Urban renewal after the Berlin Wall: A place-based policy evaluation

Abstract: We use a quasi-experimental research design to study the effects of a spatially targeted renewal policy implemented in Berlin, Germany, in the aftermath of the city's division during the Cold War period. Our results suggest that over the course of 20 years the policy helped to reduce (increase) the propensity of buildings being in poor (good) condition within the targeted areas by, on average, 1.2–3% (0.6–2.5%) per year. The estimated effects on property prices range from 0.1–2% per year. In each case, the lower-bound estimate is not statistically significant. We find little evidence of positive housing externalities or positive welfare effects.

Keywords: Urban, renewal, revitalization, redevelopment, quasi-experiment, placed-based policy evaluation, real estate, Berlin

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1 Introduction

Evidence-based policy-making, that is, the idea that public policies must be based on rigorous and objective evidence, has rapidly gained popularity in recent decades. This type of policy-making obviously depends on the availability of careful empirical policy evaluations. The credibility of a policy evaluation, in turn, critically hinges on the inclusion of a valid counterfactual, i.e., the expected outcome in the absence of a policy, to which the policy outcome can be compared. Truly experimental
methodologies like randomized control trials, where randomly selected treated subjects can be followed over time and compared to similar non-treated subjects, are not feasible in many fields of policy evaluation. Researchers have responded to this limitation by applying quasi-experimental research designs to ex-post outcomes of existing policies which are, however, for good reasons typically implemented non-randomly. One policy area where the application of program evaluation techniques is severely complicated by the non-random nature of the analyzed policies is spatially targeted policies that aim at local economic growth. As place-based policies typically focus on areas that are deemed to be in need, according to some selection criteria, it is difficult to find comparison areas that are similar, but are not exposed to the policy in question. As a result it is often difficult to find compelling empirical evidence of the effects of place-based policies.¹

With this contribution we aim at providing evidence of a type of place-based policy where existing evidence is particularly scarce: urban renewal areas which are popular but empirically understudied spatial planning instruments designed to prevent urban decline and induce renewal.² Our objectives are twofold: Firstly, we aim at estimating the causal economic effect of a major renewal policy implemented in the aftermath of Berlin’s (German) unification. The first-order empirical question is whether the policy has sustainably increased the condition of buildings and the value of properties in the targeted areas. If so, the question that directly follows is whether there is an effect on the pure value of location – rather than the structures – because of a spatial externality so that the generated values exceed the public money spent. Secondly, we aim at informing the place-based policy evaluation literature more generally about the sensitivity of treatment estimates to distinct empirical design features that affect the counterfactual. We also provide a novel sensitivity analysis to evaluate how the validity of the estimated treatment effects depends on the number of subject and control areas included in the analysis.

There are numerous sizable programs targeting neighborhoods in need around the world. In the US the Community Development Block Grant (CDBG) provides between $3 and $10 billion each year to cities and local administrations to improve conditions in low-income urban areas (Brooks and Phillips, 2007). Another example is the Home Investment Partnership (HOME) program, which supports affordable housing with approximately $2bn per year. In Germany, the budget for various

¹ Kline & Moretti (2014b) provide an introduction to the general welfare economics of place-based policies along with a recent survey of the empirical literature.

² Saiz & Wachter (2011) provide a recent analysis of neighborhoods in decline.
urban development programs ("Städtebauförderung"), which are typically jointly financed by the federal government and the federal states, amounts to approximately €350 million ($453.1 million) to €500m ($647.3m) per year (Bundesinstitut für Bau, 2009).³

To date there are only a few rigorous empirical evaluations of revitalization policies that aim specifically at the improvement of the quality of existing housing stock. Rossi-Hansberg et al. (2010) [hereafter RH] investigate property prices in and around four renewal areas⁴ and one control area, which was initially considered but ultimately excluded from the program in Richmond, Virginia. RH find a large effect on the prices of non-renovated properties, from which they infer the presence of a sizable positive housing externality and a return of $2 to $6 per dollar invested. Other results are more ambiguous (Santiago et al., 2001) or seem to suggest that such policies have a positive effect only if they are sufficiently large (Ding et al., 2000) or spatially concentrated (Galster et al., 2006).

