# Public support to clusters
# A firm level study of French "Local productive systems"*

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**Abstract**

This paper analyzes empirically a public policy promoting industrial clusters in France. Cluster policies have become popular in many countries but have not been extensively evaluated. We propose in this paper the first quantitative evaluation of a cluster policy exploiting firm-level data. We use data on production and employment for firms that benefited from the policy and on firms that did not, both before and after the policy started. We first show that the policy selected firms in sectors and regions in relative decline. Second, the policy did not succeed in reversing the relative decline in productivity for the targeted firms. The policy had no robust effect on employment or exports.

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# 1 Introduction

Industrial clusters are popular among policy makers. Since the end of the 1980s, national and local governments in Germany, Brazil, Japan, South Korea, the Spanish Basque country, and France, inter alia, have attempted to foster their development. The work by Michael Porter (1998, 2000), the leading figure of cluster strategies, has been very influential in this matter and is invariably used as a justification for cluster policies. Very large amounts of money are often spent on clusters initiatives (1.5 billion euros for the French "Competitiveness Clusters" from 2006 to 2008 and from 2009 to 2011, 45 billion euros for the "Northwest Regional Economic Strategy" from 2006 to 2026 in UK for example). There is however surprisingly little macro or micro empirical analysis of their effect on firms performance. The present paper attempts to fill this gap. To our knowledge, it is the first one to analyze quantitatively the effect, on individual firms, of a specific cluster policy.

A typical defense of cluster policies is that clusters bring economic gains and should therefore receive public support. Porter's definition of a cluster – "a geographically proximate group of interconnected companies and associated institutions in a particular field, linked by commonalities and complementarities" – is not very far from what economists call an agglomeration. The idea that clusters bring economic gains because firms perform better when located near other firms in the same sector is hardly new. In the late nineteenth century, Alfred Marshall identified several benefits of clusters or industrial districts. The different sources of agglomeration externalities, were first analyzed by Marshall and later rediscovered by Kenneth Arrow and Paul Romer. Those are 1) input externalities that save on transportation costs and make inputs purchases more efficient; 2) Labour market externalities that foster the creation of pools of specialized workers, who acquire cluster-specific skills valuable to the firms; 3) Knowledge externalities through which industrial clusters facilitate the exchange of information and knowledge.

Advocates of cluster policies need to address three questions:

1. How large are the gains from agglomeration? In particular, how much does the productivity of a firm increase when other firms from the same sector decide to locate nearby?

2. Do firms internalize these gains when making their location decisions? In particular, are "natural" clusters too small?

3. Can public policies that attempt to foster clusters affect positively the performance of the firms that belong to those clusters?

There is a large empirical literature that has attempted to answer the first question. The survey of Rosenthal and Strange (2004) reports that in the many empirical studies on agglomeration, the doubling of the size of a cluster (generally measured as employment of a given sector in a given region or as local density of employment) leads to a productivity gain between 3% and 8%. In another paper on French firm-level data, (Martin, Mayer, and Mayneris (2008)), we estimate this elasticity to be around 4% while Combes, Duranton, and Gobillon (2008) find an elasticity of French individual wages to local density of around 3%. The starting point of those who defend cluster strategies is thus right: economic gains from clusters exist. Their enthusiasm should however be tamed; these effects are modest. In Martin, Mayer, and Mayneris (2008), we also find evidence that French firms internalize part of these productivity gains when they choose where to locate: the size of existing "natural" clusters is not very different from the size that would maximize, in the short-run, productivity gains[1]. Hence, the case for public intervention in favor of clusters can be made but there is no evidence that the expected gains should be large.

Finally, even if one assumes that there is a case for public intervention (gains from clusters exist and there are not entirely internalized by firms), there is little evidence on the answer to the third question. Can cluster policies actually help? The present paper is to our knowledge the first one to focus on this question using firm-level data.

Cluster policies could do so in two ways. First, cluster policies could increase the size of existing clusters and thus improve the performance of firms if the cluster size is suboptimally small. Second, for a given size of clusters, cluster policies could improve the workings of externalities (input market externalities, labour market externalities and technological externalities). Both mechanisms could increase productivity of firms in the cluster.

In this paper, we exploit a rich French firm-level dataset to analyze the impact of a specific cluster policy that was implemented in 1999, by the Délégation Interministérielle à l'Aménagement et à la Compétitivité des Territoires (Diact, ex Datar), the French administration in charge of spatial planning and regional policy. The policy provided support to groups of firms, located in the same area and belonging to the same industry, called the "Local Productive Systems" (LPS). The main aim of the policy was to encourage cooperation among firms and to increase the competitiveness of firms in the cluster. From this point of view, the objective of the LPS policy was to improve the performance of firms in the cluster without necessarily aiming at increasing its size.

We assess the impact of public support to LPS on several dimensions of firm-level performance (TFP, employment, exports). We use several evaluation techniques (difference in difference, triple differences, matching) on a firm-level detailed dataset that spans over the 1996-2004 period, during which a subsample of firms were selected to benefit from the policy. We also investigate the existence of potential externalities of the policy by running the analysis at the area and industry level, and not only at the firm level.

We first analyze the characteristics of "treated" firms. This is interesting because it raises important political economy issues. Our results show clearly that the French LPS policy targeted firms located in backward regions and operating in declining industries. Hence, the policy turned out to be of a defensive type. The official objective was to promote agglomeration externalities and clusters dynamics and was supposed to mark a radical shift of the French regional policy, from traditional spatial equity to efficiency considerations. Our results suggest that the traditional equity objective was in reality still at play.[2] We also find that LPS firms receive on average more public subsidies than the others. This is consistent with the study by Beason and Weinstein (1996) on Japan. They show that the reality of Japanese industrial policies implemented between 1955 and 1990 clashed with the official objective to help the growth of winners. Indeed, they find a negative correlation between the growth of a given industry and the intensity of the aid it received. Our results on the French cluster policy we study as well as those of Beason and Weinstein (1996) are consistent with two interpre-

[1]The estimated positive elasticities in the literature, taken literally, would suggest that larger clusters are always better. In fact, exploiting annual variations of variables, we find that productivity gains first increase and then decrease (due to congestion costs) with the size of clusters, allowing us to estimate an optimal size of the cluster.

[2]An additional indication of that spatial equity objective is that the LPS projects are relatively evenly spread out on the national territory.

tations. One is that subsidies to declining industries reveal government political preferences (Corden (1974), Krueger (1990)). Another possible mechanism is provided by Baldwin and Robert-Nicoud (2007). These authors show that governments often "pick losers", or more exactly that public subsidies are captured by declining firms because these latter have a greater incentive to lobby for subsidies.

We also find that the French cluster policy was unable to reverse the relative decline of TFP at work for firms selected by the policy. We find no effect on employment or on exports. At the area-industry level, an effect is detected on exports, but its statistical significance and magnitude strongly depend on the specification. No significant effect is detected on survival.

Criscuolo, Matin, Overman, and Van Reenen (2007) find that the Regional Selective Assistance in UK, designed to subsidize firms in backward areas, has had a positive impact on firms' employment and investment but no effect on firms' productivity. By supporting less efficient firms, the authors judge that such a policy may slow down reallocations from less efficient plants and affect negatively aggregate productivity growth.

A more positive conclusion is reached by Branstetter and Sakakibara (2002) who analyze Japanese R&D public policy and its effect on the patenting activity of firms involved in government-sponsored research consortia. They find a positive impact, though quite small when all controls are included. Their method, which consists in examining the relative patenting path of consortia firms the years after the inception of the consortium is close to ours.

A related literature has analyzed the effect of subsidies given to firms to locate in specific regions. Crozet, Mayer, and Mucchielli (2004) study for example the determinants of location choice by foreign investors in France over the period 1985-1995. They measure the impact of a French subsidy (the "Prime d'Aménagement du Territoire", PAT) and of European grants for regional policy on firms' location choice. They find a generally positive, but very weak and hardly significant effect of those policies. Devereux, Griffith, and Simpson (2007) study the effect of Regional Selective Assistance (RSA) grants on firms location in United Kingdom[3]. They also find a positive but very weak effect of the policy. Head, Ries, and Swenson (1999) analyze the effect of state level policies in United States to attract Japanese firms and find that the probability to attract these firms increases with the subsidies. However, given that all states have such policies, the location of firms is not affected in equilibrium. Finally, Bondonio and Greenbaum (2007) find that "enterprize zones", which are programs in the US aimed at encouraging business in specific areas, have no impact either on employment or on activity. This is due to a higher growth rate of new firms compensated by a higher rate of failures.

The paper is structured as follows. We first describe in section 2 the Local Productive Systems policy and our data. We then lay out in section 2.5 our empirical strategy. We present our results in section 3 and some robustness checks in section 4. Section 5 concludes.

# 2 What are the "Local Productive Systems"?

## 2.1 The policy

The French agency in charge of regional policy (DIACT) issued in 1998 a tender intended to fund collaborative projects between firms of a given industry located in the same area. The purpose was clearly to promote agglomeration externalities and clusters dynamics. This

[3]Which is very similar to the French PAT.

policy corresponds to a quite radical shift in the objectives of French regional policy, from traditional spatial equity to taking more into account efficiency considerations in the geographic distribution of economic activities. One of the motivations was to replicate the alleged success of Italian industrial districts in the 1980's: the idea was to enhance, through public intervention, collaborations which developed "naturally" in Italy.

Around one hundred projects were submitted and around fifty of them received a subsidy in 1999. An additional fifty were funded in 2000, when the agency in charge issued a new tender. The tender was then transformed into a permanent one, and each year new or old propositions (only a handful of them now) were getting approved and funded by an ad hoc national commission. The policy was more or less abandoned in the second half of the 2000's.

The stated aim of the policy was to give a small monetary incentive (the average subsidy is around 37,500 euros) to set off or reinforce clusters. Conditions to receive this subsidy were not very restrictive at the beginning of the process. Conditions were then more demanding (established collaborations, credibility of the proposed action, knowledge of direct competitors etc.). Officially, the policy funds a project held by a collective organization. This is important since the subsidy is consequently not directly given to firms but to the collective structure. Very often, the official candidate organizing the project is a local public authority, and private firms join once the structure has secured the necessary funding. A wide range of actions can be funded: A study of feasibility for the development of a common brand, the creation of a grouping of employers or the implementation of collective actions in the field of exports for instance. The geographical scale of a LPS is generally the département or the employment area[4].

