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the case of EIB lending in Central and Eastern Europe

European Investment Bank
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Authors:

Áron Gereben (European Investment Bank)

Anton Rop (Frankfurt School of Finance and Management)

Matic Petriček (Bank of Slovenia)

Adalbert Winkler (Frankfurt School of Finance and Management)

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economics@eib.org

www.eib.org/economics

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The impact of international financial institutions on small and medium enterprises: the case of EIB lending in Central and Eastern Europe*

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Áron Gereben[†], Anton Rop[‡], Matic Petriček[§], Adalbert Winkler[¶]

November 12, 2019

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 5,223 SMEs in eight countries of Central and Eastern Europe (CEE) during 2008-2014. Our results, derived from propensity score matching and difference-in-difference estimation exercises, indicate that EIB lending has a positive effect on employment, revenues and profitability. This positive effect holds irrespective of the economy entering a prolonged crisis or a seeing a recovery in the years following EIB funding. 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
Key words: International financial institutions, SMEs, impact, financial crisis

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[†]European Investment Bank

[‡]Frankfurt School of Finance and Management

[§]Bank of Slovenia

[¶]Frankfurt School of Finance and Management

1 Introduction

Development and promotional banks are thought to play an important catalytical role in supporting development in specific sectors (Anginer et al., 2011; Griffith-Jones et al., 2017; de la Torre et al., 2017). Small and medium-sized enterprises (SMEs) are important beneficiaries, reflecting the view that they a key driver of growth and employment. At the same time, they 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 engaged in counter-cyclical activities aimed at preventing 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. For an overview focusing on developing countries, see Kersten et al., 2017). 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). By contrast, in several 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 state interventions into the financial sector via development banks still meet substantial scepticism, mainly due to the governance challenges involved (World Bank, 2012), 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) during the global financial crisis and the Eurozone sovereign debt crisis. We do so as SMEs represent one of EIB's five key operational priorities (EIB, 2013) 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).

Concretely, we exploit EIB lending data and blend it with publicly available data on individual SMEs financial and economic performance from the Bureau van Dijk's 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 compare the outcomes of firms located in countries where the crisis continues after firms received EIB funding, with the outcome of EIB-funded firms located in countries where the recovery sets in, relative to non-treated firms.

Our results show that 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 of EIB lending are upheld, i.e. they neither decline nor are they reinforced. An exception is the EBITDA ratio where we find that the impact of EIB lending on the ratio is even larger when firms face a prolonged crisis period compared to a period of recovery. Overall, this suggests that the positive impact of EIB lending on revenues and employment does not depend on post-treatment economic developments in the countries beneficiaries operate in. EIB funded firms record larger revenues and higher employment relative to the control group irrespective of the country remaining in a recessionary environment after treatment or recovering from the financial crisis driven downturn.

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 (Lee et al., 2015). 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, 2018). Factors related to the SMEs themselves also contribute to a 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]

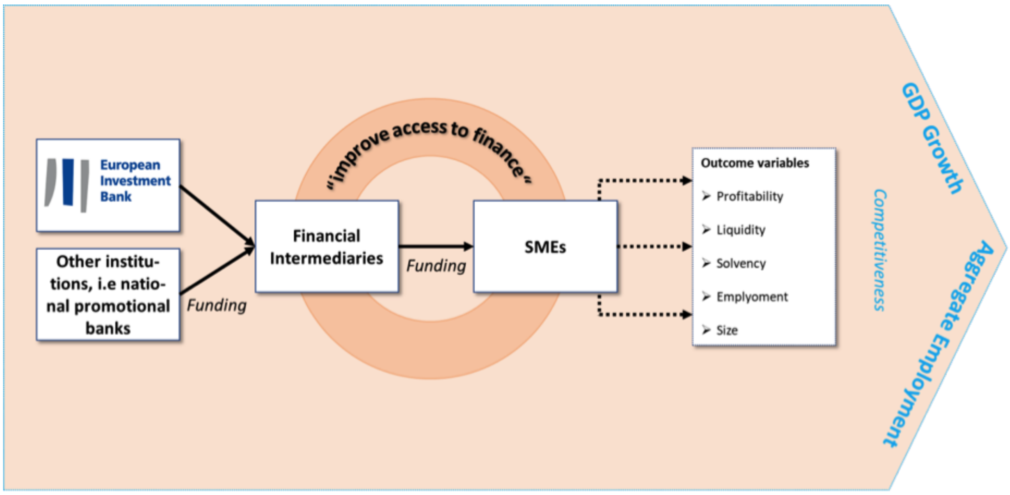
IFIs and promotional banks might facilitate access to credit in two ways:

- 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). For instance, 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. The financial advantage can also materialise in loan components other than the cost of borrowing. For example, in certain cases the IFI-supported loans offer longer maturities than the normally available ones.

[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., 2017; 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 (source: EIB, 2013, own compilation)



- Second, the IFI contribution might consist of alleviating constraints on the intermediary's 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.

We are unable to differentiate between the above mentioned impact channels as the available data does not allow us to compare recipients of loans funded by EIB with recipients of loans funded by other banks' sources. 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, i.e. if the 'treated' firms do better than similar SMEs not receiving EIB funding, and this 'doing better' is caused by the treatment. Our paper aims at exactly this comparison.

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 (Glennerster and Takavarasha, 2013). 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, 1995). Thus, there is a selection bias problem. With regard to onlending of IFI funds the bias might take two forms. First, the on-lending banks 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. Second, the selection bias might lead to a selection of risky and low-growth businesses while other firms receive loans funded via traditional channels, such as deposits or other borrowings. In both cases, the performance of the non-treated firms in terms of the outcome variables might 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.

[4]The PSM methodology goes back to Rubin (1974) and Rosenbaum and Rubin (1983). An introduction is provided by Caliendo and Kopeinig (2008).

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 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 propensity score matching 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 is often used in the policy evaluation literature (see for example Javorcik and Sawada, 2018), and also in particular for impact assessments of SME credit lines. Combining a propensity score matching approach with difference-in-difference estimations Bah et al. (2011) find that USAIDs 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 Hungary's "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). Further work using credit guarantee data can be found in Bertoni et al. (2018) and Bertoni et al. (2019a).

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. They also allow us to test for the impact of the crisis on the impact of EIB funding. Having said this, there is a potential unobserved, time-varying confounder that our empirical approach described above – the combination of PSM and difference-in-differences – may not fully account for. This problem is not unique to our study: it is a feature that is also present in most of the papers cited above. (See Asdrubali and Signore, 2015, Bertoni et al., 2018, Bertoni et al., 2019a or Bertoni et al., 2019b. See also Caliendo et al., 2016 for an explicit discussion of the issue of unobservable confounders in an empirical application.) The issue is the following. By construction, treated firms exhibit credit demand at the time of the treatment. Among the firms in the control group, however, some firms may not have credit demand at that time, because they lack a profitable investment opportunity. Our identification strategy cannot account for this type of unobserved heterogeneity, as we do not know which of the control firms have asked for a loan. As a consequence, in principle, for any difference in outcome variables we measure between the treatment and the control groups, we may not be able to determine the extent this difference is attributable to the effect of an EIB loan, or to a difference in credit demand between the two groups.[5] In line with the literature, however, we argue that the observables we make use of in the PSM show a strong correlation with the (unobservable) determinants of credit demand. This suggests that the difference-in-difference analysis provides us with a proper assessment of the impact of EIB funding. In addition, we will address this issue in more detail as part of the robustness checks.

[5]On a similar note, as already mentioned, the fact that we do not have information on the credit applications of control group firms restricts us to measure the overall effect of EIB loans. We cannot determine the role of the different channels, e.g. whether the impact on the outcome variables manifests itself through better conditions, such as the transfer of financial advantage, or through alleviating credit constraints by providing access to finance to firms that have not received a loan otherwise.

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.

