Is the EIB Distorting Competiton? Evidence from Portugal*

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Abstract

The European Investment Bank finances projects of medium and large sized firms through "project loans". Because they involve favorable conditions such as low interest rates and a long repayment period, there is a question of whether the EIB could be distorting market competition and reinforcing eventual dominant positions. This paper aims to empirically test this hypothesis. Using data from Portuguese firms between 2007 and 2015, we use an empirically robust estimator to assess the impact of project loans on firms' market shares, which proxies market power. As a byproduct of this research, we also investigate the impact on firms' investment rate. We find no evidence that assisted firms experience abnormal increases in their market shares in the years that follow the attribution of the loans. Thus, we fail to reject the hypothesis that project loans do not distort market competition.

Keywords: European Investment Bank, Competition, Impact Evaluation

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1 Introduction

According to the Article 309 of the Treaty on the Functioning of the European Union, the mission of the European Investiment Bank (EIB) is to contribute to the "balanced and steady development of the internal market in the interest of the Union" by granting loans on a not-for-profit basis for projects promoting regional development, modernizing undertakings and of overall common interest to several Member States.

In practice, this mission materializes in a variety of goals. From promoting convergence among Member States, developing the transport networks, or accelerating the transition to a green economy, a broad spectrum of objectives could justify the bank's intervention. Honohan (1995) argues that the main public policy role of the EIB is to stimulate bank competition and efficiency in regions where the local banking system is costly and inefficient. The role of the bank of financing large projects when significant amounts of capital are difficult to raise is also at the root of its creation.

Although the EIB lending activity has the potential to mitigate certain market distortions, it can create others. It's attractive conditions, which often include low interest rates and a long repayment period, could distort competition in goods and services markets by giving the eligible firms an advantage over their rivals. If the EIB activity resulted in artificial advantages for some large firms, this would be inconsistent with Article 140 of the Financial Regulation of the EU (Regulation 966/2012), which states that the EU financial instruments should only be used in a way that does not distort competition in the internal market.

Several studies have been conducted by the EIB's Evaluation Department in regard to the ex-post evaluation of the bank's lending, both on a microeconomic level (Thommson and Goodwin, 2005) and on a macroeconomic level (EIB, 2018). To our knowledge, studies aiming at evaluating the impact of the EIB on market competition have never been made. This work project aims to give a first result to this matter by measuring whether the beneficiary firms from project loans, which are large by Portuguese standards, experience abnormal increases in their market shares, which proxies market power. As a byproduct of this research, we also investigate the impact on firms' investment rate. We focus our analysis in Portugal, over the period from 2007 and 2015, with a sample comprising 45 project loans amounting to 3.927M euros disbursed by the bank. In the estimation, we

use a difference-in-differences matching estimator (Heckman, Ichimura and Todd, 1997) that controls for observed and unobserved time-invariant selection bias.

This work project is structured as follows. Section 2 provides information about the EIB activity in Portugal. Section 3 contains a literature review. Section 4 explains the methodology used. Section 5 presents the dataset and variables used, together with descriptive statistics. Section 6 describes de empirical findings and section 7 summarizes our results.

2 Background

The European Investment Bank (EIB), often referred to as the EU bank, was stablished in 1958 under the Treaty of Rome. It is a long-term credit bank that offers attractive rates and specializes in the financing of infrastructure and other fixed capital. The EIB signed its first project in Portugal in April 1976, funding the construction of a thermal power plant in Setúbal. It was after Portugal officially joined the EU that its relationship with the EIB became more intense. Ever since, the EIB has been an active player in Portugal.

Being a cohesion country, Portugal is one of the EU Member States primarily targeted for convergence policies. Also, all Portuguese regions apart from Lisbon and Algarve are classified as "less developed regions" [1] with regards to the to the EU Regional Policy from 2014 to 2020. Accordingly, Portugal is one of the countries where EU economic and social cohesions objectives are most significant, thus explaining the strong actuation of the EIB. In 2017, the EIB loans to Portugal ammounted to 9.9% of the its GDP (Maurin and Santos, 2018), well above the EU average exposure of 3.4% of the GDP. Nonetheless, other objectives justify the EIB intervention in Portugal. From accelerating the transition to a green economy, promoting connectivity among Member States with more developed transport networks, or increasing the reliability of the energy supply, a broad spectrum of projects in Portugal could be financed by the bank.

Honohan (1995) argues that the main public policy role of the EIB is to stimulate bank competition and efficiency in regions where the local banking market is inefficient. The EIB internvetion has the potential to directly lower the funding costs of valuable

[1] By having a GDP per capital smaller than 75% of the EU27 average.

projects, and also to induce efficiency in the domestic banking market through increased competition. The role of the bank of financing large projects when significant amounts of capital are difficult to raise is also at the root of its creation.

