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POLICIES FOR LOCAL DEVELOPMENT: AN EVALUATION OF ITALY'S "PATTI TERRITORIALI"

by Antonio Accetturo[*] and Guido de Blasio[*]

Abstract

In Italy, Patti Territoriali (Territorial Pacts) are one of the main government-sponsored programmes to foster growth in disadvantaged areas. A territorial pact is an agreement among the local authorities and representatives of civil society (mainly entrepreneurs and trade unions) of a number of neighbouring municipalities that is subsequently endorsed by the central government. It consists in a fully-fledged development plan, including a series of private and public investments for which public funding is provided. This paper evaluates the effectiveness of territorial pacts by comparing the economic performance, in terms of employment and number of plants, of participating municipalities with a sample of municipalities not involved in the policy. The results suggest that the programme has been largely ineffective in stimulating growth.

JEL Classification: R0, H2.
Keywords: regional aid, regional growth, ownership.

Contents

[*] Bank of Italy, Economics, Research and International Relations.

1. Introduction[1]

Ensuring cohesion – similar social and economic conditions – across areas of the same political entity (unions of states, states, regions) is a prime objective of economic policy. Indeed, the endogenous concentration of the most innovative and dynamic industrial activities in the core regions of a country frequently raises concerns among economists and policy makers that peripheral areas might become impoverished and more vulnerable to macroeconomic shocks or to competition from emerging markets. This may explain why a large number of governments around the world are now actively engaged in programs to spur local development in backward areas; instances are the US *Entrerpise* and *Empowerment Zones*, the French *Zones Franches Urbaines*, and the English *New Deal for Communities*.

While these programs are widely implemented, little is known about their effectiveness in reaching the stated goals. One reason is that to evaluate effectiveness one has to answer the question of what would have happened without the program. In other words, evaluating a program is a counterfactual exercise. Although the econometric tools of program evaluation have now been included in the standard tool-box of applied economists, regional applications are not as widespread. Indeed, most of the time the focus of the interventions is on small geographic areas, and counterfactual analyses are severely

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constrained by the availability of data at a very detailed geographical level.[2] This paper aims to contribute to the literature assessing the impact of policies for local development by using a program evaluation approach. It examines the impact of Italy's *Patti Territoriali* (Territorial Pacts, TPs) on the dynamics of employment and number of plants.

Established in 1996, the TP program aims to trigger growth and employment in the backward regions of the country. The program is based on a full "bottom-up" approach: a Pact is an agreement signed by local governments and representatives of civil society – mainly entrepreneurs and trade unions – of a number of neighboring municipalities, which is subsequently endorsed by the central government. The agreement consists of a fully-fledged plan for the development of the area that includes a series of private and public investments for which public funding is provided. The program is implemented on a voluntary basis. Eligible municipalities are all those located in areas allowed to receive public funds by the European Union. Therefore, all southern municipalities are eligible, as the whole *Mezzogiorno* is included in the EU Objective 1 area, while in the Centre and North eligibility is restricted to a portion of the territory: only municipalities located in the EU Objective 2 and 5b areas can join the program.[3]

In this paper, we perform a counterfactual analysis of the economic impact of the program over the period 1996-2004. We compare the economic performance – in terms of employment and number of plants – of a municipality taking part in a TP with a sample of municipalities not involved in the policy. Given the particular features of the policy, identification of its economic effect is not straightforward. Participation in the program

[2] Counter-examples are Bondonio and Engberg (2000), O'Keefe (2004) and Bondonio and Greenbaum (2007) on the effectiveness of US enterprise zones; Busso and Kline (2008) on the US empowerment zones; Rathelot and Sillard (2007) on the French *Zones Franches Urbaines*; Criscuolo et al. (2008) on the UK regional *selective* assistance. As for Italy, Bronzini and de Blasio (2006) and Bronzini et al. (2008) consider two programs of investment incentives for firms in backward areas (grants and fiscal bonus, respectively).

[3] Here we refer to the terminology used for the 1994-1999 period of EU structural funds.

requires a municipality to be located in an eligible area, to join the TP on a voluntary basis and to coordinate with its neighbors. Therefore, a number of identification issues must be tackled, as post-program economic performance might be driven by non-random program placement, self-selection and group-membership bias. Our empirical strategy relies on a difference-in-differences framework compounded with Propensity Score Matching. We are able to identify the average treatment effect on the treated municipalities located in the Centre and North by implementing an IV approach that exploits the rule assigning eligibility status. We also provide a non-causal measure of the effectiveness of the policy for the municipalities located in the South. Our results suggest that the TP program has been largely ineffective, as the growth in employment and businesses in the municipalities involved in the policy does not differ significantly from that in the comparison group of cities not involved. This result is robust to a number of specification checks: it does not seem to depend on the initial level of economic development of the area; it does not seem to be driven by self-selection or by the opportunity to join the program given neighbors' choices. Finally, the ineffectiveness seems unlikely to be due to the slow provision of public money under the policy or the availability of a concurrent aid program that targeted the same areas eligible for TP assistance.

The paper is organized as follows. Section 2 describes the TP program. Section 3 illustrates the data used in the analysis. Section 4 details the identification strategy, while Section 5 shows the econometric results. Section 6 concludes, suggesting some interpretations for the findings.

2. The program

The TP program – TPs were established by the 1997 National Budget Law (Law 662/1996) as part of a wide-ranging re-organization and rationalization of public intervention in backward areas.[4] TPs belong to a special class of public programs called "negotiated planning" . Compared with Italy's earlier policies to spur regional convergence that featured a substantial degree of *centralism,* negotiated planning gives special attention to the involvement of local communities in the policy design. The idea is that in order to maximize the effectiveness of development programs they should originate from local stakeholders. In particular, an agreement among agents in potential conflict – such as trade unions and entrepreneurs – should be better able to deliver an effective development strategy, in which all agents are willing to cooperate to achieve the common goal of economic growth.[5] Among negotiated planning programs,[6] TPs are the most important instrument in terms both of population and of the public aid involved. Since December 2006, the Ministry for Economic Development has approved 220 TPs; almost half of the Italian population live in a municipality belonging to a TP; budget allocations for the program have reached 5.5 billion euros.

A Pact is an agreement signed by local governments and representatives of civil society (mainly entrepreneurs and trade unions) of a number of *neighboring municipalities*,

[4] Italy's tradition of public aid for deprived areas dates back to 1950. See Braunerhjelm et al. (2000) for an historical account.

[5] This approach clearly reflects the ownership strategy adopted, more or less in the same period, by the World Bank and the International Monetary Fund, according to which local policy-makers and stakeholders should truly "own" a policy; that is, they share both the objectives and instruments of a development program. From a regional science-based perspective see Bartik (2005), according to whom only a bottom-up approach is capable of generating development in distressed areas.

[6] Two other instruments are (i) the *Contratti d'Area* program for the urban and industrial regeneration of areas with large industrial plants in crisis, and (ii) the *Contratti di Programma*, targeted at the promotion of inward foreign direct investments in deprived areas.

indicating a *local coordinating authority*[7] in charge of the policy and containing a fully-fledged *development plan*. The development plan includes both private and public investments. Public funding is provided for both. In particular, total public money is limited to 50 million euros for each TP. Investments in public infrastructure should not exceed 15 million, while the remaining amount can be awarded to private firms as grants for the creation, extension, modernization, conversion, restructuring, reactivation and relocation of productive plants. Grants must not exceed 70 percent of the value of the firm's investment project. A first assessment of the economic soundness of investment projects is made by a private bank (only projects that pass this check are included in the development plan).