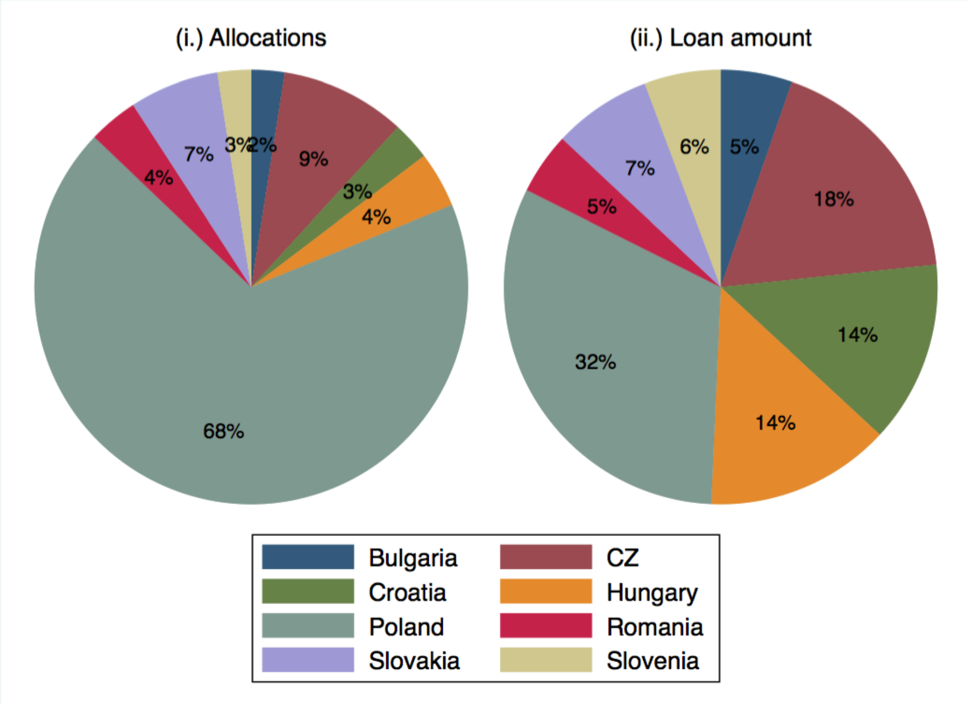
MBILs target improved access to finance and improved financing conditions to SMEs and possibly mid-caps. As such, they contribute to the EIB public policy goal of supporting SME and midcap finance. Based on specific eligibility criteria for final beneficiaries and underlying projects, MBIL operations 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 subloans 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. In this paper we study the impact of EIB funding on firms located in the following CESEE countries: Bulgaria, Croatia, Czech Republic, Hungary, Poland, Romania, Slovakia and Slovenia.

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 EIB's participation.

The tables list in total 142,263 allocations to 103.735 SMEs (i.e. beneficiaries) between 2008 and 2014. 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. The bulk of these allocations, 97,205 in total (68%), are allocated to firms

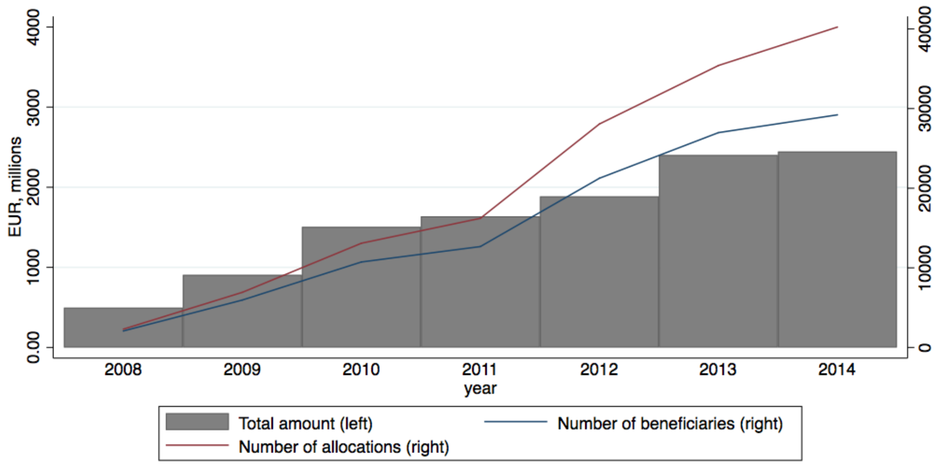
Figure 2: (i.) Number of allocations and (ii) Total loan amount by country



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 funds from 126 intermediaries based on 210 contracts between the EIB and local intermediaries.

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 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.

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



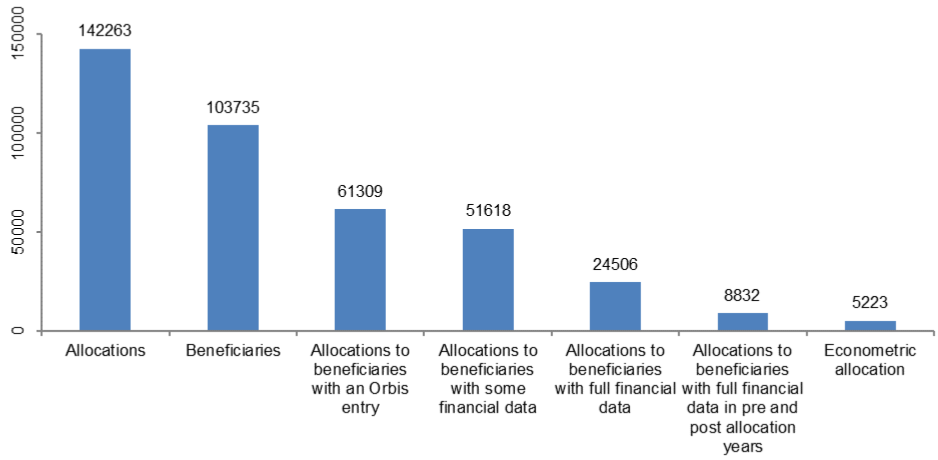
4.2 Bureau van Dijk Orbis database

The EIB allocation tables do not contain sufficient information on the firm's 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 of EIB funded firms is measured.[6]

[6]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

Figure 4: Creating the final sample of allocations



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 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 5,223 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 on 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 partially account for the missing data bias using three different techniques as part our robustness checks. 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.

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.

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, firms which have not received EIB funding do not necessarily serve as a good substitute for the unobserved counterfactual as EIB funded firms 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 a 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. This provides us with 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 5,223 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 pre-treatment data. Thus, we compute three pre-treatment years averages for all key financials which are to be included in explaining the selection into treatment.

Following the literature on credit scoring models (Volk, 2014), 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 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 characteristic 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: Probit model estimation for the propensity score

	Level	Squared	Cubic
Real revenue	-4.53e-09***		
	(4.93e-10)		
Solvency ratio	0.00720***	-0.0000655***	-0.00000111***
	(0.000738)	(0.0000105)	(0.000000152)
Current ratio	-0.00891*		
	(0.00424)		
Change in current ratio	-0.0413***		
	(0.00669)		
EBITDA ratio	0.0176***	-0.0000430	-0.00000225***
	(0.00129)	(0.0000264)	(0.000000413)
Change in EBITDA ratio	0.00728***	-0.000106**	-0.00000128**
	(0.00159)	(0.0000351)	(0.000000400)
Log employment	0.0890***		
	(0.00744)		
Change in employment	0.195***	-0.0187***	0.00000278***
	(0.0253)	(0.00301)	(0.000000448)
Constant	-3.376***		
	(0.212)		
Country FE:	Yes		
Industry FE:	Yes		
Cohort FE:	Yes		
Number of observations:	29695	Pseudo R2:	0.1789

[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.

