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# ANYTHING NEW IN TOWN? THE LOCAL EFFECTS OF URBAN REGENERATION POLICIES IN ITALY

by Giuseppe Albanese[*], Emanuele Ciani[**] and Guido de Blasio[**]

## Abstract

The paper estimates the local effects of urban regeneration policies by using evidence from interventions that took place in small and medium-sized cities in the Centre and North of Italy over the period 2008-12. By using an Oaxaca-Blinder reweighting estimator, we find little support for the idea that urban regeneration projects could stimulate local economic growth in the short to medium term. Only the largest scale interventions that focused on improving the public realm seem to have led to an increase in house prices, but they have had no impact on other economic outcomes.

**JEL Classification**: R58, R11, O18.
**Keywords**: urban regeneration, house prices, reweighting.

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# 1. Introduction[1]

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 transportation or other infrastructures. They might involve skill 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 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 no evidence of positive effects on low-income residents.

[1] The authors are grateful to Maria De Paola, Paolo Sestito and seminar participants at Bank of Italy (Rome, 2018), CERUP (Rome, 2018), Urban Economics Association (Düsseldorf, 2018), AISRE (Bolzano, 2018), COMPIE (Berlin, 2018), EALE (Lyon, 2018) and University of Calabria (Rende, 2019) for helpful comments. The views expressed in this paper are those of the authors and do not necessarily correspond to those of the Institution they are affiliated.

This paper contributes to the literature by providing 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 from 2008 to 2012 with the stated goal of spurring local economic growth. 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 issue of concurrent programs. Assuming *selection on observables* and using the Oaxaca-Blinder reweighting estimator (Kline, 2011) we find 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, at least in the period of time in which we can examine all these outcomes (2007-2015). House prices seem to be the only variable on which the programs have an impact, but increases in housing values materialize only for programs with the highest funding and focused on improving public realm. 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 unobservable characteristics (Kline and Moretti, 2014; Andini and de Blasio, 2016).

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 Programs

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/or 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 has been introduced. It integrates physical projects with interventions aimed at favoring local development, with particular attention to the fight against poverty and social exclusion.

The main first-round effect of such policies is to improve the residents' quality of life (WWG, 2014, 2015). Estate renewal interventions do this by improving the quality of housing. This has been found to also have spill-overs on surrounding buildings (Rossi-Hansberg et al., 2010). Projects focused on public realm instead provide or upgrade public goods, such as schools and parks. In both cases, the increase in the value of local amenities makes the area a more attractive place to leave, thereby increasing the demand for housing and local prices (Glaeser, 2008). The distributive effects of this change depend on who owns the land, as the upsurge in rents might entirely compensate the increase in the quality of life. Furthermore, even if we do not observe a change in total population (for instance because housing supply is extremely rigid), there might be a change in the income-composition of residents. Disadvantaged households might leave the area as rents become unaffordable.

The increase in amenities can generate second-round effects. According to standard urban economic models (Glaeser, 2008), the effects of an increase in amenities on wages could be ambiguous. Local 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.[2] 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. These subsidies are closer to those generally provided by place-based policies, in particular those implemented through EU structural funds.

All in all, the effects of URPs are potentially wide, and some channels might offset each other. However, on the empirical side, the evidence is scant and it is generally unsupportive of a significant impact of regeneration programs. This is particularly true for income and wages, employment, and poverty (see for a survey WWG, 2014, 2015). One exception, for the US case, is the paper by Collins and Shester (2013), who provide evidence that a federal urban renewal program lead to an increase in income and population. Instead, there are some studies suggesting a positive impact of URPs on house prices and rents. These

[2] Other second round effects concern the 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).

are mostly studies regarding estate renewal (Schwartz et al., 2006; Rossi-Hansberg et al., 2010; Collins and Shester, 2013; Ahlfeldt et al., 2017). Brown and Geoghegan (2011) evaluate an urban regeneration policy in Worcester (Massachusetts) that invested not only on urban renewal (via housing subsidies) but also on the creation of new public facilities, in particular a high-quality school. They also find evidence of an increase in housing prices. This relates to a wider literature that discusses the link between school quality and house prices. For instance, Cellini et al. (2010) provide evidence that investments in school facilities raise the value of the local housing stock by more than the overall expenditure (on the topic, see also Black, 1999, and Gibbons and Machin, 2003). Positive returns in terms of capitalization in house prices have been shown also for investments in other public facilities, like sports stadia (Ahlfeldt and Kavetsos, 2014). Closer to our work, González-Pampillón et al. (2017) study 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 compare treated areas with others that applied for the policy but were rejected (or never financed), by means of an Oaxaca-Blinder matching estimator. They do not find any impact on population composition, with the exception of Barcelona historical districts.

Some further indirect evidence on the effect of urban regeneration comes from the literature evaluating the effects of big events, which usually come together with large investments in infrastructure and local requalification. Baade and Matheson (2016) review the evidence on the impact of Olympic games and find that in most cases the costs exceed the overall short and long term benefits. Similarly, Feddersen and Maennig (2012) conclude that the literature is quite consistent in finding no employment effects of big sport events and related facilities, although they find that the 2006 FIFA World Cup had a small positive effect on the number of jobs in the hospitality sector. Bronzini et al. (2019) found that the Jubilee in year 2000 had a positive effect on the employment rate in Rome, but this came through a shift of production towards less productive sectors (constructions and low value added services). The impact on house prices was limited to the periphery that benefitted from investment in transport infrastructure.

## 3. URPs in Italy

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 (Table 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 (see below).

The scope of this paper is to provide rigorous evidence on the local impact of this intervention. To this aim, we restrict the attention to a smaller set of URPs in order to obtain 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.[3] It could be therefore 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 (Valle d'Aosta, Friuli-Venezia Giulia, Trentino Alto Adige), which have a stronger responsibility for urban policy (in particular with regard to the environment, local transportation and housing). Second, we trim the sample according to population size. We exclude municipalities with more than 50,000 inhabitants to minimize attenuation concerns, since the available information does not allow us to geo-reference projects and outcomes at the neighborhood level, and therefore the effect of URPs could be statistically indistinguishable in bigger cities. Finally, we exclude two projects that split interventions in more than one municipality.