As for the size of a TP, no requirement (for instance, in terms of number of municipalities or population involved) is imposed by law. In principle, any small number of neighboring municipalities (even just two) could get together to create a TP. In reality, fixed costs to set-up the local coordinator and the development plan have encouraged large groupings. For the 51 TPs involved in our evaluation (see Table 1) the average number of municipalities is 26.7 and the average population 235,000. In order to meet the requirement of proximity, the policy does not require the municipalities to have a common border; it only requires them to be "close" enough to each other to share the benefits of the policy.

Eligibility – TPs can only be created in underdeveloped areas of the country, as defined by the EU criteria for Structural Funds in the 1994-1999 planning cycle. This means that: i) all southern municipalities are eligible, as the whole *Mezzogiorno* is included

[7] The local coordinating authority can be either a local public administration (for example, the provincial administration) or a local private association (even a newly created company might be chosen for this role). It promotes coordination, penalizes non-cooperative behavior and has the role of conciliator when conflicts arise among participants in the TP.

in the EU Objective 1 area; ii) only municipalities in the Centre and North located in EU Objective 2 and 5b areas can join the program.[8] Objective 1 status is defined on the basis of a simple rule: regional GDP per capita (at NUTS2 level) must be less than the 75 percent of the EU average. On the contrary, the status of Objective 2 and 5b area is assigned on the basis of a two-step process. First, three economic parameters at the NUTS3 level (province) are taken into account to select *potentially* eligible areas. These areas should exhibit both an unemployment rate and a share of industrial employment greater than the European averages. Moreover, these areas should have experienced a decrease in employment for several years. Second, within each potentially eligible NUTS3 area, only some municipalities are chosen to be *actually* eligible for the measures designed for the Objectives. Due to limitations in the European budget, the European Commission sets a ceiling for the population that can be involved in the Objectives 2 and 5b for each EU country. Given this constraint, EU authorities and national governments negotiate the selection of eligible municipalities within each NUTS3 potentially eligible area.

Figure 1 shows the areas of eligibility in both the Centre and North and the *Mezzogiorno*.

Implementation – Municipalities located in eligible areas can coordinate with their neighbors and join the policy on a voluntary basis. The agreement signed by local stakeholders is then endorsed by the central government. With endorsement a TP is said to be *established*, that is it can receive public money. Funding follows the establishment.

[8] The EU structural funds regulation 1994-1999 envisages two groups of objectives: Objective 1, aimed at promoting development and structural adjustment in poor regions; Objectives 2 and 5b, designed to support social and economic conversion of areas experiencing structural difficulties, such as heavy specialization in declining manufacturing sectors.

Upon receipt of the first installment, a TP is said to be *activated*. TPs were introduced in several waves (see Table 1). A first wave of 12 TPs, all in the South, was established and activated in 1997. A second wave of 39 TPs (19 in the Centre and North, 20 in the South) was established in 1997 and activated in 1999. These two waves form our reference sample due to data availability. Finally, a third wave of TPs (28) was established in 2001 and gradually activated from 2002 to 2006.[9] We also use this sample for a robustness experiment. Figure 2 shows the distribution of our reference sample and the third wave over the national territory: TPs are evenly distributed across all the eligible areas, especially in the South; in the Centre and North, they are concentrated in the heavily industrialized areas of Piedmont (in the North-West), Veneto (in the North-East) and Tuscany (in the Centre).

It is worth noting that at the end of the 1990s policy makers were quite confident about the growth-enhancing role that TPs could perform (see e.g., Ministero del Tesoro, 2000). High expectations were confirmed even afterwards. According to the estimates of the Ministry for Economic Development in 2006 (RGSEP, 2006), the additional employment due to the program should have amounted to 80,000 employees.[10] While this paper provides the first econometric evaluation of TPs, a number of previous studies have

[9] In the paper we refer to nationally endorsed TPs with no limitations as to the economic activities that can be financed. Note, however, two additional categories of TPs were also implemented. First, European TPs (10 of them) were established and activated in 1998 with an endorsement procedure of the European Commission. European TPs are directly financed by the EU and differ from national TPs in a number of respects. First, the selection of industrial projects was much more careful, since European authorities in charge of the endorsement also acted as consultants in the creation of business plans. Second, disbursement of public funding was much faster. Third, development plans were allowed to include a number of additional activities, such as investment in local labor market qualification. Because of these differences, we exclude European TPs in the analysis below. However, we considered European TPs in a previous version of the paper, with results very much in line with those presented in this version. Note also that in the third wave, beyond the 28 TPs we refer to above, a further 91 agricultural TPs (aimed at sustaining the agricultural sector in rural areas) and 50 sectoral TPs (to assist specific sectors, such as tourism and textiles) were endorsed. We do not consider these TPs in the analysis.

[10] The reference here is to all national TPs, including those activated in the period 2002-2006.

dealt with implementation issues.[11] Two aspects need to be mentioned in this connection. First, as highlighted by Magnatti et al. (2005), one features of TPs was the slow provision of public money due to the inefficiency of the Italian public spending process. For instance, on 30th June 2002, the share of money disbursed out of the total budget allocated to the program was still less than 50 percent for the first wave and less than one-third for the second wave. Second, the intervention was not implemented in *a vacuum*. Over the period in which the TP program was implemented, an additional policy measure – investment grants awarded through Law 488 – targeted the same areas eligible for TP assistance. These implementation issues are dealt with in the empirical section below. We will analyze the role of the paucity of disbursements and that of the availability of a concurrent aid program on the overall efficiency of TPs.

3. Data

Our main data source is the Istat (Italian Statistical Office) Census, which provides us with the key performance and control variables used throughout the paper. We use Census releases for the years 1996 and 2001. However, we also exploit 1991 data to calculate pre-intervention time trends. These data have a number of advantages and some drawbacks. In particular, the Censuses provide us with a large number of variables for all Italian municipalities (i.e. because they cover the whole country there is no sample selection problem). Moreover, the 1996 release represents an excellent "before" year as its information relates to the starting period of the program. A drawback in the use of the Census is that the latest release (2001) might be considered too early to show the impact of

[11] See, for example, Sviluppo Italia (2001), Cersosimo (2000), Mirabelli (2000), Cersosimo and Wolleb (2001), Barbera (2001), Casavola and Utili (2002). In many cases these papers also provide very interesting case studies.

TPs on local economies. We cope with this problem by integrating our Census dataset with data from the ASIA-UL archive (*Archivio Statistico Imprese Attive – Unità Locali*: statistical archive for active firms – plants), also released by Istat and containing information on the number of firms and employees at municipal level for the year 2004. According to the Italian Statistical Office, ASIA-UL is directly comparable with the Census, but unfortunately, for confidentiality reasons, it does not include information for towns with fewer than 5,000 inhabitants. Therefore, once we extend the analysis to 2004, we are forced to restrict it to municipalities larger than that threshold.

For the municipalities eligible for the policy and those participating in a TP, we use the official dataset provided by the Ministry of Economic Development.

4. Identification strategy

The aim of the paper is to assess the impact of TPs in terms of two outcomes (Y_i, i=1,2): employment and number of plants. The general setting can be described as follows. Let D, a binary treatment status denoting *participation* in the program, be a function of the triple (X, U, Z), where X and U represent respectively a set of observable and unobservable characteristics of the municipalities, possibly correlated to the outcomes, and Z is the *eligibility* status for participating in the program. To estimate the effects of D(X, U, Z) on the performance variables (Y_i), a few aspects of our identification strategy have to be noted.

