Something New in the City? The Local Effects of Urban Regeneration Policies in Italy*

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Abstract

The paper estimates the local effects of urban regeneration policies by using evidence from interventions that took place in small and medium-sized Italy's Centre-North cities, over the period 2007-15. By using an Oaxaca-Blinder reweighting estimator, we find little support for the idea that urban regeneration could stimulate local economic growth.

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1. Introduction

Regeneration programs aim to improve social, economic and physical conditions for a given location. These interventions seek to improve the quality of housing supply, the built environment and other local amenities, also through investment in transport or other infrastructure. They might involve skills training and active labor market initiatives, as well as tax breaks and other fiscal measures. While these interventions have boomed in the last two decades, their economic rationale remains highly disputed. For instance, the impact of these interventions might be limited to housing and neighborhood quality, without improving local economic conditions. In this case, however, it is hard to justify why other communities should contribute to financing urban regeneration of other places (especially communities that are poorer than the ones subsidized). Differently, to the extent that these programs are capable of stimulating economic growth, therefore providing a wider payoff, spending money on particular areas might be less controversial.

On the empirical side, the evidence on the impact of URPs is limited to few papers. As summarized by the report of the *What Works Center for Local Economic Growth* (see: WWG, 2015), which examined 21 experimental or quasi-experimental studies conducted on URPs carried out in US, UK, Germany and Australia, these programs have likely led to an increase in property values, land prices and rents, while they have had a limited impact on the local economy (in terms of income and employment) and on some other socio-economic indicators (crime, social exclusion). A second report (WWG, 2014) examined a smaller (11) number of studies that analyzed interventions on public spaces: streets, squares, parks and other amenities ("public realm"). This evidence suggests that only big scale projects had success in attracting new residents and improving the working conditions of the area. At the same time, this type of intervention can have displacement effects on existing families and businesses, as the improvement of public facilities and services translates into an increase in property prices and there is essentially no evidence of positive effects on low-income residents in the area. More recently, González-Pampillón et al. (2017) studied the impact of a set of place-based policies, implemented in Catalonia between 2004 and 2010, that invested in public facilities in deprived neighborhoods with the aim of attracting higher-income individuals and reducing segregation. They compared treated areas with others that applied for the policy but were rejected (or never financed), by means of an Oaxaca-Blinder matching

estimator. They did not find any impact on population composition, with the exception of Barcelona historical districts.

This paper contributes to the provision of rigorous evidence on the local impact of urban regeneration programs (URPs). It exploits the newest wave of URPs, approved for the municipalities of Italy's during the years between 2008 to 2012. We pay particular attention to identification issues. Our treated group includes only small cities – to minimize attenuation concerns – belonging to the Centre-North of the country – to lessen the issues of concurrent programs. Assuming *selection on observables* and using the Oaxaca-Blinder reweighting estimator (Kline, 2011) we find very little support for the claim that URPs spur local development, as picked by a large number of proxies of economic activity such as employment, plants, income and population. House prices seem to be the only variable on which the programs have an impact, but increases in housing values materialize only in the medium term or for programs featured by the highest funding. These results do not seem to hinge on the selection-on-observables assumption. They nicely survive when we use as control group the future-treated municipalities, which are likely to share with the treatment ones both observable and unobservables characteristics (Andini and de Blasio, 2014; Kline and Moretti, 2014).

The paper is structured as follows. The potential local effects of the URPs are described in Section 2. The data and the identification strategy are the focus of Section 3. The results are described in Section 4. The last section concludes.

2. The Effects of Urban Regeneration Programmes

According to a traditional definition, a URP aims at the physical renewal of a city or neighborhood, by improving the quality of housing (mostly without creating new volumes), and increasing the provision of services and public spaces. However, starting from the 1990s (first in the US and UK, then in Italy and the rest of Europe), a new generation of URPs have been introduced. They integrate physical projects with interventions aimed at favoring local development, with particular attention to the fight against poverty and social exclusion.

Urban and regional economics (Glaeser, 2008) suggests that the increase in the value of local amenities associated with URPs should lead ceteris paribus to an increase in the supply of work and the demand for housing. The effects on wages would be more ambiguous: wages might diminish, due to the "compensating wage differentials" channel, but the higher

cost of housing could more than offset this effect. Similarly, the area targeted by a URP might or might not attract new business, according to the bundle of local prices that will prevail in equilibrium. For instance, if local housing capitalizes the benefits of the intervention, the higher cost of producing in the area might discourage firms, especially those in the tradable sector. However, non-tradable activities might gain from a URP, as the regeneration often improves housing and neighborhood qualities, making therefore the area more suitable for consumption and leisure activities. Finally, the impact of the new generation of URPs is even more difficult to foresee, as the increase in the value of local amenities, which might discourage – under some circumstances – local economic activity, goes hand in hands with a direct stimulus, such as tax breaks or a subsidized labor input.