The EIB categorizes its lending activity into different types of loans designed to reach different kinds of entities. For instance, "intermediated loans", conducted through local banks, are designed to support essentially small-and-medium-sized businesses. Support to larger firms is mainly made throught "project loans". Only midcaps and large corporates are eligible for these loans which typically have a minimum lending value of around 25M euros that should cover a maximum of 50% of the investment costs. In addition to a long replacement period which can be as high as 30 years, project loans often include interest rates lower than the market would generally require. The latter type of loans will be the one analyzed in this work project.

3 Literature Review

Previous work has been done to quantify the impact of the EIB overall activity. At the macroeconomic level, a recent study (EIB, 2018) estimates the bank's impact through a dynamic spatial computable general equilibrium model, finding that the banks' activity within the EU between 2015 and 2016 will increase GDP by 2.3% and create 2.25 million jobs by 2020.

At a microeconomic level, the bank's Evaluation Department conducts ex-post evaluation studies divided in distinct categories. "Technical studies" such as the analysis on several Public–Private Partnership (PPP) projects (Thommson and Goodwin, 2005), evaluate the projects financed by the bank based on performance indicators and KPIs. "Thematic studies" analyze the developments in specific objectives of the EIB. For example, a study was made regarding the EIB's climate action activity within the EU between 2010 and 2014 where the bank's overall lending contributions to renewable energies projects was assessed (EIB, 2015).

To our knowledge, studies aiming to evaluate the impact of the EIB activity on competition have never been made. This work project aims to give a first result to this matter by measuring whether firms that receive project loans experience abnormal increases in

their market shares, which proxies market power. To allow for a better understanding of the loan's impact, the effect on investment is also assessed.

More generally, there are a number of studies that access the firm-level impact of government lending programs on firm size. Maggioni et al. (1999) analyzes an Italian government program comprising loans with reduced interest rates and found no impact on firm's size, measured as sales. Similiar results were found by Kim et al. (2015), who analyzed a Korean government lending program designed to support R&D. Kang and Heshmati (2008) studied the impact of another Korean credit guarantee program on firm performance and found no impact on firm's sales. Dvouletý (2017) analyzed a Czech program supporting enterprises with zero interest soft loans and credit guarantees and found a negative relation on firm's sales. Other studies found a positive impact of government lending programs on firm size. Zecchini and Ventura (2009) analyzed the impact of an Italian credit guarantee scheme on firm performance and found a positive influence on firm's sales. Garcia-Tabuenca and Crespo-Espert (2008) study a Spanish credit-guarantee program and reach the similar results.

The aforementioned papers focus on the impact on firms' sales, ignoring their position in the final market. In our analysis, as the firms that receive project loans are large, an otherwise positive result of a government program that yields a positive effect on firms' size could translate into higher levels of market power, potentially even creating or sustaining dominant positions. For this reason, we focus our analysis not on the impact of project loans on the absolute size of the firms, but on their relative size in respect to their industry.

Finally, a choice has to be regarding the empirical method used in our analysis. Different kinds of estimators are employed by the literature to evaluate the firm-level impact of government financial assistance programs. Three of the most widely used are Heckman-type models, fixed effects estimators and matching estimators. When only cross-section data is available, Heckman-type models (Heckman, 1976) are used in an attempt to control for potential endogeneity caused by selection bias. Faulk (2002) and Kraybill and Gabe (2002) both use a Heckman's two-step model that first estimates the propensity to participate in the government program, and then estimates the impact of the program on firm employment taking into account that participation in the program is likely en-

dogenous. When panel-data is available, fixed-effects estimators are often conducted to account for time-invariant unobserved endogeneity. Dimitris (2004) uses a fixed-effects model to determine the impact of capital subsidies on several firm performance indicators and is able to account for the selection bias caused by observable and unobservable firm characteristics that are constant over time.

Matching methods, promoted by Rubin (1974), have become an increasingly popular alternative to deal with selection bias as they do not require any functional form of dependency between the outcome variable and the covariates. These methods can yield accurate estimations even when only a few units of the control group are comparable to the treated units (Dehejia and Wahba, 2002). The panel-data version of matching, suggested by Heckman, Ichimura and Todd (1997), combines Matching with Differences-in-Differences (MDID). This method allows to control for unobserved systematic differences between treated and untreated units, as long as they are time invariant. Smith and Todd (2005) found that, by eliminating potential sources of temporally-invariant bias, MDID estimators can perform substantially better than standard matching and various traditional regression-based estimators. As only a small set of specific firms are eligible to receive project loans, we employed a MDID estimator that restricts the analysis to the most similar firms of the control group.

4 Methodology

4.1 Market Shares

Market power relates to the firm's ability to sustain prices above marginal costs. Since marginal costs are not observable, estimating firms' market power is empirically challenging. One of the most widely used indicators of market power is market shares. In fact, the classical procedure of competition authorities to assess market power is to start by defining the relevant market, and subsequently measuring the market shares held by the firms. The higher the market share of a firm has, the higher the market power it is presumed to have (Mota, 2004).

The conventional way to measure market shares is the following:

$$S_i = \frac{Sales_i}{\sum_{i=1}^{N} Sales_i}, \tag{1}$$

where $Sales_i$ represents the total sales (or turnover) of firm i, and N stands for the total number of firms in the market of firm i.

