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European Funds and Firm Performance: Evidence from a Natural Experiment

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ABSTRACT

European Funds and Firm Performance: Evidence from a Natural Experiment*

This paper analyses the impact of European Union (EU) funds on the performance of private firms. In particular, we examine a quasi-natural experiment consisting of a redrawing of administrative areas that expanded regional eligibility and led to a sudden increase in accessibility to EU grants for firms located in 33 Portuguese municipalities. Using a comprehensive linked employer-employee administrative dataset that covers the universe of private firms between 2003 and 2010, our difference-in-differences estimates uncover a significant and positive causal effect of increased eligibility on firms' sales, labour productivity, and average wages, while employment is not significantly altered. While firms' sales in the non-tradable sectors are positively impacted, firms' sales in more competitive, tradable, sectors remain unaffected by increased access to EU funds.

JEL Classification: C21, R10

Keywords: grants, regional policy, private firm, municipalities, Portugal

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I. Introduction

As stated in the Treaty of Lisbon, the European Union (EU) pursues socioeconomic cohesion among its member countries, namely by channelling substantial funds from the EU's budget to regions with income per capita below 75 percent of the EU average.[5] The stated objective is to boost regional income, employment growth, and facilitate business creation. Thus far, the actual results of this policy are hard to assess, 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). Nonetheless, effects vary considerably depending on local conditions (Becker et al. 2013), the effect of transfers seems to be subject to decreasing returns (Becker et al. 2012; Cerqua and Pellegrini 2018), or its effects are merely temporary (Barone et al. 2016; Di Cataldo 2017; Becker et al. 2018). In fact, GDP per capita across EU-15 metro regions has 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 (Portugal), where administrative areas were redrawn, leading to a sudden increase in eligibility to EU funds in specific regions.[6] This decision came in the wake of the Lisbon area surpassing the 75 percent of the EU average income per capita threshold, compromising the flow of funds to several municipalities. The region was split so that the poorer, further away from Lisbon, municipalities maintained privileged eligibility status.

As EU regions tend to progressively lose eligibility, due to convergence, an administratively mandated increase in eligibility is a rare occurrence. Thus, and unlike most empirical papers thus far, here we analyse the impact of *increased* eligibility on firm performance.[7] We further contribute to the literature by using a comprehensive linked employer-employee administrative dataset that covers the totality of Portuguese private firms, between 2003 and 2010, allowing us to uncover heterogeneous effects ignored in the literature to date, and crucial in understanding the mechanism through which EU grants have affected the performance of firms to date.

[5] While relative to national budgets the EU's common budget is small – accounting for close to 1 percent of its 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 status.

We find evidence that a higher eligibility status to EU funds for a municipality increased private firms' sales, *on average*, by 7.4% *vis-à-vis* firms in municipalities that experienced no change. While average wages increase marginally, total employment numbers do not seem to change in response to the shock. Remarkably, our novel heterogeneity analysis shows that the positive effects are solely concentrated in firms in the less competitive, *Non-Tradable,* sectors. In other words, the performance of firms subject to fiercer international competition remains unaltered after the treatment.

Our intent-to-treat estimates from a difference-in-differences specification (hereafter, diff-in-diff) yield the unbiased causal impact of increased eligibility under the parallel trends assumption – had the redistricting not been implemented, the outcomes of Treated firms would have evolved similarly to those of comparison firms. For that reason, our control group only considers firms that were and remained in high-eligibility status regions, with the exception of those in municipalities that neighbour Treated firms (to account for possible spillover effects). The parallel trend assumption is supported both by descriptive graphical inspection and event study specifications. Moreover, our findings are robust to exercises exploiting the time and the spatial dimensions, including alternative control groups and the exclusion of the crisis period (2009-2010).

A full accounting of the effects of the policy must consider possible spillover effects of treated to neighbouring comparison areas. 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 are not present. Furthermore, we also uncover a null impact on the total number of firms and firms' creation in Treated municipalities, as well as no change in the probability of firm closure, indicating increased access to EU funds does not significantly affect firms' dynamics.

We shed light on the possible mechanisms of adjustment, relying on municipal-level data to analyze geographic exposure to the policy. Firstly, we find that European funds transferred to firms in Treated areas increased substantially, while there is no change in transfers of European funds to local government, nor central government's transfers from the state budget, and no change in current local government expenditures. These results suggest the improvement in firms' performance is indeed the result of increased eligibility for EU funds. Notably, increased eligibility leads to an increase in local wealth, as proxied by families' electricity consumption - suggesting that increased firm sales may stem from greater access to funds by individuals and workers.

Our results have important policy implications for policymakers willing to improve the design of place-based policies. First, we highlight the importance of relying on firm-level data. Indeed, we show that the effects of increased EU eligibility are heterogenous, with the effect on sales driven solely by firms in *Non-Tradable* sectors. Second, the null effects on sales, productivity gains and employment by firms in *Tradable* sectors 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 heretofore strongly supported by the EU's Cohesion Policy witness 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. Section 5 concludes.

II. Related literature

Growth and convergence across European regions have been a political priority of the EU for decades. It gained importance over time, as relatively prosperous countries in Southern and then Eastern Europe adhered.

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] Other empirical approaches have been attempted such as instrumental variable estimates - Ramajo et al. (2008); (dynamic) panel data techniques - Rodríguez-Pose and Fratesi (2004); a combination of the two - Bouayad-Agha et al. (2013); bayesian methods – Cuaresma et al. (2012); or spatial growth models – Fiaschi et al. (2017). However, the empirical evidence is mixed and remains controversial (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) with data from three programming periods (from 1989 to 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.

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 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 Italian regions more efficiently.