Berlin offers a unique institutional setting for an analysis of revitalization policies due to the 20th century history of the city. For several decades, the former capital of Germany suffered from either economic isolation (West Berlin) and loss of market access (Redding and Sturm, 2008) or transformation into a non-market economy (East Berlin), both of which severely affected the economy of the city. After reunification in 1990, the adverse economic performance was mirrored by a poor physical condition of the housing stock, especially in the eastern part (Senatsverwaltung für Stadtentwicklung Berlin, 1992, p. 16). In response to this situation, 22 renewal areas out of 39 originally proposed investigation areas ("Untersuchungsgebiete") were designated between 1993 and 1995 as target areas for a renewal program.⁵ By late 2010 (the period of the last official report on the renewal program), as much as €1.94 billion ($2.62 billion) had been spent on these areas. Our quasi-experimental research design compares property price trends within these 22 selected conservation areas over the period 1990 to 2012 to various counterfactuals. We consider the runner-up areas not selected for the program as a control group for comparison but also make use of other control groups that are close to the treated areas either in spatial or socio-economic terms.

We add to the aforementioned literature in a number of important respects. First, the program analyzed is by several orders of magnitude larger than previously analyzed programs. This is true

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³ Aggregate renewal financing data at the European level are not available.

⁴ Throughout this article we refer to the term renewal area, however, the terms redevelopment or revitalization area are often used interchangeably.

⁵ The First Berlin Renewal Program (Erstes Gesamtberliner Stadterneuerungsprogramm).
both in terms of its ambitions and in terms of the public spending involved, thus making it a particularly interesting study case. Second, the relatively large number of 22 control areas (the areas considered, but not selected for the program) help to establish a counterfactual that is less sensitive to unobserved shocks in particular areas compared to a setting where only a few, or as in the case of RH, only one control area is available. Third, this large number of potential control areas also allows us to evaluate the potential gains from being able to draw from a larger pool of control areas in the analysis of place-based policies more generally. Fourth, our data allow us to analyze not only the capitalization effects of property prices associated with the policy, but also the effect on the external condition of buildings in the targeted areas, the genuine focus of many renewal programs.

Previewing our findings, our results indicate that the policy led to a significant upgrade of the housing stock. Property prices in the targeted areas tended to increase at an above-average rate, although the evidence of a causal policy effect is somewhat weaker. Unlike RH, who find positive effects on non-renovated properties, we do not find strong evidence of the existence of housing externalities, i.e., multiplier effects of the policy. Our sensitivity analysis suggests that our estimated place-based policy effects become sensitive to unobserved local shocks if we use a small number of subject or control areas, even though we cannot claim that this finding necessarily generalizes to other settings.

In general terms we add to literature strands that have analyzed urban renewal processes (Ahlfeldt, 2011; Clay, 1979; Noonan, 2014) and housing externalities (e.g. Autor et al., 2014; Ellen et al., 2001; Helms, 2012; Ioannides, 2002; Koster and Van Ommeren, 2013; Rossi-Hansberg et al., 2010; Schwartz et al., 2006). We also contribute to a literature that has assessed the impact of various local public policies via capitalization effects (e.g. Ahlfeldt and Kavetsos, 2014; Cellini et al., 2010; Dachis et al., 2012; Dehring et al., 2008; Eriksen and Rosenthal, 2010; Gibbons and Machin, 2005; Oates, 1969) and the economic effects of spatially targeted policies more generally (Baum-Snow and Marion, 2009; Boarnet and Bogart, 1996; Briant et al., 2015; Busso et al., 2013; Freedman, 2012, 2014; Freedman and Owens, 2011; Gobillon et al., 2012; Ham et al., 2011; Kline, 2010; Kline and Moretti, 2013, 2014a; Murray, 1999; Neumark and Kolko, 2010; Sinai and Waldfogel, 2005).

Our analysis further connects to a more general research strand in urban economics that examines the amenity value of cities (e.g. Albouy, 2009, 2012; Blomquist et al., 1988; Gabriel and Rosenthal,

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Some of the 39 initial investigation areas were partially selected for the program, resulting in 22 self-contained zones that were treated as well as a further 22 zones that remained untreated.
2004; Gyourko and Tracy, 1991; Tabuchi and Yoshida, 2000) or neighborhoods within cities (e.g. Brueckner et al., 1999; Carlino and Coulson, 2004; Cheshire and Sheppard, 1995; Ioannides, 2003). This literature argues that there has been a re-orientation toward attractive central cities, especially among high-skilled young professionals, sometimes referred to as the creative class (Florida, 2002). The consumption value of cities has therefore become increasingly important for the attraction of a highly skilled labor force and, hence, the economic success of cities (Carlino and Saiz, 2008; Glaeser et al., 2001). Our findings inform this literature on whether revitalization policies and other neighborhood policies such as historic preservation may contribute to the development of targeted neighborhoods and thus promote gentrification.

