Decomposing the employment effects of investment subsidies

Third revision

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Abstract: Most governments tackle the economic issues of underdeveloped areas by offering subsidies aimed at fostering economic activities and local employment. Localized policies put constraints on where businesses may locate to receive subsidies, but they generally place few restrictions on whom subsidized businesses must hire. Using administrative data on firms and workers in Italy, we adopt a multi-cutoff regression discontinuity design to empirically assess and decompose the employment effect of substantial incentives for the replacement or establishment of new capital. Our empirical strategy allows identifying the geographical origin and labor market status of new hires. The results show how the majority of recruits come from new entrants to the labor market, in particular, young people and students, while displacement effects are limited. It appears that subsidized companies tend to keep their most valuable staff and hire more qualified young people. Overall, we find only a modest spatial dispersion of the effects or a possible crowding-out of the local labor market.

Keywords: local labor market; place-based policy; labor mobility; regression discontinuity

JEL codes: C14; H25; J60; R23

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1. Introduction

Most governments support underdeveloped regions by offering investment subsidies that claim to protect jobs and reduce unemployment by fostering local economic activities. Such place-based programs are expected to bring many new hires in the local labor market (LLM), which, ideally, should come from the pool of non-employed, namely youngsters and the unemployed, residing in the targeted area. However, “a common feature of these programs is that, while restricting where businesses may locate or invest in order to receive subsidies or tax breaks, they place few constraints on whom subsidized businesses must hire” (Freedman, 2015, p. 1). The critical aspect is the mobility of labor as subsidized firms’ new hires might come from other companies and/or other LLMs, thereby reducing and diluting the positive local effects of the program. This means that there could be a substantial difference between the targeted zone and the area affected by the policy. Another important aspect is the ‘stealing workers effect’, namely the possibility that financed companies might poach (skilled) workers from non-subsidized companies in the LLM by offering higher wages (see Soskice, 1994).

Although the literature on the employment effects of place-based policies has recently been enriched (see, for instance, Giua, 2017; Mayer et al., 2017; Criscuolo et al., 2019; Accetturo et al., 2020), the analysis of labor mobility between areas or within areas caused by place-based policies is still scarce. A notable exception is Freedman (2015), who finds that a large share of the new jobs created by tax incentives in low-income US neighborhoods does not go to residents of targeted neighborhoods. Our study contributes to this strand of literature by analyzing the effects on employment and labor sub-markets – regarding the subsidized company as well as the area where it is located – of the Italian Law 488/92 (henceforth L488), a high-budget policy consisting of capital subsidies for the replacement or establishment of new capital aimed at reducing territorial disparities. This kind of analysis requires a wealth of data with detailed and accurate information on the population of workers and plants as well as on workers’ mobility. To this end, we built our database by combining two restricted-access data archives: the Italian social security administration (INPS) archive, which covers the universe of Italian employer-employee matches in the private sector, and the administrative L488 archive containing detailed information on all subsidized and non-subsidized projects. Another challenge is the presence of endogeneity. This makes causal inference difficult since the subsidy assignment process and the extent of the impact also depend on unobservable characteristics of the areas on which the different labor markets act and are therefore correlated to the dynamics of employment and local unemployment. We tackle this issue by exploiting the sharp discontinuities in the L488 allocation mechanism, which creates the conditions for a local random experiment. Using the non-parametric robust bias-corrected regression discontinuity design (RDD) estimator, with covariate adjustment (Calonico et al., 2019) for the case of noncumulative multiple cut-offs (Cattaneo et al., 2020b), we compare the employment composition and dynamics in the firms ranked around the cutoff point. Using this method, we evaluate the effects of incentives on the labor market, identifying where new hires come from (from the same LLM, a neighboring LLM or a distant LLM), and from which pool of individuals (those working for another company, students or inactive people).

Our contribution to the literature is threefold. First, our study contributes on the employment effects of capital subsidies (see, among others, Bondonio and Greenbaum, 2014; Cerqua and Pellegrini, 2014; Andini and de Blasio, 2016; Criscuolo et al., 2019). Given the considerable uncertainty on this parameter in the literature, our analysis presents detailed and methodologically robust results based on the universe of Italian firms, which, differently from balance-sheet data, allows creating a database that is not skewed towards larger firms. Second, we contribute to the literature by assessing the degree of employment displacement, and how this reflects in the targeted LLM in terms of unemployment. While some studies find the presence of displacement in the aggregate (for the L488, see Bronzini and de Blasio, 2006; De Castris and Pellegrini, 2012), we provide a detailed evaluation of the origin of the new hires brought about by a plant-level shock. Third, we add to the literature on the evaluation of the internal and external effects of public incentives (e.g., Freedman, 2015; Einiö and Overman, 2020), i.e., the employment leakages from the targeted LLM and therefore the extent of flows from outside. By focusing on the employment differential between the treated and the untreated firms close to the discontinuity in the treatment allocation, we estimate internal and external effects to the LLM, identifying the relative weight of the leakage and 'stealing' worker effects, and reducing issues related to plant-level omitted variables, labor market unobservable characteristics and simultaneity.

The results confirm the positive effect of the L488 subsidies on employment in subsidized plants. However, we show that the large part of recruits come from new entrants to the labor market, in particular, young people and students. Further, one-sixth of the employment impact is due to workers who managed to keep their jobs thanks to the L488 subsidies. The overall effect is a change in the composition of employment, both in terms of age and human capital. The share of workers coming from the same labor market is also considerable, but lower. There is also a non-negligible share that comes from non-contiguous LLMs, suggesting that there are leakage effects but they are low. Therefore, the main conclusions are that the local effects are significant, with reduced leakage to the outside. The primary sources of new employment are young people and graduates from the same LLM, and the effects of 'stealing' workers from non-subsidized companies in the same LLM or neighboring LLM appears limited.