We contribute to the literature evaluating cohesion policy funds using the universe of private firms as units of observation, rather than municipalities or NUTS 2 regions. Fattorini et al. (2020), using propensity score matching techniques and focusing exclusively on manufacturing firms, report 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 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. Another relevant reference

[10] Pellegrini et al. (2013) largely confirm the results of Becker et al. (2010) using Eurostat data.

is Benkovskis et al. (2019), who show that, after conditioning on the fact that more productive and larger firms have a higher propensity to acquire EU funds, these regional support programmes boost firms' turnover and employment in Lithuania.

For Portugal, Santos (2019) relies on a sample of around 300 firms that applied for an innovation subsidy granted by European funds during 2007-2011, finding positive effects on employment, sales, and investment for those who received it. Using data for the same time period, Martins (2021) investigates the impact of a large training programme sponsored by the European Social Fund (ESF) concluding that it had a significant positive effect on sales, value added, employment, and productivity. Alexandre et al. (2022) examines the impact of being awarded a second investment grant to the same firm, uncovering a positive effect on labour productivity for small firms.

Our paper further relates to the literature on the causal impact of place-based policies, surveyed in Kline and Moretti (2014a). Leveraging on 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 observed an increase in employment and local workers' real wages. Kline and Moretti (2014b) study the long-run effects of 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 neighbouring areas may also be negative if spatially targeted policies have business-stealing effects (Hanson and Rohlin 2013; Andini and Blasio 2014; Einiö and Overman 2020). In this paper, we also consider possible spillover effects by analysing the effects of treatment on neighbouring municipalities.

[11]Gobillon et al. (2012) and Mayer et al. (2017) discuss similar schemes in France, while Einiö and Overman (2020) and Criscuolo et al. (2019) do it for two cases in the UK using firm-level data. Examining manufacturing firms in Italy, Bernini and Pellegrini (2011) investigated the impact of state regional policy to subsidized firms, 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 Institutional background

Portugal has been a recipient of European funding in the context of the distinct 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 fewer (more) resources from the EU cohesion and structural funds.

After 2007, Mainland Portugal can be divided into three distinct regional groups as far as eligibility to EU funds is concerned, as illustrated in Figure A1 in the Appendix. The first comprises the North, Centre, and Alentejo regions, which are part of the Convergence objective, associated with the most favourable access to funding. The second is the Algarve, in the south, a region part of the phasing out regime, with per capita GDP above the 75% income threshold – for the 25 EU countries considered at the time, 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 Competitive objective, with lower eligibility.[13]

One important difference between the CSF before (*QCA – Quadro Comunitário de Apoio III*) and after (*QREN – Quadro de Referência Estratégico Nacional*) 2007 is related with the thematic allocation of funds: investment in infrastructures saw its relative importance diminished from 48% to 39%, while investment in professional training increased, from 17% to 23% (Pires, 2017).

3.2 Data

In the empirical analysis in this study, we benefit from a longitudinal administratively linked employer-employee dataset, *Quadros de Pessoal*, compiled by the ministry responsible for employment affairs and, for that reason, of mandatory compliance. *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 and education, and

[13] As can be seen in Figure A1 in the Appendix, the previous NUTS 2 region of Lisbon and the Tagus valley was severely reduced with the incorporation of some its NUTS 3 regions in other NUTS 2: Oeste and Médio Tejo was transferred to NUTS 2 Centre, whereas Lezíria do Tejo was transferred to 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.

firm level - sales, number of employees, sector of economic activity, location, and legal structure.[15]

We selected four firm-level indicators to evaluate firm performance. Those indicators are total sales - in € per year, the number of total workers, labour productivity - measured as the sum of sales per worker, and monthly average wages - which includes the fixed and the variable wage components. We winsorize these levels at 1% from each tail.[16]

We take the inverse hyperbolic sine (ihs) transformation of the first two dependent variables, an approach that has the advantage of allowing us to consider zeros in variables such as the number of graduate workers. 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.

We complement our analysis with municipal-level administrative data. Micro beneficiary data on European funds at the firm level is unfortunately not available for the pre-treatment period. However, we were able to obtain data on European funds transferred to firms from the Central Government, namely the Central State Administration, which is the Management Authority for the Competitiveness and Internationalization of European Funds for firms (COMPETE) and to municipalities from the Directorate general of local government (DGAL), both aggregated per municipality. Information on electricity consumption, for domestic and industrial purposes - in thousands of kilowatt hours, is obtained from the government agency for Energy and Geology (DGEG). Data regarding transfers from the central government to municipalities, as well as concerning the current expenses of municipalities (in Euros), are obtained from DGAL.

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

3.3 Identification strategy and econometric analysis

[15] We imposed four data restrictions. First, we excluded employees whose registered age was under 17 or over 65 years old. Second, we focused solely on workers with a monthly wage higher than the mandatory national minimum wage. Third, we excluded firms without sales in every year they appear in the dataset. Finally, we excluded firms with more than one establishment, as information on sales is not available at that level.

[16] Our results are robust if we don't winsorize our sample and are shown in previous working paper versions of this manuscript.

Considering that each firm's likelihood of access to EU funds depends on a range of observed and unobserved variables, a mere comparison between subsidized and non-subsidized firms in a certain region will likely produce biased results. Instead, we rather assess the impact of higher eligibility on firm performance using a natural experiment, a change that is entirely exogenous from the point of view of individual firms, in an intention to treat setting.

We thus exploit the spatial discontinuity in access to European funds which occurred between 2006 and 2007, derived from the redistricting of the Lisbon NUTS 2 area. This change was decided by the central government, with the approval of the European Commission and the Eurostat.[17] A set of contiguous municipalities to the north of Lisbon were singled out and witnessed a sudden raise in eligibility to EU funds, thus experiencing a break between the pre-treatment period, 2003 to 2006, and the post-treatment period, from 2007 to 2010. We do not include the period after 2011 as it marks the year when Portugal was subject to constraints stemming from the request for financial assistance from the IMF, the European Commission, and the European Central Bank, and forced to dramatically change its economic policies. Our identification strategy uses the universe of 39 748 private firms located in the 33 municipalities pertaining to the NUTS 3 regions of Oeste, Médio Tejo, and Lezíria do Tejo, those who gained greater access to EU funds due to the administrative territorial redrawing.