[3] 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).

After this selection, our treated group includes 26 municipalities in 5 regions (Lazio, Liguria, Piemonte, Toscana and Umbria), started in the years from 2008 to 2012. Figure 1 plots the treated over the map.

Table 3 illustrates, for each of the 26 treated municipalities, the general characteristic of the programs. On average, the largest share of the expenditure was on public realms and other local amenities (Table 3). This share ranged from 50 to 100 per cent of the total funds. For instance, Fondi (Lazio), a town of approximately 35 thousand inhabitants, used 80 per cent of the funds to improve public spaces, in particular by building a new public pre-school and a sport campus, improving the road network, requalifying one of the squares and creating new green areas. Biella (Piemonte, around 45 thousands inhabitants) spent almost 40 per cent of the funds to develop and protect the cultural heritage, by building a new public library and completing museums. Another large fraction, nearly one third, was employed to improve mobility by building a new public parking, renovating the cable railway stations and improving the bike sharing service.

The money spent to improve housing quality (through direct intervention on public housing and subsidies to local residents) was less relevant on average, although it reached 20 per cent of total spending in 9 municipalities. Many interventions also provided subsidies aimed at promoting local growth. These envisage skills training, active labor market initiatives and subsidies for small and medium enterprises as well as self-employment. These subsidies are similar to those from European structural funds, who have been found to have a small positive effect on employment (Ciani and de Blasio, 2015), although the evidence on the impact of EU funds in Italy suggests an overall low effectiveness (see, for instance, Becker et al., 2013).[4]

As the projects mostly improved the local amenities, from a theoretical point of view (Section 2) we expect them to impact the residents' quality of life and, as a result, to increase

[4] In the heterogeneity analysis of the effect of EU funds on growth by Becker et al. (2013), Italian regions rank among those with the lowest likelihood of a positive effect. Ciani and de Blasio (2015) also find an overall limited effectiveness for the period 2007-13. A study by Giua (2017), focusing on municipalities across the geographical boundary between treated and untreated regions, suggests instead a positive impact on local employment.

house prices. The increased quality of life attracts population mobility, but if the housing supply is rigid we may not observe a significant change in the number of residents. Instead, the URP might induce a change in the income-mix of residents, with an influx of more affluent households that can afford higher prices. We therefore also look at average taxable income and at measures of income inequality (Gini index).

Theoretical predictions for second-round effects on employment and economic activity are ambiguous, but previous evidence suggests that URPs are unlikely to have any effect. Nevertheless, the new generation of URPs, in particular the one that we study, has been implemented with the specific goal of fostering local growth. For this reason we assess the impact of this policy on local employment and number of plants, too. Evidence about the effect on these outcomes is also key for the literature evaluating the effect of European structural funds. In the current programming cycle, urban regeneration is considered as a possible driver for local development.[5]

An important issue concerns the size of the projects considered here. The average financing was 8 million euros. However, this amount refers only to the funds provided by the EU programs (structural funds and national co-financing). There is no information available for all projects about the use of other local and private funds, even if this was a common practice. According to our calculations based on 15 of our sample projects, the average ratio of funds by EU programs to total investment was about 45%. Thus, the estimated total investment, which includes non EU-related financing, is 611 euro per capita. This is quite in line with the average size of similar projects implemented in the past. For instance, the European URBAN I program launched in 1994 in 118 cities in all EU-15 member states provided investment equals to 562.5 euro per capita.[6]

As a matter of fact, Table 4 shows that the projects led to a large increase in public capital expenditures in the treated cities. This appears to be true both for the payments by 2007-2013 EU programs and the contemporaneous spending from the municipality's balance

[5] See https://ec.europa.eu/regional_policy/en/policy/themes/urban-development/ (last access: 09/01/2019).
[6] See: http://ec.europa.eu/regional_policy/archive/urban2/urban/initiative/src/frame1.htm (last access: 09/01/2019).

sheet, and is also robust to taking into account differences in cities by means of an Oaxaca-Blinder matching estimator (see Section 3.2).[7]

Nevertheless, we acknowledge that our analysis is on the lower spectrum of project intensity with respect to previous evidence on URPs that intervened on public realm. In Brown and Geoghegan (2011), the per-capita investment was slightly more than 8,000 euro (adjusted to 2007 PPP). The Catalonian projects evaluated by González-Pampillón et al. (2017) invested more than 3,000 (adjusted to 2007 PPP), although the authors find zero effects on the population independently from project size. Papers looking at the impact of URPs focused on estate renewal are also generally focused on large per-capita investment (Ahlfeldt et al., 2017; Rossi-Hansberg et al., 2010). However, positive effects on house prices and population are found even for less strong projects (Collins and Shester, 2013) and the relation between project intensity and size of the effect is unclear.[8] For instance, Ahlfeldt et al. (2017) find weaker effects than Rossi-Hansberg et al. (2010) despite they evaluate a much bigger scale programme. In the empirical analysis we therefore also look at whether results are different for projects above the average per-capita financing, whose estimated expenditure was between 850 and 1,600 euro per capita.

# 4. Empirical framework

## 4.1 The dataset

To identify the effect of URPs on target cities, their performance is compared with those of a control group made up of 606 municipalities in 9 regions in the Centre-North of Italy (those of treated sample, plus Emilia Romagna, Lombardia, Marche and Veneto). To ensure common support, we delete all municipalities with more than 50,000 inhabitants. We also exclude those with less than 10,000, as none of the municipalities in the treated group is smaller than this size. Control municipalities are also shown in Figure 1.

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[7] Moreover, estimates (not reported here) show that the capital expenditures by the treated cities in the period before the policy (2001-2007) were not significantly different from those by the control group.

[8] A precise calculation of the average intensity in Collins and Shester (2013) is complicated by the fact that they only have total nominal figures for investments that took places over 25 years. We used their average nominal per-capita sum (177 dollars) and we assume that the expenditure was equally distributed (in real terms, using the CPI) over the 25 years. Updating the figure to 2007 euro in PPS, the average investment was nearly 1,300 euro per capita.

