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Do local subsidies to firms create jobs? Counterfactual evaluation of an Italian regional experience

by Giuseppe Porro and Valentina Salis

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ABSTRACT

The paper evaluates the impact on employment dynamics of four programs implemented in Lombardy (Italy) and providing financial incentives to firms on the period 2008-2013.

A counterfactual analysis is performed, via coarsened exact matching, that allows for multi treatment cases, distinguishing the effects of single programs and taking into account different possible levels of incentives.

Linear estimates suggest that the programs have a positive effect on short-run employment growth only when the amount of financial incentives is sufficiently high; moreover, the efficacy seems to increase when the incentives take the form of interest-rate subsidies, instead of capital grants. A non-linear representation of the policy effects, on the contrary, shows that the result of public intervention is, on the whole, negative or non-significant, and that the positive estimates in the linear models are due to small groups of outliers, which received very high subsidies. Thanks to this new methodological tool, the results of the vast majority of the literature about the impact of public subsidies on job creation are confirmed and strengthened.

Data allow for a short-run analysis only, but the results cast some doubts on the effectiveness of the programs also in a medium-long run perspective.

Keywords: REGIONAL POLICIES; FIRM INCENTIVES; JOB CREATION; COUNTERFACTUAL ANALYSIS.

JEL Classification: R50; H71; C14.

Do local subsidies to firms create jobs? Counterfactual evaluation of an Italian regional experience[ϕ]

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1. Introduction

A considerable effort has been devoted, in recent years, to the analysis of the impact of financial incentives and subsidies on the economic performance of recipient firms.

The studies have provided a twofold contribution, both theoretical and methodological. On the one hand, they have shed some light on the relationship between these public policies and the productivity and competitiveness of firms (Harris and Trainor 2005; Bergström 2000; Lee 1996); on the other hand, they have been an interesting experimental field for the development and application of counterfactual techniques: it is a quite common approach, in fact, to estimate the effect of financial subsidies comparing the performance of recipient firms to the one of firms whose applications to the policy was rejected or, in any case, of firms that did not benefit from any public intervention.

It has to be noted that the evidence provided about the efficacy of these incentives and subsidies in terms of productivity of firms is fairly mixed: Harris and Trainor (2005) estimate a positive effect on total factor productivity (TFP), while Bergstrom (2000) finds an impact on growth but no significant effect on TFP; Lee (1996), on the other hand, estimate a negative effect on productivity.

Less attention has been paid to the evaluation of the impact of the policies on the employment levels (Hamermesh 1993; Faulk 2002; Gabe and Kraybill 2002; for a detailed discussion of recent contributions, see section 5). In fact, an increase in productivity or competitiveness would yield higher growth rates of firms and, at least in the medium run, an advantage in terms of employment. Nevertheless, it cannot be excluded that public subsidies might generate employment effects in the short run,

We wish to thank the participants to the 26[th]AISRe (*Italian Association of Regional Science*) National Conference (Arcavacata di Rende, 14-16 September 2015) for helpful comments. All remaining errors are our own.

despite its relative inefficacy on firm productivity due, for instance, to time or cross-sectional substitution effects (Lee 1996; Adda and Cooper 2000; Klette *et al.* 2000).

The question becomes more interesting since the assignment of financial resources to firms has progressively assumed the form of local public policies, where these substitution effects might be more significant. The decentralization of policymaking is a part in a well known process of subsidiarity and devolution in many EU countries and in the US (Bardhan 2002); at the same time, local policies are traditionally conceived as a measure for reducing territorial disparities or supporting industrial clusters and districts (Martin *et al.* 2011)[1].

In this paper we aim at contributing both to the impact analysis of local financial policies on firms' performance and to the implementation of adequate methodological tools. In particular, we examine the effect of four regional programs (three of them are financed by EU funds) implemented by local authorities in Lombardy – a Northern Italy region - in the 2008-2013 period. It will be shown that, despite the unclear relationship between subsidies and firms' earnings, a short-run pattern relating incentives and employment seems to emerge, at first sight, from linear models: public intervention does not have a positive impact on employment growth, unless the amount of financial aid is high enough. The efficacy increases when the incentive does not take the form of capital grants. A closer examination - using nonparametric methods and representing the policy result as a continuous function of the subsidy amount - reveals that the policy is, on the whole, ineffective and that the positive effects shown by linear models are due to a few outliers, i.e. to a small group of firms which received a very high amount of subsidy. The period analysed only allows for short-run results, but some comments can be proposed also in a medium-long run perspective.

To carry out the counterfactual analysis, we make use of the coarsened exact matching (CEM) technique (Iacus *et al.* 2011, 2012), that allows for the study of multitreatment policies, both as analysis of different programs and of the interactions among different programs and different resource levels.

The paper is structured as follows: the four programs are presented in the following section, while section 3 describes the dataset and variables. Section 4 contains the evaluation of the policy effects and also illustrates the adopted methodology. The results of the analysis are discussed in section 5, and section 6 concludes.

[1]See Brancati (2015) and the previous annual reports for a systematic survey of industrial policies in Italy.

2. Four regional programs in Lombardy (2008-2013)

The local authorities in Lombardy give support to the growth of productive investment providing - among other interventions - financial subsidies to firms that make new investments. To this aim, the main instruments implemented by the local policymaker are regional programs funded by EU resources that mainly provide non-repayable grants; in addition, other regional measures are adopted, funded by local resources.
Our study is conducted on EU-funded regional programs and on regional measures implemented in Lombardy between 2008 and 2013. We have selected the actions to evaluate and the firms to examine according to the following criteria:

— programs and measures analyzed in this study have the explicit goal to improve competitiveness and survivability of firms, supporting restructuring initiatives, re-organization, acquisition of new equipment. Programs providing R&D incentives have not been considered, as they have, in many cases, specific target beneficiaries (associations of firms, public research institutions) and specific goals (increase in R&D expenditure and employees, number of patent licences, export levels);
— in order to rule out possible spillover effects, firms with productive units located outside Lombardy have been dropped from the analysis: these firms might, in fact, have benefited from local programs other by those implemented by regional authorities in Lombardy;
— all the subsidized projects considered in the study started before the end of 2010 and were concluded within January 2013: this has left us a time interval large enough to monitor the short-run effects of the policy. On the other hand, this constraint has excluded from the analysis the "Start-up and Re-start" program, devoted to start-up and firms' spin-off, whose projects were not concluded within the beginning of 2013.

