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Assessing the impact of the EIB's intermediated lending to SMEs during funding shocks

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Abstract We look at the impact of intermediated funding provided by the European Investment Bank (EIB) on the performance of small- and medium-sized enterprises (SMEs) in the European Union between 2008 and 2014 — the Great Financial Crisis (GFC) and its aftermath. We use a combination of propensity score matching and difference-in-differences to evaluate the impact of EIB-supported credit on corporate performance using firm-level data. We find that access to EIB-supported funding had a positive effect on employment and investment in beneficiary firms. This positive effect was stronger in those countries where banks have traditionally relied more strongly on non-core liabilities, such as interbank funding. All in all, our results indicate that EIB-supported funding made a significant and positive difference to the economic and financial performance of the beneficiary SMEs in the aftermath of the Great Financial Crisis.

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Plain English Summary Small firms' employment and investment activity can effectively be supported by public financial institutions' lending programmes through intermediary banks, particularly when these banks face funding constraints. In this paper, we examine the lending support provided by the European Investment Bank (EIB) on the performance of small- and medium-sized enterprises (SMEs) in the 28 member countries of the European Union between 2008 and 2014 — the Great Financial Crisis (GFC) and its aftermath. We find that access to EIB-supported loans had a positive effect on employment and investment of the beneficiary firms compared to a sample of similar firms without a link to an EIB-supported loan. This positive effect was stronger in those countries where banks heavily used interbank funding to finance their activities. We argue that reliance on interbank funding increases banks' exposure to funding shocks; thus, after the GFC, wholesale-funded banks curtailed credit supply to firms more than banks with stable funding. For wholesale-funded banks, the stable, long-term funding provided by the EIB appeared to be able to mitigate in part the impact of the shock on credit supply. This manifested in the better performance of the beneficiaries of the EIB-supported loans relative to other firms that faced tight credit supply. Overall, from a policy perspective, our findings give support to public sector intervention to SME credit markets in the form of intermediated lending, in particular after financial downturns that are associated with funding shocks to banks.

Keywords SMEs · Access to finance · International financial institutions · Funding shocks

JEL Classification G01 · H81 · L25

1 Introduction

Public financial institutions, both national and international, often target the access to finance problems faced by small- and medium-sized enterprises (SMEs), using credit guarantees and funding instruments with preferential conditions.[1] Alem and Madeira (2015) look at the scope of operations of 8 major public development finance institutions from different countries, and find that all of them are engaged in operations targeting SMEs. Gutierrez et al. (2011) confirm, on the basis of a much broader global survey of 373 public development banks from 92 countries, that the most common target for public development finance institutions around the world is the SME market: about 60% of the studied institutions have targeted products at SMEs. [2]

The economic justification for public sector involvement in the financial sector in support of SMEs is generally derived from the identification of market failures. Information asymmetries can lead to moral hazard and adverse selection of low-quality borrowers. This can make private sector financial institutions unwilling to extend credit to SMEs and mid-caps, especially in the absence of collateral, even at high interest rates (Jaffee and Russell, 1976; Stiglitz & Weiss, 1981). The result is credit rationing, i.e. an equilibrium where banks decide to keep the supply of credit below demand, rather than to tap the extra loan demand at higher interest rates. As a consequence, some SMEs with potentially viable investment projects are financially constrained, and unable to obtain the necessary financing from financial intermediation on a pure market basis (Beck & Demirguc-Kunt, 2006). This in turn prevents SMEs from implementing investments with potentially high marginal returns. As a result, the “SME financing gap” (OECD, 2006) is often considered to be a general economic policy concern. It represents a loss of aggregate output, employment and productivity, compared to a solution that would emerge without information asymmetries.

While there is a need for public intervention in the steady state, it is thought to widen in economic downturns and financial crises. In such periods, private sector banks become more risk averse, given that crisis-related losses can make bank capital scarce. Empirical evidence shows that low and declining bank capital has a negative impact on corporate lending activities by banks (Gambacorta & Shin, 2016). Moreover, a number of studies concluded that credit constraints are exacerbated by increasing market concentration and the profile of the banking sector. Ryan et al. (2014) show that bank market power is associated with an increase in financing constraints, and thus leads to lower SME investment levels. Carbo-Valverde et al. (2016) argue that during the Great Financial Crisis (GFC), credit-rationed firms were detached from bank funding and actively managed their trade credit accounts to cushion the liquidity shock instead. Ferrando et al. (2019) find that following the announcement of the European Central Bank’s (ECB’s) Outright Monetary Transactions Program, credit access improved relatively more for firms borrowing from banks with high balance sheet exposures to impaired sovereign debt.

Intermediated public intervention supporting SMEs typically comes either in the form of credit guarantees, or in the forms of preferential funding. Guarantees provide a partial transfer of the SME credit risk from the books of the private lending intermediaries to a public sector entity or international financial institution. Through the transfer of credit risk, these instruments act as a partial supplement to bank capital. Funding instruments provide financial intermediaries preferential access to liquidity: larger volumes, better pricing and/or better maturity conditions than sources

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

[2] Addressing the access to finance problems faced by SMEs is also a key policy objective of the European Investment Bank (EIB). In 2018 alone, 36% of the EIB’s new lending was dedicated to SMEs and mid-caps (EIB, 2019). The bulk of the EIB’s support to SMEs takes the form of intermediated lending, whereby the EIB provides funding to financial intermediaries that commit to this funding to extend loans to SMEs, and to pass on, at least in part, the preferential funding conditions.

otherwise available on the market. The credit risk, however, remains with the financial intermediary.

A large amount of empirical research has already been dedicated to the effectiveness and the impact of public guarantees (e.g. Brown and Earle, 2017). This literature generally confirms that public support through loan guarantees has a positive impact on the beneficiary firms. Less attention has been devoted, however, to those interventions where the support takes the form of funding to financial intermediaries. Empirical evidence on these interventions is therefore scarce and focuses chiefly on emerging markets. 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). 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). Banerjee and Duflo (2014) study the effects of policy changes related to the access criteria of a directed lending programme in India to demonstrate the existence of firm-level credit constraints. Our study is an extension of Gereben et al. (2019), who look at the impact of EIB funding on the performance of 5074 SMEs in eight countries of Central and Eastern Europe during 2008–2014. They find that EIB lending has a positive effect on employment, revenues and profitability.

A theoretical underpinning of the benefits of funding support by public banks is provided by Eslava and Freixas (2021). They develop a model where the screening of firms applying for credit is costly, and thus the equilibrium credit supply is sub-optimal. First, they show that subsidised public funding provides incentive to banks for more screening and therefore more lending — more efficiently than credit guarantees. Second, their analysis also looks at the counter-cyclical role of public banks, and show that preferential funding has additional beneficial effects in the presence of funding shocks compared to the case when commercial banks have unconstrained access to credit. Consequently, the desired level of activity of a public bank may be different along the business cycle. These two theoretical results can be interpreted as the basis of the hypotheses of our empirical investigation.

In this paper, we ask if public funding support to financial intermediaries make a difference for the beneficiary firms located in the EU, and whether this impact is dependent of the banks' funding environment. First, we assess the economic performance of firms that received EIB-supported intermediated lending — measured by employment levels and investment into fixed assets — against otherwise identical firms that did not receive such benefits. We focus on job creation and investment, as the two key impact indicators used by policy makers at the EU level. Second, we also assess whether the EIB support was more influential in countries where funding shocks affecting the banks were more severe. Our time horizon is between 2008 and 2014, spanning the GFC and its aftermath, when bank lending and credit conditions were heavily influenced by such funding shocks; therefore, our dataset is particularly suitable to such an analysis.

Our empirical strategy is as follows. We take the firm-level data that is reported back to the EIB by intermediary financial institutions as our starting point. We merge it with publicly available data on individual SMEs' financial and economic performance, which are collected and standardised by Bureau van Dijk in the Orbis data set. Data merging allows us to track financial performance of firms with EIB support, both in the years before and after the EIB loans were signed. Then we apply propensity score matching to find for each of the EIB beneficiaries a firm with similar observable characteristics but not being reported to have received the EIB support. In this way, we construct a treatment and a control group. Then we run difference-in-differences (DID) regressions on the matched sample to test whether SMEs receiving EIB-supported loans provided via local intermediaries perform differently with respect to our outcome variables, compared to other firms that did not receive EIB funding. In the estimation, we control for a broad set of fixed effects, including firm-level fixed effects and country-sector-year interactions, addressing potential omitted variable problems.

To assess whether the impact of funding support on firm performance was different in those countries where funding shocks to the financial system were more prevalent, we follow Iyer et al. (2013), Bremus and Neugebauer (2018) and De Jonghe et al. (2019). We use interbank dependence ratio — measured as the share of interbank funding within total liabilities — as an indicator of possible funding shocks. We explore whether EIB support had a different impact in countries where banks exhibit a stronger dependence on interbank funding, using a triple DID approach.

Our results indicate that firms benefiting from EIB-supported intermediated lending are characterised by significantly higher post-treatment employment and investment in the 3 years following the disbursement of the loan, than firms with otherwise similar observable characteristics but without EIB support. These results are robust for a range of alternative modelling specifications. In addition, we find that in EU countries that were characterised by stronger dependence on interbank funding — hence were more vulnerable to funding shocks — the impact of access to EIB-supported intermediated lending on firm-level employment and investment is substantially higher.

Our paper adds to the literature in three ways. First, we contribute to the empirical literature on public SME support by providing evidence that intermediated funding instruments have had a significant positive effect on the employment and investment performance of beneficiary SMEs in the EU in the aftermath of the GFC. To our knowledge, our paper is the first to demonstrate the effectiveness of intermediated public funding support to SMEs in an advanced economy context. Second, our work also contributes to the the rapidly expanding literature on the transmission of bank funding shocks to credit supply. The existing work focuses mainly on the impact of potentially volatile, non-core bank liabilities (such as interbank funding) on the supply of credit in the aftermath of financial shocks. We complement these findings by shedding light on the role of public policy in mitigating the consequences of such vulnerabilities through demonstrating that funding-type policy instruments have stronger firm-level impacts in countries where banks relied more heavily on interbank funding. Third, our paper provides empirical support to the model of Eslava and Freixas (2021) on public banks.