The LPS can be seen as the first cluster policy in France. A new policy, called "Competitiveness clusters" that started in 2005 is a much more ambitious and costly cluster policy than the one analyzed here (note however that a quarter of LPS projects have been transformed into competitiveness clusters). Even though the LPS policy is modest in terms of financial support, we believe that studying it is still relevant for the analysis of cluster policies. Indeed, all cluster policies are not absorbing large amounts of public spending. In Austria, the "CIR-CE (Co-operation in Innovation and Research with Central and Eastern Europe)", financed by national budgets and European structural funds between 2005 and 2008, the funding could not exceed 150,000 euros per network. The "Micro-clusters reinforcement programme", implemented in Catalonia, provides each micro-cluster initiative with a starting subsidy of 20,000 euros that can then increase to 100,000–120,000 euros. In the same vein, the Spanish Basque country, often presented as a pioneer in terms of cluster policy, promotes an approach based on a light funding by public authorities. However, the importance of these "small" cluster policies should not be underestimated: They often exert a leverage effect as firms in publicly sustained clusters can get more money from other financing schemes. As many other public policies, the LPS policy is specific in several dimensions so that our results cannot be generalized to other cluster policies. Nevertheless, we believe that the study of the LPS policy can highlight some drawbacks linked to the implementation of cluster policies that are common to many countries.

[4]The départements are administrative areas. Employment areas are economic entities defined on the basis of workers' commuting. There are 94 départements and 341 employment areas in continental France.

## 2.2 The data and methodology

We use French annual business surveys[5] data, provided by the French ministry of Industry. We have information at both the firm and plant levels. This is restricted to firms with more than 20 employees and all the plants of those firms. Our data cover the period 1996-2004. At the firm level, we have all the balance-sheet data (in particular, production, value-added, employment, capital, exports, aggregate wages) and information about firm location, firm industry classification and firm structure (e.g. number of plants).

At the plant level, data are less exhaustive; they contain plant location, plant industry classification, plant number of employees and information about the firm the plant belongs to.

We obtained from the public authority in charge of the LPS policy, the DIACT, the list of LPS and the information about the subsidies obtained as well as the structure which administers. We contacted individually during the year 2006 around 90 LPS, to ask them the list of their adherents. Workable files were obtained for 57 of them, which represents 3,233 firms. We however lost information when we merged these firms with the annual business surveys to obtain data on production and employment. Many of the LPS reported the name and the address of firms, but not their national identification number. We consequently had to find out most firms in the annual business surveys thanks to their name and their zip code only. We merged successfully only 641 firms (the others are probably firms with less than 20 employees or with badly collected information), from 45 LPS created between 1999 and 2003.

From a geographic point of view, we dropped all firms located in Corsica and in overseas départements. Consequently, our sample covers the 94 continental French départements and 341 employment areas. From a sectoral point of view, we only retained firms belonging to manufacturing sectors.[6] In particular, food-proceeding firms had to be dropped, since the information related to those firms come from a different survey, not entirely compatible with the rest of manufacturing.

The observations for which value-added, employment or capital is missing, negative or null are dropped.[7] We deflated value-added data by a branch price-index and capital data by a an investment price index valid for all industrial sectors. In the end, the sample is an unbalanced panel involving 483 firms which belong to a LPS. Eighty-eight 3-digit industrial sectors and thirty-nine LPS are represented.

Several remarks are in order with respect to the important difference between the number of LPS firms we identified and the number of treated firms we have in our sample. First, from a total of around 100 LPS labeled by public authorities during the period under study, we successfully contacted 90 LPS. 57 LPS out of the 90 that responded provided us with exploitable data. It is likely that the LPS that replied and gave us exploitable information are the most involved in the monitoring of their network. Consequently, if we have a selection bias in our sample, we have good reasons to think that it is an upward bias with respect to the impact of the policy. Observations are lost because many firms in the LPS are smaller than 20 employees. This can be a problem if the impact of the policy is heterogeneous according to the size of firms. We test on our sample the existence of such an heterogeneity and results

[5] Called in French "Enquêtes Annuelles d'Entreprises".

[6] In the French 2-digit classification, manufacturing sectors correspond to sector 17 (textile) to sector 36 (miscellaneous), sector 23 (refining) excluded.

[7] We also dropped outliers, dropping 1% extremes for the following variables: capital intensity, yearly capital intensity growth rate, yearly capital growth rate, yearly employment growth rate.

show that this is not the case; however, if the threshold for relevant heterogeneity is below 20 employees, we cannot capture it and our results are therefore best interpreted as valid for firms larger than 20 employees. Moreover, if the determinants of the selection of small firms in the LPS do not differ from these determinants for large firms, the first step of our analysis, on the determinants of the LPS policy, is not impacted by the reduction of our sample. Finally, it is possible that due to poorly collected or misreported information, we do not identify, for a given LPS, all the firms that are in our sample. In this case, the analysis conducted at the industry-area level should correct for this.

For employment areas data, we use the "Atlas des zones d'emploi" published by the INSEE, the French institute for national statistics, in 1998.

## 2.3 Which industries are targeted by LPS?

Some simple descriptive statistics on the industries (defined at the 3 digit level) targeted by the LPS policy are useful. We had to drop the "Weapons and ammunitions" industry, which is a clear outlier in terms of evolution during the period. We distinguish the manufacturing industries which are not represented in the LPS (25 non-treated industries), the industries represented by less than 10 LPS firms (46 industries) and the industries represented in the LPS by at least 10 firms (16 industries). The average of several indicators for these three categories are presented in table 1.

Table 1: Industry level summary statistics

| Variables | Non LPS-treated industries | Industries with less than 10 LPS-treated firms | Industries with at least 10 LPS-treated firms |
|---|---|---|---|
| | Average level in 1996 | | |
| Labour productivity | 43.64 | 43.54 | 37.64 |
| Capitalistic intensity | 65.72 | 57.27 | 40.31 |
| Export share | 0.34 | 0.34 | 0.24 |
| | Evolution between 1996 and 2004 (in %) | | |
| Employees | -10.68 | -8.82 | -3.84 |
| Value added | 19.30 | 23.26 | 26.27 |
| Labour productivity | 34.54 | 32.54 | 34.31 |
| Exports | 23.19 | 50.15 | 56.81 |

Note: Labour productivity=value added/employees, capitalistic intensity=capital stock/employees, export share=export value/sales. Values are in thousands of real euros.

In 1996, the average labour productivity is lower in industries where LPS are the most important than in the rest of manufacturing industries. LPS industries are also much more labour intensive than the others.

Between 1996 and 2004, the employment loss for the average French non-LPS manufacturing industries is 10.68%. LPS industries lost much less employment (8.82% and 3.84%). Their value added also increased more (23.26% and 26.27% vs 19.30%), but not proportionally to employment, so that labour productivity increased on average by 34.54% in non-LPS industries, and by only 32.54% and 34.31% in LPS industries. Finally, LPS firms belong to industries that export less than the average but their exports grew faster over the period.

To summarize, LPS industries are on average much more labour intensive than the rest of manufacturing; they destroyed less employment than other industries in the 1996-2004 period but their productivity gains were also lower.

## 2.4 Who are LPS firms?

We now analyze the characteristics of firms that participated to one of the selected LPS. Table 2 presents summary statistics about the LPS firms of our sample. They are larger and less productive than non LPS firms. However, the standard deviation for all their characteristics (except for the number of employees) is lower than for other firms. This suggests that the policy targeted firms with specific characteristics.

Table 2: Summary statistics about firms

| | LPS firms | | | Non LPS firms | | |
|---|---|---|---|---|---|---|
| Variable | Obs | Mean | Std. Dev. | Obs | Mean | Std. Dev. |
| Value added | 3286 | 11806.79 | 35426.5 | 171322 | 6679.1 | 45160.81 |
| Employees | 3286 | 262.19 | 750.18 | 171322 | 131.43 | 647.91 |
| Capital Stock | 3286 | 17362.01 | 69287.36 | 171322 | 8274.4 | 105470.3 |
| Labour productivity | 3286 | 39.92 | 19.03 | 171322 | 43.05 | 37.42 |

Note: Value added, capital and labour productivity are expressed in thousands of real euros

To go further in this analysis, we estimate, with a probit model, the probability for a firm $i$, from sector $s$ and located in département $z$ to become a LPS-firm. We take into account average firm-level characteristics prior their entrance in a LPS. We also control for characteristics of the employment areas where the firms are located. The way we compute firm-level average characteristics is not trivial. Our panel is unbalanced. Moreover, firms entered the LPS scheme in different years between 1999 and 2003. Hence, the number of years for which we can observe the firm characteristics prior their entrance in a LPS is not the same for all firms. If firms' characteristics are affected by annual common shocks, the computation of pre-LPS average characteristics could therefore be noisy; hence, we correct all individual observations for yearly trends. We then compute for each firm its average characteristics for the years before its "entry" in a LPS. For non-LPS firms and firms in LPS sustained in 2003, these average characteristics are computed with all the available de-trended observations from 1996 to 2003. All the firms that disappeared before 1999, and which could consequently not be in a LPS, are dropped. We keep in the end 345 LPS firms in the sample.

The results are displayed in table 3. The index of TFP we use is obtained with an estimate of a production function at the 2 digit industry level, following an OLS approach. In the appendix, we show that our results are qualitatively robust when we use a GMM TFP index (see table 14). Column (1) presents results from a simple probit, where we control for the size (number of employees) of the firm, its TFP and TFP growth rate, the amount of subsidies (other than LPS) it receives and the number of other firms of its own industry in the département. In this very simple specification, LPS firms appears bigger than the others and seem to receive more public subsidies overall. These two characteristics of LPS are very robust. One interpretation is that LPS firms are important for local politicians because they are big employers and that they are good at lobbying for public subsidies. LPS firms also tend to be less productive than the others.