Potentially relevant are also the distributive effects. For instance, to the extent that the intervention results in higher house prices a relevant distinction is between those who already owned a house, whose value is raised, and those who must bear a higher cost of housing services. Moreover, the improvement of facilities and amenities could promote participation in civic networks and the adoption of new behaviors (also through peer effects). For instance, less degraded areas could discourage crime behavior, through the so-called "broken windows" effect (Harcourt and Ludwig, 2006). Again, URPs could reduce discrimination, for instance in presence of a stigmatization of the inhabitants of a certain area, on the basis of stereotypes that can reduce their opportunities, either directly (job offer) or indirectly (self-esteem).

All in all, the effects of URPs are potentially very wide, and some channels might even offset each other’s. However, on the empirical side, there is scant evidence on this subject, even if the existing literature is generally unsupportive of a significant impact of regeneration programmes. This is particularly true for income and wages, employment, and poverty (see for a survey WWG, 2014, 2015). Nevertheless, for the US case, Collins et al. (2013) provide some evidence of a positive result on income and wages. Again, there are a few previous studies that find an increase in property and land prices and rents (Schwartz et al., 2006; Rossi-Hansberg et al., 2010; Ahlfeldt et al., 2017). The remaining of the paper will investigate the effects of URPs in Italy on a large number of proxies of economic activity and welfare, such as employment, plants, income, house prices and population. In line with González-Pampillón et al. (2017), we will also study the impact on population composition, as reflected in the Gini index and the share of immigrants from abroad.

3. Empirical framework

3.1 The dataset

Starting from mid-2000s, urban regeneration in Italy was mainly undertaken by regional authorities. However, due to the difficulties of local finance during the crisis, the interventions were mainly funded by the European structural funds. Using the information available on the programming period 2007-13, it is possible to identify 9 regions (out of 21) that have activated URPs (Tab. 1). The number of projects funded equals 125, although this number varies significantly between regions. Overall, the financial dimension of the intervention was about 2 billion euros, corresponding to an average size of 14 million euros. In general, these projects provided public works for urban amenities and economic incentives for housing and business.

[Table 1]

The scope of this paper is to provide rigorous evidence on the local impact of this intervention. To this aim, we restrict the sample in order to gauge credible identification (Table 2). First, we include only municipalities located in the Centre-North of the country. We do this to minimize the issues of concurrent programs, since Southern regions were the target of most of the European transfers. In the programming period 2007-13, more than 80% of the total financing at the national level was allocated to this area (Ciani and de Blasio, 2015). Accordingly, it could be difficult to distinguish the impact of URPs from several other projects that could regard the same areas. Furthermore, the choice of excluding Southern Italy aims at reducing the degree of heterogeneity. Regions located in the South showed quite different trends in employment, population and house prices during the period of interest, as they were more strongly hit by the recession. For comparability reasons we have also excluded regions with a special statute, which enjoy of a different and major responsibility in urban policy (in particular, environment, local transportation, housing). Second, we trim the data as follows. On the one hand, we exclude bigger cities to minimize attenuation concerns, given that available information doesn't allow us to geo-reference projects at the neighborhood level. In particular, we exclude municipalities with more than 50,000 inhabitants since the effect of URPs at the city level could be statistically indistinguishable in this case. On the other hand, we delete all municipalities with less than 10,000 inhabitants to ensure common support, as no projects have been activated below this size. Finally, we exclude two projects that split interventions in more than one municipality.

[Table 2]

After this selection, our treated group includes 24 municipalities in 5 regions (Lazio, Liguria, Piemonte, Toscana and Umbria), implemented in the years from 2008 to 2012. To identify the effect of URPs on target cities, their performance is compared with those of a control group made up of 608 municipalities in 9 regions in the Centre-North of Italy (the same as before, plus Emilia Romagna, Lombardia, Marche and Veneto). Figure 1 plots the treated and the standard controls over the map. Appendix 1 illustrates for each of the 24 treated municipalities the specific characteristic of the programs. Public realms and other local amenities are a common theme. However, many interventions also have the wider scope of promoting local growth and envisage skills training and active labor market initiatives. Only few projects provide a direct intervention on the quality of housing.

[Fig. 1]

Given the high potential number of channels through which the stimulus percolates (see Section 2), we evaluate the effectiveness of URPs by considering a large number of socio-economic outcomes at city level: population, percentage of foreign people, house prices, income, inequality, number of plants and employment. Demographic data are obtained from Census and from the Intercensus demographic balance reconstruction carried out by the Italian National Statistical Institute (Istat). Plants and employment figures come from Census data and ASIA-UL database. House prices come from the *Osservatorio Immobiliare*; given that they are released every semester, we took a simple average over the whole year.[2] Finally, income data come from the Italian tax office *Agenzia delle Entrate*. Summary statistics on outcomes are reported in Table 3.

