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European Funds and Firm Performance: Evidence from a Natural Experiment

Abstract

Expanding regional eligibility in the access to grants can have important consequences for the performance of firms. We examine a quasi-natural experiment that consisted of an administrative redistricting intended to increase accessibility to European Union (EU) funds using a rich administrative dataset that covers the universe of Portuguese private firms between 2003 and 2010. Our results uncover a positive causal impact of increased eligibility on firms' sales. In contrast, employment and labour productivity do not seem significantly impacted by the reform. The effects are heterogeneous: while sales of firms in the services and non-tradable sectors are positively impacted, sales of firms in more competitive sectors are not affected.

JEL Classification: C21, R10

Keywords: quasi-natural experiment, European funds, firm performance, municipalities

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I. Introduction

As stated in the Treaty of Lisbon, the European Union (EU) aims to pursue economic, social, and territorial cohesion among its member countries. An important element in attaining cohesion is regional convergence, the objective that structures the channelling of substantial funds from the EU's budget to regions with income per capita below 75 percent of the EU average.[5] The stated aim is to boost regional income and employment growth and facilitate business creation. However, thus far, the actual results of this policy are hard to assess, and the evidence is ambiguous, suggesting a dire need for micro-based causal empirical evaluations of the impact of EU funds on regional fortunes. The empirical evidence suggests that, *on average*, transfers appear to have been effective in promoting growth and lowering regional disparities (Becker et al. 2010, Pellegrini et al. 2013, Giua 2017). However, the effects vary considerably depending on local conditions (Becker et al. 2013) and cohesion transfers may suffer from decreasing returns (Becker et al. 2012 and Cerqua and Pellegrini 2018) or have only temporary effects (Barone et al. 2016, Di Cataldo 2017, Becker et al. 2018). In fact, GDP per capita across EU-15 metro regions have been diverging since the mid-2000s (Ehrlich and Overman 2020).

In this paper, we rely on a quasi-natural experiment which takes advantage of a redistricting of the Lisbon NUTS 2 area, in Portugal, with the purpose of increasing eligibility to EU funds in specific regions.[6] This decision came in the wake of the Lisbon area reaching income levels well above the 75 percent EU income threshold, thus threatening the flow of funds to several municipalities which were below that threshold. The region was split so that those municipalities maintained privileged eligibility status. EU regions tend to progressively lose eligibility, as income per capita grows, so that an administratively mandated increase in eligibility is a rare occurrence. It is notable that, in contrast to most empirical papers, which exploit decreases in access, here we analyse the impact of increased eligibility on firm performance.[7]

We use a rich linked employer-employee administrative dataset that covers the universe of Portuguese private firms between 2003 and 2010 to compute intent-to-treat estimates from a difference-in-differences specification – hereafter diff-in-diff. Under parallel

[5] While relative to national budgets the EU's common budget is small – accounting for close to 1 percent of the Union GDP – the Structural and Cohesion Funds constitute a major budget line, second only to agriculture-related transfers.

[6] NUTS (Nomenclature of territorial units for statistics) classification is set up by the Eurostat to divide EU's territory and produce regional statistics.

[7] An exception is Becker et al. (2018), who compare the effects of gaining versus losing eligibility under the Objective 1 (or Convergence) objective.

trends, our diff-in-diff offer estimates of the causal impact of increased eligibility of European funds on a series of firm performance indicators. The identification strategy would then yield unbiased effects if outcomes of Treated firms would evolve similarly to those of comparison firms had the redistricting not been implemented. Comparison firms are those that are in high-eligibility status regions, excluding those in municipalities that neighbour Treated firms. The parallel trend assumption is supported by descriptive graphical inspection and an event study specification and is robust to exercises exploiting both the time and the spatial dimensions, including alternative control groups and the exclusion of the crisis period (2009-2010).

We find evidence that increased eligibility to EU funds for a municipality increased, *on average*, private firms' sales by 7.4% in Treated areas *vis-à-vis* firms in municipalities that saw no change in their eligibility status. However, total and college-educated employment numbers as well as labor productivity do not seem to change in response to the shock, whereas average wages marginally increased. Moreover, the positive effects are solely concentrated in firms in the *Non-Tradable* and *Services* sectors. Lastly, we also show a null impact on the total number of firms and firms' creation in Treated municipalities, as well as in the probability of a firm closing, indicating no significant changes in firms' dynamics.

A full accounting of the effects of the policy must also consider possible spillover effects of treated to neighbouring comparison areas. Therefore, we show that municipalities neighbouring our area of interest do not witness significant changes *vis-à-vis* the same set of comparison municipalities, suggesting that spillover effects were not present in this context.

We further exploit a complementary research strategy using the geographic exposure to the policy at the municipal-level data to shed light on the possible mechanisms of adjustment. First, we show there is no change in central government's transfers to Treated municipalities, nor in local government current expenses, suggesting the increase in firm sales is indeed the result of increased eligibility of EU funds. In addition, while increased eligibility leads to an increase in local wealth, as proxied by families' electricity consumption, suggesting that the increased sales may stem from greater access to funds by individuals, industrial electricity spending does not display higher activity levels, consistent with our findings on the *Manufacturing* sector.

Our results have important policy implications for policymakers willing to improve the design of place-based policies in general, and EU funding in particular. First, we show the importance of relying on firm-level data, as most studies on the subject have used more aggregated regional data. For example, we highlight that the effects of increased EU eligibility

are heterogenous as the effect on sales is driven solely by firms producing in the *Non-Tradable* and *Services* sectors. Second, the null effect on firms' productivity gains and employment numbers (including of more educated workers) suggest that EU regional funds acted as a distributional, rather than a productivity enhancing policy. These lessons are especially important in a period when European regions that have been strongly supported by the EU's Cohesion Policy have witnessed an increase in voting for Eurosceptic political parties (Fidrmuc et al. 2019, Crescenzi et al. 2020, Rodríguez-Pose and Dijkstra 2021).

The remainder of this paper is organized as follows. Section 2 reviews the relevant literature on the effect of regional funds, Section 3 presents the estimation methodology and the data, and Section 4 the results and robustness tests. Section 5 concludes.

II. Related literature

Growth and convergence across European regions have been a political priority of the European Union for decades. It gained importance over time, as relatively prosperous countries in Southern and then Eastern Europe adhered to the EU. Hampered by several econometric issues, empirical evidence on the success of EU regional policy is mixed. The first contributions to the debate, such as Sala-i-Martin (1996), and Boldrin and Canova (2001), detected no statistically significant effects of EU regional policy on per-capita-income growth of recipient regions, conditional on standard drivers of economic growth. Positive effects on agglomeration and industry location issues are reported in Midelfart-Knarvik and Overman (2002).[8] The ambiguity of results may stem from econometric issues that stand in the way of clear estimates. The first such issue is reverse causality, whereby regional characteristics condition access to EU funds. A second issue resides in how dynamics are considered in the estimation procedure. A third difficulty is the possibility of omitted variables, variables that affect economic performance but are not, or cannot be, explicitly considered. In addition, the selection of appropriate control variables is an issue.[9]

[8] Basile et al. (2008) find that Structural and Cohesion funds allocated by the EU to laggard regions have helped to attract subsidiaries of multinationals from both within and outside Europe.

[9] More sophisticated empirical approaches have been attempted, and some papers identify effects using techniques such as instrumental variable estimates - Dall'Erba and Le Gallo (2008), Ramajo et al. (2008); (dynamic) panel data techniques - Rodríguez-Pose and Fratesi (2004); a combination of the two - Esposti and Bussoletti (2008); Mohl and Hagen (2010), Bouayad-Agha, Turpin, and Védrine (2013); bayesian methods - Cuaresma, Doppelhofer, and Feldkircher (2012); or spatial growth models - Fiaschi, Lavezzi, and Parenti (2017). However, here the empirical evidence is mixed and remains controversial, as pointed out by Dall'erba and Fang (2015).