Municipalities eligible for the program do not represent a random sample of the Italian population of municipalities: they belong to the underdeveloped areas of the country, as defined by European rules (see Section 2). This circumstance may create a bias (usually called non-random program placement), as the impact of the program could be due

to the effects of the pre-existing disparities.[12] To minimize the selection issue driven by observable pre-treatment disparities (that is, the potential effect of X on the outcomes), we compare eligible (Z=1) and non-eligible (Z=0) municipalities that display strong similarities before the start of the program. This is accomplished by using the Propensity Score Matching (PSM, Rosenbaum and Rubin, 1983).

The set U is allowed to include two types of unobserved variables (say, $U=U_A, U_B$).

U_A accounts for some unobservable characteristics of municipalities that are correlated with the outcomes, but not with their variation over time. Empirically, this case is dealt with in a difference-in-differences (diff-in-diffs) framework (see, for example, Angrist and Krueger, 1999; Card, 1999; and Meyer, 1995); that is, we use pre-intervention difference in outcomes between treatment and control groups to control for (time-invariant) unobservable differences.

Note that the *actual* treatment status, D, does not coincide with the *assigned* treatment status, Z, because some municipalities located in eligible areas might decide not to join the program. In other words, being part of a TP is a voluntary decision. Therefore, U_B includes the unobservable characteristics relevant for the municipality's decision to comply with the assignment. Since we do not know the compliance mechanism, we design the evaluation strategy as if these unobserved characteristics were correlated to the potential outcomes, i.e. as if it were a source of selection bias, which remains even after the time-invariant unobserved selection bias has been removed.[13]

[12] For example, according to a neo-classical convergence process (Solow, 1956), backward areas are likely to grow at a faster pace than more advanced regions, due to the existence of decreasing returns of production factors. On the other hand, according to a New Economic Geography mechanism (Baldwin et al., 2003), more developed areas might grow faster due to the productivity effects of agglomeration economies.

[13] Therefore, a direct comparison of the two treatment arms (i.e. the estimate of the parameter $E[Y|D=1,Z=1]-E[Y|D=0,Z=1]$, which we also provide in the paper) should be regarded as affected by the usual selection bias problem because the two groups are not equivalent with respect to U_B.

The circumstance that municipalities choose to join the program unduly complicates the identification strategy. However, as explained by van der Klaauw (2007), if i) *the rule that assigns the eligibility status can be approximated by a random scheme*, we can compare the two groups of eligible and non-eligible municipalities and estimate the *Intent-to-Treat* (ITT) effect: E[Y|Z=1]-E[Y|Z=0]. The ITT measures the impact of having access to a treatment regime compared with not having it. It might be relevant for a policy maker who wants to know the effect of having established a given eligibility rule, rather than that of the program itself on those who decide to join. Moreover, if ii) *Pr(D=1|Z=1)≠ Pr(D=1|Z=0)*, that is, the possibility of receiving the treatment for those with Z=1 is different, presumably higher, than the chance of being treated for those with Z=0, we can use Z as instrumental variable for D and estimate the *Average effect of the Treatment on the Treated* (ATT). The ATT can be identified as the ratio of the causal effect of Z on Y to the causal effect of Z on D. To see this point, note that the outcome Y can be written as $Y=Y_0+\beta D$, where Y_0 denotes the counterfactual outcomes in the absence of treatment and $\beta=Y_1-Y_0$ is the ATT. Therefore, the ITT (E[Y|Z=1]-E[Y|Z=0]) can be conveniently rewritten as:

$$(1) \qquad \{E[Y_0|Z=1] - E[Y_0|Z=0]\} + \beta \{Pr(D=1|Z=1) - Pr(D=1|Z=0)\}$$

where the first term in curly brackets represents the difference in counterfactual outcomes between eligible (Z=1) and non-eligible (Z=0) municipalities. Under the assumption of random assignment of Z, this term is equal to zero. Therefore, it holds that:

$$(2) \qquad E[Y|Z=1] - E[Y|Z=0] = \beta \{Pr(D=1|Z=1) - Pr(D=1|Z=0)\}$$

Then, β is identified by the IV estimator:

$$(3) \qquad \beta = \{E[Y|Z=1] - E[Y|Z=0]\} / \{Pr(D=1|Z=1) - Pr(D=1|Z=0)\}$$

provided that Pr (D=1|Z=1) ≠ Pr (D=1|Z=0).[14]

In the case of TPs, ii) is trivially verified, as municipalities outside the areas of eligibility are excluded from participating (that is, Pr(D=1|Z=0)=0), while we observe that a number of the eligible municipalities receive the treatment (that is, Pr(D=1|Z=1)>0). Requirement i), however, needs to be discussed. First, notice that the concept of randomization that matters here is that of random assignment *conditional* on the observables. That is, requirement i) amounts to saying that once that the municipalities have been made equivalent in terms of their observable pre-treatment characteristics, as we do by using the Propensity Score Matching, the eligibility status can be approximated by a random scheme (i.e. Z|X is independent of potential outcomes). Second, recall (from Section 2) that in the Centre and North eligibility is established on the basis of a two-step process, through which a set of potentially eligible NUTS3 areas is first singled out and then, within the areas of potential eligibility, a group of actually eligible municipalities is selected as a result of a political bargaining process. The consequence of the selection process that assigns the (actual) eligibility status is that two municipalities which are very similar in terms of economic conditions may, however, be differently categorized as to EU Objectives: one is eligible and the other is not. For instance, the non-eligible municipality can derive its status from the fact that it happens to be located in a NUTS3 area which is not potentially eligible (for instance, a poor municipality in a rich *provincia*). Note also that the NUTS3 borders are the result of historical traditions that bear no relation to the current prosperity of the municipalities. Moreover, the non-eligible municipality may derive its

[14] If one is willing to make the assumption of homogeneous β, the ATT can be interpreted as the common treatment effect. In the general case of heterogeneous effects (see Angrist, Imbens and Rubin, 1996), the estimated ATT represents the effect on the sub-population of complier municipalities.

status from the fact that it happens to be discarded in the political bargaining process between national and European authorities. For these reasons, it can be argued that in our setting requirement i) becomes a hypothesis that is not too far from reality.

However, in the evaluation exercise we do not rely only on the suitability of the above reasoning. We also directly test whether the assumption of random assignment holds in the data by estimating an empirical counterpart for the difference in counterfactual outcomes between eligible (Z=1) and non-eligible (Z=0) municipalities (which represents the first term in curly brackets of expression (1)).