[Table 3]

There are of course many other possible sources of selection bias that might plague an evaluation of the URPs. As discussed next, one way to tackle this issue is by re-weighting the control group in order to make it comparable in terms of pre-determined characteristics, using an Oaxaca-Blinder matching estimator (Kline, 2011). To do this, we complement our data with a set of 29 geographical, demographic, and socio-economic variables from Census and

[2] Data at the municipality level are an average across different neighborhoods and house characteristics. See Appendix A in Auricchio et al (2017) for more details.

Istat. Furthermore, time-variant characteristics are measured in 2001 and in 1991, in order to control for differences not only in levels but also in trends. As shown in Table 3, for many of them, there is a significant difference between treated and controls. The fraction of provincial capitals is higher among the treated, hence it is not surprising that they are also more populated. The municipalities with an URP display also different orographic characteristics, which play an important role in the level and dynamics of the housing market. Daily mobility outside the municipality is also lower. The main demographic difference is in the fraction of elderly and in the fraction with at least a high school diploma, which are lower in the control group. The treated municipalities have a worse performance in terms of labor market, with a lower participation rate and higher unemployment. Employment tends to be less concentrated in the industrial sector and more in non-trade services and construction.

[Table 4]

3.2 The identification strategy

Our main aim is to recover the causal effect of URPs on a set of outcome. Let y_{0i} be the outcome without the URP and y_{1i} with it. Given that the policy takes place at some point in time, we exploit a difference-in-differences strategy by assuming that:

$$E(y_{0i}|g,t) = \lambda_g + \gamma_t \qquad (1)$$

where $g \in \{treated, control\}$ identifies the treatment group. The policy takes place in different points in time, but for all treated municipalities the projects started in the years from 2008 to 2012. We therefore take 2007 as the first year, as it is also the first year of the Structural Funds budget cycle, and the last available year (2015) as the post period. We compare only the first and last year, because this kind of projects are likely to have an impact in the medium rather than in the very short term. Hence $t \in \{2007,2015\}$.

Equation 1 embeds the crucial assumption that the two groups have parallel trends. We also assume that the treatment effect is additive (but not necessarily constant across individuals), so that on average:

$$E(y_{1i}|g = treated, t) = E(y_{1i} - y_{0i}|g = treated) + E(y_{0i}|g = treated, t) = E(y_{1i} - y_{0i}|g = treated) + \lambda_{treated} + \gamma_t \qquad (2)$$

Given that in the post period (2015) we observe $y_i = y_{1i}$ for the treated and $y_i = y_{0i}$ for the control group, while we observe y_{0i} for both groups before the policy (2007), we

know that by looking at changes between the two periods we identify the Average Treatment effect on the Treated (ATT):

$$\begin{aligned} &E(\Delta y_i | g = treated) - E(\Delta y_i | g = control) \\ &\quad = E(y_{1i} - y_{0i} | g = treated) + (\gamma_{2015} - \gamma_{2007}) - (\gamma_{2015} - \gamma_{2007}) \\ &\quad = E(y_{1i} - y_{0i} | g = treated) \end{aligned} \qquad (3)$$

The validity of the result hinges on the credibility of the parallel trends assumption.

One way to relax this assumption is to assume that the time trend depends on a set of fixed or pre-determined characteristics:

$$E(y_{0i} | g, t, X_i) = \lambda_g + \gamma_t(X_i) \qquad (4)$$

so that the parallel trends assumption only holds conditional on X_i. Similarly, we can assume something similar to (2) but conditional on X_i. Hence, comparing the change in the outcome between the treated and control group for municipalities with similar characteristics we identify:

$$\begin{aligned} &E(\Delta y_i | g = treated, X_i) - E(\Delta y_i | g = control, X_i) \\ &\quad = E(y_{1i} - y_{0i} | g = treated, X_i) + (\gamma_{post}(X_i) - \gamma_{pre}(X_i)) - (\gamma_{post}(X_i) - \gamma_{pre}(X_i)) \\ &\quad = E(y_{1i} - y_{0i} | g = treated, X_i) \end{aligned} \qquad (5)$$

Given that $\lambda_{treated}$ is anyway removed by first differencing, what is crucial is that we allow the time trend to depend on X_i. In the paper we will use fixed characteristics and pre-determined covariates defined in 2001 and 1991. The ATT is identified by averaging the estimated $E(y_{1i} - y_{0i} | g = treated, X_i)$ in the treated group (that is across the distribution of X_i in the treated group).