Becker et al. (2010) first exploited the fact that Objective 1 funding is based on a simple assignment rule, with a clear and simple threshold that affects a region's eligibility: NUTS 2 regions are eligible for funding if their GDP per capita is less than 75% of the EU average. These authors exploited a fuzzy regression discontinuity design (RDD), and use data from three programming periods (1989-1993, 1994-1999, and 2000-2006), to find that, *on average*, Treated regions grow significantly faster than do regions just above the 75% threshold.[10] No effects on employment growth were uncovered. Becker et al. (2012) distinguished average and marginal effects, in which the former may be positive but the latter negative, implying that the optimal funding has been surpassed. Becker et al. (2013) show that regions with high levels of human capital and good institutions were able to use funds more efficiently, that is, deliver the most growth.

Four more recent papers analyse the impacts of funds for different regions within a single country with regional data. Barone et al (2016) focused on the post-expiry period to examine the persistence of the economic boost to "convergence" regions after the termination of access to EU Regional Funds. Their findings highlighted that exiting the program has a negative impact on regional per-capita GDP growth. Giua (2017) examined municipalities contiguous to the municipalities affected by a policy-change, together with a measure of distance to identify the effects of EU Regional Policy in a panel of Italian regions. She finds a positive impact on employment levels produced by EU Regional Policy. Di Cataldo (2017) estimated the impact of EU funds in Cornwall and South Yorkshire, regions which were among the greatest beneficiaries of EU funds in the UK. Using synthetic control methods, they show that the income gap across regions has fallen with EU funding and labour market prospects have improved. Cerqua and Pellegrini (2018) use a regression discontinuity design and conclude that, despite portraying an average positive effect on regional growth, exceeding funds could have been allocated to other lagging regions more efficiently.

As pointed out, it is still rather meagre the body of literature evaluating cohesion policy funds using firms as units of observation, rather than municipalities or NUTS 2. Fattorini, Ghodsi, and Rungi (2020), using propensity score matching techniques and focusing exclusively on manufacturing firms, in a multi-country dataset, reported a positive effect of EU Regional Funds aimed at investments in R&D on firms' total factor productivity, particularly amongst the least efficient firms in the region. However, regarding

[10] Pellegrini et al. (2013) largely confirm the results of Becker, Egger, and von Ehrlich (2010) using data from Eurostat.

EU Regional Funds that promote overall business, no effects were uncovered. Bondonio and Greenbaum (2006), analysing firms in Northern and Central Italy's *Objective 2 regions*, find a positive impact of EU Regional Funds on employment growth, however estimating relatively high costs per job created.

Our paper further relates to the literature on the causal impact of place-based policies, surveyed in Kline and Moretti (2014a). Using rejected and future applicants to the US Empowerment Zones program as comparison groups, Busso et al. (2013) show that neighbourhoods receiving considerable Federal assistance in the form of tax breaks and job subsidies an increase in employment and local workers' real wages. Kline and Moretti (2014b) study the long-run effects from the Tennessee Valley Authority policy using as controls similar institutions proposed but never approved by the US Congress, showing how manufacturing employment increased after federal transfers had fallen.[11]

Place-based policies, such as the EU Structural and Cohesion funds, can possibly deliver effects that go beyond those found in the targeted area (Glaeser and Gottlieb, 2009).[12] In theory, spillover effects can have either positive or negative effects. If policies are successful at creating new establishments and jobs that would not have emerged in the absence of incentives, there may be a positive effect on surrounding areas through the forces of agglomeration and local multipliers (Moretti, 2010). However, the effects on the neighboring areas may also be negative if spatially targeted policies have business-stealing effects. A seemingly relevant reference on this count is Hanson and Rohlin (2013) on the spillover effects associated with the US Empowerment Zone (EZ) Program. They find that locations that border (or are economically similar) to EZ regions experience a decline in firm numbers. Andini and Blasio (2014) study an Italian place-based program and also find evidence of negative spillovers. In addition, Einiö and Overman (2020) examine the impacts of an intervention that aimed to increase entrepreneurship and employment in disadvantaged English areas using a regression discontinuity design that exploits the eligibility rule. Their findings indicate employment increases in Treated areas close to the treatment area boundary at the cost of significant employment losses in untreated localities just across the boundary. We also consider possible spillover effects from Treated to neighbouring areas in this study by analysing the effects on neighbouring municipalities.

[11] Gobillon et al. (2012) and Mayer et al. (2017) discuss similar place-based schemes in France, while Einiö and Overman (2020) and Criscuolo et al. (2019) do it for two cases in the UK. Examining manufacturing firms in the Southern regions of Italy, Bernini and Pellegrini (2011) investigated the impact of state aid to subsidized firms under a regional policy, finding higher growth rate of sales, employment, and investments, although with a negative impact on total factor productivity.

[12] For a discussion on the importance of spillovers in other contexts see Isem (2014).

III. Empirical approach

3.1 Natural experiment

Portugal has been a recipient of European funding in the context of several Community Support Framework (CSF) phases. Regions whose per capita GDP lies below the threshold of 75% of the European average were eligible for Objective 1 funding (before 2006) or Convergence region funding (after 2007). Differences in regional eligibility imply that more (less) developed regions face a lower (higher) likelihood of having a given project accepted and receive less (more) resources from the EU cohesion and structural funds.

Mainland Portugal has three distinct regional groupings as far as eligibility to EU funds is concerned, as illustrated in Figure A1 in the Appendix. The first comprises the North, Center, and Alentejo regions, which are part of the Convergence objective, associated with the most favourable access to funding. The second regional grouping is the Algarve, in the south, part of the phasing out regime, with per capita GDP above the 75% income threshold – when the 25 EU countries at the time are considered, but still below the 75% of average income for EU-15. Finally, the smaller NUTS 2 Lisbon region that resulted from the administrative breakup stands as the only area above the 75% average for EU-15, and thus part of the lower eligibility Competitive objective.[13]

3.2 Data

Data sources

For the empirical analysis in this study, we benefit from a longitudinal administrative linked employer-employee dataset, *Quadros de Pessoal*, conducted by the ministry responsible for employment affairs and, for that reason, of a mandatory nature. *Quadros de Pessoal* covers virtually all firms with at least one wage earner in the whole of mainland Portugal.[14] We retrieved information both at the worker level - including earnings, age, gender, education, and firm level - sales, number of employees, economic activity, location, and legal structure.[15]

[13] The period between 2003 and 2006 saw the region of Lisbon and the Tagus valley surpassing the threshold of 75% of EU average income per capita and jumping to a transitory regime called phasing-out. In 2002 this area was severely reduced with the incorporation of some NUTS 3 regions in NUTS 2 Center and Alentejo. Oeste and Médio Tejo are now part of the new NUTS 2 Center, whereas Lezíria do Tejo is now part of the new NUTS 2 Alentejo.

[14] Cases of self-employment are excluded. In addition, organizations falling outside the partnership or sole proprietorship legal definitions were also omitted due to their non-profit nature.

[15] We imposed four data restrictions. First, we excluded employees whose registered age was 16 or under - legal minimum working age, or over 65 - the ordinary retirement age. Second, we focused solely on workers with a monthly wage superior to the mandatory national minimum wage. Third, we excluded firms with sales equal to zero in every year they appear in the dataset. Finally, we also excluded firms with more than one establishment, as information on sales is not available at the establishment level. Our final dataset contains 72% of the firms from the initial dataset.

We complement our analysis with municipal-level administrative data. These sources provide information on the electricity consumption, both for domestic and industrial purposes, measured in thousands of kilowatt hours (kw/h), obtained from the DGEG – the Portuguese Directorate general for Energy and Geology. Data regarding transfers from the central government to Portuguese municipalities, as well as concerning the current expenses of municipalities, measured in Euros (€), was obtained from the DGAL – Directorate general of local government.

Outcome variables

We select five firm-level indicators to evaluate firm performance in the regions that underwent a change in eligibility to EU regional funds. Those indicators are total sales (measured in € per year), the number of total workers and workers with a bachelor's degree (hereinafter, BSc), labour productivity (measured as the sum of sales per worker), and monthly average wages (which includes the fixed and the variable component of wages).