Intermediated funding to SMEs provided by multilateral banks and national promotional institutions bears strong resemblance to the some of the unconventional monetary policy tools that have been developed in the post-crisis period, which have aimed at stimulating credit supply to businesses by offering longer-term loans to banks at favourable costs and encouraging them to lend to the real economy. Examples include the Bank of England's Funding for Lending Scheme (FLS), the Funding for Growth Scheme (FGS) of the National Bank of Hungary and the Targeted Long-Term Refinancing Operations (TLTRO) by the ECB. Our work is therefore related to the studies that empirically assess the impact of these programmes on credit supply using bank level-data (Havrylchyk, 2016 in case of FLS, Laine, 2019, Boeckx et al., 2020 and Andreeva & García-Posada, 2021 for the TLTRO) and firm-level data (Endresz et al., 2015 for the FGS).

The paper is organised as follows. Section 2 provides a primer on funding shocks. Section 3 presents our data sources. Section 4 describes our empirical strategy. Section 5 presents our main results on the firm-level impact on EIB funding, and a range of robustness checks. It also provides insights on how reliance on interbank funding influences the impact of EIB support. Section 6 concludes.

2 Transmission of funding shocks to lending

We define funding shocks as sudden loss of access to short-term credit by financial institutions. Funding shocks are a manifestation of rollover risk, and concern financial institutions that fund themselves, at least partially, on wholesale markets. The inability to roll over short-term funding in the face of a credit crunch may force the concerned banks to curtail lending, to rapidly sell assets, or both.

Funding shocks can have a significant impact on lending, and on SME lending in particular. At a macro-level, Hahm et al. (2013) and Barattieri et al. (2021) argue that the presence of non-core liabilities, such as interbank funding in commercial banks' balance sheets, creates vulnerabilities that amplify the impact of financial cycles on credit supply. Khwaja and Mian (2008), Paravisini (2008) and Schnabl (2012) provide evidence at firm level of the transmission of interbank funding shocks to the availability of corporate credit in an emerging market context. Ivashina and Scharfstein (2010) demonstrate that US banks with less access to deposit financing reduced their lending more than other banks after the run by short-term bank creditors that followed the collapse of the Lehman Brothers in September 2008. Garcia-Appendini and Montoriol-Garriga (2013) also show that the missing funding at firm level was partially substituted by trade credit from cash-rich suppliers.

Funding shocks also contributed to the decline in corporate credit supply in the EU in the aftermath of the GFC. For instance, de Haan et al. (2017) document that eurozone banks responded to wholesale funding shocks between 2008 and 2013 (either interbank funding or securities issuance) by reducing household and corporate loan growth and by raising interest rates. Using loan-level data from Portugal, Iyer et al. (2013) show that banks that relied more heavily on interbank borrowing before the GFC decreased their credit supply in the post-crisis period significantly more than others, and the credit supply reduction was stronger for smaller firms. Bremus and Neugebauer (2018) find that the decline in cross-border banking flows via the interbank lending channel led to a deterioration in the borrowing conditions of small firms in the EU after 2010. Using aggregate data from eight euro area financial systems, Alvarez et al. (2019) find that following a liquidity funding shock, both credit and GDP decline in different amounts and lengths. Finally, De Jonghe et al. (2019) find evidence that Belgian banks reallocated credit towards lower-risk clients after the negative funding shock they experienced following the bankruptcy of Lehman Brothers in 2008.

Against this background, it seems likely that funding shocks to many of the European banks may have had a direct impact on the supply of credit for firms, and that many of these firms were unable to replace the missing bank credit with other sources of finance such as loans from other banks, trade credit or other types of debt. In addition, decreasing access to finance could have been particularly severe for smaller firms which generally have very limited access to alternative funding sources. It is thus reasonable to develop the hypothesis that publicly supported intermediated funding instruments, such as the EIB's intermediated lending, may have alleviated funding shocks by partially replacing the potentially volatile non-core (interbank) liabilities with long-term funding at stable, and predictable, cost and conditions.

Access to such funding might in turn allow financial intermediaries to mitigate the effect of funding shocks on lending conditions to firms. Credit rationing starts with the more risky borrowers, because of their lower risk-adjusted returns in the overall portfolio. Due to the asymmetric information, this problem gets exacerbated by adverse selection in credit markets. As banks cut back lending, relatively safe entrepreneurs could not get funded because of the lemons problem (Ikeda, 2020). By providing funding to banks faced with a credit crunch, it is not only possible to sustain credit flow to more financially constrained firms — which are expected to have display higher impact results — but to also alleviate the lemons problem.

Our hypothesis that larger exposure to funding shocks translate to stronger reaction to policy interventions at easing funding conditions is also supported by the empirical findings of Boeckx et al. (2020), who look at the impact of the post-GFC credit easing instruments of the ECB using bank-level data. They show that banks that are more dependent on the wholesale market turn out to be more responsive to central bank support, and exhibit stronger loan growth as a response to funding support than those banks that rely more retail funding.

3 Data

We use data from three distinct sources: EIB proprietary data on the individual loans to the beneficiary firms, financial and economic performance indicators of firms from Bureau van Dijk's Orbis data set, and indicators on interbank dependence, aggregated at country level from the ECB's consolidated banking data (CBD2).

In the following, we first briefly describe the EIB intermediated lending programme, the participating financial institutions and their representativeness relative to the financial sectors of EU countries. Then we present the firm-level data on the beneficiaries of EIB-supported lending and their key summary statistics. We carefully explain the method of merging the beneficiary data with firm-level financial data from Orbis, and the resulting losses of observations due to imperfect merging and/or low coverage of Orbis in certain countries and years. We describe in detail how the resulting data attrition affect our analysis. We then present our strategy to generate a sample of potential counterfactual companies, which serves as an input to the PSM model. Finally, we describe the data on interbank dependence.

3.1 EIB intermediated lending

EIB funding products targeting SMEs typically take the form of a Multiple Beneficiary Intermediated Loan (MBIL). To allocate funding to SMEs, the EIB leverages on the local expertise of financial institutions across the EU, using an intermediated lending model

Table 1 Basic statistics of the EIB-related financial intermediaries

		2008	2009	2010	2011	2012	2013	2014
Total assets (%)	Share	75	73	73	72	71	70	71
Total capital ratio (%)	All banks	12.0	14.3	14.5	14.3	15.9	15.3	15.6
	EIB-related	12.2	14.4	14.7	14.4	16.0	15.4***	15.7
Tier 1 ratio (%)	All banks	9.1	11.6	12.1	12.2	13.7	12.6	13.0
	EIB-related	9.3	11.8	12.3	12.3	13.8	12.7***	13.1
NPL ratio (%)	All banks	2.2	3.9	4.4	5.2	5.4	5.9	5.3
	EIB-related	2.3	4.1	4.6	5.3	5.6	6.1*	5.5
Interbank ratio (%)	All banks	82.4	95.5	103.4	104.7	108.6	99.6	97.9
	EIB-related	84.7	99.1	107.0	107.9	111.6	102.1	100.1
Loans/dep. ratio (%)	All banks	57.8	61.3	62.5	65.6	64.0	63.6	66.0
	EIB-related	58.6	62.1	63.2	66.3	64.5	64.0**	66.4***

Coverage expressed as a share of EIB-related banks to all banks. Weighted means of the respective variables for the EIB clients and the sample of all the EU commercial and savings banks. Weights correspond to the value of total assets in a given year. The difference between the samples in each year is assessed through a two-sample test, under the null hypothesis of equivalence of weighted means. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels

involving private financial service providers.[3] The EIB provides funding to these intermediaries directly (or indirectly, via public promotional institutions) at conditions that are somewhat better than those available on the market at a given moment. In exchange, they commit to use the funds to extend loans to SMEs, and to partially transfer the financial benefit to the final beneficiaries in the form of an interest rate reduction and/or the provision of longer tenors. This is a scalable mechanism that makes EIB financing available to SMEs and mid-caps in a swift and efficient manner.

The minimum MBIL contract size is EUR 50m, and it supports a portfolio of at least EUR 100m of final SME loans originated by an intermediary institution. While there is vast heterogeneity between such intermediaries, the business reality dictates that they must have sufficient allocation capacity. They are moreover monitored on an ongoing basis for breaches of financial covenants and contractual clauses.

While in the light of this one could expect the EIB's partner intermediary institutions to be potentially larger and financially stronger than the rest of the market, our analysis presented in the following reveals that within the EU banking system, the EIB partner intermediaries constitute, in fact, a fairly representative sample.[4] Specifically, we benchmark the EIB clients against the sample of commercial and savings banks available in the BankFocus database, compiled by the Bureau van Dijk (BvD). The comparison is given in Table 1.

The original EIB data consist of 386 unique financial intermediaries, including both commercial banks and other non-bank financial institutions (e.g. leasing firms).[5] For consistency reasons, we focus only on bank-type entities. We are able to identify financial accounts in BankFocus for 140 banks (unconsolidated and consolidated accounts), which constitute our benchmark sample.[6] While the total sample of EU banks available in BankFocus consists of 1599 entities, the EIB-related banks represent from 70 to 75% of their total asset value.[7] Furthermore, we compare

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

[4]This limits a potential selection bias in the results presented in Section 5, whereby the estimated impact could be driven by the differences in bank characteristics rather than final beneficiaries.

[5]We are able to determine the type of an institution only after consulting the BankFocus. Therefore, we are not able to give a clear proportion of each type in the sample, as some of the institutions are not covered by the database.

[6]The main results from Section 5 are fully preserved for allocations originated by the benchmark intermediaries.

[7]On consolidated level, the EU28 banks available in BankFocus represent around 60–75% of total banking assets throughout the sample years, as reported by the ECB in the CBD2 database. Even though we cannot provide an unambiguous market benchmark for our sample of banks, at the aggregate level, the EIB

weighted means of the basic banking statistics, including the total capital ratio, tier 1 ratio, Non-Performing Loans (NPL) ratio, interbank ratio and loans/deposits ratio. It can be readily observed that while the aggregate numbers point to a modestly higher capitalisation, interbank dependence, loans/deposits and NPL metrics for the EIB clients, the differences are hardly statistically significant. In fact, only towards the end of the sample period the differences become apparent for the capitalisation and loans/deposit ratio.

While we cannot unambiguously benchmark other types of financial intermediaries, at least within the bank-type entities, we believe the EIB clients cover a wide and representative market spectrum to claim there is sufficient chance for control firms to have relations with them. Furthermore, the possible systematic bias in intermediary selection should be absorbed by fixed effects in our empirical framework.

