Local Development that Money Can't Buy: Italy's *Contratti di Programma*[°]

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15th February 2012

Abstract

The paper evaluates the effectiveness of a major Italy's place-based policy (*Contratti di Programma*), through which the Government endorses and finances an industrialization plan proposed by private firms. By using as counterfactuals the areas that will be exposed to the same policy later in time, the study finds evidence of a positive impact on plants and employment, which is however confined to a small area (municipality) and does not extent to the local labor market area (aggregation of few neighbouring municipalities).

JEL Classification: R11, R58, C14.

Keywords: Regional economic activity, location-based policies, program evaluation.

[°] Part of this work was undertaken while Monica Andini was visiting the Structural Economics Analysis Department. We thank Anna Giunta, Paolo Sestito, Marco Paccagnella, Francesco D'Amuri, Andrea Linarello and the participants to the *Seminario di analisi economica territoriale* (Bank of Italy, December 2011) for comments and suggestions. Usual disclaimers apply.

1. Introduction

The rationale of location-based (or place-based) policies is now under close scrutiny. Little agreement, however, seems to be on the way. For instance, the World Bank's World Development Report (World Bank, 2009) argues that economic growth in itself is going to be spatially unbalanced, and try to spread it out might likely end in discouraging it. On the other hand, the OECD Reports on regional growth (OECD, 2009a and 2009b) strongly argue in favor of growth-enhancing policies that target lagging regions.[1] Against this background, the evaluation analyses of implemented programs are going to make a difference. For instance, policies proven to be effective in fostering local development will clearly suggest that the OECD vision squares with the facts more than its World Bank counterpart. In contrast, a lack of effectiveness will argue in favor of the Washingtonian view.

Even though the evaluation industry has expanded steadily during the last years (see: Banerjee and Duflo, 2009), the share of place-based policies that have been evaluated is still extraordinarily small compared to the thousands of programs implemented all over the world.[2] Because of the presence of a large area of underdevelopment (the largest in Europe) and a restless policy attitude to move resources towards poor territories, Italy is an extraordinary source of quasi-experimental evidence for evaluating location-based policies. This paper takes advantage of this fact and evaluates the effectiveness of one of the most important Italy's interventions. The program is named *Contratti di Programma* (Planning Contracts, PCs) and has the aim of stimulating industrialization in lagged areas. It is an agreement between the Central Government and the private firms, which can be both large firms established in non-lagged areas and SMEs already located in lagged territories. Public money follows the endorsement of a full-fledged industrial plan that sets targets mainly in terms of plants and employment.

Evaluating the PCs can be of some interest for both the policy makers and the economists.

i) The policy is an example of old-fashion intervention, in which the agreement is between the private sector and a centralized authority: no local stakeholders' involvement (ownership) is envisaged. Therefore, it could be useful to have a sense of whether those types of place-based program might work.[3]

ii) It is also important to acknowledge that the PCs were not implemented in a *vacuum*. Over the same period in which the program was into operation, other location-based programs were also underway. In particular, there were other two programs explicitly targeted towards territories: the Territorial Pacts, which were based on a bottom-up approach with a substantial role of the local community in agreeing on the development plan, and the Area Contracts, aimed at regenerating urban and industrial areas with large industrial plants in crisis. In

[1] A nice summary of these diverging views can be found in the discussion on Voeux.org between Indermit Gill, on the one hand, and Fabrizio Barca and Philip McCann, on the other (see: Vox, 2010a and 2010b). More provocative arguments against location-based policies can be found in the posts of Henry Overman (see: SERC, 2011). On the role of development traps, which is the economic mechanism that helps to rationalize the interventist view, see: Kline and Moretti, 2011.

[2] Overall, the evidence seems to point to a lack of effectiveness. However, the key lesson that emerges from this literature is that *the devil is in the details*. Similar program might have quite different effects, according to implementation features such as, for instance, the assignment mechanism, the types of recipients, and the timing of the program. Therefore, the fact that the majority of the programs so far assessed have been mostly ineffective does not imply that *all* location based policies will invariably be so.

[3] These old-fashion interventions were basically dismissed before evaluation techniques made their appearance.

addition, there was a major incentive scheme, the Law 488, intended to subsidize firms that were located in lagged areas. The contemporary presence of many programs poses serious challenges (tackled in the empirical section below) in evaluating a single program. As for the policy recommendations, however, comparing the results of one program with those from the contemporary ones (which refer to the same territories and are implemented over the same period of time) can be seen as extremely valuable. The comparison might uncover the relative merits of different types of programs, thereby providing useful hints for the design of location based policies.[4]

iii) Under the PC program two different aims are envisaged: a localization target (reputable producers are paid out to locate in disadvantaged territories in order to bring industrialization) – and a cooperation goal (SMEs established in lagged areas get the money to work together in order to benefit from agglomeration economies). The twofold target of the PCs allows us to gauge the respective virtues of two approaches that have received lots of attention in the long standing discussion on development tools.

Since 1986, the PCs have been implemented in a scattered way overtime. This is the aspect of the policy that we exploit to obtain identification. In particular, for the PCs financed starting from year 2000, on which the paper concentrates, we are able to compare PCs approved at the beginning of the decade (2001-2003) with PCs that will be approved only after some years (starting from 2008). Therefore, we are left with a period of time (our estimation window, 2001-2008) in which the group of treated municipalities is contrasted with a control group of future PCs (that is, municipalities that will be exposed to the same policy later in time). As shown, in Busso *et al.* (2011) among others, if the endorsement process is similar at the beginning and the end of the decade, this ought to yield a set of control municipalities with both observable and unobservable characteristics similar to those of the treated units.

As underscored by Glaeser and Gottlieb (2009), place-based policies are likely to deliver effects that go beyond those related to the area involved into the treatment. An example is that of local multipliers (Moretti, 2010), through which the increase in economic activity triggered by a program in one place might impact positively on the welfare of the surrounding places. On the other hand, the effects on the neighboring areas can be negative. This happens, for instance, when a program boosts economic activity in an assisted area at the expense of decreasing growth in an unassisted area. An important contribution of our study is to gauge whether these sorts of effects are going on.

We estimate the program effect on the 2001-2008 growth rates of plants and employment in the southern municipalities [5]. Our results provide evidence in favor of a positive impact of the program, which is however limited to small areas (the municipalities). In particular, the effect on the cities involved in the policy amounts to a 2001-2008 cumulative 6.3 percent increase in plants and 7 percent increase in employment (corresponding to annual growth slightly above 1 percent for both the outcomes). Unfortunately, the results do not survive to increasing the level of aggregation of the units of observation from municipalities to local labor markets,

[4]The Territorial Pacts have been evaluated by Accetturo and de Blasio (2011); the Law 488 by Bronzini and de Blasio (2006). Both exercises points to results of overall ineffectiveness. The evaluation of the Area Contracts is now underway by Accetturo, D'Ignazio and Franceschi (2011).

[5]In this period, the average annual GDP growth rate of the Mezzogiorno was equal to 0.2 percentage points, less than half of the Italian average annual GDP growth rate (0.7). All the southern regions showed homogenous dynamics (with a standard deviation of 0.28). The average annual per-capita GDP growth rate showed a similar pattern.

which include few surrounding municipalities. This happens because spatial crowding out effects materialize: the increase in economic activity for the treated municipalities comes at the expenses of the development of the surrounding municipalities. Finally, to capture potential impacts of the policy that might go beyond those on plants and employment, we use as outcomes aggregate measures of local economic wellbeing (population and real estate values). We find that the result pointing to a lack of effectiveness receives additional support.

The rest of the paper is organized as follows. Section 2 describes the program. In particular, it focuses on the features of PCs that are most relevant for the evaluation exercise. Section 3 describes the data and the identification strategy. Section 4 discusses the results. It first presents the baseline results together with extensive robustness. Then, it shows the findings related to spatial spillovers and population and housing values outcomes. Section 5 concludes.

