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Policies for Local Development: an Evaluation of Italy's Territorial Pacts[*]

(Please do not quote)

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Abstract
From 1996, Territorial Pacts (TPs) have represented one the most important instruments designed to foster growth in disadvantaged areas (those allowed to receive EU funds). A TP is an agreement signed by the local governments and the representatives of the 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 development plan, which includes a number of private and public investments for which public funding is provided. This paper evaluates the effectiveness of TPs in the period 1996-2004, by comparing the economic performance – both in terms of employment and number of plants – of the municipalities belonging to a TP with a comparison sample of cities not involved in the policy. The results suggest that the program has been quite ineffective.
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1. Introduction

This paper examines the impact of Italy's program Territorial Pacts (TPs) on the dynamics of employment and number of plants.

Established in 1996, the TP program is an area-based initiative aimed at triggering growth and employment in the lagging regions of the country. The program is based on a full "bottom-up" approach: a Pact is an agreement signed by the local governments and the representatives of the 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, which include a number of private and public investments for which public funding is provided. The program represents one of the most important area-based initiative in Italy: as of December 2006, the Ministry for Economic Development has approved 220 TPs; almost half of the Italian population lived in a municipality belonging to a TP (80 per cent in the South); budget allocations for the program reached 5.5 billion euros. All TPs locate in areas allowed to receive public funds by the European Union (i.e. area Objective 1, in the South and areas Objective 2 and 5b in the Center-North of the country[1]).

In this paper, we perform a counterfactual analysis of the economic impact of the program in the period 1996-2004. We compare the economic performance – both in terms of employment and number of plants – of the municipality belonging to a TP with a comparison sample of cities not involved in the policy. Given the peculiarities of the policy, the identification of the effect is not straightforward. In particular, participation in the program requires that a city is located in an eligible area, that it joins a TP on a voluntary basis, and that it coordinates with neighbors (that should similarly be happy to take part in the initiative). Therefore, a number of econometric issues must be tackled, as post-program economic performance might be driven by *non-random program placement*, *self-selection* or *group-membership bias*. Additionally, compared to the financial plans at the time the local agreements were signed, the actual disbursements of public money were delayed; while the existence of concurrent programs might have crowded-out the most efficient firms, originally involved in a TP.

This paper is related to the recent literature that assesses the impact of policies for local development by using a program evaluation approach. For instance, Bondonio and Engberg (2000), O'Keefe (2004) and Bondonio and Greenebaum (2007), studies the effectiveness of US enterprise zones, Rathelot and Sillard (2007) analyze that of French Zones franche urbaine , while Criscuolo et al. (2008) focus on the UK *regional selective assistance*. As for Italy, Bronzini e de Blasio (2006) and Bronzini et al. (2008) consider two programs of investment incentives for firms in lagging areas (grants and fiscal bonus, respectively).

[1] We refer to the period 1994-1999 of EU structural funds.

Our results suggest that TPs have been quite ineffective, as employment and firm growth of the municipalities involved in the policy do not significantly differ from that of the comparison group of cities. This result is robust to a number of specifications checks, which allow to disentangle the role of the confounding factors due to policy peculiarities. In particular, it does not seem to depend on the initial level of economic development of the area. Moreover, it does not seem to be driven by self-selection or the opportunity to joint the program given the neighbors choices. Finally, the actual amount of public money disbursed does not seem to have had a role.

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 we employ for the assessment of the TP, in the light of the confounding factors that might bias the evaluation. Section 5 shows the econometric results. Section 6 concludes, offering some suggestive interpretations for the findings.

2. The program

TPs were established by the 1997 National Budget Law (Law 662/1996), in a context of general re-organization and rationalization of public intervention in the lagging areas (mainly the South of Italy). TPs belong to a special class of public programs named "Negotiated Planning," which represented a substantial departure from previous policies aimed to regional convergence that were featured by a substantial degree of *centralism*. These programs place a particular attention to the involvement of local communities in the policy design. The idea is that, in order to maximize their effectiveness, development programs should originate from local stakeholders. In particular, an agreement among agents in potential conflict - like, e.g., trade unions and entrepreneurs - should be better capable to deliver an effective development strategy, in which all agents are willing to cooperate to achieve the common goal of economic growth[2].

Among this class of programs, TPs represent one the most important instruments in terms both of population and of public aid involved. As percentage of the total public resources committed to deprived areas in Italy, TPs rank in the third position, after the investment grants provided by law 488/1992 and the tax credit for investment envisaged by law 388/2000 (RGSEP, 2006).

A Pact is an agreement signed by the local governments and the representatives of the civil society (mainly, entrepreneurs and trade unions) of a number of *neighboring municipalities*, containing the indication of a *local coordinating authority* in charge of the policy; and a fully-fledged *development plan*.

As for the size of a TP, no requirement (for instance, in terms of number of municipalities or

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

mass of the population involved) was imposed by law. In principle, any small number (even only two!) of neighboring municipalities could get together and create a TP. In reality, fixed costs to set-up the local coordinator agent and the development plan (see below) induced large groupings. As matter of fact, for the 51 TPs involved in our evaluation the average number of municipality is 27 and the average population amounts to 216 thousand individuals.

The local authority has a crucial role in the design and management of the program. Either a local public administration (like, for example, the provincial administration) or a private local association or a newly created company might be chosen for this role. The local authority should promote coordination and sanction non-cooperative behaviors. It has the role of conciliator in case of conflicts arising among the participants to the TP. The local authority is the only manager of the public funding and it has the right to repeal the public contribution to a private firm, if the private contribution falls below the minimum required by law (30 per cent of investment outlines, see below). Moreover, it has the right to reassign the existing contributions to new members in case of one participant withdraw or its contribution is revoked.

The development plan includes both private and public investments. Public funding is provided for both. In particular, total public money is limited to 50 millions euros for each PT. Investments in public infrastructure should not exceed 15 millions, while the remaining amount could be awarded to private firms as grants for creation, extension, modernization, conversion, restructuring, reactivation and relocation of productive plants. Grants, in any case, must not exceed 70 per cent of the value of the investment project carried out by a firm. The evaluation of the economic soundness of investment projects is in the hand of private banks (only projects that pass this ex-ante profitability check are then included in the development plan). Once signed, the agreement is subsequently endorsed by the Central Government. With endorsement a TP is said to be established, that is it can receive public money. Funding however follows the establishment only slowly (see below). Upon the receipt of the first installment, a TP is said to be activated. According to the 1996 Budget law, TPs are allowed to be created across the entire country, but public funding is limited to the areas designed as "Objective 1", "Objective 2" and "Objective 5b" by the European Structural Fund cycle 1994-1999[3].

