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LOCAL DEVELOPMENT THAT MONEY CAN'T BUY: ITALY'S *CONTRATTI DI PROGRAMMA*

by Monica Andini[†] and Guido de Blasio[¶]

Abstract

The paper evaluates the effectiveness of a major Italian place-based policy (*Contratti di Programma*), by means of which the state approves and finances industrial projects proposed by private firms. Using the areas to be exposed to the same policy at a later date as counterfactuals, the study finds little evidence of it having had a positive effect. It estimates a limited impact on plant and employment growth rates, which is confined to a small area (a single municipality) and crowds out the economic growth of the surrounding areas.

JEL Classification: R11, R58, C14.
Keywords: regional economic activity, location-based policies, program evaluation.

Contents

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1. Introduction[1]

The rationale of location-based (or place-based) policies is now coming 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 is likely to be spatially unbalanced, and that trying to spread it out risks discouraging it. On the other hand, the OECD reports on regional growth (OECD, 2009a and 2009b) argue strongly in favour of growth-enhancing policies that target lagging regions.[2] The evaluation analyses of implemented programs will make a difference in this respect. For instance, policies found to be effective in fostering local development will clearly suggest that the OECD vision squares with the facts better than the vision of the World Bank and vice versa where a lack of effectiveness is found.

Even though the evaluation industry has expanded steadily during the last few years (see Banerjee and Duflo, 2009), the proportion of place-based policies that have been evaluated is still extraordinarily small compared with the thousands of programs implemented all over the world. For the policies that have been evaluated, the evidence seems to point to a lack of effectiveness. However, a key lesson from this literature is that *the devil is in the details*. Similar programs are likely to have very different effects, according to implementation features such as the assignment mechanism, the types of recipients and the timing of the program.[3]

Because it has a large area of underdevelopment (the largest in Europe) and a restless policy of moving resources towards poor parts of the country, 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 Italian policies, *Contratti di Programma* (Planning Contracts, PCs), which are intended to stimulate industrialization in backward areas. Planning Contracts involve an agreement between the central government and private firms, which can be large firms based in non-backward areas or SMEs located in backward areas. Public money follows the approval of a full-fledged industrial plan that sets targets mainly in terms of plants and employment.

Evaluating PCs can be of interest to both policy makers and economists.

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[2] A 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: Gill 2010; Barca and McCann, 2010). More provocative arguments against location-based policies can be found in the posts of Henry Overman (see Overman, 2011). On the role of development traps, the economic mechanism that helps to rationalize the interventionist view, see Kline and Moretti (2011).

[3] The fact that most of the programs so far assessed have been ineffective thus does not imply that all location-based policies will inevitably be so.

i) The policy is an example of old-fashioned intervention, in which the agreement is between the private sector and a centralized authority: no local stakeholder involvement (ownership) is envisaged. Therefore, it would be interesting to have an idea whether such place-based programs work.[4]

ii) It is also important to acknowledge that Italy's PCs were not implemented in a *vacuum*. During the period in which the program was in operation, other location-based programs were also under way. In particular, there were two other territorially targeted programs: Territorial Pacts, which were based on a bottom-up approach with the local community playing a key role in agreeing the development plan, and Area Contracts, which aimed at regenerating urban and industrial areas with large industrial plants in crisis. In addition, there was a major incentive scheme, Law 488/1992, intended to subsidize firms located in backward areas. The simultaneous presence of many programs poses 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 of the others (which refer to the same territories and were implemented over the same period of time) can be seen as extremely valuable. The comparison should reveal the relative merits of different types of programs, thereby providing useful hints for the design of location-based policies.[5]

iii) Under the PC program two different aims are envisaged: a localization target (producers of good repute are paid to locate in disadvantaged areas in order to bring industrialization) and a cooperation goal (SMEs established in backward areas get money to work together in order to benefit from agglomeration economies). The respective virtues of the two approaches have received considerable attention in the long-standing discussion on development tools. Our analysis will enable us to say something on the relative effectiveness of the two approaches.

Since 1986, PCs have been implemented in a scattered way over time. This is the aspect of the policy that we exploit to obtain identification. In particular, for PCs financed starting from 2000, on which the paper concentrates, we are able to compare PCs approved at the beginning of the decade (2001-2003) with others that were approved only after some years (starting from 2008). Therefore, we are left with a period of time (our estimation window, 2001-08) in which the group of treated municipalities is contrasted with a control group of future PC municipalities (*i.e.* those that would be exposed to the same policy at a later date). As shown in Busso *et al*. (2013) among others, if the approval 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 found in the treated area. An example is that of local multipliers (Moretti, 2010), where the increase in economic activity triggered by a program in one place might impact positively on the welfare of the surrounding areas. On the other hand, the effects

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[4] These old-fashioned interventions were basically abandoned before evaluation techniques made their appearance.

[5] The Territorial Pacts have been evaluated by Accetturo and de Blasio (2011); Law 488/1992 by Bronzini and de Blasio (2006). The results of both exercises point to overall ineffectiveness. Evaluation of Area Contracts is now underway by Accetturo, D'Ignazio and Franceschi (2013).

on the neighbouring areas may 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 such effects occur.