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

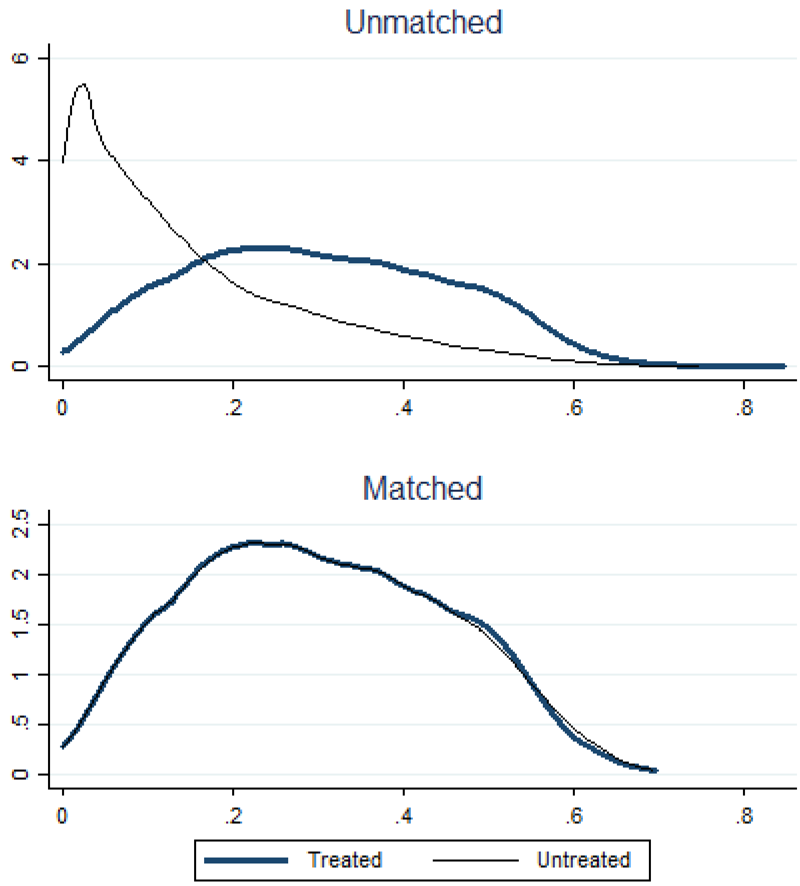


Table 1 presents the results of the propensity score estimation. The estimated model provides us with a propensity score that represents the estimated probabilities of being treated conditional on observed characteristics of firms in the sample. To obtain the list of counterfactuals we need to pair every 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

[8]For the benefits and details of nearest neighbour matching see Caliendo and Kopeinig (2008)

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.

Table 9 and Figure 6 illustrate the outcome of the matching process from the viewpoint of the key observables that enter into the probit model. They confirm that the matching resulted in a sizable reduction in the difference between the treated and the control group. The standardised bias indicator is below +/-5 per cent rule of thumb suggested by Caliendo and Kopeinig (2008) for all variables.

Table 10 displays the key summary statistics for the treated, the potential controls and matched control sample with respect to our final outcome variables that we use in the difference-in-difference framework.

5.3 Difference in difference estimation of the causal effects

An important test of the success of our matching strategy before applying the difference in difference estimation is testing for a common trend of pre-treatment outcome variables, i.e. whether in the pre-treatment period the chosen counterfactuals behave in a similar way as EIB funded firms. A violation of the common trend assumption would indicate that unobserved characteristics, which are not taken into account in our propensity score model, influence the selection into treatment (i.e. successfully applying for an EIB-funded loan). Moreover divergences in outcome variables after treatment could not be interpreted as the treatment effect, i.e. as caused by EIB funding, as they would have been observed already before treatment with respect to those outcome variables.

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

Concretely, we estimate (1), with $y_{i,t}$ representing the outcome variable of interest for firm i at time t.[9] Variable $treat_i$ is a dummy variable that takes the value 1 if firm i received EIB funding and 0 otherwise. We estimate (1) for observations preceding the treatment (i.e. $t < 0$). Variable t represents a linear time trend.

We focus on the coefficient of the interaction term of the treated and the trend, β_3, i.e. in the presence of common trend we expect β_3 to be zero. Tables 2 and 3 provide the results of the test for variables where the common trend is confirmed, i.e. β_3 fails to be significantly different. These variables are the log levels of

[9] In our setup, the time index t represents the time measured in years relative to the year of the loan allocation. It can take values from -3 to +3, and takes 0 in the year of treatment.

EBITDA, revenues and employment (Table 2) as well as the EBITDA, current and solvency ratios (Table 3).

Table 2: Common trends - log variables

	(1) Employment (log)	(2) EBITDA (log)	(3) Revenues (log)
trend	0.0548***	0.00686	0.0363*
	(0.0168)	(0.0203)	(0.0192)
treated	-0.0231	0.291***	0.331***
	(0.0474)	(0.0565)	(0.0539)
trend*treated	0.000915	0.0279	0.0382
	(0.0221)	(0.0264)	(0.0253)
Constant	3.087***	12.21***	14.51***
	(0.0363)	(0.0438)	(0.0413)
Observations	29800	27048	29921
R^2	0.001	0.005	0.006
Adjusted R^2	0.001	0.004	0.006

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

Table 3: Common trends - ratios

	(1) EBITDA ratio	(2) Solvency ratio	(3) Current ratio
trend	-0.00560	1.006***	-0.0431
	(0.146)	(0.272)	(0.0353)
treated	-0.163	-0.128	0.0706
	(0.433)	(0.826)	(0.0946)
trend*treated	-0.00761	-0.0934	0.00417
	(0.203)	(0.380)	(0.0506)
Constant	12.18***	35.59***	1.785***
	(0.314)	(0.596)	(0.0661)
Observations	29051	29850	29993
R^2	0.000	0.001	0.000
Adjusted R^2	-0.000	0.001	0.000

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

We continue by estimating the average treatment effect on the treated ATT with a difference in difference regression and run the following model:

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

where $y_{i,t}$ denotes the outcome variable of interest. 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 firm (i.e. received EIB funding) are included as separate variables.

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

We also aim at disentangling the effect between the three post-treatment periods separately. To this end 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:

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

where the interactions 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.

6 Results

Tables 4 and 5 present the results of the estimations of models 2 and 3. For each outcome variable, both models are estimated. Figure 7 in the Appendix shows visually the pre- and post-treatment developments of the respective variables for control and treatment group. 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.

There is a significant positive effect on both profits, measured by EBITDA, and revenues - see columns (3) and (5) in Table 4. The effect of EIB funding is positive for all post-treatment years for both measures. Whereas the effect on revenues increases over the post-treatment period (column (6)), the effect on profits peaks in the second post-treatment year (column (4)).

We also find a significant positive effect on employment, escalating over the post-treatment years (see columns (1) and (2) in the table 4). Overall the EIB funding increases employment of SMEs which have received funding by 13%, relative to those SMEs which have not received EIB funding.

EIB funding is also associated with a positive impact on efficiency (see columns 1 and 2 in Table 5). The significant positive coefficients of the interaction terms indicate that firms with access to EIB funds do not only increase their capacity to generate revenues but are also able to reduce costs.

We find negative effects of EIB funding on liquidity and solvency ratios of the respective beneficiaries compared to the group of counterfactuals. We believe that

Table 4: Difference-in-differences - log variables

	(1)	(2)	(3)	(4)	(5)	(6)
	employment (log)		EBITDA (log)		revenues (log)	
treated	-0.0165	-0.0165	0.259***	0.259***	0.277***	0.277***
	(0.0156)	(0.0156)	(0.0187)	(0.0187)	(0.0178)	(0.0178)
post	0.00717		-0.00708		-0.0780***	
	(0.0184)		(0.0226)		(0.0217)	
treated × post	0.131***		0.106***		0.155***	
	(0.0238)		(0.0290)		(0.0280)	
post_1		0.0337		-0.0390		-0.0342
		(0.0269)		(0.0330)		(0.0315)
post_2		0.0110		-0.0233		-0.0813**
		(0.0270)		(0.0338)		(0.0321)
post_3		-0.0233		0.0422		-0.119***
		(0.0272)		(0.0339)		(0.0333)
treated × post_1		0.0905***		0.0988**		0.114***
		(0.0346)		(0.0422)		(0.0404)
treated × post_2		0.138***		0.132***		0.161***
		(0.0348)		(0.0429)		(0.0413)
treated × post_3		0.164***		0.0870**		0.190***
		(0.0351)		(0.0433)		(0.0426)
Constant	2.998***	2.998***	12.20***	12.20***	14.45***	14.45***
	(0.0119)	(0.0119)	(0.0144)	(0.0144)	(0.0136)	(0.0136)
Observations	69487	69487	62135	62135	69852	69852
R^2	0.001	0.001	0.008	0.008	0.009	0.009
Adjusted R^2	0.001	0.001	0.008	0.008	0.009	0.009