It is well-known that methods comparing outcomes in a treated region to those in adjacent regions may yield biased estimates for policies with spillover effects (Jardim et al. 2022). Hence, we exclude firms neigbouring Treated areas in a "buffer-zone" or "donut-hole" approach to mitigate the possibility of spillover effects from treatment. The comparison municipalities have not been subject to any change in eligibility status, therefore, absent possible spillover effects, they are untreated by the redistricting shock. We also investigate for this possibility in this paper in Section 4.5.

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 in theirs or their neighbours' eligibility for EU Regional Funds, as shown in Figure 1. For the vast majority of variables employed in this study, balance tests, presented in Table A2, do not uncover any statistically significant difference between Treated and control groups for

[17] Unlike what happens in some EU member states, including 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 until the end of our sample period. In Portugal, there is no layer of government between the central government and municipalities.

2006, the year prior to the start of the treatment. Nevertheless, as a robustness exercise, we include the more distant NUTS 2 North region in the control group.

[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:

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

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 eligibility under *Objective 1/convergence*. γ_t are year fixed effects, and α_i are firm fixed effects - i.e., controls for characteristics of firms that are time-invariant.[18] 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 outcome of interest is δ, 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, which offers several advantages (Roth et al., 2023). First, we can explore further evidence suggesting that there are no differential pre-trends between treatment and comparison groups (Roth, 2022). In the absence of different pre-trends, the identifying assumption is that no systematic factors drive both the shock and the outcomes of interest. Note that this also mitigates concerns with possible anticipation effects. 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 – in this case, 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:

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

[18] Our results are robust to the inclusion of the municipality fixed effects and are shown in previous working paper versions of this manuscript.

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 four 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.[19] We also observe a positive causal effect of the increase in eligibility on sales and labour productivity in 2008 and 2009. The fact that the impact of treatment for 2007 is not statistically significant is consistent with the idea that, in that year, there may still be some spending from the previous funding period and, at the same time, some of the funding from the new period may be slow to start off. In addition, we find that the effect is not persistent and drops to zero in 2010. As to the remaining dependent variables, we find economically 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 (in column 1), corresponding to an increase of about 7.4 percent *vis-à-vis* firms in comparison municipalities.[20] We uncover estimates that are statistically indistinguishable from zero for the effect of the treatment on the number of workers (in column 2), suggesting that, while Treated firms sell significantly more, this does not create more employment. As for average wages (in column 3), our estimates advocate for a significant increase, albeit of small magnitude, so that producing in a region that gains access is associated, *on average*, with a wage increase of around 11€/month, or about 2% of the average value of monthly average wages in the treatment and control groups. We also find a significant rise in labour productivity (in column 4).

[19] Figure A2 in the Appendix presents descriptive graphical evidence that further corroborates the plausibility of the parallel trend assumption, for all outcome variables, in this context (Angrist and Pischke, 2009).
[20] We also find an effect above 7% if we use the logarithmic instead of the inverse hyperbolic sine transformation. We present these results in Table A3 in the Appendix.

[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

In Table 1 – panel B, we find that the impetus behind the sales and the labour productivity increase is driven solely by the *Non-Tradable* sector, with a statistically significant increase in sales for firms in this sector of more than 9%, on average.[21] There is no effect whatsoever for both indicators 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 – 11€/month. In what regards to employment, we find no evidence of a significant effect in both sectors.

Heterogeneous effects seem to be quite important, quantitatively, when assessing the impact of higher grant eligibility on private firms. Our contribution is therefore especially relevant given that most recent studies investigating the impact of EU cohesion funds on firm performance, at the firm level, focus exclusively on manufacturing firms (Fattorini et al. 2020; Bachtrögler et al. 2020).

4.3 Robustness

Our diff-in-diff 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, is reassuring. However, as there could still exist contemporaneous shocks that may threaten our identification strategy, we subject our evidence to a battery of robustness checks. First, as 2009 and 2010 coincides with one of the greatest recessions in economic history, in the wake of the Sovereign Debt Crisis, there is a concern that Treated municipalities might have been differently affected by shocks during

[21] The *Tradable* sector was defined according to the statistical classification of economic activities in the European Community and covers agriculture, forestry and fishing; mining and quarrying; manufacturing; transportation and storage; professional, scientific and technical activities; and administrative and support service activities.

our post-treatment period. If this recession produced differential effects across regions in a way correlated with our breakdown of municipalities into Treated and control, it would introduce confounding effects in our estimates. The event studies in Figure 2 uncover a sudden drop in the positive effect found in firms' sales in 2010, the last year of the analysis.[22] We investigate whether this event is driving our diff-in-diff results by re-estimating eq. (1) after excluding the years 2009 and 2010 from the sample,[23] We find that these exercises, as reported in Table A4 in the Appendix, are in line with the baseline estimates.

A second 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 in NUTS 2 regions close to Treated municipalities whose eligibility status – as well as their neighbours', remains unchanged. However, as shown in Table A5 in the Appendix, even if we add municipalities in the North NUTS 2, that, one the one hand, are geographically, socioeconomic, and demographically more distant from the Treated area but, on the other hand, also did not experienced any change in European funds eligibility status, our results remain unchanged, particularly for the distinct effect on firms' sales in the Non-Tradable *vis-à-vis* Tradable sectors.