2. The L488 policy

L488 is an Italian State law enacted in 1992, which became operational in June 1996. Over the period 1996-2007, about €23 billion were spent on funding investment projects. This policy allocates subsidies through a rationing system based on 'calls for tender' that mimics an auction mechanism, and that guarantees compatibility of demand and supply of the incentives. L488 makes grants available for investment projects in less-developed regions. The grants are to help build new productive units or increase production capacity and employment or improve ecological conditions associated with production processes, technological updates, restructuring, relocation, and reactivation. The implementation of the selection process is centralized, and a specific department of the Ministry of Economic Development presides over the selection process. The procedure includes a screening based on a technical report and a business plan presented by the company, and a viability analysis, which also evaluates the funding eligibility of the project. The amounts awarded are paid out in three equal installments.

Incentives allocation is based on regional competitive auctions. For each auction, there are as many rankings as the number of regions involved, and each ranking has a different cutoff point, related to the regional availability of financial resources. In each auction, five objectives and predetermined criteria are used to rank the investment projects: 1) the share of owners' funds on total investment; 2) the new job creation by unit of investment;[1] 3) the ratio between the subsidy requested by the firm and the highest subsidy applicable; 4) a score related to the priorities of the region concerning location, project type, and sector; 5) a score related to the environmental impact of the project. The five criteria carry equal weight: the values related to each criterion are normalized, standardized, and added up to produce a single score that determines the place of the project in the regional ranking. The rankings are drawn up in decreasing order of the score awarded to each project and the subsidies are allocated to projects until the funding granted to each region is exhausted. Several checks during, and after the investment period are carried out to determine whether subsidized firms have respected their targets. If a treated firm does not reach its goals, the subsidy is partially or entirely revoked. Furthermore, L488 requires that firms applying for the subsidies renounce any other public subsidies, avoiding dual subsidization and the use of other public grants.

Many papers have assessed the impact of this policy given the characteristics of the grant allocation process, which allows exploiting identification conditions with credible identification hypotheses. After the pioneering study by Bronzini and de Blasio (2006), several studies have assessed the causal

[1] This criterion was introduced to favor investment projects aimed at increasing employment in order to counter the theoretical prediction of capital-labor substitution engendered by capital subsidies (see Patrick, 2016).

impact of L488 at the firm-level (see, among others, Cerqua and Pellegrini, 2014; Biagi et al., 2015; Pellegrini and Muccigrosso, 2017; Bernini et al., 2017) or at the area-level (De Castris and Pellegrini, 2012; Cerqua and Pellegrini, 2017). These studies agree, using different evaluation approaches and data sources, on identifying a positive and significant effect of the policy, except for Bronzini and de Blasio (2006), who argue that L488 "incentives would above all have induced effects of anticipation of investment decisions". In a more recent paper, Cerqua and Pellegrini (2014) take full advantage of the discontinuity created by the presence of several rankings in each targeted region, and shows that subsidized firms hire from 5 to 8 extra employees on average with respect to nonsubsidised firms and that these positive effects are not due to intertemporal substitution. However, none of these studies provides evidence on the geographic origin and the previous job position of new hires.

3. Data and method

3.1 Data

Our analysis refers to the period 1995–2004 and focuses on the four calls for tender that were concluded by 2001,[2] which mainly targeted manufacturing firms. Although L488 has financed firms in all Italian regions, our focus is on southern Italy since the use of L488 has been substantial only there: it has covered almost all the 325 LLMs and with a much higher incentive intensity than in the center-north of Italy (Cerqua and Pellegrini, 2020). L488 funds were concentrated in the south because all eight southern regions (Abruzzi, Basilicata, Calabria, Campania, Molise, Apulia, Sardinia and Sicily) were lagging in terms of economic performance and, therefore, were considered as Objective 1 regions regarding the European Union Structural Funds.[3]

The final database was built by integrating two data archives: an administrative dataset containing detailed information on all subsidized and non-subsidized firm-level projects by L488 and a unique dataset covering the universe of employer-employee matches in the private sector made available by the Italian social security administration (INPS). The latter dataset contains high-quality data as they are used to collect contributions from employers, define pension rights and eligibility to social programs (Boeri and Garibaldi, 2019). The integration process of these datasets, for employees per

[2] The application deadline was June 1996 for auction 1 (last installment on November 1998), February 1997 for auction 2 (last installment on July 1999), April 1998 for auction 3 (last installment on October 2000), and November 1998 for auction 4 (last installment on May 2001).

[3] Objective 1 funding forms the most substantial part of the overall Structural Funds Program budget and has the explicit aim of fostering economic growth in regions that are lagging behind (Becker et al., 2010). In the center-north only the areas designated as Objective 2 or 5b were targeted, i.e., areas which suffer from high unemployment and/or have high shares of employment in declining industries but with a much lower subsidy intensity.

enterprise and per year, was based on VAT or tax codes and was conducted by INPS's IT facilities. The employer-employee database also provides plant-level data, which allows to precisely locate all plants within multi-plant firms, removing any measurement error due to the use of firm-level data. The starting point was all the firms that applied to be funded by L488 in the first four auctions in southern Italy. We then cleaned the integrated dataset to obtain our final sample. Some of the rankings have too few observations in the proximity of the threshold and, therefore, cannot be analyzed via the non-parametric robust bias-corrected RDD estimator. The final sample is then made up of 15 rankings and the details of its construction are described in Online Appendix A.[4] This sample is made up of 4,197 continuing firms (of which 2,253 were treated), which displayed positive employment levels throughout the period 1995-2001. This sample is much larger than the one used by previous studies covering the same period of analysis (see, among others, Bronzini and de Blasio, 2006; Cerqua and Pellegrini, 2014) as the use of the INPS archive rather than balance-sheet data guarantees a much higher matching rate. Further, it ensures that our final sample is not skewed towards larger firms as those used in previous studies.