The inclusion of industry-fixed effects in regression (2) and of départements fixed effects in regression (3) does not change these results except that the coefficient on TFP is now positive, but insignificant. Given that the coefficient on TFP is negative and significant when

Table 3: LPS determinants

| Dependent Variable: | lps_status | | | |
|---|---|---|---|---|
| Model : | (1) | (2) | (3) | (4) |
| Mean (ln $\text{Sales}_{it}$) | $0.041^c$ | $0.055^b$ | $0.102^a$ | $0.105^a$ |
| | (0.023) | (0.025) | (0.027) | (0.025) |
| Mean ($\text{TFP}_{it}$) | $-0.147^c$ | -0.044 | 0.018 | 0.013 |
| | (0.081) | (0.070) | (0.088) | (0.072) |
| Mean (TFP $\text{growth}_{it}$) | -0.038 | -0.045 | -0.036 | -0.036 |
| | (0.096) | (0.116) | (0.121) | (0.125) |
| Mean (ln $\text{Subsidies}_{it}$) | $0.038^a$ | $0.040^a$ | $0.034^a$ | $0.034^a$ |
| | (0.007) | (0.008) | (0.007) | (0.008) |
| Mean (ln (# of firms, same ind.-dép.$_{it}$)) | $-0.096^b$ | $-0.143^a$ | $0.173^a$ | $0.155^b$ |
| | (0.037) | (0.052) | (0.057) | (0.062) |
| ln Mean (Taxable net $\text{income}_{z1994}$) | | | | $2.109^a$ |
| | | | | (0.496) |
| ln Mean (Taxable net income growth $\text{rate}_{z1984-1994}$) | | | | $-0.992^a$ |
| | | | | (0.321) |
| ln Population $\text{density}_{z1994}$ | | | | -0.102 |
| | | | | (0.065) |
| ln Industrial jobs $\text{share}_{z1994}$ | | | | $0.724^a$ |
| | | | | (0.156) |
| ln Share of population with vocational $\text{training}_{z1990}$ | | | | $-1.237^a$ |
| | | | | (0.476) |
| Industry fixed effects | no | yes | yes | yes |
| Département fixed effects | no | no | yes | yes |
| N | 16527 | 16527 | 16527 | 16527 |
| $R^2$ | 0.027 | 0.07 | 0.193 | 0.206 |

Note: Robust standard errors in parentheses $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. All regressions are clustered at the employment area level. Necessarily, t $\leq$ lps_year we do not control for industry and département fixed effects, this confirms that LPS firms operate in less productive industries (see subsection 2.3) and are located in less productive départements.[8]

Note also that the number of firms from the same industry in the département, which is a proxy for potential localization economies, is negative and significant in regressions (1) and (2). This is surprising; the LPS policy is supposed to be a cluster-promotion policy and we expected a positive coefficient on this variable. But the coefficient is strongly positive and significant when départements fixed effects are added. Hence, an explanation would be that the LPS policy targeted clusters which are relevant at a local level, but not at a national level.

In regression (4), we include some characteristics of the employment areas where firms are located. The results are robust to this inclusion. Moreover, these regressions show that, relative to the average in the département, LPS firms are located in areas which are more dependent on industry, richer, and with less workers with vocational training. Note however, their average taxable income growth was smaller over the 1984-1994 period. We will use this regression and what it tells us about the observable characteristics of LPS firms to construct our sample for the matching approach when we analyze the impact of LPS status on firm performance.

### 2.5 Empirical methodology

The stated objective of the LPS policy is to improve firms' competitiveness. To analyze whether it was successful in this respect we quantify the impact of the LPS policy on firms' total factor productivity (TFP). We also analyze its impact on firms' employment and on firms' exports. We use several techniques developed in the evaluation literature. The first one is the standard "difference-in-difference" method (DD) (see Bertrand, Duflo, and Mullainathan (2004)).

$y_{it}$ is our dependent variable (firms' TFP, employment or exports). The relation we bring to data is the following:

$$y_{it} = \gamma \text{lps}_i + \theta \text{lps\_in}_{it} + \text{d}_t + \epsilon_{it} \tag{1}$$

where $\text{lps}_i$ is a dummy variable that identifies firms which at some point benefit from the LPS label. This dummy captures all time-invariant unobservable characteristics specific to firms targeted by the LPS policy. $\text{lps\_in}_{it}$ is a dummy which equals 1 for LPS firms the years after the public decision to subsidize their LPS. $\text{d}_t$ is a time trend, common to all firms. If $\epsilon_{it}$ is orthogonal to the regressors, $\theta$ is the DD estimator of the effect of LPS policy on firm's performance. It is indeed obtained by comparing the evolution of performance for LPS firms before and after their entry in the LPS, to the evolution of performance for non-LPS firms during the same period. However, section 2.4 showed that LPS firms had particular characteristics, especially in terms of location and industries, which both determined their probability of belonging to a LPS and their performance before. This suggests several sources of bias in our estimates of $\gamma$ and $\theta$. This is the reason why we progressively add several fixed effects to end up with an individual fixed effect estimation. Since some firms change area or industry over the period, this leads to a firm-industry-département fixed effect (in that case,

[8]This is also confirmed by the fact that LPS firm are located in départements which receive the "Prime d'Aménagement du territoire" (PAT), one of the main instruments of regional policy in France and which have a high share of subsidized employment: see table 15 in the Appendix.

the variable $\text{lps}_i$ is captured by the fixed effect and it is thus dropped from the estimation). This is the "true" DD estimator of $\theta$, since it compares the evolution of $y$ before and after the treatment for a given LPS firm to the evolution of $y$ for a given non-LPS firm. This estimation corrects for individual characteristics of firms (given their industry and location) invariant across time.

However, if the fact of being in a LPS is also correlated to specific shocks or to temporal trends (if $\epsilon_{it} = \text{u}_i + \eta_{it}$ and if $E(\eta_{it+1} - \eta_{it})$ is different for LPS and non-LPS firms), our estimation will suffer from a simultaneity bias. The best way to control for both unobserved invariant characteristics and unobserved idiosyncratic shocks would be to instrument the LPS variables. There is however no obvious set of natural instruments that would be good predictors of entry into the LPS scheme, while being unrelated to the firm's performance. We address this issue by resorting to several alternative techniques. We first add an industry-year fixed effect to control for shocks at the industry level. However, this method will not entirely solve the problem if there are unobserved dynamics at the firm-level, and not at the industry-level, correlated to the policy. This is why we also use estimators which control for unobserved dynamics at the firm-level: we use the individual fixed effect estimator developed by Baltagi and Wu (1999) which models the unobserved disturbance as an AR(1). We also implement a triple-differences approach, using the first difference of $y$ as a dependent variable: it amounts to estimating the impact of the policy on the growth rate of variables; it will control for individual trends which are invariant across time. In the end, we combine these methods with a matching approach. This accounts for the fact that LPS firms are specific in the observable characteristics and identifies a group of non treated firms with the most similar set of observables; if the evolution of performance can be predicted by firm-level observables, matching controls for the potential remaining unobserved dynamics.

# 3 Results

## 3.1 LPS and Productivity

We first present our results on TFP. To estimate firm TFP, we regress firm value-added on employment and capital and keep the residuals. We estimate production functions at the 2 digit industry level; the estimated elasticities for employment and capital are respectively on average 0.80 and 0.15. In the appendix, we discuss the limitations of the OLS approach to TFP estimation and perform robustness checks where we estimate TFP thanks to the method developed by Levinsohn and Petrin (2003). Results are very similar.

### 3.1.1 A graphical exploration

We start with a graphical analysis of the evolution of productivity differential between LPS and non-LPS firms. We estimate the following four regressions:

$$\text{tfp}_{it} = \sum_{j=-2}^{5} \alpha_j \text{lps\_in}_{ijt} + \text{d}_t + \epsilon_{it} \quad (2)$$

$$\text{tfp}_{it} = \text{fe}_z + \sum_{j=-2}^{5} \alpha_j \text{lps\_in}_{ijt} + \text{d}_t + \epsilon_{it} \quad (3)$$

$$\text{tfp}_{it} = \text{fe}_z + \text{fe}_s + \sum_{j=-2}^{5} \alpha_j \text{lps\_in}_{ijt} + \text{d}_t + \epsilon_{it} \quad (4)$$

$$\text{tfp}_{it} = \text{fe}_i + \sum_{j=-2}^{5} \alpha_j \text{lps\_in}_{ijt} + \text{d}_t + \epsilon_{it} \quad (5)$$

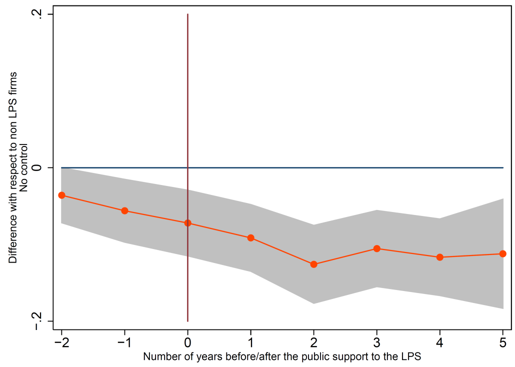
where $\text{lps\_in}_{ijt}$ equals 1 if $j$ years separate time $t$ from the moment when firm $i$ will become (resp. has become) a LPS firm. The first regression simply estimates the difference of productivity between LPS and non-LPS firms according to the number of years which separate the LPS firm from the reception of the subsidy. We then add fixed effects with increasing levels of detail: département $z$, then département $z$/sector $s$, and finally firm-département-sector level $i$[9]. Only the last regression actually yields a difference-in-difference estimator of the LPS effect. The four sets of results are presented in panels (a) to (d) of figure 1.

We first perform the estimation on the whole sample. The grey zone on each panel corresponds to the 5% confidence interval. According to the first estimation, in the absence of any control in panel (a), LPS firms are not very significantly different from the others two years before their entry in a LPS, but a negative and significant productivity gap grows over time between both types of firms. With départements and industry controls in panel (c), the negative gap is reversed and results exhibit a positive productivity differential between LPS and non LPS firms before entry. Nevertheless, LPS firms still seem to be on a declining path in terms of productivity, even though the differential with non LPS firms for a given year is never significant at the 5% level. The graphical analysis clearly suggests that LPS firms are on a different trend from the others. When firm-département-industry fixed effects are controlled for in panel (d), results should be interpreted in terms of productivity growth differential. The decline seems to be stopped after the entry in the LPS, but then reemerges afterwards.

Figure 2 presents the same results for single-plant firms. Indeed, the LPS policy is supposed to help firms better coordinate their strategies with firms nearby and more generally to enable firms to benefit more from the network of firms in the region. Multi-plant firms, which are also typically bigger, may be less dependent on their local environment and therefore respond less to the LPS policy. Moreover, and maybe more importantly, we do not have the information on the LPS status at the plant level. Hence, for multi-plant firms, the effect of the policy may be both weaker and mis-measured. Hence, we analyze the case of single plant firms (353 LPS firms) which do not suffer from those problems. Comments are roughly the same.

We now turn to proper difference-in-difference econometric analysis to investigate the robustness of those first results more systematically. In table 4, on the whole sample, the

[9]Remember that some firms change département or industry during the period; in order to control for geographic and sectoral unobservables, the right individual fixed effect is consequently a firm-département-industry fixed effect.