There are certain methodological limitations when measuring market shares. To compute it, the relevant market needs to be correctly defined, which often requires a case-by-base analysis. Furthermore, information about all the firms operating in the defined market is needed: in markets exposed to international trade, the relevant market is unlikely to coincide with the internal market and information about foreign competitors would be required. In this work project, we follow Amador and Soares (2012) assuming the relevant geographic market of the firms being analyzed to be the Portuguese internal market. Furthermore, each relevant product market is assumed to be delimited by the Statistical Classification of Economic Activities in the European Community (NACE) Rev.2 at the 3-digit level. Since this assumption implies that each firm competes in only one market, multi-product firms might be a source of bias in our analysis. The use of statistical industry classifications to define markets is standard in cross-market studies due to the availability of data (Du and Chen, 2010).

4.2 Empirical Method

We are interested in analyzing the effects from receiving the treatment (i.e. a project loan). This effect can be captured by comparing the "factual performance" with the "counterfactual performance". The difficulty lies in finding a valid counterfactual.

In the context of a randomized controlled trial (RCT), where some indivudals are randomly assigned to receive a certain treatment, the difference in the outcome of interest between treated and untreated individuals can be attributed to the impact of the treatment. With a large enough sample size, differences in the factors that affect the outcome between the two groups will be averaged out and both groups will be essentially equivalent - apart from the fact that one received the treatment, and the other did not.

However, when participation in the treatment is not random, the conclusions are not as straightforward. If some types of individuals are more prone to receiving the treatment

than others, then the treated and the control groups might end up being fundamentally different, and the differences in the outcome variables of the groups might not be explained by the treatment itself, but by other characteristics that affect the outcome variable and differ across groups.

In our analysis, this is likely to be the case: neither is the decision to apply for a project loan random, nor is the decision of the bank to grant the financing. For instance, if only less leveraged firms receive project loans, then simply comparing the increase in market shares of treated and untreated firms after the attribution of the loans might not give us the effect of the loans, but also the influence of leverage on the firms' market shares. Not accounting for this would mean that our results would be inaccurate as there would be no internal validity.

Rubin (1974) models the issue by framing the effect of the treatment in terms of potential outcomes. For each firm i, let $Y_i(1)$ denote the potential outcome of firm i when exposed to the treatment, and $Y_i(0)$ the outcome of firm i when not exposed to it. If both $Y_i(0)$ and $Y_i(1)$ were observable, the effect of the treatment on firm i would be simply the difference between the two scenarios: $Y_i(1) - Y_i(0)$, and the average treatment effect on the treated (ATT) would be defined by:

$$ATT = E[Y_i(1) - Y_i(0)|D_i = 1] = E[Y_i(1)|D_i = 1] - E[Y_i(1)|D_i = 0], \tag{2}$$

where D_i is a dummy variable taking value 1 if the firm i received treatment.

The problem is that we can only either observe $Y_i(1)|D = 1$ or $Y_i(0)|D = 0$, as we cannot observe what would have happened to a firm that received treatment if it did not receive it, or to a firm that did not receive treatment if it did receive treatment.

The idea of matching is precisely to search for a valid counterfactual. If receiving the treatment is random for firms with similar values of pre-treatment observable variables, the missing outcome, $Y_i(1)|D = 0$, can be replaced through the outcome of nonparticipant firms with similar covariates, $Y_i(0)|D = 0$.

Three statistical assumptions need to be fulfilled in order to obtain consistent estimators. The first is the Conditional Independence assumption (CIA), also known as the "unconfoundedness assumption" (Rosebaum and Rubin, 1983; Lechner, 2002):

$$D \perp (Y(0), Y(1))|X \tag{3}$$

This assumption means that, conditional on the observable characteristics, participation in the treatment is random. In other words, the variables that influence participation status must be observable and controllable. For this reason, matching estimators are more reliable when a wide range of controls are available. In our case, as we have access to a rich database with detailed information on the balance sheet of each firm, we are able to control for several firm-specific characteristics which we believe to influence the probability of receiving a loan. In order to avoid the treatment affecting the firms' matching variables, these were used in the year preceding the loan signing.

The second assumption is the Matching Assumption:

$$0 < P(D = 1|X = x) < 1, \tag{4}$$

which requires that, for each treated unit, there is an identical untreated unit in terms of covariates. If it would be the case that all firms of a given covariate would have received treatment, there would not be any untreated similar firm to be used as a counterfactual. As we have a small number of treated firms and virtually the whole universe of Portuguese firms to be used as potential controls, this assumption is likely to hold in our sample. In section 5.3 we further discuss about this assumption.

The third and last assumption is the Stable Unit Treatment Value Assumption (SUTVA):

$$Y_i|(D_1, D_2, ..., D_N) = Y_i(D'_1, D'_2, ..., D'_N) \ \textit{if} \ D_i = D'_i, \tag{5}$$

which states that the potential outcomes of the untreated units should be unaffected by the treatment of other firms.

