

A New Phoenix?

Large Plants Regeneration Policies in Italy*

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Abstract. In this paper we evaluate the effectiveness of a place-based program (Contratti d'Area), carried out in Italy since the late 90s, aimed both at generating private investments and at increasing the employment in areas affected by deep industrial decline. We show that the program did not have any significant impact on the level of investments, employment and turnover of firms operating in the areas involved in the policy. We discuss this evidence in the light of some potential weaknesses that can undermine these kind of programs policies: (a) badly designed policy; (b) low quality of local administrators; (c) allocation of the benefits based on political considerations.

Keywords: Local development, place-based policies, program evaluation

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1 Introduction

Downsizing of large manufacturing plants was a pervasive phenomenon in the '70s and the '80s in all advanced countries. As argued by Trento and Warglien (2003), downsizing is often the outcome of organizational innovations introduced by the new technologies of information and communication. In fact, these allow for a better monitoring of the single phases of a production process and induce the adoption of more horizontal management practices and a stronger recourse to external outsourcing. As a result, since the '70s large plants fragmented into several smaller ones; in some cases competition by smaller, more productive and innovative production units pushed out the market old Fordist plants. Between 1961 and 2001, in Germany and France, the share of employees working in plants with more than 100 workers shrank, respectively, by 9 and 11 percentage points. Italy was no exception: its downsizing has been stronger despite the fact that the initial size distribution of firms was already highly fragmented.

Shutting down a large industrial plant is usually a source of great concerns for policy makers. Worries for the loss of employment are generally associated to the fear of negative spillovers to other surviving firms that have input-output linkages with the closing plant. Apprehension for these phenomena are particularly stinging when the closing plant is located in poor areas, where finding an alternative occupation is particularly hard for displaced workers. For this reason, also in combination with the de-industrialization process in the advanced countries (Rowthorn and Ramaswamy (1997)), situations of crisis of large industrial plants have become one of the main concern for regional policy. Accetturo and de Blasio (2012) and Neumark and Simpson (2014) provide a literature review on this topic.

In this study we analyze a regeneration policy adopted in Italy: Contratti d'Area (Area Contracts, ACs). The ACs were designed in 1996 for a few locations selected by the Italian Government among the areas affected by deep industrial decline, resulting in a very high unemployment rate. Disbursements actually started in 1998. The program had two different goals: first of all, interventions were expected to provide an incentive for local firms to undertake further investments; however, the ultimate objective was to generate new employment in manufacturing, agribusiness, services, and tourism. The areas involved in the policy were characterized by the presence of large industrial plants in crisis; often this was the result of the failure of past Central Government policies aimed at locating large plants in poor areas. The AC program was completely different in spirit, being based on a bottom-up approach, where many responsibilities were attributed to the local authorities. The initiative to apply for funds rested with the local governments: the application consisted of a fully-fledged plan for the development of the area that included a series of private and public investments for which public funding was provided. Applications were subsequently evaluated by the Central

Government that chose, in a very independent way, the areas where to channel the funds.

This paper performs a counterfactual analysis of the economic impact of the program over the period 1998-2007. The identification of a causal effect is not an easy task due to the presence of usual confounding factors such as non-random program placement and self-selection. To cope with these problems, we rely on a difference-in-differences estimation in a Propensity Score Matching (PSM) and Instrumental Variable (IV) framework. The analysis is conducted in two steps. First, by using PSM, we match AC cities with a number of eligible non-treated municipalities that were heavily reliant on the employment by a large manufacturing plant that experienced a downsizing over the period 1991-96. Second, we compare the evolution of a number of firm-level variables (investments, total labor costs, sales) for the firms located in treated areas with those of the companies in control cities. The possible non-random allocation of the AC status is dealt with an IV approach. The instrument is a dummy variable equal to one if local and central governments shared the same political color at the time when the decision to undertake the policy was taken. This actually hints at the fact that Governments are generally more prone to channel funds to local administrations that have the same political color, regardless the actual perspective of long-run growth of the receiving areas.

Our results suggest that the AC program has been largely ineffective in steadily increasing turnover, investment or employment, as their growth in the whole period do not differ significantly whether the firms were involved or not in the policy. Accordingly, our results are in line with the disappointing evidence of other place based policies carried out in Italy (Bronzini and de Blasio (2006); Accetturo and de Blasio (2012); Andini and de Blasio (2016)). Notwithstanding, even in absence of a significant economic impact, we document that the ACs are positively associated with the probability of reelection for incumbent local politicians in the treated municipalities.

The paper is organized as follows. Section 2 describes the AC program. Section 3 illustrates the construction of the sample and the identification strategy. Section 4 shows our main results, while Sections 5 and 6 present a number of sensitivity checks. Section 7 provides an in-depth discussion of a series of factors that could have undermined the impact of the policy. Section 8 concludes.