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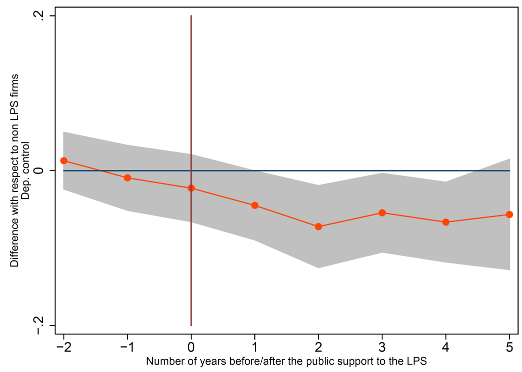


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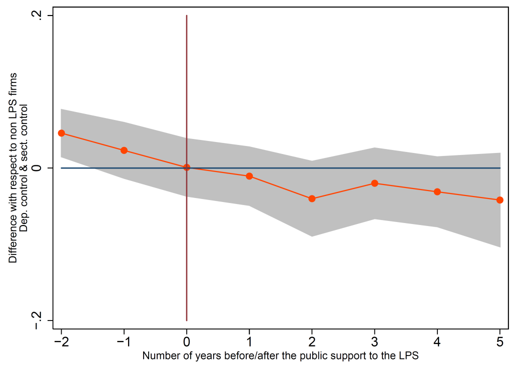


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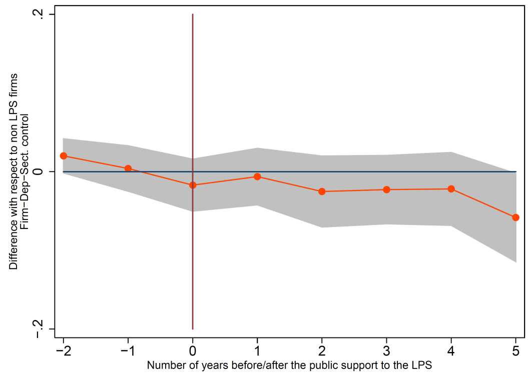


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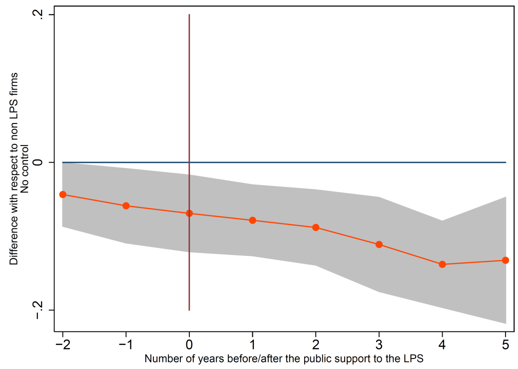


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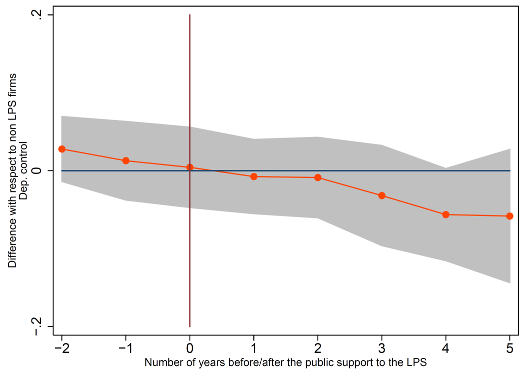


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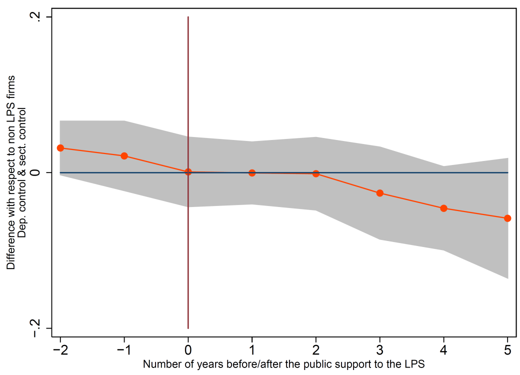


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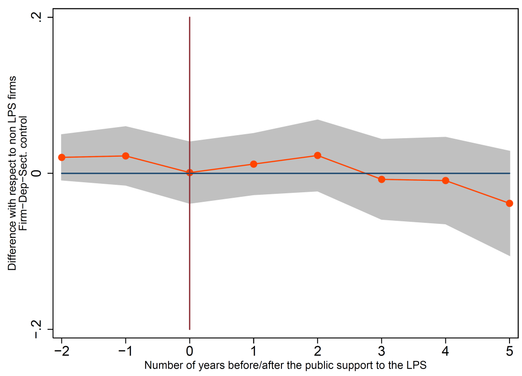



(d)

Figure 2: LPS single-plant firms and evolution of OLS TFP

simple OLS regression confirms that the LPS firms are "structurally" less productive than the others (with a negative and significant coefficient on the LPS dummy). Moreover, they experience a negative and very significant relative productivity drop once they are in a LPS. Interestingly, once industry and départements fixed effects are taken into account (regression (3)), the coefficient on "LPS firms" becomes positive and significant; again this means that LPS firms belong to less productive industries and départements. Nevertheless, "Being in a LPS" still has a negative coefficient which persists when we introduce Firm-Département-Industry fixed effects in regression (5), though closer to zero and less significant. There are several possible interpretations of this rather pessimistic result on the LPS policy. One is that the LPS policy causes this negative effect. It is possible that the firms that receive the LPS label become more receptive to public pressure to postpone workers layouts. In this interpretation, firms may choose to forego labor saving productivity improvements. Another interpretation - not exclusive of the first one - is that firms that enter a LPS do it when they face difficulties: $\eta_{it}$ and $\text{lps\_in}_{it}$ are certainly correlated and there would consequently be a simultaneity bias in the estimation of the causal impact of the policy on firms' TFP. The graphical analysis tends to corroborate this idea since we showed that the productivity of LPS firms exhibit a relative declining pattern with respect to non LPS firms, from two years before to five years after the entry in the LPS. We address this issue in subsection 3.1.2.

Results on single plant firms are presented in the bottom part of table 4. They confirm our main conclusions but some subtle differences emerge: LPS single-plant firms operate in less productive industries and are located in less productive départements, but once we control for this, the coefficient on "Being in a LPS" is not significant any more: their productivity growth following the implementation of the policy is not different from the one of non LPS firms.

In both samples, we control in regression (6) for the amount of subsidies a firm receives on a given year. We have already stated that the LPS subsidy was a one shot subsidy granted to the structure that manages the LPS and not to firms directly. However, since we have seen that LPS firms are firms which perceive more subsidies than the others, we may capture something else than the effect of the LPS policy with our estimation. The inclusion of the total amount of subsidies a firm receives each year does not affect our results.

### 3.1.2 LPS and temporal endogeneity

The graphical analysis showed that LPS firms were on a declining path - relative to other firms - before their entry in a LPS. We do not control for this in the difference-in-difference approach which can bias our results. We do not have any natural instrument to purge our estimations from trends which would be specific to LPS firms. We consequently resort to alternative strategies:

- We introduce in the DD estimation industry-year fixed effects in order to purge the estimation from shocks common to all firms from the same industry in a given year (technically, we run the DD regressions on the variables de-trended for industry-year fixed effects).

- The industry-year fixed effect approach corrects for industry level dynamics. However, it does not control for firm-level dynamics. To do so, we use the fixed-effect estimator developed by Baltagi and Wu (1999); it allows us to take into account an auto-regressive

Table 4: LPS and OLS TFP

| Dependent Variable: | ln TFP | | | | | |
|---|---|---|---|---|---|---|
| Model : | (1) | (2) | (3) | (4) | (5) | (6) |
| | All firms | | | | | |
| LPS firm | -0.044$^a$ | -0.001 | 0.000 | 0.030$^b$ | | |
| | (0.017) | (0.014) | (0.017) | (0.014) | | |
| Being in a LPS | -0.066$^a$ | -0.062$^a$ | -0.059$^a$ | -0.057$^a$ | -0.023$^c$ | -0.023$^c$ |
| | (0.017) | (0.016) | (0.017) | (0.016) | (0.014) | (0.014) |
| Total amount of other subsidies | | | | | | -0.000$^c$ |
| | | | | | | (0.000) |
| N | 174608 | 174608 | 174608 | 174608 | 174608 | 174608 |
| $R^2$ | 0.02 | 0.28 | 0.10 | 0.31 | 0.02 | 0.02 |
| | Single plant firms | | | | | |
| LPS firm | -0.053$^a$ | -0.024 | 0.014 | 0.019 | | |
| | (0.020) | (0.016) | (0.020) | (0.016) | | |
| Being in a LPS | -0.054$^a$ | -0.049$^a$ | -0.043$^b$ | -0.042$^b$ | -0.009 | -0.009 |
| | (0.020) | (0.018) | (0.020) | (0.018) | (0.014) | (0.014) |
| Total amount of other subsidies | | | | | | -0.000 |
| | | | | | | (0.000) |
| N | 117286 | 117286 | 117286 | 117286 | 117286 | 117286 |
| $R^2$ | 0.02 | 0.28 | 0.14 | 0.32 | 0.02 | 0.02 |
| Year fixed effects | yes | yes | yes | yes | yes | yes |
| Industry fixed effects | no | yes | no | yes | no | no |
| Département fixed effects | no | no | yes | yes | no | no |
| Firm-Département-Industry fixed effects | no | no | no | no | yes | yes |

Note: Standard errors in parentheses. $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are corrected to take into account individual autocorrelation.

process of the first order of the disturbance term at the individual level[10].

- We also run our regressions using annual TFP growth rate as a dependent variable. Put differently, we estimate the effect of the LPS policy using a triple differences approach, which allows us to control for firm-specific temporal trends that do not change over time.

- Finally, we can improve the estimation of the LPS policy impact by combining the preceding estimation procedures with a matching strategy. We saw in subsection 2.4 that there was a clear selection of LPS firms on observable characteristics. If those characteristics are also correlated with the evolution of firms' TFP, this can correct for the remaining endogeneity. It also corrects the estimation for potential heterogeneity in the sensitivity to the policy by removing from the sample firms which had no chance or very little chance to be treated. Using the last regression of table 3 in subsection 2.4, we compute the probability for all firms to belong to a LPS. Note that in this regression, all the firms in industries or départements which are not represented in the LPS have already been eliminated. We then reduce the sample to firms that share similar observable characteristics. To do so, we eliminate LPS firms that have very different characteristics from non-LPS firms and vice versa, based on the probability to enter in a LPS we have computed for each firm. The average probability for LPS firms to enter in a LPS is around 10%; the same probability is close to 2% for non LPS firms. We drop from the sample those firms that have a probability to be treated above the 99th

[10]We use the xtregar procedure implemented in Stata

percentile of non-LPS firms (this gives us an upper bound probability in our sample equal to 17.6%) and below the 5th percentile of LPS ones (this gives us a probability threshold equal to 0.7%). This helps us comply more confidently with the common support condition of the matching approach, according to which the probability to be treated must have the same support for treated and non treated firms in the sample. 260 LPS firms remain in the sample, out of which 169 are single plant firms. Tables 21 and 22 show that that matching contributes greatly in making treated and non-treated firms ex-ante more similar.