2. The program

The *Planning Contracts* have the purpose to re-equilibrate development disparities by promoting *large* domestic and foreign industrial investments in the disadvantaged areas of the Italian territory. Table A in the Appendix lists the 121 PCs that have been implemented since the birth of the policy in 1986. Among the others, prominent PCs were those signed by Fiat (automobile), Barilla (food) and Texas Instruments (electronics).[6]

The date of approval has been quite dispersed over time (see also, Figure 1). The first two PCs were endorsed in 1988. For more than a decade, there have been no more than few PCs approved each year. Conversely, a surge in endorsements, also due to the availability of larger allocations following an EU decision [7], occurred at the beginning of the 2000s and in the last years of the decade.

[Figure 1]

The PC initiative represents one of the major Italy's place-based programs, in terms both of geographic coverage and amounts involved. At the end of 2010, 413 municipalities were exposed to the program. Total investments planned under the policy amounted to 21 billions of Euro (40% of which are financed by public funds). As backwardness in Italy is concentrated in the South, this area is overwhelmingly considered under the policy. 103 out of the 121 PCs include at least one southern municipality while 67% of the overall involved municipalities are located in the Mezzogiorno; the share of public funds channeled towards this area is as high as 94%. Figure 2 maps, over the southern territory, the municipalities that receive the PC financing. All southern regions have been considered under the policy (Puglia, Sicilia and, to a lesser extent, Sardinia, have relatively been more exposed).

[6] The table also includes a number of PCs (20 of them) that (at the time we write the paper) were already endorsed even though the formal approval had still to come (these PCs will be used, together with those endorsed since 2008, to construct the control groups of the future PCs; see: Section 3). The table does not include the 12 PCs for which the public disbursement was stopped as firms were not carrying out the investment that have pledged. These PCs are those that officially turned out in failures. They are not considered in the empirical exercise below. Excluding them from the exercise introduces a source of upward bias for the results. This however is not an issue, given the overall estimated ineffectiveness.

[7] A note of the EU Commission (n. SG (2000) D/105754) extended to PC some financing sources previously limited to other programs.

[Figure 2]

The program works on a bilateral "public-private" basis: it is an agreement between the Central Government and the private firms. Once the Government announces the availability of the allocations, the firms interested in the program apply by presenting a full-fledged *industrial plan*, which singles out the targets mainly[8] in terms of plants and employment[9] and takes note of the infrastructures needed.[10] Then, a negotiation process between the two sides takes place. According to the official PC guidelines (see: Law 64/1986; CIPE deliberation 10/1994), the negotiation process "follows the logic of the bilateral bargaining between public and private agents to match the reciprocal goals", and the contract is signed once the agreement is reached. On the features of the negotiate, little is known. The negotiation is conduced by an high-level policy committee (the Interdepartmental Committee for Economic Planning, *CIPE*), which relies on the advices of a technical commission. During the negotiate, public authorities might ask for variations to the initial plan submitted by the private firms. These requests might either be accommodated by the proponents or lead to refusal. Disbursement follows the endorsement according to an installment schedule, which is agreed at the time the contract is signed (and that can be stopped if the monitoring activity reveals that the firms are not carrying out the investments that have pledged). In principle, PCs can be implemented in both tradables and non tradables sectors. As a matter of fact, the bulk of initiatives refers to the sectors of tourism, manufacturing and agro-industry.[11] In 1990, the initiative, originally thought to stimulate large firms (or corporate groups) to locate in lagged areas, was made available also to SMEs *already* located in depressed areas.

3. Data and empirical strategy

Information on PCs has been collected through the archive of deliberations of the Interdepartmental Committee for Economic Planning. The effectiveness of the policy is mainly evaluated in terms of plants and employment growth rates, for the sectors of industry and non financial services. Data sources for both outcomes are from the Census, which is available for 2001, and the ASIA-UL archive, which provides annually Census-type information from 2004 onwards. As the latter source only records municipalities with more than 5,000 inhabitants, our sample has been accordingly restricted. We also make use of data on population and rents. They are taken, respectively, form the Italian Institute of Statistics (Istat) and the Observatory on Real Estate Market of the Territorial Agency.

The paper focuses on the PCs approved after year 2000. This allows us to get a sizable dataset by exploiting the fact that at the beginning of the decade (see: Figure 1) there was a boom

[8] Additional targets refer to research activity developed by the firms and training and re-qualification of new and old employees.

[9] Proponent firms must also present a detailed financial plan, which shows internal and external funding sources.

[10] The industrial plan might require investment in local (material or immaterial) infrastructures, which will be totally funded with public resources.

[11] Even though one of the aims of the PCs was to stimulate foreign direct investments, only 6 PCs were signed with non Italian companies.

in approvals. Our treatment group is made up of PCs endorsed during the period 2001-2003. This permits us to consider the 2001 Census information as a reasonable pre-treatment date.[12]

The unit of observation is the municipality.[13] This represents the most detailed level of stratification available with the data at hand. We start with a sample of 106 municipalities involved in 31 PCs approved in the period 2001-2003. Excluding the centre and north counterparts has the advantage of providing a more homogenous sample, as the Mezzogiorno differs from the rest of the country for a multiplicity of factors, such as access to markets, infrastructures, geography, cultural habits, etc. Therefore, by focusing only on southern territories we minimize the risk of mistakenly reflecting confounding factors, while the price we pay in terms of information loss is quite negligible (only 2 PCs[14], including 4 municipalities are from the Centre North). As the program was implemented continuously from 1988 to 2010, we drop from the treatment group both the municipalities that are treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards. This leaves us with 80 southern municipalities involved in 19 PCs approved in the period 2001-2003. As the data source for the outcomes of interest is the ASIA-UL archive, we can only focus on municipalities with more than 5,000 inhabitants. This leaves us with 56 treated cities. Table 1, Panel A summarizes the sample construction. Figure 3a plots the treatment group over a map of the South of Italy. Treated municipalities are located in Campania, Basilicata, Calabria, Sardinia and Sicilia.

[Table 1]

[Figure 3]

Treated municipalities are contrasted with a group of control municipalities, *i.e.* a group of municipalities that ought to mimic the behavior of the treated ones in absence of the program. The paper makes use of two control groups.

The first one is a standard one (Table 1, Panel B). It is made up of 49 municipalities selected through a propensity score (PS) matching among the 616 southern municipalities that never received the PC treatment. The PS-matching uses 2001 Census data at the city level for the following variables: the (log of) employees, the (log of) number of plants, the (log of) surface, the activity rate, the unemployment rate, the labor productivity,[15] the share of highly educated people. Moreover, it uses a measure of local public spending inefficiency.[16] Table 2, Panel A describes this sample. Pre-treatment values for the matching variables of the treated group are described in first column. The corresponding values for the 49 control municipalities are provided in the second column. For each variable, the p-value of the balancing property test

[12]Note also that the information available for our exercise is basically that provided in the Census, which is available only in 1981, 1991, 1996, and 2001. Therefore, only 26 out of 121 PCs approved between 1988 and 2000 would have been adequately endowed with reasonable pre-treatment information.

[13]However, we will also provide estimates at the higher level of aggregation, which is the local labor market that includes the municipality (see: Section 4.2).

[14]One of which involves both North and South municipalities.

[15]The productivity of labor is measured at the local labour market level.