We estimate the effect of the program on the 2001-08 growth rates of plants and employment in the southern municipalities with more than 5,000 inhabitants.[6] Our results provide little evidence in favour of a positive impact. First, when the analysis is limited to small areas (the municipalities themselves), we estimate an impact that amounts to a 2001-08 cumulative increase in plants of 6.3 per cent and in employment of 7 per cent, corresponding to annual growth of slightly above 1 per cent for both variables. Moreover, the results do not survive when the level of aggregation of the units of observation is increased from municipalities to local labor markets, which include several neighbouring municipalities. This happens because spatial crowding out effects materialize: the increase in economic activity in treated municipalities comes at the expenses of the development of the neighbouring municipalities. Finally, to capture the potential impact of the policy beside that on plants and employment, we use aggregate measures of local economic wellbeing (population and real-estate values). We find that the results pointing to a lack of effectiveness receive 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 important for the evaluation exercise. Section 3 describes the data and the identification strategy. Section 4 discusses the baseline results, which refer to municipalities, together with extensive robustness checks. Section 5 documents the crowding out effects and examines the impact on population and housing values. Section 6 concludes.

2. The program

The aim of Planning Contracts is to overcome development disparities by promoting large domestic and foreign industrial investments in the backward areas of Italy. The program works on a bilateral "public-private" basis: it is an agreement between the central government and private firms. Once the government has announced the availability of resources, firms interested in the program apply by presenting a full-fledged industrial plan,[7] which singles out the targets, mainly in terms of plants and employment,[8] and indicates the infrastructures needed.[9] Then, a negotiation takes place between the two sides. According to the official PC guidelines (see Law 64/1986 and CIPE resolution 10/1994), the negotiation process "follows the logic of bilateral bargaining between public and private agents to meet their reciprocal goals", and the contract is signed once agreement is reached. The negotiations are conducted by a high-level policy committee (the Interdepartmental Committee for Economic Planning, CIPE), which relies

[6] In this period, the average annual GDP growth rate of the southern regions was 0.2 per cent, less than half the Italian average (0.7 per cent). 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.

[7] Proponent firms must also present a detailed financial plan showing internal and external funding sources.

[8] Additional targets refer to firms' research activity and the training and retraining of new and old employees.

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

on the advice of a technical commission. During the negotiations, the public authorities may ask for changes to the initial plan submitted by the private firms. These requests may be either accepted or refused by the proponents. Disbursement follows an installment schedule, which is agreed at the time the contract is signed (and which can be stopped if the monitoring activity reveals that the firms are not carrying out the investments that they have pledged to carry out). On the content of the negotiations little is known from official sources. However, Giunta and Florio (2002) and Giunta and Mantuano (2010) have collected some views, mainly through interviews with firms involved in the program, pointing to an accommodative stance on the part of the public authorities. In principle, PCs can be implemented in both tradable and non-tradable sectors. In practice, most initiatives were in the tourism, manufacturing and agro-industrial sectors.[10] In 1990, the initiative, originally intended to encourage large firms (or corporate groups) to locate in backward areas, was extended to SMEs already located in depressed areas.

Table A in the Appendix lists the 121 PCs that have been implemented since the birth of the policy in 1986. The first PCs were signed under Law 64/1986, which allowed intervention only in the South of Italy. Later, Law 488/1992 and CIPE resolution 24/1994 extended the territorial eligibility to all the disadvantaged areas of Italy, as identified by EU rules.[11] Among others, prominent PCs were those signed by Fiat (automobiles), Barilla (food) and Texas Instruments (electronics).[12]

The date of approval has been quite widely dispersed over time (Figure 1). The first two PCs were endorsed in 1988. For more than a decade, only a few PCs were approved each year. Conversely, a surge in endorsements, also due to the availability of larger allocations following an EU decision,[13] occurred at the beginning of the 2000s and in the last years of that decade.

The PC initiative is one of Italy's major place-based programs, in terms of both geographical coverage and amounts involved. At the end of 2010, 413 municipalities had been involved into the program. Total investments planned under the policy amounted to €21 billion (40% of which to be financed by public funds). As backwardness in Italy is concentrated in the South, this area is overwhelmingly present under the policy. No less than 103 of the 121 PCs include at least one southern municipality while 67% of all the municipalities involved are located in the South; the share of public funds channeled towards this area is as high as 94%. Figure 2 maps, over the South, the municipalities that received PC financing. All the southern regions have been covered by the policy (Puglia, Sicily and, to a lesser extent, Sardinia, have been most involved).

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[10] Even though one of the aims of PCs was to stimulate foreign direct investment, only 6 PCs were signed by non-Italian companies.

[11] The disadvantaged areas are those belonging to the Objectives 1, 2 and 5b defined by the EU Commission and those eligible to receive government aid according to Article 87.3.c of the Treaty of Rome.

[12] The table also includes 20 PCs that, at the time of writing, had already been *de facto* endorsed even though formal approval had still to come (these PCs will be used, together with those endorsed since 2008, to construct the control groups of 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 investments that they had pledged to carry out. These PCs are those that officially turned out to be failures. They are not considered in the empirical exercise below. Excluding them introduces a source of upward bias for the results, but this is not an issue, given the overall estimated ineffectiveness.

[13] A note of the EU Commission (SG (2000) D/105754) extended to PCs some financing sources that had previously been restricted to other programs.