Standard errors in parentheses

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

Table 5: Difference-in-differences - ratios

	(1)	(2)	(3)	(4)	(5)	(6)
	EBITDA ratio		Solvency ratio		Current ratio	
treated	-0.0569	-0.0569	-0.580**	-0.580**	0.0394	0.0394
	(0.143)	(0.143)	(0.272)	(0.272)	(0.0326)	(0.0326)
post	-1.955***		3.752***		0.444***	
	(0.170)		(0.329)		(0.0413)	
treated × post	1.351***		-1.992***		-0.327***	
	(0.230)		(0.447)		(0.0553)	
post_1		-1.653***		2.589***		0.213***
		(0.245)		(0.482)		(0.0501)
post_2		-2.456***		3.598***		0.499***
		(0.262)		(0.501)		(0.0705)
post_3		-1.757***		5.086***		0.619***
		(0.259)		(0.510)		(0.0698)
treated × post_1		1.079***		-2.323***		-0.174**
		(0.331)		(0.650)		(0.0721)
treated × post_2		1.736***		-1.730***		-0.395***
		(0.349)		(0.670)		(0.0902)
treated × post_3		1.239***		-1.923***		-0.412***
		(0.352)		(0.690)		(0.0884)
Constant	11.97***	11.97***	34.03***	34.03***	1.882***	1.882***
	(0.104)	(0.104)	(0.196)	(0.196)	(0.0224)	(0.0224)
Observations	67712	67712	69270	69270	70065	70065
R^2	0.003	0.003	0.003	0.004	0.002	0.003
Adjusted R^2	0.003	0.003	0.003	0.004	0.002	0.003

Standard errors in parentheses

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

these effects are 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, by taking EIB funded loans the level of debt relative to equity. This has a negative impact on the solvency ratio of EIB funded firms 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

We extend our impact analysis by accounting for the fact that during the observation period the economies EIB funded firms opearted in were hit by financial crises. Thus, EIB lending - at least partly - took place during the crisis. This raises the question whether the crisis affects the impact of EIB lending on firm performance. Conceptually, two opposing effects might occur. First, the crisis could reinforce EIB impact as it 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. Second, the crisis might dampen the EIB impact on firm performance as it is associated with a decline in aggregate demand, i.e. a recession. In such an economic environment firms face a tougher challenge to reap the benefits of an easing in credit constraints provided by EIB funding. We aim to estimate which of the two effects prevails by obtaining the *causal* effect of the crisis on the impact of EIB funding. The emphasis is crucial from a policy perspective, as it indicates that 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. We are unable to address the latter question as this would require to control for the difference between the firms receiving EIB funding in crisis times and firms receiving in normal times. However, the observation period starts in 2008 only, i.e. the year of the Lehman brothers default. Moreover, many CEE countries were also hit by the euro crisis. Thus, 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.

[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 ratio of the treated decreases relative to the counterfactuals.

Against this background, we measure the 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 firms which receive funding during the crisis, but the loan allocation is followed by non-crisis years. We follow Lo Duca et al. in defining the crisis years in the countries under review[11] and estimate the following model.

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

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 the effect of a continuing crisis on the effectiveness of EIB funding. [12]

Table 6 indicates that the economic developments differ substantially when countries are in crisis compared to a non-crisis period. 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 crisis year is 0.20% if the crisis continues, 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)

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

[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.

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: DIFF-IN-DIFF-IN-DIFF: LOG VARIABLES

	(1)	(2)	(3)	(4)	(5)	(6)
	EBITDA (LOG)		REVENUE (LOG)		EMPLOYMENT (LOG)	
TREATED	0.380***	0.0287	0.384***	-0.00930	0.0686***	-0.294***
	(0.0245)	(0.0318)	(0.0233)	(0.0303)	(0.0203)	(0.0262)
POST	-0.0130	0.00407	-0.0798***	-0.0590	0.00580	0.0117
	(0.0300)	(0.0383)	(0.0286)	(0.0361)	(0.0244)	(0.0313)
TREATED × POST	0.0573	0.00981	0.131***	0.120**	0.107***	0.106***
	(0.0382)	(0.0495)	(0.0365)	(0.0475)	(0.0311)	(0.0403)
CRISIS		-0.406***		-0.473***		-0.499***
		(0.0387)		(0.0365)		(0.0318)
POST × CRISIS		-0.0409		-0.0499		-0.0129
		(0.0611)		(0.0583)		(0.0492)
TREATED × CRISIS		0.852***		0.948***		0.874***
		(0.0493)		(0.0467)		(0.0408)
POST × TREATED × CRISIS		0.108		0.0251		0.000587
		(0.0769)		(0.0735)		(0.0626)
CONSTANT	12.19***	12.36***	14.45***	14.65***	3.011***	3.218***
	(0.0190)	(0.0243)	(0.0179)	(0.0226)	(0.0158)	(0.0202)
N	35021	35021	39289	39289	39115	39115
R^2	0.013	0.029	0.015	0.033	0.002	0.023
ADJ. R^2	0.013	0.029	0.015	0.033	0.002	0.022

STANDARD ERRORS IN PARENTHESES
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Columns (DDD) in Table 7 indicate that the crisis does not affect the impact of EIB funding on profits, revenues and employment. While the overall effect of EIB lending continues to be significant and positive for revenues and employment, it does not matter whether EIB funded firms face a prolonged crisis after the loan allocation relative to those firms that experience a recovery of the respective economies after taking the loan. A similar result holds for the current and the solvency ratios. While the analysis confirms the overall negative effect (τ_1), the crisis has no significant impact on the impact of EIB funding. The exception to the rule is the EBITDA ratio with a significant positive coefficient for the triple interaction term. Thus, there is some evidence that the crisis raises the impact of EIB funding on the EBITDA ratio. Overall, the analysis suggests that the positive effects found in the baseline regressions (Tables 4 and 5) hold irrespective of economic conditions in the post-treatment period. It does not matter for EIB impact whether the economies the beneficiaries operated in a post-treatment period characterized by a prolonged crisis or whether the post-treatment years show

Table 8: DIFF-IN-DIFF-IN-DIFF: RATIOS

	(1)	(2)	(3)	(4)	(5)	(6)
	EBITDA RATIO		CURRENT RATIO		SOLVENCY RATIO	
TREATED	0.494*	0.211	-0.214***	0.216***	0.330	0.537
	(0.283)	(0.244)	(0.0595)	(0.0520)	(0.550)	(0.461)
POST	-1.797***	-1.857***	0.348***	0.438***	2.278***	4.116***
	(0.369)	(0.286)	(0.0834)	(0.0618)	(0.722)	(0.552)
TREATED × POST	1.589***	0.360	-0.268***	-0.208**	-1.552*	-2.178***
	(0.462)	(0.395)	(0.100)	(0.0938)	(0.898)	(0.769)
CRISIS		-0.338		0.262***		2.039***
		(0.283)		(0.0567)		(0.544)
POST × CRISIS		0.0597		-0.0904		-1.839**
		(0.467)		(0.104)		(0.909)
TREATED × CRISIS		0.283		-0.430***		-0.208
		(0.373)		(0.0790)		(0.717)
POST × TREATED × CRISIS		1.229**		-0.0599		0.626
		(0.607)		(0.137)		(1.182)
_CONS	11.58***	11.92***	2.010***	1.748***	35.18***	33.14***
	(0.227)	(0.169)	(0.0474)	(0.0311)	(0.435)	(0.326)
N	15682	38031	16331	39388	16179	39025
R^2	0.004	0.004	0.004	0.003	0.001	0.003
ADJ. R^2	0.004	0.003	0.004	0.003	0.001	0.003

STANDARD ERRORS IN PARENTHESES
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

already a recovery from the crisis.[13]

8 Robustness checks

We run a series of robustness checks to assess whether and to what degree our baseline results are sensitive to changes in the sample and the methodology applied.

Alternative specification of the control group - exact matching by country and cohort. We start by imposing on the matching exercise the condition that the nearest neighbor, i.e. the counterfactual, represents the same country and cohort as the treated firm. In other words, each treated firm from a given country that receives a loan in a given year is matched with a control firm from the same country and the same year. Results for our main difference-in-differences estimation (Equation 2) using this alternative specification for the control group can be found in Tables 11 and 12 in the Appendix. They confirm that the estimated

[13]It should be noted that this conclusion is strictly limited to the impact on individual SME borrowers, i.e. it does not imply that the overall macroeconomic impact of EIB funding is unaltered by economic developments.

coefficients are of similar magnitude as under our baseline.

Group fixed effects. We continue by running the baseline DID estimation (Equation 2) by controlling for industry, 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 (Tables 13 and 14 in the Appendix). By doing so, we control for potential unobserved heterogeneity that may be associated with the group categories, for example macroeconomic factors. The results suggest that the addition of group fixed eeffects to the baseline regression does not change the parameter values and their significance in any substantial manner.