3.2 EIB allocation data

The EIB allocation data are gathered annually in a standardised format, starting in 2008. The data are provided by those financial intermediaries that have a direct relation to the SME. They contain information on the size of the company and the main sector of operations (using the NACE Rev. 2 four-digits classification), as well as loan-specific information, such as the date of loan disbursement, loan volume and maturity. For the purpose of this analysis, we consider allocations up until end-2014 only.[8] The aggregate statistics of the allocation data set, including the number of allocations, number of partner financial intermediaries, total amount allocated and average allocation sizes, by country, year and employment size, are presented in Table 2.

The data set includes 520,746 individual allocations to 403,788 different firms.[9] Total EIB lending to European SMEs and mid-caps through these allocations amounted to EUR 72,4bn over the 7 years between 2008 and 2014.

The data show significant heterogeneity across countries. The largest recipients were Spain, Poland and Italy. Not surprisingly, these are also the countries with the most numerous EIB-related financial intermediaries. In addition, the average allocated amounts show large variations from country to country. Looking at the data by year, we see a gradual increase in the number of allocations and the amounts over time. The largest share of loan recipients are companies with 2 to 10 employees. However, measured in euro, the largest share of the total funding (29%) was allocated to firms with 51 to 250 employees. Overall, the data structure is visibly skewed towards SMEs, which is also the reason why we rather consider the following analysis as representative of lending to SMEs, rather than to mid-caps.

3.3 Merging with Orbis and the resulting data attrition

We use the Orbis data set to obtain information on the financial and economic performance of EIB loan beneficiaries. Our Orbis feed is updated semi-annually in vintages, where each vintage is cleaned up from companies which have not reported any information for 10 years or more. Therefore, to correct for the survivorship bias, we aggregate the data for all the vintages to obtain a sample covering years until 2017, which is also the last available year at the time we updated the files.

We work with firms' unconsolidated accounts with all monetary values expressed in euro. We clean up the data by excluding observations with odd or inconsistent values in the spirit of Barbiero et al. (2020).

works with virtually all large financial institutions across the EU.

[8] Allocation data are readily available for the following years, too. However, in line with the literature, for practical purposes, we consider only those allocations that can be monitored for a sufficiently long period after the loan has been disbursed. This allows us to take possible lags in the impact of the loan disbursement into account. To guarantee at least 3 years of follow-up, we cut the allocation sample in 2014. This allows us tracking the financial performance of the beneficiaries up to 3 years after, as 2017 is the last available year for the financial data in Orbis.

[9] It is possible that a company received multiple EIB-supported loans in the same year, or across the years. We treat multiple allocations in the following way. For loans contracts signed in the same year, we choose the loan with the largest nominal amount, as possibly corresponding to the largest investment project. In case there are multiple loans with the same largest nominal amounts in the same year, we pick the one with the earliest signature date. If there are still multiple allocations for the same firm in the same year, we consider such reporting as a typo and do not include such records in the analysis. For firms which received multiple loans across years, we take the loan allocated in the first reported year.

In particular, we drop firm-year observations in which total assets, fixed assets, intangible fixed assets, sales, long-term debt, loans, creditors, debtors, other current liabilities or total shareholder funds and liabilities have negative values. On top of that, we check for the reporting consistency and drop the firm-year financial statements which violate the basic balance-sheet equivalences by more than 10%. Specifically, we impose that (i) total asset match total liabilities, (ii) total assets match the sum of fixed assets and current assets and (iii) current liabilities match the sum of loans, trade credit and other current liabilities. We also deflate variables using the country-specific Harmonised Index of Consumer Prices (HICP) deflators. Finally, we winsorise the series by years at 1% levels.

Out of our 403,788 unique beneficiaries, we find corresponding entries in Orbis in 152,381 cases. Short

Table 2 EIB allocation data

By country

	Allocations (in #)	Intermediaries (in #)	Share (in %)	Amount (in mEUR)	Share (in %)	Mean size (in kEUR)
Austria	2377	20	0.46	1539	2.13	648
Belgium	9784	11	1.88	1577	2.18	161
Bulgaria	3525	12	0.68	601	0.83	170
Croatia	4008	10	0.77	1458	2.01	364
Cyprus	782	11	0.15	274	0.38	350
Czech Republic	12,577	19	2.42	1908	2.64	152
Denmark	3494	7	0.67	397	0.55	114
Estonia	5	1	0.00	12	0.02	2429
Finland	1524	4	0.29	359	0.50	236
France	37,009	18	7.11	6210	8.58	168
Germany	11,149	27	2.14	3770	5.21	338
Greece	4227	8	0.81	1962	2.71	464
Hungary	5876	20	1.13	1526	2.11	260
Ireland	3313	7	0.64	512	0.71	155
Italy	77,173	77	14.82	17,560	24.25	228
Latvia	1856	5	0.36	197	0.27	106
Lithuania	27	5	0.01	48	0.07	1767
Luxembourg	1451	9	0.28	724	1.00	499
Netherlands	7071	10	1.36	2278	3.15	322
Poland	97,137	36	18.65	3547	4.90	37
Portugal	12,034	16	2.31	3208	4.43	267
Romania	5239	20	1.01	504	0.70	96
Slovakia	11,375	20	2.18	1543	2.13	136
Slovenia	3558	10	0.68	640	0.88	180
Spain	193,451	58	37.15	17,809	24.60	92
Sweden	4642	5	0.89	88	0.12	19
UK	6082	9	1.17	2149	2.97	353
Total	520,746	386†	100	72,401	100	

Table 2 (continued)

By year

	Allocations (in #)	Intermediaries (in #)	Share (in %)	Amount (in mEUR)	Share (in %)	Mean size (in kEUR)
2008	39,129	96	7.51	6142	8.48	157
2009	42,722	130	8.20	7259	10.03	170
2010	63,865	148	12.26	10,082	13.92	158
2011	63,849	176	12.26	13,148	18.16	206
2012	75,796	156	14.56	9326	12.88	123
2013	101,185	148	19.43	11,311	15.62	112
2014	134,200	167	25.77	15,134	20.90	113
Total	520,746	386†	100	72,401	100	

By employment class

	Allocations (in #)	Intermediaries (in #)	Share (in %)	Amount (in mEUR)	Share (in %)	Mean size (in kEUR)
0–1	115,774	302	22.23	10,209	14.10	88
2–10	186,913	361	35.89	14,781	20.42	79
11–50	135,184	363	25.96	20,034	27.67	148
51–250	76,443	363	14.68	20,913	28.88	274
250–500	3404	184	0.65	2732	3.77	803
501 or missing	3028	173	0.58	3733	5.16	1236
Total	520,746	386†	100	72,401	100	

The numbers correspond to the raw data and therefore include multiple allocations to the same beneficiary. †*There are 386 unique financial intermediaries*

of unique numerical company identifiers, we use company details as matching variables,[10] and use string-based matching algorithms to pair EIB beneficiaries with corresponding company records in Orbis. Given the presence of typos, different spelling conventions and often non-consistent use of accents and special characters in the two data sets, we could not rely only on perfect matches. Our main tool is BvD's own string matching algorithm, which gives a score based on matching probabilities. For further analysis, we consider only the matches of 85% accuracy and above.

Once we identify the firms in Orbis, we check if the data coverage is sufficient. Many firms have incomplete corporate records in Orbis. For our exercise, we needed at least a basic set of balance sheet and income statement data, together with the number of employees, for 3 years before and after the allocations.[11]

Table 3 shows the success of the matching and the data extraction from Orbis by country, year and employment class, and it illustrates the resulting loss of observations. We successfully paired 44.6% of the EIB allocations with a record in Orbis. However, only 13.25% of the original allocations had sufficient data coverage in Orbis to be included in the propensity score matching. This does seem to fall within the attrition range reported in the other studies. For instance, Gereben et al. (2019) work only with 4.8% of the

[10]In particular, the matching is carried out on company name, physical address and reported primary sector of activity, following the probabilistic matching procedure proposed by Geurts (2016).

[11]See section 4.1 for the list of variables used in the PSM. The econometric framework allows for some data gaps; therefore, not all control firms need to have the full 7-year data set.

Table 3 Data attrition

By country

	Total EIB	With BvDID		With useful data	
	(in #)	(in #)	(in %)	(in #)	(in %)
Austria	1861	699	37.56	1	0.05
Belgium	7673	2797	36.45	361	4.70
Bulgaria	2633	1955	74.25	963	36.57
Croatia	3394	1741	51.30	1159	34.15
Cyprus	765	303	39.61	0	-
Czech Republic	9092	7126	78.38	2659	29.25
Denmark	2097	1412	67.33	0	-
Estonia	3	1	33.33	0	-
Finland	1257	924	73.51	221	17.58
France	24,991	12,081	48.34	2722	10.89
Germany	8134	3144	38.65	246	3.02
Greece	3688	298	8.08	66	1.79
Hungary	3953	2631	66.56	1233	31.19
Ireland	3021	334	11.06	0	-
Italy	56,918	22,124	38.87	8911	15.66
Latvia	1219	368	30.19	94	7.71
Lithuania	26	12	46.15	0	-
Luxembourg	1011	537	53.12	52	5.14
Netherlands	5825	1851	31.78	96	1.65
Poland	70,761	22,630	31.98	605	0.85
Portugal	10,681	4156	38.91	2887	27.03
Romania	3855	3558	92.30	2789	72.35
Slovakia	7950	4897	61.60	1751	22.03
Slovenia	2668	1986	74.44	975	36.54
Spain	161,054	49,543	30.76	24425	15.17
Sweden	4058	3217	79.28	1273	31.37
UK	5200	1741	33.48	2	0.04
Total	403,788	152,066	37.66	53491	13.25

By year

	Total EIB	With BvDID		With useful data	
	(in #)	(in #)	(in %)	(in #)	(in %)
2008	29,354	8847	30.14	3055	10.41
2009	34,433	12,710	36.91	4506	13.09
2010	49,591	17,402	35.09	5886	11.87
2011	47,975	19,312	40.25	6808	14.19
2012	57,126	17,269	30.23	5160	9.03
2013	79,869	22,088	27.66	7344	9.20
2014	105,440	54,438	51.63	20,732	19.66
Total	403,788	152,066	37.66	53,491	13.25

Table 3 (continued)

By employment class					
	Total EIB	With BvDID		With useful data	
	(in #)	(in #)	(in %)	(in #)	(in %)
0–1	101,893	16,945	16.63	2280	2.24
2–10	156,875	66,469	42.37	20,069	12.79
11–50	95,261	46,041	48.33	20,941	21.98
51–250	46,408	20,748	44.71	9470	20.41
250–500	1813	1032	56.92	488	26.92
501 or missing	1538	831	54.03	243	15.80
Total	403,788	152,066	37.66	53,491	13.25

'Total EIB' corresponds to the figures as reported in the EIB allocation tables, 'With BvDID' describes number and percentage of firms successfully paired with Orbis, and 'With useful data' shows number and percentage of firms with sufficient data coverage to be included in the propensity score matching (PSM)

original number of treated firms, whereas Asdrubali and Signore (2015) with 18%.[12]

When grouping the data by observable categorical variables, it is visible that the share of missing data are not balanced across these categories.[13] Data attrition is particularly unevenly distributed across the countries. Lack of Orbis coverage in some key variables results in certain countries dropping out of the sample altogether, like in the case of Cyprus, Denmark, Estonia, Ireland, Lithuania or the UK. Most of the the largest beneficiary countries have good Orbis coverage, however. The only exception is Poland, where only less then 1% of the allocations are successfully matched and populated with data. Table 3 also indicates that data availability varies somewhat less by allocation year. This is also generally true for firm size, although individual entrepreneurs are less likely to have a complete Orbis record than larger firms.