3. Data and empirical strategy

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

The paper focuses on the PCs approved after 2000. This allows us to obtain a sizable data set by exploiting the fact that at the beginning of the decade there was a boom in approvals (Figure 1). Our treatment group is made up of PCs endorsed during the period 2001-03. This permits us to consider the 2001 census information as reasonable pre-treatment information.[14]

The unit of observation is the municipality.[15] This represents the most detailed level of stratification possible with the data available. We start with a sample of 106 municipalities involved in 31 PCs approved in the period 2001-03. Excluding the municipalities in the Centre and North of Italy has the advantage of providing a more homogenous sample, as the South of Italy differs from the rest of the country in a multiplicity of ways, such as access to markets, infrastructures, geography, cultural habits, *etc*. Therefore, by focusing only on southern regions we minimize the risk of mistakenly reflecting confounding factors, while the price we pay in terms of information loss is negligible (only 2 PCs,[16] involving 4 municipalities, were in the Centre and North of Italy). As the program was implemented continuously from 1988 to 2010, both the municipalities treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been dropped from the treatment group. This leaves us with 80 southern municipalities involved in 19 PCs approved in the period 2001-03. As the data source for the outcomes of interest is the ASIA-UL archive, we can only focus on those with more than 5,000 inhabitants. This leaves us with 56 treated municipalities. Table 1, Panel A summarizes the sample construction. Figure 3a plots the treatment group on a map of the South of Italy. The treated municipalities are located in Campania, Basilicata, Calabria, Sardinia and Sicily.

As the whole of the South of Italy is eligible and we have information only on the PCs actually approved, we estimate the causal impact of the program by comparing average outcomes by treatment status. Under this methodology, the critical issue is which units would best represent the treated units had they not been treated (see, for instance, Imbens, 2004). Our identification strategy is straightforward. The estimated ATT (average treatment effect on the

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

[15] However, a key section of the paper provides estimates at the higher level of aggregation, which is the local labor market that includes the municipality (see Section 5).

[16] One of which involves both northern and southern municipalities.

treated municipalities) might be biased because of both observable and unobservable factors. To deal with the selection bias due to observables, we use a control group selected on the basis of the Propensity Score method (operationally, we adopt the procedure proposed by Becker and Ichino, 2002). To deal with the possible selection bias due to unobservables, we refer to a comparison group (made up of future treated units) for which the similarity in terms of unobservables with the treated units is likely to be maximized. Therefore, the paper makes use of two control groups.

The first control group 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 PC treatment. The PS-matching uses 2001 census data at the municipal level for the following variables: the (log of) employees, the (log of) the number of plants, the (log of) the surface area, the activity rate, the unemployment rate, labor productivity,[17] and the proportion of highly educated people. Moreover, it uses a measure of local public spending inefficiency.[18] Table 2, Panel A describes this sample. Pre-treatment values for the matching variables of the treated group are described in the 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 does not reject the null hypothesis of equality of means. For reference purposes, in the fourth column we report the average values for the 616 untreated southern cities, from among which our control group is chosen using PS-matching. This group is very different from the treatment group, as the test values reveal. Figure 3b plots the treated municipalities and the standard control municipalities on the map.[19]

The standard control group is a valid one provided PS matching does justice to all the pre-treatment characteristics which might determine selection into treatment. This is not the case if some unobservables drive the likelihood of receiving treatment. For instance, treated municipalities might be more likely those with worse infrastructures or less endowed with social capital.[20] Typically, kind-hearted policy makers give greater weight to areas more in need of aid. However, an opposite mechanism might be at work, with firms choosing the relatively less backward areas among those that are eligible.[21]

As suggested by Busso *et al*. (2013) among others,[22] the group of future PC municipalities – *i.e*. those that will be exposed to the same policy at a later date – have the desirable feature of having both observable and unobservable characteristics similar to those of the treated units, provided the approval process is similar for the two groups. A similar assignment mechanism will reasonably take care of both time-invariant unobservables (for instance, a worse endowment of local infrastructures) and time-variant ones (for instance, a recent surge in crime). Therefore,

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17 Labor productivity is measured at the local labor market level.

18 A description of this measure can be found in Barone and Mocetti (2011).

19 Note that the PS-selected standard controls also happen to be located in regions in which there were no treated municipalities.

20 To mention just two of the many aspects for which we have no data available at the municipal level.

21 There could also be political mechanisms at work. For instance, the industrial plans submitted by private firms might stand more chance of being approved if the municipalities involved were part of the electoral constituency of the ruling central government.

22 See also Boarnet and Bogart (1996) and Bell *et al.* (1995).

future PC municipalities can provide a more suitable counterfactual. According to Giunta and Florio (2002) and Giunta and Mantuano (2010), the approval process remained unchanged over the first decade of the 21st century. Basically, public authorities have always been reluctant to act pro-actively in the negotiation process. A few exceptions are found for very large firms (such as Fiat), whose bargaining power was able to shift the contractual terms of the agreement in their favour. Overall, the assumption of a stable assignment mechanism seems to be supported. In any case, in Section 4.5 we provide a robustness check that includes only SMEs; that is, the types of firms for which the assumption of unchanged approval is most likely to hold.