Addressing the potential bias from missing data. As shown in section 4.2, we lose a significant proportion of our initial observations during the merging procedure with ORBIS. Out of 142,263 initial allocations we can only use 5,223 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 on 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 use two different exercises to gain insight on whether and to what extent the missing data problem is distorting our results.

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 use five employment size categories and six industry classes as strata generating-variables. Within each of the resulting 30 strata, we re-weight the observations such that the overall weight of the given stratum in the final sample would match the weight of the same stratum in the original allocation data-set. In other words, we overweight those observations that belong to strata with above average missing data, and underweight those coming from strata with less-than-average missing data. The resulting re-weighted sample can be considered representative of the original sample with respect to employment size and industry classification, and we can use the weighted sample to eliminate the missing data bias that are associated with the uneven data attrition along these two variables. The diff-in-diff results using the inverse probability weighted sample are reported in Tables 15 and 16 in the Appendix. They are in line with our baseline results, suggesting that our findings are robust to the effects of data attrition.

Second, we re-calculate our results on a sub-sample of firms from Romania, which is the country where the missing data problem is the least prevalent. Out of 3,867 unique Romanian firms in the allocation dataset we identify 1,372 with the needed data in ORBIS. In other words, 35.5 per cent of Romanian firms in the original data are also represented in our final sample, in contrast to the 5 per cent value of CESEE in general. The idea is that if the results hold in Romania, where missing data is much less of a problem than elsewhere, this would indicate that the findings are robust to the missing data bias. We run the matching for Romania only, and then re-estimate the baseline diff-in-diff equation using this restricted sample. The results - reported in Tables 17 and 18 of the Appendix - are close to the baseline specification, indicating that our results could possibly generalised beyond the SATT.

Cluster-robust inference. Bertrand et al. (2003) highlight that the traditional difference-in-differences estimators do not necessarily account for the serial correlation of the error term that occurs when multiple time periods are observed before and/or after the treatment. As a consequence, regression standard errors may be underestimated in these cases. In order to eliminate the serial correlation for these clustered observations, Bertrand et al. (2003) propose to calculate pre- and post-treatment means of the outcome variables, and run the DID regressions using these average outcomes. We follow this suggestion. The results with the new parameter estimates, together with the standard errors, are given in Table 19 and Table 20. It appears that the correction for the serial correlation of the errors does not affect the statistical significance of our results.

Controlling for credit demand One of the potential drawbacks of our baseline analysis is that we cannot determine if we are measuring the impact of an EIB-supported loan, or simply the difference in performance between firms who have exhibited credit demand and those who have not. In other words, it is possible that among the firms in the control group, some firms may not have had profitable investment projects to be funded, and therefore they may just simply not have exhibited credit demand.

We address this drawback by adding an extra condition at the probit model which aims at capturing non-treated firms with credit demand when selecting the control group. The additional condition we impose reflects the fact that we observe in the treatment group a significant decline in the solvency ratio in the year of treatment. As already mentioned, we interpret this as an increase in leverage due to external finance, i.e. the EIB-funded loans the firms receive. When selecting the control group, we choose firms showing a similar deterioration in the solvency ratio in the same year. Following our interpretation, this also indicates the use of external finance, i.e. we restrict our control group to firms that presumably exhibited credit demand at the same time as the treated firms and received a loan.

Against this background, we add the change in the leverage ratio in the treatment year to the probit model, and we use the corresponding model for the propensity score matching.[14] The matching technique and the following DID regressions remain unchanged. The results are presented in Table 21 and Table 22 of the Appendix, as well as Figure 8 in the Appendix. The difference in the solvency ratio between the two groups diminishes to a minimum, indicating that the match is successful. For the remaining outcome variables we find similar differences between the two groups as in the baseline. From this we conclude that our results also hold when comparing treated firms only with those firms showing positive credit demand.

9 Conclusion

In this paper we assess the impact of EIB funding on SME performance in Central, Eastern and South-Eastern Europe (CESEE) during 2008-2014, a period significantly affected by the global financial crisis and the Eurozone sovereign debt crisis.

Our results, derived from a propensity score matching and difference-in-difference estimation exercise are consistent with the rationale of public sector intervention into SME financing and the theory of change underlying the activities of International Financial Institutions. Moreover, they reinforce the results of previous studies on the impact of IFI support for a period of financial crisis, and based on dataset involving a substantially larger group of treated and non-treated firms and a longer observation period than used in other studies. Concretely, our results 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. We believe that these effect are 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 and revenues is not substantially different when firms face a prolonged crisis after the loan allocation, relative to the case of a rapid subsequent recovery. We only detect a

[14] Generally it is not recommended in the literature to use observations potentially influenced by the treatment in the propensity score model (see for example Imbens (2004)). It is due to the fact that the use of 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 by testing whether they hold in spite of this potential under-estimation.

slightly more positive impact on profitability for those firms that face a prolonged crisis. In this respect, our results are reassuring as they point to a consistently positive impact of IFI funding in a crisis situation irrespective of how the economy evolves in the post-treatment period.

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 CESEE countries in a period characterized by financial and economic turmoil.

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Appendices

Table 9: Balancing properties

Variable	Unmatched Matched	Mean Treated	Mean Control	Bias (%)	Reduction in bias (%)	t-test t	t-test P
Real revenue	U	7.70E+06	1.30E+07	-7		-3.57	0
	M	7.70E+06	8.60E+06	-1.1	84.4	-2.19	0.028
Solvency ratio	U	33.264	37.3	-14.2		-8.61	0
	M	33.288	33.307	-0.1	99.5	-0.04	0.97
Current ratio	U	1.9403	2.4441	-15.4		-9.12	0
	M	1.9309	1.8712	1.8	88.2	1.15	0.249
EBITDA ratio	U	12.039	11.038	7.3		4.4	0
	M	12.023	12.202	-1.3	82.2	-0.73	0.463
Change in employment	U	0.18449	0.98933	-2.1		-1.06	0.289
	M	0.18464	0.2127	-0.1	96.5	-1.8	0.071
Log employment	U	2.9713	2.7761	12.5		7.62	0
	M	2.9646	3.0111	-3	76.1	-1.53	0.127
Change in EBITDA ratio	U	-0.25211	-0.45626	2.5		1.4	0.161
	M	-0.25013	-0.26442	0.2	93	0.12	0.908
Change in current ratio	U	-0.03886	0.13637	-9.6		-5.94	0
	M	-0.02423	-0.00648	-1	89.9	-0.59	0.554

Figure 6: Balancing properties

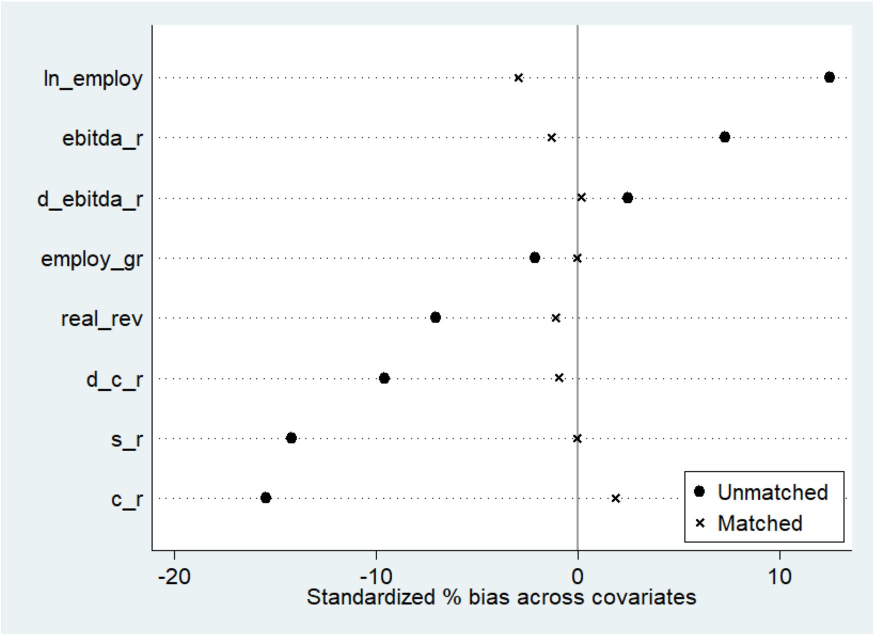


Table 10: Summary statistics

Potential controls				
	count	mean	p50	sd
Employment (log)	193522	2.6	2.6	1.8
EBITDA (log)	147663	12.0	12.0	2.1
Revenues (log)	194649	13.9	14.0	2.3
EBITDA ratio	170208	10.2	8.1	18.6
Solvency ratio	188908	38.4	39.6	35.4
Current ratio	197680	2.8	1.3	5.7
Matched controls				
	count	mean	p50	sd
Employment (log)	34783	3.0	2.9	1.7
EBITDA (log)	30282	12.2	12.3	1.9
Revenues (log)	34916	14.4	14.5	2.0
EBITDA ratio	33581	11.1	8.7	15.2
Solvency ratio	34590	35.6	35.2	29.7
Current ratio	35063	2.1	1.3	3.7
Matched treated				
	count	mean	p50	sd
Employment (log)	34704	3.0	3.0	1.4
EBITDA (log)	31853	12.5	12.6	1.6
Revenues (log)	34936	14.8	14.8	1.6
EBITDA ratio	34131	11.6	9.4	14.1
Solvency ratio	34680	34.2	33.3	27.5
Current ratio	35002	2.0	1.2	3.4