Third, we tested whether our results were robust to a more refined comparison group using coarsened exact matching (CEM). The advantage of CEM is that the creation a new control group resembling the Treated firms more closely in terms of pre-treatment observable characteristics - see Appendix B for more details.[24] This procedure reduces concerns related to confounding effects biasing our estimates, 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. In Table A6 in the Appendix, we present estimates combining the CEM and diff-in-diff approaches. Once again, our main results remain significant and mostly unaffected, except for sales and labour productivity in our baseline estimation, whose point estimate becomes statistically non-significant. However, the significant increase in sales for

[22] Possible explanations might be in line with Becker et al. (2018) who show that eligibility effects weakened during the financial crisis, and with Fantino and Cannone (2013) who find that the impact of policies designed to boost innovation in small firms in an Italian region were effective exclusively in the short run.

[23] This also prevents possible confounding effects from an important shock to firms after October 2010: the introduction of tolls in the SCUT highway system (Audrestch et al. 2020; Branco et al. 2023).

[24] Iacus, King and Porro (2012) show that this method produces lower model dependence, estimation error, variance, bias, and reduces imbalance *vis-à-vis* remaining commonly employed matching methods.

Non-Tradable sectors persists, as well as the positive effect on average wages in every sector. Employment remains statistically indistinguishable from zero.

Fourth, proximity to Lisbon can be a further confounding factor, if and only if the magnitude of possible spillover effects from the capital to its vicinity changed after the implementation of the new EU eligibility status after 2007. We show in Table A7 in the Appendix a further robustness test in which we exclude all firms in the 5 treated municipalities that are closest to the capital. Our central findings remain unchanged, with differences point estimates across sectors, if anything, becoming more pronounced. Our estimated effect on sales, for the entire sample, increases in magnitude, but is measured more noisily.

Finally, we show that our results are very similar to baseline if we winsorize our data at 5% from each tail in Table A8 in the Appendix.

4.4 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. We address these issues by looking at three outcomes: the total number of firms, number of new firms and probability of exiting the sample.[25] Table A9 in the Appendix reports our results on the evolution of the total number of firms and the number of new firms at the municipality level, as well as a more granular analysis, at the firm level, analysing the probability of firms exiting the market, where we employ a linear probability model where the outcome variable takes the value 1 if the firm exits.

In all three cases, the estimated coefficients are statistically indistinguishable from zero, suggesting the absence of eligibility effects on firm dynamics. Importantly, the fact that firm dynamics are not significantly altered in the treatment period is a good indicator 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 the Treated or the control pool of firms. 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. This does not seem to be a cause for concern in this case.

[25] Figueiredo et al. (2002) and Carias et al. (2022) show that Portuguese entrepreneurs tend to locate their businesses in their place of residence. Furthermore, Branco et al. (2023) report that they do not tend to move in response to negative shocks.

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 as suggested in Glaeser and Gottlieb, 2009. To that purpose, we redefine our treatment group in this subsection to include only neighbouring municipalities – termed *Neighbours*, which experienced no change in eligibility but border a municipality where that change in eligibility occurred.. This new treatment group includes firms from 14 different municipalities, as shown in Figure 1. We keep as control group the exact same set used to produce the baseline estimates.

As observed in Table 2, the coefficients of interest for sales and total workers are small and non-statistically significant, suggesting no spillovers. On the contrary, results for average wages and labour productivity confirm the existence of positive spillovers from the Treated areas towards their *Neighbours*.

[Insert Table 2 here]

4.6 Mechanisms

Finally, we demonstrate that there was a substantial change in EU funding for firms in the treated area and discuss possible potential alternative mechanisms or confounding factors which could explain our findings using municipal-level administrative data.

In Table 3 column (1), we show how the amount of EU funds directed to firms in the Treated areas increased substantially, in the wake of the eligibility change, relatively to the evolution in the control areas. This is expected, given that transfers to firms in these areas were limited before the reform. Relatedly, as presented in Table 3 column (2), transfers from EU funds to local governments, i.e. municipalities, were not impacted. These two pieces of evidence combined confirm that the increase in eligibility was especially relevant for private firms, with no change in funding provided through local authorities.

We investigate this possibility further, by analysing the amount of transfers from the central government to municipalities, and the latter´s current expenditures. If the amount of transfers from the central government to Treated municipalities increased sizably in the period following the treatment, our initial results could be due from such change, and not the increased eligibility to EU funding itself. In Table 3 column (3), we show evidence that government transfers have not increased in Treated municipalities *vis-à-vis* our control group municipalities.

Additionally, it could also be the case that Treated municipalities autonomously increased their expenditures, financed by higher debt or local taxes, not necessarily due to transfers from central government. In column (4), we show that Treated municipalities' current expenditures have not increased, strengthening that there is no evidence that increased spending by the central or local governments played a role in our results.

Veiga (2012), in her study of the determinants of the assignment of EU funds in Portugal, argues that more funds are transferred to municipalities whose electoral results are in line with the party ruling at the national level. We present descriptive evidence that this does not affect our results: in the 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. In the post-treatment period (2007-2010), these percentages remain remarkably constant, and the differences are not statistically significant (39% for the Treated, versus 36% for the 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, we find no effects whatsoever for electricity use by manufacturing. This is additional evidence in favour of the idea that, while Treated municipalities benefitted from higher income, access to EU regional funds did not affect firm's output or productivity in the *Tradable* sectors.

[Insert Table 3 here]

V. Conclusion

The EU administers important policy initiatives that generate large public transfers to lower-income regions across the continent. While the purpose of these policies is to promote convergence though more dynamic local economies, the evidence on their impact is very mixed, and seldom based on causal empirical methodologies. This paper exploits a unique quasi-natural experiment where a redistricting decision led to a sudden increase in accessibility to EU grants for firms located in 33 Portuguese municipalities. We investigate, for the first time, the impact of regional eligibility EU cohesion funds on firm performance

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

relying on a diff-in-diff framework that uses administrative microdata for the universe of firms in the country. We place our paper in a stream of research that attempts to empirically assess the impact of place-based policies, such as the EU regional policy, on economic convergence.