[16]This was generously provided by Guglielmo Barone. Details on this measure can be found in Barone and Mocetti, 2011.

does not reject the null hypothesis of equality of means. For reference, we also report in the fourth column the average values for the 616 southern untreated cities among which our controls are PS chosen. They are largely different from the treatment group, as the test values reveal. Figure 3b plots the treated and the standard controls over the map.[17]

[Table 2]

The standard control group is a valid one provided that PS matching makes justice of all pre-treatment characteristics which might determine selection into treatment. This is not the case if some unobservables drive the likelihood of receiving the treatment. For instance, treated municipalities might be more likely those with worse infrastructures or those less endowed with social capital.[18] Typically, kind-hearted policy makers give more weight to the territories more in need of aid. However, even an opposite mechanism might be at work: firms might indeed be choosing the relatively less lagged areas among those that are eligible. [19]

As suggested by Busso *et al.* (2011) among others,[20] the group of future PCs – that is, municipalities that will be exposed to the same policy later in time – have the desirable feature of having both observable and unobservable characteristics similar to those of the treated cities, provided that the endorsement process is similar for the two groups.[21] Therefore, future PCs can provide a more suitable counterfactual. To construct this group we use the municipalities involved in a PC approved after 2008 (that is, the 18 PCs approved over 2008-2010 and the 20 PCs that in 2010 were waiting for the formal approval).[22] This leaves us with an estimation window that goes from 2001 to 2008, which is reasonable as our outcomes – the growth rates of plants and employment – will reflect the impact of the treatment over the medium term, that is after enough time for the effects to materialize. As reported in Table 1 (Panel C), our sample includes 74 municipalities involved in PCs approved after 2008. Similarly to what we have done for the standard control group, we PS-select 33 municipalities from the 74 future PCs. Table 2, Panel B reports the descriptives and the tests. Note that a high degree of similarity between treated and controls is already shown (see the test in Column 5) before running the PS-matching routine. This supports the idea that future PCs represent a more appropriate control group than the standard one. However, the PS-matching further levels differences out. Figure 3c plots the treated and the future PCs municipalities over the map.[23]

[17] Note that PS-selected standard controls also happen to be located in regions where no treated is located.

[18] To mention only two aspects (among the many) for which we have no data available at the municipality level.

[19] There could also be political economy mechanisms. For instance, the industrial plan submitted by the private firms might have more chances to get the approval if the municipalities involved are those belonging to the electoral constituency of the ruling central administration.

[20] See also Boarnet and Bogard (1996) and Bell *et al.* (1995).

[21] This requirement in our case is factual as no variation in the assignment mechanisms occurred from 2000 to 2010 (see: Giunta and Mantuano, 2010).

[22] In principle, we could have used PCs approved before 2008; this however would have had the undesirable implication of critical reducing the estimation window.

[23] Some sort of spatial mismatch at the regional level between treated and controls still remains. It is however lower that that with the standard control group.

4. Results

We start by showing (Section 4.1) our baseline results and corroborating them with a number of sensitivity checks. Then (Section 4.2), we study the extent of spatial spillovers and the possibility of effects that go beyond those on plants and employment.

4.1 *Baseline results and robustness*

4.1.1 *Baseline.* Table 3, Panels A displays the naïve estimates (mean differences) for plant and employee growth rates between the 56 treated municipalities and the 616 municipalities among which we will PS-select the group of 49 standard counterparts. Clearly, these results are hardly convincing, since they have been obtained by comparing groups featured by massive heterogeneity (see: Table 2). They would have suggested that the program is effective for plants (with a cumulative point estimate of 3.4%, which corresponds to an annual increase of roughly 0.5%) but not for employment. Panel B presents the estimates of the ATT (average treatment effect on the treated) calculated by using the nearest neighbor matching routine (with the replacement option on) for the comparison between treated cities and the 49 PS-selected standard untreated. Under the unconfoundeness assumption, according to which the treatment status of units identical in terms of observables is determined only by chance, these estimates would suggest a result of full ineffectiveness, both for plants and employment. As explained in the previous section, we believe that the unconfoundeness cannot be taken for granted and that a more suitable control group is provided by future PCs.

Panel C displays the naïve estimates we obtain by contrasting the treated with all the 74 available municipalities that started to be considered under the program in 2008. Note that these estimates suggest a positive impact for plant (with a point estimate of 5.5%, highly significant); as for employment, the estimated effect is lower (2.4%) and it is not significant. These results highlight that the previous findings were likely to be plagued by a downward omitted variable bias, which makes sense if the assignment mechanism is biased in favor of underperforming municipalities.[24] Panel D makes this case even stronger. When we estimate the impact of the program by using as counterfactuals only the 33 PS-matched (future PCs) untreated, we find that the (nearest neighbor matching) ATT (average treatment effect on the treated) is equal to 6.3% for plants and 7.0% for employment[25] (which amount to 1.13% and 1.25% annual growth rates,[26] respectively). Both estimates receive high statistical significance.

[24] To the extent that the underperformance is captured by the overtime pattern of plants and employment, this occurrence might be tested. We can calculate the growth rates for the two variables over a pre-treatment interval and check their similarity for the two groups of municipalities that are being compared. We have done this by using 1991-2001 data for the treated and 1996-2007 data for the future treated. Pre-treatment growth rates are basically the same for plants. However, for employment treated cities show lower growth rates than the untreated ones. Therefore, we also estimate the impact of the program by selecting among the untreated only those with an employment pre-intervention growth rate in line with that of the treated (see Blundell *et al.*, 2004, and Bronzini and de Blasio, 2006). As the results (not shown but available upon request) are extremely similar to those shown in Table 3 Panel D, we conclude that the downward omitted bias cannot adequately be captured by the past (observable) pattern of our outcomes.

[25] Considering the amounts spent by the Government, our estimates suggest that one additional job has been paid slightly over than 26.000 Euro (which is a reasonable amount compared to figures refereeing to other Italian policies).

[26] Annual growth rates are calculated taking into account that the treatment started in 2001 for 6 municipalities, in 2002 for 21 municipalities and in 2003 for 29 municipalities. The cumulative average duration is therefore equal to 5.59 years. Therefore, they are measured as a weighted average of the treatment duration with weights equal to the fraction of municipalities that become treated, respectively, since 2001, 2002 and 2003.

We label this last set of results as our baseline.[27]

[Table 3]

4.1.2 *Robustness to alternative routines.* Table 4 provides a first robustness check. It shows that our estimates are rather insensitive to using different routines to estimate the ATT (for all routines, results have been obtained under the common support restriction; see: Dehejia and Wahba, 1999 and 2002). The nearest neighbor matching method matches each treated with the control unit that has the closest propensity score (i.e. the nearest neighbor) and, allowing for replacement, a control unit can be the best match for more than one treated unit (as it happens in our case). The advantage of this method is that all treated units find a match but poor matches can occur if units with fairly different propensity score end up to be matched. Given this limitation, we follow the rule-of-thumb of double-checking the findings with alternative routines. As highlighted by Ichino and Nannicini (2002), none of the available alternatives is a priori superior to the nearest neighbor matching; however, their joint adoption is useful to asses the robustness of the estimates. Panel A presents the results we obtain by using the stratification method. This method computes the ATT as a weighted average of the ATT computed in blocks such that within each block treated and controls have on average the same propensity score, with weights given by the distribution of treated units across blocks. This approach discards observations in blocks where either treated or controls are absent. Panel B provides results obtained by using the radius matching method. The latter matches treated units with controls whose propensity score belongs to a neighborhood (*i.e.* the radius) with a dimension that is arbitrarily chosen by the researcher. A small radius might generate higher quality matches at the cost of unmatched treated units. A bigger radius might increase the number of matches at the cost of lower quality matches. We use a radius equal to 0.1, the minimum necessary in order not to loose unmatched treated observations. Panel C presents the results we obtain by using the kernel matching method. This routine matches all treated units with a weighted average of all controls, with weights inversely proportional to the distance between the propensity scores of treated and controls. As shown in the table, our evidence is robust to the choice of a particular routine, with the only exception of the estimation of the ATT for employment with the radius method.