Figure 7: Impact graphs

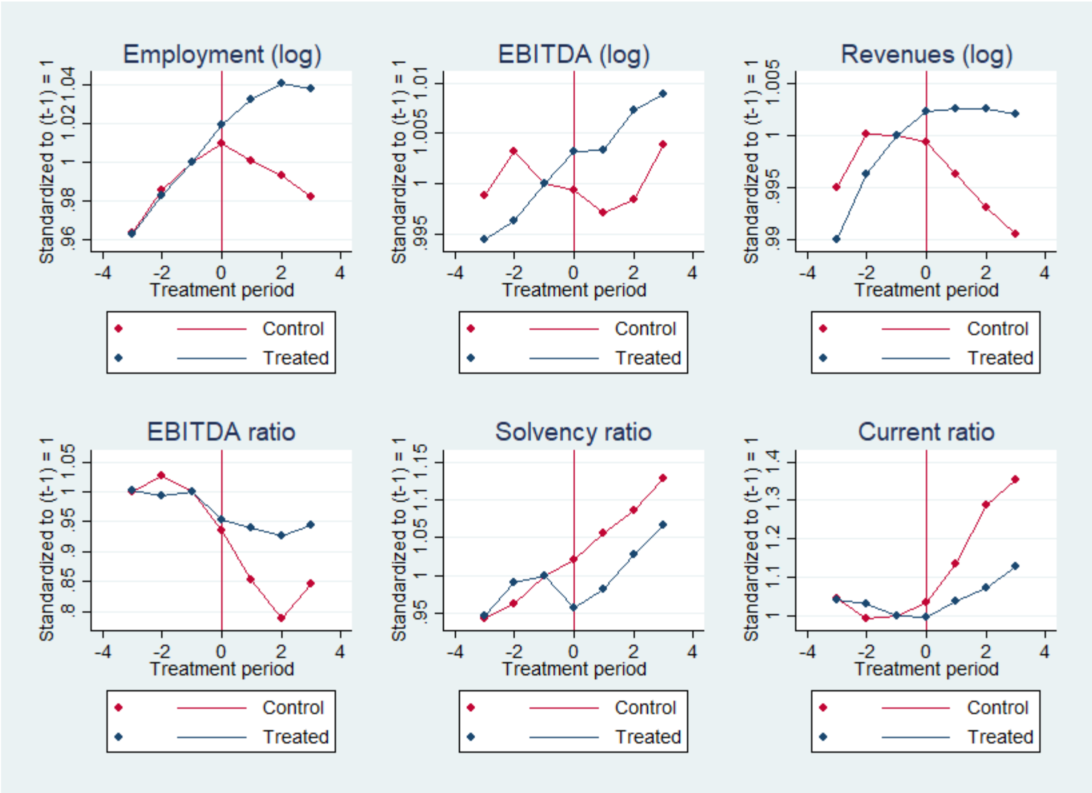


Table 11: Difference-in-differences with PSM by country and cohort - log variables

	(1)	(2)	(3)	(4)	(5)	(6)
	employment (log)		EBITDA (log)		revenues (log)	
treated	-0.0624***	-0.0624***	0.201***	0.201***	0.241***	0.241***
	(0.0159)	(0.0159)	(0.0189)	(0.0189)	(0.0182)	(0.0182)
post	-0.00371		-0.0191		-0.0747***	
	(0.0187)		(0.0230)		(0.0220)	
treated × post	0.139***		0.122***		0.151***	
	(0.0242)		(0.0295)		(0.0285)	
post_1		0.0258		-0.0403		-0.0442
		(0.0273)		(0.0335)		(0.0321)
post_2		-0.00235		-0.0375		-0.0793**
		(0.0275)		(0.0345)		(0.0326)
post_3		-0.0347		0.0214		-0.101***
		(0.0276)		(0.0345)		(0.0332)
treated × post_1		0.0955***		0.106**		0.123***
		(0.0352)		(0.0430)		(0.0413)
treated × post_2		0.150***		0.148***		0.158***
		(0.0354)		(0.0438)		(0.0420)
treated × post_3		0.173***		0.112**		0.173***
		(0.0357)		(0.0442)		(0.0429)
Constant	3.021***	3.021***	12.23***	12.23***	14.46***	14.46***
	(0.0121)	(0.0121)	(0.0145)	(0.0145)	(0.0138)	(0.0138)
Observations	67130	67130	59910	59910	67510	67510
R^2	0.001	0.001	0.005	0.006	0.007	0.007
Adjusted R^2	0.001	0.001	0.005	0.005	0.007	0.007

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

Table 12: Difference-in-differences with PSM by country and cohort - ratios

	(1)	(2)	(3)	(4)	(5)	(6)
	EBITDA ratio		Solvency ratio		Current ratio	
treated	0.0774	0.0774	-0.617**	-0.617**	0.0101	0.0101
	(0.148)	(0.148)	(0.281)	(0.281)	(0.0343)	(0.0343)
post	-1.764***		4.397***		0.474***	
	(0.178)		(0.335)		(0.0447)	
treated × post	1.244***		-2.656***		-0.352***	
	(0.238)		(0.456)		(0.0586)	
post_1		-1.597***		3.095***		0.279***
		(0.257)		(0.491)		(0.0600)
post_2		-2.058***		4.466***		0.436***
		(0.273)		(0.502)		(0.0643)
post_3		-1.638***		5.645***		0.706***
		(0.270)		(0.516)		(0.0820)
treated × post_1		1.112***		-2.863***		-0.237***
		(0.343)		(0.665)		(0.0803)
treated × post_2		1.416***		-2.612***		-0.327***
		(0.360)		(0.678)		(0.0866)
treated × post_3		1.203***		-2.495***		-0.491***
		(0.363)		(0.703)		(0.0993)
Constant	11.70***	11.70***	34.09***	34.09***	1.928***	1.928***
	(0.110)	(0.110)	(0.203)	(0.203)	(0.0242)	(0.0242)
Observations	65372	65372	66890	66890	67728	67728
R^2	0.002	0.002	0.004	0.005	0.003	0.003
Adjusted R^2	0.002	0.002	0.004	0.005	0.002	0.003

Standard errors in parentheses

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

Table 13: Difference-in-differences, controlling for industry, country, cohort and country-cohort fixed effects - log variables

	(1)	(2)	(3)	(4)	(5)	(6)
	employment (log)		EBITDA (log)		revenues (log)	
treated	-0.0316**	-0.0316**	0.248***	0.248***	0.252***	0.252***
	(0.0147)	(0.0147)	(0.0181)	(0.0181)	(0.0169)	(0.0169)
post	0.00726		-0.00832		-0.0792***	
	(0.0176)		(0.0222)		(0.0210)	
treated × post	0.131***		0.106***		0.156***	
	(0.0225)		(0.0280)		(0.0267)	
post_1		0.0342		-0.0381		-0.0349
		(0.0257)		(0.0323)		(0.0305)
post_2		0.0109		-0.0247		-0.0829***
		(0.0259)		(0.0331)		(0.0312)
post_3		-0.0235		0.0389		-0.120***
		(0.0261)		(0.0332)		(0.0323)
treated × post_1		0.0903***		0.0997**		0.115***
		(0.0327)		(0.0407)		(0.0385)
treated × post_2		0.138***		0.131***		0.162***
		(0.0329)		(0.0414)		(0.0394)
treated × post_3		0.164***		0.0871**		0.191***
		(0.0333)		(0.0419)		(0.0407)
Constant	3.898***	3.898***	13.53***	13.53***	15.50***	15.50***
	(0.0978)	(0.0977)	(0.254)	(0.254)	(0.203)	(0.203)
Observations	69487	69487	62135	62135	69852	69852
R^2	0.108	0.108	0.077	0.077	0.105	0.105
Adjusted R^2	0.108	0.108	0.076	0.076	0.104	0.104