TPs were approved in several waves. A first wave of 12 TPs was established and activated in 1997. A second wave of 39 TPs was established in 1997 and activated in 1999. This group of 51 TPs represents our reference sample. Given the data-availability constraint, this sample is ideal for evaluation purposes. As explained in Section 3, we will basically use information available for 1996, 2001, and 2004. As no TP was activated before 1997, year 1996 can be used as perfect pre-intervention period. Focusing on the earliest waves gives us the opportunity to have a time-window long enough (in 2004, 7 and 5 years respectively for the two waves) to disentangle the effects of the program. After the first two waves 10 additional TPs (so called European TPs) were established and activated in 1998 with an endorsement procedure of the European Commission. European TPs are

[3] As matter of fact, there are no TP without public funding; that is, all TPs locate in underdeveloped areas.

financed by the European Structural Funds and they differ from the first and second waves (so-called national TP) under a number of aspects. First, the selection of industrial projects was much more accurate, 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 initiatives like investments on local labor market qualification. Finally, 169 additional national TPs were established in 2001 and gradually activated from 2002 to 2006.[4] At the end of 2006, there were 220 national and 10 European TPs.

At the end of the Nineties, 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[5].

While this paper provides the first econometric evaluation of TPs, a number of previous studies has dealt with implementation issues[6]. Magnatti et al. (2005) highlights the slowness in the provision of public money. For instance, on June 30th 2002, the share of money disbursed on the total budget allocated to the program was 44.3% for the first wave and 27.8% for the second wave. While the sluggishness of disbursements could be to some extent explained by the low efficiency of the Italian public spending process, withdrawals of firms from the program could have played a role. According to DPS (2003), withdrawals could be explained by the existence of concurrent programs. In particular, the investment grants provided under the law 488/1992 were considered by many entrepreneurs as a quicker way to obtain public funding.[7] In the empirical section below, we will analyze the role of the paucity of disbursements on the overall efficiency of TPs[8]. In particular, we will be exploiting a sample of TPs with an high rate of disbursements over allocations and check whether their performance is anyhow different form that of low disbursement-rate TPs. We will also control for the availability of concurrent aid program at the local level.

[4] This wave includes 91 TPs aimed at sustaining the agricultural sector in rural areas (so-called agricultural TPs).
[5] The reference here is to all the National TPs, including those activated in the period 2002-2006.
[6] See, for example, Sviluppo Italia (2001), Cersosimo (2000), Mirabelli (2000), Cerosimo e Wolleb (2001), Barbera (2001). These papers provide also a number of case studies. Casavola and Utili (2002), instead, provide a statistical analysis on the causes of the heterogeneous economic performance of the TPs.
[7] Corte dei Conti (2002) points out a lack of selection in the ex-ante analysis of the investment profitability carried out by private banks. In some cases, the local authority repealed funding, because the private contribution fell below the minimum required (30% of the total value of the investment). In some case, the re-assignment was harmed by the fact that some revocations ended up in long and painful lawsuits.
[8] However, it is important to emphasize (see also that Cersosimo and Wolleb, 2001) that the slowness in public spending should not have been the main obstacle to the working of the policy. The aim of the TP is the pact itself, that is the involvement of many local actors to sign an agreement that, in turn, should be able to increase the economic cooperation with an area. Therefore, public contribution should represent an incentive to collaborate, rather than the ultimate goal.

3. Data

Our main data source is Istat (Italian Statistical Office) Census, which endows us with the key performance and selection variables used throughout the paper. We use Census releases mainly for the years 1996 and 2001 (we also exploit 1991 data for a robustness exercise). These data have a number of advantages and some drawbacks. In particular, censuses provide us with a large number of variables for all Italian municipalities (i.e. as for the coverage of the national territory, there is no sample selection problems). Moreover, as explained in the previous Section, 1996 release represent an excellent "before" year, since its information relates to the starting period of the program. A drawback in the use of Census is that the latest release (2001) might be considered as too early date to display the impact of TPs on local economies. While this problem is more relevant for the second wave of TPs, which were activated in 1999, it might also impact on the first wave because of the sluggishness in disbursements. In order to cope with this problem, we integrated our Census dataset with data from ASIA-UL archive (*Archivio Statistico Imprese Attive – Unità Locali*; Statistical archive for active firms – establishments), released by Istat as well 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 disclosure policies, it does not include information for cities with less than 5,000 inhabitants. Therefore, once we extend the analysis to 2004, we are forced to restrict it to municipalities larger than that threshold.

Evaluation analyses of this paper mostly concentrate on the first and the second waves of TPs. These include 51 TPs, 1,363 municipal administrations with more than 11 million of inhabitants.

4. Identification strategy

The aim of the paper is to assess the impact of TPs in terms of employment and number of plants. We use a counterfactual analysis, which compares at the municipality level the performance in presence of the program with the hypothetical performance in its absence. Empirically, we adopt a difference-in-difference (*diff-in-diff*) framework (see, for example, Angrist and Krueger, 1999; Card, 1999; and Meyer, 1995); that is, we use pre-period difference in outcomes between treatment and control group to control for pre-existing differences between the groups. Note that the simplest version of a diff-in-diff equation is the following:

$$Y_{it} = \alpha + \beta_1 * TP_i + \beta_2 * POST_t + \beta_3 * (TP_i * POST_t) + \gamma_1 * X_i + \gamma_2 * (X_i * POST_t) + \varepsilon_{it} \quad (1)$$

where Y_{it} is the outcome variable, for instance, the (log of) employment level or the log of) the number of establishments in town *i* at time *t*. TP_i is a dummy variable equal to one when municipality *i* belongs to a TP. $POST_t$ is a dummy equal to one in the years following the activation of the program.

X_i represent a number of additional controls at municipal level which are included in each regression in levels and interacted with dummy $POST_t$. In the case of Eq. (1), the coefficient of interest is β_3, which represents the differential trend due to the participation to the program (it is called ATT, the effect of the treatment on the treated).[9]

We adapt this simple strategy to take into account the specific characteristics of the TP program. Next paragraph explains how we tackle the potential biases due to non-random program placement and self-selection. Subsequently, we detail how a number of additional issues are dealt with empirically. In particular, we take into account the effects of: the existence of concurrent programs, the varying intensity of disbursements, and the potential asymmetric location of activities within TPs. Finally, we discuss potential long run effects that our identification strategy might fail to capture.

4.1 Eligibility and Participation

TPs do not represent a random sample of the Italian population of municipalities, as they belong to the most deprived areas of the country as defined by European rules (see Section 2). Figure 1 illustrates both eligible and non-eligible areas. Southern regions are all eligible, while the coverage of the policy for center-northern regions is only partial. This circumstance may create a bias (non-random program placement) in the estimation of the impact of the policy, as a differential performance between eligible and non-eligible areas could be due to the effects of the pre-existing disparities. For example, according to a neoclassical convergence process (Solow, 1956), lagging areas area likely to grow at a faster pace than more advanced regions, due to the existence of decreasing returns of the production factors. On the other hand, according to a New Economic Geography mechanism (Baldwin et al., 2003), more developed areas might occur to grow faster due to the productivity effects of agglomeration economies.