2 Description of the policy

The ACs were introduced in Italy in mid-90s as an attempt to correct the failure of top-down policies of the 1960s and 70s, based on fostering development by locating large plants owned by big state companies operating in the heavy industry

(mainly, in the steel, metallurgy, energy and chemicals sectors). In particular, many of these interventions (aimed at industrialising the South of Italy) had been devised by a national public agency, i.e. the “Cassa per il Mezzogiorno”, that was established in 1951 and then abolished in 1984 (although the Rome-centred intervention to promote economic growth in the South ended up in 1992 only). This strategy soon began to show her weaknesses, being undermined by lack of accountability and capture of private interests. Moreover, several of the sites built by the “Cassa” were hit by oil crises and the recession in the western economies. In the meanwhile, also the intervention policy experienced a radical change, in connection with the influence of EU cohesion policy and the need of simplification (OECD, 2001). At the beginning of 90s, this was reflected in a shift from top-down policies to concerted forms of planning. In particular, growing responsibility was given to a multiplicity of central and local authorities, in an attempt to deliver an effective development strategy, in which all agents cooperate to achieve the common goal of economic growth. The rationalization of public intervention in lagging areas was completed with the 1997 National Budget Law (Law 662/1996), that introduced the legal source of “negotiated programming”, identifying different intervention instruments. In particular, it encompassed three main territorially targeted programs:¹ Territorial Pacts, designed to foster growth in large disadvantaged areas through a bottom-up approach with the local community playing a key role in agreeing on the development plan (Accetturo and de Blasio (2012)); Planning Contracts, targeted at the promotion of inward foreign direct investments in deprived areas (Andini and de Blasio (2016)); and finally ACs, aimed at regenerating selected areas characterized by unemployment problems, often in connection with large industrial plants in crisis.

As the other forms of “negotiated programming”, ACs are the result of a bottom-up approach. They originate from a negotiation that took place between municipalities and regional authorities under the approval of the Central Government (formed by the centre-left coalition that had won the election in 1996). In particular, the initiative to apply for funds rested with the local governments: the application consisted of a fully-fledged plan for the development of the area that included a series of private and public investments for which public funding is provided. Applications were subsequently evaluated by the Central Government that chose, in a very independent way, the areas to receive funds (such agreements were signed between 1998 and 1999). Eligible municipalities were all those located in areas allowed to receive public funds by the European Union. Therefore, all southern municipalities were eligible, as the whole Mezzogiorno was included in the EU Objective 1 area, while in the Center and North eligibility was restricted to

¹In addition, there was a major incentive scheme, Law 488/1992, intended to subsidise firms located in economically depressed areas (Bronzini and de Blasio (2006)).

a portion of the territory: only municipalities located in the EU Objective 2 and 5b areas could join the program. In the whole, the program had two different goals: first of all, interventions were expected to encourage firms to undertake further investment at their current location with the ultimate objective to generate new employment. However, the program intended also to improve industrial relations, reduce red-tape, and facilitate access to credit. In a few cases, the AC required also investments in local infrastructures, which were totally funded with public resources. As for the sectors involved, the initiative was envisaged for manufacturing, agro-industrial, services and tourism. The main benefit was related to the introduction of subsidies for investments.

The AC program was the smallest among the “negotiated programming” instruments in terms of funds allocated, but more selective in targeting treated areas than the other place-based programs starting in Italy during 1990s. In the period 1998-99, there were signed 15 ACs. €2.0 billions of public funds were injected (as against 4.7 in Planning contracts and 5.0 in Territorial pacts). The number of municipalities involved was 95 (as against over 400 in Planning Contracts and 1300 in Territorial Pacts). Among them, there are two separate groups of projects: 10 strictly relative to areas with large industrial plants in crisis and 5 regarding areas with severe unemployment more in general and not specifically linked to the crisis of a large plant.² The first group of ACs consists of the projects characterizing as the main recipients of the policy. In the next sections we will restrict the analysis to the first group of “more homogeneous” ACs only, to gain identification.

3 Data and identification strategy

Information on ACs was collected from official documents.³ We started with a sample of 15 ACs approved in the period 1998-1999. As discussed before, there are among them two separate groups of projects: the first one is strictly related to areas with large industrial plants in crisis, while the second one is regarding areas with severe unemployment more in general. Excluding the latter group has the advantage of providing a more homogeneous sample and thus minimizing the potential presence of confounding factors. This left us with 47 municipalities involved in 10 ACs. Yet municipalities eligible for the program do not represent a random sample of the Italian population of municipalities. This circumstance may create a selection bias due to non-random program placement, as the impact of the program could be due to the effects of the pre-existing disparities. To cope with this issue, we compare only municipalities having similar characteristics before the

²In the period 2000-03, other 3 ACs were signed, all belonging to the second type.

³See, in particular, Presidency of the Council of Ministers (2004).

start of the program. This is accomplished by using the propensity score matching (Rosenbaum and Rubin (1983)).