We are interested in verifying whether capital subsidies modify the composition of workers a plant employs and whether they induce firms to pay its workers more. To carry out the task, we have chosen LLMs as the most appropriate unit of spatial analysis for identifying labor mobility.[5] We first use the number of employees in 2001 as the dependent variable and then split each individual registered to be working in a treated or a control plant in 2001[6] in 9 mutually exclusive categories depending on their employment conditions in the pre-treatment year, i.e., 1995:

i) those working in the same plant;
ii) those receiving unemployment benefits;
iii) those working in another plant located in the same LLM;
iv) those working in another plant located in a neighboring LLM;
v) those working in another plant located far-away from the LLM;
vi) those not registered to be working in 1995 who were 24 or less and were born in the same LLM;

[4] In Online Appendix A, we report the number of observations in each ranking (Table A1), the descriptive statistics for all groups of firms split by treatment status (Table A2) and the sectorial composition of the final sample (Table A3).

[5] The criteria used to determine Italian LLMs are similar to those used to define Metropolitan Statistical Areas in the US or Travel to Work Areas in the UK.

[6] In the main analysis, we focus on the short-term impact of the policy. However, In Section 4.3, we will look at the medium-term impact of such a policy using 2004 instead of 2001 as the reference year.

vii) those not registered to be working in 1995 who were 24 or less and were born in another LLM;

viii) those not registered to be working in 1995 who were 25 or above and were born in the same LLM;

ix) those not registered to be working in 1995 who were25 or above and were born in another LLM.

Individuals not registered to be working by the INPS archive in 1995 could have been students, working in the primary sector, working in the public sector, self-employed, or inactive, which we are not able to identify separately. Nevertheless, we assume that younger people are on the whole students, and therefore splitting individuals by age group (the cutoff is 24 years) allows to roughly separate high-school/university graduates from the other categories.

We also collect data on the number of employees and the average wage in 1995, which will be used as control variables in the RDD regressions.

3.2 Method

The key feature of the RDD is the existence of a forcing variable for each firm in the sample, which sharply determines subsidy assignment: all firms whose score is above the cutoff obtain the subsidy, while all firms whose score is below this cutoff do not. The parameter of interest is the local average treatment effect (LATE) that reflects the impact of the L488 subsidies on the employment mobility and composition of firms close to the threshold. In such a setting, identification, estimation, and inference proceed by comparing the responses of firms near the cutoff, taking those below as the comparison group to those above (treatment group) (Calonico et al., 2019).

Our setting differs from the basic RDD framework as we must combine 15 rankings having a different cutoff point. To this end, we use the RDD estimator developed by Cattaneo et al. (2020b) for the case of noncumulative multiple cutoffs. This estimator first estimates the treatment effect for each ranking via the non-parametric robust bias-corrected RDD estimator with covariate adjustment (Calonico et al., 2019); and second, carries out a weighted average of the cutoff-specific treatment effects in order to acquire the global treatment effect of the policy under analysis. The weights are based on the number of firms within the bandwidth in each ranking.[7]

[7] A potential alternative called 'pooled multiple RDD approach' (see Gamse et al., 2008), consists of grouping observations from different rankings into a single dataset by re-centering and standardizing the forcing variable. However, pooling observations from different rankings could bias the LATE estimates due to the potentially different characteristics

The non-parametric estimates are robust bias-corrected with covariate adjustment (Calonico et al., 2019) and are conducted with triangular kernel weights. The bandwidths for each non-parametric local linear regression are selected using the mean squared error (MSE)-optimal bandwidth selector, with observations outside the bandwidth receiving zero weight in the estimation.[8] The addition of pre-treatment covariates allows increasing the precision of the RDD estimates.

3.3 Descriptive evidence

Our analysis concerns the LLMs of southern Italy, in which the average yearly wage in 1995 was below €10,000, the average employees' age was 32, the vast majority of workers were blue-collars (77%), and the shares of part-time workers (3%) and of women (21%) was limited. Table 1 compares the characteristics of subsidized and non-subsidized firms in our sample. As shown in columns (1) and (2), subsidized plants tend to be larger, pay more to their employees, and hire more women. Conversely, they are very similar to non-subsidized plants regarding the share of blue-collar, the share of part-time employees, the share of employees born in center-north of Italy, firm, and employees' age.

INSERT TABLE 1

In the spirit of the RDD framework, in columns 3 and 4, we report the same descriptive statistics as in columns 1 and 2 but limited to 25% of the observations closest to the threshold. In this case, baseline plant differences shrink, especially the employment gap. More importantly, when comparing plant characteristics on either side of the threshold, they do not vary discontinuously at the L488 assignment threshold. Column (5) of Table 1 shows that, when applying the non-parametric robust bias-corrected RDD estimator with noncumulative multiple cutoffs, we find no evidence of statistically significant pre-treatment differences at 5% level around the cutoff point between subsidized and non-subsidized plants in terms of number of employees, average yearly wage, share of blue-collars, share of part-time employees, share of female employees, share of employees born in the center-north of Italy, firm and employees age. These estimates confirm that our RDD estimator does a good job of randomizing the full set of pre-treatment covariates around the cutoff.

of treated firms in different rankings. We will use the pooling approach with the addition of sector and ranking dummies as a robustness check (see Online Appendix C).

[8] The Stata program 'rdmc' (Cattaneo et al., 2020b) is used to produce bias-corrected non-parametric point estimates with accompanying robust standard errors for the case of noncumulative multiple cutoffs.