Consequently, the programs we have analyzed are the following:

— *Feasr*[2]: Measure "Modernizing rural firms", Regional Program of Rural Development 2007-2013. Potential beneficiaries are rural firms in Lombardy. The program aims at promoting competitiveness of rural sector and a sustainable local development. Employment is included among the aims. The program is financed by EU FEASR funds for rural policies. Around 213 mln € have been assigned by the end of 2013.

[2] *Programma di sviluppo rurale 2007-2013* – Misura 121 "Ammodernamento delle aziende agricole".

— *Docup* [3] : Measures "Incentives to investment", "Incentives to modernizing and retraining of tourism firms", "Support to creation of new firms", Single Programming Document 2000-2006 (funds have been assigned up to 2010). Potential beneficiaries of the measures – aiming at improving the competitiveness of the local economic systems – are small and medium-sized enterprises (SMEs), with particular attention to tourism activities. The program is financed by EU FESR funds. The three measures have assigned around 89 mln € by the end of 2013.
— *Frim* [4] : Action line "Business development", Revolving Fund for Enterpreneurship. Potential beneficiaries are SMEs: the aim of the intervention is improving competitiveness on the internal and international markets, supporting modernization and business expansion. The fund is financed only by regional resources. More than 50 mln € have been assigned by the end of 2013.
— *Fesr*[5]: Action "Product and process innovation in Lombary SMEs", Regional operative program FESR 2007-2013. Potential beneficiaries are SMEs and large firms in partnership with SMEs: the action aims at supporting competitiveness, improving firms' know-how and knowledge management. The program is financed by EU FESR funds. More than 44 mln € have been assigned through the action by the end of 2013.

The resources assigned by *Feasr*, *Docup* and *Fesr* are *capital grants*[6], while *Frim* provides *interest-rate subsidies*[7]: one of the aims of the analysis is to test whether the policy effect depends, to some extent, on the funding method.

3. Data and variables

The data archives and the execution reports prepared by the Lombardy local government for each intervention provide information about applicant and recipient firms. The recipient firms whose subsidized

[3]*Documento Unico di Programmazione Obiettivo 2 Lombardia 2000-2006* – Misura 1.1 "Incentivi agli investimenti alle imprese"; Misura 1.3 "Incentivi all'ammodernamento e riqualificazione delle aziende ricettive"; Misura 1.5 "Sostegno alla creazione di nuove imprese".

[4]*Fondo di rotazione per l'imprenditorialità (FRIM)* - Linea di intervento 1 "Sviluppo aziendale".

[5]*Programma operativo regionale FESR 2007-2013* – Azione 1.1.1.1 C "Innovazione di prodotto e processo delle PMI lombarde".

[6] Non-repayable funds granted as a proportion of the eligible investment expenditure.

[7]Resources offered to offset a percentage of the interest-rate costs related to loans from private financial institutions.

projects were concluded within January 2013 have been considered for the analysis. A few filters have been applied: *a)* firms that received subsidies from more than one programs have been dropped, in order to avoid overlapping effects; *b)* firms with productive units located outside Lombardy have been excluded, in order to avoid possible external spillover effects; *c)* firms that turn out to be no longer active in 2014 have been dropped.

The number of recipient firms examined is 1,359: among them, 1,142 took part in *Feasr* program, 55 in *Docup*, 70 in *Fesr* and 92 in *Frim*.

The dataset SMAIL[8], managed by Unioncamere Lombardia[9] is the source of information for non-recipient firms. All the firms with productive units in Lombardy that are registered and active in 2014 have been considered. Firms registered after 2008 have been excluded; firms that received any kind of public subsidy in the period of analysis have been excluded; firms with missing data have been dropped. The number of firms that are used as control units in the analysis is 502,835.

The outcome variable is the employment change between 2008 and 2014:

$$EMP_{VAR} = EMP_{14} - EMP_{08}$$

The following features of the firms are used as matching variables or as control variables[10]: *FORM*: legal form; *DIM*: size, based on the number of employees; *PROV*: province; *CODE*: sector, based on Ateco classification codes of economic activity.

FinContrib is the amount of public financial resources received by the firm.

4. Impact evaluation

In the first part of the section the counterfactual analysis will be carried out comparing the firms supported by the programs to all potential recipients, i.e. all the firms included in the dataset, regardless of whether they applied to one of the programs or not. On one hand, this provides estimates of the policy impact which aim to be general in their validity, because do not impose any *ex ante* restriction to the magnitude and characteristics of the control group. On the other hand, this means that we will make use of treatment and control groups coming from different data sources and – as Heckman *et al.* (1997, 1998) point out – this may enlarge the effect of possible confounding unobservables. In particular, the application to the programs might imply some self-selective processes

[8] *Sistema di Monitoraggio Annuale delle Imprese e del Lavoro*.

[9] The Union of Chambers of Commerce in Lombardy.

[10] See the Appendix for details.

that can make applicant firms different, to some extent, from non-applicant ones: for this reason, at the end of the section we will try to estimate the significance of possible biases induced by unobservable features of applicant firms.

4.1 Binary treatment

A first attempt to evaluate the impact of the programs makes use of a binary treatment model: all the firms that applied to one of the programs and were granted financial resources are considered "treated", irrespectively of the program they participate. In other words, we estimate the following model:

$$EMP_{VAR_i} = \alpha + \beta TREAT_i + \gamma EMP_{08_i} + \delta FORM_i + \varepsilon_i \qquad i = T, C \qquad (1)$$

where *TREAT* is a dummy variable indicating the recipient firms and parameter β is an estimate of the policy impact (see Table 1).

In Table 1 the OLS estimates are reported (column a): what we get is that the policy effect is positive but weakly significant.