The control group of future PC municipalities might not be without problems. Insofar as such units have an expectation of being treated later, "anticipation effects" could materialize. In particular, firms may decide to postpone investments in order to wait for the subsidy. In the framework of PCs, this is extremely unlikely as the approval of a PC depends on budgetary allocations that are quite unpredictable. In any case, anticipation effects would reasonably imply an upward bias, which would not challenge the conclusions of this paper. To construct the group of future PC municipalities, we use those involved in a PC approved after 2008 (*i.e.* the 18 PCs approved between 2008 and 2010 and the 20 PCs that in 2010 were waiting for formal approval).[23] 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 treatment over the medium term, *i.e.* after enough time has elapsed for the effects to materialize. As reported in Table 1, Panel C our sample includes 74 municipalities involved in PCs approved after 2008. As it has been done for the standard control group, we PS-select 33 municipalities from the 74 future PC municipalities. Table 2, Panel B reports the descriptive statistics and the tests. It should be noted that a high degree of similarity between treated and control units is already found (see the test in column 5) before running the PS-matching routine. This supports the idea that future PC municipalities represent a more appropriate control group than the standard one. However, the PS-matching further narrows the differences. Figure 3c plots the treated and future PC municipalities on the map.[24,25]

4. Results

This section shows our baseline results, derived at municipal level, and corroborates them with a number of sensitivity checks.

4.1 *Baseline results*. Table 3, Panel 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 marked

[23] In theory, we could have used PCs approved before 2008; but this would have had the undesirable effect of critically reducing the estimation window.

[24] Some sort of spatial mismatch at the regional level between treated and control municipalities still remains. It is nonetheless lower than that with the standard control group.

[25] As for the type of PCs involving treated municipalities, 84% of projects were in the industrial sector, 10% in the tourism sector and 6% in agriculture. In the future PC control group, 55% of projects were in the industrial sector and 45% in the tourism sector.

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 calculated using the nearest-neighbour matching routine (with the replacement option on) for the comparison between treated municipalities and the 49 PS-selected standard untreated ones. Under the unconfoundedness 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 complete ineffectiveness, for both plants and employment. As explained in the previous section, we believe that unconfoundedness 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 comparing the treated municipalities with all the 74 available municipalities that have been considered under the program since 2008. It should be noted 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 is not significant. These results highlight that the previous findings were likely to be distorted by a downward omitted-variable bias, which makes sense if the assignment mechanism is biased in favour of underperforming municipalities. Panel D makes this case even more strongly. When we estimate the impact of the program by using only the 33 PS-matched (future PC) untreated municipalities as counterfactuals, we find that the (nearest-neighbour matching) ATT is equal to 6.3% for plants and 7.0% for employment[26] (corresponding to annual growth rates of respectively 1.13% and 1.25%[27]). Both estimates are statistically highly significant. We label this last set of results as our baseline.[28] As our framework relies basically on difference-in-differences estimates, we have also checked for the presence of different pre-treatment time trends between the treated and control groups, which might invalidate the estimates of the ATT (as for the failure of the parallel trend assumption, see Blundell *et al*., 2004; and Bronzini and de Blasio, 2006). We have found that pre-treatment (1991-2001) growth rates are basically the same for both plants and employment.[29]

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[26] Considering the amounts spent by government, our estimates suggest that each additional job has been paid slightly more than €26,000 (which is a reasonable amount when compared with other Italian policies).

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

[28] To investigate the role of regional mismatch between treated and control municipalities (see footnote 19) as regards the results reported, we have replicated the specifications of Table 3 both by including a full set of regional fixed effects and by requiring each control municipality to be located in the same region as its treated match (in this last experiment, the number of untreated PS-selected municipalities is reduced in both Panel B and Panel D). Results from these checks are very similar however to those shown in Table 3 (they are not reported but are available upon request).

[29] To the extent that after 2001 the pattern of plants and employment for the untreated municipalities underperformed that of the treated ones (and this was due to something that was not included in the government's approval rule) our estimates could be biased upwardly. To make some progress in this respect, we calculate the growth rates for the two variables using 1991-2001 data for the treated municipalities and 1996-2007 data for the municipalities to be treated in the future. Pre-treatment growth rates are basically the same for plants. However, for employment, treated municipalities show higher growth rates than the untreated ones. Estimating the impact of the program by selecting among the untreated ones only those with an employment pre-intervention growth rate in line

4.2 *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-neighbour matching method matches each treated municipality with the control unit that has the closest propensity score (*i.e.* the nearest neighbour) and, allowing for replacement, a control unit can be the best match for more than one treated unit (as happens in our case). The advantage of this method is that all treated units find a match although poor matches will occur if units with very different propensity scores end up by being matched. Given this limitation, we follow the rule-of-thumb of double-checking the findings with alternative routines. As highlighted by Becker and Ichino (2002), none of the available alternatives is *a priori* superior to nearest-neighbour matching; however, their joint adoption is useful to asses the robustness of the estimates. Panel A presents the results we obtain 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 control municipalities 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 control municipalities are absent. Panel B shows results obtained using the radius matching method. The latter matches treated units with control units whose propensity score belongs to a neighbourhood (*i.e.* the radius) with a dimension that is arbitrarily chosen by the researcher. A small radius is likely to generate higher quality matches at the cost of unmatched treated units. A bigger radius is likely to 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 lose unmatched treated observations. Panel C presents the results we obtain using the kernel matching method. This routine matches all treated units with a weighted average of all the control units, with weights inversely proportional to the distance between the propensity scores of the treated and control units. As shown in the table, our evidence is robust to the choice of a particular routine, except for the estimation of the ATT for employment with the radius method.