4. Results

As we are in an RDD framework, it is essential to rule out possible discontinuities in the conditional density of the forcing variable (the score of the project in the regional ranking), which would indicate evidence of manipulation in the incentives assignment. We tested the null hypothesis of no manipulation with the robust test of Cattaneo et al. (2020a) for the pooled multiple RDD (p-value=0.33), as well as for each ranking. Only in one out of the 15 rankings did the p-value turn out to be below the 0.05 threshold.[9] Overall, this robust test indicates that there is no statistical evidence of sorting.

The starting point of our empirical analysis is to verify the extent to which the reception of capital subsidies led treated firms to hire more workers and, more importantly, their geographic origin as well as their previous job position. We then look at additional dependent variables and examine the medium-term impact of the policy.

4.1 Employment estimates

For a valid application of the RDD, it is key to graphically present the changes in the variables of interest at the cutoff point. Given the presence of multiple rankings, we use the pooled dataset to provide graphical evidence. Before turning to the estimates disaggregated by the working situation in 1995, in Panel A of Figure 1, we plot two first-order local polynomial regressions estimated on both sides of the threshold. There is a clear and statistically significant jump in employment right at the threshold, with a magnitude of approximately 8 additional employees. In Panel B of Figure 1, we graphically disaggregate such an estimate by the working situation in 1995. For most categories of workers, there appears to be a statistically significant discontinuity at the threshold, suggesting that the overall employment impact of L488 was absorbed by new entrants in the labor market, by individuals already working for other plants, as well as by individuals from outside the targeted LLM.

INSERT FIGURE 1

Although graphical evidence is important in an RDD setting, only the application of a valid RDD estimator allows obtaining accurate estimates. The multi-cutoff RDD estimator presented in Section 3.2 mitigates potential concerns due to the creation of a single dataset with a re-centered and standardized forcing variable. Table 2 reports the aggregated (Panel A) and disaggregated (Panel B)

[9] In case of multiple testing, a few statistically significant estimates are expected to arise by chance from time to time and do not automatically invalidate the underlying assumption of an RDD (Eggers et al., 2015).

average employment increase in subsidized plants with respect to non-subsidized plants at the threshold. The estimated coefficients are all positive and are statistically significant at the 5% level, except for individuals receiving unemployment benefits. The average employment impact is positive and sizable: subsidized firms hire 8.6 extra employees on average with respect to non-subsidized firms. Looking at the disaggregated estimates, we see that a large share of the employment impact comes from the non-employed young people born in the same LLM (2 additional employees) and from those who were working in the same LLM but at a different plant (1.4 additional employees). We also find a positive impact between 0.4 and 1.2 for the following groups: those who were working in a neighboring LLM, those who were working in a distant LLM, the non-employed young people born in another LLM, the non-employed older people born in the same LLM and the non-employed older people born in another LLM. All these estimates are statistically significant at the 1% level. We also find that subsidized firms managed to keep a larger share of the employees (1.5 additional employees) who were already working in that firm in 1995. Conversely, we detect a positive but less statistically significant estimate when considering those who were receiving unemployment benefits (0.3 additional employees).

INSERT TABLE 2

We can use these estimates to highlight some interesting results: 55.8% of new employment comes from new entrants into the labor market, including the unemployed (categories ii), vi), vii), viii) and ix)), while the remaining 44.2% comes from individuals who were already working. Among the new entrants, the majority (63.4%) are less than 24 years old (categories vi) and vii)) and, therefore, likely to have recently graduated from high-school or university. Our interpretation of these results is that new investments require up-to-date skills, more common among the young entrants, who could also be trained more easily (see, among others, De Koning and Gelderblom, 2006). Another interesting element concerns the presence of leakages: 33.6% of additional workers come from outside the LLM where the investment was made, or they were born in another LLM, if they did not previously work (categories iv), v), vii) and ix)). There are, therefore, broader spatial effects of the investment compared to the LLM, which, however, are not very wide, and concern almost a third of the new jobs generated by the subsidized investments.

Finally, the effects related to the poaching of workers from other companies concern about 27.8% of the jobs created (categories iii), iv) and v)). The share of those coming from other companies in the same LLM is 15.3% (category iii)). These are upper-bound estimates as workers might have changed jobs both because of better working conditions (e.g., higher wages) or because the previous employer has reduced the workforce. Further, as we will show in Section 5, only 0.39% of the treated firms' workforce in 2001 comes from firms that applied for the incentives but were not financed because they scored too low in the L488 ranking. Overall, the results indicate that capital subsidies have not pushed incentivized companies to ‘steal’ a sizable share of workers from unincentivized firms. It is crucial to note that although the estimate regarding those who were receiving unemployment benefits is only a proxy for the impact on local unemployment, the limited extent of such coefficient suggests that the impact on local unemployment is more limited than that found in other place-based policies (see, for instance, Criscuolo et al., 2019).

In Online Appendix B, we add to the employment analysis by investigating whether capital subsidies induced subsidized firms to transfer part of the subsidy benefits to their employees. We find that capital subsidies increased the average skill endowment of subsidized firms, and this was achieved by keeping the most experienced workers as well as attracting the local young individuals with the most up-to-date skills.

4.2 Impact on other variables and medium-term impact estimates

In this sub-section, we investigate whether L488 affected other aspects of treated companies such as the average employees’ age, the share of women, the share of blue-collar, the share of part-time workers and the probability of survival. Table 3 reports the estimates concerning such outcome variables. While there appears to be no significant impact of the subsidy in the employment composition concerning the share of women, blue-collars, and part-time workers, the subsidized plants’ workforce in 2001 is about 1.3 years older than the counterfactual scenario. This finding is in line with the evidence reported in the previous section and in Online Appendix B, where we showed that subsidized firms kept their most skilled/experienced employees (with higher wage) and hired young people with more up-to-date skills (also compensated with a higher wage). In line with previous literature (see Pellegrini and Muccigrosso, 2017), we find that L488 subsidies increase the probability of plant survival.