In this first regression we are comparing 1,359 recipient firms to more than 500,000 firms in the control group, and the distribution of covariates values is quite different between treated and untreated firms (see Table A1 in the Appendix).

In order to compare more balanced groups, we pre-process our data through the Coarsened Exact Matching (CEM) algorithm (Iacus *el al.* 2011, 2012). CEM is a matching procedure that finds balanced groups of treated and untreated units by coarsening the original covariates' space: in other terms, CEM creates a partition of the covariates' space and considers as matched (i.e. similar according to the covariates) the treated and untreated units that fall into the same stratum of the partition. Units that fall into strata where only treated or only untreated units can be found are considered out of the common support and discarded from the subsequent analysis. After matching, any statistical model can be applied on matched units, to control for the residual imbalance between treated and untreated units or to take into account covariates not included in the matching step.

In our exercise, treated and untreated firms are matched on the following covariates: *DIM*, *PROV*, *CODE*. All the matching covariates are discrete and the control units set is large enough to allow for an exact matching on all the covariates. Therefore, we obtain two sets of treated and control firms perfectly balanced, at the cost of dropping from the analysis only 15 treated firms, which fall out of the common support. The untreated firms that find a match are 48,640.

We re-estimate model (1) on matched data (Table 1, column b): now the estimated policy effect is negative and significant. This seems to

exclude that a so called "*picking the winners effect*"[11] occurs: in fact, if a comparison with all potential recipient firms may suggest a weakly positive impact, the after-matching effect becomes significantly negative, showing that firms which do not receive funds have, on average, a better employment performance. On the whole, at this stage of the analysis, the local policy appears ineffective.

Table 1 – *Employment growth (2008-2013), binary treatment, OLS estimate*

	(a)		(b)		(c)	
(Intercept)	0,4534	***	11,4596	***	14,2566	***
	(51,016)		(74,464)		(29,820)	
TREAT	0,1636	*	–1,7299	***	–1,7462	***
	(1,990)		(–4,907)		(–4,955)	
EMP08	–0,0005	***	–0,1303	***	–0,1300	***
	(–7,047)		(–33,806)		(–33,736)	
FORM consortium	–0,2562	**	0,4599		–0,2276	
	(–2,934)		(0,556)		(–0,273)	
FORM co-operative	0,6823	***	0,0015		–0,4491	
	(15,544)		(0,003)		(–0,818)	
FORM public entity	0,6629	***	–7,2206	*	–6,1665	.
	(5,324)		(–1,963)		(–1,675)	
FORM individual	–0,4133	***	–10,5715	***	–10,8042	***
	(–38,913)		(–63,080)		(–62,922)	
FORM partnership	–0,3939	***	–9,8834	***	–10,4194	***
	(–31,081)		(–49,961)		(–48,253)	
Inverse Mills ratio					–0,9665	***
					(–6.179)	

Legenda:
Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
(a) without match;
(b) with match;
(c) with match and inverse Mills ratio.
"FORM corporation" is the baseline for variable FORM.

4.2 The four programs

To investigate the possible different effectiveness of the four programs implemented, a four treatment version of model (1) is estimated:

$$EMP_{VAR_i} = \alpha + \sum_{j=1}^{4} \beta_j \, PROG_{ij} + \gamma EMP_{08_i} + \delta FORM_i + \varepsilon_i \qquad i = T, C \quad (2)$$

where $PROG_j$ is a dummy variable indicating recipient firms participating in the *j-th* program. A substantial ineffectiveness of the policies seems to be proved: on the unmatched data, the only positive

[11]It happens when the policymaker assigns the resources to the best performance firms, so that a positive impact on performance has to be read as a joint result of the policy and the selection bias. For an example of *"picking the winners effect"* in a firms' subsidization policy on the Italian agro-food sector, see Pavone *et al.* (2015).

(and significant) impact is showed by *Fesr*, while all the other programs have non-significant effects (Table 2, column a). After matching, on the contrary, the evidence confirms the results obtained from model (1): all the programs exhibit negative parameters, though with different significance levels (Table 2, column b).

Table 2 – *Employment growth (2008-2013), four programs, OLS estimate*

	(a)		(b)		(c)		(d)		(e)	
(Intercept)	*0,4528*	***	*11,4920*	***	*14,2067*	***	*1,4275*	***	*2,7576*	***
	(50,930)		(74,522)		(29,698)		(25,848)		(12,327)	
PROG Feasr	*0,0961*		*-1,2408*	**	*-1,3107*	***	*0,5609*	*	*0,3892*	
	(1,072)		(-3,231)		(-3,412)		(2,376)		(1,638)	
PROG Docup	*-0,3655*		*-4,1894*	*	*-3,9587*	*	*-0,6191*		*-0,6325*	
	(-0,896)		(-2,423)		(-2,290)		(-1,239)		(-1,266)	
PROG Fesr	*1,5920*	***	*-3,4354*	*	*-3,2071*	*	*2,3663*	***	*2,3910*	***
	(4,404)		(-2,254)		(-2,104)		(6,735)		(6,809)	
PROG Frim	*0,2292*		*-4,9934*	***	*-4,6751*	***	*1,2107*	***	*1,2733*	***
	(0,727)		(-3,768)		(-3,526)		(7,403)		(7,775)	
EMP08	*-0,0005*	***	*-0,1293*	***	*-0,1291*	***	*-0,0324*	***	*-0,0335*	***
	(-7,063)		(-33,422)		(-33,384)		(-8,732)		(-9,010)	
FORM consortium	*-0,2580*	**	*0,4505*		*-0,2169*		*-0,5766*		*-0,7111*	
	(-2,954)		(0,545)		(-0,260)		(-0,760)		(-0,938)	
FORM co-operative	*0,6831*	***	*-0,0880*		*-0,5136*		*-0,8355*	**	*-0,8551*	**
	(15,562)		(-0,162)		(-0,935)		(-2,906)		(-2,976)	
FORM public entity	*0,6591*	***	*-7,1504*	.	*-6,1431*	.	*-0,9256*		*-1,0576*	
	(5,294)		(-1,941)		(-1,666)		(-0,382)		(-0,437)	
FORM individual	*-0,4126*	***	*-10,613*	***	*-10,8345*	***	*-1,2962*	***	*-1,2116*	***
	(-38,828)		(-63,146)		(-62,980)		(-20,869)		(-19,054)	
FORM partnership	*-0,3930*	***	*-9,9398*	***	*-10,4544*	***	*-1,0819*	***	*-1,1349*	***
	(-30,998)		(-50,047)		(-48,333)		(-12,926)		(-13,496)	
Inverse Mills ratio					*-0,9393*	***			*-0,4117*	***
					(-5,995)				(-6,135)	