We find strong evidence of a demand effect whereby Total sales of firms in Treated areas increased, on average, by more than 7%, *vis-à-vis* firms in comparison municipalities. A significant increase in Labour Productivity is also registered. The sales increase is driven entirely by firms in the *Non-Tradable*, with no effect whatsoever in the more competitive *Tradable* sectors. This heterogeneity is novel in the empirical analysis, and crucial to understand how EU grants impact firms' performance. Furthermore, increased access to EU funds did not produce significant increase in employment, and only a marginal, though significant, increase in average monthly wages, equivalent to 2% of the average value. Our results raise questions regarding the effectiveness and the sustainability of these effects in the long-term.

This paper suggests 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 EU funds.

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Tables

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

	Sales (ihs)	Total Workers (ihs)	Average Wages	Labour Productivity
	(1)	(2)	(3)	(4)
Panel A: Full Sample				
Treated * Post-Treatment	0,074*	-0,003	11,193***	1 575,692**
	(0,04)	(0,02)	(2,42)	(704,46)
Adj R2	0,36	0,88	0,73	0,71
N	451 318	451 442	451 442	451 317
Panel B: By Sector – Non-Tradable versus Tradable				
Non-Tradable				
Treated * Post-Treatment	0,094**	-0,004	11,334***	2 108,291**
	(0,04)	(0,02)	(3,20)	(831,16)
Adj R2	0,36	0,87	0,73	0,74
N	297 737	297 811	297 811	297 736
Tradable				
Treated * Post-Treatment	0,022	-0,001	10,695***	-124,995
	(0,06)	(0,02)	(3,03)	(940,35)
Adj R2	0,38	0,90	0,73	0,64
N	151 226	151 274	151 274	151 226
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes

Notes: : Dependent variables in columns (1) and (2) were transformed using the inverse hyperbolic sine approach; 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
	(1)	(2)	(3)	(4)
Neighbours * Post-Treatment	0,013	-0,003	20,147***	2 604,540***
	(0,04)	(0,01)	(5,13)	(793,69)
Adj R2	0,36	0,88	0,74	0,70
N	376 606	376 719	376 719	376 605
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes

Notes: Dependent variables in columns (1) and (2) were transformed using the inverse hyperbolic sine approach; Our regressor of interest, Neighbours * Post-Treatment, indicates firms producing in one of the 14 municipalities neighbours 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

	EU transfers – firms (ihs)	EU transfers – municipalities (ihs)	Government transfers (ihs)	Municipalities' current expenses (ihs)	Electricity	
					For domestic purposes (ihs)	For industrial purposes (ihs)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Treated						
Treated * Post-Treatment	1,787**	-0,264	0,015	0,014	0,032***	-0,016
	(0,72)	(0,76)	(0,01)	(0,02)	(0,00)	(0,10)
Adj R2	0,43	0,53	0,96	0,97	1,00	0,98
N	1 096	1 096	1 096	1 096	1 096	1 096
Panel B: Neighbours						
Neighbours * Post-Treatment	-0,995	0,141	0,030**	0,005	-0,010	-0,073
	(1,28)	(0,22)	(0,01)	(0,08)	(0,01)	(0,08)
Adj R2	0,446	0,56	0,97	0,79	1,00	0,98
N	944	944	944	944	944	944
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Our regressors of interest, Treated * Post-Treatment and Neighbours * Post-Treatment indicate firms producing in Treated or Neighbors municipalities, respectively, during the treatment period (2007-2010). Our analysis spans 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 Neighbours and Comparison Municipalities

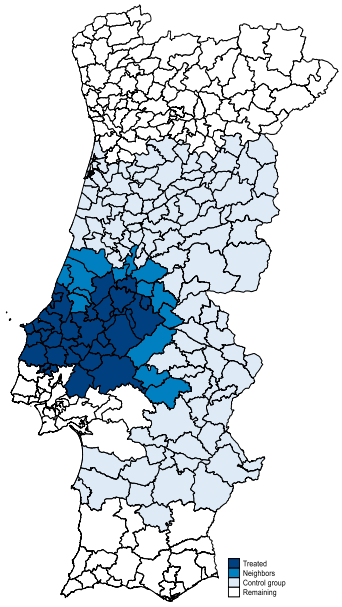
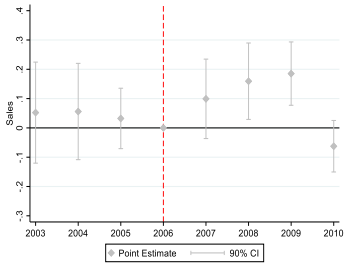
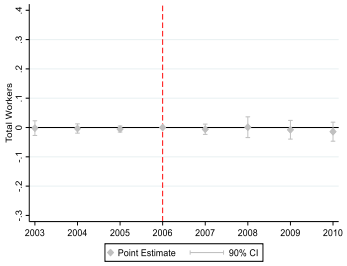


Figure 2. Event studies

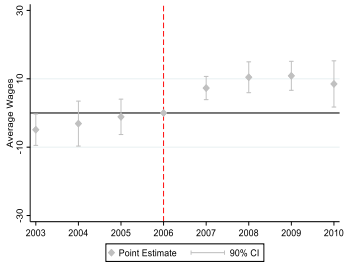
Sales (€ / year)



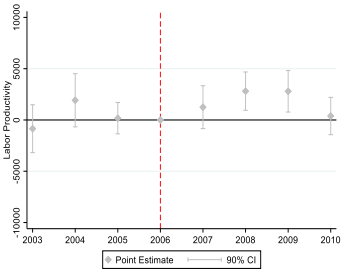
Total Workers



Average Wages (€ / month)



