differ substantially. While the change in real GDP growth between a crisis year and a continuing crisis year is on average -0.14 p.p., the average change in GDP growth between a crisis year and the first post crisis year is 0,68 p.p.. Furthermore, on average GDP growth for the three years following a non last crisis year is 0.20% while the three year average post crisis growth rate is 1,68%.

Table 6: Crisis and non crisis economic outcomes

	Δ GDP GROWTH	3Y AVERAGE GDP GROWTH
NON CRISIS	0.68	1.68
CRISIS	-0.14	0.20

Source: own calculation based on crisis definition provided by Lo Duca et al. (2017)

[11] Financial crises: Croatia: 2008 to 2012, Hungary: 2009 to 2010, Romania: 2008 to 2010, Slovenia: 2010 to 2014, Bulgaria: 2008 to 2010, Czech Republic: 2008 to 2010, Poland: 2008 to 2009, Slovakia: 2009 to 2010.

[12] As already mentioned, a better setting to control for the composition of firms for the crisis and non crisis cohorts would be the one where the analysis would only focus on the firms which have received funding in normal periods and contrast the outcome for firms which have experienced a crisis in post treatment years against the outcome for firms for which normal times continued. This is due to the fact that one would expect that economic outcomes differ more between the first crisis year and a continuing non-crisis period than they do between the first post crisis year and a continuing crisis period. Our data does not, however, allow us to conduct such an analysis due to a lack of observations where an allocation was made before the crisis and a crisis followed.

Tables 7 and 8 provide the results of the difference in difference in difference estimation of the effect of the crisis on the effectiveness of EIB lending. For every outcome variable of interest a regular difference in difference model is estimated on the narrowed sample, i.e. a sample of firms which fit into either of the two categories, (columns denoted (DD)) and the difference in difference in difference model (denoted (DDD)). The coefficients of interest are those of the triple interaction terms indicating that an allocation is: a) followed by a crisis (crisis), b) the period is a post treatment period (post) and c) that a firm was treated (treat).

Table 7: Regression results table: log levels

	(DD)	(DDD)	(DD)	(DDD)	(DD)	(DDD)
	log(EBITDA)		log(revenues)		log(employment)	
treated	0.459***	0.332***	0.497***	0.357***	0.228***	0.113***
	(0.0271)	(0.0307)	(0.0253)	(0.0293)	(0.0211)	(0.0241)
post	0.0414	0.0214	-0.0634*	-0.0492	0.0221	0.0441
	(0.0332)	(0.0400)	(0.0305)	(0.0370)	(0.0243)	(0.0292)
treat_post	-0.0159	-0.0804	0.101**	0.0356	0.0771**	0.00575
	(0.0421)	(0.0510)	(0.0396)	(0.0489)	(0.0323)	(0.0392)
crisis		-0.0758***		-0.0765***		-0.0679***
		(0.0246)		(0.0230)		(0.0191)
post_crisis		-0.0437		-0.137***		-0.147***
		(0.0535)		(0.0495)		(0.0391)
treat_crisis		0.352***		0.390***		0.319***
		(0.0341)		(0.0324)		(0.0286)
treat_post_crisis		0.160*		0.170**		0.187***
		(0.0756)		(0.0714)		(0.0588)
_cons	12.07***	12.11***	14.29***	14.34***	2.829***	2.871***
	(0.0212)	(0.0260)	(0.0192)	(0.0239)	(0.0157)	(0.0197)
N	29540	29540	33330	33330	33137	33137
R^2	0.016	0.022	0.023	0.029	0.009	0.015
adj. R^2	0.016	0.022	0.023	0.029	0.008	0.015

Standard errors in parentheses

* $p < 0.1$, * $p < 0.05$, *** $p < 0.01$

Columns (DDD) in table 7 indicate that the crisis affects the impact of EIB funding on profits revenues and employment. The increase in a comparative advantage of firms which receive funding due to the crisis prevails over a dampening effect of crisis on the demand. The positive impact of EIB lending is therefore stronger in all dimensions for those firms that faced a prolonged crisis after the loan allocation, relative to those firms that experienced a rapid recovery after taking the loan.

Columns (DDD) in table 8 suggest that there is no effect of the crisis on the impact of EIB funding on profitability and solvency. However, the crisis does intensify the negative effect of EIB finding on liquidity.