Legenda:
Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
(a): without matching;
(b): matching on binary treatment;
(c): matching on binary treatment, with inverse Mills ratio;
(d): matching on three-level treatment;
(e): matching on three-level treatment, with inverse Mills ratio;
"FORM corporation" is the baseline for variable FORM.

Being that the pre-matching distribution of covariates is very dissimilar between treated and untreated firms, the results presumably less affected by confounding variables are clearly the ones obtained after matching: therefore, the conclusion one may draw is that public

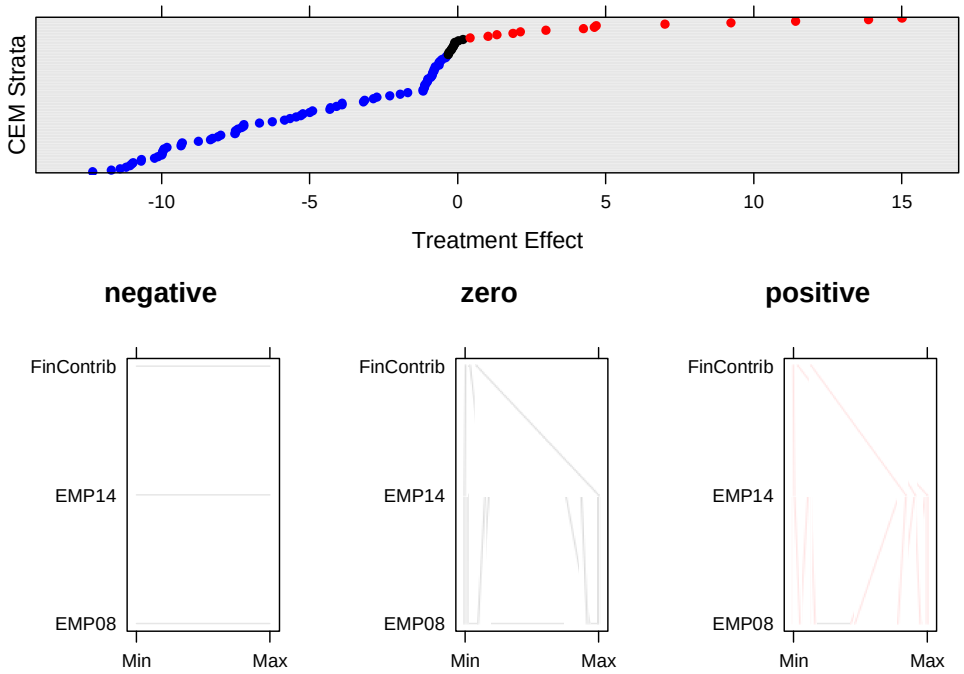
intervention does not yield any gain to recipient firms, at least in terms of short-term employment increase. If one ignores the bias induced by (observable) confounding factors, on the contrary, some positive effects of *Fesr* might be supposed, partially compensated by null or negative effects of the other programs, leading to an overall evaluation of weakly positive effect of the public intervention.

A deeper examination of the evidence, in our opinion, may lead to more thorough judgments and policy prescriptions.

4.3 A Plot of the average treatment effect: visual inspection

Figure 1 illustrates the distribution of the treatment effects estimated in model (2) with matched data, as a function of the joint distribution of covariates, described by the CEM strata (see Iacus *et al.* 2009 for details).

Figure 1 – *Linear regression model on CEM matched data*



Legenda: Upper part: distribution of treatment effect by CEM strata. Lower part: parallel plots, indicating the variables' values taken by treated firms with negative, null and positive treatment effects, respectively.

The treatment effect estimates, sorted in numerical order, are reported on the x axis, while the CEM strata are on the y axis: clearly, in the large majority of the strata a negative effect is estimated (blue dots). In order to extract information from CEM strata about the distribution of covariates and treatment variable, in the lower part of the figure some parallel plots show the variables' values of the units for each range of effect (negative, null, positive): each line represents the variables of a single observation.

A visual inspection of the parallel plots suggests that the firms in strata associated to positive treatment effect (red dots) received more frequently an amount of financial resources above the average (treatment variable *FinContrib*): this means that the subsidy level may play a role in determining the efficacy of the policy. To test the hypothesis, we estimate a multilevel treatment model (see Figure 1).

4.4 Multilevel treatment and the interaction model

Table 3 shows the distribution of subsidies according to their size: the range of the contributions is between € 414.6 and € 1,472,000, with an average value of € 107,500. We assume a threshold closed to the mean and classify the financial subsidy into two classes: low (< €100,000); high (≥ €100,000) and consider a three-level treatment: 0: no subsidy; A: low subsidy; B: high subsidy. Of the 1,359 recipient firms, 989 firms receive a low treatment, 370 firms receive a high treatment.

Table 3 – *Distribution of the amount of financial incentives (€)*

Min	1st Quartile	Median	Mean	3rd Quartile	Max
414,6	32,500	57,090	107,500	110,200	1,472,000

Let us now repeat the matching exercise: treated and untreated firms are matched on *DIM*, *PROV*, *CODE* with respect to the three-level treatment: again we get an exact match, but the common support changes: 52 low-treated and 78 high-treated firms are unmatched. Anyway, less then 10% of the treated units are dropped from the analysis. The control units are now 26,282.

First of all, we re-estimate model (2) after matching on the three-level treatment, and note that the sign of the policy effect reveals some changes: the parameters associated to the single programs (excluding *Docup*) are now positive and (for *Fesr* and *Frim*) strongly significant (see Table 2, column d).