Finally, our results also complement the analysis by Ahlfeldt et al. (2015), who estimate a general equilibrium model of simultaneous household and firm location using the exogenous variation that stems from the rise and fall of the Berlin Wall. Our results provide further evidence that the fundamental reorientation to the pre-World War II equilibrium the city experienced after the fall of the Berlin Wall is unlikely to be explained by the renewal policies and is likely attributable to economic agglomeration and dispersion forces.

2 Background

After World War II, the building stock in Berlin was fairly degenerated. Especially in the eastern part, which was part of the former German Democratic Republic (GDR), many buildings had not or had only been insufficiently renovated prior to the unification due to tight budget constraints. Additionally, private incentives to rebuild housing stock were low, as private real estate ownership was not encouraged in the GDR and rents had been frozen at a low level since 1945. These developments resulted in an overall poor condition of the building substance of original housing stock and inner city district centers, including massive vacancies and an increased need for renovation following unification in 1990.

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7 This study complements research examining the effects of spatial density on the productivity of workers and firms (e.g. Ahlfeldt et al., 2015; Ciccone, 2002; Ciccone and Hall, 1996; Glaeser et al., 1992; Glaeser and Mare, 2001; Rauch, 1993; Rosenthal and Strange, 2001)

8 Alternative determinants include transport affordability (LeRoy and Sonstelie, 1983), housing cycles (Brueckner and Rosenthal, 2009), housing demand shocks (Guerrieri et al., 2013) or natural amenities (Lee and Lin, 2012).
The main instrument to overcome these problems was the initiation of the First Berlin Renewal Program in July 1992, which identified 39 investigation areas (Untersuchungsgebiete) as areas in need (Problemgebiete) (Senatsverwaltung für Stadtentwicklung Berlin, 1992). The boundaries of these investigation areas were drawn to encompass areas of urban decline and deprivation and did not necessarily coincide with higher-level administrative units such as postal codes, voting precincts or census wards. Not surprisingly, the vast majority of these areas were located in the eastern part of Berlin (Maennig, 2012).

Because of funding constraints not all of the selected areas could eventually be designated. Qualitative reports were commissioned for each of the 39 investigation areas, followed by public hearings where residents, landlords, and other groups had the right to express their views. Officially, there was no formalized selection process based on a ranking of the investigation areas according to deprivation. Instead, the Senate of Berlin designated 22 renewal areas in 1993, 1994, and 1995 without further specifying the exact nature of the selection process. These areas purposely reflected the spatial distribution of the investigation areas. In some instances, renewal areas were split and only a fraction were designated to achieve this objective (Senatsverwaltung für Stadtentwicklung Berlin, 1992, 1995a). Briefly summarized, the official documents suggest that selection among the candidate areas was random with respect to deprivation levels. Our analysis of the selection process, discussed in more detail in section 3.1 and the appendix (section 3.2), however, suggests that it is possible to predict whether or not an area would be designated based on a number of explanatory variables, a feature that we will exploit in our identification strategy.

The 22 renewal areas covered an overall area of approximately 8.1 square kilometers, 5,723 plots, and approximately 81,500 dwelling units, with an average population of 5,000 residents per renewal area (Senatsverwaltung für Stadtentwicklung Berlin, 2001). The remaining parts of the 39 investigation areas formed 22 self-contained areas, which in terms of location (see Figure 1) and observable characteristics (see Table 1) closely resemble the designated renewal areas.

Within these renewal areas, private investments in the building stock were supported through tax reductions, loans, cash advances, and further financial support such as co-financing. After 2002 the focus was set on improvements in the social infrastructure and living quality of the neighborhoods.