In our study, there are two mechanisms that could result in this assumption not holding. First, there could be spillovers resulting from the attribution of the loans. Indeed, one of the EIB priorities is to support projects that have positive impacts on the overall economy. Thus, we should expect some externalities on the untreated firms derived from the treatment of the remaining. Still, we believe this effect to be mostly insignificant as the spillovers derived from the financed investments are usually spread out across the

economy and not concentrated to certain groups of individuals.

The second mechanism, and perhaps most concerning, is related to the business-stealing effect that the beneficiary firms could have over their rivals. If the remaining firms of the market decrease investment and output in response to the treatment, our ATT could be overestimated. It would not only include the possible positive effect on the outcome variable derived from the loan, but also the negative effect on the counterfactuals used.

Accordingly, there is a trade-off in our analysis. If we match more similar and closely related firms, we are more likely to fulfill the CIA assumption. On other hand, the SUTVA assumption is less likely to hold. For this reason, we conducted our analysis in two different setups. First, in order to get the most similar counterfactuals possible, we forced matched firms to belong to the same market, as defined by the 3-digit NACE industry classification. Then we compare the results avoiding matches at same market but forcing them to belong to the same broader industry, as defined by the 2-digit NACE industy classification.

4.3 Differences-in-Differences Matching

In case the CIA assumption does not hold, even after controlling for observables there may still be systematic differences between treated and untreated units. To make our estimation more robust, we combine Matching with a Difference-in-Differences estimator (MDID). Instead of analyzing investment and market-shares directly, we focus on changes of these variables in respect to a pre-treatment period. This makes it possible to get consistent estimators even if there are unobserved differences between firms who received project loans and those who did not, as long as these differences are time-invariant.

When using this estimator, an important aspect to decide is on how long to follow the firms after the loan has been granted. As argued by Fredrik (1998), if we choose a too short period, we risk mispresenting the effects of the loans as they may take several years to have real effects on the firms. Choosing a too long period, however, makes it more difficult to isolate the effects from the loans. Following Ankarhem et al. (2010), our differences-in-differences estimates for market shares were calculated one, three and five years after the loan signing date. The effect on investment rate was measured first

solely in the year of the signature, and then in the average investment on the three years following the loan signature.

4.4 Algorithm

Finally, as different techniques imply a trade-off between bias and variance, a choice has to be made regarding the technique used to match treated and control units. Matching estimators that use only the best counterfactual available lead to reduced bias, as the counterfactual will be the most similiar possible to its corresponding treated unit. However, this leads to an increase variance compared to estimators that match each treated unit with several controls.

In this work project, we employ the Abadie and Imbens (2002) nearest-neighbor matching estimator that matches each treated unit with the most similar untreated firm available. To ensure robustness, we compare the results with the case when matching each treated unit with its three most similiar counterfacuals.

5 Description of the Data

5.1 Datasets and Data Cleaning

Two datasets were used in this work project. The first was constructed based on the EIB's internal loan management system (SERAPIS). The second is the Banco de Portugal Central Balance-Sheet Database (Central de Balanços, CB).

SERAPIS contains loan-specific information of all loans made by the EIB Group such as their contract name, signature date, the amount disbursed, and the sector targeted. For this work project, we collected data regarding project loans provided to Portugal between 2007 and 2015 from this database.

Central de Balanços contains detailed information on the balance sheet and income statements items of virtually all non-financial Portuguese firms, using data drawn from a survey on corporations (Informação Empresarial Simplificada, IES). It comprises an unbalanced panel-data of about 350 thousand observations per year, from 2006 to 2016.

To ensure robust estimations, some observations were eliminated from this database. Firstly, we only considered firms with strictly positive values of assets, turnover and

number of workers. Secondly, in order to clean outliers, we eliminated all observations below the 1st percentile and above the 99th percentile either in the distribution of ROA or in the distribution of change in market share. Finally, all firms that were consider micro according to the European Commission definition were discarded. These firms would not provide reasonable counterfactuals as they are not eligible for the project loans.

In order to merge both databases, it was necessary to identify the firms that benefited from each individual loan. All multi-component loans made to national or local public entities designed to finance multiple projects were discarded, as it was impossible to trace down the individual beneficiary firms. For the remaining, we were able to correspond each loan with a firm. In the situations where a corporate group, rather than a single firm, was involved in the borrowing we assumed that the beneficiary was the firm that owned the project being selected. [2]

We focused our analysis on sectors where competition issues could potentially arise from the provision of these loans. As such we did not consider loans provided to 1) legal monopolies such as firms of the water supply sector or the railways industry; 2) the construction of roads and highways; 3) public entities that are not considered business units (e.g. hospitals, schools, etc.). Furthermore, we did not consider loans to the telecommunications sector since all the beneficiary firms identified suffered multiple mergers in the period considered. In section 5.3 further details about the loans considered in our analysis will be given.