Note that our unit of observation is a city. However, the eligibility status is not established on the basis of the economic conditions prevailing at the city level. Rather, eligibility depends on parameters defined at a larger spatial scale, namely NUTS2 (for the South of Italy, Objective 1) and NUTS3 (for the areas of the Centre-North of Italy, Objectives 2 and 5b). That is, eligibility does not necessarily reflect the economic development of each municipality. Moreover, while all municipalities in the South belong to Objective 1, not all the northern cities located in distressed NUTS3 regions belong to the Objectives 2 and 5b. Due to limitations in the European budget, the selection of cities actually belonging to these Objectives was agreed within a political bargaining process between national and European authorities. In other words, since eligibility is defined on the basis of NUTS2

[9] In other words, the diff-in-diff estimator provides an unbiased estimate of the treatment effect under the assumption that absent the treatment the outcomes in the two groups would have followed parallel trends.

and NUTS3 averages,[10] we can hope to find cities that share similar pre-intervention economic condition but are differently located in eligible and non-eligible areas.

Being part of a TP is a voluntarily decision. Indeed, only a sub-sample of eligible municipalities (see: Figure 2) decides to gather and form a TP (self-selection bias). This circumstance may affect our estimates. For example, it is plausible to imagine that TP municipalities are more committed to develop than their non-TP counterparts. Thus, self-selection might imply that we mistakenly attribute to the effect of the policy some city characteristics that are already there, irrespective of the program. The strategy we adopt to tackle the self-selection bias is based on the concept of "eligibility effect", rather than "treatment effect" (Murdoch, 1998; van der Klaauw, 2007). In particular, we estimate the effect of having the possibility to join the PT program, rather than that related to the actual participation into the program. Empirically, Eq. (1) is augmented by adding a third comparison group, denoted by $ELIG_i$, which refers to cities that belong to an eligible area but are not part of the program:

$$Y_{it} = \alpha + \beta_1 * TP_i + \beta_2 * ELIG_i + \beta_3 * POST_t + \beta_4 * (TP_i * POST_t) + \beta_5 * (ELIG_i * POST_t) + \gamma * X_{it} + \varepsilon_{it} \quad (2)$$

The coefficients of interest are now β_4 and β_5, whose average, called the Average Eligibility Effect (AEE), represents the differential trend due to the eligibility to the program. By estimating the AEE, we actually try to counterbalance the positive self-selection of the treated group with the negative self-selection of those municipalities which had the possibility to join the program but decided not to do so.

The reliability of this approach is tested by using an Instrumental Variables approach proposed by Battistin and Rettore (2008).

In the light of the non-random program placement issue, this strategy can be sensible if we can find cities that before the start of the program display high similarity. This is accomplished by using the Propensity Score Matching (PSM, Rosenbaum and Rubin, 1983)[11] in two stages. In the first stage, each treated municipality is matched with the eligible non-TP city displaying the nearest propensity score. In the second stage, each city belonging to this new treatment group (in which the positive self-selection of treated is balanced with the negative self-selection of the eligible non treated) is matched with the non-eligible municipality displaying the nearest propensity score. The Propensity Score is (Probit) estimated by using explanatory variables that are standard in the literature (see, for instance, O'Keefe, 2004): (log of) employment, (log of) number of firms, unemployment rate, labor

[10] Or even by a political bargaining process from the which some equally developed areas are differently included into the areas of eligibility.

[11] We used the routine proposed by Leuven and Sianesi (2003). Matches were selected with the method of the nearest neighbor with replacement, on the common support of fitted probabilities (see: Dehejia and Wahba, 2002).

productivity, (log of) total population, (log of) city area, activity rate, educational level of population, sector composition of employment and altitude.[12] All variables refer to 1996.

The estimate of the AEE raises two main additional concerns.

First, the non eligible group comprises of municipalities located exclusively in the North of the country, as all southern regions are eligible under Objective 1 (see: Fig. 1). Therefore, southern treated and eligible non treated cities can only be compared with northern non eligible counterparts. Given the well-known differences between the two areas of the country, this might represent a problem. In order to cope with this, our econometric specifications below include spatial fixed effects at the NUTS1 level, which differentiate out time-invariant region characteristics and NUTS1 spatial fixed effects interacted with time dummies, to control for time-varying region-specific factors (basically, they capture potential misalignments across business cycles at the NUTS1 region level).

Second, there is an issue of opportunity to participate to a TP. To be sure, TP membership depends not only on the willingness (self-selection) of a municipality to participate in the program, but also that it finds neighboring municipality that are equally happy to take part in the initiative. Suppose, for instance, that municipality A belongs to a TP (it self-selects in), while city B on the border of A decides not to participate into the program (it self-selects out). In this scenario, A and B both have the opportunity to participate. Therefore, to the extent they share the same pre-intervention features as measured by the PSM, the positive self-selection of A is counterbalanced by the negative self-selection of B. This reasoning might not apply to municipality C, which is geographically distant from a TP border. Perhaps, C wishes to join the program but - being far from a TP - this opportunity is just not there (alternatively, C did not succeed in convincing its neighbors to set up a TP). Thus, the fact that a TP requires a group membership might imply the correction for self-selection above is not sufficient. In order to cope with this problem, in a robustness check we selected the eligible non treated group only among the neighboring[13] municipalities, for the which it is safe to assume that they had the possibility to join a TP but decided not to do so.

A final concern reflects a feature of the diff-in-diff estimator. As it is well-known (see: Blundell et al., 2004) systematic differences in levels between treated and control groups are not a concern, since they can be controlled using the diff-in-diff methodology. However, violation of the parallel trend assumption may invalidate the estimates. To cope with this issue, we use as control groups municipalities for the which the time trend (1991-1996) of the outcome variables of treated cities is mirrored.

More in detail, we first compute a the average pre-treatment trend for the treated for both dependent variables (g_T). Then, we discard all matches for which pre-treatment trend falls outside the

[12] All the variables are measured at the city-level, except unemployment rate and labor productivity that refer to the local labor market.

[13] We use the following concept of neighborhood: a town B is close to town A if it is one of the 20 closest municipalities of A (Euclidean distance). The reason we used this concept instead of contiguity is twofold. First, a first-order contiguity matrix for municipalities in Italy is not available. Second, TP membership does not require common borders, but only proximity across towns.

interval $[g_T-a\sigma; g_T+a\sigma]$, where σ is the standard deviation of the sample of pre-treatment trend for the treated group and a corresponds to the size of the band. We chose two bands, a narrow for a=0.7 and a large for a=1.3.