We take the inverse hyperbolic sine (ihs) transformation of the first three dependent variables.[16] This approach has the advantage of allowing us to consider non-positive values.[17] Following Bellemare and Wichman (2019), with the hyperbolic sine, the interpretation of marginal effects approximates the natural logarithm of that variable when the untransformed means of such variables are large enough.

We test whether treatment has an impact on firm dynamics looking into the probability of exit using a dummy variable that takes value one if the firm closes and zero otherwise. Moreover, we aggregate firm data at the municipal-level to examine the effect on the number of total firms and on the number of firms entering the market. Finally, we assess the impact of the redistricting using the previously described municipal outcomes.

Table A1 displays the descriptive statistics for the variables used in the analysis.

3.3 Identification strategy and econometric analysis

Considering that each firm's likelihood of access to EU funds depends on a range of observed and unobserved variables, we argue that a mere comparison between subsidized and non-subsidized firms in a certain region will produce biased results. Instead, we rather assess the impact of higher eligibility on firm performance using a natural experiment, a

[16] The ihs transformation was first advocated by Johnson (1949) and employed by Burbidge et al. (1988).

[17] In Table A2 we show that our results are very similar irrespective of employing the logarithmic or the inverse hyperbolic sine transformation.

change that is entirely exogenous from the point of view of firms, in an intention to treat setting.

We explore the spatial discontinuity in access to European funds which occurred between 2006 and 2007 and derived from the redistricting of the Lisbon NUTS 2 area.[18] The municipalities to the north of Lisbon were singled out so as to gain access to higher eligibility and further benefit from EU funds, experiencing a break between the pre-treatment period, 2003 to 2006, and the post-treatment period, from 2007 to 2010.[19] Our identification strategy uses private firms located in the 33 municipalities pertaining to the NUTS 3 regions of *Oeste*, *Médio Tejo*, and *Lezíria do Tejo*, those gaining greater access to European funds due to the administrative redrawing of the territory.

Methods that compare outcomes in a treated region to those in adjacent regions may yield biased estimates for policies that have spillover effects (Jardim et al., 2022). Hence, we exclude neighbouring firms of Treated areas in a "buffer-zone" or "donut-hole" approach to mitigate the possibility of spillover effects from Treated areas. These municipalities have not seen any reduction in their eligibility therefore, absent possible spillover effects, we expect that they are untreated by the redistricting shock. We also check this possibility in this paper.

The comparison group is composed of firms in any of the 104 municipalities pertaining to Centre and Alentejo regions who have experienced no change whatsoever either in their nor their neighbours' eligibility to EU Regional Funds, as shown in Figure 1. We also add the NUTS 2 North region in the control group as a further robustness exercise.

[Insert Figure 1 here]

Our baseline diff-in-diff regressions estimate the average intent-to-treat effects derived from a standard ordinary least squares (OLS) model as follows:

$$Y_{imt} = \delta_1 Post_t \cdot Treated_m + \gamma_t + \alpha_i + \alpha_m + e_{imt}(1) \quad (1)$$

[18] Unlike what happens in most EU member states, including smaller countries like Belgium and the Netherlands, NUTS 2 regions do not inherit political power and their administrative competences are extremely limited. This was the only change since NUTS 2 were defined in 1989. Even in 2013, when NUTS 3 were slightly modified, NUTS 2 borders remained untouched.

[19] The first (1989-1993) and the second CSF (1994-1999) are not included in our analysis, and do not display similar discontinuities in access. We also do not consider the period afterwards as 2011 marks the year Portugal requested financial assistance from the IMF, the European Commission, and the European Central Bank, dramatically changing its economic policies.

where Y_{imt} are the outcome variables for a firm i, in a municipality m, in year t. $Post_t$ accounts for the treatment period (2007-2010) and $Treated_m$ is a binary variable signalling firms producing in municipalities that gained greater *Objective 1/convergence* eligibility status. γ_t are year fixed effects, and α_i and α_i are firm and municipalities fixed effects (i.e., those controls for characteristics of firms and municipalities that are time-invariant).[20] e_{it} accounts for clustered standard errors per NUTS 3, the level of assignment to treatment, as in Bertrand et al., 2004, and Abadie et al., 2017. The main outcome of interest is δ_1, measuring the impact on a firm located in a region whose eligibility to access EU funds increases.

We also implement a difference-in-differences event study design, with several advantages. First, we can test the exogeneity of the treatment by examining pre-trends in a more detailed manner. In the absence of a pre-trend, the identifying assumption requires no systematic factors driving both the shock and the outcomes of interest. Second, the event study makes it possible to evaluate the impact of the shock in the outcome variables in the very short and medium run - up to four years. Denoting Y_{imt} as the outcome variable in firm i, municipality m, and year t, the regression model reads as follows:

$$Y_{imt} = \sum_{k=2003}^{2005} \delta_k \; Treated_m + \sum_{k=2007}^{2010} \delta_k \; Treated_m + \gamma_t + \alpha_i + \alpha_m + e_{imt} \quad (2)$$

where δ_k is our outcome of interest measuring the year-by-year effect of producing in a Treated region, and the remaining variables are defined as before. The omitted year is 2006, the last year before treatment.

IV. Results

4.1 Baseline Results

We start by presenting the event study diff-in-diff estimates from computing eq. (2) in Figure 2 for the five main dependent variables using 90% confidence intervals. As can be seen for all cases, we find evidence indicating that the parallel trends' assumption is not rejected in this setting.[21] We also observe a positive causal effect of the increase in eligibility

[20] Our results are robust to the exclusion of the municipality fixed effects.
[21] We present descriptive graphical evidence in Figure A2 in the Appendix further corroborating the plausibility of the parallel trend assumption, for all outcome variables, in this context (Angrist and Pischke, 2009).

on sales. The effect is, however, not persistent and drops to zero in 2010. As to the remaining dependent variables, we find very small (as in the case of average wages) or non-significant treatment effects.

[Insert Figure 2 here]

Our baseline diff-in-diff specification estimates from eq. (1) are presented in Table 1 – panel A and confirm the statistically significant positive impact of the eligibility on firms' sales (see column (1)), corresponding to an increase of about 7.4 percent *vis-à-vis* firms in comparison municipalities. We uncover estimates that are statistically indistinguishable from zero for the effect of the treatment on the number of workers, suggesting that, while Treated firms sell significantly more, this does not create more employment. Results in Table 1 for the hiring of workers with a BSc degree by firms in Treated municipalities also show this pattern. If anything, the number of college-educated workers decreases slightly in Treated firms. As for average wages, our estimates in column (3) advocate for a significant increase, albeit of small magnitude, so that producing in a region that gains access to funds is associated, *on average*, with a wage increase of around 12€ per month, or about 2% of the average value of monthly average wages in the treatment and control groups. We also find no evidence of a significant rise in efficiency, as measured by labour productivity (in column 4).

[Insert Table 1 here]

We next turn our attention to the possibility that EU regional funds may spur sales and have purely distributional effects, at the sector of activity level, without any real effects on firm performance through productivity and efficiency.

4.2 Heterogeneous Effects: *i)* Tradable versus Non-Tradable, *ii)* Services versus Industry

In Table 1 – panel B, we find that the impetus behind the sales increase is driven solely by the *Non-Tradable* sector, with a statistically significant increase in sales for firms in this sector of around 10%, on average. There is no effect whatsoever on sales on the *Tradable* sector, i.e., for firms competing in the international markets, suggesting that increased access to EU regional funds does not promote a more efficient entrepreneurial context, rather it

increases sales by firms sheltered from competition (as proxied by their sector of activity). The monthly average wages increase relatively uniformly across sectors, though slightly more so in *Non-Tradable* – 13€, *vis-à-vis* 9€ in the *Tradable* sector. In what regards employment and labour productivity, we find no evidence of a significant effect estimated in both sectors.