As far as the matching is concerned, our main data source is the Istat (Italian Statistical Office) Census. In particular, we use the releases for the years 1991 and 1996. These data have a few advantages. First, they provide us with a fair representation of the pre-treatment situation by being referred to the period exactly before the implementation of the program. Second, the Census contains information for all Italian municipalities (i.e. since it covers the whole country there is no sample selection problem). However, since our treated municipalities are not distributed uniformly across space, but are grouped in only a few limited zones (that includes large plants in crisis and their surrounding area), we sampled from the distribution of Local labor systems (LLS). Accordingly, we considered a total of 17 treated LLS (including at least one treated municipality in one of the 10 ACs considered in the analysis), that were matched with untreated eligible LLS (those belonging to the Objectives 1, 2, 5b defined by the EU Commission) across: share of large plants (>100 employees); large plants employment dynamics in 1991-96; unemployment rate; log of population; a dummy for southern LLS. We ended up with 14 matched LLS, from which we obtained 238 matched municipalities (“controls”).⁴

Figure 1 plots the treated municipalities and the selected controls. For reference purposes, Columns 1 to 3 of Table 1 report pre-treatment values of the matching variables respectively for the full sample of Italian municipalities, the treated and the controls. The last two groups differ significantly from the average: they are characterized both by a higher incidence of large firms and a declining path of employment. Still, Column 4 assesses the balancing properties of the matching. The tests do not reject the null hypothesis of equality of means for treated and controls.

Due to the nature of the program, its effectiveness was evaluated in terms of performance of the firms operating in the area involved in ACs. In particular, we consider the following baseline OLS specification:

$$y_{imp} = \alpha + \beta t_{mp} + X_{imp}\gamma + \epsilon_{imp} \quad (1)$$

where, for firm i operating in the municipality m at time p , X_{imp} is a set of control variables (including sector-year FE and firm FE), and t_{mp} is the treatment status dummy, which takes value 1 in the post-policy period (year 2000 and beyond) if the firm is in the treatment group (i.e., it is operating in a treated municipality). Our main data source is the Cerved archive, containing balance-sheet information on all Italian corporations, covering the period 1993-2007. In particular, our analysis focuses on 3 key performance indicators (our dependent

⁴Matches are made by using a one-to-many nearest neighbor matching with replacement.

variables): capital assets, total labor expenditures,⁵ and sales. Although only the first two were a direct target of the policy, the last one is used in order to verify if the policy had an effect on the economic performance of targeted firms. In the analysis, we check also for short-term effects on current assets, bank loans and financial costs, in order to verify if the policy had an indirect impact on the financial situation of targeted firms.

The main identification challenge with the estimation of model (1) is that the treatment dummy is likely to be correlated with the error term. Municipalities have been selected also because of the characteristics of the firms operating there. Even if we tackled this issue by using a matching on observable characteristics, selection on unobservables could still be a concern. Thus, there could be features influencing both the treatment status and the performance of firms at local level that we are not able to control for. Our identification strategy relies on a peculiar feature of the policy. As discussed before, while the AC initiative belonged to labor and firms' representatives and local administrations, Central Government was to decide about its implementation. On this regard, area contracts we focus on were signed between 1998 and 1999 under the centre-left government coalition. Departing from the literature that emphasises the importance of party alignment for governmental transfers (see, for instance, Solé-Ollé and Sorribas-Navarro (2008); Arulampalam et al. (2009); Brollo and Nannicini (2012); Bracco et al. (2015)), we suppose that local administrations led by a centre-left majority in 1995 (before the policy was launched) became randomly more likely to access the program when the centre-left coalition won the national election in 1996. Accordingly, we estimate equation (1) by 2SLS using a simple dummy instrument equal to one if the city and regional administrations were both led by centre-left in 1995.⁶

Table 2 shows our first stage, which predicts treated municipalities according to the extent of their political alignment with central government in 1996. The probability of being involved in an AC in 1996 was significantly larger for the municipalities led by a centre-left coalition in 1995 with respect to the municipalities led by centre-right (28% and 19%, respectively). When considering jointly the political alignment characterizing both the municipality and the region, the rela-

⁵Note that we recur to total labor expenditure (wL) as a proxy of the number of employees (L), since the latter is only available in Cerved for a very limited subsample of firms. The working hypothesis is that salaries in the treated and untreated firms have followed a common trend. This is quite acceptable if one considers the overwhelming role of centralized bargaining in Italy (Du Caju et al. (2008)).

⁶Since the endogenous explanatory variable is binary, we also followed the two-step procedure suggested by Wooldridge (2002). First, we estimated the treatment propensity using a probit model; then, we used the fitted probabilities as instrument in the 2SLS estimation of (1). This procedure has several useful advantages: in particular, the estimator is asymptotically efficient and robust to misspecification of the first stage equation. However, results (available upon request) are very similar to those reported.

tionship underlying our first stage is even larger: 44% of the municipalities led by a centre-left coalition and located in a region with a centre-left wing administration in 1995 were involved in an AC, a proportion about three times that of right wing cities in right wing regions. In other terms, the last two columns confirm that the share of the municipalities led by a centre-left coalition and located in a region with a centre-left wing administration in 1995 is sharply higher among treated than eligible municipalities.

Turning to the exclusion restrictions, our empirical strategy relies on the assumption that, conditional on all the controls used for the matching procedure, the outcome variables are independent of the funding mechanism. This assumption does not hold if aligned municipalities are systematically characterized by lower or higher quality projects. However, not meeting the exclusion restriction seems quite unreasonable in the case of a politically motivated allocation of funds. In particular, we can exclude the case where projects of the aligned municipalities are more promising: as the likely driver behind the allocation decision is self-interest and political consensus, the decision will not presumably be based on the potential economic effects of the grants. On the other hand, we can also exclude the case when the political favoritism ends up by selecting the worst municipalities, ignoring their initial economic conditions: our estimation sample includes municipalities which are matched along a wide set of economic indicators.