Note that this empirical strategy can only be implemented in the areas of the Centre and North of Italy, as the whole *Mezzogiorno* represents a single area of eligibility under the EU Objective 1. Since in this area there are no non-eligible municipalities, neither the ITT nor the ATT can be estimated. However, the absence of an indication as to the effectiveness of the TPs in the South leaves us with a deep sense of dissatisfaction. After all, the program is intended to spur growth in the backward areas and it would be quite paradoxical for the evaluation exercise to remain silent only for the areas with most underdevelopment. To deal with this, we provide for the South PSM diff-in-diffs estimates obtained by comparing participating cities with eligible non-participating ones (i.e. we estimate $E[Y_1|D=1]–E[Y_1|D=0]$). To gauge the distortion implied in this estimate we also calculate the same parameter for the Centre and North and compare it with the ATT. Clearly, the parameter $E[Y_1|D=1]–E[Y_1|D=0]$ does not deliver estimates of the causal effects of the TP program in the South. Nevertheless, we believe it might provide some preliminary guidance on the effectiveness of the policy.[15]

[15] An alternative strategy could be to use non-eligible municipalities located in the Centre and North as a comparison group for the participating and eligible non-participating southern municipalities. The South, however, differs from

The empirical section also deals with a number of robustness issues.

a. *Concurrent program*. During the years in which TPs were in effect a concurrent aid program targeted the same areas eligible for TP assistance (see Section 2). Note that for the Centre and North the existence of a concurrent program could only imply an upward bias, as municipalities that are not eligible for TP assistance are also not entitled to receive concurrent financial assistance. Therefore, if treated or eligible non-treated municipalities had benefited from extra money out of the TP pocket, this would have led us to erroneously estimate larger ITT and ATT. As for the South, however, the bias implied by the availability of the concurrent program can in principle go in both directions. If treated municipalities benefited more than control municipalities from the alternative scheme, we are likely to overestimate the parameter $E[Y_1|D=1]–E[Y_1|D=0]$. If treated municipalities benefited less than controls, our results are likely to be downward biased. In the empirical section we investigate the role of the concurrent program for our results by estimating the impact of the TP program only for sub-samples of municipalities (both treated and controls) that did not receive any money under the alternative source of aid.

b. *Opportunity to participate*. TP membership depends not only on the willingness of a municipality to participate in the program, but also on it finding neighboring municipalities that are equally happy to take part in the initiative. Thus, we should take into account the fact that a TP requires group membership. Note that this issue does not affect the estimate of the ITT, as this parameter measures the impact of the eligibility rule irrespective of whether eligible municipalities decide to join the program or not. Group

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the rest of Italy in several respects, such as infrastructure, access to markets, functioning of local governments, social capital etc. Hence it is possible that local omitted variables driving the results remains an issue. This is why we prefer to focus on estimating a non-causal (less ambitious) parameter, which is not plagued by potentially omitted differences between the two areas of the country. Note, however that this alternative strategy was followed in a previous version of this paper with results in line with those presented here.

membership, however, can be a source of bias for both the ATT and the parameter $E[Y_1|D=1]–E[Y_1|D=0]$ because it poses an additional constraint for joining the program, that is, being close to other willing-to-join municipalities. To cope with this problem, in a robustness check we restrain the group of eligible non-treated municipalities to be located within a close distance of TP municipalities. In other words, we limit the eligible non-participating municipalities to those for the which distance would not be an issue for membership.

c. *Funding*. As highlighted in Section 2, a feature of TPs is the sluggishness of disbursements of public money. Note that for programs based on ownership, the role of funding remains a matter of debate. For instance, the current prevailing view at the World Bank is that money cannot buy cooperation among local economic agents and what really matters is the agreement on the development plan. However, public financing can still have a role, perhaps facilitating the process of agreement. To gauge the role of the actual receipt of public money on the effectiveness of the program, we estimate the effect of the program for a sub-sample of TPs, which the Ministry of Economic Development identifies as cases of excellence in this respect. In other words, these TPs exhibit the highest rates of disbursements over allocations.

d. *Longer-term effects*. It is a matter of debate how long it takes for the effects of a policy for local development to materialize. In this respect, and also in light of the sluggishness of disbursements, one could worry that our Census data period (1996-2001) is too short to capture the effects of the policy. To check for this possibility, we make use of a different dataset (the ASIA-UL archive) and extend the analysis to 2004. By this date the first wave of TPs had been implemented for 7 years, the second wave for 5 years, while the disbursements had used up almost all of the budget.

e. *Asymmetric effects in treated areas*. TPs are also expected to help to rationalize and reorganize the economic landscape in the area. For example, in order to exploit agglomeration externalities or knowledge spillover effects, some TPs provided for the creation of an industrial cluster within the area. The internal reorganization of the economic landscape within a TP may thus create an issue in estimating the impact of the program due to the use of PSM in the sample selection. In particular, the PSM discards all the observations for which it does not find a match. In principle, therefore, it is possible that the very municipalities due to receive most of the area's economic activities are thrown away. For instance, take the case in which a TP decides to concentrate industrial activity in a very poor rural municipality and the PSM cannot find a similar municipality among the non-eligible population. We tackle this issue by estimating the effect of the program using the data on *all* the municipalities (that is, not only PSM-selected municipalities) included in a TP and contrasting TPs from the first two waves with TPs from the third wave. This last group comprises municipalities that were untreated in the period 1996-2001, but which would have been treated later under the same policy (see also Busso and Kline, 2008, for a similar experiment).

5. Results

Table 2 shows the balancing properties for the baseline sample (comprising municipalities located in the Centre and North) selected by PSM.[16] The Propensity Score is (Probit) estimated using explanatory variables that are fairly standard in the literature (see, for instance, O'Keefe, 2004): (log of) employment, (log of) number of establishments,

[16] We use the routine proposed by Leuven and Sianesi (2003). Matches are selected by the method of the nearest neighbor with replacement and within a caliper (0.001 percentage points), on the common support of fitted probabilities (see Dehejia and Wahba, 2002).

unemployment rate, (log of) labor productivity, (log of) area of the municipality, activity rate, educational level of population and sector composition of employment. All variables refer to 1996, that is, one year before the first wave of TPs was activated. As is well-known (see, for instance, Blundell et al., 2004) systematic differences in levels between treated and control groups are not a concern, as they can be controlled using the diff-in-diffs methodology. However, violation of the parallel trend assumption may invalidate the estimates. To cope with this issue, we also include as a proxy for the pre-intervention time trend, the variation in (log) employment over the period 1991-1996.[17]

As a result of the PSM, the sample includes 2,247 eligible (Z=1) municipalities (435 of which are participating in the program) and 896 non-eligible (Z=0) municipalities. Columns [1] and [2] report, respectively, the mean and standard deviation for the eligible and non-eligible group for each explanatory variable. Column 3 displays the mean differences. The two groups appear very similar for all observables.

Figure 3 plots the propensity score histogram for the two groups. It should be noted that the area of common support extends over all the [0.1] space, and the relative densities of municipalities everywhere along the support are quite balanced.

Table 3 reports our baseline results. We present diff-in-diffs estimates for two outcomes: the (log of) employment and the (log of) number of plants. All specifications include NUTS2-level fixed effects (since the specification is a diff-in-diffs, they enter also

[17] Note that all the variables considered by EU rules to select *potentially* eligible areas at the NUTS3 level in the Centre and North (unemployment rate, share of industrial employment, decrease in employment for several years) are therefore included in the Propensity Score. Notice also that, as we checked, PSM selected municipalities are also very similar with respect to their pre-intervention time trend measured as the variation in (log) number of plants over the period 1991-1996.

interacted by time). This inclusion is warranted. As a consequence of the matching method employed, control municipalities might be in different regions from those participating in the program. As highlighted by Busso and Kline (2008), this is a desirable feature as the estimates are substantially less susceptible to contamination by spillover or general equilibrium effects than those in which treated and controls share the same region.[18] However, we have to make sure that our estimates are not driven by fixed or time-variant specificity at the region level. The estimation period is 1996-2001. Standard errors are corrected for the potential clustering of the residuals (Moulton, 1990) at the municipality eligibility status interacted by time.