Standard errors in parentheses

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

Table 14: Difference-in-differences, controlling for industry, country, cohort and country-cohort fixed effects - ratios

	(1)	(2)	(3)	(4)	(5)	(6)
	EBITDA ratio		Solvency ratio		Current ratio	
treated	0.0799	0.0799	-0.714***	-0.714***	0.0366	0.0366
	(0.137)	(0.137)	(0.266)	(0.266)	(0.0325)	(0.0325)
post	-1.946***		3.706***		0.444***	
	(0.165)		(0.324)		(0.0411)	
treated × post	1.317***		-1.993***		-0.326***	
	(0.222)		(0.436)		(0.0549)	
post_1		-1.653***		2.559***		0.213***
		(0.238)		(0.474)		(0.0497)
post_2		-2.436***		3.545***		0.499***
		(0.256)		(0.494)		(0.0700)
post_3		-1.750***		5.031***		0.619***
		(0.253)		(0.502)		(0.0693)
treated × post_1		1.045***		-2.318***		-0.173**
		(0.318)		(0.633)		(0.0715)
treated × post_2		1.692***		-1.723***		-0.395***
		(0.338)		(0.654)		(0.0896)
treated × post_3		1.216***		-1.938***		-0.411***
		(0.341)		(0.676)		(0.0878)
Constant	21.40***	21.40***	33.32***	33.32***	1.472***	1.472***
	(2.414)	(2.414)	(2.947)	(2.941)	(0.121)	(0.122)
Observations	67712	67712	69270	69270	70065	70065
R^2	0.076	0.076	0.052	0.053	0.017	0.017
Adjusted R^2	0.075	0.075	0.052	0.052	0.016	0.016

Standard errors in parentheses

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

Table 15: Inverse probability weighted diff-in-diff - log variables

	(1)	(2)	(3)	(4)	(5)	(6)
	employment (log)		EBITDA (log)		revenues (log)	
treated	-0.749***	-0.749***	-0.347***	-0.347***	-0.479***	-0.479***
	(0.0241)	(0.0241)	(0.0290)	(0.0290)	(0.0282)	(0.0282)
post	0.0174		0.0141		-0.0777**	
	(0.0268)		(0.0334)		(0.0323)	
treated × post	0.188***		0.165***		0.210***	
	(0.0365)		(0.0443)		(0.0449)	
post_1		0.0403		-0.0318		-0.0195
		(0.0391)		(0.0501)		(0.0452)
post_2		0.0218		-0.0163		-0.0820*
		(0.0388)		(0.0510)		(0.0475)
post_3		-0.0100		0.0931**		-0.132**
		(0.0399)		(0.0468)		(0.0531)
treated × post_1		0.131**		0.155**		0.158**
		(0.0534)		(0.0660)		(0.0629)
treated × post_2		0.194***		0.220***		0.220***
		(0.0529)		(0.0652)		(0.0662)
treated × post_3		0.239***		0.119*		0.253***
		(0.0536)		(0.0629)		(0.0711)
Constant	2.932***	2.932***	12.14***	12.14***	14.38***	14.38***
	(0.0176)	(0.0176)	(0.0211)	(0.0211)	(0.0195)	(0.0195)
Observations	69487	69487	62135	62135	69852	69852
R^2	0.046	0.046	0.007	0.007	0.012	0.012
Adjusted R^2	0.046	0.046	0.007	0.007	0.011	0.012

Standard errors in parentheses

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

Table 16: Inverse probability weighted diff-in-diff - ratios

	(1)	(2)	(3)	(4)	(5)	(6)
	EBITDA ratio		Solvency ratio		Current ratio	
treated	1.089***	1.089***	-3.371***	-3.371***	0.215***	0.215***
	(0.274)	(0.274)	(0.442)	(0.442)	(0.0670)	(0.0670)
post	-2.153***		3.957***		0.411***	
	(0.259)		(0.497)		(0.0602)	
treated × post	1.590***		-1.936***		-0.364***	
	(0.439)		(0.731)		(0.0997)	
post_1		-1.868***		2.846***		0.189**
		(0.381)		(0.746)		(0.0818)
post_2		-2.685***		3.715***		0.490***
		(0.442)		(0.773)		(0.0978)
post_3		-1.908***		5.329***		0.552***
		(0.368)		(0.743)		(0.0929)
treated × post_1		1.284**		-2.967***		-0.225
		(0.624)		(1.077)		(0.150)
treated × post_2		1.725**		-1.587		-0.469***
		(0.687)		(1.112)		(0.145)
treated × post_3		1.768***		-1.232		-0.397***
		(0.649)		(1.108)		(0.138)
Constant	12.30***	12.30***	33.93***	33.93***	1.940***	1.940***
	(0.147)	(0.147)	(0.294)	(0.294)	(0.0357)	(0.0357)
Observations	67712	67712	69270	69270	70065	70065
R^2	0.005	0.005	0.007	0.008	0.001	0.002
Adjusted R^2	0.005	0.005	0.007	0.008	0.001	0.002

Standard errors in parentheses

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

Table 17: Difference-in-differences, Romania only - log variables

	(1)	(2)	(3)	(4)	(5)	(6)
	employment (log)		EBITDA (log)		revenues (log)	
treated	-0.688***	-0.688***	-0.269***	-0.269***	-0.265***	-0.265***
	(0.0319)	(0.0319)	(0.0407)	(0.0407)	(0.0392)	(0.0392)
post	0.0672*		0.0748		-0.0380	
	(0.0386)		(0.0494)		(0.0480)	
treated × post	0.137***		0.102		0.197***	
	(0.0488)		(0.0631)		(0.0616)	
post_1		0.0975*		0.00851		0.00240
		(0.0556)		(0.0731)		(0.0699)
post_2		0.0725		0.0665		-0.0506
		(0.0568)		(0.0728)		(0.0710)
post_3		0.0307		0.151**		-0.0663
		(0.0576)		(0.0737)		(0.0725)
treated × post_1		0.0847		0.0949		0.174*
		(0.0703)		(0.0925)		(0.0892)
treated × post_2		0.149**		0.143		0.217**
		(0.0715)		(0.0918)		(0.0908)
treated × post_3		0.178**		0.0695		0.200**
		(0.0726)		(0.0943)		(0.0933)
Constant	3.220***	3.220***	12.19***	12.19***	14.27***	14.27***
	(0.0251)	(0.0251)	(0.0314)	(0.0314)	(0.0303)	(0.0303)
Observations	17375	17375	15216	15216	17824	17824
R^2	0.040	0.040	0.005	0.005	0.003	0.003
Adjusted R^2	0.040	0.039	0.005	0.005	0.003	0.002

Standard errors in parentheses

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

Table 18: Difference-in-differences, Romania only - ratios

	(1)	(2)	(3)	(4)	(5)	(6)
	EBITDA ratio		Solvency ratio		Current ratio	
treated	0.323	0.323	-4.041***	-4.041***	0.0679	0.0679
	(0.318)	(0.318)	(0.616)	(0.616)	(0.0796)	(0.0796)
post	-2.827***		5.576***		0.457***	
	(0.384)		(0.741)		(0.0906)	
treated × post	1.535***		-3.088***		-0.422***	
	(0.531)		(1.021)		(0.122)	
post_1		-2.741***		4.085***		0.233**
		(0.554)		(1.088)		(0.117)
post_2		-3.557***		5.447***		0.427***
		(0.613)		(1.112)		(0.126)
post_3		-2.173***		7.229***		0.711***
		(0.597)		(1.148)		(0.166)
treated × post_1		1.519**		-4.015***		-0.233
		(0.757)		(1.499)		(0.167)
treated × post_2		2.121***		-2.676*		-0.438***
		(0.814)		(1.521)		(0.166)
treated × post_3		0.954		-2.538		-0.597***
		(0.833)		(1.591)		(0.198)
Constant	12.13***	12.13***	30.09***	30.09***	2.034***	2.034***
	(0.220)	(0.220)	(0.447)	(0.447)	(0.0527)	(0.0527)
Observations	17054	17054	17429	17429	17934	17934
R^2	0.005	0.005	0.011	0.012	0.002	0.002
Adjusted R^2	0.005	0.005	0.011	0.011	0.002	0.002