5.2 Variables used

Table 1. presents the variables used in our analysis. For each year, the treatment variable discriminates firms that received a project loan in that year from those who did not. In order to avoid endogeneity, all the controls used were lagged one year in respect to the loans assignment year. The control variables used in the matching are those we believe to influence the probability of receiving a project loan. As these loans are devoted to sizeable firms capable of implementing large projects, the number of workers (*employment*), the firms' turnover (*turnover*) and number of establishments (*establishments*) control for size

[2]For instance, the project "EDP REPOWERING II" involved the re-powering of two pumped storage hydropower plants managed and owned by EDP - Gestão da Produção de Energia, S.A. As such, we considered this firm to be the beneficiary from the loan.

Table 1: List of Variables

Variable	Definition
$Treated_{i,t}$	Dummy variable, taking value 1 if the firm received a project loan in year t
$MarketShare_{i,t}$	Outcome variable, calculated as the firm's turnover over the total turnover of the market
$Sector_i$	Control variable, referring to the NACE Rev.2 industry classification at a 2 digits level
$FirmAge_{i,t}$	Control variable, referring to age of the firm since it was officially established
$Establishments_{i,t}$	Control variable, referring to number of establishments of the firm
$Employement_{i,t}$	Control variable, referring to the number of workers in the firm
$Turnover_{i,t}$	Control variable, referring to the turnover of the firm
$InvestmentRate_{i,t}$	Control variable, calculated as the ratio of investment in tangible assets over total assets
$Leverage_{i,t}$	Control variable, calculated as the ratio of non-current liabilities over total assets
Return on Assets $(ROA)_{i,t}$	Control variable, calculated as the ratio of EBITDA over total assets
$CashflowAssets_{i,t}$	Control variable, calculated as the ratio of operating net income over total assets
$CashAssets_{i,t}$	Control variable, calculated as the ratio of cash and bank deposits over total assets
$Trend_{i,t}$	Control variable, calculated as the firms change in market share from $t-3$ to $t-1$

in different aspects. The age of the firm ($FirmAge$) might also influence its probability to conduct new projects and apply for a loan, as well as the banks' decision to grant the financing. As financing particulal sectors of the economy is one of the EIB's priorities, a variable was added (*sector*). Previous investment behavior is another characteristic we believe to be related to the probability of applying for a loan ($InvestmentRate$). Furthermore, as the banks' risk management department is involved in the decision to grant financing, several financial ratios that proxy the financial health of the firms were included.

Lastly, following Daniele et. al (2016), a variable was added refering to the firm-specific trend in market share in the years preceding the treatment. It may be the case that firms whose position in the market is shrinking (or growing) are more prone to applying for project loans. Additionally, this allows to control for time invariant unobserved heterogeity that has an effect in the growth (and not in the level) of our outcome variables. However, adding this variable decreases our sample size as we do not have access to firms' characteristics before the year 2006. Thus, our estimations were made both including and excluding this variable.

5.3 Descriptive Statistics

The total project loans attributed to Portugal between 2007 and 2015 amounted to 11.563M euros and were mainly attributed to the Transportation and Storage industry (29%), and to the Electricity, Gas, Steam and Air Conditioning Supply industry (22%).

As explained before, not all project loans were considered in our analysis. Our sample,

comprising of 3.927M euros (around 34% of the total project loans), is mainly composed of loans made to the Electricity, Gas, Steam and Air Conditioning Supply industry (61%), and to the Manufacturing industry (29%). Apart from the fact that some loans were discarded due to methodologic difficulties, this difference reflects mainly the susceptibility of different industries to raising competitions concerns. To give an example, all loans to the Water Supply, Sewerage, Waste Management and Remediation Activities industry were not considered in our analysis since they were attributed to legal state-owned monopolies. It would not make sense to study whether these loans have the potential to distort the competition of markets which, by law, are not competitive.

Table 2 includes the mean and the standard deviations of the variables used in our analysis over the period from 2007 to 2015. It gives a sense of the differences between firms that received project loans and those that did not. As expected, supported firms are, on average, much larger than the remaining in different aspects: market share, turnover, employment and number of establishments. They are also older and present higher levels of leverage and returns on assets. These differences largely reflect the sample selection in the application and receival of the loans.

In Table 3 we test for balance by comparing the covariates between treated firms and their respective counterfactuals in the matching estimation [3], at the time of receiving the treatment. We can see that the two groups are relatively homogenous in most of the variables used, as the matching algorithm is able to focus on a small subset of firms more comparable to the ones that received treatment. Thus, the Matching Assumption appears to hold. Nonetheless, we should be careful with our conclusions as the average value of some variables (namely *Employement* and *InvestmentRate*) somewhat differs across groups.

[3] In the standard specification where we did not include the variable "trend", firms were matched in the same industry and only one counterfactual is used per treated unit.