4.3 *Robustness to 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 Law 488/1992. The presence of concurrent initiatives might bias our results and the sign of the distortion is not known *a priori*: it will be an upward bias if treated municipalities receive extra aid on top of that provided by PCs; it will be a downward bias if control municipalities are considered by the other location-based initiatives. It should be noted that the overlap of programs in our sample is substantial: among the 56 treated municipalities, 29 were involved in TPs, 5 in ACs, and 53 received funds under Law 488 (28 of which were also involved in the other two programs); among the 33 untreated municipalities, 18 were involved in TPs, 4 in ACs, and 31 received funds under Law 488 (17 of which were also involved in the other two programs). Therefore, the overwhelming majority of our sample

with that of the treated ones, we obtain results very similar to those shown in Table 3, Panel D. Nevertheless, it should be noted that any upward bias will not challenge the conclusion of this paper.

municipalities were involved in concurrent programs. However, the extent of involvement was quite balanced between treated municipalities (96%) and control municipalities (96%). The results provided in Table 5 (Panel A) are derived by computing the ATT conditioning on the existence of concurrent programs in the same areas, *i.e.* conditioning on: a dummy that takes the value of one if the municipality is included in a Territorial Pact; a dummy that takes the value of one if the municipality belongs to an Area Contract; a dummy that takes the value of one if the municipality received a non-zero share of Law 488 funds.[30] Observations are weighted using the PS routine weights assigned to treated and control municipalities in Table 3, Panel D. As a matter of fact, controlling for the existence of concomitant programs, we find that the estimated effect of PCs is slightly lower for both plants and employment, while remaining highly significant. Panel B presents the same exercise using the share of funding received by the municipality as the measure for Law 488 financing (instead of the dummy). These results are slightly higher than those of the baseline. All in all, it seems safe to conclude that the bias caused by concurrent policies can be deemed negligible for our results. This might be due to the concurrent programs being shown to be quite ineffective (see Bronzini and de Blasio, 2006; Accetturo and de Blasio, 2011) or to the positive bias caused by the impact of the concurrent programs on the treated municipalities being counterbalanced by the negative bias caused by the effect of the concurrent programs on the untreated municipalities.

4.4 *Robustness to funding heterogeneity*. An important check refers to the role of funding for effectiveness. The distribution of public money across municipalities is not uniform: three municipalities (Battipaglia, Bernalda and Nocera Inferiore) receive an overwhelming share of funds. While the sample average amounts to €6.12 million, dropping the three highest-subsidized municipalities (which correspond to the 95th percentile of the funding distribution) reduces the average injection of funds to €3.83 million.[31] Therefore, we are concerned that these municipalities might be driving our results. Table 6, Panel A shows that this is not the case: by dropping the municipalities corresponding to the 95th percentile of the funding distribution, the results 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 (5th percentile of the funding distribution). The results are consistently higher than those of the baseline. Finally, Panel C presents the results for a sample that drops both the 5th and the 95th percentiles. The general impression is that effectiveness is higher for intermediate intensities of financing.

4.5 *Robustness to types of PCs*. As explained above, PCs provide two types of incentive. One is to stimulate large firms to locate in backward areas. The other is to subsidize local increases in activity for SMEs established in such areas. It should be noted that the relative merits of these two different policies have been debated since the end of WWII. For instance, the idea that industrialization can be sustained by attracting plants from multinationals underlay a whole phase of the policies promoted by the World Bank during the sixties. This idea was then dropped

[30] A more drastic robustness check would have been to drop municipalities treated under other programs (see Accetturo and de Blasio, 2011). However, given the low number of observations and the high degree of overlap, this strategy is not available with our data.

[31] A similar ranking is obtained using the average per capita subsidy.

in favour of policies stressing the role of small and medium-sized enterprises and start-ups.[32] Table 7 provides a first look 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, that for employment falls to 5% (with a statistical significance well below conventionally acceptable levels).[33,34] 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). It should be noted that this comparison can be considered at most as only suggestive: among the 56 treated municipalities, only 7 were involved in PCs that referred to a localization target. However, limiting the exercise to SMEs, for which the assumption of unchanged approval is most likely to hold, provides evidence in favour of the overall rationale of using future treated municipalities as control units (see Section 3).

5. Spatial crowding out and the impact on population and rents

In this section we first study the spatial extent of the results so far described and then analyze the possibility of effects that go beyond those on plants and employment.

5.1 *The impact on surrounding areas*. PC programs may have spillover effects. 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 surrounding areas to the treated ones. For instance, this finding has emerged as the main obstacle to the effectiveness of the US Enterprise Zones (see Elvery, 2009; Lynch and Zax, 2011; Boarnet and Bogart, 1996).[35]

To give a first look at this issue, we move to the (higher) level of aggregation provided by local labor markets (LLMs).[36] For instance, if the effect found at the municipal level goes hand

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

[33] 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 over time. This contrasts with the technical ability of large enterprises, for which investment and its financing are a recurrent business activity (indeed, they have accurate planning and budgeting procedures in place). To investigate this possibility, we 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). A lack of planning capacity should be signalled by ATTs that decrease over time. This however is not supported by the data.