4.2 Concurrent programs, intensity of disbursements, and asymmetric locations

Italy's tradition of public aids for deprived areas dates back to 1950. In this long period, a number of policy instruments stratified on the same areas. This fact makes the identification of the effect of each measure particularly difficult. This is true also for the TPs, as in the 1996-2004 period two policy measures – investment grants awarded through the law 488/1992 (law 488) and investment tax credit allowed by law 388/2000 (law 388) – targeted the same areas eligible for TP assistance. Moreover, they shared the same aim of spurring private investments[14]. Our specifications below include a measure of the (per capita) intensity of the concurrent programs at the city level. In particular, for the law 488 (that started in 1996) we use data referring to the amounts of grants disbursed for auctions 1, 2, 3, 4 (for the period 1996-2001) and 8 (for the period 2001-2004); for the law 388, which started in 2001, the tax bonuses refers to the period 2001-2004.[15]

As explained in Sect. 2, TPs were featured by sluggishness of disbursements. To gauge the role of the actual receipt of public money on the effectiveness of the program, three additional robustness checks are performed.

First, the TP sample is split according to the wave of approval. The idea here is that, if the lags in disbursement really matter, the first wave should show a better performance than the second wave, as in 2001 it was featured by an higher rate of disbursements over allocations. Second, the analysis is extended to 2004 by making use of the ASIA-UL archive. Once again, if the actual receipt of money is relevant, the effect of the program over the period 1996-2004 should outperform that of the phase 1996-2001, as cumulative disbursements filled almost entirely the budged. Third, we estimate the effect of the program for two sub-samples of cases of excellence. The first is the so-called "well established" TP (WETP) (DPS, 2003), which is a group of first and second wave TPs selected by the Ministry of Economic Development on the basis of (i) public money received was faster than average; (ii) cooperation among agents was particularly high; (iii) there was a strong integration between private and public investments. The second is made by cities belonging to European TPs. As explained in sect. 2, these TP were on average characterized by a more accurate selection of investments programs and faster disbursements by the European Commission.

[14] As we underlined in section 2, according to some authors (see, e.g., DPS, 2003), law 388 and 488 crowded out the investment grants of the TPs and concurred in creating strong delays in disbursement of public funds due to continuous reallocation of grants by the local authority.

[15] Programs according to the laws 488 and 388 represented most of the public aids for lagging areas in that period. For example, the share of public expenditures for deprived regions of those programs averaged, in the period 2000-2006 around 63 per cent. It is worth noting, anyway, that according to Rossi et al. (2005), spatial overlapping between TP and law 488 does not seem particularly at issue, at least for the South.

4.4 Asymmetric/redistributive effects in treated areas

TP were expected to contribute to rationalize and reorganize the economic landscape in the area. For example, in order to exploit agglomeration externalities or knowledge spillovers effects, some TP provided for the creation of an industrial cluster within the treated area. The internal reorganization of the economic landscape within a TP may thus create some problems in the estimate of the AEE due to the use of PSM in the sample selection. In particular, in the matching procedure, we discard all treated observations for which it was not possible to find a match. It is possible, therefore, that we could discard the very municipality aimed at receiving most of the economic activities of the area. We solve this problem by aggregating the observations by TP. In this robustness check, anyway, we face the problem of finding a control group: the borders of TP hardly ever mirror the borders of the typical Italian geographical units (local labor system, province or region). It is not possible, therefore, to find an area similar to a TP in terms of population or surface. In this paper, we exploited the existence of a last generation of TP, established in 2001 and subsequently activated in the period 2002-2006. These territories have he advantage of being directly comparable with existing TP in terms of observable (population, employment, surface etc.) and unobservable (positive self-selection) characteristics. The limited number of observations does not allow us to split the sample across geographical area or generation, but the lack of the self-selection problem (both groups are characterized by positive self-selection) let us directly assess the effect of treatment by estimating equation (1).

5. Results

Table 1 shows the descriptive statistics and the sample balance properties for the sample of the baseline regressions (Table 2) for the TPs in the North (Panel a) and in the South (Panel b) for period 1996-2001. Columns [1], [2] and [3] report, respectively, the mean and standard deviation for the treated, eligible non treated and non eligible groups for each selection variable. Columns [4] and [5] report the mean differences between treated and eligible non treated (column [4]) and treated and non-eligible group (column [5]). Ideally, a well balanced sample should never show statistically significant mean differences, thus implying that all control groups do not display significant departures from the characteristics of the treated group. As Panel (a) shows, this property is reasonably satisfied for the Northern municipalities. The only statistically significant differences emerge for labor productivity in the comparison between the eligible and the eligible non treated group (column [4]) and for surface and the share of industrial employment in the comparison between the eligible and non eligible group (column [5]). Also sample balance for the South (Panel (b)) is practically fulfilled. Mean differences are statistically significant at 5 per cent, for the surface in the comparison between the treated and the eligible non treated group and for the share of secondary education in the comparison between the

treated and the non eligible.

Table 2 reports the results we obtain from estimating Eq. (2) for the period 1996-2001. Panel (a) display the results for the North, and Panel (b) for the South. For both areas, we show results from 3 different specifications. Column [1] presents the results when no additional control is included. In Column [2] the generosity of concurrent programs at the city level is controlled for (together with its interaction with time). Column [3] adds spatial controls (region fixed and time-varying effects) to control for structural differences and misalignments in the business cycles across areas.

The AEE represents the effect of the exposure to the TP policy on the economic performance of a city. Arguably, it can be interpreted as the effect of the treatment once we control for the self-selection problem. The AEE is computed as the average between TP*POST and ELIG*POST, whose significance is assessed by an F test with null hypothesis AEE=0. Results show that being exposed to TP does not statistically change the economic performance of a municipality. This is confirmed for both the North (Panel (a)) and the South (Panel (b)) and for both the dependent variables, employment and establishments. In particular, the negative sign for the effects on employment and establishment turns insignificant once we introduce controls for "concurrent programs" (column [2]) in the North and NUTS1 effects for the South.

In the following of this section, we will check the robustness of this result by taking into account four possible sources of bias: violation of the parallel trend assumption, the "opportunity effect", the intensity of treatment and reallocation of activities within each TP.

Parallel Trend – As underlined in the previous section, the violation of the "parallel trend" assumption may bias our estimates. In table 3, we show the estimates of the AEE once we take this issue into account. Panel (a) report the estimates of the AEE once we choose a large band (a=1.3) for all three specifications described above, for both the areas of the country and for both the performance variables. Results do not seem to differ from the baseline regression since the AEE turns insignificant once we introduce controls for "concurrent programs" (specification [2]). The same result emerge from Panel (b), in which, by selecting a = 0.7, we choose a more stringent condition on the pre-treatment trend.