Table 2: Descriptive Statistics

	Treated		Control	
	Mean	St. Dev.	Mean	St. Dev.
Market Share	14,88%	27,56%	0,26%	1,85%
Turnover	$5,61 * 10^8$	$1,53 * 10^9$	$8,22 * 10^6$	$6,12 * 10^7$
Establishments	4,09	5,36	1,44	2,64
FirmAge	26,68	21,29	17,66	14,93
Employememt	654,16	797,84	30,58	99,61
InvestmentRate	0,061	0,061	0,049	0,080
Leverage	0,294	0,207	0,147	0,206
ROA	0,113	0,131	0,079	0,120
CashflowAssets	0,106	0,129	0,079	0,125
CashAssets	0,058	0,067	0,110	0,147

Table 3: Balancedness

	Treated		Control	
Variable	Mean	St.Dev.	ATT	St.Dev.
Turnover	$6.80 * 10^8$	$1.51 * 10^9$	$5.11 * 10^8$	$1.24 * 10^9$
Establishments	3.11	3.94	3.39	3.98
FirmAge	30.5	23.42	29.25	23.38
Employement	701.79	681.56	464.54	680.62
InvestmentRate	0.113	0.257	0.049	0.044
Leverage	0.310	0.227	0.274	0.245
ROA	0.132	0.111	0.130	0.137
CashAssets	0.036	0.069	0.030	0.067
CashflowAssets	0.123	0.111	0.130	0.128

6 Results

Table 4 to Table 11 present the estimated impact of project loans in firms' market shares after one, three and five years following the signing of the loans. In most of the estimated models, no statistically significant effect of the loans on market shares was found, meaning that beneficiary firms did not experience increases in their market shares different from their most similar firms that did not receive a project loan.

In table 4, when firms are matched in the same market (i.e. 3-digit NACE Rev.2 classification), 17 out of the 21 estimations employed were insignificant. In the remaining 4 cases, a significant effect was found at a 5% significance level, but the sign was negative. As such, our hypothesis that beneficiary firms would experience higher increases in their market shares than their counterfactuals does not seem to hold for any of the estimations. In table 5, when firms are matched in the same broader industry (i.e. 2-digit NACE Rev.2 classification) but not in the same market, similar results were found. Out of the 21 estimations employed, 19 were insignificant and 2 were significant, at a 5% significance level. The two significant estimations presented a negative effect on market shares 1 year and 5 years after the loans signing.

In Table 6 and 7, we repeat the exercise including trend as a control variable. In Table 6, 11 out of the 15 estimations employed were found to be insignificant and, in Table 7, 14 out of the 15 estimations were found to be insignificant, both at a 5% significant level. As a final robustness check, Table 8 to Table 11 present similar specifications to Table 4 to Table 7 apart from the fact that the matching algorithm used the three most similar counterfactuals for each treated firm, instead of only one. The results were identical and again, most of the estimations were insignificant.

Altogether, the 8 different specifications employed suggest that project loans do not have an effect on firms' market shares either one, three or five years following the signing of the loans.

A different question concerns the effect of the project loans on firm's investment rate. In Table 12 and Table 13, the impact on firms' investment rate at the year of receival is estimated, first by matching firms in the same market and then in the same broader industry. When not including trend as a control, 3 out of the 9 estimations employed yielded significant and positive results when matching firms in the same market; and 4

Table 4: ATT Estimates on Market Shares (percentage points)*

	After 1 year		After 3 years		After 5 years	
Year of loan signing	ATT	St.err.	ATT	St.err.	ATT	St.err.
2007	5.58	4.73	1.38	3.22	0.82	5.79
2008	1.24	1.78	1.09	2.66	0.23	1.81
2009	0.75	0.52	1.64	1.75	1.18	2.08
2010	-2.96	1.38*	-2.36	2.03	-3.08	2.60
2011	-0.72	0.44	-1.07	0.48*	1.43	1.00
2012	0.03	0.04	-0.50	0.12*		
2013	1.18	0.86	0.58	0.39		
2014	-0.18	0.07*				
2015	3.23	1.84				

*Firms matched in the same market, variable trend not included, one match per treated unit.

Table 5: ATT Estimates on Market Shares (percentage points)*

	After 1 year		After 3 years		After 5 years	
Year of loan signing	ATT	St.err.	ATT	St.err.	ATT	St.err.
2007	-3.04	4.61	-6.82	4.39	-8.98	3.82*
2008	1.44	1.78	2.45	3.00	1.34	2.48
2009	0.23	1.40	0.69	2.76	1.14	3.60
2010	-6.19	1.90*	1.70	7.06	0.74	0.74
2011	-0.98	0.54	-1.47	0.90	-1.57	1.08
2012	1.21	0.69	3.10	2.82		
2013	0.69	1.21	1.16	0.91		
2014	0.08	0.36				
2015	-5.26	7.96				

*Firms matched in the same industry, variable trend not included, one match per treated unit.