4 Main results

Our estimates show that the policy was largely ineffective. Table 3 reports our baseline panel model OLS and IVE results, estimated over the period 1993-2007. OLS estimates suggest that the policy has a positive impact on firm assets, with an elasticity of about by 9% on average. However, IVE coefficients are much lower and not statistically different from zero. Hence, when we control for endogenous selection in the sample, the policy shows no impact. Moreover, the direction of the bias suggests that areas where firms already invest more are selected by the policy. Both OLS and IVE results show no impact on employment, proxied by the expenditure in labor cost, and on firm sales.

In order to test for the short term impact of the policy we run our model over the period 1997-2003. The results, reported in Table 4, are very similar to those obtained over the full period. However, while IVE coefficients are still not statistically different from zero, they are now much larger with respect to the full-period estimate. We also check for a short-term impact of the policy on the firm financial structure. All in all, we also find no impact of the policy (Table 5). Coherently with the lack of effect on investments, the policy does not lead to an increase in bank loans; also current assets and financial costs remain unchanged.

While the policy is overall ineffective when considered over an 7 year period, it could still have displayed its effects over a shorter period.

5 Robustness on identification

As argued by Bertrand et al. (2004), our baseline standard errors could be biased due to errors serial correlation. In order to provide a more robust estimate, we follow Bertrand et al. (2004) and average our outcome variables over two periods only, before and after the treatment, ending up with a panel of length two. Hence, we run again our model. Both OLS and IVE results, reported in Table 6, confirm our previous findings.

We also perform a falsification exercise aiming at supporting the validity of the exclusion restrictions of the 2SLS estimation. To do so, we first restrict our sample to the untreated eligible firms only; then we run an OLS regression of the outcome variables on the instrumental variable, firm FE and sector-year FE. If exclusion restrictions hold, our instrument should not have any impact on the output variables, as the only effect that a valid instrument is allowed to have on the outcome variables is indirect, passing through the treatment. The results (Table 7) support the validity of the exclusion restrictions, since the instrument is never significant across all regressions.

Again, we test for a number of placebo policies in the period before and after the introduction of ACs. In particular, we perform the same analysis as before, but considering a set of placebo treatments starting in each year from 1997 to 2003. Table 8 report the results for each fake treatment (note that the 2000 entry corresponds to the actual policy). The placebo analysis confirms the absence of any differential trend in the key economic variables of treated firms. Furthermore, we can exclude the presence of implementation lags and anticipation effects of the policy.

6 Heterogeneous effects

While the policy does not show any impact overall, it might still have affected firms in a heterogeneous way, according to the characteristics of both the aid and the firms. In order to assess for heterogeneous effects, we consider three different dimensions: aid intensity, firm sector, firm size.

In order to check whether the policy impact was different as regards the intensity of the aid, we split our municipalities according to the share of public funds over population. In particular, we compute the intensity of the aid as a ratio between the total amount of public funds channeled to the municipalities involved

in the area contract and the total population of those municipalities. Then, we run separate estimates for firms located in municipalities characterized by “high intensity aid” areas, and for firms located in “low intensity aid” ones (Table 9). Our results show that there is a positive and statistically significant effect of the policy on investments in the first group of ACs (Table 10). This could suggest that in some ACs the scale of intervention was not sufficient to induce firms to invest more. However, in any case, we find no evidence of significant economic benefits on employment and firm performance. In the areas with “high intensity aid”, the fact that the policy led to increased investments while there were no impacts on production levels could be in line with evidence reporting unproductive or wrong investments (abandoned plants, very expensive builds, maladministration)⁷ and fraud (for instance, police enquiries show evidence of false bills for the purchase of “ghost machineries”, or machineries bought and re-sold several times).⁸ These results also suggest that the policy had a negative effect on firms’ total factor productivity since it induced the companies to buy production factors they did not really need.

With regard to heterogeneous effects of firm sector and size, we never find a significant impact of the policy when dividing our sample between manufacturing and services, or small and large firms (see Table 11 and Table 12). Notwithstanding, the comparison between OLS and IVE signals that the selection bias is due to investment by large manufacturing firms. This should indicate that, at least in part, the probability with which an area was selected in the program has depended on the prior presence of investment plan by those corporations.

7 What’s behind our results?

In this section we focus on the possible reasons that undermined the impact of the ACs, whose ineffectiveness is however in line with that of other Italian place-based policies (Bronzini and de Blasio (2006); Accetturo and de Blasio (2012); Andini and de Blasio (2016)). On a theoretical ground, a relevant argument against place-based policies is the potential displacement effects they induce (Einio and Overman (2016)). In particular, positive effects in treated areas could be reached at the expense of untreated neighbouring areas. Concerning the ACs, however, the extent of ineffectiveness of the policy seem to be larger, as we fail to find any positive effects in treated areas, to start with.