Labour Productivity (Sales / Workers)



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

Appendix – Tables

Table A1. Descriptive statistics

Variable:	N	Mean	SD
Panel A. Firm-level			
Treated			
Sales (€ / year)	158 912	450 965	1 077 308
Total Workers	158 952	5,13	8,97
Average Wages (€ / month)	158 952	673,88	283,35
Labor Productivity (Sales / Workers)	158 912	69 924	102 354
Neighbours			
Sales (€ / year)	80 437	490 083	1 103 751
Total Workers	80 458	5,65	9,60
Average Wages (€ / month)	80 458	722,58	319,59
Labor Productivity (Sales / Workers)	80 437	71 238	101 708
Control group			
Sales (€ / year)	310 185	421 021	1 026 399
Total Workers	310 283	5,32	9,48
Average Wages (€ / month)	310 283	655,80	277,87
Labor Productivity (Sales / Workers)	310 184	64 732	93 583
Panel B. Municipal-level			
Treated			
Government transfers	264	7 022 024	3 723 572
EU transfers – firms	264	415 352	816 405
EU transfers - municipalities	264	1 197 675	1 174 696
Municipalities' current expenses	264	10 873	6 422
Electricity for domestic purposes	264	32 578	23 706
Electricity for industrial purposes	264	43 306	44 015
Neighbours			
Government transfers	112	7 395 859	4 260 908
EU transfers – firms	112	1 334 892	2 410 486
EU transfers - municipalities	112	1 084 268	908 869
Municipalities' current expenses	112	9 371	9 184
Electricity for domestic purposes	112	29 687	41 147
Electricity for industrial purposes	112	70 622	116 876
Control group			
Government transfers	832	6 925 835	3 562 690
EU transfers – firms	832	1 033 265	3 094 024
EU transfers - municipalities	832	1 163 089	1 482 077
Municipalities' current expenses	832	8 334	7 725
Electricity for domestic purposes	832	21 520	28 730
Electricity for industrial purposes	832	51 728	141 915

Table A2. Balance tests

Variable:	Treated	Control group	Diff
	(1)	(2)	(3)
Panel A. Firm-level			
Sales (ihs)	11,57	11,55	0,02
	(3,87)	(3,78)	(0,88)
Total Workers (ihs)	1,79	1,80	-0,01
	(0,93)	(0,95)	(0,84)
Average Wages (€ / month)	664,15	650,93	13,21
	(273,97)	(274,65)	(0,28)
Labor Productivity (Sales / Workers)	67 683,97	63 256,22	4 427,75
	(100 658,07)	(93 009,20)	(0,14)
N	19 826	38 300	58 126
Panel B. Municipal-level			
Government transfers (ihs)	16,30	16,32	-0,02
	(0,463)	(0,427)	(0,75)
EU transfers – firms (ihs)	7,74	8,95	-1,21
	(5,4)	(6,23)	(0,50)
EU transfers – municipalities (ihs)	13,59	13,63	-0,03
	(3,62)	(2,67)	(0,93)
Municipalities' current expenses (ihs)	9,78	9,46	0,32***
	(0,55)	(0,58)	(0,01)
Electricity for domestic purposes (ihs)	10,81	10,19	0,63***
	(0,79)	(0,92)	(0,01)
Electricity for industrial purposes (ihs)	10,77	9,97	0,80**
	(1,29)	(1,66)	(0,02)
N	33	104	137

Notes: The analysis corresponds to 2006, the last year prior to treatment. Clustered standard errors, at the NUT3 level, are presented in parenthesis, except for column (3), where p-values are in parenthesis; Significance level at which the null hypothesis is rejected: *** 1%, ** 5%, * 10%.

Table A3. Robustness: Employing a logarithmic transformation

	Sales (log) (1)	Total Workers (log) (2)
Panel A: Full Sample		
Treated * Post-Treatment	0,071*	-0,003
	(0,04)	(0,02)
Adj R2	0,37	0,89
N	451 318	451 442
Panel B: By Sector – Tradable versus Non-Tradable		
Non-Tradable		
Treated * Post-Treatment	0,091**	-0,003
	(0,04)	(0,02)
Adj R2	0,37	0,88
N	297 737	297 811
Tradable		
Treated * Post-Treatment	0,021	-0,001
	(0,06)	(0,02)
Adj R2	0,39	0,91
N	151 226	151 274
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes

Notes: Dependent variables 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 A4. Robustness: without crisis period (2003-2008)

	Sales (ihs)	Total Workers (ihs)	Average Wages	Labour Productivity
	(1)	(2)	(3)	(4)
Panel A: Baseline				
Treated * Post-Treatment	0,102*	0,002	10,523***	1 653,125**
	(0,06)	(0,02)	(2,28)	(762,81)
Adj R2	0,41	0,89	0,73	0,73
N	335 063	335 063	335 063	335 063
Panel B: By Sector – Tradable versus Non-Tradable				
Non-Tradable				
Treated * Post-Treatment	0,107*	0,003	10,657***	1 913,341*
	(0,06)	(0,02)	(2,78)	(949,51)
Adj R2	0,41	0,88	0,73	0,76
N	220 039	220 039	220 039	220 039
Tradable				
Treated * Post-Treatment	0,073	-0,000	11,487***	443,604
	(0,08)	(0,02)	(3,34)	(752,74)
Adj R2	0,42	0,91	0,74	0,67
N	112 686	112 686	112 686	112 686
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes

Notes: Dependent variables in columns (1) and (2) were transformed using the inverse hyperbolic sine approach; 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 A5. Robustness: including North Region in the control group