The sharpest results are obtained when we take into account the interactions between the four programs and the treatment levels (low/high), estimating an eight-level model:

$$EMP_{VAR_i} = \alpha + \sum_{j=1}^{4}\sum_{k=1}^{2} \beta_{jk}(PROG_{ij})(LEV_k) + \gamma EMP_{0_{Bi}} + \delta FORM_i + \varepsilon_i$$

$$i = T, C \quad (3)$$

where LEV_k, (k=1,2) represents the two levels of subsidy received (low/high).

Table 4 describes the distribution of subsidized firms according to the programs and the subsidy level.

Table 4 – *Number of subsidized firms according to programs and subsidy level*

	A: low subsidy				B: high subsidy			
	Feasr	*Docup*	*Fesr*	*Frim*	*Feasr*	*Docup*	*Fesr*	*Frim*
No firms	913	37	7	32	229	18	63	60

Table 5 – *Employment growth (2008-2013), four programs, low/high subsidy level, OLS estimate*

	(a)		(b)		(c)	
(Intercept)	*0,4528*	***	*1,3748*	***	*2,7016*	***
	(50,928)		(25,027)		(12,157)	
A: PROG Feasr	*0,0780*		*-0,2810*		*-0,4675*	.
	(0,779)		(-1,019)		(-1,685)	
A: PROG Docup	*-0,3347*		*-0,5300*		*-0,5413*	
	(-0,673)		(-1,018)		(-1,040)	
A: PROG Fesr	*-0,2849*		*0,2610*		*0,2275*	
	(-0,249)		(0,107)		(0,093)	
A: PROG Frim	*0,1041*		*0,2446*		*0,3091*	.
	(0,195)		(1,436)		(1,811)	
B: PROG Feasr	*0,1682*		*2,6139*	***	*2,4799*	***
	(0,841)		(6,098)		(5,782)	
B: PROG Docup	*-0,4288*		*-1,0346*		*-1,0683*	
	(-0,601)		(-0,639)		(-0,660)	
B: PROG Fesr	*1,8010*	***	*2,4622*	***	*2,4880*	***
	(4,725)		(6,986)		(7,063)	
B: PROG Frim	*0,2959*		*10,4539*	***	*10,4971*	***
	(0,758)		(20,134)		(20,228)	
EMP08	*-0,0005*	***	*-0,0322*	***	*-0,0333*	***
	(-7,063)		(-8,713)		(-8,992)	
FORM consortium	*-0,2583*	**	*-0,5254*		*-0,6597*	
	(-2,958)		(-0,697)		(-0,876)	
FORM co-operative	*0,6833*	***	*-0,3628*		*-0,3748*	
	(15,564)		(-1,231)		(-1,273)	
FORM public entity	*0,6584*	***	*-0,9097*		*-1,0421*	
	(5,288)		(-0,378)		(-0,433)	
FORM individual	*-0,4126*	***	*-1,2275*	***	*-1,1433*	***
	(-38,825)		(-19,858)		(-18,070)	
FORM partnership	*-0,3930*	***	*-1,0336*	***	*-1,0865*	***
	(-30,998)		(-12,427)		(-13,003)	
Inverse Mills ratio					*-0,4107*	***
					(-6,161)	

Legenda:
Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
A: low subsidy
B: high subsidy
a) without match;
b) with match;
c) with match and inverse Mills ratio.
"FORMcorporation" is the baseline for variable FORM.

As Table 5 (column b) shows, the impact of a high subsidy is positive and significant for three of the four programs, while low subsidies yield

non-significant effect on employment variation[12]. In particular, *Frim* seems to be the most effective intervention: its impact is the highest (four times higher than the other significant effects) among the high subsidized firms.

It is only the case to point out the role of matching in letting a new pattern of policy effect emerge: if we estimate the eight-level multitreatment model before matching, all the coefficients related to the policy effects – with the exception of *high subsidy Fesr* – are non-significant, suggesting a substantial inefficacy of public subsidies (see Table 5, column a). In other words, comparing the subsidized firms to the most similar unsubsidized ones (that represent, anyhow, a bunch of more than 26 thousand controls), at the cost of discarding less than 10% of treated firms, allows to express a judgment on the policy effectiveness that is quite different from what one can obtain from the analysis of the raw data.

4.5 A graphical representation

To illustrate the results graphically and in a more detailed way, we plot the employment change *determined by the policy* as a function of the subsidy amount received by the firms. The CEM procedure allows to obtain a plot of the policy effect, net of the confounding observable covariates, as follows:

— Step 1: match firms according to binary treatment (subsidized/ unsubsidized) on all the covariates previously considered;
— Step 2: estimate, in each CEM stratum, the treatment effect (TE) as the difference of the average employment variation between treated and untreated firms;
— Step 3: impute TE to each treated firm in the stratum and plot TE as a function of the amount of subsidy received;
— Step 4: fit the scatterplot with a nonparametric regression (Racine and Li 2004; Li and Racine 2004).

In order to make the result comparable with the model estimated in the previous subsection, we should have matched on the same variables (*DIM*, *PROV*, *CODE*) and control for the imbalance of EMP_{0s} and *FORM* using them as regressors in the following linear model estimated inside each CEM stratum:

$$EMP_{VAR_i} = \alpha + \beta FinContrib_i + \gamma EMP_{0s_i} + \delta FORM_i + \varepsilon_i \qquad i = T, C$$