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9 In Richmond, the object of the RH (2012) analysis, the four targeted areas had an average population of 1,900 residents and, on average, 1,000 housing units.
Private modernizations were no longer co-financed through public investments, but significant tax abatements remained as an implicit subsidy.\textsuperscript{10}

By late 2010, the expenses comprised about €1.94bn ($2.62bn) in public investments, amounting to approximately €880m ($1.19bn) for modernization and reinstatement, and approximately €645m ($873m) for expenses on infrastructure and social environment. The remaining disbursements consist of preparation costs (€77m/$104m), allowances (€123m/$166m), other regulatory measures including compensations (€143m/$193m), and other building measures (€63m/$85m).\textsuperscript{11} The average expenses are approximately €88m ($119m) per renewal area, translating into per capita expenses of €17,500 ($23,700) distributed over a period of some 15 years.\textsuperscript{12} This compares to per area payments of $3.5m and per capita expenses of $1,800 in Richmond over a period of four years. Currently, 19 of the 22 considered renewal areas have been released from their renewal status; Figure 1 in the data section shows the geographic locations of the renewal and investigation areas in Berlin.\textsuperscript{13}

3 Empirical strategy

3.1 Baseline specification

We use a combination of hedonic (Rosen, 1974) and difference-in-differences (DD) methods to estimate the causal effect of the renewal policy on the building condition and the sales prices of property transactions in the targeted areas. We estimate time-varying treatment effects using the following empirical specification:

\[
Y_{isnt} = \alpha_3T_i + f(T_i \times V_{st}) + \delta(T_i \times A_{st}) + X_{it}b + G_{it}c + \phi_t + \mu_n + \epsilon_{isnt}
\]  

\textsuperscript{10} Generally, modernization costs for own use or renting can be amortized completely over a runtime of 10 to 12 years. For a detailed account of the regulations, compare § 154 and 177 in the building law code (BauGB) and § 7h, 10f, and 11a of the income tax law code (EStG).

\textsuperscript{11} See Senatsverwaltung für Stadtentwicklung Berlin (2012), where the local administration provides detailed budget accounting information for the different time periods. To the best of our knowledge, more up-to-date figures are not yet available.

\textsuperscript{12} The total investment amounts to about 35% of the housing stock value. See section 2.3 in the technical appendix for further details.

\textsuperscript{13} See Table A1 in the technical appendix for details on designation date, district, and expiration of the renewal areas. An overview of the area is shown in Figure 1; a snapshot providing more detailed graphical information can be found in Figure A1 in the appendix.
where \( i \) indexes a property, \( s \) indexes the nearest renewal area, \( n \) indexes the housing block a property is located in and \( t \) indexes time. \( Y_{int} \) is one of the following variables: a 0,1 indicator variable, which is one if a property \( i \) at time of transaction \( t \) is in poor exterior condition and zero otherwise; a 0,1 indicator variable, which is one if a property at time \( t \) is in good exterior condition and zero otherwise; the log of the price at which a property \( i \) is sold at time \( t \); The central elements of this specification are an indicator variable \( T \), which denotes whether a property falls within one of the renewal areas we are investigating (\( T=1 \)) or into the control area (\( T=0 \)), and the function \( f(T_i \times V_{st}) \), which captures the interaction effect of being located within one of the renewal areas and the number of years this area has been designated (\( V_{st} \)). We discuss the employed functional forms later in the text after providing a description of the control variables and control groups used.

**Control variables**

For a number of renewal areas, we observe transactions after their release from designation status (\( A_{st} = 1 \)). We control for a potential capitalization effect, which might be related to the option value associated with the designation status via the interaction term \( T_i \times A_{st} \). We will not interpret this ancillary treatment effect \( \delta \) because usually there are few observations after an area has been released from designation status. \( X_i \) is a vector of property and locational characteristics discussed in the data section and \( b \) is the vector of the respective implicit prices. We control for otherwise unobserved time-invariant location characteristics via fixed effects \( \mu_n \) defined for statistical block x investigation area x renewal area cells.\(^{14}\) Standard errors are clustered at the same level and, thus, accommodate a spatial structure in a relatively flexible manner. Macroeconomic factors that are assumed to be invariant across the treatment and control groups are captured by year fixed effects \( \varphi_t \).