Having pointed this out, we cannot assume that the data are missing completely at random. As a consequence of the uneven nature of data attrition, treatment effects calculated based our final sample can be considered as *sample* average treatment effects on the treated, which cannot necessarily be generalised as *population* average treatment effects on the treated.

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

3.4 Potential controls

In the next step, we construct a pool of potential counterfactual firms. In principle, all EU SMEs and mid-caps that have been active between 2008 and 2014 could have been eligible for an EIB-supported loan. However, for practical considerations, we populate our potential control pool with a unified number of firms per stratum, which show broad similarity to the treated firms, and thus have a high chance

[12]To verify if low Orbis coverage has an impact on our results we re-estimated our econometric models on the Romanian subsample of our data set, as in this particular country, the coverage of the Orbis data set is the highest. In case of Romania, we found matching Orbis records with complete data for 72% of the EIB beneficiaries. Running the results on the Romanian sample has provided qualitatively the same results as the full-sample analysis, suggesting that our conclusions are not driven by the data attrition. The results of the analysis for Romania are available from the authors upon request.

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

to serve as a reasonable comparison pair. Such a data standardisation improves the asymptotic properties of the PSM setting, and therefore of our impact estimates, as the proportions between the treated and non-treated firms in each stratum become more homogenous.

For this purpose, we use a stratified sampling approach. While there is a range of possible data partitions to define the strata, we rely on the dimensions proposed in the empirical design of the EIB Investment Survey, as sufficient to capture the within-EU corporate heterogeneity (Brutscher & Ferrando, 2016). In particular, strata are defined along dimensions: country (28 categories), size groups by number of employees (4 categories for 0–9, 10–49, 50–250 and 250+ employees) and industry groups by NACE codes (6 categories for Agriculture (section A), General Industry (B, C, D, E), Construction and Real Estate (F, L), Trade (G), Transportation and Accommodation (H and I) and Other (other sections). To capture the time dimension, we add the 4th strata for the allocation year (7 categories for years 2008–2014). These strata dimensions generate 4704 actual clusters, from which 3055 actually contain at least one firm from the treated sample. We then randomly sample firms for each cluster from the Orbis. For each cluster, we attempt to sample 15 times the number of firms in the treated group.[14] We sample only from those firms that had 3 years of financial and employment data available in Orbis both before and after the (presumed) treatment.

As Orbis is not uniformly well-populated in some countries and company categories, we have not always found 15 suitable firms for each cluster. Finally, our pool of potential controls consists of 820,162 individual firms with a complete data record. The summary statistics for the potential control firms are given in Table 5; however, for brevity reasons, we discuss them together with the PSM results in Section 4.

[14]There is no clear-cut rule to pick the optimal number of controls per stratum, as the full Orbis accounts are greatly unbalanced. Sampling $k = 15$ times the number of treated firms per strata reaches the control-to-treatment ratio of more than 5:1. However, even if it seems arbitrary, k does not directly affect our results, which fully hold even for a fixed number of 100 sampled control firms per each stratum.

3.5 Data on interbank funding

Following Iyer et al. (2013), Bremus and Neugebauer (2018) and De Jonghe et al. (2019), we use reliance on interbank dependence as an indicator of banks' vulnerability to funding shocks. We obtain country-level aggregates of the interbank dependence ratio from the ECB's CBD2 database, for each country and year in our sample.

Interbank dependence shows significant heterogeneity both over time and across EU countries. The median value of the interbank dependence ratio, measured as interbank liabilites divided by total assets, is 13.8%. The maximum value is 48.9 (Latvia, 2008), while the minimum is 1.7% (Slovakia, 2012). We also observe, as expected, a decline of interbank dependence over time. The cross-country median value of the interbank dependence ratio fell from 17.4 to 10.4% from 2008 to 2014. Overall, we believe the data set provides sufficient heterogeneity over time and across-regions to support the evaluation of the EIB lending portfolio.

4 Empirical strategy

Our empirical strategy follows a two-step approach. Firstly, we describe the method to construct the counterfactual control sample by the propensity score matching (PSM) method. We then estimate the treatment effects in the difference-in-difference (DID) framework, controlling for the potential confounding variables. In the main body of the paper, we focus on general description of our approach; however, careful explanation of the concepts and technical details are provided in the online Appendix.

Empirical frameworks using PSM, DID or a combination of the two have been used before in the literature to assess the impact of financial support to SMEs by public sector institutions, focusing chiefly on guarantee instruments rather than funding support. For instance, Oh et al. (2009) evaluate the effect of the credit guarantees in Korea from 2000 to 2003 using PSM, and find that that guarantees supported firms' ability to maintain their size, and increase their survival rate, but not to increase their investment. Brown and Earle (2017) analyse the impact of loans provided by the US Small Business Administration (SBA) on employment. Their results indicate an increase of 3-3.5 jobs for each million dollars of loans, and that the impact is stronger for younger and larger firms.

Studies in a similar vein have also been conducted within the EIB Group to assess the impact of guarantee instruments targeting SMEs. Asdrubali and Signore (2015) show that SMEs in the Central and South Eastern Europe (CESEE) region, which received funding guaranteed by the EU SME Guarantee Facility managed by the European Investment Fund (EIF), recorded an increase in the number of employees and in sales compared to a respective control group of SMEs. The results were estimated on a sample of firms receiving EIF-supported loans between 2005 and 2007. Bertoni et al. (2018) show that, on average, French SMEs benefiting from guaranteed loans created more jobs and grew more in terms of assets and sales . Bertoni et al. (2019) repeat the exercise on guaranteed loans granted under the EU programmes MAP and CIP on SMEs' growth in Italy, the Benelux and the Nordic countries from 2002 to 2016.[15] Guaranteed loans are found to positively affect the growth in assets, sales, employment and the share of intangible assets.

4.1 Propensity score matching

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

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

where Φ is the cumulative normal distribution, variable T is a dummy determining if a firm i was treated or not in year t, matrices X contain a set of firm-specific controls which and matrix μ contains a set of fixed effects including additively age class and employment, industry, country and year strata.[16]

Our aim is to include all important variables that affect both the selection into the treatment and the outcome of the treatment in our model. We begin with a set of indicators of corporate performance describing size, sales, profitability, leverage, liquidity, asset tangibility, innovativeness, which we deem as possible inputs into the loan assignment decision. They include leverage ratio (defined as a share of current and non-current liabilities as a share of total assets), employment (in logs), total assets (in logs), cash ratio (cash and cash equivalents as a share of total assets), current ratio (current assets as a share of current liabilities), turnover ratio (operating revenues as a share of total assets) and sales growth. Regarding the measures of innovativeness, we consider two generic dummy indicators, i.e. if a company filled at least one patent application or it published at least one patent in a given year.[17]

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

While developing the PSM model, we closely follow the strategy, proposed by Dehejia and Wahba (1999), to keep significant regressors together with their corresponding higher order (squared and cubic) terms if they improve the goodness of fit of the

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

[16] The stratification groups are taken as described in Section 3.4. For the age classification, we use 5 groups: [0,2), [2,5), [5,10), [10,20) and 20 and more years after the date of incorporation of a firm.

[17] More detailed explanation of the selection of variables into the PSM model, together with their multicollinearity assessment, is given in the online Appendix.

model.[18] As the data, especially regarding the income statements, are missing for many of the treated firms, in the process, we try to keep the balance between the number of regressors and the final number of matched firms. Overall, however, the final ATET results turn out to be robust to various PSM specifications. Table 4 shows the results for our final specification, which we consider as an optimal compromise between inputs and outputs.

Even though we treat the probit model as instrumental to the estimation of ATET, the estimated probit coefficients seem to reveal several interesting patterns. Higher leverage can be a signal of already acquired financial literacy, making it more likely to fill another loan application. Loan recipients are also more likely to be the growing companies or firms with innovative capacity, as exemplified by the turnover ratio, the growth in sales and the number of patents. Tangible assets, which are typically used as collateral, are yet another important predictor for receiving an external loan. According to the results, however, firms which are cash-rich are less likely to seek external finance.

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

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

Beyond the close similarity of the propensity scores, we also verify the impact of the matching on the key variables of interest, related to treatment assignment and outcome. Table 5 summarises the aggregate characteristics of the data set for the pre-treatment period. In particular, we look at the original pool of potential controls, matched controls and the treated firms. The improvement in the aggregate statistics is striking. The PSM-matched controls show a very close similarity to the treated firms with respect to all variables of interest. This is not restricted only to the mean aggregates, but also to higher moments of the data distribution.

As the last step, we look at the balancing properties of the PSM in more detail. Specifically, for each variable, we calculate the standardised percentage bias, before and after the matching, defined as a percentage difference of the sample means in the treated and control groups (either potential or matched) as a percentage of the square root of the average of the corresponding sample variances (Rosenbaum & Rubin, 1985). Table 6 shows the balancing properties before and after the matching with respect to the variables used in the probit model. We can see that the matching resulted in a very significant bias reduction across all the variables, even though differences in the cash ratio and the current ratio remain statistically significant. These anomalies can be related to unobservable factors, like corporate group dependence or supply-chain structures, hence difficult to account for in the PSM specification.