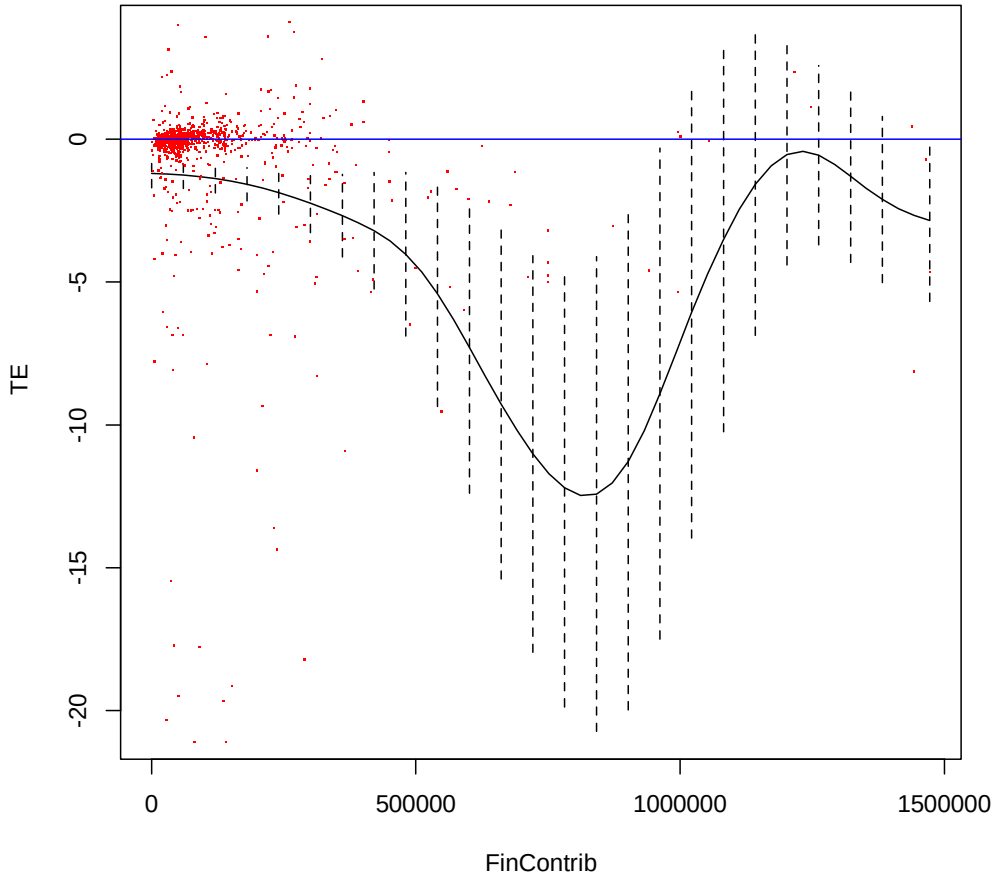
where *FinContrib* is the amount of subsidy.

Unfortunately, the limited variability of the categorical covariate *FORM* in some strata prevents from including this variable in the linear

[12]Program *Docup* has a negative but non-significant effect, due to the small number of firms (only 18) receiving high subsidies.

regression[13]. Therefore, at step 1 we match on four variables (*DIM*, *PROV*, *CODE*, *FORM*) and run the linear regression only on EMP_{08}. As a consequence, neither the matching nor the results we obtain are directly comparable with the ones we got in the previous subsection, despite the fact that 1,290 treated firms find a match in this exercise, and hence only 5.1% of treated units are discarded from the analysis (see Figure 2).

Figure 2 – *Treatment effect (TE) as a function of the amount of subsidies (FinContrib); nonparametric regression (dotted line: 95% non parametric confidence intervals)*



The pattern emerging from Figure 2 allows for a detailed interpretation of the policy effectiveness: the average effect of the public intervention is, in general, negative or non-significant. Small groups of outliers drive the trend of the nonlinear regression: in particular, a few cases of very high subsidies (over 1 million €) yield positive effect on employment growth and explain the results obtained by the multilevel linear model (3), reported in Table 5. This is not enough to make the policy effective on the whole.

[13]The categories of *FORM* are not all represented in each stratum: consequently, the linear regression cannot be run on *FORM*.

This plot has not to be misinterpreted as a continuous treatment effect function. Representing the effect of the subsidization policy as a continuum treatment is actually inhibited by the curse of dimensionality: in each stratum obtained by the CEM partition, in fact, we should observe all the possible treatment levels (zero subsidy included), in order to estimate a net effect of the policy which is independent of the covariates' values. This is clearly not the case: hence, what we get is, more simply, a graphical way to depict the effects of the policy that has been *in fact* implemented, taking into account that a part of these effects is due to the covariates' combination[14].

4.6 Heckman correction for selection bias

It is possible, in our dataset, to identify 312 firms that applied to at least one of the four programs, without obtaining any subsidy. One may consider these firms as a more reliable control group for the evaluation of the policy effect: they might be, in fact, more similar to the subsidized firms also according to unobservable features, being that they have experienced the same possible self-selective process. Unfortunately, this bunch of firms is not large enough to constitute a valid control group to our aims: in fact, an attempt to match the subsidized firms to these non-recipient applicants gives back a match only for 221 treated firms (less than 20% of recipient firms). Therefore, the results we obtain re-running the analysis with these new treated and control groups are not comparable to the policy effects estimated in the previous exercises. Similarly, non-comparable results are produced if we relax the matching criteria in order to enlarge the number of treated matched firms.

To control for possible selection bias, we apply the Heckman (1979) correction method adding the inverse Mills ratio to our models: the results for the "after matching" cases are reported in Table 1 (column c), Table 2 (column c and e), Table 5 (column c). As one can see, the coefficient of the inverse Mills ratio is negative and significant in all the regressions[15]: this indicates that a self-selection process occurs and that the OLS estimates are downwardly biased. Again, this leads to exclude a "*picking the winners effect*" (see Sect. 4.1), i.e. it suggests that applicant firms do not have, on average, a better employment performance, compared to non-applicant ones. However - despite this selection bias and probably due to the small number of non-recipient applicant firms - the sign, magnitude and significance of the parameters connected to the

[14]For a comparable approach, based on a two-step matching estimator and an application to the Italian case, see Adorno *et al.* (2007).

[15]The inverse Mills ratio coefficient, on the other side, is always non-significant when the models are estimated on "before matching" data.

policy effects do not change[16] in all the equations, and this leaves the evaluation of the policy effectiveness unchanged.