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 of individual income, 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.[9] Finally, income data come from the Italian tax office *Agenzia delle Entrate* and refer to sources subject to personal income taxation. The Gini inequality index calculated on this data is based on individual income, as no family data is available.[10] It may nevertheless capture relevant changes in the distribution, such as the growth in residents that have high earnings or the contraction of those with low-paid jobs. Summary statistics on outcomes are reported in Table 5.

There are of course many sources of selection bias that might plague an evaluation of the URPs. As discussed in Section 4.2, 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 28 geographical, demographic, and socio-economic variables from Census and 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 6, 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 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

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[9] 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.

[10] For Italy, personal income taxation data have been used by Acciari and Mocetti (2013) to discuss the geography of inequality in Italy. They show high correlation in the regional Gini index calculated on this source and more standard Gini indices calculated on survey data (using equivalized household disposable income).

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.

## 4.2 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. We exploit a difference-in-differences strategy by assuming that:

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

where $\lambda_g$ are group effects ($g \in \{treated, control\}$) while $\gamma_t$ are time effects. The policy took 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 EU budget cycle. As the post period we take 2015, because (i) since 2016 most of the projects of the new EU funds programming cycle are already operative (and therefore there are confounding factors) and (ii) this is the most recent available year across all variables.[11] 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 of municipalities would have experienced similar economic and demographic trends in the absence of the URPs. We also assume that the treatment effect is additive (but not necessarily constant across municipalities), 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):

[11] The year 2015 was also the last year when the EU funds of cycle 2007-2013 had to be spent (according to the "N+2" mechanism). Thus, even if we do not have information about the projects' closure, it is very plausible that they were mostly completed within our sample period.

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

The validity of the result hinges on the credibility of the parallel trends assumption. For instance, in our context, the group of municipalities with an URP was likely hit by the economic recession differently than the control group, as the sectoral composition of employment was different and the labor market was already weaker before the start of the policy.

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

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

In other terms, we assume that the parallel trends assumption holds only after accounting for differences in $X_i$.[12] Hence, comparing the change in the outcome between the treated and control group for municipalities with similar characteristics we identify the effect for municipalities with similar characteristics:

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

In the paper we account for fixed characteristics and pre-determined covariates defined in 2001 and 1991 (the previous Census waves), assuming that they capture the relevant differences between municipalities with an URP and the others. 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 in these characteristics, we use a reweighting 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

[12] Similarly, we assume something similar to (2) but conditional on $X_i$.

treated units (26) is actually smaller of the number of covariates (50), while Kline's (2011) Oaxaca-Blinder (OB) reweighting estimator can overcome this issue. However, even limiting the number of covariates, Sloczynski (2015) shows that, in finite-sample settings, OB estimator decomposition performs better than propensity score matching and other estimators within the class of matching methods (such as inverse probability weighting, kernel matching, matching on covariates, and bias-corrected matching).

The OB estimator has been used for the evaluation of place-based policies by Kline and Moretti (2014) and González-Pampillón et al. (2017; see Section 2), among others. It assumes that the counterfactual time trend ($\Delta y_{0i}$) is linear in the controls:[13]

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

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. To make sure that this assumption holds, we restrict the sample to the Centre-North and to a range of medium sized municipalities, in order to limit the differences. Inspecting Table 5, other differences in $X_i$ do not seem as large as to violate assumption (7).

To provide further 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. This allows us to detect whether the two groups were already on a diverging path. Given that we do not use pre-trends nor lagged outcomes as covariates, any significant differences would be informative about a possible violation of assumption (4), as the trends in our period of interest (2007-2015) are likely to be correlated with those in the previous one.

Apart from the issue of parallel trends, another problem is that the municipality level might not be the one at which we expect to find effects. This can be broken into three different issues: (i) the effects are actually localized at a sub-municipal level, and therefore

[13] This also implies that the counterfactual can be identified by a regression of $\Delta y_i$ among the controls (where $\Delta y_i = \Delta y_{0i}$), and therefore we only need the 50 covariates not to be collinear for this regression, which includes 606 observed municipalities. Kline (2011) proves that this approach is equivalent to a reweighting estimator in which the odds of treatment are linear in the covariates.

when we look at municipalities we get an attenuation bias; (ii) the projects induce a reallocation between different neighborhoods within the same municipality; (iii) there are spill-overs on contiguous municipalities. With respect to the first issue, we do not have data at a higher detail of georeferencing.[14] Furthermore, for several projects we do not have sufficient info to identify the precise location of the interventions, or we know that they were spread around the whole municipality. Nevertheless, our treated municipalities are not much bigger than neighborhoods in big cities: on average, population is 31 thousand inhabitants, while the urban area covers 11 $km^2$. We believe that in this context the attenuation bias is mild and we can safely consider the whole municipality as treated. Similar points can be raised with respect to issue (ii). On top of that, if there was reallocation between different neighborhoods of our small municipalities, then our estimates can be interpreted as the total effect taking into account the (very) local reallocations. Issue (iii) raises some concerns with respect to the choice of control and treated units. As a sensitivity check, we also try to either exclude contiguous municipalities from the control group or include them in the treated one (see Table 10).

Finally, our evaluation essentially looks at the short and medium run impact of these projects. We are unable to perform an analysis on the longer run, because of data availability and the concerns related to the start of the new programming period. Some effects, though, might materialize in the longer run. We try to provide some indirect evidence by looking at the projects that started earlier only, and by updating results to 2017 for population and house prices, for which we have more recent data.

# 5. Results

## 5.1 Testing the parallel trend assumption (2001-07)

We start in Table 7 from estimates on the period before the policy (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, i.e. $E(\Delta y_i|g = treated) - E(\Delta y_i|g = control)$ (Panel A). The estimates display several

[14] Data at a lower level of aggregation are generally available only on the Census, but our period of interest starts between two census waves (2001 and 2011) and ends far from the next one (2021). For house price data we have access to sub-municipal zones, but during the period 2007-2015 there was a change in the mapping of these zones and therefore we cannot compare them along time.

diverging trends, which 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. This result 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.