Furthermore, we consider the sectoral differences in the impact of increased eligibility for firms in the secondary and tertiary sector in Table 1 – panel C.[22] We again uncover heterogeneous effects: firms in the *Services* sector benefit from a 13% increase in sales after a rise in eligibility, while firms in *Manufacturing*, on the contrary, experience a non-statistically significant increase in sales, with the point estimate at around 3%. Effects on employment and labour productivity are close to zero in both sectors, while average wages are positively impacted on the secondary and tertiary sector, with increases of similar magnitude as in our baseline results – of 15€ and 10€, respectively.

We believe the fact that most recent studies investigating the impact of EU cohesion funds on firm performance, at the firm level, focusing exclusively on firms in the secondary sector (Fattorini et al., 2020; Bachtrögler et al., 2020), gives further relevance to our result. Heterogeneous effects seem to be quite important, quantitatively, when assessing the impact of higher grant eligibility on private firms.

4.3 Robustness Checks

Our diff-in-diff identifying strategy is convincing if and only if it occurs in the presence of no confounding shocks other than the policy (Mayer et al., 2017). The absence of pre-trends, as shown in the event studies in Figure 2, are reassuring. However, there could still be shocks contemporaneous to the treatment that may threat our identification strategy. Put differently, while the presence of pre-trends is a sign of endogeneity, the absence of pre-trends is not a formal proof of exogeneity. Therefore, we present several robustness checks to assure the reliability of our baseline results.

First, as the treatment occurs in 2007, and as the years of 2009 and 2010 correspond to one of the greatest recessions in economic history, in the wake of the world's financial crisis, a possible cause of concern is that Treated municipalities were differently affected by shocks during our post-treatment period. If this recession did have differential effects across regions in a way correlated with our Treated and comparison breakdown, it would introduce confounding effects in our estimates. Our event studies presented in Figure 2 uncover a sudden drop in the positive effect found in firms' sales in 2010, the last year of the analysis.

[22] Since *Quadros de Pessoal* may underreport the activity of firms in the primary sector, we focus our analysis in the remaining, more representative, sectors of activity (Portugal et al., 2018).

This is in line with Becker, Egger, and von Ehrlich (2018) who show that eligibility effects weakened during the world financial crisis period. We investigate whether this event is driving our diff-in-diff results by re-estimating eq. (1) after excluding the years of 2009 and 2010 from our baseline sample. Our results, reported in Table A3 in the Appendix, are very similar to baseline, this being true for the entire sample and for the different sectors of activity that we analyse.

Second, another concern related with the identification of causal effects from place-based policies is to construct a valid counterfactual in the absence of the policy. As mentioned in the methodology section, our control group includes firms from all Portuguese mainland municipalities close to Treated areas whose eligibility statues – as well as their neighbours', remain unchanged. However, as shown in Table A4 in the Appendix, even if we add the more distant, and potentially more different, *North* region in the comparison group, our results remain unchanged as far as the *Non-Tradable* and *Services* sectors.

Third, we tested whether our results were robust to a more refined comparison group using the coarsened exact matching (CEM). The advantage of the CEM is that it creates a new control group of firms resembling the Treated firms more closely in terms of observable characteristics prior to the treatment.[23] Consequently, concerns relating to confounding effects biasing our estimates are reduced, assuming that the more firms are alike in terms of observables before treatment, the more plausible is the parallel trends assumption that had there not been any treatment, the evolution of firms' performance would be the same for Treated and control group observations. More details can be found in Appendix B.

In Table A5 of the Appendix, we present estimates combining the CEM and diff-in-diff. Once again, our main significant results remain roughly unaffected, with the exception for the statistically significance of the positive impact on sales in our baseline estimation. However, a significant increase in sales for the *Non-Tradable* and *Services* sectors remains, as before, as well as a statistically significant positive effect on average wages in every sector. Employment and labour productivity impacts are statistically indistinguishable for zero, as was the case in Table 1.

4.4 Effects on firm dynamics

Another important aim of our study is to identify whether access to a higher eligibility status had an impact on firm dynamics at Treated municipalities, particularly in the total number of firms, firm' entry and exit. Table A6 in the Appendix reports our results regarding

[23] Iacus, King and Porro (2011) show that this method produces lower model dependence, estimation error, variance, bias, and reduces imbalance vis-à-vis remaining commonly employed matching methods.

the evolution of the total number of firms and number of new firms at the municipality level, as well as a more granular analysis, at the firm level, concerning the probability of firms exiting the market, by employing a linear probability model.[24]

In all three cases, we present coefficients statistically indistinguishable from zero, indicating null eligibility effects on firm dynamics. Importantly, the fact that firm dynamics have not been significantly altered in the treatment period is also a good indication that our baseline results are not biased due to composition effects. Indeed, had treatment influenced firms' entry or exit rates, part of our results could have been driven by a change in the composition of Treated or the control groups. For example, higher eligibility status could have prevented some below-average firms from leaving the market in Treated municipalities, which in turn could have generated a negative bias on the average performance of firms in Treated municipalities. However, based on our estimates from Table A6, this is not a cause for concern in our case.

4.5 Are there spillover effects to neighbouring municipalities?

We now investigate if there are spillover effects from firms in Treated areas to neighbouring untreated areas (Glaeser and Gottlieb, 2009). To do so, we define our treatment group in this subsection to include only neighbouring municipalities – termed Neighbours', which experienced no change in eligibility, but saw, at least, one of their neighbours being treated. This new treatment group includes firms from 14 different municipalities (see Figure 1).[25] The control group remains unchanged.

As one can observe in Table 2, the coefficients of interest are all small and non-statistically significant, except for the average wages, corroborating that even firms close to the Treated areas do not seem to receive spillover effects.

[Insert Table 2 here]

4.6 Alternative Mechanisms

Lastly, we discuss possible potential alternative mechanisms or confounding factors which could explain our findings using municipal-level administrative data. One possible candidate is transfers from the central government. Indeed, if the amount of transfers the

[24] Figueiredo et al. (2002) and, more recently, Carias et al. (2022) show that Portuguese entrepreneurs tend to locate their businesses in their place of residence.

[25] We do not include in this group municipalities whose neighbors lost access to funds (Lisbon region).

government grants to Treated municipalities increases sizably in the period following the treatment, our initial results could derive from such actions, and not only by increased eligibility to EU funding. In Table 3 column (1), we show that government transfers have not increased in Treated municipalities *vis-à-vis* our control group municipalities. In addition, it could be the case that Treated municipalities increased their expenses through debt financing or local taxes, not necessarily due to transfers from the central government. In column (2), we show that municipalities' current expenses have not increased in Treated municipalities. In sum, we find no evidence in favour of explaining our results by increased spending by the central or local governments.

In addition, Veiga (2012), in her study of the determinants of the assignment of EU funds in Portugal, argues that more funds are transferred to municipalities where the ruling national party had been supported by voters. We present some descriptive evidence that this unlikely to affect our results. In our pre-treatment period (2003-2006), 39% of the municipalities in the treated group are aligned with the party in the central government, while this figure is 42% for the comparison group. Moreover, in the post-treatment period (2007-2010), these percentages remain remarkably constant, and the difference is, once again, non-statistically significant (39% in treatment vs. 36% in control).

In Table 3, we further analyse whether other indicators at the municipal level have experienced different growth rates for Treated and non-Treated municipalities. We focus on electricity consumption as a proxy for municipalities' income.[26] While electricity for domestic consumption increases by more than 3%, on average, in Treated versus comparison municipalities, no effect is found for consumption of electricity for industrial purposes. We consider this additional evidence in favor of the idea that, while Treated municipalities benefitted from higher income, access to EU regional funds did not affect firm productivity, especially in the secondary sector.

[Insert Table 3 here]

V. Conclusion

The European Union sets up regional policy initiatives that generate large public transfers to lower income regions across the continent. This paper exploits a unique quasi-natural experiment resulting from a redistricting decision meant to increase eligibility to EU

[26] Unfortunately, there are no good GDP or personal income tax measures, at the municipal level, during our sample period.

funding, whereby a "convergence" region was administratively created in Portugal. We investigate, for the first time, the impact of regional eligibility EU cohesion funds on firm performance relying on a diff-in-diff framework that uses administrative microdata from the universe of firms in the country. We place our paper in a stream of research meant to empirically assess the impact of place-based polices such as the EU regional policy on economic convergence.