To choose the control group that minimizes observable differences, we use a matching estimator. The main problem with this approach is that we have a reasonably large number of predetermined covariates available, but few treated units. In this context, estimating a propensity score is unfeasible, because the number of treated units (24) is actually smaller of the number of covariates (52). Even limiting the number of covariates we would still be very likely to perfectly predict the treatment due to the limit sample size of the treated group. However, Kline's (2011) Oaxaca-Blinder (OB) reweighting estimator can overcome this issue. It assumes that the counterfactual is linear in the controls. As we work in first

differences and the crucial issue pertains to Δy_{0i}, we assume that the change in the counterfactual (that is the parallel trend) is linear:

$$E(\Delta y_{0i}|g = treated, X_i) = \gamma_{post}(X_i) - \gamma_{pre}(X_i) = X_i'\beta^0 \quad (6)$$

Given that this counterfactual can be identified by a regression of Δy_i among the controls (where $\Delta y_i = \Delta y_{0i}$), we only need the covariates not to be collinear for this regression. Kline (2011) proves that this approach is equivalent to a reweighting estimator in which the odds of treatment are linear in the covariates. As in any matching estimator, we also need to assume a common support assumption:

$$P(g = treated|X_i) < 1 \quad (7)$$

Interestingly, if the common support assumption holds, even if the odds are not really linear (and equation 5 is only an approximation), then the OB estimator would still use the best linear approximation to the true non-parametric weights. In our context, very few units are available and therefore we cannot explicitly check this assumption: as discussed above, estimating the propensity score is practically unfeasible. We nevertheless restricted the sample to the Centre-North and to a range of medium sized municipalities in order to limit the differences. Inspecting Table 4, other differences in X_i do not seem so large as to violate assumption (6). We also provide, in the robustness checks, results from an alternative strategy in which we compare our treated cities only with those that will implement similar URPs during the current programming period 2014-20. If the selection into treatment depends on unobservable characteristics, this comparison should also account for this source of heterogeneity. When we look at our period of interest (2007-13), this control group is not yet treated, but is more likely to share both observables and unobservables drivers of Δy_0, given that it will later adopt URPs. See Andini and de Blasio (2014) and Kline and Moretti (2014) for a similar strategy.

To provide evidence that, conditional on X_i, the parallel trends assumption is likely to hold, we estimate a placebo OB on the previous 2001-2007 period. Given that we do not use pre-trends nor lagged outcomes as covariates, any significant differences would be informative about a violation of assumption (4).

An additional concern could be the presence of localized spill-overs. For instance, if the amenities bring about an increased population density, the migration might be mostly from surrounding municipalities. In this case, we would overestimate the effect, because we would the downward trend in the control group is also caused by the URPs. It is also possible

to outline examples in which the opposite bias would affect the estimators. We therefore resort to a simple strategy of excluding surrounding municipalities from the control group.

4. Results

4.1 Placebo and pre-trends

We start in Table 5 from placebo regressions on the period 2001-2007, to understand whether there are significant signs of violation of the parallel trends assumption. We first simply compare the change in the outcomes between the treated and control groups, that is $E(\Delta y_i|g = treated) - E(\Delta y_i|g = control)$ (Panel A). The estimates display several diverging trends, that are significant not only from the statistical point of view, but also in terms of magnitude. However, when we compare the treated group to the OB reweighted control group (Panel B), we do not find any evidence of diverging trends. As we do not use pre-trends or initial levels as covariates, this result is not simply due to the mechanics of the regression, and therefore it lends support to the parallel trends assumption conditional on observable (pre-determined) X_i.

In all cases we use standard errors clustered at the Local Labor Market level, which is defined by the Italian Statistical Institute as an approximately self-contained area in terms of commuting (on the basis of census data). We do this to account for random shocks correlated over space. We also use Conley's spatial HAC standard errors, estimated as in Kline and Moretti (2014) assuming a maximum radius of 200 miles. Using robust standard errors would lead to similar results.

[Table 5]

4.2 Main results

Table 6 shows the main results. Without covariates we detect a positive effect on the share of foreigners and an increase in inequality (Gini index). However, both effects disappear (both in terms of statistical significance and size) when we use an OB estimator (Panel B). Essentially, the URPs do not seem to have had any effect on population, house prices and economic conditions.

[Table 6]

4.3 Spill-overs and future treated

Our estimates might be biased if the control group is significantly affected by localized spill-overs. In Table 7, Panel A, we run the OB regressions by excluding surrounding municipalities. The results are reassuring, as we still fail to detect any effect.

[Table 7]

An alternative strategy could be to compare the municipalities that were treated in 2007-15 with those that are currently planning to carry out URPs in the following programming period (2014-20). In such a group, the parallel trends assumption is more likely to hold, given that there are similar long-term conditions that led the local administrations to implement URPs. Importantly, if selection into treatment also depends on unobservable drivers of Δy_{0i}, the fact that these municipalities will later adopt similar programs is a signal that they are closer to our treated group in terms of characteristics that we cannot observe.