## 5.2 Main results (2007-15)

Table 8 shows the main results. Without covariates we detect a positive effect on the share of foreigners and an increase in inequality of individual incomes (Gini index), but these results may be due to differential trends, as in the period 2001-07. Indeed, both effects disappear (both in terms of statistical significance and size) when we use the OB estimator (Panel B). Essentially, the URPs do not seem to have had any effect on population, house prices and economic conditions.

As discussed above, the effects may depend on the scale of the projects, while estimates in Table 8 pooled the different project independently from size. In Panel A of Table 9 we instead look only at projects that have a per-capita investment above the average. Interestingly, in these cases we detect a positive effect on house prices, while there is still no impact on other economic outcomes, with small point estimates in all cases. The effect is quite sizeable, but we should consider that these URPs involved municipalities that started from a disadvantaged background and from very low house price values (1,382 euro per $m^2$). In order to better compare the effect with results from other papers, we can evaluate the return to the public investment at average values. The per-capita housing stock can be calculated my multiplying the average price per $m^2$ by the average size of houses (101.8 $m^2$) and dividing by average household size, which gives a value of 58,911 euro. The per-capita return is 0.134 (the effect on house prices) times this value (7,894 euro). Given that the estimated average public investment is 1,045, the ratio between gains and expenditure is approximately 7.55. This value is larger than the above mentioned results about the effect of estate renewal policies. Rossi-Hansberg et al. (2010), who evaluated a slum clearance project in Richmond (USA), found ratios between 2.13 and 6.73. Ahlfeldt et al. (2017), who studied a urban renewal policy in Berlin, found smaller ratios, with estimates range between 0.06 and 1.35. One explanation might be that the creation of public goods has stronger effects than the direct intervention on some of the buildings of the neighborhood. For instance, the overall gains in Rossi-Hansberg et al. (2010) are not due only to the renovation of the building units directly affected by the slum clearance, but also to the spill-overs on other buildings. It is possible that residents evaluate amenities and public goods more than the quality of the other residential buildings that surround them. Brown and Geoghegan (2011) found that the establishment of a high-quality high school in Worcester (Massachusetts) had a strong impact on nearby houses, but unfortunately they do not offer an overall calculation for the ratio between gains and expenditure. More in general, studies assessing the impact of changes in amenities and public services suggest that the returns to local residents are generally larger than the costs (see, for instance, Ahlfeldt and Kavetsos, 2014, on sports stadia, and Cellini et al., 2010, on school facility investments).

Projects differed in terms of the composition between public works, housing and subsidies. Most of the investment was, on average, on public realm. However, some municipalities spent non-negligible sums also on directly improving housing quality and on subsidies for households and businesses. In Panel B we look at the 8 projects with at least 90% of expenditure devoted to public realm. The results provides further evidence that URPs focused on public goods are more likely to have a positive impact on house prices. However, this finding should be interpreted in relation with project size, because 4 of these URPs included in Panel B are also among those with above-the-average financing. In panel C we instead look at projects with at least 20% of expenditure devoted to housing improvement. The only difference with the average estimated effects is that there seems to be a slightly positive impact on the fraction of foreigners. This might be due to the fact that the investment on housing was mostly directed towards public housing, which could attract more foreigners due to their lower socio-economic status. Panel D looks at URPs with at least 20% expenditure on subsidies. Also in this case results are overall similar to those presented in Table 8.

Our main estimates look at the short impact of the policies. To check whether effects are more likely to materialize in the medium run, in Panel E we restrict attention only to projects that started before 2011. Also in this case we are able to detect a positive effect on house prices, while all the other results are still small and not significant. Also in this case, it is not easy to disentangle the date-of-start effect from the project-scale one, because the group of URPs that started before 2011 include also those with above-the average investment, apart from one (Verbania). To further discriminate between the two, we exploit the fact that for house prices and population we have available information also up to 2017. Table 10 (Panel A) reports the corresponding OB estimates for the period 2007-2017. The previous results are confirmed and extended to other demographics outcomes (number of births, outflow and inflows of people). This suggests that assessing the effects on a longer horizon is not necessarily going to lead to different results. Again, positive effects are found only for projects with larger scale (Table 10, Panel B).

## 5.3 Robustness checks

Localized spill-overs might bias our estimates. 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 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.

To tackle this problem, we resort to a simple strategy of excluding surrounding municipalities from the control group. In Table 11, Panel A, we run the corresponding OB regressions. The results are reassuring, as we still fail to detect any effect. Alternatively, we can include the contiguous municipalities among the treated. The previous results are confirmed also in this case (Table 11, Panel B).

To check that our results are not sensitive to the methodological choice, we consider a standard Propensity score (PS) reweighting estimator as proposed by Rosenbaum and Rubin (1983). Given the small number of treated units, we need to reduce the dimensionality of covariate vectors. In order to do so, we follow the "double selection" procedure proposed by Belloni et al (2014). We select a subset of variables by using a Least Absolute Shrinkage and Selection Operator (LASSO), which minimizes the sum of squared residuals and an additional penalty parameter that aims to reduce the overall size of the model. For each outcome, we use the covariates that are selected in either the regression of $\Delta y_i$ on $X_i$ or in the one for $1[treated]_i$ on $X_i$. Table 12 shows that the results of this procedure are in line with the baseline.

Finally, an alternative strategy could be to compare the municipalities that were treated in 2007-13 with those that are currently planning to carry out URPs in the following programming period (2014-20). In such a group, the unconfoundedness assumption is more likely to hold, given that there are similar conditions that led the local administrations to implement URPs. Importantly, if selection into treatment also depends on unobservable drivers of $\Delta y_{0i}$, 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 24 control municipalities that will carry out URPs in the current cycle.[15] Due to the decrease in sample size, also in this case we applied a standard PS reweighting estimator, using the subset of variables obtained previously by LASSO. Table 13 shows that our results are overall confirmed.

# 6. Conclusions

This paper provides 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, which 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

[15] By considering the information available on the programming period 2014-2020, it is possible to identify 148 projects, funded in 16 regions that have activated URPs. By applying the same filters than before (see Table 2), we finished with a sample of 24 municipalities in 4 regions (Lombardia, Veneto, Toscana and Marche). Because of the delay in the approval of operational programs, all of these projects have started after 2015.

EU programming period. This suggests that the degree to which our estimates are biased by unobservables might be very limited.