[Table 4]

4.1.3 *Robustness for concurrent programs.* Next, we control for the confounding effects that might derive from the fact that, over our estimation period, other location-based programs were also underway. As explained in Section 1, the major concurrent programs were the Territorial Pacts (TPs), the Area Contracts (ACs), and the Law 488 (L488). The presence of concurrent initiatives might bias our results and the sign of the distortion is not known *a*

[27] To investigate the role of regional mismatch between treated and controls for the results reported, we have replicated the specifications of Table 3 either by including a full set of regional fixed effects or imposing that a control must be located in the same region of its treated match (in this last experiment, the number of untreated PS-selected municipalities in both Panel B and Panel D are reduced). Results from these checks are however very similar to those shown in Table 3 (they are not reported but are available upon request).

priori: it will be an upward bias if treated receive also extra aid on the top of that provided by PCs; it will be a downward bias if controls are considered by the other location-based initiatives. Note that the overlap of programs in our sample is substantial: among the 56 treated, 29 are involved in TPs, 5 in ACs, and 53 receive L488 funds (28 of which are involved also in the other two programs); among the 33 untreated, 18 are involved in PTs, 4 in ACs, and 31 receive L488 funds (17 of which are involved also in the other two programs). Therefore, the overwhelming majority of our sample of municipalities is involved in concurrent programs. However, the extent of involvement is quite balanced between treated (96%) and controls (96%). The results shown in Table 5 are derived by computing the ATT via a weighted regression method (with the weights equal to those used to provide results in Table 3, Panel D) where, beyond the treatment indicator, we include a dummy that takes the value of one if the city is included into a Territorial Pact, a dummy that takes the value of one if the municipality belongs to an Area Contract and a dummy that takes the value of one if the city received a non-zero share of Law 488 funds.[28]As matter of fact, controlling for the existence of concomitant programs (Panel A), we find that the estimated effect of PCs is moderately lower for both plants and employment, while remaining highly significant. Panel B presents the same exercise by using as measure for the L488 financing the share of funding received by the municipality (instead of the dummy). These results are moderately higher than those of the baseline. All in all, it seems safe to conclude that the bias caused by concurrent policies can be deemed as negligible for our results.

[Table 5]

4.1.4 *Robustness for funding heterogeneity.* An important check refers to the role of funding for the effectiveness. The distribution of public money across municipalities is not uniform: 3 municipalities (Battipaglia, Bernalda, and Nocera Inferiore) receive an overwhelming share of funds. While the sample average amounts to 6.12 millions of Euro, dropping the 3 highest-subsidized municipalities (which correspond to the 95^{th} percentile of the distribution of the fund shares) reduces the average injection of funds to 3.83 millions.[29] Therefore, we are concerned that these cities might be driving our results. Table 6, Panel A shows that this is not the case: by dropping the municipalities corresponding to the 95^{th} percentile of the fund shares distribution, the results nicely mirror those of the baseline. We also find that effectiveness is lower for the municipalities that receive a relatively minor share of funds. Panel B estimates the impact of the program for a sample that excludes the 12 lowest-subsidized cities (5^{th} percentile of the distribution of funding). The results are consistently higher than those of the baseline. Finally, Panel C presents the results for a sample that drops both the 5^{th} and the 95^{th} percentiles. The general impression is that effectiveness is higher for intermediates intensities of financing.

[Table 6]

[28]A more drastic robustness check would have been dropping municipalities treated under other programs (see: Accetturo and de Blasio, 2011). Given the low number of observations and the high degree of overlaps, this strategy is however not available with our data.

[29]A similar ranking is obtained by using the average per-capita subsidy.

4.1.5 *Robustness for types of PCs.* As explained in previous sections, PCs provide two types of incentives. One is to stimulate large firms to locate in lagged areas. The other is to subsidize local increases in activities for SMEs established in retarded areas. Note that the relative merits of these two different policies are, since the end of WWII, at the heart of the discussion on development tools. For instance, the idea that industrialization can be sustained by attracting plants from multinationals has informed during the Sixties a whole phase of the policies promoted by the World Bank. Then, it was dismissed in favor of policies stressing the role of small and medium-sized enterprises and start-ups.[30] Table 7 provides a first cut at this issue. In Panel A we consider only the municipalities involved in PCs stipulated by SMEs. While the estimated ATT for plants does not change, the one for employment reduces now to 5% (with a statistical significance far from conventionally acceptable levels).[31,32] Panel B provides the estimates for the baseline controlling for the presence of concurrent programs (as in Table 5, Panel A). Broadly speaking, we find that the two types of policies have similar effects (the impact seems slightly higher for localization measures).

[Table 7]

4.2 *Extensions*

4.2.1 *The impact on surroundings.* We now investigate the spatial extent of the results so far described. As a consequence of the PC program, spillover effects might materialize. On the one hand, the increase in economic activity in one city might impact positively on the welfare of the surrounding municipalities, through a local multiplier mechanism (see: Moretti, 2010; and de Blasio and Menon, 2011). On the other hand, by altering the structure of location incentives for footloose firms and households (see: Glaeser and Gottlieb, 2009) the program might trigger a substitution of economic activity from the surroundings to the treated areas. For instance, this finding has emerged as the main obstacle for the effectiveness of the US Enterprise Zones (see: Elvery (2009), Lynch and Zax (2011), Boarnet and Bogart (1996)).[33]

To give a first cut at this issue, we move to the (higher) level of aggregation provided by the local labor markets (LLMs).[34] For instance, if the effect found at the municipality level goes

[30] As highlighted by Braunerhjelm et al (2000), a similar shift had occurred in the place-based policies operated in Italy.

[31] The reduced estimated employment impact for this type of PCs could be related to the lack of planning capacity of small firms. For instance, practitioners highlight that it is difficult for these firms to anticipate the increase in plants and employment that can be sustained overtime. This would contrast with the technical abilities of large enterprises, for which the investment and its financing are recurrent business activity (indeed, they have accurate planning and budgeting procedures in place). To investigate this possibility, we have calculated the impact of PCs stipulated by SMEs over estimation windows of varying lengths (3-, 4- and 6-years after the start of the policy, respectively). A lack of planning capacity should be signaled by ATTs that decrease overtime. This however is not supported by the data.

[32] Our findings therefore contrast with those of Billings (2009), who focuses on the Colorado Enterprise Zones and finds a positive effect on employment of existing establishments and a non-significant effect on the location of new business units.

[33] Similar issues are highlighted by Criscuolo *et al.* (2007) for the English RSA program.

[34] Local labor markets are defined by the Italian National Institute of Statistic (Istat, 1997). They are aggregations of two or more neighbouring municipalities based on daily commuting flows from place of residence to place of work as recorded in the 2001 Population Census. Local labor markets are thus largely 'selfcontained': within a given unit, both the share of working residents working locally and the share of employees

hand in hand with a similar impact at the LLM level - which also includes surrounding municipalities - then positive spillovers are called for. Table 8, Panel A provides the estimates for the baseline where the outcomes at the municipality-level have been replaced by the outcomes at the LLM-level for each of the 56 traded and 33 controls. These results point to an impact that is quite reduced for plants and basically zero for employment.

The fact that the impact is lost by moving from city to LLM can in principle be due to the fact that the other municipalities in the control LLM receive aid from the concurrent location-based programs while this does not happen for the municipalities in the treated LLM. However, this does not happen to be the case. Panel B provides the estimates obtained by controlling for the presence of alterative funding at the level of LLM. In particular, we focus only on LLMs in which no other municipality (but the treated or the untreated cities, for which we have the appropriate controls – those of the specification of Table 5, Panel A – in place) is involved in concurrent programs. Results suggest that the lack of impact at the LLM level is unlikely to be driven by the existence of concurrent programs.