We find evidence of a strong demand effect that raises sales, on average, by more than 7% on firms in Treated vis-à-vis firms in comparison municipalities, but no evidence of a solid impact on labour productivity in those same firms. The sales increase is driven entirely in the *Non-Tradable* – as opposed to *Tradable*, and in the *Services* – as opposed to the *Manufacturing* sectors. Furthermore, access to EU funds did not produce a significant increase in employment, or in the number of workers with a BSc degree, and only a marginal, though significant, increase equivalent to 2% of the average value in monthly average wages.

Robustness checks involving alternative time and spatial specifications, as well as employment of the coarsened exact matching methodology combined with the diff-in-diff confirm our findings on sales and the sectoral asymmetry of effects. We show these results cannot be explained by higher expenditures from the local, nor higher transfers from the central government.

Our results point to the urgency of a rigorous assessment, at the firm-level and across sectors, of the impact of European funds on private firm performance. Exploiting specific, well-defined policy episodes, can enlighten us as to the nature, the quantitative impact, and the causal mechanisms associated with increased eligibility to access EU funds.

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Tables

Table 1. Diff-in-diff baseline results (Panel A) and sectoral analysis (Panels B and C)

	Sales (ihs)	Total Workers (ihs)	Average Wages	Labour Productivity	Workers with a BSc (ihs)
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline					
Treated * Post-Treatment	0,074*	-0,003	11,786***	3 245,363	-0,010*
	(0,04)	(0,02)	(2,88)	(4 188,18)	(0,01)
Adj R2	0,36	0,88	0,53	0,45	0,85
N	451 318	451 442	451 442	451 317	451 442
Panel B: By Sector – Tradable versus Non-Tradable					
Non-Tradable					
Treated * Post-Treatment	0,096**	-0,004	12,605***	5 065,980	-0,011**
	(0,04)	(0,02)	(3,61)	(6 466,73)	(0,00)
Adj R2	0,36	0,87	0,52	0,45	0,85
N	297 737	297 811	297 811	297 736	297 811
Tradable					
Treated * Post-Treatment	0,021	-0,001	9,480**	-2 404,872*	-0,009
	(0,06)	(0,02)	(4,02)	(1 212,69)	(0,01)
Adj R2	0,38	0,90	0,55	0,33	0,86
N	151 226	151 274	151 274	151 226	151 274
Panel C: By Sector – Services versus Manufacturing					
Services					
Treated * Post-Treatment	0,127***	-0,001	10,261***	5 465,534	-0,011**
	(0,04)	(0,02)	(2,76)	(8 087,44)	(0,00)
Adj R2	0,37	0,88	0,53	0,47	0,85
N	276 243	276 306	276 306	276 242	276 306
Manufacturing					
Treated * Post-Treatment	0,034	-0,014	14,649***	-1 087,061	-0,012
	(0,05)	(0,03)	(3,61)	(2130,16)	(0,01)
Adj R2	0,35	0,89	0,53	0,39	0,87
N	140 931	140 969	140 969	140 931	140 969
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipal Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variables in column (1), (2), and (5) have suffered an inverse hyperbolic sine transformation; Our regressor of interest, Treated * Post-Treatment, indicates firms producing in one of the 33 Treated municipalities, during the treatment period (2007-2010). Our analysis includes the 2003-2010 period. Clustered standard errors, at the NUT3 level, are presented in parenthesis; Significance level at which the null hypothesis is rejected: *** 1%, ** 5%, * 10%.

Table 2. Diff-in-diff spillover results

	Sales (ihs)	Total Workers (ihs)	Average Wages	Labour Productivity	Workers with a BSc (ihs)
	(1)	(2)	(3)	(4)	(5)
Neighbors * Post-Treatment	0,017	-0,003	42,747***	3 259,363	0,003
	(0,04)	(0,01)	(9,75)	(1 917,38)	(0,01)
Adj R2	0,37	0,89	-0,01	0,24	0,86
N	376 606	376 719	376 719	376 605	376 719
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipal Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variables in column (1), (2), and (5) have suffered an inverse hyperbolic sine transformation; Our regressor of interest, Neighbors * Post-Treatment, indicates firms producing in one of the 14 municipalities neighbors to the Treated municipalities, during the treatment period (2007-2010). Our analysis includes the 2003-2010 period. Clustered standard errors, at the NUT3 level, are presented in parenthesis; Significance level at which the null hypothesis is rejected: *** 1%, ** 5%, * 10%.

Table 3. Alternative Mechanisms

	Government transfers (log)	Municipalities' current expenses (log)	Electricity	
			For domestic purposes (log)	For industrial purposes (log)
	(1)	(2)	(3)	(4)
Panel A: Treated				
Treated * Post-Treatment	0,015	0,014	0,032***	-0,016
	(0,02)	(0,02)	(0,01)	(0,07)
Adj R2	0,96	0,97	1,00	0,98
N	1 096	1 096	1 096	1 096
Panel B: Neighbors				
Neighbors * Post-Treatment	0,030**	0,005	-0,010	-0,073
	(0,01)	(0,02)	(0,01)	(0,06)
Adj R2	0,97	0,97	1,00	0,98
N	944	944	944	944
Year Fixed Effects	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes

Notes: Our regressors of interest, Treated * Post-Treatment and Neighbors * Post-Treatment indicate firms producing in Treated or Neighbors municipalities, respectively, during the treatment period (2007-2010). Our analysis includes the 2003-2010 period. Clustered standard errors, at the NUT3 level, are presented in parenthesis; Significance level at which the null hypothesis is rejected: *** 1%, ** 5%, * 10%.

Figures

Figure 1.

Geographical Distribution of the Neighbors and Comparison Municipalities

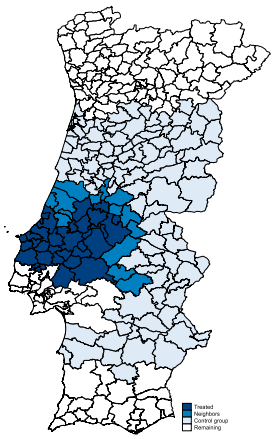
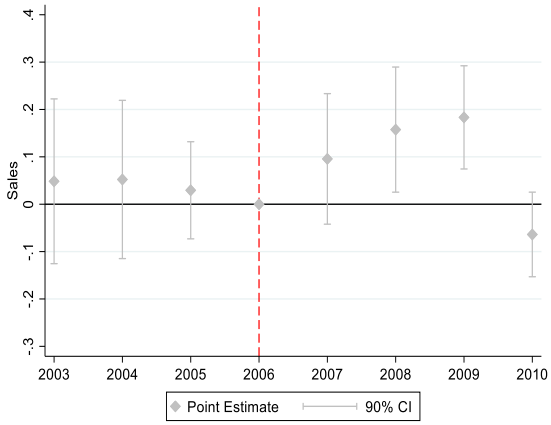


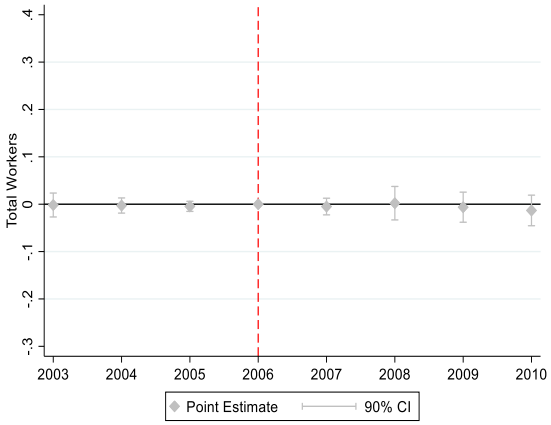
Figure 2.