Applying the same selection criteria that led to our sample, we recover a group of 9 control municipalities that will carry out URPs in the current cycle. Table 7, Panel B, shows the results. We detect only a marginally significant positive effect for population. Even neglecting statistical significance, which is hard to assess in such a small sample, it is important to notice that all differences between the treated and control group are quite small.

4.4 Timing and intensity

Our estimates pooled the different project independently from timing and size. Nevertheless, it is obviously easier to detect some effect after some times from the completion of the project. The size of the intervention might also play a role. In Panels A and B of Table 8 we look only at project that, respectively, either started before 2011 or have larger size. Interestingly, in these cases we detect a positive effect on house prices. The effect might seem quite large, but in interpreting it we should remember that house prices also discount future evaluations, as they incorporate expectations.

[Table 8]

5. Conclusions

We provide an evaluation of recent URPs carried out in Italian municipalities. Using an OB reweighting estimator we compare the 2007-15 growth in population, house prices, income and other relevant outcomes between the group of treated municipalities and a group

of similar (re-weighted) controls. The validity of the parallel trends assumption conditional on observables, that we maintain throughout the exercise, is corroborated by a placebo exercise on 2001-07. Very similar indications are found by using as control group a set of municipalities that is going to benefit from the same type of policies during the current EU programming period. This suggests that the degree to which our estimates are biased by unobservables might be very limited.

On average, we do not detect any effect of URPs. We are able to detect a positive impact on house prices only for programs that started earlier or had larger size. Looking at this result trough the lenses of an urban economics model (Glaeser, 2008), real income in the treated cities decreased. This implies that the URPs improved amenities, which are positively valued by residents. Income as well as employment did not change, hence it seems that these interventions did not increase local productivity. A standard model would predict that, with better amenities, population density should increase as well, but we are unable to detect any change in population. It is possible that, in the time-horizon considered, housing supply is extremely rigid, so the equilibrium over space is guaranteed by movements in house prices, which are sufficient to keep population stable. The effect on population might appear in the long term. Of course population stability might come together with a change in its composition, but there does not seem to be a change in the fraction of foreigners and in average income, nor in the Gini inequality index.

Who capitalizes these housing price benefits? As 72 percent of the population owns their house in these municipalities, according to the 2011 census, these URPs seem to have brought benefits mostly to local residents.

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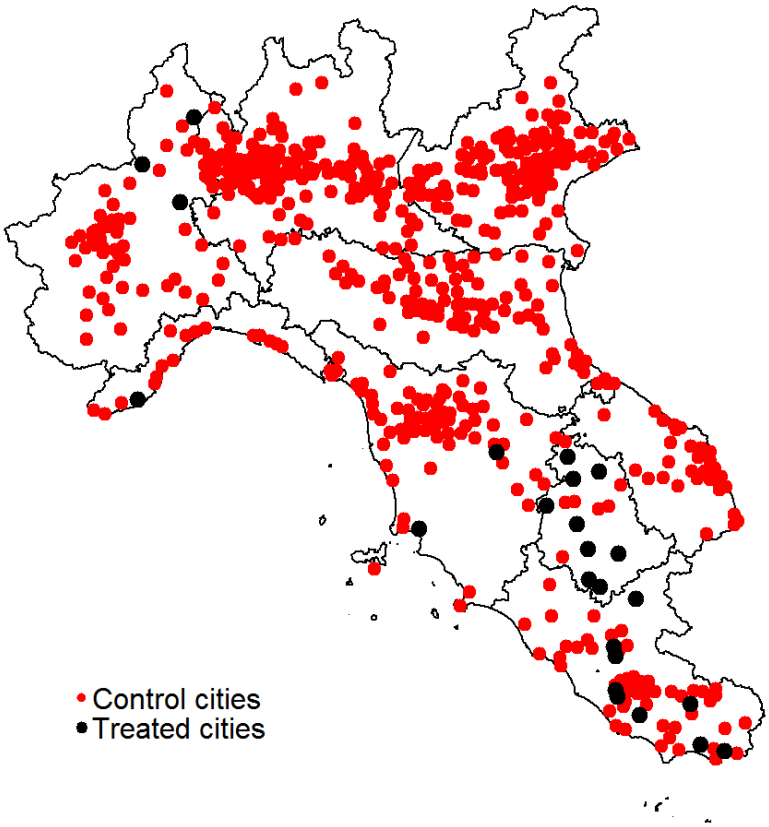
Figures

Figure 1: Cities in the sample



Notes. Treated cities are: Albano Laziale, Amelia, Biella, Castiglione del Lago, Cisterna di Latina, Città di Castello, Follonica, Fondi, Fonte Nuova, Formia, Frosinone, Gubbio, Imperia, Marino, Marsciano, Monterotondo, Montevarchi, Narni, Rieti, Spoleto, Todi, Umbertide, Verbania and Vercelli.