The outstanding differences between the treated firms and PSM-matched controls remain below the rule-of-thumb 0.1 threshold (Stuart et al., 2013). Even though they are still statistically significant for two covariates, we believe that any potential structural bias will be absorbed by the vector of firm-level fixed effects in the subsequent DID model. Overall, the results of the matching exercise give us comfort to claim that the control group shows sufficient similarity to the treated group to serve as a fair basis of

[18] In fact, adding higher order terms to the probit model improves the balancing properties for the majority of variables reported in Table 6, and therefore we keep them in our preferred specification. The subsequent results, however, are virtually unchanged when matching on the probit model without the squared and cubic terms.

Table 4 Probit model results

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

Table 4 (continued)

	Dependent variable: Assignment into treatment (T_i)								
	Lag 1	Lag 2	Lag 3	Lag 1 (sq)	Lag 2 (sq)	Lag 3 (sq)	Lag 1 (cub)	Lag 2 (cub)	Lag 3 (cub)
Patents	0.115***	0.052*	0.072***						
	(0.027)	(0.030)	(0.028)						
Size class FE	Yes								
Age class FE	Yes								
Allocation year FE	Yes								
Sector FE	Yes								
Country FE	Yes								
Observations	737,162								
R2	0.069								

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

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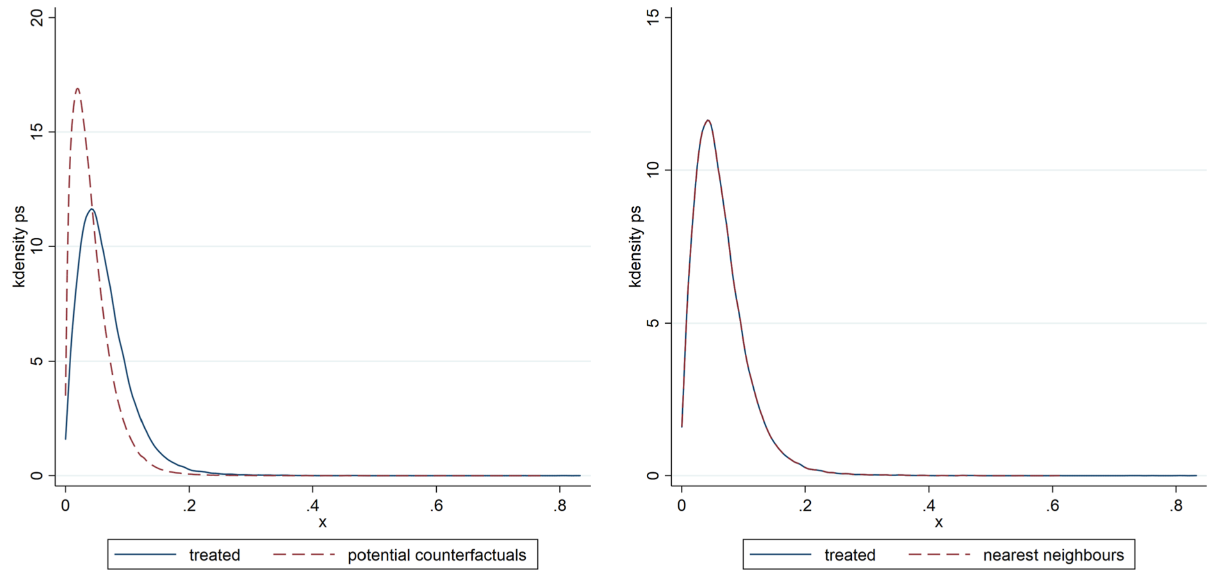

Fig. 1 Density plots of propensity scores before and after the matching. Fitted propensity scores from a probit model for the EIB loan beneficiaries ('Treated'), a full pool of potential controls ('Potential counterfactuals') and the matched controls ('Nearest neighbours')

comparison, and to satisfy the conditional exogeneity assumption.

4.2 Difference-in-differences

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

As data points $Y_i^0|T_i = 1$ are unobservable for the post-treatment period, we carry out the experiment on the pre-treatment period only.[19] Specifically, we estimate the following OLS model on the $t < 0$ sample for each outcome variable Y

$$Y_{it} = \alpha_0 + \alpha_1 t + \alpha_2 (t \times T_i) + \xi_i + \varepsilon_{it}, \qquad (2)$$

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

The results are depicted in Table 7. It can be readily observed that the coefficients capturing the interaction between time trend and treatment group are not significant for neither employment nor fixed assets. The evidence suggests therefore that the trends between treated and matched controls are parallel in the pre-treatment phase.

We calculate ATET in a linear regression framework. Under the assumption that the error term is conditionally mean-centered (or more precisely $\mathrm{E}[\varepsilon | I_{t>0}, T] = 0$), it can be verified that in the presence of unobserved country-sector-year time-invariant heterogeneity, the plug-in estimator of ATET matches the estimate of β_2 from the following panel regression

$$Y_{it} = \beta_1 I_{t>0} + \beta_2 (T_i \times I_{t>0}) + \nu_{cst} + \xi_i + \varepsilon_{it}, \qquad (3)$$

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

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

Table 5 Summary statistics

Unmatched controls						
	Obs.	Mean	Median	St. dev.	Min.	Max.
Leverage ratio	2,460,476	0.64	0.65	0.34	0.03	2.33
Employment (log)	2,460,486	2.59	2.48	1.24	0.69	6.04
Assets (log)	2,460,486	14.04	13.98	1.74	10.00	17.75
Cash ratio	2,460,476	0.14	0.07	0.17	0.00	0.78
Tangible ratio	2,460,476	0.28	0.21	0.25	0.00	0.94
Current ratio	2,453,597	2.43	1.37	3.87	0.09	31.02
Turnover ratio	2,367,320	1.63	1.31	1.29	0.02	7.34
Sales growth	2,290,178	0.10	0.02	0.50	−0.78	3.25
Patent (app)	2,460,486	0.01	0.00	0.10	0.00	1.00
Patent (pub)	2,460,486	0.01	0.00	0.11	0.00	1.00
Matched controls						
	Obs.	Mean	Median	St. dev.	Min.	Max.
Leverage ratio	149,109	0.68	0.69	0.29	0.03	2.33
Employment (log)	149,109	2.80	2.77	1.19	0.69	6.04
Assets (log)	149,109	14.36	14.36	1.63	10.00	17.75
Cash ratio	149,109	0.10	0.05	0.14	0.00	0.78
Tangible ratio	149,109	0.31	0.26	0.24	0.00	0.94
Current ratio	149,109	1.94	1.27	2.86	0.09	31.02
Turnover ratio	149,109	1.62	1.36	1.18	0.02	7.34
Sales growth	146,206	0.13	0.03	0.52	−0.78	3.25
Patent (app)	149,109	0.01	0.00	0.12	0.00	1.00
Patent (pub)	149,109	0.02	0.00	0.13	0.00	1.00
Matched treated						
	Obs.	Mean	Median	St. dev.	Min.	Max.
Leverage ratio	157,547	0.68	0.70	0.27	0.03	2.33
Employment (log)	157,547	2.81	2.71	1.19	0.69	6.04
Assets (log)	157,547	14.36	14.36	1.59	10.00	17.75
Cash ratio	157,547	0.10	0.04	0.13	0.00	0.78
Tangible ratio	157,547	0.31	0.26	0.24	0.00	0.94
Current ratio	157,547	1.82	1.28	2.45	0.09	31.02
Turnover ratio	157,547	1.62	1.35	1.16	0.02	7.34
Sales growth	154,992	0.13	0.04	0.50	−0.78	3.25
Patent (app)	157,547	0.01	0.00	0.12	0.00	1.00
Patent (pub)	157,547	0.02	0.00	0.13	0.00	1.00

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

Table 6 Balancing properties

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

Standardised percentage bias before and after matching in the 3-year pre-treatment period. Employment is measured as number of employees. Patents are measured as dummies if a company filled at least one patent application or publication in a given year. *p*-values correspond to the test of equivalence in means between the treated and control groups for a given variable

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

$$Y_{it} = \gamma_1 I_{t=1} + \gamma_2 I_{t=2} + \gamma_3 I_{t=3} + \gamma_4 (T_i \times I_{t=1}) + \gamma_5 (T_i \times I_{t=2}) + \gamma_6 (T_i \times I_{t=3}) + \nu_{cst} + \xi_i + \varepsilon_{it}, \quad (4)$$

Table 7 Assessment of common trends

	(1) Employment (log)	(2) Fixed assets (log)
Time trend × treated	−0.002	−0.002
	(0.002)	(0.003)
Time trend	0.033***	0.095***
	(0.001)	(0.002)
Const.	2.869***	13.222***
	(0.002)	(0.003)
Firm-level FE	Yes	Yes
Observations	306,656	305,756
R2	0.972	0.964

Estimation of differences in trends between treated and control groups in 3-year pre-treatment period. Employment is measured as number of employees. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels

where γ_4, γ_5 and γ_6 correspond to ATET for years $t = 1$, $t = 2$ and $t = 3$, respectively.[20]

In the last step, we shed more light on the potential underlying mechanism why the EIB-support can make a difference to the SME performance. In particular, we measure whether the EIB's impact was higher in countries with specific banking characteristics, as exemplified by the high interbank dependence ratio. We estimate a conditional ATET by extending the model in Eq. 3 to a triple DID framework, including interactions terms representing interbank funding conditions at a country level. Our baseline specification becomes

$$Y_{it} = \delta_1 I_{t>0} + \delta_2 (T_i \times I_{t>0}) + \delta_3 (I_{IB_{ct} > \bar{IB}_t} \times I_{t>0}) + \delta_4 (T_i \times I_{IB_{ct} > \bar{IB}_t} \times I_{t>0}) + \nu_{cst} + \xi_i + \varepsilon_{it}, \quad (5)$$

where $I_{IB_{ct} > \bar{IB}_t}$ is an indicator function equal to 1 if specific banking variable IB_{ct} is above the EU average $\bar{IB}_t$ in a given year, and 0 otherwise. It is important to note that this specification also absorbs the vector of country-sector-year fixed effects, hence reducing the possible cyclical distortions to the estimates.

[20] To ensure the robustness of the results, we expand the DID model into several directions. For details, see section 5.3 on robustness checks.