Event studies

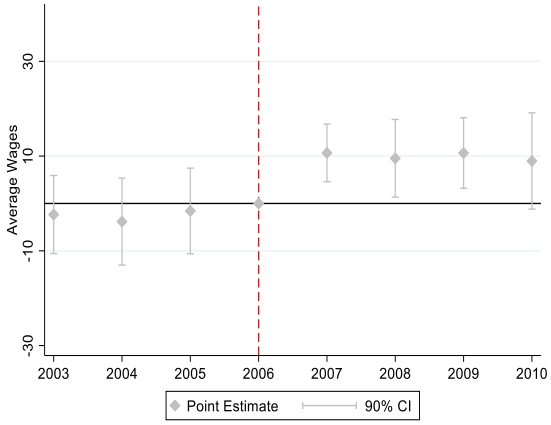
Sales (€ / year)



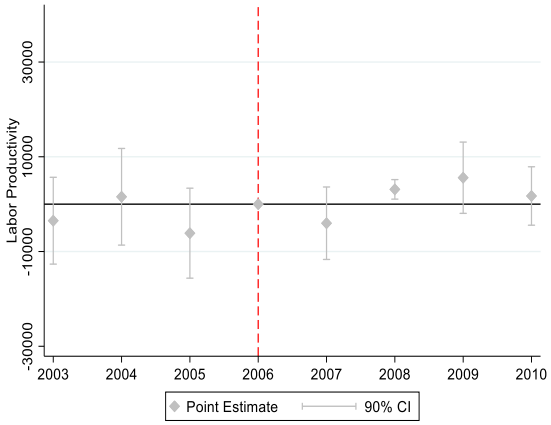
Total Workers



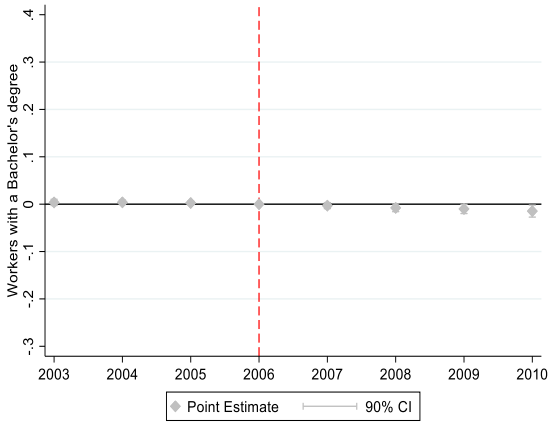
Average Wages (€ / month)



Labour Productivity (Sales / Workers)



Workers with a Bachelor's degree



Notes: This Figure presents the results of equation (2), with a confidence interval of 90%. Sales, Total Workers, and Workers with a bachelor's degree were transformed using the inverse hyperbolic sine approach.

Appendix – Tables

Table A1. Descriptive statistics

Variable:	N	Mean	SD	Range
Panel A				
Treated				
ihs Sales (€ / year)	158 912	11,81	3,50	0 - 20,61
ihs Total Workers	158 952	1,78	0,94	0,88 - 7,07
Average Wages (€ / month)	158 952	678,67	355,57	357 - 31 744,40
Labour Productivity (Sales / Workers)	158 912	82 833,93	574 505,00	0 - 128 000 000
ihs Workers with a BSc	158 952	0,25	0,62	0 – 7,61
ihs Total Wages (€ / month)	158 952	8,15	1,17	6,57 – 14,37
ihs Number of firms (Municipality)	264	6,74	0,88	4,70 – 8,45
ihs Number of new firms (Municipality)	264	4,30	0,90	1,82 – 6,05
ihs Number of exiting firms (Municipality)	264	4,32	0,95	1,82 – 6,56
Neighbours				
ihs Sales (€ / year)	80 437	11,92	3,50	0 – 19,63
ihs Total Workers	80 458	1,85	0,97	0,88 - 7,06
Average Wages (€ / month)	80 458	738,03	2 765,64	357 - 775 646
Labour Productivity (Sales / Workers)	80 437	80 092,20	227 306	0 – 18 700 000
ihs Workers with a BSc	80 458	0,26	0,61	0 – 5,58
ihs Total Wages (€ / month)	80 458	8,28	1,23	6,57 – 14,25
ihs Number of firms (Municipality)	832	6,15	0,93	4,09 – 8,80
ihs Number of new firms (Municipality)	832	3,61	0,99	0,88 – 6,41
ihs Number of exiting firms (Municipality)	832	3,63	1,04	0,88 – 6,85
Control group				
ihs Sales (€ / year)	310 185	11,74	3,49	0 – 20,75
ihs Total Workers	310 283	1,79	0,96	0,88 – 8,07
Average Wages (€ / month)	310 283	1,40	0,81	0,69 – 7,38
Labour Productivity (Sales / Workers)	310 184	660,69	373,87	357 - 79 160,50
ihs Workers with a BSc	310 283	0,25	0,62	0 – 7,61
ihs Total Wages (€ / month)	310 283	8,13	1,18	6,57 – 15,56
ihs Number of firms (Municipality)	112	6,52	1,16	4,61 – 9,10
ihs Number of new firms (Municipality)	112	3,91	1,20	1,44 – 6,70
ihs Number of exiting firms (Municipality)	112	4,00	1,21	1,44 – 7,01
Panel B				
Treated				
log Government transfers	264	15,64	0,48	14,84 – 16,89
log Municipalities' current expenses	264	9,13	0,57	7,87 – 10,60
log Electricity for domestic purposes	264	10,11	0,79	8,21 – 11,56
log Electricity for industrial purposes	264	10,02	1,30	6,41 – 12,03
Neighbours				
log Government transfers	112	15,70	0,46	15,09 – 16,92
log Municipalities' current expenses	112	8,86	0,69	7,78 – 10,75

log Electricity for domestic purposes	112	9,68	1,05	8,03 – 12,07
log Electricity for industrial purposes	112	9,55	1,94	6,62 – 12,83
Control group				
log Government transfers	832	15,65	0,43	14,35 – 17,14
log Municipalities' current expenses	832	8,80	0,61	7,19 – 11,17
log Electricity for domestic purposes	832	9,49	0,91	7,37 – 12,33
log Electricity for industrial purposes	832	9,27	1,66	5,40 – 14,02

Table A2. Robustness: Employing a logarithmic transformation

	Sales (log)	Total Workers (log)	Average Wages	Labour Productivity	Workers with a Bsc (log)
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline					
Treated * Post-Treatment	0,071*	-0,002	11,786***	3 245,363	-0,008*
	(0,04)	(0,02)	(2,88)	(4 188,18)	(0,00)
Adj R2	0,37	0,90	0,53	0,45	0,85
N	451 318	451 442	451 442	451 317	451 442
Panel B: By Sector – Tradable versus Non-Tradable					
Non-Tradable					
Treated * Post-Treatment	0,093**	-0,002	12,605***	5 065,980	-0,009**
	(0,04)	(0,02)	(3,61)	(6 466,73)	(0,00)
Adj R2	0,37	0,88	0,52	0,45	0,85
N	297 737	297 811	297 811	297 736	297 811
Tradable					
Treated * Post-Treatment	0,020	-0,001	9,480**	-2 404,872*	-0,008
	(0,06)	(0,02)	(4,02)	(1 212,69)	(0,01)
Adj R2	0,39	0,91	0,55	0,33	0,87
N	151 226	151 274	151 274	151 226	151 274
Panel C: By Sector – Services versus Manufacturing					
Services					
Treated * Post-Treatment	0,121***	0,000	10,261***	5 465,534	-0,009**
	(0,03)	(0,02)	(2,76)	(8 087,44)	(0,00)
Adj R2	0,38	0,89	0,53	0,47	0,85
N	276 243	276 306	276 306	276 242	276 306
Manufacturing					
Treated * Post-Treatment	0,033	-0,011	14,649***	-1 087,061	-0,010
	(0,05)	(0,02)	(3,61)	(2 130,16)	(0,01)
Adj R2	0,37	0,90	0,53	0,39	0,87
N	140 931	140 969	140 969	140 931	140 969
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipal Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variables in column (1), (2), and (5) have suffered a logarithmic transformation; Our regressor of interest, Treated * Post-Treatment, indicates firms producing in one of the 33 Treated municipalities, during the treatment period (2007-2010). Our analysis includes the 2003-2010 period. Clustered standard errors, at the NUT3 level, are presented in parenthesis; Significance level at which the null hypothesis is rejected: *** 1%, ** 5%, * 10%.