[34] 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.

[35] Similar issues are highlighted by Criscuolo *et al.* (2012) for the English RSA program.

[36] Local labor markets have been defined by the Italian National Institute of Statistics (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 'self-contained': within a given unit, both the share of working residents working locally and the share of employees residing locally must be at least 75%. This definition is consistent with standard definitions of municipalities 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

in hand with a similar impact at the LLM level – which includes surrounding municipalities – then positive spillovers are present. Table 8, Panel A provides the estimates for the baseline where the outcomes at the municipal level have been replaced by those at the LLM-level for each of the 56 treated municipalities and 33 control municipalities. These results point to an impact that is very small for plants and basically zero for employment.

The fact that the impact is lost by moving from municipality 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 alternative funding at the LLM level. In particular, we focus only on LLMs in which no other municipality (except for treated or untreated municipalities for which we have appropriate controls, *i.e.* those of the specification of Table 5, Panel A) 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.

It should be noted that the results in the first two panels of Table 8 are derived by replacing the outcomes at municipal level with the same outcomes at LLM level for our sample of PS-selected future PC municipalities. These experiments highlight what happens at the higher level of aggregation for the municipalities for which the analysis has so far been conducted. However, the appropriateness of the two groups of treated and control municipalities can be questioned as it is derived by comparing units at the municipal (and not the LLM) level. To reduce 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 LLMs treated (over the 2001-03 period) and compare them with the LLMs PS-selected among 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 is not effective in increasing either plants or employment.

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, 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), shows that the former do worse than the latter.[37] Altogether, these results suggest that spatial substitution, not attenuation, is behind our findings. This means that the increase (however small) of economic activity for treated municipalities comes at the expense of the economic growth of the surrounding areas.

5.2 *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 the workforce with training. Even more importantly, as an effect

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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 local labor markets were defined. They had an average population of 83,084 and a standard deviation of 222,418.

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

of the approval of a PC, appropriate infrastructures might be delivered to the area. It is therefore plausible that having a PC in place might deliver benefits to the local community that are not capitalized in additional plants and employment.[38] Since data on the wide array of potential payoffs are not available, we turn to estimates of the impact of PCs on 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.[39] Moreover, Roback-type models of spatial equilibrium (Glaeser, 2008) highlight that location-specific factors that positively affect both firms' productivity and households' welfare will result in higher prices for non-tradable factors, such as houses. In Table 9 we test whether the impact of PCs translates into higher population and house-price growth rates. We find (Panel A) that in the baseline this is not the case for either outcome. Nor is any effect found when we move to LLM outcomes (Panel B). Similar results are obtained when we consider only PCs for SMEs (at both municipal level, Panel C, and LLM level, Panel D), for which training and infrastructure are relatively more important.

6. Conclusions

In this paper we evaluate the effectiveness of so called Planning Contracts, a major Italian place-based policy intended to overcome development disparities by promoting large industrial investments in the disadvantaged areas of Italy.

Using the areas to be exposed to the same policy at a later date as counterfactuals, the study finds little evidence of the program having had a positive effect. We estimate a limited positive impact of the program on either plants or employment at municipal level, which triggers spatial substitution and does not extend to the whole local labor market. We also find that incentives for large firms have impacts that, at municipal level, are only moderately larger 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.

Although, as for any evaluation exercise, the external validity of our findings may be called into question (see, for instance, Deaton, 2010), our results suggest two things. First, the program's effectiveness is limited to micro-geographic areas. Crucially, benefits accruing to a municipality are at the expense of the surrounding areas. This suggests that it might be better for a municipality not included in a program to stay away from those involved or, even, to lobby to prevent its neighbour from receiving the treatment. Thus, PCs might undermine the incentive for neighbouring municipalities to 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 the fact that in Italy, unfortunately, *the devil is not*

[38] This is the first line of defence adopted by the advocates of place-based policies when they are confronted with negative evaluations. As a high-level official from an important Italian Ministry once 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 capital that was provided because of the program? Those local communities are now better off!".

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

in the details. On the contrary, regardless of whether the approach is bottom-up or top-down, whether the money goes to large or small firms, which assignment mechanism is used, *etc*., a lack of effectiveness prevails.

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Figure 1: Number of PCs by year of approval and type