Tables

Table 1: Urban regeneration program in Italy (2007-2013)

Region (NUTS2)	Macro-area	Program	Year	Number of projects	Size (mln euros)
Abruzzo	South	Progetti integrati di sviluppo urbano (PISU)	2008	4	40,8
Calabria	South	Progetti integrati di sviluppo urbano (PISU)	2011	8	255,4
Campania	South	Programmi integrati urbani (PIU) Europa	2009	19	623,0
Lazio	Center-North	Piani locali urbani di sviluppo (PLUS)	2012	16	147,2
Liguria	Center-North	Progetti integrati di sviluppo urbano (PISU)	2008	10	124,0
Piemonte	Center-North	Progetti integrati di sviluppo urbano (PISU)	2011	8	110,0
Puglia	South	Progetti integrati di sviluppo urbano (PISU)	2010	31	169,0
Toscana	Center-North	Piani integrati di sviluppo urbano sostenibile (PIUSS)	2009	17	134,0
Umbria	Center-North	Progetti urbani complessi (PUC)	2010	12	166,4

Table 2: Sample construction

Panel A: Treated group	
Number of municipalities involved in URPs in 2007-2013	125
Dropping projects in southern municipalities	63
Dropping municipalities with more than 50,000 inhabitants	26
Dropping projects with multiple locations	24
Panel B: Control group	
Number of municipalities not involved in URPs in 2007-2013	7,967
Dropping southern municipalities	5,472
Dropping Centre-North municipalities in Regions with a special statute	4,847
Dropping municipalities with more than 50,000 inhabitants	4,812
Dropping municipalities with less than 10,000 inhabitants	618
Dropping municipalities with missing data	608

Table 3: Descriptive statistics for outcomes

	Mean	S.D.	Min	Max	Obs.
		Panel A: Outcomes in 2007			
Population	18,728	9,336	9,394	51,030	632
Share of foreign citizens	0.07	0.03	0.01	0.19	632
House prices	1,574	588	689	5,839	632
Average income	22,322	2,427	16,757	37,208	632
Gini	0.39	0.03	0.32	0.52	632
Plants	1,690	972	445	6,177	632
Employment	6,553	4,180	864	26,457	632
		Panel B: Change 2007-2015			
Log population	0.050	0.053	-0.075	0.380	632
Share of foreign citizens	0.030	0.019	-0.030	0.109	632
Log house prices	-0.039	0.166	-0.659	0.608	632
Log average income	0.083	0.020	0.002	0.149	632
Gini	-0.009	0.027	-0.107	0.119	632
Log plants	-0.047	0.054	-0.257	0.161	632
Log employment	-0.092	0.104	-0.425	0.626	632

Sources. Census data, Istat, *Osservatorio immobiliare*, *Agenzia delle Entrate*. The average income is only among individuals who are income recipients.

Table 4: OB covariates

Variable	Description	Year	Treated Mean	Controls Mean	P-value
Provincial capital city	Dummy equals to 1 if the city is a provincial capital		0.25	0.01	0.000
Coastal location	Dummy equals to 1 if the city is on a coastal location		0.17	0.11	0.408
Altitude	Altitude in meters		237.96	148.33	0.002
Slope	Splope in meters		658.92	306.59	0.000
Tourism specialization	Dummy equals to 1 if the city is in a tourism specialized LLM		0.00	0.03	0.380
Industrial district	Dummy equals to 1 if the city is in a industrial district		0.13	0.42	0.004
Total population	Total population (log terms)	2001	10.23	9.65	0.000
Total population over surface in sq. km.		2001	486.17	741.34	0.157
Elderly dependency index	Ratio (percent) of population over 64 to population aged 15-64	2001	31.13	27.03	0.003
Youth dependency index	Ratio (percent) of population aged up to 14 to population aged 15-64	2001	19.90	19.60	0.509
Fraction of foreigners	Foreign-citizen residents per 1000 Italian residents	2001	26.69	28.20	0.589
Average household size	Ratio of total population resident in households to number of households	2001	2.64	2.60	0.316
Fraction of own-housing	Percent of houses owned by the residents	2001	73.25	74.26	0.408
Average house size	Average size of houses	2001	96.83	101.16	0.069
Potential use of buildings	Ratio (percent) of unused buildings to total buildings	2001	14.16	10.83	0.148
Average age of buldings	Average age of buildings built after 1962	2001	23.78	23.39	0.444
Fraction of historical buildings	Percent of inhabited houses built before 1919	2001	13.33	11.24	0.173
Index of housing expansion	Percent of inhabited houses that were built in the last decade	2001	7.57	10.44	0.056
Adults with diploma or university degree	Percent of 25-64 adults with high school diploma or university degree	2001	46.63	41.03	0.000
Labor force participation rate	Labor force participation rate (pop aged 15+)	2001	47.99	52.40	0.000
Unemployment rate	Unemployment rate (pop aged 15+)	2001	9.69	5.91	0.000
Employment in agriculture	Ratio (percent) of employment in agriculture to total employment	2001	5.09	3.91	0.120
Employment in industrial sector	Ratio (percent) of employment in the industrial sector to total employment	2001	31.75	41.02	0.000
Employment in trade services	Ratio (percent) of employment in trade services to total employment	2001	20.49	19.67	0.336
Daily mobility for work	Percent of daily commuters (for work) that go outside the municipality	2001	72.38	154.01	0.000
Daily mobility for study	Percent of daily commuters (for study) that go outside the municipality	2001	29.96	59.01	0.000
Mobility with public transportation	Percent of the population moving daily that uses public transport	2001	10.29	10.67	0.660