As shown in Column [1], the ITT is not statistically different from zero (point estimates are even negative) for both outcomes. As highlighted in Section 4, this first piece of evidence is not completely informative about the effect on the municipalities that joined the program. As the ITT averages across participating and eligible non-participating municipalities, it could be that a positive effect on the former was attenuated by a negative trend experienced by the latter. However, this possibility has to be discarded. As shown in Column [2], the effect of the program on the participating municipalities (ATT) goes further into negative territory for both employment and plants,[19] while it is now very imprecisely measured. As explained in Section 4, the appropriateness of using Z as an instrumental variable for D relies on the assumption that the rule assigning the eligibility status can be approximated by a random scheme. In Column [3] we test an implication of this assumption, namely that the difference in counterfactual outcomes between eligible

[18] For instance, it is possible for job and plant growth inside TPs to occur at the expense of neighboring municipalities outside the Pact if firms merely relocate between municipalities without expanding production.
[19] The instrument is highly significant. In the first stage the *p* value of the *F* statistics is zero at the first four decimals.

(Z=1) and non-eligible (Z=0) municipalities should be equal to zero (that is, $E[Y_0|Z=1]-E[Y_0|Z=0]$; see equation 1). We implement a placebo experiment where $E[Y_0|Z=1]$ is measured as the outcome of eligible non-treated municipalities while $E[Y_0|Z=0]$ is taken to be the outcome of non-eligible, therefore non-treated, municipalities. The results support our identification strategy.

In Table 4 we present our estimates of the parameter $E[Y_1|D=1]–E[Y_1|D=0]$. As explained above, this parameter does not identify a causal effect because it is likely to be plagued by selection bias. The estimates are reported for both the Centre and North (Column [1]) and the South (Column [2]). Balancing properties for the two groups of treated and eligible non-treated municipalities are shown in the Appendix for both the Centre and North (where the sample includes 593 treated and 463 eligible non-treated municipalities) and the South (669 and 406 municipalities, respectively). Notice that the samples are quite well-balanced for both areas of the country; this is not surprising since these samples are selected among the eligible (i.e. most deprived) municipalities, which are likely to share similar economic conditions. To the extent that the parameter $E[Y_1|D=1]–E[Y_1|D=0]$ estimated for the Centre and North gives us some hints about the direction and size of the bias that characterize this parameter compared with the ATT, we can hope to learn something about the true effect of the policy for the *Mezzogiorno* as well, where estimating the ATT is not feasible.[20] As matter of fact, the results show that in the Centre and North the estimates of the parameter $E[Y_1|D=1]–E[Y_1|D=0]$ basically confirm the result of an overall lack of effectiveness, while providing an upward biased approximation of the ATT. This amounts to saying that if we had been forced

[20] Clearly, the extent to which these results might provide some additional information for policy making relies on the appropriateness of inferring a selection-bias correction from estimates run over a different set of municipalities.

to make use only of information on participating and eligible non-participating municipalities in the Centre and North the inaccuracy of the estimates would have resulted in an overestimation of the effects of the program.[21] As for the South, we find a positive and significant effect for employment. Over the period 1996-2001, the growth of employment in treated municipalities outperformed that of eligible non-treated ones by 2.9 percent (that is, by an annual 0.58 percent). The effect for plants is also positive, but not statistically significant. In light of the overestimation found by estimating the parameter $E[Y_1|D=1]–E[Y_1|D=0]$ for the Centre and North, these results should be taken with a pinch of salt. In any case, we will show below that they do not survive further robustness checks.

Next we check the role of the concurrent program for the empirical evidence presented so far. For the Centre and North, the existence of the concurrent scheme cannot be blamed for the result of an overall lack of effectiveness, as it may only provide a source of upward bias (see, Section 4). On the contrary, the availability of the other scheme could be very relevant for the South, as no restriction on the sign of the bias can be envisaged for this area. To tackle this issue, we exploit the availability of data reporting the municipality-level distribution of Law 488 grants disbursed over the period 1996-2001,[22] and select only municipalities for the which no disbursement was recorded. Then, we matched municipalities by PSM as explained above. As a result, we obtain two samples for the which the existence of concurrent programs is not an issue.[23] By using these samples, Columns [1] and [2] in Table 5 show that both the ITT and the ATT remains basically

[21] Admittedly, for policy purposes the overestimation is of limited importance. Both the causal measure and the non-causal one indicate that the program was not effective in the Centre and North.
[22] These data were kindly provided by Flavia Terribile from the Ministry of Economic Development.
[23] In a previous version of this paper, instead of throwing away municipalities with positive Law 488 disbursement, we added the amount of Law 488 financing received as an additional control. Results were very similar to those presented in this version.

unchanged. However, the parameter $E[Y_1|D=1]–E[Y_1|D=0]$ estimated for the South (Column [3]) turns out now to be not significantly different from zero for both employment and plants.[24]

Table 6 tackles the opportunity-to-participate issue. As explained above, a willing-to-participate municipality might not find neighboring mates. If this is the case, then the estimates of the ATT and that of the parameter $E[Y_1|D=1]–E[Y_1|D=0]$ might still be biased because they do not take into account that participation requires group membership. In this experiment we focus among the eligible non-participating municipalities only on those located close to a TP. That is, we only consider those for the which the distance from an existing TP is not an issue. We include in the eligible non-participant group those municipalities which are among the 20 closest to a participating municipality.[25] Results obtained so far remain undisputed as we fail to find any significant effect of the policy.

The role of the sluggishness of disbursements is the focus of Table 7. We make use of the group of TPs for the which delays in disbursements are not a problem (or a less important problem than for other TPs), as indicated by the Ministry of Economic Development. These TPs include 507 municipalities (221 in the Centre and North and 286 in the South), from both first and second wave TPs, among which we PSM select the treatment group. Estimates are not very different from those obtained considering the whole sample which includes also TPs that, on the contrary, experienced delays in

[24] This might suggest that the positive effect on southern employment estimated in Table 4 could have nothing to do with the TP program. Rather, it could have been driven by the availability of Law 488 grants. This interpretation is not at odds with the findings of Bronzini and de Blasio (2006) and Carlucci and Pellegrini (2003), according to whom the main effect of Law 488 was to promote substitution between labor and capital.

[25] Using smaller bandwidths for this distance delivers similar results.

obtaining the funds allocated by central government.

In Table 8 we extend the analysis from 1996-2001 to 1996-2004. To extent the analysis beyond the last date of availability of Census data (2001), we make use of the ASIA-UL archive, which is available for 2004 and is comparable with the Census. This archive, unfortunately, includes only municipalities of over 5,000 inhabitants. Therefore, the estimates presented in this table are not directly comparable with those presented in Table 3 and Table 4, which refer also to smaller municipalities. Note, however, that the results referring to 1996-2001 for the sub-sample of ASIA-UL municipalities are remarkably similar to those presented for the whole sample of municipalities (which is why we do not report them). Results from this experiment are still very similar to those obtained before.

Results obtained so far support the idea that the policy did not change the dynamics of employment and number of plants. In principle, however, it is possible that the effects of the intervention were asymmetrically distributed over the municipalities included in a Pact and the PSM failed to find adequate matches for the municipalities that received the bulk of the intervention. By using the data on *all* the municipalities (that is, not only PSM-selected TP municipalities) included in a TP we should be able to cope with this issue. In the experiment in Table 9 we contrast for the period 1996-2001 all the municipalities included in one of the 51 TPs of the first and second waves (activated, respectively, by 1997 and 1999) with a control group of (all) municipalities included in one of the 28 TPs of the third wave (activated from 2002). The estimation sample comprises 587 municipalities for the Pacts of the first and second waves and 241 municipalities for the Pacts of the third wave. Note that in this experiment we are comparing treated units with untreated ones, which would be

treated later on under the same policy. Therefore, the selection issues relating to voluntarily participation and those pertaining to group membership are likely to be minimized. The results presented in Table 9 confirm that the policy had an insignificant effect on both employment and number of plants.