Table A3. Robustness: without crisis period (2003-2008)

	Sales (ihs)	Total Workers (ihs)	Average Wages	Labour Productivity	Workers with a BSc (ihs)
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline					
Treated * Post-Treatment	0,101*	0,002	11,915***	1 089,194	-0,008
	(0,06)	(0,02)	(2,71)	(3 740,42)	(0,00)
Adj R2	0,41	0,89	0,55	0,40	0,86
N	335 063	335 063	335 063	335 063	335 063
Panel B: By Sector – Tradable versus Non-Tradable					
Non-Tradable					
Treated * Post-Treatment	0,108*	0,004	12,202***	1 452,584	-0,009*
	(0,06)	(0,02)	(3,90)	(5 576,23)	(0,00)
Adj R2	0,41	0,88	0,54	0,40	0,86
N	220 039	220 039	220 039	220 039	220 039
Tradable					
Treated * Post-Treatment	0,069	-0,001	12,497**	-1 455,795	-0,007
	(0,08)	(0,02)	(4,49)	(1 295,75)	(0,01)
Adj R2	0,42	0,91	0,56	0,34	0,87
N	112 686	112 686	112 686	112 686	112 686
Panel C: By Sector – Services versus Manufacturing					
Services					
Treated * Post-Treatment	0,144**	0,006	11,962***	2 302,704	-0,009**
	(0,06)	(0,02)	(2,93)	(6 875,10)	(0,00)
Adj R2	0,42	0,89	0,57	0,41	0,86
N	202 031	202 031	202 031	202 031	202 031
Manufacturing					
Treated * Post-Treatment	0,057	-0,004	12,182***	-1 570,155	-0,010
	(0,08)	(0,03)	(3,84)	(1 546,34)	(0,01)
Adj R2	0,39	0,90	0,49	0,37	0,87
N	107 735	107 735	107 735	107 735	107 735
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipal Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variables in column (1), (2), and (5) have suffered an inverse hyperbolic sine transformation; Our regressor of interest, Treated * Post-Treatment, indicates firms producing in one of the 33 Treated municipalities, during the treatment period (2007-2010). Our analysis includes the 2003-2008 period. Clustered standard errors, at the NUT3 level, are presented in parenthesis; Significance level at which the null hypothesis is rejected: *** 1%, ** 5%, * 10%.

Table A4. Robustness: including North Region in the control group

	Sales (ihs)	Total Workers (ihs)	Average Wages	Labour Productivity	Workers with a BSc (ihs)
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline					
Treated * Post-Treatment	0,036	-0,017	10,722***	2 936,537	-0,008*
	(0,03)	(0,02)	(2,58)	(3 821,66)	(0,00)
Adj R2	0,36	0,89	0,43	0,70	0,85
N	1 094 724	1 094 982	1 094 982	1 094 716	1 094 982
Panel B: By Sector – Tradable versus Non-Tradable					
Non-Tradable					
Treated * Post-Treatment	0,071*	-0,021	11,725***	4 074,272	-0,011***
	(0,04)	(0,02)	(2,13)	(5 985,58)	(0,00)
Adj R2	0,36	0,87	0,51	0,71	0,85
N	703 766	703 933	703 933	703 759	703 933
Tradable					
Treated * Post-Treatment	-0,048	-0,010	8,582*	-624,620	-0,001
	(0,04)	(0,02)	(4,20)	(827,20)	(0,01)
Adj R2	0,38	0,91	0,35	0,40	0,86
N	384 954	385 043	385 043	384 953	385 043
Panel C: By Sector – Services versus Manufacturing					
Services					
Treated * Post-Treatment	0,126***	-0,010	8,755***	4 103,906	-0,011***
	(0,02)	(0,02)	(2,67)	(7 618,36)	(0,00)
Adj R2	0,37	0,87	0,57	0,44	0,85
N	652 832	652 979	652 979	652 827	652 979
Manufacturing					
Treated * Post-Treatment	-0,075	-0,043	15,295***	1 279,651	-0,003
	(0,06)	(0,03)	(2,43)	(2 422,58)	(0,01)
Adj R2	0,36	0,89	0,25	0,75	0,86
N	393 614	393 698	393 698	393 611	393 698
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipal Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variables in column (1), (2), and (5) have suffered an inverse hyperbolic sine transformation; Our regressor of interest, Treated * Post-Treatment, indicates firms producing in one of the 33 Treated municipalities, during the treatment period (2007-2010). Our analysis includes the 2003-2010 period. Our control group includes the North Region (see Figure 1). Clustered standard errors, at the NUT3 level, are presented in parenthesis; Significance level at which the null hypothesis is rejected: *** 1%, ** 5%, * 10%.

Table A5. Robustness: Coarsened Exact Matching

	Sales (ihs)	Total Workers (ihs)	Average Wages	Labour Productivity	Workers with a BSc (ihs)
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline					
Treated * Post-Treatment	0,058	-0,002	9,751***	-2 006,952	-0,010**
	(0,04)	(0,02)	(2,74)	(2 020,11)	(0,00)
Adj R2	0,39	0,89	0,50	0,23	0,86
N	297 643	297 722	297 722	297 642	297 722
Panel B: By Sector – Tradable versus Non-Tradable					
Non-Tradable					
Treated * Post-Treatment	0,088**	-0,003	10,201**	-1 462,967	-0,012***
	(0,04)	(0,02)	(3,50)	(2 874,30)	(0,00)
Adj R2	0,38	0,87	0,49	0,22	0,85
N	198 225	198 271	198 271	198 224	198 271
Tradable					
Treated * Post-Treatment	-0,007	0,001	8,880**	-3 174,229**	-0,006
	(0,05)	(0,02)	(3,44)	(1 243,73)	(0,01)
Adj R2	0,39	0,91	0,52	0,28	0,86
N	99 418	99 451	99 451	99 418	99 451
Panel C: By Sector – Services versus Manufacturing					
Services					
Treated * Post-Treatment	0,105**	0,000	7,008**	-2 836,289	-0,011***
	(0,04)	(0,02)	(2,87)	(3 057,77)	(0,00)
Adj R2	0,39	0,89	0,50	0,21	0,86
N	176 646	176 684	176 684	176 645	176 684
Manufacturing					
Treated * Post-Treatment	0,002	-0,011	14,871***	-1 277,289	-0,010
	(0,05)	(0,02)	(3,12)	(1 166,38)	(0,01)
Adj R2	0,37	0,89	0,50	0,36	0,87
N	99 321	99 345	99 345	99 321	99 345
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipal Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variables in column (1), (2), and (5) have suffered an inverse hyperbolic sine transformation; Our regressor of interest, Treated * Post-Treatment, indicates firms producing in one of the 33 Treated municipalities, during the treatment period (2007-2010). Our analysis includes the 2003-2010 period. Clustered standard errors, at the NUT3 level, are presented in parenthesis; Significance level at which the null hypothesis is rejected: *** 1%, ** 5%, * 10%.

Table A6. Firm dynamics

	Number of firms (ihs) (1)	Number of new firms (ihs) (2)	Probability of closing (3)
Panel A: Baseline			
Treated * Post-Treatment	-0,011	0,046	0,003
	0,040	0,052	0,005
Adj R2	0,99	0,91	0,35
N	1 096	1 096	451 442
Year Fixed Effects	Yes	Yes	Yes
Municipal Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	No	No	Yes

Notes: Dependent variables in column (1), and (2) have suffered an inverse hyperbolic sine transformation; The first two columns are presented at the municipality level, while column (3) is at the firm level; We define entry in the market if the firm was not observed in the previous two years, and exit if the firm is not observed in the following two years. Our regressor of interest, Treated * Post-Treatment, indicates firms producing in one of the 33 Treated municipalities, during the treatment period (2007-2010). Our analysis includes the 2003-2010 period. Clustered standard errors, at the NUT3 level, are presented in parenthesis; Significance level at which the null hypothesis is rejected: *** 1%, ** 5%, * 10%.