Table 4 (continue)

Variable	Description	Year	Treated Mean	Controls Mean	P-value
Short mobility	Percent of the population that commutes for less than 30 minutes	2001	79.99	79.88	0.949
Vulnerability index	Average of seven indexes of socio-economic disadvantage	2001	98.45	97.30	0.000
Total population	Total population (log terms)	1991	10.21	9.60	0.000
Total population over surface in kmq		1991	470.68	717.65	0.184
Elderly dependency index	Ratio (percent) of population over 64 to population aged 15-64	1991	24.58	21.23	0.010
Youth dependency index	Ratio (percent) of population aged up to 14 to population aged 15-64	1991	21.97	20.55	0.040
Fraction of foreigners	Foreign-citizen residents per 1000 Italian residents	1991	5.86	5.81	0.943
Average household size	Ratio of total population resident in households to number of households	1991	2.90	2.86	0.379
Fraction of own-housing	Percent of houses owned by the residents	1991	71.28	70.77	0.729
Average house size	Average size of houses	1991	95.21	100.52	0.029
Potential use of buildings	Ratio (percent) of unused buildings to total buildings	1991	14.69	11.62	0.205
Average age of buldings	Average age of buildings built after 1962	1991	17.55	17.59	0.919
Fraction of historical buildings	Percent of inhabited houses built before 1919	1991	17.60	15.53	0.254
Index of housing expansion	Percent of inhabited houses that were built in the last decade	1991	7.72	10.61	0.040
Adults with diploma or university degree	Percent of 25-64 adults with high school diploma or university degree	1991	29.81	25.51	0.001
Labor force participation rate	Labor force participation rate (pop aged 15+)	1991	49.32	52.94	0.000
Unemployment rate	Unemployment rate (pop aged 15+)	1991	14.64	9.51	0.000
Employment in agriculture	Ratio (percent) of employment in agriculture to total employment	1991	6.81	5.18	0.122
Employment in industrial sector	Ratio (percent) of employment in the industrial sector to total employment	1991	34.28	44.70	0.000
Employment in trade services	Ratio (percent) of employment in trade services to total employment	1991	19.21	18.57	0.459
Daily mobility for work	Percent of daily commuters (for work) that go outside the municipality	1991	65.55	132.32	0.000
Daily mobility for study	Percent of daily commuters (for study) that go outside the municipality	1991	27.55	56.52	0.000
Mobility with public transportation	Percent of the population moving daily that uses public transport	1991	14.24	14.58	0.754
Short mobility	Percent of the population that commutes for less than 30 minutes	1991	81.71	82.33	0.722
Vulnerability index	Average of seven indexes of socio-economic disadvantage	1991	99.11	97.89	0.000

Sources. Census data and Istat.

Table 5: Placebo results over 2001-2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: 2001-2007 change in:						
	log population	Share of foreign citizens	log house prices	log income	gini	log plants	log employment
	PANEL A: OLS ESTIMATES (NO CONTROLS)						
policy	-0.020**	-0.003	0.102**	0.049***	0.013***	0.031	0.060**
	(0.010)	(0.003)	(0.046)	(0.012)	(0.004)	(0.025)	(0.028)
	PANEL B: OB ESTIMATES						
policy	0.006	0.003	0.007	0.004	0.003	0.001	-0.015
	(0.012)	(0.004)	(0.058)	(0.011)	(0.004)	(0.030)	(0.035)
	[0.010]	[0.005]	[0.067]	[0.012]	[0.003]	[0.028]	[0.028]
N	632	632	632	632	632	632	632

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at LLM level in parentheses (Spatial HAC se in brackets, assuming a 200 miles maximum radius). Estimates are obtained using the code distributed by Kline and Moretti, 2014.