6. Concluding remarks

The TP program is a key policy instrument for fostering growth in the disadvantaged areas of Italy. Since its introduction in 1996, it has represented a great novelty in the landscape of Italy's traditional forms of public aid for backward areas (in particular, the South), which date back to the 1950s. The novelty lies in the bottom-up approach of the TP program. In contrast with the centralized framework of previous schemes, a TP involves an agreement between local public authorities, entrepreneurs and trade unions on a development plan, which is later endorsed and funded by central government. In other words, unlike previous intervention schemes a TP puts great emphasis on the ownership of the program by local stakeholders. In this paper we assess the effectiveness of the TP program over the period 1996-2004 and find very gloomy results. When evaluated through a counterfactual analysis, using the dynamics of employment and plants as measures of efficacy, we find very little support for the effectiveness of the program. Moreover, the results are very robust to a number of specification tests and sensitivity exercises.

While the depressing results presented in the paper can hardly be overstated, the analysis is not able to provide us with any justification of *why* the program has failed to reach its targets. Below, we review the relative merits of two possible explanations for this failure.

The first explanation relates to a deficiency of public money. Indeed, the total amount of public expenditure for each TP was limited to 50 million euros and this amount might have been insufficient to trigger an autonomous growth process. Unfortunately, our analysis cannot offer the final word on this issue. For instance, if one believes that backward areas were locked in a poverty trap, one cannot exclude that the total amount of public money was less than that needed to escape. However, we also note that the results presented above, in particular those regarding the role of disbursements over allocations, do not lend much credit to this explanation, as we found that the role of actual funding for the overall performance of the program was negligible. Moreover, the program did not work in either the gravely underdeveloped South or in the relatively less deprived areas of the Centre and North. As the latter areas are less likely to find themselves in a poverty trap, the suitability of this explanation requires an effect to be found.

The second possible explanation for the failure of the initiative is a cynical one. As recognized by Drazen and Isard (2004), building or creating ownership when it is weak can be a daunting task. Moreover, the extent to which ownership can be improved and fostered through policy actions may be seriously impaired when backward areas have a tradition of heavy public intervention (Alesina et al., 2001) that discourages the development of market activities (and a long-standing practice of misuse of public money, as is often the case in the South of Italy). In such circumstances, it is very unlikely that the TP program would have built enough ownership and, therefore, would have worked properly (see Rossi, 2005). A more likely scenario is that rent-seeking predominates: the agreement could have been signed by the local stakeholders for the sole purpose of exploiting public funding, regardless of the local development target of the TP program. While our results are not in contrast with this cynical interpretation, they can hardly be considered, again, a final proof.

Perhaps, this is a good thing: such an explanation would leave very little hope for the development prospects of Italy's backward regions.

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Figure 1

MAP OF ELIGIBLE AND NON-ELIGIBLE AREAS

Figure 2

MAP OF ELIGIBLE, NON-ELIGIBLE AND TREATED AREAS

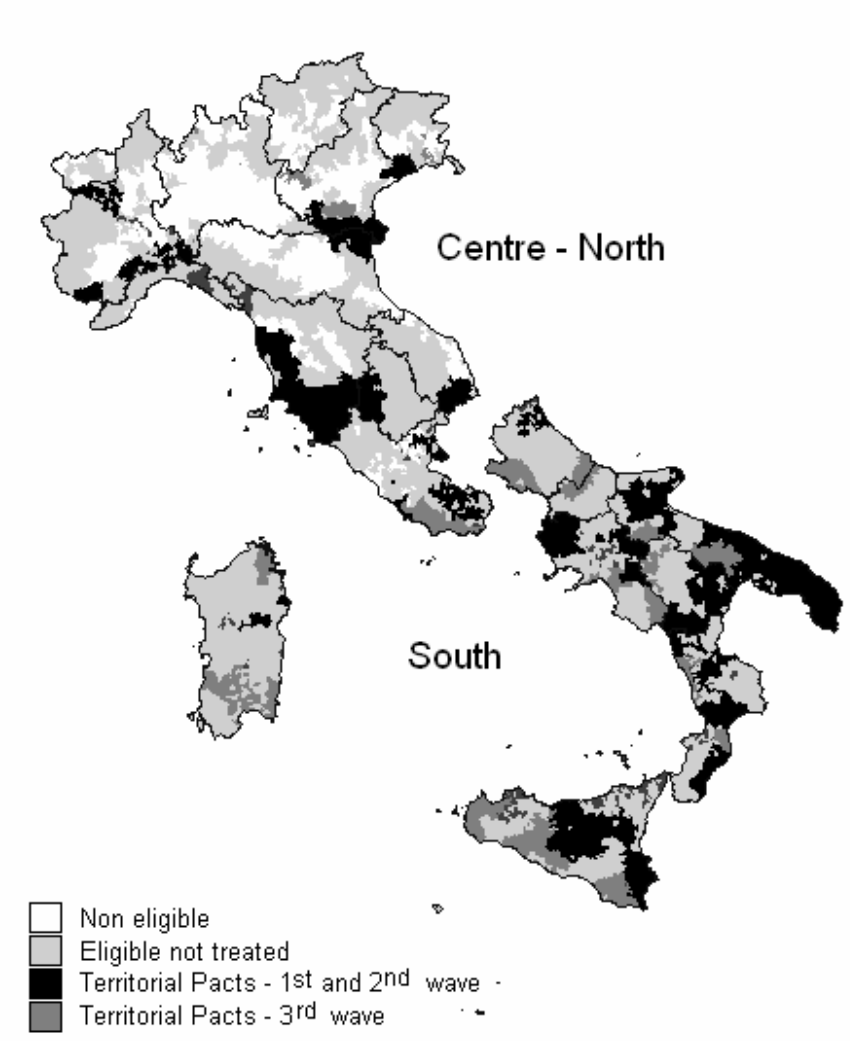

Figure 3

DISTRIBUTION OF THE PROPENSITY SCORES FOR ELIGIBLE AND NON-ELIGIBLE MUNICIPALITIES
(BASELINE SAMPLE)