Appendix –Figures

Figure A1.

Geographical dispersion

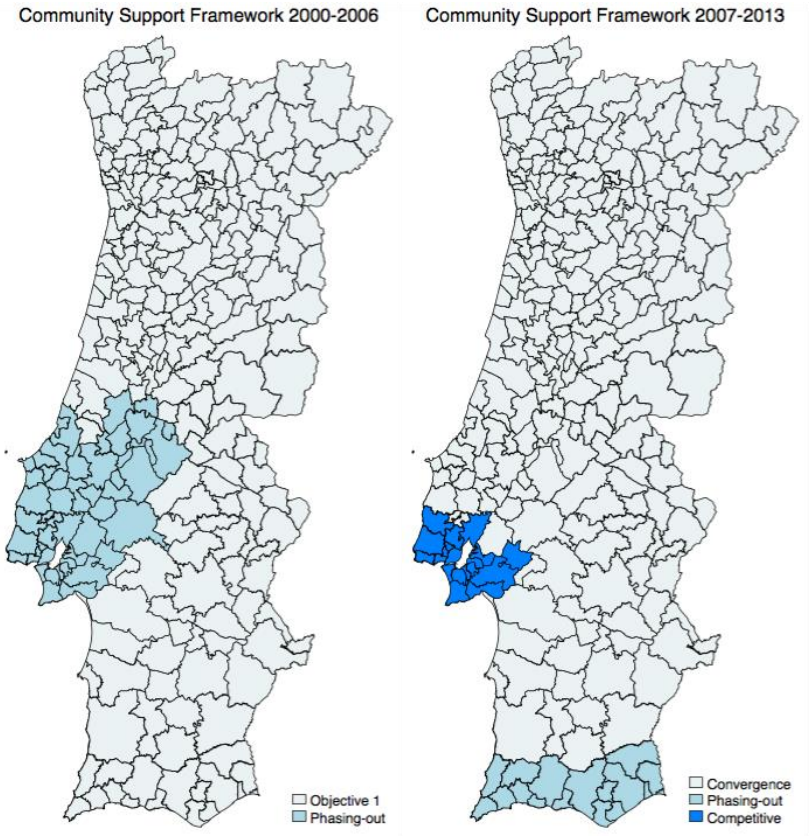
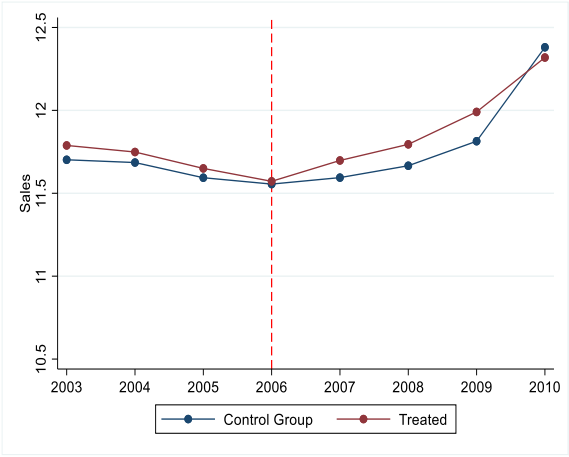


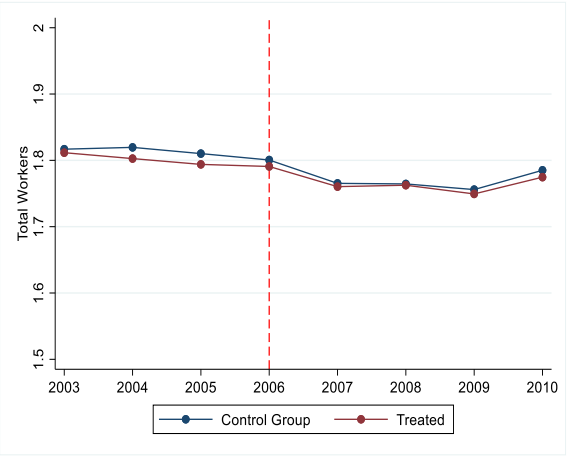
Figure A2.

Descriptive graphical evidence

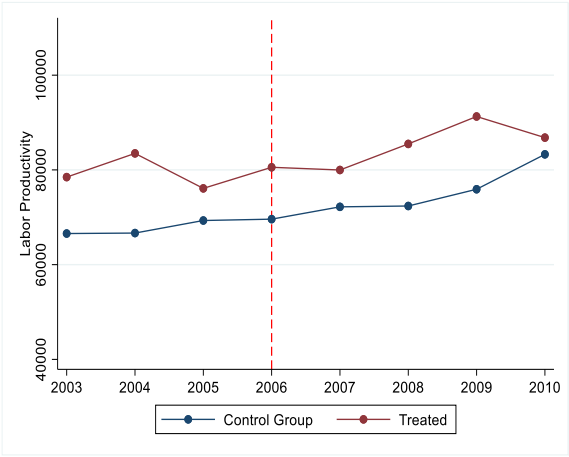
Sales (€ / year)



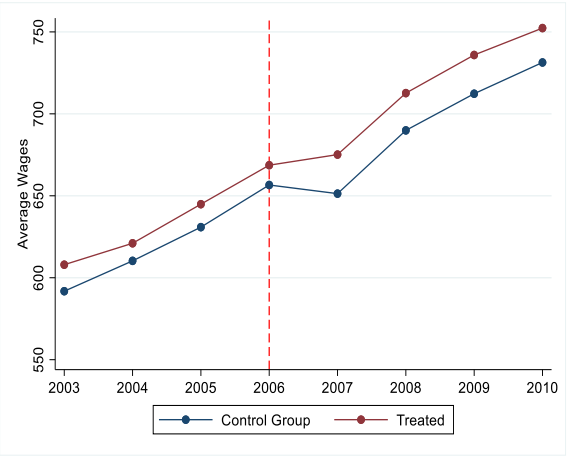
Total Workers



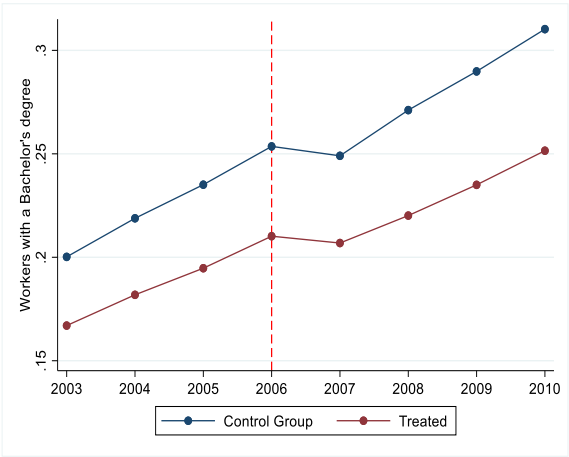
Average Wages (€ / month)



Labour Productivity (Sales / Workers)



Workers with a Bachelor's degree



Notes: This Figure presents the evolution of our five main outcome variables, for the Treated and control group. Sales, Total Workers, and Workers with a bachelor's degree were transformed using the inverse hyperbolic sine approach.

Appendix – B. Further details on the CEM method

The first phase of the CEM method is to stratify firms according to their observables. In our case, we form groups of firms that are in the same decile regarding the distribution of sales, number of workers, and average wages in the year preceding treatment (i.e., 2006). This way, we create a total of 1000 stratums, so that firms in the same stratum belong to the same decile in the distribution of sales, number of workers and average wages. Out of those 1000 stratums, in only 12 are firms in both the Treated and control groups, so firms in the remaining stratums were excluded from this analysis for not having a compatible enough counterfactual. From our initial baseline specification, about a third of the observations were excluded – taking us from around 451 000 to 297 000 observations.

The second part of the method is to estimate our DiD equation on this new reduced sample, with the CEM weights. The CEM weights guarantee that within each stratum, the sum of the weights of Treated and control group firms are the same, and that each Treated observation is weighted the same, regardless of its stratum.