Note that the results in the first two panels of Table 8 are derived by replacing the outcomes at the municipality level with the same outcomes at the LLM level for our sample of PS-selected future PCs municipalities. These experiments highlight what happens at the higher level of aggregation for the municipalities for which the analysis has been so far conducted. However, the appropriateness of the two groups of treated and controls can be questioned as it is derived by comparing units at the municipality level (and not at the LLM one). To lesser this concern, Table 8, Panel C provides the results we obtain by replicating the entire exercise at the LLM level. Therefore, we start from the treated LLMs (over the 2001-2003 period) and compare them with PS-selected LLMs among of future PCs. Again, for this sample (which includes 30 treated and 14 untreated local labor markets) we find that the program at this level of aggregation does not show to be effective in increasing both plants and unemployment.

In principle, the fact that the effect on municipalities evaporates by moving to local labor markets might be due to the dilution of the treatment over a wider area (attenuation). However, by comparing the outcome performances of untreated municipalities located in *treated* LLMs with the performances of untreated municipalities located in *untreated* LLMs (Table 8, Panel D), we find that the first ones do worse than the latter.[35] Altogether, these results suggest that spatial substitution, not attenuation, is behind our findings.

[Table 8]

residing locally must be at least 75%. This definition is consistent with standard definitions of cities in urban economics that define them through commuting patterns. It is also consistent with the notion of 'functional region', defined as 'a territorial unit resulting from the organization of social and economic relations in that its boundaries do not reflect geographical particularities or historical events' (OECD, 2002). Italian local labor markets also roughly follow the criteria used to define Metropolitan Statistical Areas in the US, Travel to Work Areas in the UK, or Metropolitan areas and employment areas in France. Italian local labor markets span the entire national territory. In 2001, 686 of them were defined. They had an average population of 83,084 and a standard deviation of 222,418.

[35]Results are obtained by replacing each treated and control municipality outcome with weighted averages of the outcomes for the surrounding untreated municipalities (with weights proportional to their surface and population).

4.2.2 *Effects on population and rents.* Finally, we check whether the program might have had effects beyond those on plants and employment. This possibility cannot be easily dismissed. For instance, as documented in Section 2, the industrial plan could also foresee that firms increase their R&D activity or provide training to the workforce. Even more importantly, as an effect of the approval of a PC, relevant infrastructures might be delivered to the area. It is therefore plausible that having a PC in place might deliver benefits for the local community, which are not capitalized in additional plants and employment.[36] Since data on the wide arrays of the potential payoffs are not available, we turn to (reduced-form) estimates of the impact of PCs the overall economic activity of the area. As underscored by the literature of regional science and urban economics, residential choices are motivated by the benefits accruing to mobile households.[37] Moreover, Roback-type models of spatial equilibrium (Glaeser, 2008) underscore that location-specific factors that positively affect both the productivity of the firms and the welfare of the households will result in higher prices for non-tradable factors, such as houses. In Table 9 we test whether the impact of PCs translates in higher population and house price growth rates. We find (Panel A) that in the baseline this is not the case for both outcomes. Also no effect is found when we move to LLM outcomes (Panel B). Results are still there when we consider only PCs for SMEs (both at muncipality level, Panel C, and at LLM level, Panel D) for the which training and infrastructure provisions are relatively more important.

[Table 9]

5. Conclusions

In this paper we evaluate the effectiveness of the so called *Planning Contracts*, a major Italy's place-based policy with the purpose to re-equilibrate development disparities by promoting *large* industrial investments in the disadvantaged areas of the Italian territory. By using as counterfactuals the areas that will be exposed to the same policy later in time, we find evidence of a positive impact on both plants and employment at municipality level, which however does not extend to the local labor market. We also find that incentives to large firms have impacts that, at the municipality-level, are only moderately higher than those for SMEs. Finally, to capture the potential policy impacts that might go beyond those on plants and employment, we use population and real estate values as aggregate measures of local economic wellbeing. We find that the result pointing to a lack of effectiveness receives additional support.

These results suggest a couple of things. First, the effectiveness is limited to micro-geographic areas. Crucially, benefits accruing to a city come at the expenses of the surroundings. This highlights that it might be better for a municipality that is not included into a policy to stay away from those involved or, even, lobbying for avoiding that the neighbour receives the treatment. Thus, the PCs might undermine the incentive for neighbouring municipalities to

[36] This is the first line of defence for the advocates of place-based policies when they are confronted with negative evaluations. As once a high-level official from am important Italian Ministry said: "All right, you guys are saying that the program did not bring additional plants and employment. What about the accumulation of physical and human capita that was provided because of the program? Those local communities are now better off!"

[37] The usual assumption is that individuals care about the local labor market conditions and the prices of a bundle of other location-specific amenities.

work together to improve their economic conditions. Second, this piece of evidence has to be read against the background of the disappointing results of other place-based policies in Italy. This highlights that in the case of this country (very unfortunately) the *devil is not in the details*. On the contrary, irrespectively of the single details of the program (that is irrespectively of the bottom-up/top-down approach; the fact that money goes to large/small firms; the assignment mechanism; etc), a lack of effectiveness prevails.

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Figure 1: Number of PCs per year of approval date and types

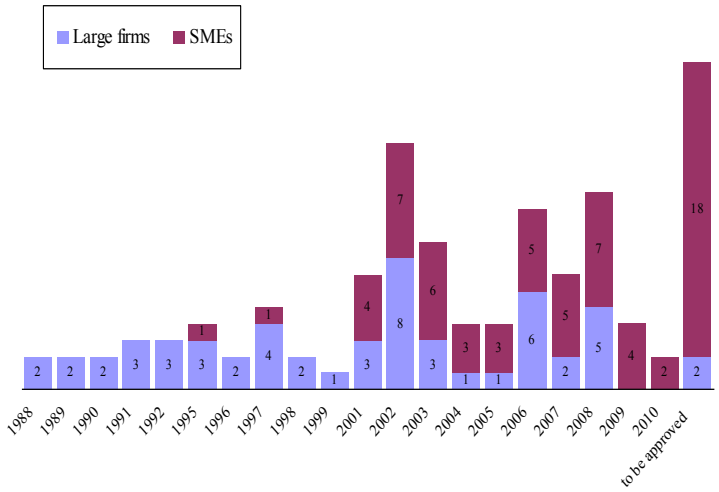
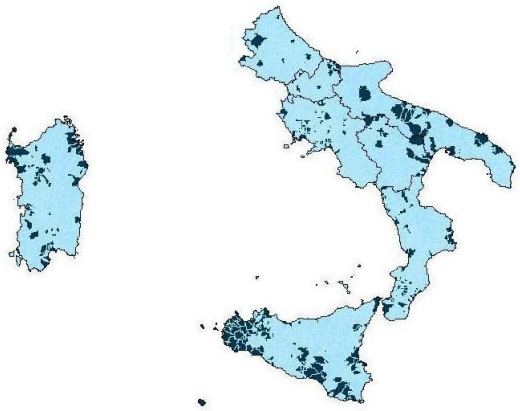


Figure 2: Southern municipalities that receive the PC financing



*Notes:*Figure 1: The Figure includes 20 PCs that have already been endorsed even though the formal approval has still to come. The Figure excludes the 12 PCs for which the public disbursement was stopped as firms were not carrying out the investment that have pledged. Figure 2: The Figure includes municipalities involved in the 20 PCs that have already been endorsed even though the formal approval has still to come. The Figure excludes municipalities involved in the 12 PCs for which the public disbursement was stopped as firms were not carrying out the investment that have.