⁷See, for instance, the following newspaper article, reporting about the AC of Ottana <http://lanuovasardegna.gelocal.it/regione/2015/09/27/news/ottana-il-grande-bluff-del-contratto-d-area-1.12167168>

⁸<http://www.statoquotidiano.it/25/03/2010/manfredonia-indagine-su-societa-contratto-d-area-truffa-e-falso/20824/>

The absence of positive effects in treated areas could reflect one or more weaknesses amongst those that can undermine place-based policies: (a) badly designed policy; (b) low quality of local administrators; (c) allocation of the benefits based on political considerations. Concerning the first point, ACs were characterized by a very complex architecture, requiring integration of several local forces (at the political, administrative, economic level). Such integration was made even more difficult by the high extent of bureaucratization on the one hand, which in several cases delayed the activation of the investments, and lack of necessary regulation from the other hand. For instance, over the first four years of activity it lacked a discipline concerning the tasks of the managers, undermining their capabilities to provide technical help. Moreover, while the policy was meant to be purely local, the investment initiatives were funded according to a national Law (i.e., Law 488/92), which was clearly less effective in the screening process of the investment initiatives. A rigorous monitoring was needed to counterbalance the lacks in the screening process, but it largely lacked. Finally, ACs failed to be completely tailored to the territorial idiosyncratic characteristics, probably both because of lack of knowledge of the territories and weak planning capabilities. All in all, ACs shared many of the features of those purely redistributive policies that they meant to replace and, with them, the same weaknesses.

Even if well designed, both individual and institutional capacities are necessary for a local policy to be effective (Auspos et al (2009)): ACs, as well as other place based policies, necessarily relied on the guidance of local governments. The arguments favoring the need of stronger local administrators, expression of the local society, had led, a few years prior to the development of local planning, to the direct election of both mayors and presidents of the provincial presidents in 1993. However, according to La Spina (2003) and La Spina (2005) this new political class proved herself not up to the task of driving the change, being caught between the perverse incentives introduced by the direct election mechanism, linked to the need of increasing consensus while in charge. These incentives might have led, in turn, to favor particularistic interests rather than setting up the road for a long lasting development. This issue could be particularly relevant in the case of ACs, due to the low level of quality of institutions in the area interested by the intervention. In particular, by considering the Institutional Quality Index (IQI) of Nifo and Vecchione (2014), we observe that 98% of treated municipalities were located in provinces with IQI below the national median.⁹

Local planning could also reflect perverse fund allocation mechanisms, following political considerations rather than policy making criteria. As argued by Luca

⁹The IQI index is a composite indicator built to measure institutional quality in Italy. It combines several sub-indexes of different institutional aspects (Regulatory quality, Government effectiveness, Rule of law, Corruption, Voice and accountability), and refers to the period 2004-2012 both at regional and provincial level.

(2016), electoral concerns consistently play a relevant role in shaping the allocation of public funds, being often linked to corruption, political clientelism, political patronage. Investigating whether political patronage might have undermined the impact of ACs is far beyond our aims. Nonetheless, in the reminder of this section we shed some light on whether this channel might have indeed operated, by looking at the correlation between public funding and the electoral chances of the incumbent political leaders.

We follow Levitt and Snyder (1997), showing that public spending has a positive impact on the probability of incumbent politicians of being re-elected; analogous results were obtained by Manacorda et al. (2011) and Finan and Mazzocco (2016); on a similar ground, Harrington (1993) show that electoral manipulation could entail implementing policies that are well received rather than those who are likely to maximize income. We repeat a similar exercise in our framework, by considering the re-election outcome of incumbent politicians after the policy introduction.

In particular, we consider the share of city administrators in 1997 (before the policy was launched) that were still in charge in 2003. In line with previous findings, Table 13 shows how incumbents in treated municipalities have a significantly higher probability of re-election than their counterparts in control ones. These results are robust to the inclusion of Region FE and a large set of political and socio-demographic controls (year of election, number of city administrators per capita, a set of dummies for the political parties in power at city level, an index of political competition, city size, population density, dependency ratio, and proxies for human capital and civic engagement).¹⁰ However, this evidence should not all be interpreted as a causal effect since a few caveats about identification apply.¹¹

8 Conclusions

In this paper we evaluate the effectiveness of a place-based program, carried out in Italy since the late 90s, aimed both at generating private investments and at increasing the employment in areas affected by deep industrial decline. Using instrumental variable estimations, relying on the political link between local and central governments, we are able to identify the aggregate impact of the program at the municipality level.

Our results show that the program did not have any significant impact on

¹⁰Furthermore, previous economic conditions are by definition similar due to sample construction.

¹¹For instance, reverse causality may exist if an increase in public spending follows from the political competition between the incumbent party and the opponent one; again, omitted variables (amount of taxes collected, skills of the politician, lobbying activity of local entrepreneurs) could present another potential problem.

the level of firm investments, employment (proxied by the labor expenditure) and turnover; firm financial variables were not affected too. On the other hand, when we split the municipalities according to the intensity of the aid, we find a positive effect on investments in those characterized by high intensity. However, high intensity municipalities still do not witness any effect when turning to employment and turnover; this evidence suggests that the additional investments were either channeled to unproductive expenses or even fraud.

Finally, a positive correlation is detected between the probability for the incumbent local administration to be re-elected after the program, and the participation of the municipality to the program. Although this latter evidence should not be interpreted as causal, it seems to confirm the claim, of a stream of research, that public spending leads to an increase in the probability of re-election of incumbent politicians.