Our evidence suggest a positive impact on house prices, in line with theoretical expectations and previous evidence, although only for programs that had larger size and invested the most of the funds on public goods. This implies that the URPs improved amenities, which are positively valued by residents. We do not find evidence that URPs spurred local economic growth. Income as well as employment did not change, hence it seems that local productivity was unaffected. Local population did not change and there are no sensible variations in the inward and outward mobility. In terms of distributional consequences, the increase the quality of life has been at least partially captured by the increase in housing prices. However, given that 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.

Two caveats are in order. First, in this paper we analyze the short to medium term impact of the policy. Consequently, we may not be able to detect some effects that could materialize only in the long term as people and firms become mobile across cities. For instance, 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, even extending results up to 2017. 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. Of course population stability might come together with a compositional change, but there does not seem to be a variation in the fraction of foreigners and in average income, nor in the Gini inequality index.

Second, with regards to the external validity of the exercise, our sample includes only medium-size projects in small cities. They are very similar in scale to the urban interventions financed by the EU budget across Europe in the past programming periods. Our evidence, though, may not apply to big URPs. Nevertheless, our finding of a positive impact on housing prices confirms results from earlier paper that looked at large scale investments.

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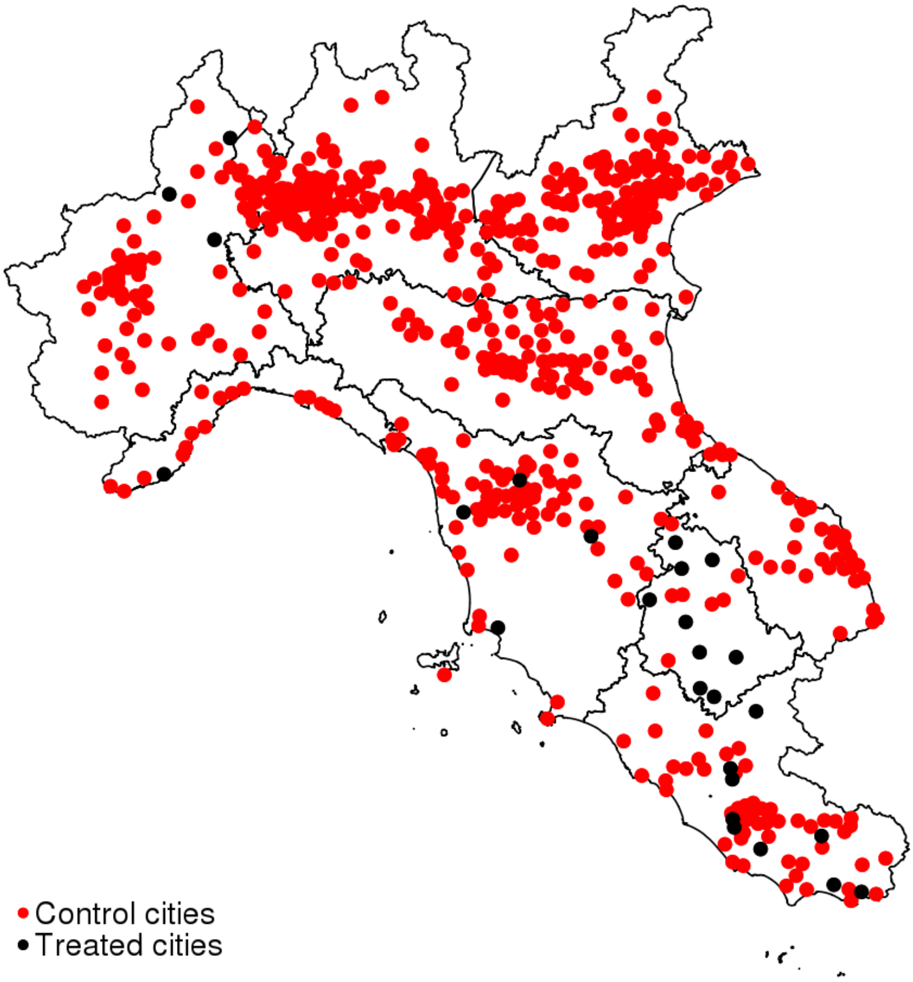
# Figures

**Figure 1: Cities in the sample**




*Notes*. Treated cities are: Albano Laziale, Amelia, Biella, Cascina, Castiglione del Lago, Cisterna di Latina, Città di Castello, Follonica, Fondi, Fonte Nuova, Formia, Frosinone, Gubbio, Imperia, Marino, Marsciano, Monterotondo, Montevarchi, Narni, Quarrata, 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 euro) |
|---|---|---|---|---|---|
| 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 | 30 |
| Dropping projects with multiple locations | 26 |
| 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 | 4,821 |
| Dropping municipalities with less than 10,000 inhabitants | 609 |
| Dropping municipalities with missing data | 606 |