Table 8: Regression results table: performance ratios

	(DD)	(DDD)	(DD)	(DDD)	(DD)	(DDD)
	EBITDA ratio		current ratio		solvency ratio	
treated	0.114	0.127	0.0437	0.0959*	1.479***	0.851*
	(0.202)	(0.240)	(0.0443)	(0.0550)	(0.392)	(0.457)
post	-1.557***	-1.389***	0.469***	0.355***	4.311***	4.863***
	(0.254)	(0.303)	(0.0622)	(0.0732)	(0.489)	(0.592)
treat_post	0.412	-0.191	-0.293***	-0.193*	-3.083***	-3.021***
	(0.334)	(0.414)	(0.0822)	(0.105)	(0.649)	(0.811)
crisis		0.0644		-0.0898**		0.192
		(0.188)		(0.0443)		(0.359)
post_crisis		-0.365		0.189		-1.232
		(0.435)		(0.118)		(0.822)
treat_crisis		-0.0515		-0.119*		1.625***
		(0.264)		(0.0642)		(0.524)
treat_post_crisis		1.606***		-0.272*		-0.145
		(0.619)		(0.161)		(1.194)
_cons	11.85***	11.81***	1.903***	1.959***	33.10***	32.98***
	(0.150)	(0.189)	(0.0295)	(0.0426)	(0.291)	(0.366)
N	32163	32163	33424	33424	32977	32977
R^2	0.002	0.003	0.003	0.003	0.003	0.003
adj. R^2	0.002	0.002	0.002	0.003	0.003	0.003

Standard errors in parentheses

* $p < 0.1$, * $p < 0.05$, *** $p < 0.01$

8 Conclusion

In this paper we ask whether, to what extent, through which channels and under which conditions IFI funding provides support to the economic performance of SMEs receiving such funding. We do so by assessing the impact of EIB funding on SME performance in Central and Eastern Europe (CEE) during 2008-2014, a period significantly affected by the financial crisis.

Our results derived from a propensity score matching and difference-in-difference estimation exercise indicate that EIB lending has a positive effect on employment, revenues, profits and profitability. Moreover, EIB funded firms record a decline in liquidity and solvency. However, we believe that this effect is largely driven by accounting mechanics, as firms receiving EIB funds by implication become less liquid and solvent compared to the control group as any investment funded by the EIB loan reduces liquidity and the funding itself raises leverage.

We also find that the positive impact of EIB funding on employment, profitability and revenues is larger when firms face a prolonged crisis after the loan allocation, relative to the case of a rapid subsequent recovery. It appears that the stronger financing constraints experienced in a prolonged crisis render the EIB-supported funding even more useful for the beneficiaries.

Overall, we conclude that EIB lending during the observation period made a difference. Given the general constraints related to the chosen methodology our results provide support to the view that EIB funding supported employment, revenues and profitability of SMEs in CEE countries in a period characterized by financial and economic turmoil.

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A Robustness checks

To confirm the robustness of our key results to alternative model specifications, we carry out a range of checks.

A.1 Dealing with the missing data problem

We showed in section 4.2 that we lost a significant proportion of our initial observations. Figure 4 shows that out of 142263 initial allocations we can only use 5074 observations in the econometric analysis. The reasons for data attrition include unsuccessful matching of company names in the ORBIS dataset, missing data in ORBIS and the exclusion of multiple allocations to the same firm from the sample.

We cannot assume that the data is missing completely at random (MCAR). When grouping the data by observable categorical variables such as country, year, employment and industry classification, the share of missing data is not balanced across these categories. As a consequence, treatment effects calculated based our final sample can be considered as sample average treatment effects on the treated (SATT), which cannot necessarily be generalised as population average treatment effects on the treated (PATT).

To correct for the missing data bias, we use inverse probability weights (IPW) to approximate the statistical properties of the original population with respect to a range of observed variables. We generate

First, we use inverse probability weights - a technique widely used to correct for survey non-response - to approximate the statistical properties of the original population with respect to some observed variables, and re-estimate our model on a weighted data set.

We re-calculate our results on a sub-sample consisting on a single country, Romania, where the missing data problem is the least prevalent.Finally, we again re-estimate the the treatment effects, this time by controlling for country, cohort and country-cohort fixed effects not only at the propensity score estimation, but also in the estimation of the impact of EIB lending.

A.2 Cluster-robust inference

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A.3 Placebo test

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