Figure 3: Municipalities in the sample

Figure 3a: Treated municipalities

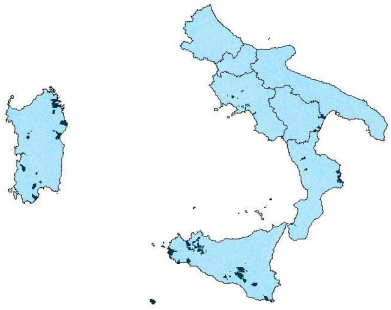


Figure 3b: Treated and (PS-Selected Standard) Controls

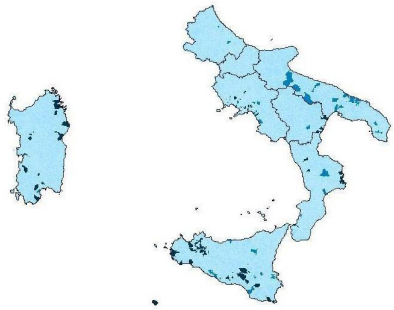
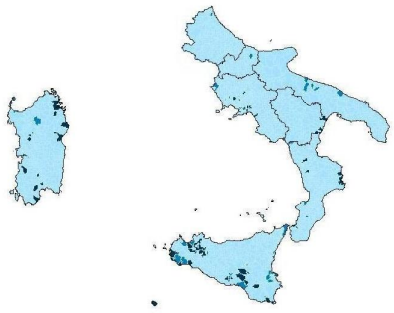


Figure 3c: Treated and (PS-Selected Future PCs) Controls



*Notes:*Figure 3a: Treated group (56 municipalities involved in PCs in the period 2001-2003.) over the map of the South of Italy. Both the municipalities that are treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. Figures 3b: Treated municipalities (dark blue); PS-Selected Standard control municipalities (light blue). To construct the control group the PS-matching has been used. Figures 3c: Treated municipalities (dark blue); PS-Selected Future PCs control municipalities (light blue). To construct the control group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses 2001 Census data at the city level for the following variables: the (log of) employees, the (log of) number of plants, the (log of) surface, the activity rate, the unemployment rate, the labor productivity , the share of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011).

Table 1: Sample construction

Panel A. Treated group	
Number of municipalities involved in PCs in 2001-03	106
Number of southern municipalities involved in PCs in 2001-03	102
Dropping southern municipalities already treated in other periods	80
Dropping southern municipalities with less than 5,000 inhabitants	56
Panel B. Standard control group	
Number of municipalities not involved in PCs in 2001-03	7785
Number of southern municipalities not involved in PCs in 2001-03	2455
Dropping southern municipalities already treated in other periods	2281
Dropping southern municipalities with less than 5,000 inhabitants	616
PS-selected southern municipalities	49
Panel C. Future PCs control group	
Number of municipalities involved in PCs since 2008	211
Number of southern municipalities involved in PCs since 2008	99
Dropping southern municipalities already treated before 2008	74
Dropping southern municipalities with less than 5,000 inhabitants	74
PS-selected southern municipalities	33

Notes: Data sources are: Census (which is available for 2001) and ASIA-UL archive (available from 2004 onwards). Information on PCs has been collected through the archive of deliberations of the Interdepartmental Committee for Economic Planning.

Table 2: Summary Statistics

Panel A. Standard control group					
Covariate	Treated	PS-Controls	BP Test	Untreated	DM Test
	56	49	(p-value)	616	(p-value)
Ln(Plants)	6.893	6.969	0.619	6.451	0.000
Ln(Employees)	7.734	7.815	0.626	7.12	0.000
Unemploym. Rate	0.248	0.238	0.424	0.158	0.059
Ln(Surface)	3.897	3.914	0.945	3.595	0.059
Share of High Educated	5.501	5.234	0.554	5.315	0.539
Activity Rate	44.325	44.514	0.824	43.759	0.304
Labour Productivity	3.897	3.893	0.969	3.851	0.709
Inefficiency	6.938	6.907	0.43	6.843	0.000
Panel B. Future PCs control group					
Covariate	Treated	PS-Controls	BP Test	Untreated	DM Test
	56	33	(p-value)	74	(p-value)
Ln(Plants)	6.893	7.032	0.462	6.892	0.991
Ln(Employees)	7.734	7.806	0.732	7.591	0.423
Unemploym. Rate	0.248	0.24	0.499	0.164	0.221
Ln(Surface)	3.897	4.135	0.361	4.117	0.294
Share of High Educated	5.501	5.878	0.453	6.254	0.063
Activity Rate	44.325	43.277	0.26	43.612	0.322
Labour Productivity	3.897	3.94	0.743	3.767	0.326
Inefficiency	6.938	6.933	0.903	6.902	0.385

Notes: Treated. To construct this group both the municipalities that are treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-Selected Standard controls. To construct this group the PS-matching procedure is used. PS-matching uses 2001 Census data at the city level for the following variables: the (log of) employees, the (log of) number of plants, the (log of) surface, the activity rate, the unemployment rate, the labor productivity , the share of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). BP Test stands for Balancing Property Test. DM Test stands for Difference in Means Test. PS-Selected Future PCs controls. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses those previously described.

Table 3: Baseline results

Panel A. Standard control group. Naive estimation					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	616	0.034	0.015	2.28
Employees	56	616	0.017	0.023	0.73
Panel B. PS-Selected Standard control group. Nearest neighbour matching					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	49	-0.023	0.024	-0.958
Employees	56	49	0.001	0.032	0.04
Panel C. Future PCs control group. Naive estimation					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	74	0.055	0.018	2.97
Employees	56	74	0.024	0.026	0.92
Panel D. PS-Selected Future PCs control group. Nearest neighbour matching					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	33	0.063	0.022	2.889
Employees	56	33	0.070	0.04	1.747

Notes: Treated. Municipalities involved in PCs in the period 2001-2003. To construct this group both the municipalities that are treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-Selected Standard control s. To construct this group the PS-matching procedure is used. PS-matching uses 2001 Census data at the city level for the following variables: the (log of) employees, the (log of) number of plants, the (log of) surface, the activity rate, the unemployment rate, the labor productivity , the share of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). PS-Selected Future PCs controls. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses those previously described. Panel A - C: Coefficients estimated with ordinary least squares method. Panel B - D: Coefficients estimated with nearest neighbour matching method. Bootstrapped standard errors.

Table 4: Robustness for alternative matching routines

Panel A. Stratification matching					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	71	0.052	0.02	2.544
Employees	56	71	0.067	0.031	2.185
Panel B. Radius matching					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	71	0.053	0.022	2.373
Employees	56	71	0.026	0.027	0.959
Panel C. Kernel matching					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	71	0.053	0.02	2.703
Employees	56	71	0.059	0.03	1.974

Notes: Treated. Municipalities involved in PCs in the period 2001-2003. To construct this group both the municipalities that are treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-Selected Future PCs controls. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses 2001 Census data at the city level for the following variables: the (log of) employees, the (log of) number of plants, the (log of) surface, the activity rate, the unemployment rate, the labor productivity , the share of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). Panel B: ATT estimated with radius equal to 0.1. Bootstrapped standard errors.

Table 5: Robustness for concurrent programs

Panel A. Dummy for TP and AC; Dummy for Law 488					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	33	0.055	0.019	2.77
Employees	56	33	0.061	0.034	1.79
Panel B. Dummy for TP and AC; Share of financing for Law 488					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	33	0.073	0.020	3.65
Employees	56	33	0.076	0.037	2.05

Notes: Treated. Municipalities involved in PCs in the period 2001-2003. To construct this group both the municipalities that are treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-Selected Future PCs controls. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses 2001 Census data at the city level for the following variables: the (log of) employees, the (log of) number of plants, the (log of) surface, the activity rate, the unemployment rate, the labor productivity , the share of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). ATT estimated with weighted regression method. Robust standard errors.