Opportunity effect – The correction for self-selection might be insufficient once we do not take into account the "opportunity effect", i.e. the actual possibility for a municipality to join a TP with its neighbors. By selecting the eligible non treated sample among the neighbors of treated municipalities, we should obtain a more reliable correction for the self-selection problem. Table 4 shows the estimates of the "opportunity effect". Also in this case, once we introduce controls for "concurrent programs", the AEE becomes insignificant. Moreover, estimates are quite similar to the ones presented in table 2, thus suggesting that the previous correction for the self-selection is adequate.

Intensity of treatment – So far, the estimated results are quite disappointing. A possible explanation refers to the slowness of disbursements experienced by many TP. To gauge the role of this explanation, we perform three additional checks.

In the first experiment (table 5), we made a sample split between waves. If intensity of disbursement really matters, we should expect that the first wave of TPs registers a better performance than the second one, since, in 2001, the former was active for a longer period than the latter. This hypothesis is not confirmed by results reported in table 5. The AEE for both waves and outcome variables turns insignificant once we controls for concurrent programs. Moreover, the estimated AEE for establishments displays quite always a negative sign.

In the second experiment (table 5), we make use of two groups of TP commonly regarded as excellence cases: WETP, i.e. the best performers among the first and second generation TP (DPS, 2003) and European TP. We should expect a superior performance for these pacts, as they succeeded in better exploiting the public funding for deprived areas and they carried on an apparently more convincing program for local development. This hypothesis is not supported by data. Results for WETP are even more pessimistic since the AEE for both dependent variables are negative but insignificant after the introduction of controls for "concurrent programs". The performance for the European TPs is similar, with the AEE turning insignificant after the introduction of time-varying spatial controls.

In the third experiment, we extend the analysis to 2004 by using the ASIA-UL archive. Table 6 presents the results for the North. AEE-1 and AEE-2 represent, respectively, the effect of exposure for the periods 1996-2001 and 1996-2004. Recall that the AEE-1 is not directly comparable with the results presented in table 2, since the former relates to those municipalities for which we were able to obtain information from the ASIA-UL archive (those larger than 5000 inhabitants). The analysis confirms the lack of positive results for the TP. Moreover, the AEE-2 is lower, although not significant, than the AEE-1 for both dependent variables.

Concentration of activities within treated areas – Results obtained so far support the idea that the institution of TPs did not significantly change the dynamics of employment and number of establishments in the municipalities exposed to the program. It is possible, anyway, that a relevant process of reallocation occurred within the TPs as a result of the policy. For instance, some development plans could have chosen to concentrate plants and infrastructures in portion of the PT territory with the aim to exploit agglomeration economies. It is possible, in this case, that our matching routine discards some of the "receiving municipalities" for which we did not succeed in finding a match. In this case, the diff-in-diff estimates is likely to register the "destruction" of economic activities in some towns but it does not register the "creation" of new activities in the discarded observations. By aggregating observations by TP and estimating the diff-in-diff equation at aggregate level, we should be able to cope with this occurrence. As explained in the previous section, we

compare the evolution of treated areas with those territories which succeeded in becoming a TP in 2001. This solves our self-selection problem and, therefore, we are able to directly assess the treatment effect by estimating equation (1). The differential trend is now picked by the TP*POST coefficient, which can now be interpreted as an ATT. Results presented in table 7 confirm that the establishment of a TP did not significantly change the dynamics of employment in treated areas, while the effect on the number of establishments is even negative and significant for all specifications.

Instrumental variables estimation – We check the robustness of the estimates of the AEE by using an alternative way to treat the self-selection problem. We adopt an Instrumental Variables (IV) approach proposed by Battistin and Rettore (2008), according to which the eligibility status may represent a good instrument for the treated status in a regression discontinuity approach. In other words, whenever we consider all the municipalities on the border between eligible and non eligible areas, we can estimate eq. (1) by instrumenting the treatment dummy (and its interaction with time dummy) with a variable indicating one whenever the city is eligible and zero elsewhere. The advantage of this approach is that we can directly estimate the treatment effect. A drawback lies in the fact that it can only be applied to Northern municipalities, since Southern treated cities do not share any border with non eligible municipalities. We considered two bands around the eligibility border, a narrow one which includes the first 6 most close municipalities across the border and a large one including the first 20 municipalities. Table 8 shows the results for both bands (Panel (a): narrow band; Panel (b): large band) and it displays both OLS and IV coefficients. Regression outcomes do not significantly change the picture shown in the main text: instrumented treatment effect is never significantly different from zero, thus confirming that TPs did not have any significant impact on the dynamics of employments and establishments in treated municipalities.

6. Concluding remarks

The TP program is one of the most important instruments for local development in deprived areas in Italy. Since its introduction in 1996, it represented a great novelty in the landscape of the Italian aid to lagging regions, due to its strong bottom-up approach. The aim of this paper is to assess the effectiveness of this program in the period 1996-2004 by estimating its effects on the dynamics of employment and establishments with a counterfactual analysis.

Our results suggest that economic performance of treated municipalities does not significantly differ from that of our control group. This result is robust to all specifications and tests. Anyway, the econometric analysis is not able to provide us with an justification of *why* the program failed in reaching its targets. In this section, we try to provide three possible explanations for this failure.

The first relates to the fact that the accumulation of social capital can hardly be achieved by public intervention. According to the policy makers' intentions, TP were supposed to encourage the

"ownership" of public aids, since the very design of the policy was supposed to be created by the stakeholders in lagging areas. This intendment contrasts anyway with the view of a number of economists (Drazen and Isard, 2004), according to which it is highly unlikely to observe a strong ownership in lagging areas. If the stock of social capital were already particularly high, we should not expect the treated area to be in need of public aids. The economic environment, in this case, should be already business friendly and ready to start a growth takeoff. If, instead, the stock were low, it is very improbable that the TP would hold enough ownership and, therefore, it would work properly. We should expect, instead, that the treaty is predatory, that it is signed with the goal to receive public funding.

The second relates to the contemporary existence of "concurrent programs" and slowness in the public expenditure. Some authors (Magnatti et al., 2005) argued that the TP management was too slow, bureaucratic and, sometimes, political. These characteristics were particularly unattractive for the most dynamic entrepreneurs, whose main target was to receive public funds as soon as possible. Some of them, therefore, decided to withdraw from the pact with the aim to join other public aids like investment grants from law 488. As a result, we are likely to observe a *negative* self-selection for the entrepreneurs remained in a TP, whose investment plants did not succeed in receiving the law 488 grants due to their scarce quality.