Table 5: LPS and OLS TFP-Simultaneity bias

| Dependent Variable: | ln TFP | | | Δln TFP |
|---|---|---|---|---|
| Model : | DD | FE | AR(1) | DD |
| | All firms | | | |
| Being in a LPS | -0.021 | -0.017 | 0.002 | 0.016[c] |
| | (0.014) | (0.015) | (0.014) | (0.010) |
| N | 137781 | 137781 | 109110 | 137781 |
| $R^2$ | 0.01 | 0.00 | 0.16 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.37 | n.a. |
| | Single plant firms firms | | | |
| Being in a LPS | -0.009 | -0.003 | 0.003 | 0.018 |
| | (0.015) | (0.015) | (0.017) | (0.012) |
| N | 92591 | 92591 | 72166 | 92591 |
| $R^2$ | 0.01 | 0.00 | 0.19 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.36 | n.a. |
| Year fixed effects | yes | n.a. | yes | yes |
| Industry-time fixed effect | no | yes | no | no |
| Firm-Industry-Département fixed effects | yes | yes | yes | yes |

Note: Standard errors in parentheses. [a], [b] and [c] respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are corrected to take into account individual autocorrelation.

Results are displayed in table 5. Resorting to triple differences mechanically reduces the sample since there are no observations of TFP growth for the year 1996. For comparability purposes, the first regression replicates the DD estimator on the sample available for triple differences. We can see that our estimations are not very different from those performed before: when all firms are considered, the DD estimator is negative and roughly equal to -0.02, but not significant anymore. It is very close to zero and not significant when single plant firms only are retained in the sample. When industry-year fixed effects are controlled for, the coefficient remains negative but increases slightly; it is not significant whatever the sample is. When we control for dynamics at the firm-level thanks to an AR(1), the coefficient becomes positive but insignificant and very close to zero. With triple differences, the coefficient is positive in both samples (respectively equal to 0.016 and 0.018); it is moreover significant at the 10% level on the sample containing all firms. The same regressions are performed on the matched sample and results are presented in table 6: the results are roughly the same, except that the triple differences estimate is significant at the 10% level in both samples (respectively equal to 0.022 and 0.028). This positive and slightly significant result corresponds to the stabilization of the declining path in the years after the entry in the LPS that we observe on Figures 1 and 2. However, these graphs show that after five years for the whole sample and two years for single plant firms, the decline accelerates again.

To conclude, the graphical analysis showed that LPS firms were on a particular trend before benefiting from the policy; the DD estimator does not control for this and this tends to bias downwards the estimation of the impact of the policy. When we control for the specific dynamics of LPS firms productivity, the results depend on the estimator: no significant effect is detected through an AR(1) estimation strategy and a weakly significant positive impact is measured through a triple-differences approach. Hence, if the LPS policy had any positive impact on firm-level productivity, the graphical and the econometric analysis suggest that it is at most a weak, short-run effect.

Table 6: LPS and OLS TFP-Matching

| Dependent Variable: | ln TFP | | | Δln TFP |
|---|---|---|---|---|
| Model : | DD | FE | AR(1) | DD |
| | All firms | | | |
| Being in a LPS | -0.003 | -0.019 | 0.020 | 0.022$^c$ |
| | (0.017) | (0.017) | (0.016) | (0.012) |
| N | 46465 | 46465 | 37846 | 46465 |
| $R^2$ | 0.01 | 0.00 | 0.15 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.39 | n.a. |
| | Single plant firms firms | | | |
| Being in a LPS | 0.020 | 0.007 | 0.029 | 0.028$^c$ |
| | (0.016) | (0.016) | (0.019) | (0.015) |
| N | 29955 | 29955 | 24055 | 29955 |
| $R^2$ | 0.01 | 0.00 | 0.19 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.38 | n.a. |
| Year fixed effects | yes | n.a. | yes | yes |
| Industry-time fixed effect | no | yes | no | no |
| Firm-Industry-Département fixed effects | yes | yes | yes | yes |

Note: Standard errors in parentheses. $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are corrected to take into account individual autocorrelation.

### 3.1.3 Further issues

A possible defense of the LPS policy is that the absence of a measurable effect comes from the small size of the monetary subsidy involved. In other words, the policy is good but should receive more funds. To test this idea, we use for all single-plant firms[11] involved in a LPS the information on the amount of the subsidy perceived by the LPS they belong to. Note that the subsidy is a small (one shot) subsidy (the average subsidy in our sample is around 40 000 euros) and is not granted directly to the firms but to the structure in charge of the LPS. In table 7, the significantly negative coefficient on the amount of the subsidy in the first regression suggests that the strongest monetary support goes to the LPS where firms are relatively more in decline. This negative coefficient cannot be interpreted in causal terms since the subsidy variable has no impact once individual fixed-effect are introduced.[12] It however confirms that equity considerations are at work in the implementation of the policy.

We also investigated the existence of heterogeneity in the impact of the policy according to the size of firms and the size or the governance structure of the LPS: no significant

[11]Since we do not know which plant obtained the subsidy, we concentrate on single-plant firms. These are also the firms for which a positive effect, if it exists, should be best measured. We have the necessary information for 294 firms.

[12]Results are the same when using an AR(1) or a triple-differences estimator.

Table 7: LPS, OLS TFP and Subsidy

| Dependent Variable: | ln TFP | | | | | |
|---|---|---|---|---|---|---|
| Model : | (1) | (2) | (3) | (4) | (5) | (6) |
| LPS firm | -0.047$^b$ | -0.028$^c$ | 0.020 | 0.019 | | |
| | (0.022) | (0.017) | (0.022) | (0.017) | | |
| Being in a LPS | 1.088$^a$ | 0.629$^a$ | 1.000$^a$ | 0.579$^b$ | -0.188 | -0.189 |
| | (0.280) | (0.243) | (0.272) | (0.242) | (0.191) | (0.190) |
| Being in a LPS×ln(Subsidy+1) | -0.312$^a$ | -0.184$^a$ | -0.283$^a$ | -0.168$^b$ | 0.048 | 0.048 |
| | (0.076) | (0.066) | (0.074) | (0.066) | (0.051) | (0.051) |
| Total amount of other subsidies | | | | | | -0.000 |
| | | | | | | (0.000) |
| N | 116928 | 116928 | 116928 | 116928 | 116928 | 116928 |
| $R^2$ | 0.02 | 0.28 | 0.14 | 0.32 | 0.02 | 0.02 |
| Year fixed effects | yes | yes | yes | yes | yes | yes |
| Industry fixed effects | no | yes | no | yes | no | no |
| Département fixed effects | no | no | yes | yes | no | no |
| Firm-Département-Industry fixed effects | no | no | no | no | yes | yes |

Note: Standard errors in parentheses. $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are corrected to take into account individual autocorrelation. Subsidy is in thousands real euros. Average subsidy≈39.49, median subsidy≈38.69.

heterogeneity could be detected.

We then tested whether the LPS policy had an effect on the size of the clusters they targeted. Table 8 shows that LPS firms belong to "local" pre-existing clusters: the number of firms from the same industry in the département (the left hand side variable in the regressions of this table) is much higher for LPS firms once département fixed effects are introduced. If département fixed effects are not taken into account, LPS seem to be, on the contrary, smaller than other clusters at the national level. However, there is no indication that the cluster policy was attractive to other firms of the same sector. If anything, the years the LPS are implemented are years during which the size of the cluster to which these firms belong relatively decreases. Since Martin, Mayer, and Mayneris (2008) have shown that the size of clusters has a positive impact on French firms productivity, this result may partly explain why we do not find productivity gains for LPS firms.

## 3.2 LPS and firms' labour demand

Up to now, the LPS policy, in spite of the official discourse presenting it as a clear break with policies in favor of regions and industries in difficulty, appears clearly as a defensive policy. If political economy factors are at the origin of the gap between the stated objectives and what we measure, we may be missing all the action when looking at the effect of the policy on productivity. The most important objective for national and local policy makers involved in the policy may in fact be employment of these firms. Preserving jobs rather than increasing productivity may have been the real objective. This is what Criscuolo, Matin, Overman, and Van Reenen (2007) conclude from the study of Regional Selective Assistance in the UK.

To look at this, we adopt the same strategy as for productivity and start with graphical analysis. It appears on figures 3 and 4 that LPS firms are "structurally" bigger than the others. Once individual fixed effects have been taken into account (DD estimator), LPS firms still appear to grow slightly faster than the others. But they do before and after their entry in a LPS, without any clear change in the pattern of differential growth rate, so that it is

Table 8: LPS and localization economies

| Dependent variable: | ln (# of firms same industry-area)$_{iszt}$ | | | | | $\Delta$ln (# of firms same industry-area)$_{iszt}$ |
|---|---|---|---|---|---|---|
| Model : | OLS | OLS | DD | FE | AR(1) | DD |
| LPS firm | -0.109$^c$ | 0.202$^a$ | | | | |
| | (0.060) | (0.049) | | | | |
| Being in a LPS | -0.092$^b$ | -0.047 | -0.017 | -0.020$^b$ | -0.019$^b$ | 0.002 |
| | (0.040) | (0.034) | (0.011) | (0.010) | (0.009) | (0.007) |
| Year fixed effects | yes | yes | yes | n.a. | yes | yes |
| Département fixed effects | no | yes | n.a. | n.a. | n.a. | n.a. |
| Industry-time fixed effects | no | no | no | yes | no | no |
| Firm-Industry-Département fixed effects | no | no | yes | yes | yes | yes |
| Observations | 134474 | 134474 | 134474 | 134474 | 106243 | 134474 |
| $R^2$ | 0.00 | 0.35 | 0.01 | 0.26 | 0.00 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | n.a. | 0.54 | n.a. | n.a. |

Note: Standard errors in parentheses. $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are clustered at the firm-level in regression (1), at the industry-time level in regressions (2) and (3).

difficult to identify a specific role of the policy.

We then concentrate on the econometric analysis. We regress firms' current employment on the two variables "LPS firm" and "Being in a LPS". We develop in appendix a more structural approach of firms' labour demand, which yields similar results.

Whatever the sample and the estimation strategy we use, the impact of the policy never appears significant, and the coefficient is very close to zero when firm-level dynamics is taken into account. This result is consistent with graph (d), which suggested that the relative growth rate of employment in LPS firms was already increasing before the entry in the LPS. Results are the same on the matched sample (tables available upon request).

In conclusion, our results suggest that the LPS policy had no effect on firms' employment.

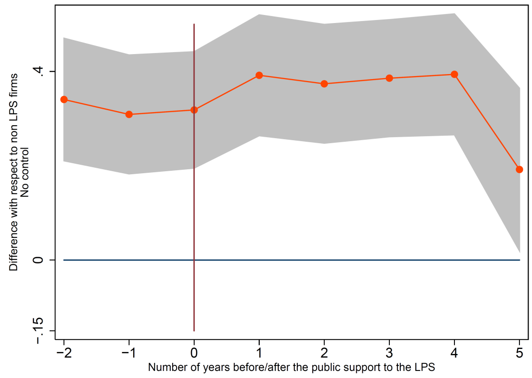
### 3.3 LPS and firms' exports

Finally, we evaluate the impact of the LPS policy on a third dimension of individual performance, firm-level exports. Following an approach inspired by gravity equations, we explain firm-level total exports by the size (in terms of employees) and the TFP of the firm, and by the dummy identifying treated firms. As expected, size and productivity have a very strong and positive impact on firm-level exports. However, the LPS policy has no significant effect, whatever the estimator we use. The same kind of results are obtained on the matched sample (results available upon request).