Table 6: Robustness for funding heterogeneity

Panel A. Drop the 95th percentile					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	53	32	0.063	0.024	2.68
Employees	53	32	0.074	0.036	2.047
Panel B. Drop the 5th percentile					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	44	26	0.085	0.025	3.466
Employees	44	26	0.094	0.044	2.149
Panel C. Drop the 5th and 95th percentiles					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	41	24	0.088	0.02	4.467
Employees	41	24	0.101	0.044	2.293

Notes: Treated. Municipalities involved in PCs in the period 2001-2003. To construct this group both the municipalities that are treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-Selected Future PCs controls. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses 2001 Census data at the city level for the following variables: the (log of) employees, the (log of) number of plants, the (log of) surface, the activity rate, the unemployment rate, the labor productivity , the share of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). ATT estimated with nearest neighbour matching method. Bootstrapped standard errors.

Table 7: Robustness for types of PCs

Panel A. Only SMEs					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	49	29	0.062	0.023	2.63
Employees	49	29	0.051	0.040	1.27
Panel B. Only SMEs. Controlling for concurrent programs					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	49	29	0.054	0.022	2.45
Employees	49	29	0.047	0.037	1.25

Notes: Treated. Municipalities involved in PCs in the period 2001-2003. To construct this group both the municipalities that are treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-Selected Future PCs controls. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses 2001 Census data at the city level for the following variables: the (log of) employees, the (log of) number of plants, the (log of) surface, the activity rate, the unemployment rate, the labor productivity , the share of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). ATT estimated with nearest neighbour matching method. Bootstrapped standard errors.

Table 8: The impact on surroundings

Panel A. Local labor market outcomes					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	33	0.023	0.014	1.65
Employees	56	33	-0.009	0.022	-0.42
Panel B. Local labor market outcomes. Controlling for concurrent programs					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	47	19	0.019	0.017	1.10
Employees	47	19	-0.026	0.025	-1.01
Panel C. Local labor market outcomes. Matching on local labor markets					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	30	14	0.015	0.018	0.833
Employees	30	14	-0.015	0.036	-0.405
Panel D. Untreated surroundings of treated and untreated municipalities					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	33	-0.056	0.033	-1.69
Employees	56	33	-0.048	0.027	-1.77

Notes: Local labor market outcomes for Treated and PS-Selected Future PCs controls. Treated. Municipalities involved in PCs in the period 2001-2003. To construct this group both the municipalities that are treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-Selected Future PCs controls. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses 2001 Census data at the city level for the following variables: the (log of) employees, the (log of) number of plants, the (log of) surface, the activity rate, the unemployment rate, the labor productivity , the share of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). Untreated areas outcomes are obtained by replacing each treated and control municipality outcome with weighted averages of the outcomes for the surrounding untreated municipalities (with weights proportional to their surface and population). Panel A - C: Coefficients estimated with nearest neighbour matching method. Bootstrapped standard errors. Panel D: Coefficients estimated with ordinary least squares method.

Table 9: Effects on population and rents

Panel A. Baseline					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Population	56	33	0.007	0.013	0.55
Rents	56	33	0.061	0.043	1.41
Panel B. Baseline. Local labor markets					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Population	56	33	-0.009	0.009	-1.01
Rents	56	33	0.038	0.045	0.86
Panel C. Only SMEs					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Population	49	29	0.005	0.015	0.39
Rents	49	29	0.029	0.037	0.79
Panel D. Only SMEs. Local labor markets					
Dependent Variable	Treated	Untreated	ATT	s.e.	t-stat
Population	49	29	-0.004	0.01	-0.45
Rents	49	29	0.014	0.049	0.29

Notes: Panel A: Treated. Municipalities involved in PCs in the period 2001-2003. To construct this group both the municipalities that are treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-Selected Future PCs controls. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses 2001 Census data at the city level for the following variables: the (log of) employees, the (log of) number of plants, the (log of) surface, the activity rate, the unemployment rate, the labor productivity , the share of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). Panel B: Local labor market outcomes for treated and PS-Selected Future PCs controls. Panel C: Only municipalities involved in PCs signed by SMEs have been considered. Panel D: Local labor market outcomes for treated and PS-Selected Future PCs controls. Only municipalities involved in PCs signed by SMEs have been considered. ATT estimated with nearest neighbour matching method. Bootstrapped standard errors.

Appendix

Table A: PCs implemented since their emergence in 1986

Name of the PC	Date of approval	Number of municipalities	Located in the South	Sector	Planned investments	Share of public funds
FIAT1	13/04/1988	21	YES	Manufacturing	1829.45	0.55
OLIVETTI	28/07/1988	6	YES	Informatics	0.40	0.75
IRI	17/05/1989	14	YES	Manufacturing	747.26	0.56
TEXAS1	07/11/1989	3	YES	Informatics	870.80	0.56
GTC	24/04/1990	1	YES	Manufacturing	99.89	0.46
BULL HN	10/05/1990	1	YES	Informatics	82.72	0.63
ENI	03/04/1991	5	YES	Energy	0.69	0.36
IBM	23/10/1991	3	YES	Informatics	0.03	0.75
FIAT2	05/11/1991	9	YES	Manufacturing	3232.92	0.45
SNIA BDP	04/02/1992	6	YES	Manufacturing	789.50	0.48
PIAGGIO	26/02/1992	3	YES	Manufacturing	0.14	0.32
BARILLA	14/04/1992	4	YES	Manufacturing	444.10	0.42
SARAS1	19/06/1995	2	YES	Manufacturing	366.53	0.32
TARI	23/06/1995	1	YES	Manufacturing	54.31	0.63
ACM	27/06/1995	9	YES	Manufacturing	0.29	0.55
COMPLASINT	27/06/1995	1	YES	Manufacturing	0.05	0.51
NATUZZI	31/10/1996	7	YES	Manufacturing	69.77	0.50
IPM	06/12/1996	4	YES	Manufacturing	73.78	0.65
UNICA1	09/04/1997	1	YES	Manufacturing	44.28	0.65
GETRAG	09/07/1997	1	YES	Manufacturing	210.54	0.52
SGS THOMSON	09/07/1997	1	YES	Manufacturing	305.59	0.56
SARAS2	10/10/1997	1	YES	Manufacturing	250.42	0.52
UNICA2	29/10/1997	1	YES	Manufacturing	45.41	0.66
NUOVA CONCORDIA	09/01/1998	1	YES	Tourism	45.41	0.66
TELIT	24/03/1998	2	YES	Manufacturing	80.77	0.58
EDS	21/10/1999	1	YES	Services	20.30	0.58
TARANTO CONT. TERM.	13/09/2001	1	YES	Manufacturing	41.00	0.55
CTM CENTRO TESSILE	04/10/2001	1	YES	Manufacturing	78.77	0.61
CONSORZIO MADIA DIANA	11/10/2001	1	YES	Agro-industry	49.20	0.65
LEAR PROMA	17/12/2001	7	YES	Manufacturing	55.00	0.40
IMPRECO	20/12/2001	2	YES	Manufacturing	164.76	0.70
TRAPANI TURISMO	21/12/2001	14	YES	Tourism	90.12	0.57
ATLANTIS	24/12/2001	3	YES	Manufacturing	21.18	0.67
SAM	23/01/2002	7	YES	Manufacturing	52.68	0.66
7C ITALIA	11/02/2002	1	YES	Services	8.24	0.49
BOSCH	13/02/2002	1	YES	Manufacturing	198.29	0.46
ATITECH	22/04/2002	1	YES	Services	23.53	0.40
SANDALIA	23/04/2002	4	YES	Tourism	87.66	0.44
DISTRETTO ELETTRODOMESTICO	24/05/2002	12	YES	Manufacturing	109.32	0.45
CONSORZIO ALISAN	29/05/2002	5	YES	Agro-industry	87.15	0.66
SARAS3	10/06/2002	3	YES	Manufacturing	65.93	0.46
CONSORZIO LATTE	09/12/2002	18	YES	Agro-industry	100.00	0.51
EDISON	09/12/2002	1	NO	Manufacturing	615.72	0.11
IVECO SPA	09/12/2002	1	YES	Manufacturing	265.61	0.46
APREAMARE	16/12/2002	1	YES	Manufacturing	49.90	0.47
BIOMASSE ITALIA	16/12/2002	2	YES	Manufacturing	130.70	0.38
EUROSVILUPPO	16/12/2002	1	YES	Agro-industry	49.05	0.54
PROCAL	16/12/2002	6	YES	Manufacturing	57.68	0.70
AGROFUTURO	11/01/2003	13	YES	Agro-industry	111.31	0.63
FELANDINA	05/03/2003	1	YES	Manufacturing	109.19	0.53
NUOVA BIOZENIT	05/03/2003	1	YES	Agro-industry	52.48	0.33
CONSORZIO SIKELIA	05/06/2003	20	YES	Agro-industry	96.80	0.52
PIRELLI	05/06/2003	1	YES	Manufacturing	167.39	0.44
COSTA D'ORO	31/07/2003	3	YES	Tourism	93.62	0.54
POLO FLORICOLO	31/07/2003	1	YES	Agriculture	48.41	0.40
SERRAMARINA	31/07/2003	1	YES	Agriculture	27.09	0.72
MARCONI MOBILE ACCESS	18/12/2003	1	YES	Manufacturing	58.23	0.28
CONS. SVILUPPO INDUSTRIALE SCARL	13/07/2004	1	YES	Food-industry	90.98	0.51
AREA AQUILANA	22/07/2004	1	YES	Manufacturing	80.03	0.28
GRUPPO FIAT	22/07/2004	3	YES	Manufacturing	1251.25	0.12
POLO TURISTICO TERMALE	29/07/2004	1	YES	Tourism	37.49	0.65