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Figure 1: Treated and control municipalities

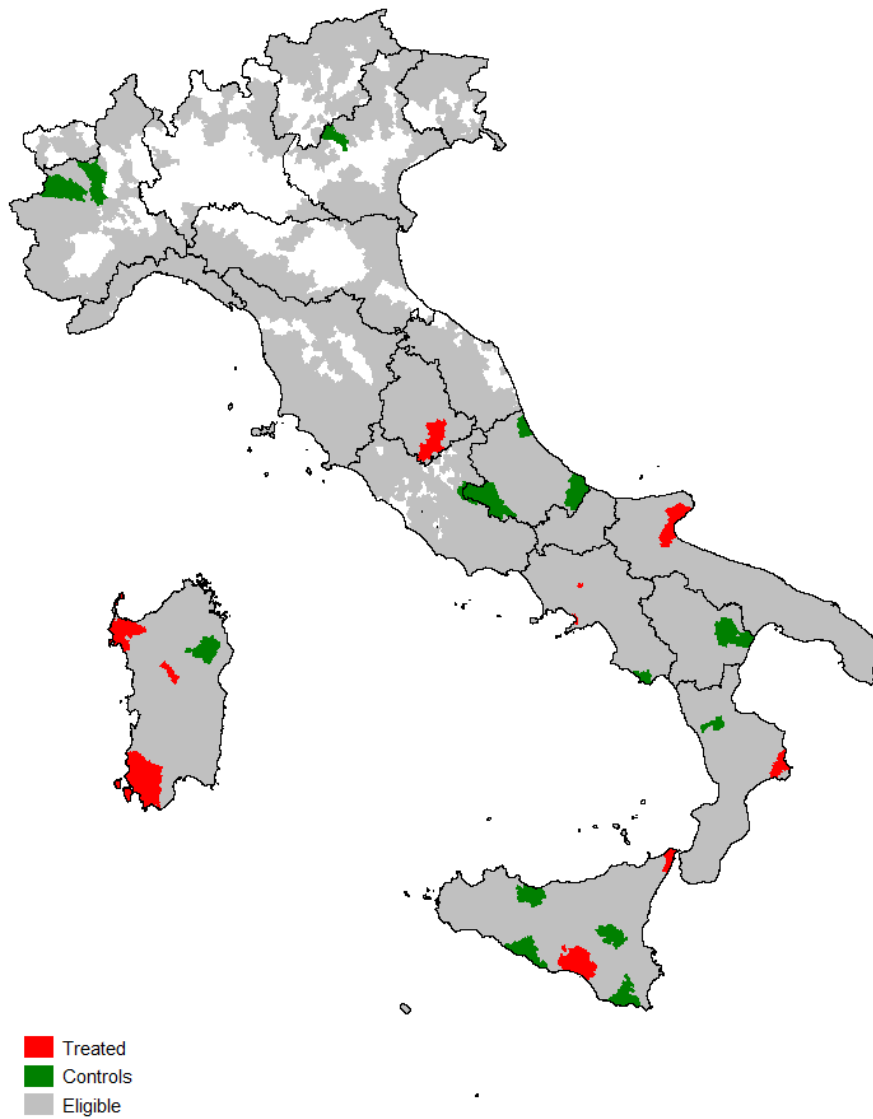


Table 1: Balancing properties

	Full sample (1)	Treated sample (2)	Control sample (3)	P-value of difference between (2) and (3)
Share large plants	0.08	0.16	0.20	0.308
Δ Employment large plants	-0.12	-0.62	-0.42	0.405
(Log)Population	10.09	11.12	11.00	0.715
Unemployment rate	0.09	0.10	0.11	0.621
South	0.56	0.88	0.82	0.657

Notes. Full sample includes all the eligible municipalities (see Figure 1). Treated sample includes municipalities involved in ACs. Control sample includes PS selected control municipalities.

Table 2: First stage evidence - political alignment

	Treated + Control Sample		Treated Sample	Full Sample
	Observations	Frequency of treated	% in the treated	% in the eligible
City administrators				
Centre-left	104	27.9	61.7	41.0
Centre-right	26	19.2	10.6	10.6
Others	155	8.4	27.7	48.4
Regional administrators				
Centre-left	107	30.8	70.2	39.4
Centre-right	178	7.9	29.8	60.6
City and regional administrators				
Centre-left + Centre-left	50	44.0	46.8	19.5
Centre-left + Centre-right	54	13.0	14.9	21.8
Centre-right + Centre-left	14	21.4	6.4	3.7
Centre-right + Centre-right	12	16.7	4.3	7.4
Others + Centre-left	43	18.6	17.0	16.2
Others + Centre-right	112	4.5	10.6	31.4

Notes. Full sample includes all the eligible municipalities (see Figure 1). Treated sample includes municipalities involved in ACs. Control sample includes PS selected control municipalities.