## 4 Robustness checks

We have conducted so far our analysis at the firm level. Two issues arise about this methodological choice:

1. Proponents of cluster policies often claim that these policies do not only affect the firms directly targeted but the whole sector in the region. In the presence of this type of

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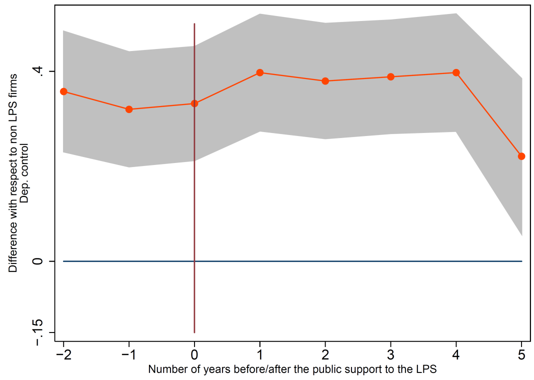


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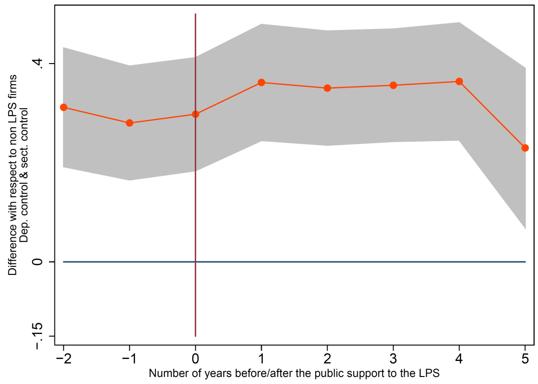


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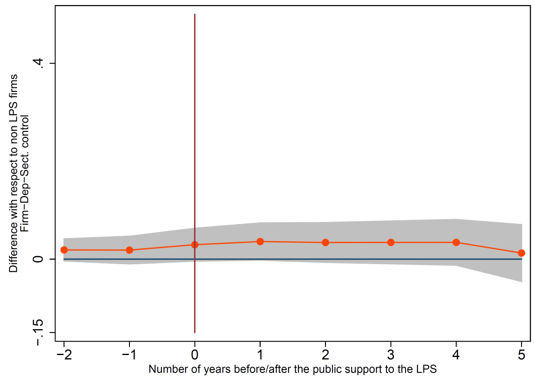


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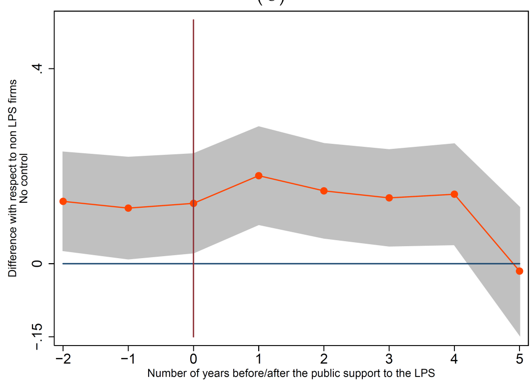


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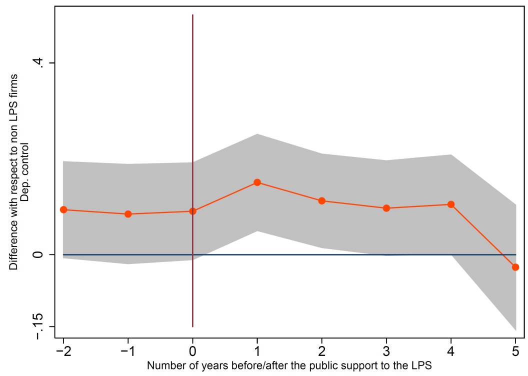


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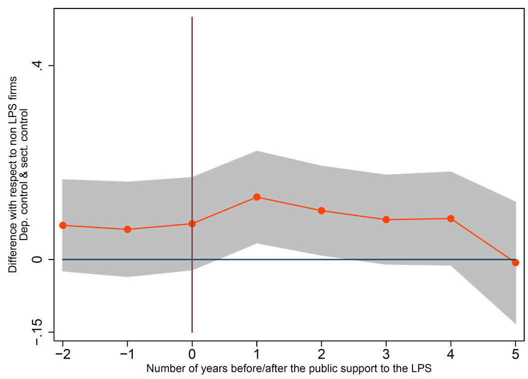


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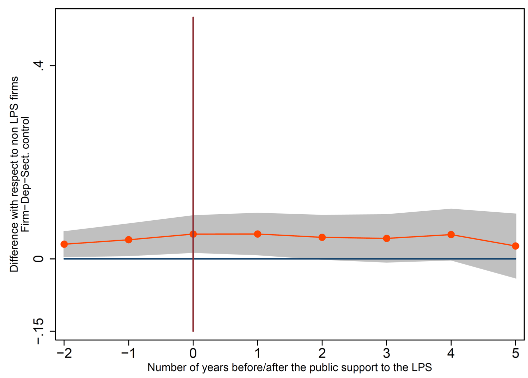


(d)

Figure 4: LPS single-plant firms and evolution of employment

Table 9: LPS and firms' labour demand-Simultaneity bias

| Dependent Variable: | ln $Employees_{it}$ | | | $\Delta$ln $Employees_{it}$ |
|---|---|---|---|---|
| Model : | DD | FE | AR(1) | DD |
| | All firms | | | |
| Being in a LPS | 0.021 | 0.021 | 0.007 | 0.002 |
| | (0.013) | (0.013) | (0.008) | (0.005) |
| N | 137781 | 137781 | 109110 | 137781 |
| $R^2$ | 0.03 | 0.00 | 0.39 | 0.03 |
| AR(1) coefficient | n.a. | n.a. | 0.69 | n.a. |
| | Single plant firms firms | | | |
| Being in a LPS | 0.017 | 0.016 | 0.006 | -0.003 |
| | (0.015) | (0.015) | (0.010) | (0.006) |
| N | 92591 | 92591 | 72166 | 92591 |
| $R^2$ | 0.03 | 0.00 | 0.47 | 0.03 |
| AR(1) coefficient | n.a. | n.a. | 0.64 | n.a. |
| Year fixed effects | yes | n.a. | yes | yes |
| Industry-time fixed effect | no | yes | no | no |
| Firm-Industry-Département fixed effects | yes | yes | yes | yes |

Note: Standard errors in parentheses. [a], [b] and [c] respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are corrected to take into account individual autocorrelation.

Table 10: LPS and firms' exports-Simultaneity bias

| Dependent Variable: | ln $Exports_{it}$ | | | $\Delta$ln $Exports_{it}$ |
|---|---|---|---|---|
| Model : | DD | FE | AR(1) | DD |
| | All firms | | | |
| ln (employees) | $1.483^a$ | $1.623^a$ | $1.364^a$ | $1.104^a$ |
| | (0.083) | (0.080) | (0.074) | (0.119) |
| ln TFP | $0.532^a$ | $0.585^a$ | $0.472^a$ | $0.516^a$ |
| | (0.052) | (0.052) | (0.044) | (0.057) |
| Being in a LPS | 0.121 | 0.112 | 0.053 | 0.117 |
| | (0.171) | (0.173) | (0.171) | (0.121) |
| N | 137781 | 137781 | 109110 | 137781 |
| $R^2$ | 0.01 | 0.01 | 0.01 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.35 | n.a. |
| | Single plant firms firms | | | |
| ln (employees) | $1.444^a$ | $1.606^a$ | $1.345^a$ | $0.980^a$ |
| | (0.114) | (0.110) | (0.102) | (0.151) |
| ln TFP | $0.546^a$ | $0.586^a$ | $0.471^a$ | $0.506^a$ |
| | (0.068) | (0.068) | (0.059) | (0.074) |
| Being in a LPS | 0.133 | 0.126 | 0.212 | 0.048 |
| | (0.225) | (0.226) | (0.224) | (0.146) |
| N | 92591 | 92591 | 72166 | 92591 |
| $R^2$ | 0.01 | 0.01 | 0.01 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.35 | n.a. |
| Year fixed effects | yes | n.a. | yes | yes |
| Industry-time fixed effect | no | yes | no | no |
| Firm-Industry-Département fixed effects | yes | yes | yes | yes |

Note: Standard errors in parentheses. [a], [b] and [c] respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are corrected to take into account individual autocorrelation.

externality, the estimation of the LPS policy at the firm level may underestimate its true economic impact.

2. There is possible measurement error in our sample of LPS firms: in our survey, it is possible that some LPS firms are identified as control firms. The reason is that we have to rely on partially incomplete information provided by managers in response to our survey.

To address both issues, we now present our analysis at the industry-départment level rather than at the firm level. This allows to capture possible local spillover effects. This also reduces the measurement error since the geographical scale of the LPS policy is the départment. Note that we also conducted in unreported investigations the analysis at the industry-employment area level and that results are qualitatively the same.

## 4.1 LPS and industry-départements' productivity, employment and exports

We define the log of performance variable (TFP or export) $y$ in industry $s$ and départment $z$ at time $t$ as a weighted sum of firms' y:

$$\mathrm{y}_{szt} = \sum \left[ \left( \frac{\mathrm{emp}_{iszt}}{\mathrm{emp}_{szt}} \right) \times \mathrm{y}_{iszt} \right] \quad (6)$$

where $\mathrm{emp}_{iszt}$ is the number of employees of firm $i$ from industry $s$, in départment $z$ at time $t$ and $\mathrm{emp}_{szt}$ is the number of employees from industry $s$, in départment $z$ at time $t$.

We define an industry-départment cell as being affected by the LPS policy when at least one firm from industry $s$ and départment $z$ has been involved in LPS over the period. For the employment analysis, we consider total employment in each industry-départment.

Conclusions remain very similar to those obtained at the firm level: table 11 shows that no impact is detected either on industry-départment TFP or on industry-départment employment. We find an impact on industry-départment exports when specific dynamics is controlled for at the industry-départment level (AR(1) and triple differences estimators). This is in line with the descriptive statistics presented in section 2.3, which show that exports in LPS industries grow faster than in the other sectors over the 1996-2004 period. We then adopt at the industry-départment level the matching strategy used for firm-level analysis; table 12 shows that when we compare LPS industry-départment to industry-départment with a similar probability to be treated, nothing is changed for productivity and employment. Regarding exports, the impact is now significant at the 5% level for the difference-in-difference estimator but results are sensitive to the choice of the estimator and to the level of analysis (in unreported regressions, we find that no significant effect can be detected at the industry-employment area level, on both matched or unmatched samples).