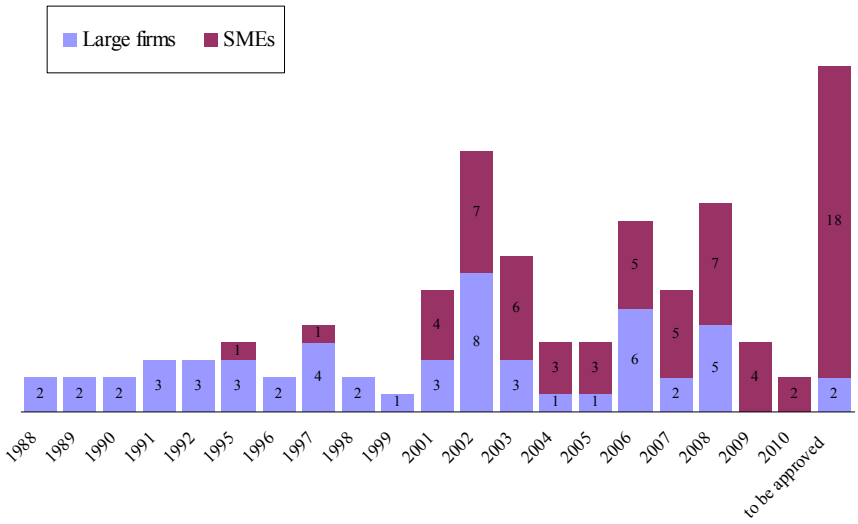
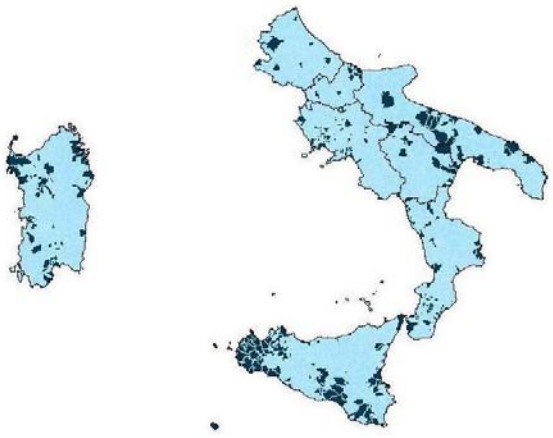


Figure 2: Southern municipalities that receive PC financing



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

Figure 3: Municipalities in the sample

Figure 3a: Treated municipalities

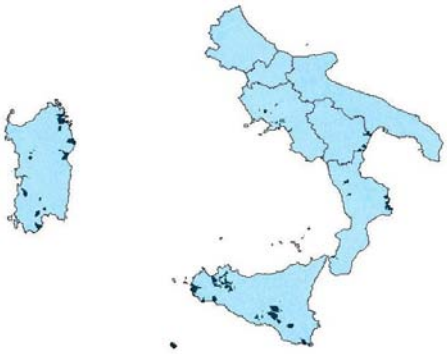


Figure 3b: Treated and (PS-selected standard) control municipalities

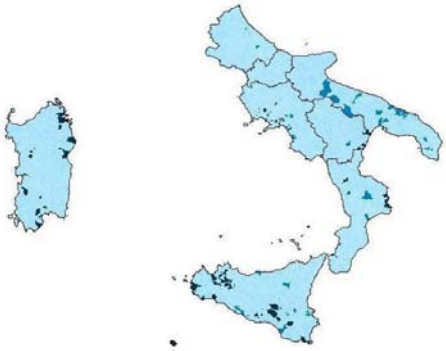
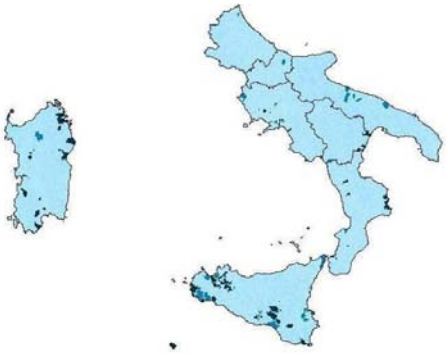


Figure 3c: Treated and (PS-selected future PC) control municipalities



Notes: Figure 3a: Treated group (56 municipalities involved in PCs in the period 2001-03) on the map of the South of Italy. Both the municipalities treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. Figure 3b: Treated municipalities (dark blue); PS-selected standard control municipalities (light blue). To construct the control group PS-matching has been used. Figure 3c: Treated municipalities (dark blue); PS-selected future PC 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 municipal level for the following variables: the (log of) employees, the (log of) the number of plants, the (log of) the surface area, the activity rate, the unemployment rate, labor productivity, the proportion 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	7,785
Number of southern municipalities not involved in PCs in 2001-03	2,455
Dropping southern municipalities already treated in other periods	2,281
Dropping southern municipalities with less than 5,000 inhabitants	616
PS-selected southern municipalities	49
Panel C. Future PC 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: the census (which is available for 2001) and the ASIA-UL archive (available from 2004 onwards). Information on PCs has been collected from the archive of the resolutions of the Interdepartmental Committee for Economic Planning.

Table 2: Balancing Property

Panel A. Standard control group					
Covariate	Treated	PS-Controls	BP Test	Untreated	DM Test
	56	49	(p-value)	616	(p-value)
Log no. of plants	6.893	6.969	0.619	6.451	0.000
Log no. of employees	7.734	7.815	0.626	7.12	0.000
Unemploym. rate	0.248	0.238	0.424	0.158	0.059
Log of surface area	3.897	3.914	0.945	3.595	0.059
Prop. of high. educ. people	5.501	5.234	0.554	5.315	0.539
Activity rate	44.325	44.514	0.824	43.759	0.304
Labor 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 PC control group					
Covariate	Treated	PS-Controls	BP Test	Untreated	DM Test
	56	33	(p-value)	74	(p-value)
Log no. of plants	6.893	7.032	0.462	6.892	0.991
Log no. of employees	7.734	7.806	0.732	7.591	0.423
Unemploym. rate	0.248	0.24	0.499	0.164	0.221
Log of surface area	3.897	4.135	0.361	4.117	0.294
Prop. of high. educ. people	5.501	5.878	0.453	6.254	0.063
Activity rate	44.325	43.277	0.26	43.612	0.322
Labor productivity	3.897	3.94	0.743	3.767	0.326
Inefficiency	6.938	6.933	0.903	6.902	0.385

Notes: Treated. Municipalities involved in PCs in the period 2001-03. To construct this group both the municipalities treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-selected standard control municipalities. To construct this group the PS-matching procedure is used. PS-matching uses 2001 census data at the municipal level for the following variables: the (log of) employees, the (log of) the number of plants, the (log of) the surface area, the activity rate, the unemployment rate, labor productivity, the proportion 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 PC control municipalities. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses the variables described above.