	Sales (ihs)	Total Workers (ihs)	Average Wages	Labour Productivity
	(1)	(2)	(3)	(4)
Panel A: Baseline				
Treated * Post-Treatment	0,036	-0,017	11,947***	1 337,932***
	(0,03)	(0,02)	(1,92)	(350,33)
Adj R2	0,36	0,88	0,75	0,71
N	1 094 724	1 094 982	1 094 982	1 094 716
Panel B: By Sector – Tradable versus Non-Tradable				
Non-Tradable				
Treated * Post-Treatment	0,070*	-0,022	11,705***	996,115*
	(0,04)	(0,02)	(2,17)	(482,56)
Adj R2	0,36	0,86	0,75	0,73
N	703 766	703 933	703 933	703 759
Tradable				
Treated * Post-Treatment	-0,048	-0,010	12,648***	1 280,735*
	(0,04)	(0,02)	(3,08)	(697,29)
Adj R2	0,38	0,90	0,75	0,65
N	384 954	385 043	385 043	384 953
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes

Notes: Dependent variables in columns (1) and (2) were transformed using the inverse hyperbolic sine approach; 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 A6. Robustness: Coarsened Exact Matching

	Sales (ihs)	Total Workers (ihs)	Average Wages	Labour Productivity
	(1)	(2)	(3)	(4)
Panel A: Baseline				
Treated * Post-Treatment	0,058	-0,002	11,071***	1 154,560
	(0,04)	(0,02)	(2,25)	(826,46)
Adj R2	0,38	0,89	0,74	0,73
N	298 555	298 634	298 634	298 554
Panel B: By Sector – Non-Tradable versus Tradable				
Non-Tradable				
Treated * Post-Treatment	0,088**	-0,003	11,101***	1 959,416*
	(0,04)	(0,02)	(2,73)	(1027,61)
Adj R2	0,38	0,87	0,74	0,75
N	198 849	198 895	198 895	198 848
Tradable				
Treated * Post-Treatment	-0,006	0,002	11,047***	-463,228
	(0,06)	(0,02)	(2,72)	(790,16)
Adj R2	0,38	0,90	0,73	0,66
N	99 706	99 739	99 739	99 706
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes

Notes: Dependent variables in columns (1) and (2) were transformed using the inverse hyperbolic sine approach; 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 A7. Robustness: Excluding the 5 closest municipalities to Lisbon

	Sales (ihs)	Total Workers (ihs)	Average Wages	Labour Productivity
	(1)	(2)	(3)	(4)
Panel A: Baseline				
Treated * Post-Treatment	0,091	-0,009	10,941***	1 455,967*
	(0,06)	(0,02)	(2,30)	(707,41)
Adj R2	0,36	0,88	0,73	0,71
N	417 949	418 069	418 069	417 948
Panel B: By Sector – Non-Tradable versus Tradable				
Non-Tradable				
Treated * Post-Treatment	0,116*	-0,009	11,181***	1 682,024*
	(0,06)	(0,02)	(3,45)	(815,07)
Adj R2	0,36	0,87	0,73	0,74
N	275 910	275 982	275 982	275 909
Tradable				
Treated * Post-Treatment	0,027	-0,008	9,979***	282,790
	(0,07)	(0,02)	(2,90)	(937,14)
Adj R2	0,37	0,90	0,73	0,64
N	139 851	139 897	139 897	139 851
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes

Notes: Dependent variables in columns (1) and (2) were transformed using the inverse hyperbolic sine approach; Our regressor of interest, Treated * Post-Treatment, indicates firms producing in one of the 33 Treated municipalities, during the treatment period (2007-2010), with the exception of firms in one of the 5 closest municipalities to Lisbon (Arruda dos Vinhos, Sobral de Monte Agraço, Benavente, Alenquer, Torres Vedras. 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 A8. Robustness: Winsorize 95%

	Sales (ihs)	Total Workers (ihs)	Average Wages	Labour Productivity
	(1)	(2)	(3)	(4)
Panel A: Baseline				
Treated * Post-Treatment	0,073*	-0,003	9,988***	1 054,536*
	(0,04)	(0,02)	(1,93)	(516,42)
Adj R2	0,35	0,86	0,75	0,74
N	451 318	451 442	451 442	451 317
Panel B: By Sector – Non-Tradable versus Tradable				
Non-Tradable				
Treated * Post-Treatment	0,092**	-0,004	10,446***	1 444,680**
	(0,04)	(0,02)	(2,70)	(556,93)
Adj R2	0,35	0,86	0,75	0,77
N	297 737	297 811	297 811	297 736
Tradable				
Treated * Post-Treatment	0,024	-0,001	8,965***	96,152
	(0,06)	(0,02)	(1,85)	(736,24)
Adj R2	0,36	0,88	0,75	0,68
N	151 226	151 274	151 274	151 226
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes

Notes: Dependent variables in columns (1) and (2) were transformed using the inverse hyperbolic sine approach; 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 A9. 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