To sum up, the analysis at a more aggregated level is consistent with the analysis at the firm level. We find an impact for exports only, but its magnitude and significance strongly depend on the estimator and the sample we use. This suggests that spillovers effects and measurement errors are not very important.

## 4.2 LPS and firms' survival

In this subsection, we test the hypothesis that the LPS policy may have affected the probability of exit of firms. Indeed, in our political economy interpretation, this policy may have

Table 11: LPS and Industry/Département performance-Simultaneity bias

| Dependent Variable: | Avg ln $TFP_{szt}$ | | | $\Delta$ Avg ln $TFP_{szt}$ |
|---|---|---|---|---|
| Model : | DD | FE | AR(1) | DD |
| Being in a LPS | -0.013 | -0.008 | -0.006 | -0.004 |
| | (0.012) | (0.012) | (0.013) | (0.007) |
| N | 31712 | 31712 | 27085 | 31712 |
| $R^2$ | 0.06 | 0.00 | 0.15 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.36 | n.a. |
| Dependent Variable: | ln $Employees_{szt}$ | | | $\Delta$ln $Employees_{szt}$ |
| Model : | DD | FE | AR(1) | DD |
| Being in a LPS | 0.012 | -0.020 | 0.001 | -0.011 |
| | (0.028) | (0.028) | (0.024) | (0.017) |
| N | 31712 | 31712 | 27085 | 31712 |
| $R^2$ | 0.01 | 0.00 | 0.07 | 0.01 |
| AR(1) coefficient | n.a. | n.a. | 0.49 | n.a. |
| Dependent Variable: | Avg ln $Exports_{szt}$ | | | $\Delta$ Avg ln $Exports_{szt}$ |
| Model : | DD | FE | AR(1) | DD |
| Avg ln firms' size | $2.065^a$ | $2.109^a$ | $2.039^a$ | $1.979^a$ |
| | (0.083) | (0.080) | (0.046) | (0.092) |
| Avg ln firms' TFP | $0.798^a$ | $0.839^a$ | $0.755^a$ | $0.807^a$ |
| | (0.113) | (0.113) | (0.072) | (0.113) |
| Being in a LPS | 0.217 | 0.157 | $0.325^b$ | $0.172^b$ |
| | (0.134) | (0.138) | (0.138) | (0.081) |
| N | 31712 | 31712 | 27085 | 31712 |
| $R^2$ | 0.10 | 0.10 | 0.09 | 0.07 |
| AR(1) coefficient | n.a. | n.a. | 0.36 | n.a. |
| Year fixed effects | yes | n.a. | yes | yes |
| Industry-time fixed effect | no | yes | no | no |
| Industry-Département fixed effects | yes | yes | yes | yes |

Note: Standard errors in parentheses. $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are corrected to take into account autocorrelation at the industry-département level.

Table 12: LPS and Industry/Département performance-Matching

| Dependent Variable: | Avg ln $TFP_{szt}$ | | | $\Delta$ Avg ln $TFP_{szt}$ |
|---|---|---|---|---|
| Model : | DD | FE | AR(1) | DD |
| Being in a LPS | -0.021 | -0.015 | -0.019 | -0.007 |
| | (0.015) | (0.015) | (0.014) | (0.008) |
| N | 10682 | 10682 | 9269 | 10682 |
| $R^2$ | 0.07 | 0.00 | 0.09 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.36 | n.a. |
| Dependent Variable: | ln $Employees_{szt}$ | | | $\Delta$ln $Employees_{szt}$ |
| Model : | DD | FE | AR(1) | DD |
| Being in a LPS | 0.022 | -0.002 | 0.011 | -0.001 |
| | (0.034) | (0.033) | (0.028) | (0.021) |
| N | 10682 | 10682 | 9269 | 10682 |
| $R^2$ | 0.01 | 0.00 | 0.03 | 0.01 |
| AR(1) coefficient | n.a. | n.a. | 0.52 | n.a. |
| Dependent Variable: | Avg ln $Exports_{szt}$ | | | $\Delta$ Avg ln $Exports_{szt}$ |
| Model : | DD | FE | AR(1) | DD |
| Avg ln firms' size | $1.987^a$ | $1.987^a$ | $1.957^a$ | $1.930^a$ |
| | (0.105) | (0.104) | (0.062) | (0.115) |
| Avg ln firms' TFP | $0.410^b$ | $0.429^b$ | $0.518^a$ | $0.709^a$ |
| | (0.177) | (0.178) | (0.109) | (0.194) |
| Being in a LPS | $0.310^b$ | 0.236 | $0.292^b$ | $0.209^b$ |
| | (0.153) | (0.151) | (0.139) | (0.098) |
| N | 110682 | 10682 | 9269 | 10682 |
| $R^2$ | 0.14 | 0.13 | 0.12 | 0.10 |
| AR(1) coefficient | n.a. | n.a. | 0.37 | n.a. |
| Year fixed effects | yes | n.a. | yes | yes |
| Industry-time fixed effect | no | yes | no | no |
| Industry-Département fixed effects | yes | yes | yes | yes |

Note: Standard errors in parentheses. $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are corrected to take into account autocorrelation at the industry-département level.

had no effect on productivity but may have helped firms to survive and therefore to maintain employment. We cannot test this hypothesis at the firm level because most of LPS managers gave us only the list of LPS firms still in activity in 2006. Hence, we cannot identify LPS firms which disappeared before our survey.

This is why we conduct the analysis at a more aggregate level. For each industry-département, we compute the share of firms present in the sample in 1996 and still alive in 2004, so that we have one observation per industry-département.

Table 13: LPS and firms' survival-Industry/Département analysis

| Dependent Variable: | Share of surviving firms 1996-2004$_{sz}$ | | | | |
|---|---|---|---|---|---|
| Model : | (1) | (2) | (3) | (4) | (5) |
| LPS industry-Département | 0.036$^b$ | 0.025 | 0.019 | 0.020 | 0.013 |
| | (0.017) | (0.017) | (0.019) | (0.017) | (0.018) |
| ln Average firms' TFP | | 0.095$^a$ | 0.159$^a$ | 0.112$^a$ | 0.187$^a$ |
| | | (0.017) | (0.021) | (0.019) | (0.024) |
| ln Average firms' size$_{sz1996}$ | | 0.025$^a$ | 0.011 | 0.036$^a$ | 0.020$^b$ |
| | | (0.007) | (0.008) | (0.007) | (0.008) |
| Industry fixed effects | no | no | yes | no | yes |
| Département fixed effects | no | no | no | yes | yes |
| Observations | 4174 | 4174 | 4174 | 4174 | 4174 |
| $R^2$ | 0.00 | 0.01 | 0.02 | 0.02 | 0.10 |

Note: Standard errors in parentheses. $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels.

The first regression shows that industry-départements targeted by the LPS policy are characterized by a higher survival rate between 1996 and 2004. We then control for the level of productivity and the average size of firms in the industry-département, which both affect positively the share of surviving firms between 1996 and 2004. It is well known that larger and more productive firms are less likely to exit (see for example Alvarez and Görg (2007)). When average individual characteristics are controlled for, no LPS premium is detected. This is in line with our finding that LPS firms are larger than average. Controlling for industry and département fixed effects then does not affect the conclusion: the coefficient on LPS policy is positive but not significant.

# 5 Conclusion

Our results on the first cluster policy implemented in France are not very positive. First, the policy targeted firms in regions and sectors that were experiencing difficult times in terms of productivity and therefore competitiveness. This was not its official objective and we can interpret the gap between the stated and revealed objectives in political economy terms. The administration in charge of the policy, the DIACT, formerly the DATAR, was created to promote territorial equity and to help lagging regions. It appears that it was not able or willing to change in practice and our results point to bureaucratic continuity. Another possible interpretation of the gap between stated and revealed objectives is that the policy was captured by firms. Second, the policy did not succeed in reversing the relative decline in productivity for the targeted firms.

Third, the policy had no effect on the employment and exports of firms involved in the LPS policy.

Our results would be consistent with a political economy interpretation: the revealed objective of the policy was to protect some large firms (LPS firms are larger than average) in declining regions and sectors. One could argue that this policy may have had no effect on firm-level performance but at least was not very costly. Note that one reason could be that the subsidies were too small to have a real impact and/or to attract the most dynamic firms. However, we have shown that the largest subsidies had been given to firms in decline. This low price tag does not apply to a more recent and ambitious cluster policy implemented in France, called competitiveness clusters, with a 1,5 billion euros price tag.

To our knowledge, our study is the first to analyze empirically, with firm level data, the impact of a cluster policy. It points to the apparent failure of the LPS policy to improve the performance of targeted firms through better cooperation and to increase the attractiveness of existing clusters. Obviously, our results cannot be generalized to other cluster policies which may have performed better. However, we interpret it as a cautionary tale for policy makers intending to commit large amounts of public money to such policies.

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# APPENDIX

Table 14: LPS determinants-GMM TFP

| Model : | Dependent Variable: lps_status (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Mean (ln $Sales_{it}$) | $0.035^c$ | $0.047^b$ | $0.103^a$ | $0.106^a$ |
| | (0.021) | (0.021) | (0.023) | (0.024) |
| Mean (GMM $TFP_{iszt}$) | $-0.130^b$ | -0.048 | 0.026 | 0.001 |
| | (0.065) | (0.067) | (0.082) | (0.080) |
| Mean (GMM TFP $growth_{it}$) | -0.034 | -0.045 | -0.038 | -0.029 |
| | (0.098) | (0.118) | (0.119) | (0.127) |
| Mean (ln $Subsidies_{it}$) | $0.038^a$ | $0.040^a$ | $0.034^a$ | $0.034^a$ |
| | (0.007) | (0.008) | (0.008) | (0.008) |
| Mean (ln (# of firms, same ind.-dép.$_{it}$)) | $-0.099^a$ | $-0.118^b$ | $0.164^a$ | $0.156^a$ |
| | (0.037) | (0.050) | (0.053) | (0.055) |
| ln Mean (Taxable net $income_{z1994}$) | | | | $2.112^a$ |
| | | | | (0.730) |
| ln Mean (Taxable net income growth $rate_{z1984-1994}$) | | | | $-0.993^b$ |
| | | | | (0.468) |
| ln Population $density_{z1994}$ | | | | -0.102 |
| | | | | (0.070) |
| ln Industrial jobs $share_{z1994}$ | | | | $0.724^a$ |
| | | | | (0.186) |
| ln Number of people with a CAP or a $BEP_{z1990}$ | | | | $-1.237^b$ |
| | | | | (0.559) |
| Industry fixed effects | no | yes | yes | yes |
| Département fixed effects | no | no | yes | yes |
| N | 16527 | 16527 | 16527 | 16527 |
| $R^2$ | 0.025 | 0.064 | 0.189 | 0.207 |

Note: Robust standard errors in parentheses $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. All regressions are clustered at the emloyment area level. Necessarily, t ≤ lps_year

Table 15: LPS and regional policies

| | LPS firm | PAT in the dép.$_{2000-2006}$ | Share of subsidized emp. in the dép.$_{2006}$ |
|---|---|---|---|
| LPS firm | 1 | | |
| PAT in the dép.$_{2000-2006}$ | $0.04^a$ | 1 | |
| Share of subsidized employment in the dép.$_{2006}$ | $0.02^a$ | $0.54^a$ | 1 |

Note: Standard errors in parentheses $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels.