To allow for time-variant implicit prices for some time-invariant location characteristics we add \( G_{it} \), which is a vector of locational characteristics interacted with year effects. \( c_t \) is a matrix of year-specific implicit prices. Unlike in real experiments, assignment to treatment and control groups is unlikely to be entirely random in a policy experiment, no matter how carefully treatment and control groups are matched to each other. If some of the attributes in which the treated and non-treated

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\(^{14}\) A statistical block is the smallest geographic statistical unit in Berlin. There are close to 16,000 blocks in Berlin, of which around 6,000 cover undeveloped areas such as forests, parks, rivers or lakes. The average size of a statistical block is 0.05 square kilometers (0.02 square miles).
differ experience a change in valuation, this will affect the counterfactual. As an example, an auxiliary analysis reported in the appendix (section 3.6) reveals that over the course of our study period the premium of a property at the center of the city relative to a similar property 10km away increased by approximately 30% from 2004 to 2012. Properties in East Berlin, on average, appreciated by about 145% relative to properties in West Berlin since unification. These changes in the spatial structure of the city are a particular concern for our analysis because many designated renewal areas are located in central parts of former East Berlin.

The problem can be remedied to some extent by allowing the effects of the respective attributes to vary over time. Therefore, we interact year effects with the distance from the central business district, a kernel-smoothed density surface of bars, pubs, nightclubs, and hotels and a set of 23 city district dummies. We note that all the variables we interact with the year effects are time-invariant to avoid problems of circular causation.

Control groups

We use several definitions of control groups to establish the counterfactual. Control group I includes all observations outside the urban renewal areas and a surrounding 500m buffer. In control group II, we impose a geographical limit by considering transactions that lie within a 500 to 2,000 meter (approx. 6,000ft.) distance from the renewal areas. We exclude the 500m buffer area because of potential spillover effects. Recent evidence suggests that housing externalities and neighborhood externalities decline steeply in distance and typically lose most of their strength after a couple of hundred meters (Ahlfeldt et al., 2015; Rossi-Hansberg et al., 2010).

Control group III consists of investigation areas that were not transformed into renewal areas – similar to RH. Officially, all investigation areas qualified for designation, but the available funding dried up after the designation of about half of the investigation areas, leading to a more or less random designation (Senatsverwaltung für Stadtentwicklung Berlin, 1995b, 1997). This claim is supported by similar observable characteristics of properties in the selected renewal areas and the remaining investigation areas (Figure 1 and Table 1). Yet, it seems possible that the selection was guided by a needs assessment at least implicitly. In a complementary approach, we therefore proceed under the assumption that there is a latent variable that summarizes the degree of area deprivation and that limited funds resulted in a cut-off point beyond which no further designation was
viable.\textsuperscript{15} As a proxy for this latent variable we recover the predicted values from an auxiliary regression of designation status on a range of investigation area characteristics. We find the threshold value in this variable that best predicts designation and argue that around this threshold the selection would be as good as random even if an implicit ranking by need existed. To restrict the identifying variation to the fraction that is most plausibly exogenous we weight all observations according to their distance from the threshold using a Gaussian kernel.\textsuperscript{16} This approach incorporates some elements of the Regression Discontinuity Design literature (Basten and Betz, 2013; Dell, 2010; Lalive, 2008) into our DD setting. A detailed discussion of the latent variable, the identified cut-off value, and the distribution of kernel weights is in the appendix (section 3.2).

As a further alternative, control group IV is created based on the propensity score matching procedure proposed by Rosenbaum and Rubin (1983). In particular, we match transactions inside and outside renewal areas based on the propensity score; a likelihood of being selected for the treatment based on observable characteristics. If transactions that are similar in observable characteristics are also similar in unobservable characteristics, the resulting control groups will produce a valid counterfactual for the treated. In the estimation of the propensity score, we choose covariates that influence both participation in the treatment and the outcome variable. Only locational variables that are measured prior to the treatment or are time-invariant are considered (Caliendo and Kopeinig, 2008). These covariates include a range of internal property and external location characteristics and are discussed in greater detail in the technical appendix (section 2.4), where we also present some descriptive statistics for the resulting sample.

\textit{Treatment functions}

We define two versions of the time-varying treatment function \( f(T_i \times V_{st}) \). The first is a relatively restrictive parametric variant.

\[
 f(T_i \times V_{st}) = \beta_0 T_i \times POST_{st} + \beta_1 T_i \times V_{st} \times POST_{st}, \tag{2}
\]

\textsuperscript{15} An anonymous referee is acknowledged for pointing us in this direction.