INSERT TABLE 3

We then conduct a medium-term analysis to test whether the observed impact of the policy lasts over time. We look at the medium-term impact of such a policy using 2004 instead of 2001 as the reference year. The coefficients in Panel A of Table 4 confirm the long-lasting effects of the policy, even if they suggest that the extent of the employment impact diminishes after three years. Such a result was expected if we consider that the job creation criterion of the assignment process has probably induced firms to overshoot the optimal amount of employment to gain a subsidy. Besides, it is plausible that in the long-term, firms start to reduce the inflated employment and increase allocative efficiency (Bernini et al., 2017).

INSERT TABLE 4

More importantly, for our core research question, the disaggregated estimates reported in Panel B of Table 6 confirm the labor mobility effects of the L488 policy. The medium-term analysis shows that there is significant mobility from other LLMs, and the workers coming from outside remain in the subsidized companies also after several years.

5. Robustness and sensitivity checks

At the beginning of this section, we carry out two robustness checks. The estimates are reported in Tables 5. We first check whether the inclusion of firms applying in Auction 1 affected the main estimates. The exclusion of the 1st auction is motivated by the inclusion of a transitory clause limited to that auction, which allowed some firms not eligible under L488 to be financed as well (see Bronzini and de Blasio, 2006). As shown in column (1) of Table 5, we find a smaller impact on employment. Such finding is expected as the average subsidy intensity in Auction 1 was over 30% higher than in the following three auctions. We then check whether the addition of the 51 large firms (having 250+ employees in 1995) affects our main estimates. We excluded such firms from the main analysis because of three reasons: i) the way we define the employment dependent variable, i.e., as the difference in the number of employees in 2001; ii) the evidence that larger firms could take the subsidy without changing their level of economic activity (Criscuolo et al., 2019); iii) the fact that large treated firms received €6.26 million on average, while the rest of treated companies received €0.61 million on average. The overall employment estimates reported in column (2) of Table 5 are about 20% larger than those reported in Table 2. This result is not surprising given the extent of the subsidy intensity for large firms and the fact that 45 out of 51 of them were subsidized (see Table A2 in Online Appendix A). Looking at the disaggregated employment estimates, we notice a substantially larger impact for those who were working in 1995 for the subsidized plant (2.8 additional employees).

INSERT TABLE 5

We then check the robustness of the non-parametric estimates with respect to the kernel and the bandwidth selector. The estimates are in Table 6. Column (1) reports the estimates obtained using two different MSE-optimal bandwidth selectors (below and above the cutoff), while the estimates in

Column (2) are made with the Epanechnikov kernel weights. Overall, they are in line with those reported in Table 2. In addition, in Online Appendix C we report the estimates obtained via the adoption of the pooling approach with the addition of sector and ranking dummies. They confirm the robustness of our findings.

Lastly, we investigate the potential presence of cross-sectional substitution, i.e., whether subsidized firms took some of the investment opportunities that unsubsidized firms would have exploited in the absence of the policy (Klette et al., 2000). In the presence of cross-sectional substitution, subsidized investments partially crowd-out non-subsidized investments and employment, making the rationale in favor of the place-based program less clear. Although it is not possible to test such a hypothesis in a partial equilibrium framework as the one we are using, for the first-time our database allows checking whether subsidized firms attracted workers from non-subsidized firms. As reported in Table 7, more workers moved from a non-subsidized plant to a subsidized one than the other way around; however, only 0.39% of the treated plants' workforce in 2001 comes from companies that applied for the incentives but were not financed because they scored too low in the L488 ranking. This finding suggests that previous literature has probably overrated the empirical impact of the stealing workers effect.

6. Concluding remarks

The effectiveness of place-based policies in terms of employment depends, among others, on three specific factors: the ability to attract new jobs from within the destination area, that the new job places are not subtracted from existing non-subsidized businesses in the same area, and the capacity to keep the job positions already present before the policy. The first effect concerns the presence of leakages or the spatial dispersion of the effects; the second is the possibility that subsidized companies might poach workers from non-subsidized companies in the LLM, reducing the net effect of incentives on the territory. The third effect considers those workers who, due to the intervention, have managed to keep their jobs. Although these effects are highlighted in the literature, few studies evaluate their empirical relevance given the lack of comprehensive data and the difficulties in tackling endogeneity issues. The use of administrative archives, linked with the characteristics related to the allocation of the L488 incentives, allowed us to overcome both problems and give an empirical answer to the magnitude of the local effect of the place-based policy.

The results show how leakages and substitution effects are present, but their impact is relatively limited. The majority of recruits come first from new entrants to the labor market, in particular, young people and students, and therefore the 'stealing effects' count for only 27.8% of the new employment generated by the policy. Taken together, the size of categories “unemployed”, “non-employed born in the same LLM” and those “working in the same plant who, due to the intervention, have kept their jobs”, count for more than half of the additional effects. From our point of view, this result confirms the positive standpoint regarding the subsidy scheme. Moreover, the share of workers coming from the same labor market is also relatively large, even if a non-negligible share comes from non-contiguous LLM, which obtains higher wages. This share is lower than in a previous analysis of the UK enterprise zones, where between 50 and 80 percent of enterprise zone businesses had relocated into the zones from outside (Papke, 1993; Neumark and Simpson, 2015) and differs from the results of Freedman (2015, p. 2), who states: “many of the new jobs created in areas that receive subsidized investment do not go to residents of targeted neighborhoods”.