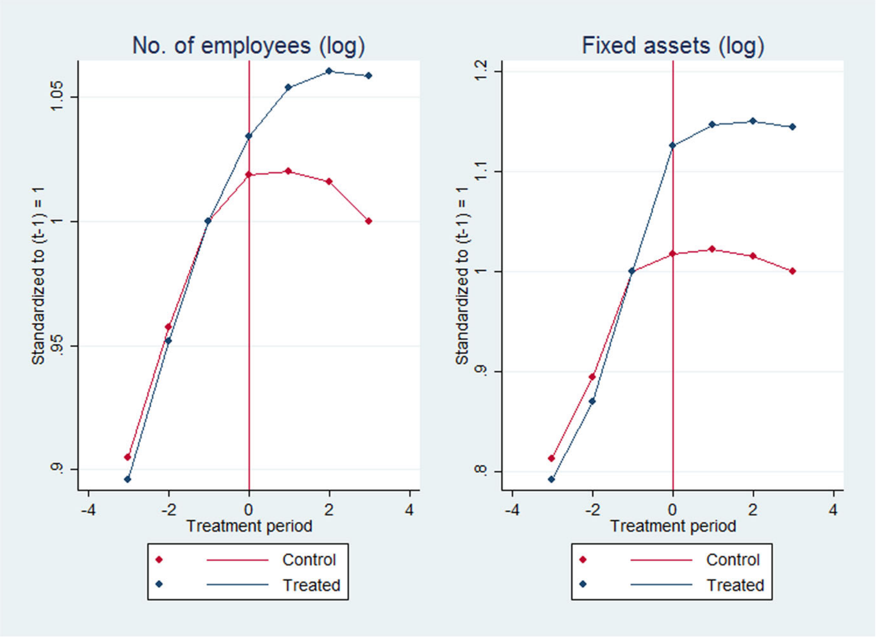

Fig. 2 Impact of EIB-supported lending to SMEs. Performance of EIB loan beneficiaries ('Treated') against the comparison group ('Control') in the 3 years before and after the loan allocation. The treatment year at $t = 0$ with standardised scale $t - 1 \equiv 1$

5 Results

5.1 Impact of EIB-supported lending

Our results indicate a significant and positive effect of EIB funding on employment and investment levels. The mean difference between treated and control firms is illustrated by Figure 2, which plots the pre- and post-allocation dynamics for the treated and control firms with respect to our two variables of interest. Tables 8 and 9 further present the estimation results of models given in 3 and 4.

The analysis suggests a significant positive effect on employment, expressed in the number of employees. Beneficiary firms exhibit on average by 5.5% higher employment numbers relative to the controls in the post-treatment period (column 1 in Table 8). Looking at the year-by-year impact, the difference in employment occurs gradually over the post-treatment years, as indicated by the estimated interaction coefficients in column 1 of Table 9. The positive pre-treatment employment trend suggests that firms in our sample — both in the treated and the control group — increased their employment in the pre-treatment period already. Upon receiving a loan, the positive effect on employment assures the possibility of adding new employees to the firm, and thereby continuing their growth. However, had these firms not received EIB funding, they would need to resort to stop hiring new staff, or even scale down their operation and decrease their employment levels.

Fixed assets also show a significant post-treatment difference relative to the control group. This is a variable that we consider as a good proxy for investment at the firm level. Column 2 of Table 8 indicate that the

Table 8 Impact of the EIB lending - main results

	(1) Employment (log)	(2) Fixed assets (log)
Post × treated	0.055*** (0.003)	0.142*** (0.005)
Post	−0.058*** (0.002)	−0.138*** (0.003)
Firm-level FE	Yes	Yes
Country x sector × year FE	Yes	Yes
Observations	665,630	665,997
R2	0.95	0.941

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

Table 9 Impact of the EIB lending — yearly decomposition

	(1) Employment (log)	(2) Fixed assets (log)
Post (1st year) × treated	0.033***	0.110***
	(0.003)	(0.004)
Post (2nd year) × treated	0.044***	0.119***
	(0.003)	(0.005)
Post (3rd year) × treated	0.058***	0.129***
	(0.004)	(0.007)
Post (1st year)	−0.055***	−0.137***
	(0.002)	(0.003)
Post (2nd year)	−0.101***	−0.233***
	(0.003)	(0.004)
Post (3rd year)	−0.160***	−0.340***
	(0.003)	(0.006)
Firm-level FE	Yes	Yes
Country × sector × year FE	Yes	Yes
Observations	665,630	665,997
R2	0.95	0.942

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

level of fixed assets are close to 15% higher for EIB beneficiaries. Similarly to the employment impact, the investments impact materialises gradually over time (Table 9). It can indicate that the increase in level of firm activity happens along with a proportionally higher accumulation of productive assets.[21]

One of the notable aspects of the results – illustrated by Fig. 2 – is that firms both in the control and the treatment group have shown significant employment and investment growth already in the years prior to the disbursement of the loan. For firms in the control group, the positive dynamics ends in the treatment year. For the treated, employment and investment growth continues after the treatment, albeit at a declining pace. Investment increases again with the treatment, then flattens out. It appears as if the treatment delayed the slowdown of growth of already fast-growing companies, rather than providing a new impetus to growth. The rapid pre-treatment growth can possibly be linked to the highly selective lending policies of banks in the post-crisis period: new loans might have been granted only to firms with strong track record. As to the post-treatment slowdown of both employment and investment growth, for firms in the control group, it could partially be related to a higher probability of their loan applications being rejected. For the firms in the treatment group, the deceleration of employment and investment growth can possibly be linked to the general post-GFC economic environment characterised with the marked slowdown of economic growth and investment in general. Testing the validity of these explanations could be an area for further research.

5.2 Vulnerability to funding shocks

As the next step, we expand our baseline results by investigating whether the positive impacts of EIB-funded intermediated lending on firms' performance depend on the exposure to funding shocks of the country where the firm is located.

Our sample period covers the aftermath of the GFC, during which a large number of banks suffered from wholesale funding shocks, as documented by de Haan et al. (2017), Iyer et al. (2013), Bremus and Neugebauer (2018), Alvarez et al. (2019) and De Jonghe et al. (2019). The same studies also point out that banks reacted to these funding shocks by a reduction of credit supply and an increase in the cost of credit to the corporate sector. Evidence also suggests that the deterioration of credit supply conditions was affecting proportionally more the small firms. The evidence also suggests that EU countries showed large heterogeneity in both the size of the funding shocks and the consequent deterioration in credit supply. de Haan et al. (2017) and Alvarez et al. (2019) point out that countries in the EU periphery, in general, were suffering larger funding shocks due to their stronger interbank dependence. As a consequence, the deterioration in credit conditions was exacerbated in these economies.

[21] Even though one cannot unambiguously account for the effects attributable to accounting mechanics (whereby taking up a loan increases the asset base), by controlling for leverage ratio and asset composition at the PSM stage, we believe this bias is minimised.

We ask whether EIB-supported funding produced more pronounced firm-level benefits in those EU countries that were exposed to larger wholesale funding shocks. We expect a stronger impact for beneficiary firms in such countries, as the availability of long-term and stable funding from the EIB would have brought more benefits for those financial intermediaries that otherwise have faced tighter wholesale funding conditions. In other words, the EIB-supported loans could have proportionally stronger advantages in volumes, costs and maturity, compared to other funding alternatives in countries where funding shocks were more severe. We verify if these proportionally better loan conditions could have translated into more pronounced differences in economic performance at firm level.

Following Iyer et al. (2013), Bremus and Neugebauer (2018) and De Jonghe et al. (2019), we use interbank dependence, measured as the share of interbank funding within total liabilities, as an indicator of vulnerability to funding shocks. To quote Bremus and Neugebauer (2018), higher interbank dependence can aggravate the funding situation of domestic banks in times of stress in the banking sector. This can lead to lower lending volumes and higher lending rates. Banking systems that rely less on interbank funding and more on other types of liabilities, like customer deposits, are funded more solidly and hence can offer less volatile lending rates. Interbank dependence can therefore be seen as a control variable for the wholesale funding situation of banks.

To test the hypothesis, we estimate the ATET differential for countries in which the interbank dependence ratio was above the EU-wide average ($I_{IB_{ct}} > \bar{IB}_t$). We use the model specification depicted in Eq. (5) as our benchmark specification (1). For robustness, we expand the model into two alternatives. In specification (2), we split the countries according to their pre-crisis interbank dependence ratio as being more representative of the GFC funding vulnerabilities, and less affected by the EIB intervention. In specification (3), we use a continuous interbank dependence ratio to allow for more variability in the interaction term. The regression results are given in Table 10.

The results confirm our hypothesis by showing that the beneficiaries of EIB-loans performed significantly better in terms of employment and fixed assets dynamics compared to the control group in the countries characterised by higher exposure to funding shocks. The difference amounts to close to 8 percentage points for employment, and 18 percentage points for fixed assets, which indicates not only statistical, but also economic significance.

Our findings indicate therefore a proportionally larger effect of intermediated lending supporting SMEs by public financial institutions in the presence of larger exposure to wholesale funding shocks. In this respect, our empirical framework confirms the theoretical results of Eslava and Freixas (2021), namely that funding shocks provide additional rationale for public intervention through intermediated lending.

5.3 Robustness

To confirm the validity of the results against a range of different modelling specifications, we turn to several robustness checks. Altogether, these tests are supportive of our earlier findings and they confirm the stability of our results with respect to various modelling assumptions. For the sake of brevity, we show here the robustness checks only for our baseline DID specification discussed in Section 5.1.

Balanced panel of firms The properties of the fixed-effects estimators may be affected by the data composition. For instance, if the missing observations in an unbalanced panel are a result of a non-random effects, the data structure may contain a selection bias. Alternatively, if the sample excludes the firms which do not report for all the years, the data may include a survivorship bias. To better benchmark the results, we complement the main results by the results obtained from the same model but on a balanced panel of firms in column 1 of Table 11.