The third possible explanation relates to the total amount of public expenditure for each TP. According to some experts, the amount of public aids for each TP (50 millions) was insufficient to trigger an autonomous growth process. Although our results suggest that the total amount of actual public expenditure does not impact on the economic performance ("Well Established" TP), we cannot exclude, at least in theory, that the total amount is smaller than the one needed to generate growth and to escape from a "poverty trap". According to this interpretation, our estimates tend to suggest that public funding was insufficient to create a critical mass to generate growth in such a deprived areas.

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Figure 1

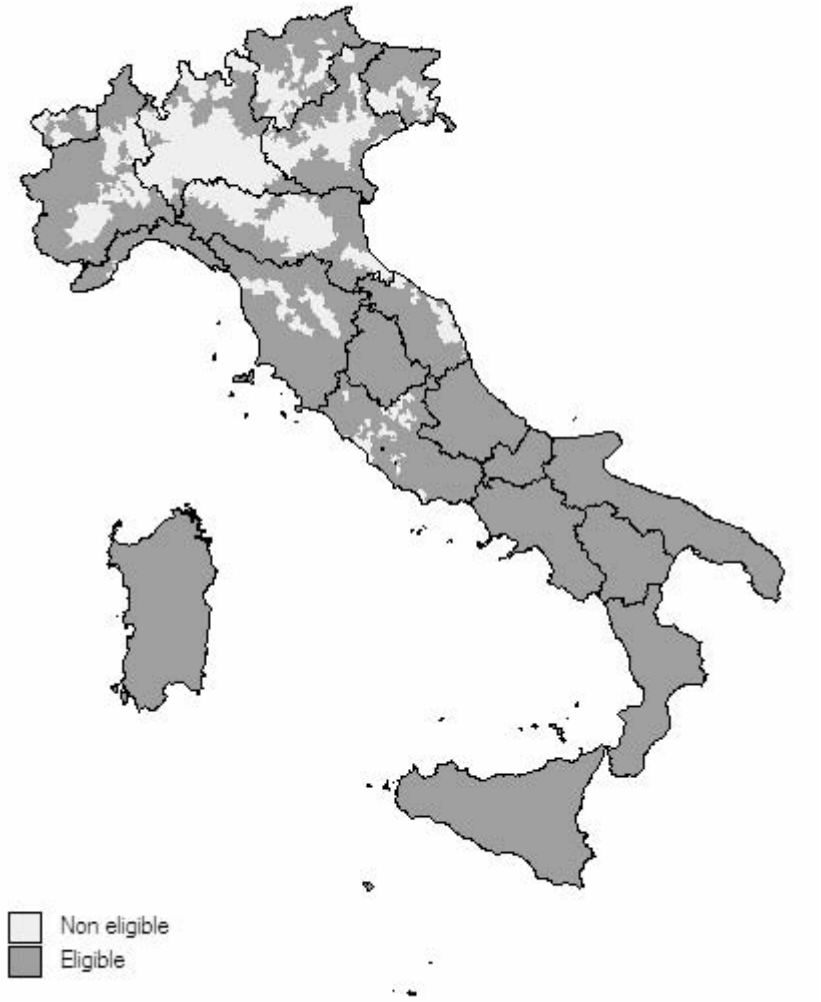

Figure 2

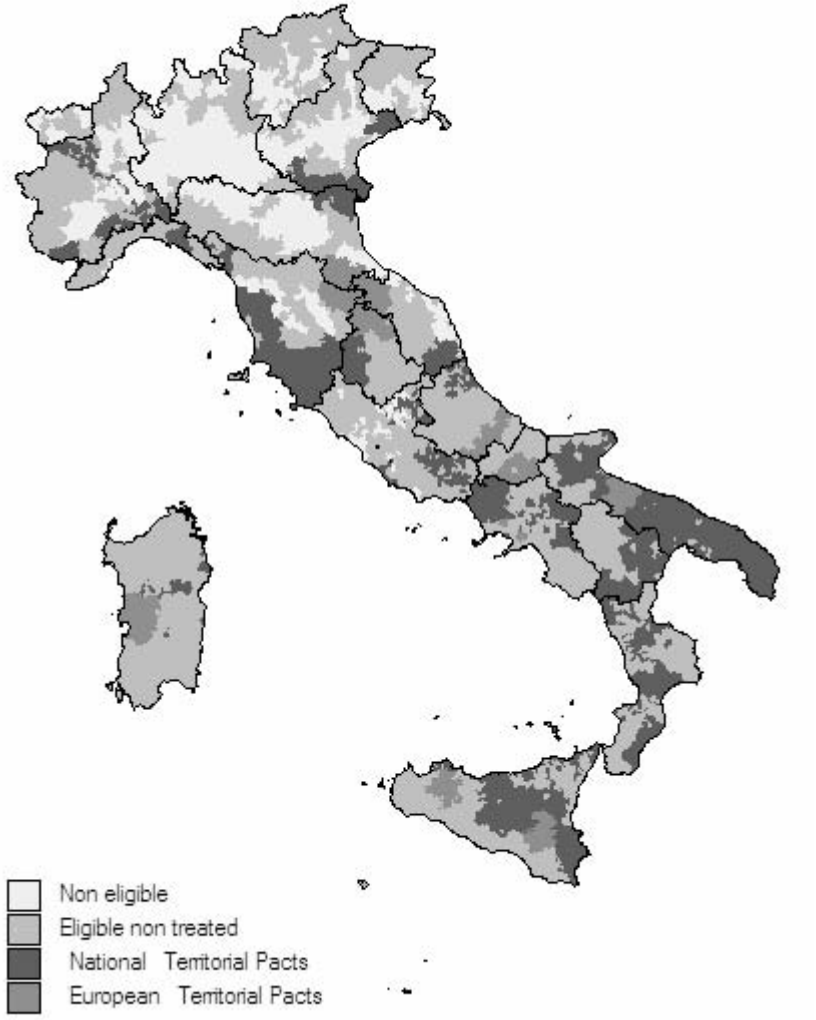
Non eligible
Eligible non treated
National Territorial Pacts
European Territorial Pacts