The conclusions are that the local effects are largely predominant, with reduced leakage to the outside, and that the effects of spatial crowding out concerning workers from non-subsidized companies in the same LLM or neighboring LLMs appears limited. Two important reasons can explain these differences with previous literature (see Slattery and Zidar, 2020): first, the geographic mobility of the workers is much higher in the US and the UK than in the south of Italy, where family support, the existence of an informal economy that still guarantees employment opportunities, albeit low income, and administrative costs of mobility discourages personal mobility (see Faini, 1999). Second, in the areas under analysis, the degree of utilization of resources, and also of the labor factor, remains low, determining a large pool of skilled and unskilled people available to work.

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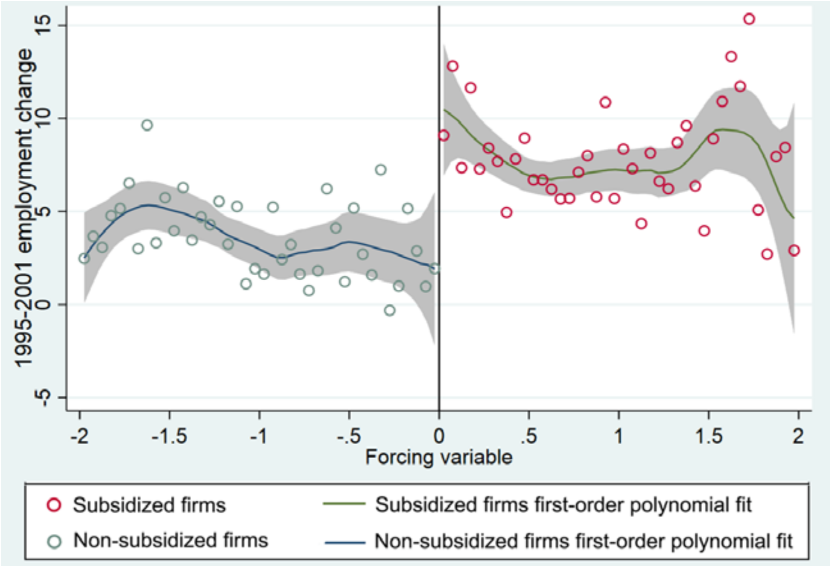
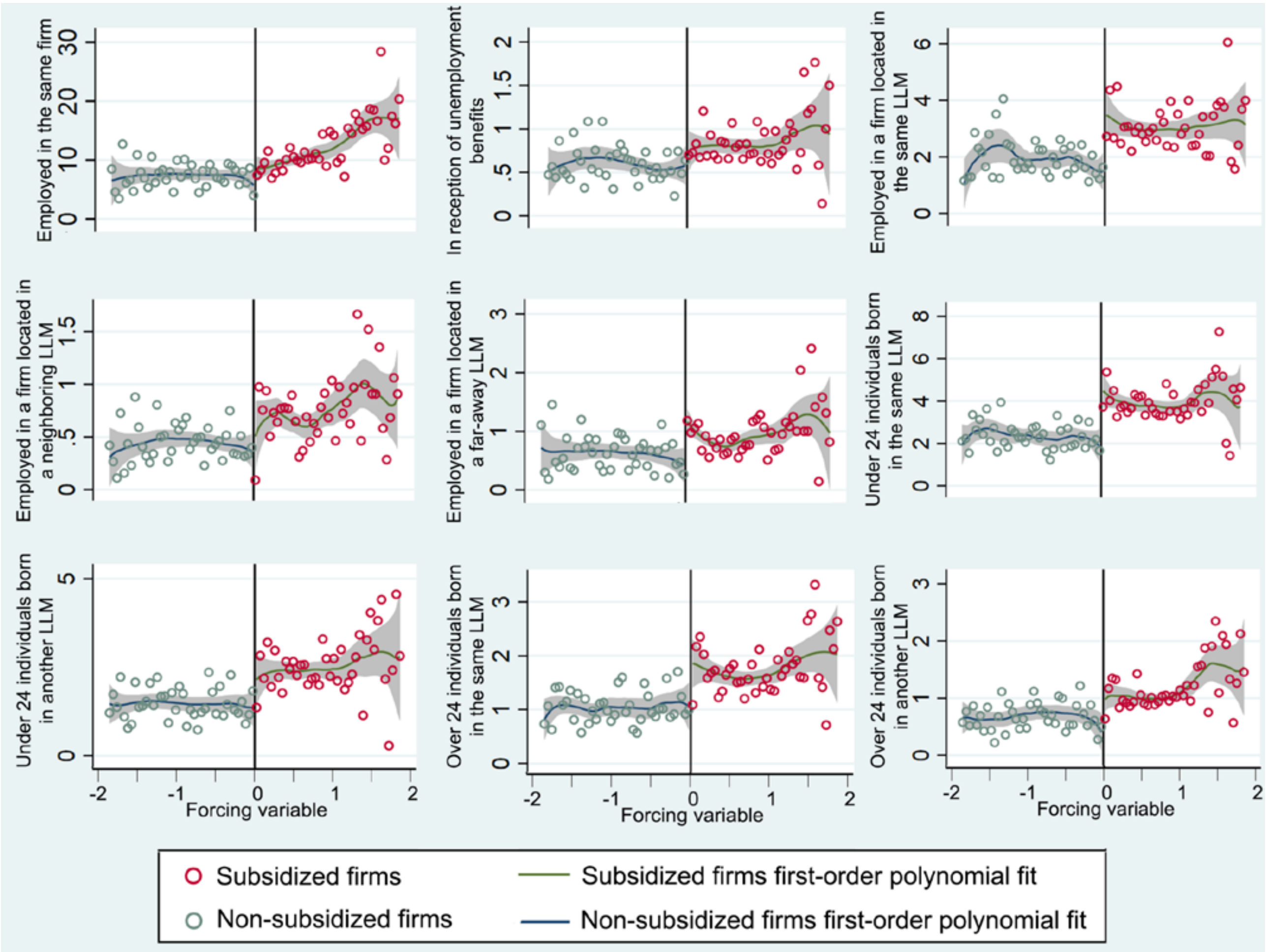
FIGURES AND TABLES

Figure 1 – Employment differences at the discontinuity between subsidized and non-subsidized firms in 2001

Panel A – Overall impact



Panel B – Estimates disaggregated by working situation in 1995



Notes: The dots are bin averages (bin width = 0.05). The solid line represents a first-order polynomial regression. The shaded area represents 95% confidence intervals.