5. Discussion

Evidence. The previous analysis has shown that regional financial incentives to firms have a non-significant (or negative) impact on short-run employment. These programs aim at strengthening the competitiveness and the survival capacity of firms: therefore, maybe that – despite the lack of incidence on employment, or just because of it – they yield positive effects on firms earnings or productivity.

Unfortunately, our original dataset does not contain variables related to the earning performance of firms: hence, to test our hypothesis we have drawn information from the AIDA[17] dataset. AIDA provides data on EBITDA[18] for corporations[19] whose annual revenues are higher than € 100,000: due to these restrictions, we found only 224 firms that took part in one of the four programs and received a financial subsidy.

The analysis we have made, therefore, is not comparable with the results obtained in section 4: that said, it is interesting to note that the correlation between employment variation and EBITDA variation is low for financed firms (Pearson correlation coefficient = 0.051) and even lower for non-financed firms (Pearson correlation coefficient = 0.007); moreover, the coefficients related to the programs are generally non-significant both in binary and in multitreatment models and hence no clear impact on earnings emerges[20].

A non-significant – and even negative – short-run impact of the policy on employment is not necessarily the indicator of an ineffective policy: in fact, the availability of larger financial resources may induce the adoption of labor-saving innovations, raise productivity and profitability of the firm, yield higher growth rates and provide higher employment levels in the medium-long run. The absence of a significant impact on earnings, on the contrary, raises more than a doubt also on this long-run perspective and strengthen the opinion that the policy has been, on the whole, ineffective.

[16]A partial exception is noted for *Feasr* and *Frim*: in particular, the two programs become weakly significant also at the low-subsidy level.

[17]AIDA (*Analisi informatizzata delle aziende*) is a database developed by Bureau Van Dijk.

[18]Earnings Before Interest, Taxes, Depreciation and Amortization.

[19]In particular, individual firms and partnerships, which are a large part of our "treated" firms, are excluded.

[20]On the other side, AIDA data confirm, to some extent, a positive effect of *Frim* program on employment in linear models. Detailed results are available from the authors.

It is worth noting that, among the four programs, *Frim* appears as the most effective (or less ineffective), at least when public subsidies are particularly high. *Frim*, in fact, is the only program that provides the subsidy as interest-rate contributions instead of capital grants: this possibly implies that the projects where *Frim* contributes to cover a large interest rate burden are also the largest in terms of capital involved, and helps to confirm the hypothesis that the dimension of the project is crucial in identifying the few cases where the public intervention has shown a positive impact. Alternatively, studies on regional policies in Italy argue that public subsidies are more effective when they support firms that face some market failures, such as difficulties in accessing credit, and this might be the case (Asvapp 2012).

Previous studies. A number of studies have considered, so far, the effect of public incentives on job creation. They concern state policies in the U.S. and do not provide strong evidence of a large positive incidence.

In particular, Gabe and Kraybill (2002) examine the impact of a policy implemented in Ohio in the 1993-1995 period, intended to create new jobs. Despite the explicit goal of the intervention – while the programs we have studied aim more generally to stimulate competitiveness of firms - they can find only very little (or even negative) effect on employment growth. As an explanation for the counterintuitive result, the authors hypothesize a negative rent-seeking effect: the effort spent to obtain public subsidies reduces time and resources devoted to improve firms' efficiency, and may have the consequence of a lower growth rate[21].

Faulk (2002) analyzes the efficacy of state employment tax credits in Georgia between 1993 to 1995 on job creation. The estimates suggest that firms create jobs in response to employment tax credits, but also emphasize the existence of a large deadweight loss: around three-quarters of the employment change in recipient firms would have been created even in the absence of the policy.

Hauptman (2014) finds positive effects of employment tax credits in Colorado between 2011 to 2013, comparing recipient firms to both eligible non-recipient and non eligible firms. After an analysis of the labor demand elasticity, the author concludes that the most plausible explanation of the result is not the lower labor cost induced by the subsidy, but rather the application of firms that had previously planned significant employment growth.

These studies adopt methodological approaches that are, to some extent, similar to ours, in that they compare the performance of recipient to non-recipient firms, through linear models that also account for participation and selection bias. None of them, on the other side, makes

[21]The effect is mentioned also with regard to the Italian case: see Alesina *et al.* (2001).

use of matching techniques or non-linear representations of the policy effects.

On the contrary, Jensen (2016) explores a state incentive program implemented in Kansas in 2009 and applies matching techniques to create a control group for each recipient firm: the methods adopted are CEM – as in the present study – and entropy balancing (Hainmueller 2012), while propensity scores are adopted as a robustness test. The policy effectiveness is estimated only through linear models. The main finding is that there is no significant difference in job creation between recipient and non-recipient firms. Interestingly, the author indicates that the matching analysis is based on managerial reports on how effective the incentives were in generating employment: this is supposed to produce estimates biased towards positive policy effects. As in previous studies, the most important reason for the policy ineffectiveness seems to be that recipient firms obtain financial incentives for already planned employment expansions, that would have happened with or without public support.

Other employment tax credit policies have produced – according to the case studies – little employment growth, or positive job growth at a high cost (Sohn and Knaap 2005; Luger and Bae 2005; Hicks and LaFaive 2011).

In partial contrast, Chirinko and Wilson (2016) evaluate the impact of 21 state employment tax credit interventions between 1990 and 2007 and find positive cumulative effects. Nevertheless, they notice that the realization of the full effect takes several years and, most important, point out the role of fiscal foresight (i.e. changes of behavior by forward-looking firms in anticipation of future policy announcements, which is strictly related to the time substitution strategy observed in other studies) in altering the net policy effect estimates: ignoring fiscal foresight, in fact, would lead to overestimate the incidence of public intervention by around 33%.

As for the Italian case, a group of studies examine the effects of Law 488/1992 – a policy intended to reduce territorial disparities providing capital grants to firms willing to invest in lagging areas - and, in some cases, compare them to other regional measures.