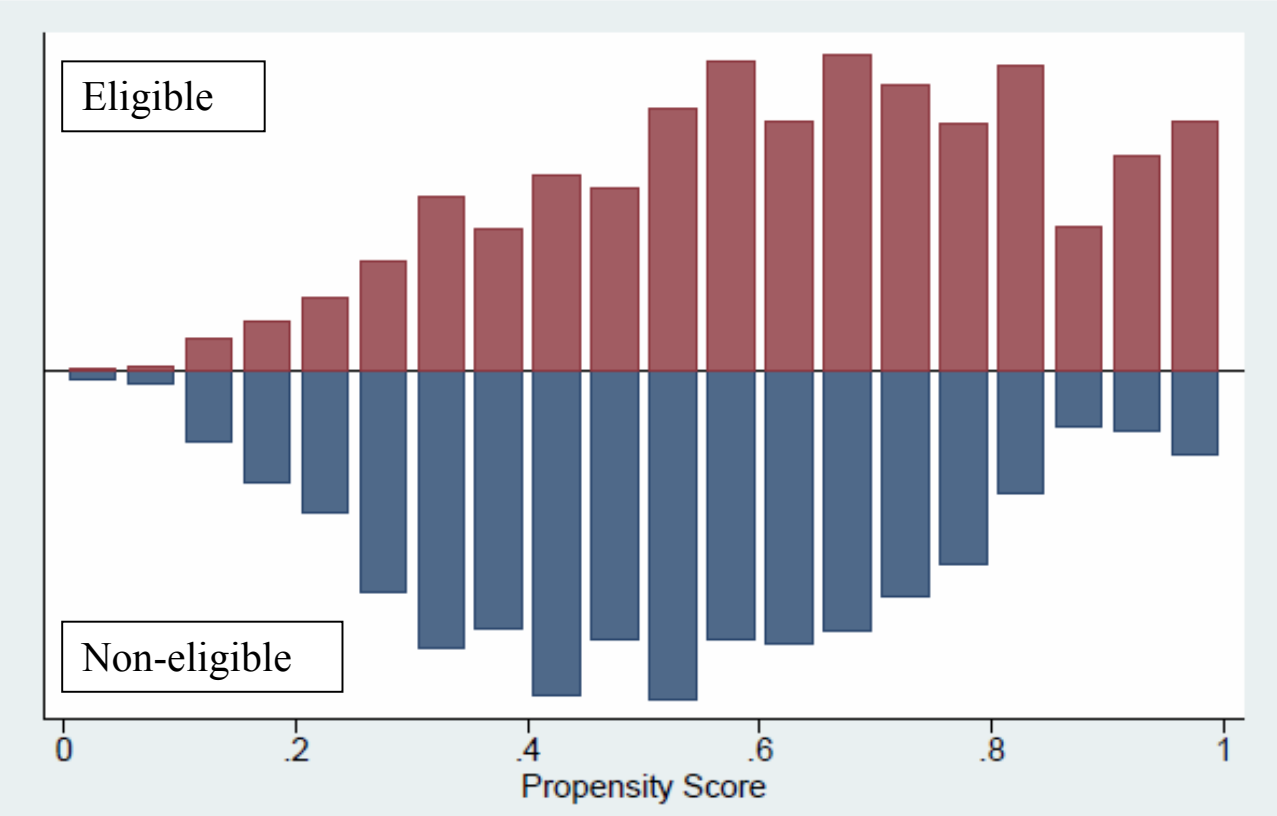

TABLE 1. SUMMARY STATISTICS FOR TERRITORIAL PACTS

		Number of TPs	Number of municipalities	Average population	Average TP area (1)	TP unemployment rate
First wave	*Centre and North*	-	-	-	-	-
	South	*12*	*336*	*350,077*	*1,430*	*23.6*
	Total	12	336	350,077	1,430	23.6
Second wave	*Centre and North*	*19*	*613*	*17,1029*	*1,389*	*10.4*
	South	*20*	*414*	*226,016*	*1,260*	*21.3*
	Total	39	1,027	199,228	1,323	16.0
Total	*Centre and North*	*19*	*613*	*171,029*	*1,389*	*10.4*
	South	*32*	*750*	*27,2539*	*1,324*	*22.2*
	Total	51	1,363	234,721	1,348	17.8

Notes. Census data. (1) Square kilometers.

TABLE 2. BALANCING PROPERTIES FOR THE BASELINE SAMPLE
(CENTRE AND NORTH)

	Mean and standard deviations		
	[1] Eligible	[2] Non-eligible	[3] Mean differences
Log(employment)	5.782	5.753	0.028
	[1.721]	[1.648]	(0.060)
Log(plants)	4.701	4.711	-0.010
	[1.465]	[1.426]	(0.051)
ΔLog(employment)1991-1996	-0.073	-0.076	0.002
	[0.284]	[0.327]	(0.011)
Log(labor productivity)	1.430	1.432	-0.001
	[0.095]	[0.099]	(0.003)
Log(surface)	3.031	3.064	-0.032
	[0.973]	[0.866]	(0.033)
Activity rate	66.141	66.012	0.129
	[4.541]	[4.212]	(0.156)
Share of industrial employment	35.652	34.743	0.909
	[22.403]	[21.790]	(0.788)
Share secondary education	14.397	14.733	-0.336
	[4.473]	[4.079]	(0.252)
Share tertiary education	1.767	1.814	-0.046
	[1.210]	[1.062]	(0.040)
Unemployment rate	0.076	0.075	0.001
	[0.038]	[0.043]	(0.001)

Notes. The sample includes 435 treated, 1812 eligible non-treated and 896 non-eligible municipalities. Columns [1] and [2] report mean and standard deviations (in square brackets) of the variables. Mean differences (column [3]) are weighted according to the number of matches carried by each observation (standard errors in parenthesis). All variables refer to 1996. All variables are measured at the municipality level, except unemployment rate and labor productivity which are only available at the local labor market level.

TABLE 3. BASELINE RESULTS

	[1]		[2]		[3]	
Parameter	ITT		ATT		$E[Y_0\|Z=1] - E[Y_0\|Z=0]$	
Dep. variable	Employment	Plant	Employment	Plant	Employment	Plant
	-0.025	-0.022	-0.146	-0.132	-0.022	-0.022
	(0.058)	(0.049)	(0.300)	(0.235)	(0.028)	(0.028)
Area	Centre-North		Centre-North		Centre-North	
Period	1996-2001		1996-2001		1996-2001	
Observations	6,286		6,286		5,416	

Notes. Census data. Diff-in-diffs results. All specifications include NUTS2-level fixed effects (also interacted by time). The sample includes 435 treated, 1812 eligible non-treated and 896 non-eligible municipalities. See Table 1 for balancing properties. Standard errors (in parenthesis below coefficient estimates) are corrected for the potential clustering of the residual at the municipality eligibility status interacted by time. *** (**) [*] denotes significance at the 1% (5%) [10%] level.

TABLE 4. ESTIMATES OF E[Y1|D=1] – E[Y1|D=0]

	[1]		[2]	
Parameter	E[Y1|D=1] – E[Y1|D=0]		E[Y1|D=1] – E[Y1|D=0]	
Dep. variable	Employment	Plant	Employment	Plant
	0.021	0.000	0.029***	0.023
	(0.148)	(0.116)	(0.004)	(0.016)
Area	Centre-North		South	
Period	1996-2001		1996-2001	
Observations	2,122		2,208	

Notes. Census data. Diff-in-diffs results. All specifications include NUTS2-level fixed effects (also interacted by time). The sample includes 600 treated and 461 non-treated municipalities for the Centre and North and 668 treated and 436 non-treated municipalities for the South. See Appendix 2 for balancing properties. Standard errors (in parenthesis below coefficient estimates) are corrected for the potential clustering of the residual at the at the municipality treatment status interacted by time. *** (**) [*] denotes significance at the 1% (5%) [10%] level.