Table 1 – Pre-treatment differences in 1995

	Average values in the whole sample		Average values in the closest 25% of the sample			Differences at the threshold
Variable	**Control**	**Treated**	**Control**	**Treated**		
	(1)	(2)	(3)	(4)		(5)
Number of employees	14.86	21.48	15.67	16.14	*Coeff. (SE)*	0.29 (3.19)
					N -/+	866/1,255
The yearly wage in €	9,476	9,708	9,681	9,121	*Coeff. (SE)*	-675 (468)
					N -/+	1,197/1,614
Share of blue-collar employees	76.22%	77.51%	78.78%	77.92%	*Coeff. (SE)*	0.10 (2.70)
					N -/+	1,131/1,556
Share of part-time employees	3.02%	2.28%	2.08%	2.15%	*Coeff. (SE)*	-0.13 (1.46)
					N -/+	1,078/1,509
Share of female workers	19.63	22.23	21.00	21.33	*Coeff. (SE)*	2.01 (3.03)
					N -/+	1,057/1,490
Share of workers born in the Center-North of Italy	3.58	2.72	3.03	2.71	*Coeff. (SE)*	-0.05 (1.31)
					N -/+	1,290/1,714
Firm age (with respect to the INPS registration)	9.11	10.22	8.50	8.51	*Coeff. (SE)*	-1.18 (1.65)
					N -/+	420/639
Employees age	32.23	32.16	32.17	31.91	*Coeff. (SE)*	0.23 (0.80)
					N -/+	975/1,399

Notes: The non-parametric robust bias-corrected estimates in Column (5) have been obtained via the multi-cutoff RDD proposed by Cattaneo et al. (2020b). . N^{-} and N^{+} denote the number of cases within the bandwidths below and above the thresholds, respectively. ***p<0.01, **p<0.05, *p<0.1.

Table 2 – Employment estimates

PANEL A		
Overall estimate: 1995-2001 employment change	*Coeff. (SE)*	8.59*** (1.96)
	N $^{-}/^{+}$	648/983
PANEL B		
Disaggregated estimates: Number of employees in 2001 by employment condition in 1995		
Working in the same plant	*Coeff. (SE)*	1.48** (0.73)
	N $^{-}/^{+}$	1,121/1,540
Unemployed	*Coeff. (SE)*	0.26* (0.15)
	N $^{-}/^{+}$	764/1,147
Working in the same LLM (but a different plant)	*Coeff. (SE)*	1.38*** (0.49)
	N $^{-}/^{+}$	892/1,284
Working in a neighboring LLM	*Coeff. (SE)*	0.44*** (0.15)
	N $^{-}/^{+}$	735/1,113
Working in a non-neighboring LLM	*Coeff. (SE)*	0.69*** (0.21)
	N $^{-}/^{+}$	635/971
Non-employed: Under 24 born in the same LLM	*Coeff. (SE)*	2.01*** (0.53)
	N $^{-}/^{+}$	828/1,221
Non-employed: Under 24 born in another LLM	*Coeff. (SE)*	1.18*** (0.38)
	N $^{-}/^{+}$	875/1,263
Non-employed: Over 24 born in the same LLM	*Coeff. (SE)*	0.86*** (0.30)
	N $^{-}/^{+}$	733/1,108
Non-employed: Over 24 born in another LLM	*Coeff. (SE)*	0.72*** (0.19)
	N $^{-}/^{+}$	730/1,096

Notes: All non-parametric estimates are robust bias-corrected (Calonico et al., 2019) and have been obtained via the multi-cutoff RDD (Cattaneo et al., 2020b). The control variables included are the number of employees in 1995 and the average wage in 1995. We considered full-time as well as part-time employees. ***p<0.01, **p<0.05, *p<0.1.

Table 3 – Subsidy impact on other firm outcomes

Outcome variable	**Sample average**		
Average age in 2001	34.79	*Coeff. (SE)*	1.28** (0.57)
		N $^{-/+}$	1,107/1,530
Share of women in 2001	31.53%	*Coeff. (SE)*	-3.82 (3.08)
		N $^{-/+}$	628/1,084
Share of blue collars in 2001	74.70%	*Coeff. (SE)*	0.36 (3.02)
		N $^{-/+}$	920/1,310
Share of part-time workers in 2001	4.00%	*Coeff. (SE)*	-0.55 (1.09)
		N $^{-/+}$	1,156/1,574
Plant survival in 2001	89.22%	*Coeff. (SE)*	7.53** (3.86)
		N $^{-/+}$	1,038/1,364

Notes: See Table 2.