## Table 3: Characteristics of sample projects

| City | Population 2007 (thousands) | Funds (millions euro) | % Costs for: Public goods | Housing | Subsidies to families and firms | Urban marketing and other services | Year project start |
|---|---|---|---|---|---|---|---|
| Albano Laziale | 36.862 | 5.1 | 83.3 | 0.0 | 13.3 | 3.3 | 2012 |
| Amelia | 11.655 | 5.3 | 52.8 | 24.5 | 18.9 | 3.8 | 2010 |
| Biella | 44.823 | 11.5 | 89.1 | 0.0 | 10.2 | 0.8 | 2011 |
| Cascina | 24.503 | 10.1 | 100.0 | 0.0 | 0.0 | 0.0 | 2009 |
| Castiglione del Lago | 15.086 | 5.8 | 51.7 | 20.7 | 22.4 | 5.2 | 2010 |
| Cisterna di Latina | 33.851 | 6.1 | 86.1 | 0.0 | 7.6 | 6.3 | 2012 |
| Città di Castello | 39.594 | 4.7 | 57.4 | 19.1 | 21.3 | 2.1 | 2010 |
| Follonica | 21.552 | 14.8 | 100.0 | 0.0 | 0.0 | 0.0 | 2009 |
| Fondi | 34.621 | 8 | 79.6 | 0.0 | 18.3 | 2.2 | 2012 |
| Fonte Nuova | 27.432 | 5.1 | 86.8 | 0.0 | 3.8 | 9.4 | 2012 |
| Formia | 35.909 | 6.1 | 67.1 | 0.0 | 20.0 | 12.9 | 2012 |
| Frosinone | 47.505 | 5 | 92.0 | 0.0 | 4.0 | 4.0 | 2012 |
| Gubbio | 32.372 | 6.6 | 65.2 | 19.7 | 15.2 | 0.0 | 2010 |
| Imperia | 40.979 | 10 | 100.0 | 0.0 | 0.0 | 0.0 | 2008 |
| Marino | 35.438 | 5 | 83.9 | 0.0 | 3.6 | 12.5 | 2012 |
| Marsciano | 18.028 | 4.7 | 45.7 | 30.4 | 23.9 | 0.0 | 2010 |
| Monterotondo | 37.042 | 5.1 | 74.1 | 0.0 | 19.0 | 6.9 | 2012 |
| Montevarchi | 23.326 | 16.7 | 100.0 | 0.0 | 0.0 | 0.0 | 2009 |
| Narni | 20.333 | 7.5 | 52.6 | 21.1 | 23.7 | 2.6 | 2010 |
| Quarrata | 41.763 | 10.3 | 97.1 | 1.9 | 0.0 | 1.0 | 2009 |
| Rieti | 45.813 | 7.1 | 76.7 | 0.0 | 20.0 | 3.3 | 2012 |
| Spoleto | 38.123 | 7.5 | 53.3 | 20.0 | 21.3 | 5.3 | 2010 |
| Todi | 16.876 | 4.5 | 55.6 | 20.0 | 20.0 | 4.4 | 2010 |
| Umbertide | 16.116 | 5.9 | 52.5 | 22.0 | 22.0 | 3.4 | 2010 |
| Verbania | 30.259 | 12.8 | 92.5 | 6.8 | 0.0 | 0.8 | 2011 |
| Vercelli | 45.413 | 11.1 | 99.2 | 0.0 | 0.8 | 0.0 | 2011 |
| Total URPs | 815.274 | 202.4 | 81.8 | 5.7 | 9.7 | 2.8 | |

**Table 4: Capital expenditure at the city level**

| | (1) | (2) |
|---|---|---|
| | 2007-2013 EU programs payments | Municipal capital expenditures |
| PANEL A: SIMPLE DIFFERENCE TREATED-CONTROLS (NO COVARIATES) | | |
| policy | 1.217*** | 0.332*** |
| | (0.125) | (0.088) |
| PANEL B: OB ESTIMATES | | |
| policy | 0.543*** | 0.169* |
| | (0.138) | (0.092) |
| N | 632 | 632 |

Note: * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is the log expenditure per capita at the city level. Standard errors clustered at LLM level in parentheses. Estimates are obtained using the code distributed by Kline and Moretti, 2014.

**Table 5: 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 (euro per $m^2$) | 1,574 | 588 | 689 | 5,839 | 632 |
| Average income (euro) | 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 6: 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 | | 0.23 | 0.01 | 0.000 |
| Coastal location | Dummy equals to 1 if the city is on a coastal location | | 0.15 | 0.11 | 0.514 |
| Altitude | Altitude in meters | | 221.81 | 148.73 | 0.008 |
| Slope | Slope in meters | | 628.31 | 306.74 | 0.000 |
| Tourism specialization | Dummy equals to 1 if the city is in a tourism specialized LLM | | 0.00 | 0.03 | 0.360 |
| Industrial district | Dummy equals to 1 if the city is in an industrial district | | 0.15 | 0.42 | 0.008 |
| Total population | Total population (log terms) | 2001 | 10.24 | 9.65 | 0.000 |
| Total population over surface in sq. km. | | 2001 | 486.54 | 742.16 | 0.141 |
| Elderly dependency index | Ratio (percent) of population over 64 to population aged 15-64 | 2001 | 30.89 | 27.02 | 0.004 |
| Youth dependency index | Ratio (percent) of population aged up to 14 to population aged 15-64 | 2001 | 19.75 | 19.60 | 0.730 |
| Fraction of foreigners | Foreign-citizen residents per 1000 Italian residents | 2001 | 27.13 | 28.18 | 0.696 |
| Average household size | Ratio of total population resident in households to number of households | 2001 | 2.65 | 2.60 | 0.163 |
| Fraction of own-housing | Percent of houses owned by the residents | 2001 | 73.40 | 74.26 | 0.466 |
| Average house size | Average size of houses | 2001 | 97.76 | 101.13 | 0.142 |
| Potential use of buildings | Ratio (percent) of unused buildings to total buildings | 2001 | 13.50 | 10.85 | 0.231 |
| Average age of buildings | Average age of buildings built after 1962 | 2001 | 23.79 | 23.39 | 0.414 |
| Fraction of historical buildings | Percent of inhabited houses built before 1919 | 2001 | 13.77 | 11.21 | 0.084 |
| Index of housing expansion | Percent of inhabited houses that were built in the last decade | 2001 | 8.54 | 10.41 | 0.197 |
| Adults with diploma or university degree | Percent of 25-64 adults with high school diploma or university degree | 2001 | 45.68 | 41.05 | 0.001 |
| Labor force participation rate | Labor force participation rate (pop aged 15+) | 2001 | 48.38 | 52.40 | 0.000 |
| Unemployment rate | Unemployment rate (pop aged 15+) | 2001 | 9.46 | 5.90 | 0.000 |
| Employment in agriculture | Ratio (percent) of employment in agriculture to total employment | 2001 | 4.95 | 3.91 | 0.155 |
| Employment in industrial sector | Ratio (percent) of employment in the industrial sector to total employment | 2001 | 32.73 | 41.01 | 0.000 |
| Employment in trade services | Ratio (percent) of employment in trade services to total employment | 2001 | 20.42 | 19.67 | 0.362 |
| Daily mobility (for work or study) | Percent of daily commuters that go outside the municipality | 2001 | 59.80 | 63.60 | 0.000 |
| Mobility with public transportation | Percent of the population moving daily that uses public transport | 2001 | 10.24 | 10.67 | 0.606 |