TABLE 5. ROBUSTNESS CHECK: CONCURRENT PROGRAMS

	[1]		[2]		[3]	
Parameter	ITT		ATT		E[Y1|D=1] – E[Y1|D=0]	
Dep. variable	Employment	Plant	Employment	Plant	Employment	Plant
	-0.026	-0.024	-0.180	-0.161	0.009	0.017
	(0.110)	(0.095)	(0.802)	(0.668)	(0.084)	(0.081)
Area	Centre-North		Centre-North		South	
Period	1996-2001		1996-2001		1996-2001	
Observations	4,174		4,174		612	

Notes. Census data. Diff-in-diffs results. All specifications include NUTS2-level fixed effects (also interacted by time). The sample includes 244 treated, 1,219 eligible non-treated and 624 non-eligible municipalities for the Centre and North, and 171 treated and 135 non-treated municipalities for South. Standard errors (in parenthesis below coefficient estimates) are corrected for the potential clustering of the residual at the municipality eligibility (in [1] and [2]) or treatment (in [3]) status interacted by time. *** (**) [*] denotes significance at the 1% (5%) [10%] level.

TABLE 6. ROBUSTNESS CHECK: OPPORTUNITY TO PARTICIPATE

	[1]		[2]		[3]	
Parameter	ITT		ATT		E[Y1|D=1] – E[Y1|D=0]	
Dep. variable	Employment	Plant	Employment	Plant	Employment	Plant
	-0.015	-0.012	-0.031	-0.026	0.058	0.021
	(0.160)	(0.133)	(0.329)	(0.281)	(0.027)	(0.025)
Area	Centre-North		Centre-North		South	
Period	1996-2001		1996-2001		1996-2001	
Observations	2,222		2,222		1,854	

Notes. Census data. Diff-in-diffs results. All specifications include NUTS2-level fixed effects (also interacted by time). The sample includes 244 treated, 1,219 eligible non-treated and 624 non-eligible municipalities for the Centre and North, and 171 treated, and 135 non-treated municipalities for the South. Standard errors (in parenthesis below coefficient estimates) are corrected for the potential clustering of the residual at the municipality eligibility (in [1] and [2]) or treatment (in [3]) status interacted by time. *** (**) [*] denotes significance at the 1% (5%) [10%] level.

TABLE 7. ROBUSTNESS CHECK: FUNDING

	[1]		[2]		[3]	
Parameter	ITT		ATT		E[Y1|D=1] – E[Y1|D=0]	
Dep. variable	Employment	Plant	Employment	Plant	Employment	Plant
	0.003	-0.013	0.043	-0.200	0.029	0.023
	(0.038)	(0.036)	(0.566)	(0.530)	(0.026)	(0.026)
Area	Centre-North		Centre-North		South	
Period	1996-2001		1996-2001		1996-2001	
Observations	5,754		5,754		966	

Notes. Census data. Diff-in-diffs results. All specifications include NUTS2-level fixed effects (also interacted by time). The sample includes 158 treated, 1,828 eligible non-treated and 891 non-eligible municipalities for the Centre and North, and 261 treated and 222 non-treated municipalities for the South. Standard errors (in parenthesis below coefficient estimates) are corrected for the potential clustering of the residual at the municipality eligibility (in [1] and [2]) or treatment (in [3]) status interacted by time. *** (**) [*] denotes significance at the 1% (5%) [10%] level.

TABLE 8. ROBUSTNESS CHECK: LONGER TERM EFFECTS

	[1]		[2]		[3]	
Parameter	ITT		ATT		E[Y1|D=1] – E[Y1|D=0]	
Dep. variable	Employment	Plant	Employment	Plant	Employment	Plant
	-0.032	-0.026	-0.175	-0.143	0.019	0.015
	(0.058)	(0.039)	(0.339)	(0.152)	(0.111)	(0.086)
Area	Centre-North		Centre-North		South	
Period	1996-2004		1996-2004		1996-2004	
Observations	928		928		1,026	

Notes. Census data. Diff-in-diffs results. All specifications include NUTS2-level fixed effects (also interacted by time). The sample includes 48 treated, 227 eligible non-treated and 189 non-eligible municipalities for Centre and North, and 347 treated and 166 non-treated municipalities for the South. Standard errors (in parenthesis below coefficient estimates) are corrected for the potential clustering of the residual at the municipality eligibility (in [1] and [2]) or treatment (in [3]) status interacted by time. *** (**) [*] denotes significance at the 1% (5%) [10%] level.

TABLE 9. ROBUSTNESS CHECK: ASYMMETRIC EFFECTS

	[1]	
Parameter	E[Y1|D=1] – E[Y1|D=0]	
Dep. variable	Employment	Plant
	-0.011	-0.014
	(0.103)	(0.085)
Area	Whole country	
Period	1996-2001	
Observations	1,930	

Notes. Census data. Diff-in-diffs results. All specifications include NUTS2-level fixed effects (also interacted by time). The sample includes 679 treated and 286 non-treated municipalities. Standard errors (in parenthesis below coefficient estimates) are corrected for the potential clustering of the residual at the municipality tratment status interacted by time. *** (**) [*] denotes significance at the 1% (5%) [10%] level.

APPENDIX. BALANCING PROPERTIES FOR THE SAMPLES USED TO ESTIMATE E[Y1|D=1]–E[Y1|D=0]

	Mean and standard deviations		
	[1] Treated	[2] Non-treated	[3] Mean differences
	CENTRE and NORTH		
Log(employment)	4.693	4.757	-0.064
	[1.517]	[1.496]	(0.123)
Log(plants)	3.924	3.933	-0.009
	[1.318]	[1.287]	(0.106)
ΔLog(employment)1991-1996	-0.133	-0.133	0.000
	(0.319)	(0.305)	(0.025)
Log(labor productivity)	1.412	1.420	-0.008
	[0.100]	[0.098]	(0.008)
Log(surface)	2.929	2.851	0.078
	[1.072]	[0.858]	(0.079)
Activity rate	62.293	62.540	-0.247
	[5.255]	[4.594]	(0.403)
Share of industrial employment	24.605	24.067	0.537
	[19.073]	[18.944]	(1.559)
Share secondary education	13.122	13.374	-0.252
	[4.386]	[4.645]	(0.370)
Share tertiary education	1.683	1.746	-0.063
	[1.168]	[1.297]	(0.101)
Unemployment rate	0.088	0.086	0.001
	[0.034]	[0.041]	(0.003)
	SOUTH		
Log(employment)	4.797	4.900	-0.103
	[1.003]	[0.901]	(0.109)
Log(plants)	4.294	4.382	-0.088
	[0.906]	[0.810]	(0.098)
ΔLog(employment)1991-1996	-0.323	-0.323	0.000
	(0.332)	(0.266)	(0.323)
Log(labor productivity)	1.117	1.078	0.039
	[0.178]	[0.222]	(0.033)
Log(surface)	3.229	3.392	-0.162
	[0.761]	[0.953]	(0.098)
Activity rate	63.117	63.519	-0.402
	[3.834]	[3.394]	(0.414)
Share of industrial employment	17.288	17.580	-0.292
	[12.204]	[10.987]	(1.327)
Share secondary education	11.831	11.417	0.413
	[3.749]	[4.112]	(0.449)
Share tertiary education	1.744	1.745	0.000
	[1.032]	[1.112]	(0.122)
Unemployment rate	0.249	0.245	0.004
	[0.068]	[0.070]	(0.007)

Notes. The sample includes 593 treated and 463 eligible non-treated municipalities for the Centre and North and 669 treated and 406 eligible non-treated municipalities for the South. Columns [1] and [2] report mean and standard deviations (in squared brackets) of the variables. Mean differences (column [3]) are weighted according to the number of matches carried by each observation (standard errors in parenthesis). All variables refer to 1996. All variables are measured at the municipality level, except unemployment rate and labor productivity which are only available at the local labor market level.

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