Table 3: Baseline results

Dependent variable:	Capital assets	Labor exp	Turnover
Panel A: OLS estimates			
Treatment	0.091** (0.040)	0.002 (0.035)	-0.034 (0.032)
Panel B: IV estimates			
Treatment	-0.124 (0.325)	-0.184 (0.228)	-0.097 (0.167)
Firm FE	YES	YES	YES
Sector X Year FE	YES	YES	YES
F-statistic (first stage)	136.5	136.5	136.5
Observations	105,071	105,071	105,071

Notes. The period of observation is 1993-2007. Treatment is a dummy equal to one for treated firms after the adoption of the policy (year 2000 and beyond). Treated firms are firms operating in municipalities involved in ACs. Control firms are firms operating in PS selected control municipalities. Standard errors clustered at city level in parenthesis. Significance at the 10% level is represented by *, at the 5% level by**, and at the 1% level by ***.

Table 4: Short-term results - economic variables

Dependent variable:	Capital assets	Labor exp	Turnover
Panel A: OLS estimates			
Treatment	0.078** (0.030)	0.006 (0.032)	-0.025 (0.033)
Panel B: IV estimates			
Treatment	0.083 (0.186)	0.144 (0.236)	0.195 (0.261)
Firm FE	YES	YES	YES
Sector X Year FE	YES	YES	YES
F-statistic (first stage)	63.3	63.3	63.3
Observations	47,222	47,222	47,222

Notes. The period of observation is 1997-2003. Treatment is a dummy equal to one for treated firms after the adoption of the policy (year 2000 and beyond). Treated firms are firms operating in municipalities involved in ACs. Control firms are firms operating in PS selected control municipalities. Standard errors clustered at city level in parenthesis. Significance at the 10% level is represented by *, at the 5% level by**, and at the 1% level by ***.

Table 5: Short-term results - financial variables

Dependent variable:	Current assets	Bank loans	Financial costs
Panel A: OLS estimates			
Treatment	0.017 (0.022)	-0.026 (0.109)	0.018 (0.022)
Panel B: IV estimates			
Treatment	0.030 (0.112)	-0.802 (1.170)	-0.120 (0.181)
Firm FE	YES	YES	YES
Sector X Year FE	YES	YES	YES
F-statistic (first stage)	63.3	63.3	63.3
Observations	47,222	47,222	47,222

Notes. The period of observation is 1997-2003. Treatment is a dummy equal to one for treated firms after the adoption of the policy (year 2000 and beyond). Treated firms are firms operating in municipalities involved in ACs. Control firms are firms operating in PS selected control municipalities. Standard errors clustered at city level in parenthesis. Significance at the 10% level is represented by *, at the 5% level by**, and at the 1% level by ***.

Table 6: Robustness checks - collapsed data set

Dependent variable:	Capital assets	Labor exp	Turnover
Panel A: OLS estimates			
Treatment	0.101** (0.047)	0.023 (0.039)	-0.042 (0.034)
Panel B: IV estimates			
Treatment	0.270 (0.343)	-0.080 (0.246)	-0.033 (0.196)
Firm FE	YES	YES	YES
Sector X Period FE	YES	YES	YES
F-statistic (first stage)	18.9	18.9	18.9
Observations	12,910	12,910	12,910

Notes. Time series informations are collapsed into a “pre” and “post” period (respectively, 1993-99 and 2000-07). Treatment is a dummy equal to one for treated firms after the adoption of the policy. Treated firms are firms operating in municipalities involved in ACs. Control firms are firms operating in PS selected control municipalities. Standard errors clustered at city level in parenthesis. Significance at the 10% level is represented by *, at the 5% level by**, and at the 1% level by***.

Table 7: Falsification test

Dependent variable:	Capital assets	Labor exp	Turnover
Instrument	-0.048 (0.070)	-0.004 (0.068)	-0.029 (0.045)
Firm FE	YES	YES	YES
Sector X Year FE	YES	YES	YES
Observations	53,989	53,989	53,989

Notes. OLS estimates. The period of observations is 1993-2007. The sample includes only control firms. Instrument is a dummy equal to one if the city and regional administrations were both led by centre-left in 1995. Standard errors clustered at city level in parenthesis. Significance at the 10% level is represented by *, at the 5% level by**, and at the 1% level by***.

Table 8: Placebo test

Dependent variable:	Capital assets	Labor exp	Turnover
1997	-0.145 (0.386)	-0.331 (0.337)	-0.123 (0.245)
1998	-0.097 (0.326)	-0.279 (0.310)	-0.074 (0.204)
1999	-0.110 (0.340)	-0.198 (0.256)	-0.063 (0.169)
2000	-0.124 (0.325)	-0.184 (0.228)	-0.097 (0.167)
2001	-0.093 (0.289)	-0.227 (0.300)	-0.084 (0.160)
2002	-0.111 (0.287)	-0.311 (0.424)	-0.162 (0.247)
2003	-0.084 (0.262)	-0.328 (0.450)	-0.199 (0.300)
Firm FE	YES	YES	YES
Sector X Year FE	YES	YES	YES
Observations	105,071	105,071	105,071

Notes. IV estimates. The period of observation is 1993-2007. Treatment is a dummy equal to one for treated firms in the placebo year and after. Treated firms are firms operating in municipalities involved in ACs. Control firms are firms operating in PS selected control municipalities Standard errors clustered at city level in parenthesis. Significance at the 10% level is represented by *, at the 5% level by**, and at the 1% level by ***.