Table 6: Main results over 2007-2015

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: 2007-2015 change in:						
	log population	Share of foreign citizens	log house prices	log income	gini	log plants	log employment
	PANEL A: OLS ESTIMATES (NO CONTROLS)						
policy	-0.005	0.010***	-0.021	-0.005	0.011**	-0.006	-0.021
	(0.015)	(0.004)	(0.043)	(0.004)	(0.005)	(0.014)	(0.020)
	PANEL B: OB ESTIMATES						
policy	0.000	0.001	0.019	-0.001	0.001	-0.008	-0.005
	(0.019)	(0.004)	(0.050)	(0.004)	(0.006)	(0.016)	(0.026)
	[0.017]	[0.004]	[0.031]	[0.002]	[0.007]	[0.017]	[0.019]
N	632	632	632	632	632	632	632

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at LLM level in parentheses (Spatial HAC se in brackets, assuming a 200 miles maximum radius). Estimates are obtained using the code distributed by Kline and Moretti, 2014.

Table 7: Robustness checks over 2007-2015

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: 2007-2015 change in:						
	log population	Share of foreign citizens	log house prices	log income	gini	log plants	log employment
PANEL A: EXCLUDING SURROUNDING MUNICIPALITIES FROM THE CONTROLS, OB ESTIMATES							
policy	0.016	0.000	-0.022	0.002	0.001	-0.001	0.015
	(0.020)	(0.005)	(0.053)	(0.005)	(0.007)	(0.017)	(0.027)
N	530	530	530	530	530	530	530
PANEL B: INCLUDING ONLY FUTURE TREATED AMONG THE CONTROLS, NO COVARIATES							
policy	0.032*	0.010	0.049	-0.002	0.004	0.006	0.019
	(0.016)	(0.006)	(0.049)	(0.005)	(0.009)	(0.022)	(0.041)
N	33	33	33	33	33	33	33

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at LLM level in parentheses. Estimates are obtained using the code distributed by Kline and Moretti, 2014.

Table 8: Heterogeneity over 2007-2015, OB estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: 2007-2015 change in:						
	log population	Share of foreign citizens	log house prices	log income	gini	log plants	log employment
PANEL A: ONLY TREATED WITH PROJECTS STARTED BEFORE 2011							
policy	-0.005	0.006	0.096**	-0.007	-0.000	-0.020	-0.011
	(0.009)	(0.004)	(0.048)	(0.005)	(0.007)	(0.013)	(0.026)
N	620	620	620	620	620	620	620
PANEL B: ONLY TREATED WITH HIGH INTENSITY							
policy	-0.008	-0.001	0.096*	-0.001	0.005	-0.016	-0.003
	(0.009)	(0.004)	(0.053)	(0.004)	(0.007)	(0.013)	(0.026)
N	620	620	620	620	620	620	620

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at LLM level in parentheses. Estimates are obtained using the code distributed by Kline and Moretti, 2014. Treatment with high intensity are those whose financing is above the median.

Appendix 1: Characteristics of sample projects

City	Types of intervention
Albano Laziale	Public buildings, public spaces, cultural facilities
Amelia	Public buildings, public spaces, cultural facilities, quality of housing, SME incentives
Biella	Public buildings, public spaces, cultural facilities, urban mobility, social services, SME incentives
Castiglione del Lago	Public buildings, public spaces, cultural facilities, SME incentives
Cisterna di Latina	Public buildings, public spaces, urban mobility, SME incentives
Città di Castello	Public buildings, public spaces, quality of housing, SME incentives
Follonica	Public buildings, public spaces, cultural facilities
Fondi	Public buildings, public spaces, cultural facilities, sport facilities, SME incentives
Fonte Nuova	Public buildings, public spaces, social services, economic subsidies
Formia	Public spaces, cultural facilities, SME incentives, economic subsidies
Frosinone	Public buildings, sport facilities, social services
Gubbio	Public buildings, public spaces
Imperia	Public buildings, public spaces
Marino	Public buildings, public spaces, cultural facilities, SME incentives
Marsciano	Public buildings, public spaces, cultural facilities
Monterotondo	Public buildings, public spaces, cultural facilities, urban mobility, social services, SME incentives, economic subsidies
Montevarchi	Public buildings, public spaces, cultural facilities
Narni	Public buildings, public spaces, sport facilities
Rieti	Public buildings, public spaces, SME incentives
Spoleto	Public buildings, public spaces, cultural facilities, urban mobility, quality of housing, SME incentives
Todi	Public buildings, public spaces, quality of housing
Umbertide	Public buildings, public spaces, cultural facilities, urban mobility, quality of housing
Verbania	Public buildings, public spaces, cultural facilities, SME incentives
Vercelli	Public buildings, public spaces, cultural facilities