**Table 6 (continue)**

| Variable | Description | Year | Treated Mean | Controls Mean | P-value |
|---|---|---|---|---|---|
| Short mobility | Percent of the population that commutes for less than 30 minutes | 2001 | 14.68 | 15.29 | 0.561 |
| Vulnerability index | Average of seven indexes of socio-economic disadvantage | 2001 | 98.39 | 97.30 | 0.000 |
| Total population | Total population (log terms) | 1991 | 10.21 | 9.60 | 0.000 |
| Total population over surface in $km^2$ | | 1991 | 469.84 | 718.50 | 0.165 |
| Elderly dependency index | Ratio (percent) of population over 64 to population aged 15-64 | 1991 | 24.56 | 21.21 | 0.007 |
| Youth dependency index | Ratio (percent) of population aged up to 14 to population aged 15-64 | 1991 | 21.77 | 20.56 | 0.068 |
| Fraction of foreigners | Foreign-citizen residents per 1000 Italian residents | 1991 | 5.83 | 5.81 | 0.979 |
| Average household size | Ratio of total population resident in households to number of households | 1991 | 2.91 | 2.86 | 0.270 |
| Fraction of own-housing | Percent of houses owned by the residents | 1991 | 71.32 | 70.76 | 0.699 |
| Average house size | Average size of houses | 1991 | 95.91 | 100.51 | 0.049 |
| Potential use of buildings | Ratio (percent) of unused buildings to total buildings | 1991 | 14.15 | 11.64 | 0.281 |
| Average age of buildings | Average age of buildings built after 1962 | 1991 | 17.57 | 17.59 | 0.947 |
| Fraction of historical buildings | Percent of inhabited houses built before 1919 | 1991 | 18.54 | 15.49 | 0.079 |
| Index of housing expansion | Percent of inhabited houses that were built in the last decade | 1991 | 7.65 | 10.62 | 0.028 |
| Adults with diploma or university degree | Percent of 25-64 adults with high school diploma or university degree | 1991 | 29.01 | 25.53 | 0.008 |
| Labor force participation rate | Labor force participation rate (pop aged 15+) | 1991 | 49.59 | 52.94 | 0.000 |
| Unemployment rate | Unemployment rate (pop aged 15+) | 1991 | 14.34 | 9.50 | 0.000 |
| Employment in agriculture | Ratio (percent) of employment in agriculture to total employment | 1991 | 6.57 | 5.18 | 0.172 |
| Employment in industrial sector | Ratio (percent) of employment in the industrial sector to total employment | 1991 | 35.53 | 44.68 | 0.000 |
| Employment in trade services | Ratio (percent) of employment in trade services to total employment | 1991 | 19.13 | 18.57 | 0.500 |
| Daily mobility (for work or study) | Percent of daily commuters that go outside the municipality | 1991 | 60.27 | 62.88 | 0.006 |
| Mobility with public transportation | Percent of the population moving daily that uses public transport | 1991 | 14.23 | 14.58 | 0.736 |
| Short mobility | Percent of the population that commutes for less than 30 minutes | 1991 | 21.52 | 21.83 | 0.800 |
| Vulnerability index | Average of seven indexes of socio-economic disadvantage | 1991 | 99.05 | 97.89 | 0.000 |

*Sources*. Census data and Istat.

**Table 7: Test for the parallel trends assumption (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: SIMPLE DIFFERENCE TREATED-CONTROLS (NO COVARIATES) | | | | | | |
| policy | -0.017* | -0.004 | 0.101** | 0.048*** | 0.012*** | 0.025 | 0.055** |
| | (0.009) | (0.003) | (0.042) | (0.012) | (0.004) | (0.024) | (0.028) |
| | PANEL B: OB ESTIMATES | | | | | | |
| policy | 0.007 | 0.003 | 0.006 | 0.006 | 0.005 | -0.005 | -0.010 |
| | (0.012) | (0.004) | (0.054) | (0.010) | (0.004) | (0.029) | (0.034) |
| | [0.009] | [0.004] | [0.063] | [0.012] | [0.003] | [0.028] | [0.028] |
| Obs. | 632 | 632 | 632 | 632 | 632 | 632 | 632 |
| # treated | 26 | 26 | 26 | 26 | 26 | 26 | 26 |

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 8: 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: SIMPLE DIFFERENCE TREATED-CONTROLS (NO COVARIATES) | | | | | | |
| policy | -0.001 | 0.009** | -0.010 | -0.005 | 0.012** | -0.007 | -0.020 |
| | (0.013) | (0.004) | (0.039) | (0.004) | (0.005) | (0.013) | (0.020) |
| | PANEL B: OB ESTIMATES | | | | | | |
| policy | 0.004 | 0.002 | 0.017 | -0.003 | 0.000 | -0.004 | -0.002 |
| | (0.016) | (0.004) | (0.044) | (0.004) | (0.005) | (0.016) | (0.026) |
| | [0.014] | [0.004] | [0.030] | [0.002] | [0.006] | [0.017] | [0.017] |
| Obs. | 632 | 632 | 632 | 632 | 632 | 632 | 632 |
| # treated | 26 | 26 | 26 | 26 | 26 | 26 | 26 |