Appendix (continue)

Table A (continue): PCs implemented since their emergence in 1986 (continue)

Name of the PC	Date of approval	Number of municipalities	Located in the South	Sector	Planned investments	Share of public funds
CONS. NAUTICO POLIFUNZIONALE	28/02/2005	2	YES	Manufacturing	106.24	0.52
CONSORZIO AQUAM	14/07/2005	1	NO	Agro-industry	46.63	0.25
CONS. SVIL. AGROIND. PIEMONTESE	14/07/2005	15	NO	Agro-industry	27.30	0.39
ALL COOP	28/07/2005	1	YES	Agro-industry	27.30	0.39
COLACEM	19/02/2006	1	YES	Manufacturing	49.80	0.38
COPRIT	19/02/2006	4	YES	Tourism	102.99	0.61
FIAT POWERTRAIN	19/02/2006	1	NO	Manufacturing	647.60	0.13
GRUPPO FIAT2	19/02/2006	4	YES	Manufacturing	43.45	0.24
CONSORZIO BSI	27/03/2006	1	YES	Agro-industry	61.80	0.50
SVILUPPO ITALIA TURISMO	27/03/2006	6	YES	Tourism	199.26	0.39
TIRRENO SVILUPPO	27/03/2006	11	YES	Tourism	45.50	0.48
CONSORZIO ALIM	04/05/2006	7	YES	Agro-industry	28.97	0.48
EQUIPOLYMERS	04/05/2006	1	YES	Chemistry	89.99	0.40
SEVEL SPA	04/05/2006	1	YES	Manufacturing	455.63	0.09
SICILIA GOLF RESORT	06/10/2006	2	YES	Tourism	97.22	0.43
CONSORZIO TUSCANIA	12/01/2007	9	NO	Tourism	168.61	0.29
CONFLAJ	17/07/2007	5	YES	Tourism	53.45	0.36
VIDEOCOLOR	25/07/2007	1	NO	Manufacturing	274.12	0.16
ST MICROELECTRONICS	26/07/2007	1	YES	Electronics	1700.00	0.26
PAUSANIA	05/09/2007	4	YES	Tourism	48.29	0.48
MOLISE AGROALIMENTARE	27/09/2007	8	YES	Agro-industry	54.96	0.44
LA LODIGIANA	04/10/2007	3	NO	Agrizootech.	24.30	0.33
FIORIFRUTTI	18/03/2008	19	NO	Agro-industry	45.87	0.38
CONSORZIO CREO	15/04/2008	3	YES	Chemistry	32.28	0.43
EURALLUMINA	09/05/2008	1	YES	Manufacturing	113.67	0.24
TROMBINI	29/05/2008	1	NO	Agriculture	30.15	0.26
POLO TECNOL. CAMPANIA NORD	11/07/2008	1	YES	Manufacturing	41.20	0.48
CONS. AGROIND. AREE SVANT. PIEM.	24/07/2008	34	NO	Agro-industry	117.39	0.32
CONS. SVIL. INDUSTR. PIEMONTE	24/07/2008	15	NO	Agro-industry	32.56	0.23
CONS. TURISTICO SICILIANO	17/09/2008	7	YES	Tourism	48.47	0.49
MEDITERRANEO VILLAGES	16/10/2008	5	YES	Tourism	104.73	0.30
CONS. AGROALIM. BASSO FERRARESE	26/11/2008	6	NO	Agro-industry	75.33	0.29
CONS. CITTÀ DEL LIBRO	26/11/2008	1	YES	Publishing	37.20	0.50
TECNESUD	26/11/2008	2	YES	ICT	62.40	0.60
CREA	27/01/2009	3	YES	Chemistry	33.63	0.35
SELEX COMMUNICATIONS	12/03/2009	2	NO	Communication	93.80	0.30
CONS. SVIL. INT. SIST. AGROAL. PIEM.	14/09/2009	4	NO	Agro-industry	28.50	0.29
SVILUPPO TURIST. GOLFO NAPOLI	24/09/2009	3	YES	Tourism	63.40	0.39
MADE IN ITALY	03/08/2010	10	NO	Vitivinicole	63.45	0.29
SAM II	03/08/2010	7	YES	Manufacturing	50.62	0.40
AGROERICINO SCPA	to be approved	9	YES	Tourism	46.93	0.50
ALIMENTA	to be approved	1	YES	Agro-industry	40.00	0.38
ANTICHE TRADIZIONI PUGLIESI	to be approved	8	YES	Agro-industry	31.99	0.37
GENESIS	to be approved	1	YES	Manufacturing	77.66	0.48
GRUPPO CIT	to be approved	3	YES	Tourism	194.56	0.48
HIPPONIUM BIOMED	to be approved	1	YES	Manufacturing	63.98	0.35
INEOS VINYLS ITALIA	to be approved	2	YES	Chemistry	44.87	0.37
OROMARE	to be approved	1	YES	Manufacturing	50.00	0.40
PICENO CONSID	to be approved	5	NO	Tourism	40.12	0.16
PICENO CONSID II	to be approved	7	NO	Agro-industry	25.88	0.28
PICENO CONSID III	to be approved	9	NO	Manufacturing	60.66	0.14
PROGETTO PORTO NAPOLI	to be approved	1	YES	Tourism	186.53	0.38
PROKEMIA	to be approved	2	YES	Manufacturing	124.53	0.35
RIVIERA DEI GELSOMINI	to be approved	10	YES	Tourism	78.30	0.55
SERRAMARINA ADDENDUM	to be approved	1	YES	Agric.&Tourism	32.64	0.47
SOCIETÀ CONS. MELILLI GROUP	to be approved	1	YES	Agro-industry	87.80	0.58
SPAS	to be approved	3	YES	Ortho/Floricult.	125.02	0.39
STT LA TERRA DEL BENESSERE	to be approved	5	YES	Tourism	84.52	0.45
SVILUPPO SICILIA	to be approved	16	YES	Agro-industry	49.05	0.54
ULIVETI DEL SOLE	to be approved	16	YES	Tourism	50.65	0.44