Table 16: Summary statistics about single plant firms

| | LPS firms | | | Non LPS firms | | |
|---|---|---|---|---|---|---|
| Variable | Obs | Mean | Std. Dev. | Obs | Mean | Std. Dev. |
| Value added | 2136 | 3300.57 | 7480.25 | 115150 | 2912.67 | 7670.64 |
| Employees | 2136 | 84.61 | 174.27 | 115150 | 68.67 | 130.00 |
| Capital Stock | 2136 | 5405.29 | 31364.98 | 115150 | 3286.84 | 16589.23 |
| Labour productivity | 2136 | 37.85 | 16.84 | 115150 | 41.34 | 34.96 |

Note: Value added, capital, capital intensity, labour productivity and exports are expressed in thousands of real euros

### The estimation of TFP

To calculate firms' TFP, we estimate a production function. We use a Cobb-Douglas framework and we suppose that the value-added of firm $i$ at time $t$, $Y_{it}$, is:

$$Y_{it} = A_{it} K_{it}^{\alpha} L_{it}^{\beta} \quad (7)$$

where $K_{it}$ and $L_{it}$ are respectively the capital and the employees of the firm.
After a log-transformation, the model we will estimate is:

$$y_{it} = \alpha k_{it} + \beta l_{it} + \epsilon_{it} \quad (8)$$

The estimation of such a production function is not trivial. Indeed, some unobserved characteristics can both affect the amount of inputs and the level of output. If the entrepreneur is less risk-averse than the others, he might tend to adopt a particular labor-capital mix, he might have different innovation strategies and also might tend to seek less risky (and potentially less lucrative) markets. On the other hand, if the entrepreneur faces a positive productivity shock, he might produce more and hire more people in the same time. Here again, the estimates of inputs-elasticities may be spurious.

An important literature has developed about the estimation of production functions. We built on Griliches and Mairesse (1995) and on Petrin, Poi, and Levinsohn (2004) to calculate two estimates of inputs elasticities. For the GMM estimation, we first-difference all the variables and we instrument inputs by their level at time $t-2$. It yields reasonable coefficients, with slightly increasing returns to scale (0.87 for labour and 0.19 for capital), but due to insufficient number of observations, we cannot run the estimation by sector. The Levinsohn-Petrin (LP) method is applied by sector and, on the contrary, exhibits a decreasing returns to scale production functions, with rather credible coefficients (generally around 0.70 for labour and 0.15 for capital).

We present the results for the GMM and the LP estimators of TFP. Results are very similar to those obtained with a simple OLS TFP index.

Table 17: LPS and GMM TFP-Simultaneity bias

| Dependent Variable: | ln TFP | | | Δln TFP |
|---|---|---|---|---|
| Model : | DD | FE | AR(1) | DD |
| | All firms | | | |
| Being in a LPS | -0.023 | -0.021 | -0.000 | 0.016 |
| | (0.015) | (0.015) | (0.014) | (0.010) |
| N | 137781 | 137781 | 109110 | 137781 |
| $R^2$ | 0.01 | 0.00 | 0.12 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.37 | n.a. |
| | Single plant firms firms | | | |
| Being in a LPS | -0.010 | -0.005 | 0.001 | 0.018 |
| | (0.015) | (0.015) | (0.017) | (0.012) |
| N | 92591 | 92591 | 72166 | 92591 |
| $R^2$ | 0.01 | 0.00 | 0.14 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.36 | n.a. |
| Year fixed effects | yes | n.a. | yes | yes |
| Industry-time fixed effect | no | yes | no | no |
| Firm-Industry-Département fixed effects | yes | yes | yes | yes |

Note: Standard errors in parentheses. $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are corrected to take into account individual autocorrelation.

Table 18: LPS and Levinsohn-Petrin TFP-Simultaneity bias

| Dependent Variable: | ln TFP | | | Δln TFP |
|---|---|---|---|---|
| Model : | DD | FE | AR(1) | DD |
| | All firms | | | |
| Being in a LPS | -0.018 | -0.015 | 0.003 | $0.016^c$ |
| | (0.014) | (0.015) | (0.014) | (0.010) |
| N | 137781 | 137781 | 109110 | 137781 |
| $R^2$ | 0.02 | 0.00 | 0.21 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.38 | n.a. |
| | Single plant firms firms | | | |
| Being in a LPS | -0.005 | -0.000 | 0.006 | 0.017 |
| | (0.015) | (0.015) | (0.016) | (0.012) |
| N | 92591 | 92591 | 72166 | 92591 |
| $R^2$ | 0.01 | 0.00 | 0.23 | 0.00 |
| AR(1) coefficient | n.a. | n.a. | 0.37 | n.a. |
| Year fixed effects | yes | n.a. | yes | yes |
| Industry-time fixed effect | no | yes | no | no |
| Firm-Industry-Département fixed effects | yes | yes | yes | yes |

Note: Standard errors in parentheses. $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are corrected to take into account individual autocorrelation.

### The estimation of labour demand

Firm-level labour demand functions are usually estimated in the literature thanks to dynamic models. Following Girma, Görg, Strobl, and Walsh (2007), we estimate the following log-linearized empirical model:

$$l_{it} = \alpha l_{it-1} + \beta y_{it} + \beta w_{it} + \epsilon_{it} \tag{9}$$

where $l_{it}$ is labour demand, $y_{it}$ is value added and $w_{it}$ is the average wage of firm $i$ at time $t$. We consider that firms are price-taker for wages, which seems to be a reasonable assumption given the low degree of variability of average wage across firms. For symmetric reasons to those mentioned about the estimation of production functions, and for technical aspects of the estimation of dynamic models, $l_{it-1}$ and $y_{it}$ are endogenous. Here again, we consequently use a GMM approach on first-differenced variables instrumented by their level at time $t-2$. All the coefficients have the expected sign (the current number of employees in a firm is positively affected by past level of employment and by current level of activity and negatively affected by current average wage) and the results are coherent with the literature.

We calculate the residuals of that regression and we use them to assess the impact of the LPS policy on firms' employment, once "core" determinants of employment have been taken into account.

Table 19: Labour demand

| Dependent Variable: ln Employees$_{it}$ | |
|---|---|
| Model : | (1) |
| ln Employees$_{it-1}$ | 0.539$^a$ |
| | (0.024) |
| ln Value added$_{it}$ | 0.134$^a$ |
| | (0.025) |
| ln Average wage$_{it}$ | -0.448$^a$ |
| | (0.024) |
| N | 120168 |
| Centered R$^2$ | -0.03 |
| Sargan-Hansen p-value | 0.19 |

Note: Standard errors in parentheses $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are clustered at firm level.

Table 20: LPS and firms' residual labour demand-Simultaneity bias

| Dependent Variable: | ln Residual employment | | | Δln Residual Employment |
|---|---|---|---|---|
| Model : | DD | FE | AR(1) | DD |
| | All firms | | | |
| Being in a LPS | 0.007 | 0.008 | 0.002 | -0.000 |
| | (0.005) | (0.005) | (0.007) | (0.005) |
| N | 112119 | 112119 | 87236 | 112119 |
| $R^2$ | 0.02 | 0.00 | 0.02 | 0.01 |
| AR(1) coefficient | n.a. | n.a. | 0.18 | n.a. |
| | Single plant firms firms | | | |
| Being in a LPS | 0.000 | 0.000 | -0.004 | 0.001 |
| | (0.006) | (0.006) | (0.008) | (0.006) |
| N | 75061 | 75061 | 57435 | 75061 |
| $R^2$ | 0.02 | 0.00 | 0.02 | 0.01 |
| AR(1) coefficient | n.a. | n.a. | 0.17 | n.a. |
| Year fixed effects | yes | n.a. | yes | yes |
| Industry-time fixed effect | no | yes | no | no |
| Firm-Industry-Département fixed effects | yes | yes | yes | yes |

Note: Standard errors in parentheses. $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels. Standard errors are corrected to take into account individual autocorrelation.

Table 21: Summary statistics on demeaned variables wrt sectoral average - Non matched sample of firms

| | LPS firms | | | Non LPS firms | | | |
|---|---|---|---|---|---|---|---|
| Variable | Obs | Mean | Sd | Obs | Mean | Sd | $\text{Mean}_{\text{LPS}}$-$\text{Mean}_{\text{Non LPS}}$ |
| Mean (ln $\text{Sales}_{it}$) | 345 | 0.239 | 1.426 | 16182 | -0.016 | 1.238 | $0.255^a$ |
| Mean ($\text{TFP}_{it}$) | 345 | -0.037 | 0.339 | 16182 | 0.043 | 0.441 | $-0.08^a$ |
| Mean (TFP $\text{growth}_{it}$) | 345 | -0.020 | 0.114 | 16182 | -0.016 | 0.170 | -0.004 |
| Mean (ln $\text{Subsidies}_{it}$) | 345 | 1.424 | 3.786 | 16182 | -0.024 | 3.580 | $1.448^a$ |

Note: $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels.

Table 22: Summary statistics on demeaned variables wrt sectoral average - Matched sample of firms

| | LPS firms | | | Non LPS firms | | | |
|---|---|---|---|---|---|---|---|
| Variable | Obs | Mean | Sd | Obs | Mean | Sd | $\text{Mean}_{\text{LPS}}$-$\text{Mean}_{\text{Non LPS}}$ |
| Mean (ln $\text{Sales}_{it}$) | 260 | 0.261 | 1.523 | 7730 | 0.178 | 1.364 | 0.083 |
| Mean ($\text{TFP}_{it}$) | 260 | -0.045 | 0.326 | 7730 | -0.008 | 0.392 | -0.037 |
| Mean (TFP $\text{growth}_{it}$) | 260 | -0.018 | 0.122 | 7730 | -0.019 | 0.167 | 0.001 |
| Mean (ln $\text{Subsidies}_{it}$) | 260 | 1.199 | 3.841 | 7730 | 0.715 | 3.603 | $0.484^b$ |

Note: $^a$, $^b$ and $^c$ respectively denoting significance at the 1%, 5% and 10% levels.