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	-0.96
Employees	56	49	0.001	0.032	0.04
Panel C. Future PC 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 PC control group. Nearest-neighbour matching					
Dependent variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	33	0.063	0.022	2.89
Employees	56	33	0.070	0.040	1.75

Notes: Treated. Municipalities involved in PCs in the period 2001-03. To construct this group both the municipalities treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-selected standard control municipalities. To construct this group the PS-matching procedure is used. PS-matching uses 2001 census data at the municipal level for the following variables: the (log of) employees, the (log of) the number of plants, the (log of) the surface area, the activity rate, the unemployment rate, labor productivity, the proportion of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). PS-selected future PC control municipalities. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses the variables described above. Panel A - C: Coefficients estimated using the ordinary least squares method. Panel B - D: Coefficients estimated using the nearest-neighbour matching method.

Table 4: Robustness to alternative matching routines

Panel A. Stratification matching					
Dependent variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	71	0.052	0.020	2.06
Employees	56	71	0.067	0.031	2.18
Panel B. Radius matching					
Dependent variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	71	0.053	0.022	2.37
Employees	56	71	0.026	0.027	0.96
Panel C. Kernel matching					
Dependent variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	56	71	0.053	0.020	2.70
Employees	56	71	0.059	0.030	1.97

Notes: Treated. Municipalities involved in PCs in the period 2001-03. To construct this group both the municipalities treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-selected future PC control municipalities. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS matching uses 2001 census data at the municipal level for the following variables: the (log of) employees, the (log of) the number of plants, the (log of) the surface area, the activity rate, the unemployment rate, labor productivity, the proportion 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.

Table 5: Robustness to 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 Law 488 financing					
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 2001-03. To construct this group both the municipalities treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-selected future PC control municipalities. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS matching uses 2001 census data at the municipal level for the following variables: the (log of) employees, the (log of) the number of plants, the (log of) the surface area, the activity rate, the unemployment rate, labor productivity, the proportion of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). Panel A: ATT estimated conditioning on 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. Panel B: ATT estimated conditioning on 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 the share of Law 488 funds received by the municipality.

Table 6: Robustness to 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	2.04
Panel B. Drop the 5th percentile					
Dependent variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	44	26	0.085	0.025	3.46
Employees	44	26	0.094	0.044	2.15
Panel C. Drop the 5th and 95th percentiles					
Dependent variable	Treated	Untreated	ATT	s.e.	t-stat
Plants	41	24	0.088	0.020	4.46
Employees	41	24	0.101	0.044	2.29

Notes: Treated. Municipalities involved in PCs in the period 2001-03. To construct this group both the municipalities treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-selected future PC control municipalities. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS matching uses 2001 census data at the municipal level for the following variables: the (log of) employees, the (log of) the number of plants, the (log of) the surface area, the activity rate, the unemployment rate, labor productivity, the proportion of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). ATT estimated with the nearest-neighbour matching method.

Table 7: Robustness to 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 2001-03. To construct this group both the municipalities treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-selected future PC control municipalities. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS matching uses 2001 census data at the municipal level for the following variables: the (log of) employees, the (log of) the number of plants, the (log of) the surface area, the activity rate, the unemployment rate, labor productivity, the proportion of highly educated people, and a measure of local public spending inefficiency (provided by Barone and Mocetti, 2011). ATT estimated with the nearest-neighbour matching method.

Table 8: The impact on surrounding areas

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	0.83
Employees	30	14	-0.015	0.033	-0.44
Panel D. Untreated surrounding areas 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 PC control municipalities. Treated. Municipalities involved in PCs in the period 2001-03. To construct this group both the municipalities treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-selected future PC control municipalities. 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 proportion 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. 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	0.010	-0.45
Rents	49	29	0.014	0.049	0.29

Notes: Panel A: Treated. Municipalities involved in PCs in the period 2001-03. To construct this group both the municipalities treated under PCs approved before 2000 and those receiving additional treatment under PCs approved from 2004 onwards have been excluded. PS-selected future PC control municipalities. To construct this group we use the municipalities involved in the PCs approved after 2008. The PS-matching uses 2001 census data at the municipal level for the following variables: the (log of) employees, the (log of) the number of plants, the (log of) the surface area, the activity rate, the unemployment rate, labor productivity, the proportion 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 PC control municipalities. 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 PC control municipalities. Only municipalities involved in PCs signed by SMEs have been considered. ATT estimated using the nearest-neighbour matching method.

Appendix

Table A: PCs implemented since their adoption 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

(cont.)

Appendix (cont.)

Table A: PCs implemented since their adoption in 1986 (cont.)

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

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