TABLE 1. BALANCING PROPERTIES FOR THE BASELINE SAMPLE

	Mean and standard deviations			Mean differences	
	[1] TP	[2] ELIG	[3] Non eligible	[4] TP vs. ELIG	[5] TP vs. Non eligible
		(a) North			
Log(employment) – 1996	5.671	5.580	5.729	-0.088	-0.057
	[1.821]	[1.945]	[1.595]	(0.114)	(0.110)
Log(establishments) – 1996	4.693	4.767	4.671	-0.074	0.022
	[1.558]	[1.654]	[1.430]	(0.097)	(0.097)
Log(labor productivity)	1.402	1.417	1.393	-0.015**	0.009
	[0.096]	[0.095]	[0.105]	(0.006)	(0.007)
Log(population)	7.549	7.628	7.538	-0.079	0.011
	[1.377]	[1.450]	[1.256]	(0.085)	(0.085)
Log(surface)	3.178	3.101	2.934	0.077	0.243***
	[1.071]	[0.984]	[0.859]	(0.062)	(0.062)
Activity rate	64.014	64.222	63.990	-0.208	0.024
	[5.061]	[5.421]	[5.083]	(0.318)	(0.333)
Share of industrial empl.	33.157	34.123	39.192	-0.966	-6.035***
	[21.038]	[22.024]	[23.739]	(1.305)	(1.495)
Share secondary education	14.096	14.051	14.376	0.044	-0.280
	[4.388]	[4.624]	[4.143]	(0.273)	(0.277)
Share tertiary education	1.870	1.873	1.961	-0.003	-0.091
	[1.260]	[1.218]	[1.269]	(0.075)	(0.083)
Unemployment rate	0.094	0.094	0.093	-0.000	0.001
	[0.036]	[0.042]	[0.046]	(0.094)	(0.003)
		(b) South			
Log(employment) – 1996	6.194	6.170	6.490	0.023	-0.296
	[1.390]	[1.600]	[1.887]	(0.168)	(0.237)
Log(establishments) – 1996	5.363	5.398	5.563	-0.035	-0.199
	[1.216]	[1.333]	[1.681]	(0.143)	(0.211)
Log(labor productivity)	1.241	1.223	1.233	0.018	0.008
	[0.159]	[0.198]	[0.105]	(0.020)	(0.017)
Log(population)	8.395	8.390	8.539	0.005	-0.144
	[1.390]	[1.137]	[1.530]	(0.125)	(0.191)
Log(surface)	3.271	3.579	3.231	-0.308**	0.039
	[0.969]	[1.057]	[0.753]	(0.114)	(0.115)
Activity rate	66.044	65.425	67.026	0.619	-0.981*
	[3.397]	[3.903]	[4.403]	(0.410)	(0.560)
Share of industrial empl.	26.850	23.311	23.716	3.539*	3.134
	[18.712]	[15.802]	[15.621]	(1.948)	(2.305)
Share secondary education	14.040	14.268	15.457	-0.228	-1.412**
	[4.290]	[4.854]	[3.914]	(0.513)	(0.557)
Share tertiary education	2.207	2.228	1.896	-0.022	0.310
	[1.469]	[1.386]	[1.707]	(0.160)	(0.223)
Unemployment rate	0.187	0.179	0.202	0.008	-0.015*
	[0.047]	[0.063]	[0.066]	(0.006)	(0.008)

Notes. The sample includes 606 TP-, 483 eligible non TP-, 407 non eligible- cities for the North and 171 TP-, 147 eligible non TP-, 61 non eligible-cities for the South. Mean differences weighted according to the number of matches carried by each observation.

TABLE 2. BASELINE REGRESSION: 1996-2001

	Dependent variable	[1]	[2]	[3]
		(a) North		
TP*POST	Employment	-0.037***	-0.042	-0.046
		(0.000)	(0.081)	(0.081)
	Establishments	-0.035***	-0.036	-0.029
		(0.000)	(0.050)	(0.069)
ELIG*POST	Employment	-0.049***	-0.052	-0.057
		(0.000)	(0.074)	(0.074)
	Establishments	-0.027***	-0.027	-0.022
		(0.000)	(0.019)	(0.062)
AEE – North	Employment	-0.043***	-0.047	-0.052
		[0.000]	[0.378]	[0.536]
	Establishments	-0.031***	-0.032	-0.026
		[0.000]	[0.460]	[0.713]
No. Obs		*2992*	*2992*	*2992*
		(b) South		
TP*POST	Employment	0.035***	0.022**	0.011
		(0.000)	(0.007)	(0.214)
	Establishments	-0.094***	-0.095**	-0.039
		(0.000)	(0.014)	(0.202)
ELIG*POST	Employment	-0.016***	-0.021***	-0.032
		(0.000)	(0.003)	(0.236)
	Establishments	-0.125***	-0.125***	-0.084
		(0.000)	(0.009)	(0.224)
AEE – South	Employment	-0.010***	0.001	-0.011
		[0.000]	[0.906]	[0.965]
	Establishments	-0.110***	-0.110***	-0.062
		[0.000]	[0.001]	[0.781]
No. Obs.		*1926*	*1926*	*1926*
Concurrent programs		NO	YES	YES
NUTS1 effects		NO	NO	YES

Notes. Weighted least squares estimates. Weighting is designed according to the number of matches for each observation. All specifications include a dummy for TP, a dummy for ELEG, a dummy for POST. Robust standard errors clustered on TP (eligible non-TP, non eligible) status-post interactions are in parenthesis below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample includes 606 TP-, 483 eligible non TP-, 407 non eligible- cities for the North and 171 TP-, 147 eligible non TP-, 61 non eligible-cities for the South. AEE is equal to the average between TP*POST e ELIG*POST. P-value in brackets is obtained by an F-test, with null hypothesis AEE=0.

TABLE 3. CITIES WITH THE SAME PATTERN OF (PRE-INTERVENTION) OUTCOME GROWTH RATES

	Dependent variable	[1]	[2]	[3]
	(a) Narrow band (a=0.7)			
AEE – North	Employment	-0.001***	-0.002	-0.005
		[0.000]	[0.916]	[0.985]
	Establishments	-0.025***	-0.026	-0.023
		[0.000]	[0.125]	[0.275]
AEE – South	Employment	0.042***	0.034	0.011
		[0.000]	[0.485]	[0.916]
	Establishments	-0.062***	-0.064*	-0.039
		[0.000]	[0.056]	[0.572]
	(b) Large band (a=1.3)			
AEE – North	Employment	-0.017***	-0.020	-0.022
		[0.000]	[0.612]	[0.776]
	Establishments	-0.028***	-0.028	-0.023
		[0.000]	[0.417]	[0.728]
AEE – South	Employment	0.002***	-0.014	-0.023
		[0.000]	[0.509]	[0.898]
	Establishments	-0,129***	-0,133***	-0,087
		[0.000]	[0.000]	[0.602]
Concurrent programs		NO	YES	YES
NUTS1 effects		NO	NO	YES

Notes. Weighted least squares estimates. Weighting is designed according to the number of matches for each observation. All specifications include a dummy for TP, a dummy for ELEG, a dummy for POST. Robust standard errors clustered on TP (eligible non-TP, non eligible) status-post interactions are in parenthesis below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. AEE is equal to the average between TP*POST e ELIG*POST. Significance level obtained by an F-test, with null hypothesis AEE=0