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

Cluster-robust estimation Bertrand et al. (2003) point out that the firm-level DID estimator uses observations of the same entity over multiple time periods. Traditional DID estimators do not necessarily account for the resulting serial correlation of the error term, and as a consequence, regression standard errors may be underestimated. To overcome this difficulty, instead of using multiple yearly observations for both the pre- and the post-treatment periods for the same firm,

Table 10 Impact of the EIB lending — by country-wide banking conditions

	1		2		3	
	Interbank ratio (dummy based on *t*)		Interbank ratio (dummy based on 2007–2008)		Interbank ratio (continuous)	
	Employment (log)	Fixed assets (log)	Employment (log)	Fixed assets (log)	Employment (log)	Fixed assets (log)
High IB × post × treated	0.076***	0.184***	0.075***	0.177***		
	−0.013	−0.02	−0.013	−0.02		
High IB × post	−0.069***	−0.189***	−0.066***	−0.169***		
	−0.009	−0.014	−0.009	−0.014		
IB × post × treated					0.002***	0.007***
					−0.001	−0.001
IB × post					−0.006***	−0.015***
					−0.001	−0.001
Post × treated	0.046***	0.119***	0.046***	0.120***	0.023**	0.046***
	−0.003	−0.005	−0.003	−0.005	−0.01	−0.017
Post	−0.050***	−0.118***	−0.051***	−0.120***	0.028***	0.066***
	−0.002	−0.003	−0.002	−0.003	−0.007	−0.012
Firm-level FE	Yes	Yes	Yes	Yes	Yes	Yes
Country × sector × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	639,663	640,076	620,007	620,499	639,663	640,076
R2	0.950	0.942	0.949	0.942	0.95	0.942

Estimation results of the main treatment effects model by country-wide banking conditions, exemplified by the interbank ratio (IB). Column 1 includes a dummy classifier whereby countries with above-average values in a given treatment year are tagged as 'High', as specified in the text. Column 2 uses a similar dummy classifier yet based on the average IB ratio in years 2007–2008. Column 3 utilises a continuous IB ratio. Employment is measured as number of employees. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels

Table 11 Impact of the EIB lending — robustness

	(1) Balanced panel		(2) Standard errors' correction		(3) Placebo treatment	
	Employment (log)	Fixed assets (log)	Employment (log)	Fixed assets (log)	Employment (log)	Fixed assets (log)
Post × treated	0.053***	0.131***	0.071***	0.156***	−0.001	0.001
	(0.003)	(0.006)	(0.003)	(0.005)	(0.002)	(0.004)
Post	−0.050***	−0.112***	−0.007***	0.057***	0.045***	0.147***
	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)
Firm-level FE	Yes	Yes	Yes	Yes	Yes	Yes
Country × sector × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	529,675	530,312	201,608	202,288	306,280	305,248
R2	0.949	0.941	0.963	0.957	0.971	0.964

Robustness results for the main treatment effects model on the balanced panel of firms (1), corrected for the serial correlation of error terms (2) and for the placebo treatment, with simulated treatment in period $t - 2$ (3). Employment is measured as number of employees. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels

Bertrand et al. (2003) propose to use the average of the outcome variables before and after the treatment and run the DID regressions on these averaged outcome variables. We follow this advice and the results with the serial-correlation-robust standard errors are given in column 2 of Table 11.

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

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

We design a simple placebo scenario, where we assume that the treatment happens in period $t = -2$. To exclude the actual treatment period, we run the core model on the pre-treatment sample only ($t < 0$), effectively comparing the levels of the outcome variables in periods $t = -3$ and $t = -1$. The results of this experiment are given in column 3 of Table 11.

The findings suggest that the model design unambiguously passes the placebo test as for none of the outcomes we observe statistically significant treatment effects in period $t = -2$.

Data attrition We show in Section 3.3 that a significant proportion of our initial observations drops out while we merge various data sets. The reasons include unsuccessful matching of beneficiary company names with Orbis records, and missing data in Orbis for already matched companies. To correct for the data attrition bias, we use inverse probability weights (IPWs). IPW is a technique widely used to correct for non-response in surveys, which re-establishes the statistical properties of the original population with respect to some observed variables.

To generate the weights, we stratify our allocation data set along the same dimensions as used for the construction of the pool of potential controls for the PSM model (see Section 3.4). They include country, allocation year, number of employees and industry classification. We then calculate the number of firms in each cluster before and after data attrition, and measure the shares of firms that survive the procedure. The IPWs are the inverse of these shares. To demonstrate the properties of the re-weighted sample, we compare it against the original data, as reported in the EIB allocation tables. We aggregate the figures by three broader country groups to offer a more concise picture of the outcome. The numbers are reported in Table 12.

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

By including the IPWs as estimation weights in Eq. (3), we re-weight the evidence of ATET contained in each observation to match the strata distribution of the original allocation database. While the findings fully hold for the whole range of weights, to improve the properties of the OLS estimators, we focus on strata for which the weights are below 50, i.e. there were more than 2% of initial firms that survived the data tuning procedures. The IPW-weighted estimation results are given in Table 13.

The main findings remain statistically significant under this specification, too. Interestingly, the magnitude of the coefficients marginally decreases, yet it remains positive and statistically significant.

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

Table 12 The properties of re-weighting

Number of firms

	Total EIB	With useful data		Re-weighted	
	(in #)	(in #)	(in %)	(in #)	(in %)
Central and East Europe	105,540	12,228	11.59	90,285	85.55
South Europe	233,093	36,289	15.57	228,190	97.90
West and North Europe	65,155	4974	7.63	46,631	71.57
EU-wide	403,788	53,491	13.25	365,106	90.42

Average loan amounts

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

'Total EIB' represents number of firms and average amounts as reported in the EIB allocation database, 'with useful data' describes number of firms and average amounts with sufficient data coverage to be included in the PSM and 're-weighted' shows number of firms and average amounts re-weighted using the IPW weights. Shares represent the proportion of Total EIB. Central and East Europe countries include Bulgaria, Croatia, Czech Republic, Hungary, Latvia, Poland, Romania, Slovakia and Slovenia; South Europe covers Greece, Italy, Portugal and Spain; and West and North Europe spans over Austria, Belgium, Finland, France, Germany, Luxembourg, Netherlands, Sweden and the UK

Table 13 Impact of the EIB lending — IPW-weighted results

	(1)	(2)
	Employment (log)	Fixed assets (log)
Post × treated	0.040***	0.079***
	(0.009)	(0.014)
Post	−0.056***	−0.137***
	(0.008)	(0.013)
Firm-level FE	Yes	Yes
Country × sector × year FE	Yes	Yes
Observations	340,046	339,103
R2	0.955	0.944

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

a more careful approach to address this problem is desired.

Similar concerns have been flagged by, for instance, Brown and Earle (2017). They point out that treated firms may have experienced idiosyncratic demand, productivity or cost shocks, precisely during the treatment year. If positive, such shocks can raise demand or productivity, and if negative they can increase cost burdens. In any case, they may motivate firms to seek external financing either to increase production or to stay alive. While Brown and Earle (2017) develop an identification method based on instrumental variables, where instruments correspond to some of the observed characteristics of loan-granting banks, linking control firms with relevant banks in our setup drastically reduces the sample size.[22] Hence, we propose an alternative strategy.

We address this drawback at the selection into treatment stage. Specifically, in a follow-up exercise, we

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

Table 14 Impact of the EIB lending — loan demand correction

	(1) PSM with leverage in $t = 0$		(2) PSM with Δ leverage in $t = 0$	
	Employment (log)	Fixed assets (log)	Employment (log)	Fixed assets (log)
Post $\times$ treated	0.057***	0.150***	0.058***	0.127***
	(0.003)	−0.005	−0.003	−0.005
Post	−0.058***	−0.141***	−0.057***	−0.118***
	(0.002)	−0.003	−0.002	−0.003
Firm-level FE	Yes	Yes	Yes	Yes
Country $\times$ sector $\times$ year FE	Yes	Yes	Yes	Yes
Observations	662,787	663,366	543,155	544,044
R2	0.949	0.941	0.952	0.945

Estimation results of the main treatment effects on the sample matched on an extended PSM model with leverage ratio in $t = 0$ (1), and matched on an extended PSM model with a change in leverage ratio in from $t = -1$ to $t = 0$ (2). The leverage ratio is calculated as a share of total debt to total assets. Employment is measured as number of employees. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels

select only those firms from the pool of all potential counterfactuals that adjusted their balance sheet structure in the treatment year in a manner consistent with taking up a loan. For instance, such a signal can be inferred from deteriorating indebtedness metrics. By imposing this additional constraint in the propensity score model, we can ensure that firms in the control group exhibited loan demand, and obtained external finance.[23]

Thus, we adjust the PSM model, in Eq. 1, by controlling for the level of leverage ratio at time $t = 0$, with the corresponding squared and cubic terms. To ensure that we pair control firms who took up external finance in $t = 0$, in the alternative specification, we adjust the PSM model for a change in leverage ratio. The leverage metric is calculated as the ratio of total debt to total assets, but the results fully hold for (i) total debt excluding trade credit and other liabilities (financial leverage) and (ii) total debt excluding trade credit and other liabilities as well as cash and cash equivalents (net financial leverage). The matching technique and the following DID regressions remain the same as in Section 4. The estimation results corrected for the demand effects are presented in columns 1 and 2 of Table 14.

Again, the effects on the main variables of interest remain virtually unchanged against the results in Table 8, at the same significance level. This suggests that the estimated impact is not a result of idiosyncratic differences between available project ideas or investment-financing strategies, but it is linked to the treatment itself.

6 Conclusions

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

Our results indicate that firms that benefited from EIB-supported lending performed better both in terms of number of employees and in terms of investment — measured as fixed assets dynamics — compared to a

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

control group of similar firms with no EIB-supported loans. These effects are both economically and statistically significant, and robust to alternative modelling specifications.

We also find that the positive impact of EIB funding is substantially stronger in those EU countries where the system of financial intermediation is more reliant on wholesale funding. We argue that such a feature increases banks' exposure to funding shocks, as exemplified by the GFC when wholesale-funded banks curtailed credit supply to firms more than banks with stable funding.

Overall, we conclude that EIB lending, during the period in question, made a difference. Conditional on data and methodological constraints, which we try to address in the best possible manner, our results provide support to the view that EIB funding supported employment and investment of SMEs across the EU countries in the aftermath of the GFC. Our results suggest that this beneficial effect has been at least partially associated with the particular funding situation that EU financial intermediaries, as the EIB support instrument was able to mitigate in part the impact of strong wholesale funding shocks on credit supply.

In this respect, our findings give support to public sector intervention to credit markets, and complements both the existing firm-level evidence on the benefit of credit guarantees (for instance Brown and Earle, 2017) by highlighting the beneficial role of intermediated lending instruments, in line with the results of the theoretical model of Eslava and Freixas (2021). The results also point out the possible useful role of such instruments in those financial downturns that are characterised by funding shocks affecting the financial system. These results are informative from a public policy perspective.