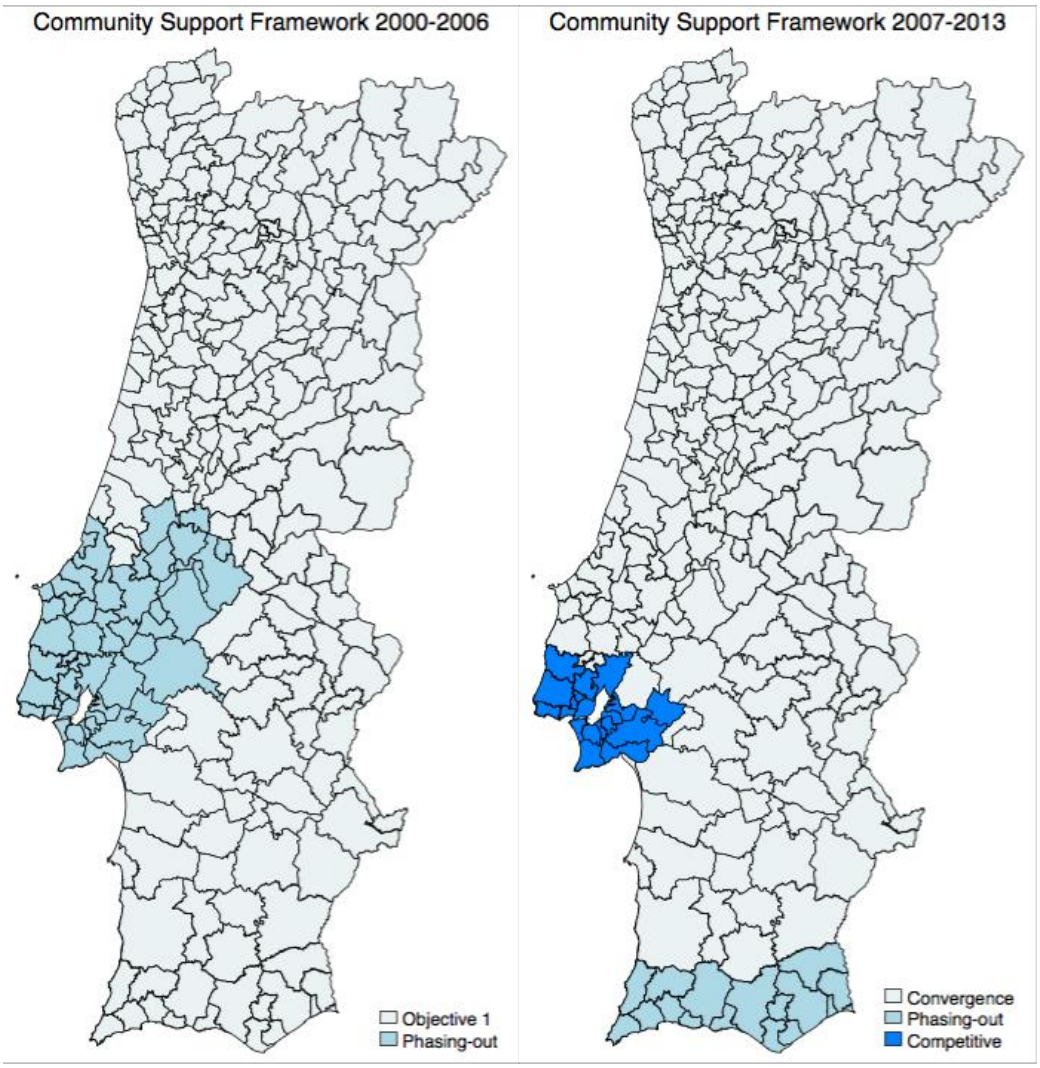
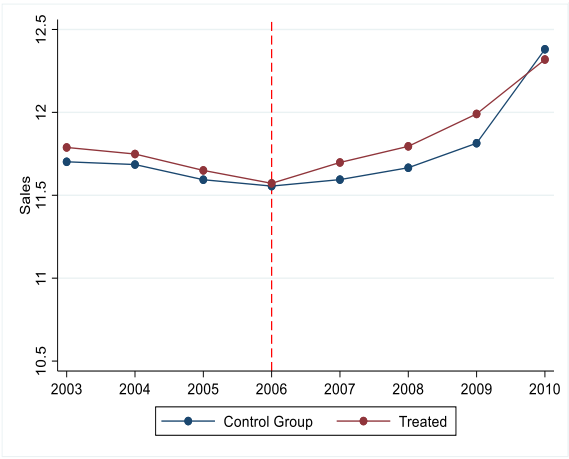


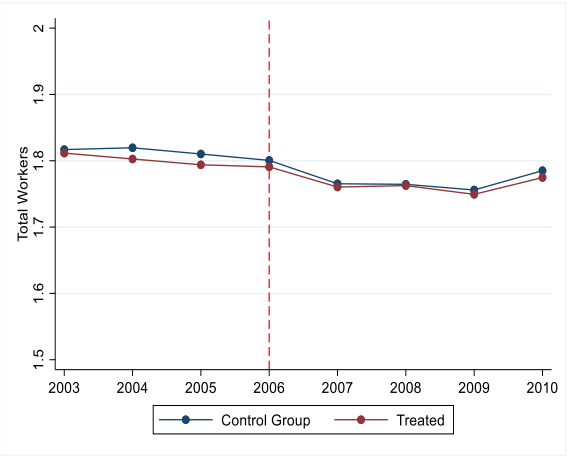
Figure A2.

Descriptive graphical evidence

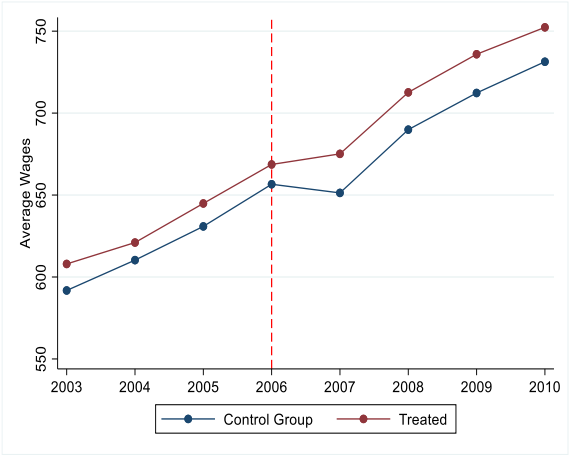
Sales (€ / year)



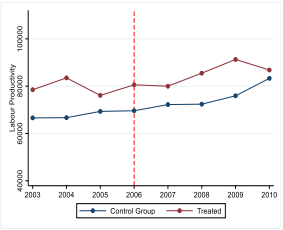
Total Workers



Average Wages (€ / month)



Labour Productivity (Sales / Workers)



Notes: This Figure presents the evolution of our four 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.