Table 6: ATT Estimates on Market Shares (percentage points)*

	After 1 year		After 3 years		After 5 years	
Year of loan signing	ATT	St.err.	ATT	St.err.	ATT	St.err.
2009	-1.03	1.69	0.98	1.93	0.05	2.48
2010	-2.28	1.19	-1.45	1.72	-2.33	2.60
2011	-0.72	0.44	-1.07	0.48*	-1.43	1.00
2012	0.16	0.04*	-0.78	0.08*		
2013	0.52	0.39	0.58	0.39		
2014	-0.18	0.07*				
2015	3.23	1.84				

*Firms matched in the same market, variable trend included, one match per treated unit.

Table 7: ATT Estimates on Market Shares (percentage points)*

	After 1 year		After 3 years		After 5 years	
Year of loan signing	ATT	St.err.	ATT	St.err.	ATT	St.err.
2009	3.17	1.63	5.73	3.69	1.14	3.60
2010	-3.81	2.21	-3.41	3.37	0.74	0.74
2011	-0.98	0.54	-1.47	0.90	-1.57	1.08
2012	1.21	0.69	3.10	2.82		
2013	0.69	1.21	0.50	0.91		
2014	-0.27	0.62				
2015	-3.80	1.68*				

*Firms matched in the same industry, variable trend included, one match per treated unit.

Table 8: ATT Estimates on Market Shares (percentage points)*

	After 1 year		After 3 years		After 5 years	
Year of loan signing	ATT	St.err.	ATT	St.err.	ATT	St.err.
2007	2.86	2.57	-0.32	1.87	-1.14	4.29
2008	1.80	1.86	2.36	2.61	1.34	1.75
2009	0.11	1.17	0.93	1.59	0.86	2.08
2010	-3.50	1.14*	-3.00	1.68	-3.87	2.29
2011	-0.14	0.85	-0.69	0.66	-0.06	1.01
2012	-0.19	0.32	-0.95	0.41*		
2013	0.48	0.69	0.69	0.66		
2014	-0.45	0.28				
2015	2.97	1.96				

*Firms matched in the same market, variable trend not included, three matches per treated unit.

Table 9: ATT Estimates on Market Shares (percentage points)*

	After 1 year		After 3 years		After 5 years	
Year of loan signing	ATT	St.err.	ATT	St.err.	ATT	St.err.
2007	-0.01	2.52	-3.24	2.52	-5.04	3.12
2008	1.78	1.63	1.82	2.49	0.47	1.86
2009	2.66	3.32	2.40	3.28	3.16	3.71
2010	-4.47	1.48*	-0.82	3.61	-1.82	3.97
2011	-0.66	0.46	-1.10	0.76	-1.13	0.87
2012	-0.04	0.73	-1.29	2.56		
2013	0.32	0.69	0.18	1.05		
2014	-0.40	0.44				
2015	0.33	4.54				

*Firms matched in the same industry, variable trend not included, three matches per treated unit.

Table 10: ATT Estimates on Market Shares (percentage points)*

	After 1 year		After 3 years		After 5 years	
Year of loan signing	ATT	St.err.	ATT	St.err.	ATT	St.err.
2009	0.09	1.11	0.79	1.61	0.75	2.11
2010	-3.45	1.15*	-2.80	1.67	-3.59	2.28
2011	-0.02	0.85	-0.65	0.66	-0.09	1.01
2012	-0.19	0.32	-0.95	0.41*		
2013	0.35	0.45	0.61	0.36		
2014	-0.03	0.26				
2015	3.01	1.99				

*Firms matched in the same market, variable trend included, three matches per treated unit

Table 11: ATT Estimates on Market Shares (percentage points)*

	After 1 year		After 3 years		After 5 years	
Year of loan signing	ATT	St.err.	ATT	St.err.	ATT	St.err.
2009	1.21	4.13	2.61	3.35	3.15	3.82
2010	-3.38	1.13*	-3.35	1.70*	-4.33	2.30
2011	-0.67	0.46	-1.10	0.76	-1.14	0.87
2012	-0.15	0.83	-1.29	2.56		
2013	0.16	0.53	-0.07	0.90		
2014	0.66	1.21				
2015	0.38	4.56				

*Firms matched in the same industry, variable trend included, three match per treated unit.

Table 12: ATT Estimates on Investment Rate at signing year (percentage points)*

	Excluding Trend		Including Trend	
Year of loan signing	ATT	St.err.	ATT	St.err.
2007	1.68	1.06	-	-
2008	-1.12	1.53	-	-
2009	4.73	2.30*	4.87	2.13*
2010	2.05	5.00	2.03	5.00
2011	-1.33	2.68	-1.33	2.68
2012	6.42	2.22*	6.42	2.22*
2013	0.31	0.13*	0.31	0.13*
2014	-5.01	2.67	-5.01	2.67
2015	-0.73	0.33*	-0.73	0.33*

*Firms matched in the same market, one match per treated unit.