Bronzini and de Blasio (2006) carry out a counterfactual analysis on Law 488/1992 but use firms' investment decision (not employment growth) as the outcome variable. The study is interesting to our aims in that – while finding a positive impact of the law on investment expenditure – the authors observe two significant substitution effects: a time substitution, which induces firms to anticipate investment projects, with the aim to obtain public resources; a cross-sectional substitution, which implies that recipient firms take some investment opportunities that, in absence of the policy, would have been exploited by non-recipient firms. Both the effects, according to the authors, cast some doubts on the

net efficacy of the law. Interestingly, Cerqua and Pellegrini (2014) examine the same data also taking into account possible spatial spillover effects and conclude that a partial crowding-out phenomenon may be in place: subsidized firms, in other words, attract part of their employees from non-subsidized firms located in the same area.

The Asvapp (2012) report examines both the national-level effect of Law 488/1992 and the regional-level impact of 25 measures implemented in Piedmont – a Northern Italy region - between 2005 and 2009. A counterfactual analysis is carried out, that finds positive effect on job creation, but points out that interest-rate subsidies outperform capital grants both in terms of investment incentive and job creation and that, more generally, large grants are ineffective.

The analysis by Adorno *et al.* (2007) is interesting under the methodological viewpoint, in that a two-step matching procedure is applied and the impact of Law 488/1992 intervention in the period 1996-2000 is estimated with a non-parametric continuous-treatment model. According to the authors, the results support the evidence of previous binary-treatment studies about a global positive effect of the policy on employment, but point out the significant variability of the outcome with respect to different treatment levels. In general, they conclude that both too small and too high grants are less effective.

Finally, studies on the employment impact of the EU 'Objective 2 areas' business incentives offered between 1995 and 1998 find positive effects – even if at a high cost - at the national level (Bondonio and Greenbaum 2006) and non-significant effects in the regional case of Piedmont (Bondonio 2002).

Methodology. From a methodological perspective, this study provides a twofold contribution:

a) due to the large dimension of the available control group, we can in fact apply a matching procedure obtaining exact matching and a large common support (i.e., the data matching step discards from the analysis only a few treated firms). The number of untreated firms discarded by matching, on the other hand, is quite large. As Tables 1, 2 and 5 show, the estimates of the same models are quite different before and after matching, both in terms of coefficient sign and significance: this means that matching each financed firm to non-financed firms with the same features, discarding firms that do not find a match, provides a piece of information about the policy effectiveness which is, as we can see, frequently different from what we get before matching: this supports the idea – suggested by Ho *et al.* (2007) – that matching should be used as a preprocessing step in any kind of analysis, in order to obtain results that are more reliable, because less model dependent;

b) despite this clear advantage, matching is not enough to make our results completely free from model dependence: linear models, in our case, still induce a simplified reading of the policy effectiveness, which appears to be driven by small groups of outliers. To overcome the problem and being able to give a correct evaluation of the public intervention, we have made use of a continuous non linear approximation of the policy results, which has proven to be quite meaningful. As we have clarified in the previous section, this cannot be intended as a proper continuous treatment representation of the impact of public intervention. Nevertheless, it is interesting to compare our evidence to the two-step matching model by Adorno *et al.* (2007), evaluating the impact of Law 488/1992 in Italy. At the first stage, subsidized and non-subsidized firms are matched on a set of covariates; at the second stage the matched firms are matched again on the subsidy level: thereby, a continuous function relating subsidy to employment can be estimated. The results confirm that – despite a global positive impact emerging from binary treatment analysis – the treatment level affects the employment growth in a non-monotonic way, and this proves the usefulness to the policymaker of this kind of representation of the policy effect.

6. Concluding remarks

The four local programs we have examined, whose aim was to improve competitiveness and survival capacity of firms, seem to have been null or negative net effects on employment growth, at least in the short run. The absence of a clear positive impact on firms' earning profile, on the other side, induces to suspect that the effect on employment cannot be relevant even in the medium-long run.

The evidence is consistent with the majority of the available studies that have analyzed the effects of public subsidies on job creation and also (in the Italian case) on other performance indicators of firms.

The main reasons these studies provide to justify a quite counterintuitive conclusion are the incidence of substitution effects and the attitude of firms' managers to put effort in rent-seeking activities rather than in improving firms' efficiency.

Unfortunately, our data do not allow for evaluating how large these effects are; however, both these kind of phenomena are presumably stronger when the policy is implemented at a local level. Therefore, given that in the EU countries an increasing portion of public subsidies to firms is assigned to local authorities and distributed through local policies, in accordance with the principle of subsidiarity and with the aim to use these financial resources to reduce territorial disparities, these potential reasons of inefficacy should be taken in greater account in future policy

design. The evaluation of their importance also constitutes a research topic for further analysis of local policy interventions.

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Appendix

Table A1 – *Descriptive statistics of "treated" and "untreated" firms*

	Treated firms		Untreated firms	
	N	%	N	%
Size				
large (≥ 250employees)	1	0,08	716	0,14
medium (50-249 employees)	58	4,27	4.387	0,87
small (10-49 employees)	233	17,15	30.486	6,07
micro (1-9 employees)	1.067	78,50	467.246	92,92
Legal form				
corporation	220	16,19	116.855	23,24
consortium	5	0,37	1.207	0,24
co-operative	65	4,78	4.886	0,97
publicentity	6	0,44	587	0,12
individual	549	40,40	268.309	53,36
partnership	514	37,82	110.991	22,07
Sector				
Primary	1.099	80,87	38.088	7,58
Secondary	207	15,23	164.971	32,81
Tertiary	53	3,90	299.776	59,61
Province				
Bergamo	102	7,51	55.857	11,11
Brescia	257	18,91	70.955	14,11
Como	36	2,65	27.436	5,46
Cremona	159	11,70	19.116	3,80
Lecco	31	2,28	16.520	3,29
Lodi	35	2,58	10.472	2,08
Monza Brianza	16	1,18	39.717	7,90
Milano	112	8,24	158.793	31,58
Mantova	267	19,65	27.612	5,49
Pavia	209	15,38	28.922	5,75
Sondrio	74	5,45	10.377	2,06
Varese	61	4,49	37.058	7,37

Nb: Sector is obtained by aggregation of the CODE variable used in the matching exercises, which is based on the Ateco statistical classification of sub-sectors of economic activity.

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