Standard errors in parentheses

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

Table 19: Cluster-robust (BDM) estimation - log variables

	(1)	(2)	(3)
	Employment (log)	EBITDA (log)	Revenues (log)
treated	-0.0466	0.240***	0.242***
	(0.0305)	(0.0362)	(0.0345)
post	0.00656	-0.0383	-0.0958**
	(0.0333)	(0.0406)	(0.0388)
treated × post	0.155***	0.154***	0.185***
	(0.0433)	(0.0525)	(0.0503)
Constant	3.011***	12.16***	14.49***
	(0.0231)	(0.0277)	(0.0262)
Observations	19997	18563	20030
R^2	0.001	0.008	0.009
Adjusted R^2	0.001	0.008	0.009

Standard errors in parentheses

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

Table 20: Cluster-robust (BDM) estimation - ratios

	(1) EBITDA ratio	(2) Solvency ratio	(3) Current ratio
treated	-0.178	-0.0188	0.0597
	(0.243)	(0.508)	(0.0518)
post	-2.408***	3.626***	0.454***
	(0.265)	(0.575)	(0.0610)
treated × post	1.592***	-2.430***	-0.347***
	(0.364)	(0.789)	(0.0824)
Constant	12.20***	33.31***	1.871***
	(0.173)	(0.359)	(0.0360)
Observations	19906	19951	20036
R^2	0.005	0.003	0.004
Adjusted R^2	0.005	0.003	0.003

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

Table 21: Difference-in-differences - log variables, solvency ratio matched at treatment year

	(1)	(2)	(3)	(4)	(5)	(6)
	employment (log)		EBITDA (log)		revenues (log)	
treated	-0.0510***	-0.0510***	0.263***	0.263***	0.271***	0.271***
	(0.0156)	(0.0156)	(0.0187)	(0.0187)	(0.0178)	(0.0178)
post	-0.00367		-0.00686		-0.0765***	
	(0.0184)		(0.0228)		(0.0216)	
treated × post	0.139***		0.103***		0.150***	
	(0.0238)		(0.0292)		(0.0279)	
post_1		0.0216		-0.0405		-0.0397
		(0.0269)		(0.0333)		(0.0315)
post_2		0.00112		-0.0105		-0.0781**
		(0.0270)		(0.0340)		(0.0320)
post_3		-0.0339		0.0318		-0.112***
		(0.0272)		(0.0341)		(0.0329)
treated × post_1		0.101***		0.0968**		0.117***
		(0.0347)		(0.0425)		(0.0404)
treated × post_2		0.145***		0.117***		0.154***
		(0.0348)		(0.0431)		(0.0412)
treated × post_3		0.171***		0.0930**		0.178***
		(0.0351)		(0.0435)		(0.0423)
Constant	3.042***	3.042***	12.20***	12.20***	14.47***	14.47***
	(0.0119)	(0.0119)	(0.0145)	(0.0145)	(0.0136)	(0.0136)
Observations	68904	68904	61652	61652	69251	69251
R^2	0.001	0.001	0.008	0.008	0.009	0.009
Adjusted R^2	0.001	0.001	0.008	0.008	0.009	0.009

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

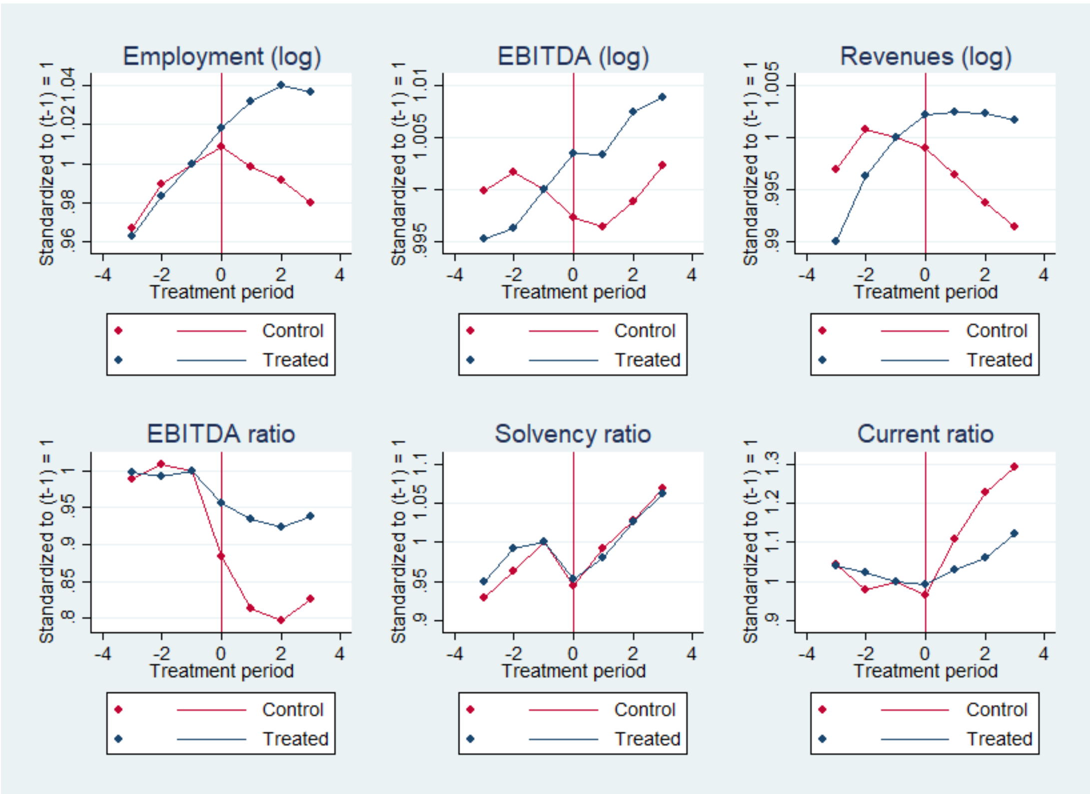
Table 22: Difference-in-differences - ratios, solvency ratio matched at treatment year

	(1)	(2)	(3)	(4)	(5)	(6)
	EBITDA ratio		Solvency ratio		Current ratio	
treated	0.258*	0.258*	-0.135	-0.135	0.0275	0.0275
	(0.143)	(0.143)	(0.271)	(0.271)	(0.0338)	(0.0338)
post	-1.922***		2.477***		0.405***	
	(0.172)		(0.331)		(0.0424)	
treated × post	1.257***		-0.791*		-0.296***	
	(0.231)		(0.446)		(0.0563)	
post_1		-1.896***		1.146**		0.214***
		(0.246)		(0.488)		(0.0574)
post_2		-2.112***		2.439***		0.441***
		(0.263)		(0.501)		(0.0674)
post_3		-1.754***		3.868***		0.561***
		(0.266)		(0.515)		(0.0706)
treated × post_1		1.268***		-0.948		-0.182**
		(0.331)		(0.651)		(0.0777)
treated × post_2		1.343***		-0.634		-0.353***
		(0.348)		(0.668)		(0.0873)
treated × post_3		1.157***		-0.798		-0.354***
		(0.357)		(0.693)		(0.0896)
Constant	11.75***	11.75***	33.87***	33.87***	1.906***	1.906***
	(0.104)	(0.104)	(0.197)	(0.197)	(0.0235)	(0.0235)
Observations	67134	67134	68967	68967	69447	69447
R^2	0.003	0.003	0.001	0.002	0.002	0.002
Adjusted R^2	0.003	0.003	0.001	0.002	0.002	0.002

Standard errors in parentheses

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

Figure 8: Impact graphs - log variables, solvency ratio matched at treatment year



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European Investment Bank
98 -100, boulevard Konrad Adenauer
L-2950 Luxembourg
+352 4379-22000
www.eib.org – info@eib.org

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