Our analytical framework have certain limitations. We consider only direct beneficial impact on firms, and our partial analysis does not take neither the second-round effects, nor the costs of the interventions into account, let alone overall welfare effects to the economy. An interesting possible extension of our research is therefore to explore the problem of publicly supported intermediated lending support in a macroeconomic framework that is able to consider the impact of these policies on a broad economy. Another possible avenue for future research is to consider the interaction between lending support by public banks and other financial policies, such as monetary and macro-prudential policies. In particular, in would be informative to explore how interventions of the separate public entities, such as the central bank and a public financial institution could amplify or attenuate the intended economic effects of each other.

We also recognise that our empirical methodology could be improved in several dimensions. Firstly, to strengthen the causal narrative, one could explore an alternative model specification whereby the treatment exogeneity is further supported by instrumental variables. While Brown and Earle (2017) suggest to use geographical variation in treatment availability as possible instruments, our current data structure does not allow for such a rich specification. A more detailed analysis with a view on credit registry and/or detailed firm-level funding structures could offer a natural avenue for future research.

Another natural methodological extension of this study is to look at term-sheet features of the EIB-supported loans, as potentially contributing to the demonstrated positive effect. While we cannot give a definite answer yet, an early-stage analysis of contractual details points to a direction that the benefits are mostly associated with long maturity profiles and attractive pricing, rather than specific loan volumes. Moreover, it would be interesting to investigate to what extent the magnitude of the transferred financial advantage affects the results. In this respect, a continuation of our setup could include a dose-response framework or Conditional Average Treatment Effects (CATE) models.

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Appendix. Analytical framework

In empirical analyses of cause and effect, it is critical to measure the impact relative to the appropriate counterfactual. In our case, what we would like to measure is the difference between the mean performance of the

EIB-funded firms, and the mean performance of the same firms, had they not been beneficiaries of an EIB loan.

We approach this problem with a two-step procedure. In the first step, we construct the matched (treated and non-treated) sample by the propensity score matching (PSM). In the second step, conditional on the validity of the propensity scores, we estimate the Average Treatment Effects on the Treated (ATET) in difference-in-differences (DID) framework on the matched sample. As our data are longitudinal, the DID estimator allows us to control for unobserved confounders, as long as they remain constant over time.

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

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

Technical details

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

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

$$Y_i = Y_i^1 T_i + Y_i^0 (1 - T_i). \tag{6}$$

Equation (6) is called the stable unit treatment value assumption (SUTVA) and it implies that the potential outcome of one firm is not affected by the treatment assignment of other firms. Given the large multinational sample of the EIB beneficiaries, we would argue that the potential cross-border bias resulting from violation of SUTVA is rather limited. We cannot eliminate the possibility of the domestic cross-sector spillovers, however, which are a result of unobservable direct and indirect effects. We believe, however, that the due diligence process and the rule book of the EIB-support work in the advantage of the SUTVA principle.

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

$$\text{ATET} = \text{E}\left[Y^1 - Y^0 | T = 1\right], \tag{7}$$

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

$$\text{ATE} = \text{E}[Y|T = 1] - \text{E}[Y|T = 0], \tag{8}$$

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

$$\text{ATE} = \text{ATET} + \mathrm{E}\left[Y^0|T=1\right] - \mathrm{E}\left[Y^0|T=0\right]. \quad (9)$$

The bias term reflects the problem that selection into treatment may depend on potential outcomes. Looking at the problem through a prism of the EIB support, it could be that firms receive EIB-backed loans simply because they happen to be on a faster growth path than other firms, for instance. In that case comparing the treated and non-treated group, averages would likely overestimate the causal effect of the EIB support as even without the EIB support, these firms would display better performance $\mathrm{E}[Y^0|T=1] > \mathrm{E}[Y^0|T=0]$.

Having pointed this out, the second assumption in this study requires that the selection bias is negligible (it is often called the unconfoundedness or exogeneity assumption). Even though the bias term is non-zero in most applications, the problem can be addressed by studying and controlling the assignment mechanism. Randomised controlled trials offer a natural solution to the selection bias, as the under the random treatment assignment, the treated and non-treated units will be similar across all the characteristics, including the unobservable Y^0. In other words, correctly designed randomised trials impose that the potential outcome variables are independent of the treatment assignment, such that $(Y^1, Y^0) \perp\!\!\!\perp T$. As bank loans are not allocated in the form of randomised trials, we control the selection bias by selection on observables.

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

$$\mathrm{E}\left[Y^1 - Y^0|X\right] = \mathrm{E}[Y|X, T=1] - \mathrm{E}[Y|X, T=0]. \quad (10)$$

Furthermore, under the requirement of common support, i.e. $0 < \mathrm{P}(T=1|X) < 1$ with probability one,[24] it follows that

$$\text{ATET} = \int \left(\mathrm{E}[Y|X, T=1] - \mathrm{E}[Y|X, T=0]\right) \mathrm{dP}(X|T=1), \quad (11)$$

where P stands for probability distribution. Equations (10) and (11) imply that the ATET can be estimated by comparing the sample of treated firms to the sub-sample of non-treated ones with the same characteristics X, which can be achieved by matching techniques. Rosenbaum and Rubin (1983) extend the result from Eq. (10) and show that under selection on observables assumption, it holds that

$$\mathrm{E}\left[Y^1 - Y^0|p(X)\right] = \mathrm{E}\left[Y|p(X), T=1\right] - \mathrm{E}\left[Y|p(X), T=0\right], \quad (12)$$

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

Propensity score matching (PSM) creates a control group among non-treated firms which at the time of the treatment are identical to treated firms with respect to observable characteristics.[25] Thus, for a given set of observable characteristics, receiving an EIB-backed loan should be "as good as random".

PSM is only able to account for observable characteristics when addressing the selection bias of the treatment group. However, treated and non-treated firms might differ with regard to unobservable confounders which (i) are not perfectly correlated with observables, (ii) are correlated with observables which are unbalanced between the treated and non-treated firms, and (iii) are important for testing the proposed

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

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

theory of change. To address these issues, firstly, we verify the matching validity by checking the corresponding balancing properties, and secondly, we exploit the time dimension of our data set to control for certain unobserved factors.

More specifically, let us standardise the treatment year for each EIB beneficiary to $t = 0$.[26] Consequently, the pre- and post-treatment periods we separate by an indicator function $I_{t>0}$, which takes value 1 if $t > 0$ and 0 otherwise. We also define the potential outcomes under treatment and no-treatment for the pre- and post-treatment periods as $Y_i^1(I_{t>0})$ and $Y_i^0(I_{t>0})$, respectively, and we note that the predetermined observed characteristics are valid only for the pre-treatment period, i.e. $X_i \equiv X_i(0)$. It follows that the post-treatment ATET becomes

$$\mathrm{ATET}(1) = \int \mathrm{E}\left[Y^1(1) - Y^0(1)|p\left(X(0)\right), T = 1\right] \mathrm{dP}(X|T = 1). \tag{13}$$

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

$$\begin{aligned} &\mathrm{E}\left[Y^0(1) - Y^0(0)|p\left(X(0)\right), T = 1\right] \\ &= \mathrm{E}\left[Y^0(1) - Y^0(0)|p\left(X(0)\right), T = 0\right]. \end{aligned} \tag{14}$$

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

$$\begin{aligned} \mathrm{ATET}(1) = &\left[\mathrm{E}[Y(1)|p\left(X(0)\right), T = 1]\right. \\ &\left.-\mathrm{E}[Y(1)|p\left(X(0)\right), T = 0]\right] \\ &- \left[\mathrm{E}[Y(0)|p\left(X(0)\right), T = 1]\right. \\ &\left.-\mathrm{E}[Y(0)|p\left(X(0)\right), T = 0]\right]. \end{aligned} \tag{15}$$

In fact, Eq. (15) can be estimated in a two-step approach. In the first step, we construct the matched (treated and non-treated) sample by PSM, as described above. In the second step, conditional on the validity of the propensity scores, we estimate Eq. (15) by linear regression in a difference-in-differences (DID) framework on the matched sample. As our data are longitudinal, the DID estimator allows us to control for unobserved confounders, as long as they remain constant over time. Throughout the paper, we refer to ATET(1) as simply ATET.

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

Variable selection into PSM

Our strategy to select the variables and their transformations in the PSM consists of 3 steps. In the first step, we determine which financial characteristics in time $t - 1$ may be relevant for explaining the probability of receiving the EIB treatment. While there is no golden standard in this respect, we closely follow the related literature (see Asdrubali and Signore (2015) and Gereben et al. (2019)). For these variables, we carry out a rule-of-thumb multicolinearity assessment through the Variance Inflation Factor (VIF). All the main variables meet the rule-of-thumb tolerance of 1/VIF above 0.1, as reported in Table 15.

In the second step, we extend the set of PSM variables to cover periods $t-2$ and $t-3$, to better capture the dynamics over time and to better align the matched controls with the pre-treatment trends. In the third step, we add the squared and cubic terms. This step aims to improve the goodness of fit of the model, and eventually allows us to match the control firms for which the possible similarities to the treated entities are only visible in nonlinear variable space.

Steps two and three can result in some collinearity; however, we believe that at this stage, they are balanced out by improved goodness of fit and the possibility to find better control firms. To ensure the stability of the PSM method, we carry out several robustness checks whereby we confirm the main paper results on a matched sample without the squared nor cubic terms in the PSM model. In Table 16, we display the results on the employment and fixed assets dynamics from such a specification. Results are virtually unchanged.

Table 15 Multicolinearity assessment of the PSM model variables

	VIF	1/VIF
Leverage ratio (lag)	1.41	0.708
Employment (log, lag)	6.92	0.144
Total assets (log, lag)	3.7	0.27
Patents (lag)	1.06	0.943
Cash ratio (lag)	1.39	0.719
Tangible assets ratio (lag)	1.33	0.753
Current ratio (lag)	1.31	0.765
Turnover ratio (lag)	1.66	0.601
Sales growth (lag)	1.07	0.935

Table 16 Impact of the EIB lending — main results using PSM model without squared and cubic terms

	(1) Employment (log)	(2) Fixed assets (log)
Post × treated	0.060***	0.144***
	(0.003)	(0.005)
Post	−0.060***	−0.131***
	(0.002)	(0.003)
Firm-level FE	Yes	Yes
Country × sector × year FE	Yes	Yes
Observations	669,011	669,387
R2	0.966	0.962

Estimation results of the main treatment effects model using PSM specification without squared and cubic terms. Employment is measured as number of employees. Patent applications are measured as a dummy depending if a company filled at least one patent application in a given year. Standard errors clustered at the firm level in parentheses. Significance codes: *** for 0.01, ** for 0.05 and * for 0.1 levels

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