Table 4 – Medium-term impact on employment and wages

		(1)	(2)
PANEL A			
		Overall estimate: 1995-2004 employment change	**Outcome variable:** Wage differential in 2004
	Coeff. (SE)	6.34** (2.43)	7.14 (4.45)
	N $^{-/+}$	553/888	576/883
PANEL B			
		Disaggregated estimates: Number of employees in 2004 by employment condition in 1995	**Disaggregated estimates:** Wage differential in 2004 by employment condition in 1995
Working in the same plant	*Coeff. (SE)*	1.17 (0.86)	9.99*** (3.86)
	N $^{-/+}$	729/1,132	485/767
Unemployed	*Coeff. (SE)*	0.09 (0.17)	2.11 (6.27)
	N $^{-/+}$	623/991	195/395
Working in the same LLM (but a different plant)	*Coeff. (SE)*	2.15*** (0.68)	-0.89 (4.51)
	N $^{-/+}$	536/869	331/656
Working in a neighboring LLM	*Coeff. (SE)*	0.66*** (0.22)	-0.08 (6.64)
	N $^{-/+}$	460/759	187/369
Working in a non-neighboring LLM	*Coeff. (SE)*	0.39 (0.25)	6.77 (8.39)
	N $^{-/+}$	563/895	184/409
Non-employed: Under 24 born in the same LLM	*Coeff. (SE)*	1.16* (0.68)	6.63* (3.87)
	N $^{-/+}$	759/1,163	522/928
Non-employed: Under 24 born in another LLM	*Coeff. (SE)*	1.05* (0.54)	8.54* (5.61)
	N $^{-/+}$	818/1,170	344/688
Non-employed: Over 24 born in the same LLM	*Coeff. (SE)*	0.72** (0.35)	1.20 (4.64)
	N $^{-/+}$	634/1,013	455/881
Non-employed: Over 24 born in another LLM	*Coeff. (SE)*	0.79*** (0.23)	1.58 (6.75)
	N $^{-/+}$	618/979	240/468

Notes: See Table 2.

Table 5 – Robustness tests for employment estimates

		No Auction 1	Addition of large firms
		(1)	(2)
PANEL A			
Overall estimate: 1995-2001 employment change	*Coeff. (SE)*	6.38** (2.67)	10.04*** (2.05)
	N $^{-/+}$	592/744	619/952
PANEL B			
Disaggregated estimates: Number of employees in 2001 by employment condition in 1995			
Working in the same plant	*Coeff. (SE)*	0.78 (0.72)	2.82* (1.71)
	N $^{-/+}$	911/996	698/1,045
Unemployed	*Coeff. (SE)*	0.04 (0.18)	0.28 (0.18)
	N $^{-/+}$	643/783	961/1,436
Working in the same LLM (but a different plant)	*Coeff. (SE)*	1.47*** (0.57)	2.43*** (0.62)
	N $^{-/+}$	627/774	731/1,103
Working in a neighboring LLM	*Coeff. (SE)*	0.48*** (0.17)	0.50*** (0.19)
	N $^{-/+}$	870/959	683/1,029
Working in a non-neighboring LLM	*Coeff. (SE)*	0.71** (0.30)	0.83*** (0.30)
	N $^{-/+}$	525/674	542/855
Non-employed: Under 24 born in the same LLM	*Coeff. (SE)*	1.44** (0.63)	2.19*** (0.60)
	N $^{-/+}$	661/801	816/1,208
Non-employed: Under 24 born in another LLM	*Coeff. (SE)*	0.75 (0.46)	1.43*** (0.46)
	N $^{-/+}$	661/804	736/1,117
Non-employed: Over 24 born in the same LLM	*Coeff. (SE)*	0.86** (0.34)	1.14*** (0.33)
	N $^{-/+}$	662/808	722/1,087
Non-employed: Over 24 born in another LLM	*Coeff. (SE)*	0.54** (0.23)	0.84*** (0.24)
	N $^{-/+}$	747/866	640/980

Notes: See Table 2.

Table 6 – Additional robustness tests for employment estimates

		Two different MSE-optimal bandwidth selectors	***Epanechnikov kernel***
		(1)	(2)
PANEL A			
Overall estimate: 1995-2001 employment change	*Coeff. (SE)*	8.54*** (1.85)	8.42*** (1.93)
	N $^{-/+}$	737/1,071	623/956
PANEL B			
Disaggregated estimates: Number of employees in 2001 by employment condition in 1995			
Working in the same firm	*Coeff. (SE)*	1.40* (0.75)	1.34* (0.73)
	N $^{-/+}$	1,198/1,276	1,032/1,458
Unemployed	*Coeff. (SE)*	0.26* (0.15)	0.28* (0.15)
	N $^{-/+}$	770/1,277	794/1,175
Working in the same LLM (but a different firm)	*Coeff. (SE)*	1.43*** (0.39)	1.55*** (0.48)
	N $^{-/+}$	988/1,787	870/1,257
Working in a neighboring LLM	*Coeff. (SE)*	0.43*** (0.15)	0.43*** (0.16)
	N $^{-/+}$	962/1,076	708/1,068
Working in a non-neighboring LLM	*Coeff. (SE)*	0.63*** (0.21)	0.67*** (0.22)
	N $^{-/+}$	961/991	612/935
Non-employed: Under 24 born in the same LLM	*Coeff. (SE)*	1.84*** (0.48)	1.91*** (0.52)
	N $^{-/+}$	843/1,388	811/1,197
Non-employed: Under 24 born in another LLM	*Coeff. (SE)*	1.02*** (0.35)	1.11*** (0.38)
	N $^{-/+}$	931/1,441	828/1,221
Non-employed: Over 24 born in the same LLM	*Coeff. (SE)*	1.01*** (0.29)	1.01*** (0.30)
	N $^{-/+}$	833/1,098	721/1,084
Non-employed: Over 24 born in another LLM	*Coeff. (SE)*	0.71*** (0.19)	0.70*** (0.19)
	N $^{-/+}$	789/1,164	693/1,032

Notes: See Table 2.

Table 7 – Job switches between treated and non-treated firms

	Non treated firms in 2001	Treated firms in 2001
Non treated firms in 1995	212	399
Treated firms in 1995	581	1,175

Notes: The cumulative number of workers in treated and non-treated firms in 2001 was 103,541 (34,803 of them in untreated firms, while 68,738 workers were in treated firms).