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 9: Heterogeneity, 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 HIGH INTENSITY | | | | | | |
| policy | -0.005 | 0.001 | 0.134** | -0.007 | -0.002 | -0.003 | 0.006 |
| | (0.011) | (0.004) | (0.061) | (0.006) | (0.009) | (0.013) | (0.035) |
| Obs. | 614 | 614 | 614 | 614 | 614 | 614 | 614 |
| # treated | 8 | 8 | 8 | 8 | 8 | 8 | 8 |
| | PANEL B: ONLY TREATED WITH AT LEAST 90% EXPENDITURE ON PUBLIC WORKS | | | | | | |
| policy | -0.022 | -0.007 | 0.145*** | -0.007 | -0.001 | -0.018 | -0.006 |
| | (0.014) | (0.005) | (0.056) | (0.006) | (0.008) | (0.019) | (0.046) |
| Obs. | 614 | 614 | 614 | 614 | 614 | 614 | 614 |
| # treated | 8 | 8 | 8 | 8 | 8 | 8 | 8 |
| | PANEL C: ONLY TREATED WITH AT LEAST 20% EXPENDITURE ON HOUSING | | | | | | |
| policy | -0.003 | 0.006* | 0.061 | -0.005 | 0.002 | -0.014 | -0.018 |
| | (0.009) | (0.003) | (0.053) | (0.006) | (0.009) | (0.011) | (0.021) |
| Obs. | 615 | 615 | 615 | 615 | 615 | 615 | 615 |
| # treated | 9 | 9 | 9 | 9 | 9 | 9 | 9 |
| | PANEL D: ONLY TREATED WITH AT LEAST 20% EXPENDITURE ON SUBSIDIES | | | | | | |
| policy | 0.004 | 0.006 | 0.008 | -0.001 | 0.004 | -0.012 | -0.009 |
| | (0.014) | (0.005) | (0.051) | (0.005) | (0.007) | (0.013) | (0.023) |
| Obs. | 619 | 619 | 619 | 619 | 619 | 619 | 619 |
| # treated | 13 | 13 | 13 | 13 | 13 | 13 | 13 |
| | PANEL E: ONLY TREATED WITH PROJECTS STARTED BEFORE 2011 | | | | | | |
| policy | 0.000 | 0.004 | 0.082* | -0.008 | -0.002 | -0.011 | -0.004 |
| | (0.009) | (0.004) | (0.046) | (0.005) | (0.006) | (0.013) | (0.028) |
| Obs. | 620 | 620 | 620 | 620 | 620 | 620 | 620 |
| # treated | 14 | 14 | 14 | 14 | 14 | 14 | 14 |

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 per capita funds is above the average.

**Table 10: Robustness checks: Up-to-date data on house prices and demography, OB estimates**

| | (1) | (2) | (3) | (4) | |
|---|---|---|---|---|---|
| | Dependent variable: 2007-2017 change in | | | | |
| | log house prices | log population | log births | log inflows of people | log outflows of people |
| | PANEL A: ALL URPS | | | | |
| policy | 0.014 | 0.005 | 0.032 | 0.006 | 0.038 |
| | (0.047) | (0.020) | (0.031) | (0.055) | (0.041) |
| Obs. | 632 | 632 | 632 | 632 | 632 |
| # treated | 26 | 26 | 26 | 26 | 26 |
| | PANEL B: ONLY TREATED WITH HIGH INTENSITY | | | | |
| policy | 0.140** | -0.001 | 0.008 | -0.004 | 0.002 |
| | (0.056) | (0.002) | (0.011) | (0.013) | (0.008) |
| Obs. | 614 | 614 | 614 | 614 | 614 |
| # treated | 8 | 8 | 8 | 8 | 8 |

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at LLM level in parentheses. Estimates are obtained using the codes distributed by Belloni et al., 2014, and Cerulli, 2014.

**Table 11: Robustness checks: Different sample composition, 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: EXCLUDING SURROUNDING MUNICIPALITIES FROM THE CONTROLS | | | | | | |
| policy | 0.014 | 0.001 | -0.003 | -0.001 | 0.001 | -0.005 | 0.006 |
| | (0.016) | (0.004) | (0.048) | (0.005) | (0.006) | (0.016) | (0.027) |
| Obs. | 525 | 525 | 525 | 525 | 525 | 525 | 525 |
| # treated | 26 | 26 | 26 | 26 | 26 | 26 | 26 |
| | PANEL B: INCLUDING CONTIGUOUS MUNICIPALITIES IN THE TREATED AREAS | | | | | | |
| policy | 0.020 | -0.002 | -0.054 | 0.005 | 0.002 | 0.007 | 0.019 |
| | (0.016) | (0.005) | (0.037) | (0.004) | (0.005) | (0.014) | (0.031) |
| Obs. | 632 | 632 | 632 | 632 | 632 | 632 | 632 |
| # treated | 133 | 133 | 133 | 133 | 133 | 133 | 133 |

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 12: Robustness checks: Estimation with LASSO + PS Reweighting**

| | (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 |
| policy | -0.003 | -0.000 | 0.019 | -0.008 | -0.000 | -0.008 | -0.030 |
| | (0.015) | (0.005) | (0.030) | (0.006) | (0.005) | (0.011) | (0.023) |
| Obs. | 632 | 632 | 632 | 632 | 632 | 632 | 632 |
| # treated | 26 | 26 | 26 | 26 | 26 | 26 | 26 |

Note: * p<0.1, ** p<0.05, *** p<0.01. Bootstrapped standard errors in parentheses. Estimates are obtained using the codes distributed by Belloni et al. (2014) and Cerulli (2014). The LASSO selected covariates are respectively: (a) Column 1: population 2001, slope, youth dependency index 2001, elderly dependency index 2001, average age of buildings 2001, vulnerability index 2001; (b) Column 2: population 2001, slope, average house size 1991 and 2001, employment in industrial sector 2001, short mobility 2001, vulnerability index 2001; (c) Column 3: population 2001, vulnerability index 2001; (d) Column 4: population 2001, employment in trade services 2001, vulnerability index 2001; (e) Column 5: population 2001, unemployment rate in 1991 and 2001, vulnerability index 2001; (f) Column 6: population 2001, youth dependency index 2001, elderly dependency index 2001, adults with diploma or university degree 2001, vulnerability index 2001; (g) Column 7: population 2001, vulnerability index 2001.

**Table 13: Robustness checks: Using future treated, PS Reweighting 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 |
| policy | 0.047* | 0.014 | 0.114 | -0.007 | -0.003 | -0.007 | -0.053 |
| | (0.025) | (0.015) | (0.141) | (0.092) | (0.008) | (0.039) | (0.129) |
| Obs. | 50 | 50 | 50 | 50 | 50 | 50 | 50 |
| # treated | 26 | 26 | 26 | 26 | 26 | 26 | 26 |

Note: * p<0.1, ** p<0.05, *** p<0.01. Bootstrapped standard errors in parentheses. Estimates are obtained using the code distributed by Cerulli (2014). The control sample includes only future treated. The PS covariates are those used in Table 12.

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