Table 9: Aid intensity

Area Contract	Population (000)	Milions of lire	Public funds per capita
Ottana	5,802	157,960	27.23
Airola	7,622	124,207	16.30
Manfredonia	77,954	492,650	6.32
Crotone	70,839	246,756	3.48
Torrese - Stabiese	114,940	178,550	1.55
Gela	118,418	96,925	0.82
Sassari - Alghero	180,197	118,969	0.66
Sulcis - Iglesiente	140,028	78,733	0.49
Terni - Narni - Spoleto	162,977	50,778	0.31
Messina	260,543	37,850	0.15

Notes. Data on public funds are drawn from Presidency of the Council of Ministers (2004).

Table 10: Heterogenous effects - Aid intensity

Dependent variable:	High intensity			Low intensity		
	Capital assets	Labor exp	Turnover	Fixed assets	Labor exp	Turnover
	Panel A: OLS estimates					
Treatment	0.163** (0.080)	0.087 (0.121)	-0.060 (0.070)	0.087*** (0.043)	-0.020 (0.039)	-0.026 (0.037)
	Panel B: IV estimates					
Treatment	0.767** (0.381)	0.689 (0.580)	0.496 (0.309)	-0.022 (0.306)	-0.133 (0.244)	-0.050 (0.196)
Firm FE	YES	YES	YES	YES	YES	YES
Sector X Year FE	YES	YES	YES	YES	YES	YES
F-statistic (first stage)	71.6	71.6	71.6	151.4	151.4	151.4
Observations	21,469	21,469	21,469	83,602	83,602	83,602

Notes. Sample split according to the share of public funds over population. The period of observation is 1993-2007.

Treatment is a dummy equal to one for treated firms after the adoption of the policy (year 2000 and beyond). Treated firms are firms operating in municipalities involved in ACs. Control firms are firms operating in PS selected control municipalities. Standard errors clustered at city level in parenthesis. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 11: Heterogenous effects - Sector

Dependent variable:	Industry			Services		
	Capital assets	Labor exp	Turnover	Fixed assets	Labor exp	Turnover
	Panel A: OLS estimates					
Treatment	0.186*** (0.061)	0.043 (0.072)	0.006 (0.056)	0.043 (0.042)	-0.018 (0.048)	-0.054 (0.037)
	Panel B: IV estimates					
Treatment	-0.575 (1.659)	-0.123 (0.873)	-0.472 (1.201)	-0.046 (0.227)	-0.194 (0.211)	-0.031 (0.137)
Firm FE	YES	YES	YES	YES	YES	YES
Sector X Year FE	YES	YES	YES	YES	YES	YES
F-statistic (first stage)	108.3	108.3	108.3	103.6	103.6	103.6
Observations	33,458	33,458	33,458	71,613	71,613	71,613

Notes. Sample split according to firm's sector. The period of observation is 1993-2007. Treatment is a dummy equal to one for treated firms after the adoption of the policy (year 2000 and beyond). Treated firms are firms operating in municipalities involved in ACs. Control firms are firms operating in PS selected control municipalities. Standard errors clustered at city level in parenthesis. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 12: Heterogenous effects - Size

Dependent variable:	Smaller firms			Larger firms		
	Capital assets	Labor exp	Turnover	Fixed assets	Labor exp	Turnover
	Panel A: OLS estimates					
Treatment	0.065 (0.063)	-0.039 (0.048)	-0.046 (0.039)	0.150*** (0.049)	0.040 (0.048)	-0.003 (0.044)
	Panel B: IV estimates					
Treatment	0.529 (0.550)	-0.018 (0.336)	-0.070 (0.242)	-0.548 (0.651)	-0.289 (0.322)	-0.170 (0.216)
Firm FE	YES	YES	YES	YES	YES	YES
Sector X Year FE	YES	YES	YES	YES	YES	YES
F-statistic (first stage)	127.1	127.1	127.1	78.5	78.5	78.5
Observations	53,310	53,310	53,310	51,761	51,761	51,761

Notes. Sample split (above/below median) according to firm's asset. The period of observation is 1993-2007. Treatment is a dummy equal to one for treated firms after the adoption of the policy (year 2000 and beyond). Treated firms are firms operating in municipalities involved in ACs. Control firms are firms operating in PS selected control municipalities. Standard errors clustered at city level in parenthesis. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 13: Side effects - probability of re-election

	(1)	(2)	(3)
Treatment	0.095** (0.045)	0.097*** (0.039)	0.108*** (0.041)
Region FE	YES	YES	YES
Political controls	NO	YES	YES
Socio-demographics controls	NO	NO	YES
Observations	276	276	276
R^2	0.11	0.25	0.30

Notes. OLS estimates. The dependent variable is the share of city administrators in 1997 that were still in charge in 2003. Treatment is a dummy equal to one for treated municipalities. Political controls include year of election, number of city administrators per capita, a set of dummies for political parties in power, and an index of political competition (Herfindahl index of parties' share in the city council). Socio-demographics controls include population, density, dependency ratio, a proxy of human capital (percentage of population with with at least secondary education), and a proxy of civic engagement (voter turnout at general election). Robust standard errors in parenthesis. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.