TABLE 4. ELIGIBLE NON TP-CITIES WITH THE ACTUAL OPPORTUNITY TO JOIN A PT

	Dependent variable	[1]	[2]	[3]
AEE – North	Employment	-0.054***	-0.057	-0.062
		[0.000]	[0.110]	[0.441]
	Establishments	-0.028***	-0.029	-0.027
		[0.000]	[0.209]	[0.672]
No. Obs.		1874	1874	1874
AEE – South	Employment	0.053***	0.048	-0.007
		[0.000]	[0.308]	[0.892]
	Establishments	-0.039***	-0.043	-0.026
		[0.000]	[0.236]	[0.733]
No. Obs.		564	564	564
Concurrent programs		NO	YES	YES
NUTS1 effects		NO	NO	YES

Notes. Weighted least squares estimates. Weighting is designed according to the number of matches for each observation. All specifications include a dummy for TP, a dummy for ELEG, a dummy for POST. Robust standard errors clustered on TP (eligible non-TP, non eligible) status-post interactions are in parenthesis below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample includes: 352 TPs, 293 eligible non-TPs, and 292 non eligible cities for the North and 124 TPs, 102 eligible non-TPs, and 56 non eligible cities for the South. See Eq. (2) for further details. AEE is equal to the average between TP*POST e ELIG*POST. Significance level obtained by an F-test, with null hypothesis AEE=0.

TABLE 5. SAMPLE SPLITS

	Dependent variable	[1]	[2]	[3]
AEE – First wave	Employment	-0.017***	-0.019*	-0.032
		[0.000]	[0.056]	[0.864]
	Establishments	-0.112***	-0.115***	-0.078
		[0.000]	[0.000]	[0.713]
No. Obs.		726	726	726
AEE – Second wave	Employment	0.030***	0.015	0.001
		[0.000]	[0.393]	[0.994]
	Establishments	-0.021***	-0.021	-0.018
		[0.000]	[0.272]	[0.848]
No. Obs.		3852	3852	3852
AEE – Well established	Employment	-0.009***	-0.023	-0.047
		[0.000]	[0.428]	[0.840]
	Establishments	-0.031***	-0.034*	-0.039
		[0.000]	[0.056]	[0.838]
No. Obs.		1980	1980	1980
AEE – European TPs	Employment	0.045***	0.027*	0.012
		[0.000]	[0.062]	[0.962]
	Establishments	-0.005***	-0.007	-0.015
		[0.000]	[0.470]	[0.936]
No. Obs.		1190	1190	1190
Concurrent programs		NO	YES	YES
NUTS1 effects		NO	NO	YES

Notes. Weighted least squares estimates. Weighting is designed according to the number of matches for each observation. All specifications include a dummy for TP, a dummy for ELEG, a dummy for POST. Robust standard errors clustered on TP (eligible non-TP, non eligible) status-post interactions are in parenthesis below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample includes for the first wave: 200 TPs, 121 eligible non-TPs, and 42 non eligible cities, for the second wave 848 TPs, 653 eligible non-TPs, and 425 non eligible cities, for the "well established TP" 416 TPs, 327 eligible non-TPs, and 247 non eligible cities, for the European TP 255 TPs, 218 eligible non-TPs, and 122 non eligible cities See Eq. (2) for further details. AEE is equal to the average between TP*POST e ELIG*POST. Significance level obtained by an F-test, with null hypothesis AEE=0.

TABLE 6. AEE ESTIMATED IN 2004

	Dependent variable	[1]	[2]	[3]
AEE – 2001	Employment	0.018***	0.013	-0.013
		[0.000]	[0.707]	[0.933]
	Establishments	-0.043***	-0.042**	-0.037
		[0.000]	[0.038]	0.768]
AEE – 2004	Employment	-0.002***	-0.011	-0.046
		[0.000]	[0.795]	[0.766]
	Establishments	-0.043	-0.046*	-0.048
		[0.000]	[0.057]	[0.694]
No. Obs.		1272	1272	1272
Concurrent programs		NO	YES	YES
NUTS1 effects		NO	NO	YES

Notes. Weighted least squares estimates. Weighting is designed according to the number of matches for each observation. All specifications include a dummy for TP, a dummy for ELEG, a dummy for POST. Robust standard errors clustered on TP (eligible non-TP, non eligible) status-post interactions are in parenthesis below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample includes: 346 TPs, 189 eligible non-TPs, and 101 non eligible cities. See Eq. (2) for further details. AEE is equal to the average between TP*POST e ELIG*POST. Significance level obtained by an F-test, with null hypothesis AEE=0.

TABLE 7. TP-WIDE ESTIMATES

	Dependent variable	[1]	[2]	[3]
TP*POST	Employment	0.003***	0.003**	0.004
		(0.000)	(0.001)	(0.006)
	Establishments	-0.008***	-0.007***	-0.011***
		(0.000)	(0.001)	(0.001)
No. Obs.		104	104	104
Concurrent programs		NO	YES	YES
NUTS1 effects		NO	NO	YES

Notes. Weighted least squares estimates. Weighting is designed according to the number of matches for each observation. All specifications include a dummy for TP and a dummy for POST. Robust standard errors clustered on TP status. Standard errors are in parenthesis below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample includes: 38 TPs and 14 non treated territories. See Eq. (1) for further details.

TABLE 8. INSTRUMENTAL VARIABLES ESTIMATIONS

	Dependent variable	[1]	[2]	[3]
	(a) Narrow band			
TP*POST (OLS)	Employment	-0.007***	-0.015	-0.019
		(0.000)	(0.012)	(0.028)
	Establishments	-0.021***	-0.019	-0.012
		(0.000)	(0.009)	(0.009)
TP*POST (IV)	Employment	-0.130	-0.213	-0.219
		(12.258)	(19.230)	(19.862)
	Establishments	-0.065	-0.071	-0.076
		(10.217)	(15.263)	(15.746)
No. Obs.		3354	3354	3354
	(a) Large band			
TP*POST (OLS)	Employment	0.001	-0.007	-0.012
		(0.000)	(0.059)	(0.043)
	Establishments	-0.023***	-0.023	-0.013
		(0.000)	(0.038)	(0.027)
TP*POST (IV)	Employment	-0.050	-0.098	-0.122
		(11.916)	(15.920)	(17.327)
	Establishments	-0.090	-0.0104	-0.098
		(9.404)	(12.292)	(13.652)
No. Obs.		6387	6387	6387
Concurrent programs		NO	YES	YES
NUTS1 effects		NO	NO	YES

Notes. Instrumental variables estimations. TP and TP*POST are instrumented by (respectively) ELIG and ELIG*POST. Standard errors are in parenthesis below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample includes: 98 TPs, 839 eligible non-TPs, and 842 non eligible cities for the narrow band; 236 TPs, 701eligible non-TPs, and 1396 non eligible cities for the large band. Narrow band includes the first 6 most close municipalities across the eligibility border; large band includes the first 20 most close municipalities across the eligibility border.