Table 13: ATT Estimates on Investment Rate at signing year (percentage points)*

	Excluding Trend		Including Trend	
Year of loan signing	ATT	St.err.	ATT	St.err.
2007	0.04	1.12	-	-
2008	1.33	2.96	-	-
2009	15.42	5.51*	12.64	4.65*
2010	10.63	9.53	10.63	9.53
2011	-3.51	2.62	-2.11	3.32
2012	6.62	2.36*	6.62	2.36*
2013	0.31	0.13*	0.31	0.13*
2014	12.37	6.01*	8.98	8.31*
2015	-1.65	0.84	-1.65	0.84

*Firms matched in the same industry, one match per treated unit.

out of the 9 estimations were significant and positive when matching firms in the same broad industry. The results are similar when including trend as a control. All in all, these exercises suggest that there is a positive impact in the firms' investment rate at the year of receival.

However, when looking at the average effect of the investment rate in the three years following the loan signature, the effect vanishes. When matching firms in the same market, only 1 out of the 9 estimations employed were positive and statistically significant when not including trend, and no positive and signinficant effect was found when including trend (Table 14). The same happens when matchng firms in the same broader industry (Table 15). These results suggest that project loans do not have an effect on firms' average

Table 14: ATT Estimates on Investment Rate of three following years (percentage points)*

	Excluding Trend		Including Trend	
Year of loan signing	ATT	St.err.	ATT	St.err.
2007	1.07	0.72	-	-
2008	-1.20	1.43	-	-
2009	2.21	1.07*	4.21	2.28
2010	-2.73	2.23	-3.02	2.34
2011	-1.99	3.30	-1.99	3.30
2012	2.67	3.15	2.67	3.15
2013	2.02	1.33	2.02	1.33
2014	0.48	1.36	0.48	1.36
2015	-1.44	0.58*	-1.44	0.58*

*Firms matched in the same market, one match per treated unit.

Table 15: ATT Estimates on Investment Rate of three following years (percentage points)*

	Excluding Trend		Including Trend	
Year of loan signing	ATT	St.err.	ATT	St.err.
2007	-0.26	1.34	-	-
2008	-1.22	1.44	-	-
2009	5.01	2.01*	2.95	2.27
2010	-3.57	2.35	-3.57	2.35
2011	-2.08	3.13	-1.79	3.17
2012	2.66	3.15	2.66	3.15
2013	2.02	1.33	2.02	1.33
2014	10.69	8.53	9.76	9.20
2015	4.74	3.00	-3.46	1.98

*Firms matched in the same industry, one match per treated unit.

investment rate in the three years following the loans signing. The same conclusions hold when we match three firms per treated unit.

7 Conclusion

The aim of this paper was to study whether EIB project loans could be distorting competition in goods and services markets by giving the large beneficiary firms an artificial advantage over their rivals. This would be inconsistent with Article 140 of the Financial Regulation of the EU (Regulation 966/2012), which states that the EU financial instruments should only be used in a way that does not distort competition in the internal

market.

Our main result is that project loans do have a statistically significant impact on firms' market shares. More specifically, we find that the firms that received project loans did not experience increases in their market shares larger than comparable firms that did not receive a project loan. This result is robust to different specifications and matching techniques. As such, we fail to reject the hypothesis that project loans do not distort market competition and reinforce the position of already large and potentially dominant firms.

To better explore this result, we analyzed the impact of project loans on firms' investment decisions. While we find that project loans may increase investment at the year of receival, its impact in the average investment of the years following the loans signing is negligible. In other words, even if indeed there is an initial increase in investment, this effect is found to be offset by the decrease in investment in the years following the loan signature. This result goes in line with several authors (Abel, 1982; Auerbach and Hines, 1988; Adda and Cooper, 2000; Bronzini and Blasio, 2006) that found that a time-substitution effect whereby firms bring forward investment projects, in the present of government investment incentives, while keeping constant their lifetime investments.

As argued by Honohan (1995), the EIB can be advantageous in the presence of financial constraints which could either be explained by inefficient and costly domestic banking systems, or by the difficulty in raising large amounts of capital to finance sizeable projects. However, as capital markets become more developed, these market failures are becoming less significant (Honohan, 1995). Furthermore, as financial constraints tend to be more relevant for younger and smaller firms (Gertler, 1988), it is not so obvious that the larger firms eligible for project loans have difficulty in arranging credit in the market by themselves. As a result, firms might resort to the EIB not because there is not an alternative lender, but simply because the EIB offers more attractive conditions. Accordingly, receiving EIB project loans financing may have no major impact on firms' investment. If the subsidy effect underlying the favorable conditions of the loans is not too large, we may expect a negligible impact on firms' market shares.

A possible limitation of our analysis lies is that the effect of the loans might not be large enough to be distinguished from the noise prevenient from the firms' usual economic

activity. In fact, the average loan of our sample amounts to 87,27M euros, which translates to 1,56% of the average annual turnover of the treated firms, and 13,72% of their average annual investment in tangible assets. If there are lenders alternative to the EIB, the actual impact of receiving a project loan (usually comprising more attractive conditions) might be minor and hard to be isolated using standard econometric techniques. Nonetheless, this would suggest the competition concerns derived from these loans to be small, and likely outweighed by possible accomplishments in different objective areas of the bank.

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