\textsuperscript{16} We weight transactions using the following area-specific weight:

\[
 w_s = \frac{1}{\lambda \sqrt{2\pi}} \exp \left( -\frac{1}{2} \left( \frac{s - s^*}{\lambda} \right)^2 \right), \text{ where } \lambda \text{ is selected according to Silverman (1986).} \]
where $V_{st}$ is the number of years the nearest renewal area has been designated. It takes a value of zero in the year before designation, negative values prior to that, and positive values from the moment an area has been designated. $POST$ is a 0,1 dummy variable denoting designation ($V_{st} > 0$). The year-specific treatment effects are defined as $\beta_0 + \beta_1 V_{st}$. By allowing for a level and a trend shift following designation this specification shares similarities with the baseline econometric specification in Galster et al. (2006). We prefer this specification to an alternative that would extrapolate a pre-trend to establish the counterfactual because our pre-periods (before designation) are relatively short compared to the post-periods (after designation).

The second approach follows Ahlfeldt and Kavetsos (2014) and is more flexible. We group the treated observations into cohorts depending on $V_{st}$. For each cohort, we then define an indicator variable $VD_{ust}$ describing whether transactions fall into the cohort, e.g., $VD_{1st}=1$ for all observations transacted within one year after designation of the respective renewal area ($V_{st} = 1$), $VD_{0st}=1$ for the year before designation, etc. Interacting all cohort indicator variables with the treatment indicator $T$, we estimate a series of difference-in-differences treatment effects that compare how prices have changed since designation in the treatment and control groups:

$$f(T_i \times V_{st}) = \sum_{u \neq 0} \beta_u (T_i \times V_{Dust})$$  \hspace{1cm} (3)

The estimated $\hat{\beta}_u$ coefficients hence form a mix-adjusted hedonic price index that flexibly reflects the evolution of the treatment group relative to the control group before and after the treatment.

These two treatment functions have distinct strengths. The former allows for a straightforward assessment of whether the policy had a significant impact on levels or trends based on only two coefficients that can be estimated with relatively small standard errors. The latter approach produces a more flexible time-varying index, but larger confidence bands due to the relatively smaller number of observations per $VD_{ust}$ cohort.

Robustness checks and extension

\[\text{17} \] This alternative specification would take the following form:

$$f(T_i \times V_{st}) = \beta_0 T_i \times V_{st} + \beta_1 T_i \times POST_{st} + \beta_2 T_i \times V_{st} \times POST_{st}.$$  

In robustness checks reported in the appendix we also use the following treatment function:

$$f(T_i \times V_{st}) = \beta_0 T_i \times V_{st} \times (1 - POST_{st}) + \beta_1 T_i \times POST_{st} + \beta_2 T_i \times V_{st} \times POST_{st}.$$  

In this model $\beta_0$ captures the trend in the treatment area relative to the control area before designation, which serves as a falsification test to evaluate the common trend assumption underlying the DD.
We subject our results to a battery of robustness tests. We consider different clustering levels for the standard errors as well as non-parametric heteroscedasticity-autocorrelation consistent standard errors (Conley, 1999), experiment with the sets of variables included in $G_{it}$, address concerns regarding the endogeneity of the amenity variable in $G_{it}$, discuss potential sample selection problems, analyze policy effects on transaction volumes and land values, test for pre-trends within the treated renewal areas as well as placebo treatment effects on the non-treated investigation areas, and experiment with various Gaussian or binary weights when using control group III. We also conduct several empirical exercises to detect potential housing externalities, i.e., increases in housing values due to the renovation of nearby buildings.

3.2 Data

Our study area comprises the area of the Federal State of Berlin, Germany. Within this study area, we observe all transactions of built-up land (including a structure) that took place between January 1990 and August 2012, which amounts to approximately 70,000 transactions. The data set includes price, transaction date, location, and a set of parameters describing building/plot characteristics. The data were obtained from the Committee of Valuation Experts Berlin 2012 (Gutachterausschuss Berlin). The transactions are geo-referenced (addresses and x/y coordinates), which allows them to be integrated into a geographical information system (GIS) environment. The building characteristics include floor space, plot area, surface area, age (we add an age-squared term), land use, location within a block of houses (e.g., a corner lot), among other variables. A special feature of our property data set is some explicit information on maintenance condition. The variables are coded by specialist teams of the Committee of Valuation Experts Berlin that undertake on-site examinations for each transaction of built-up land that takes place. The building quality is recorded as either poor, good or average. We use this information to create a 0,1 indicator variable which indexes properties in poor and good exterior condition.

Additionally, we merge a set of location variables generated in GIS. These include the distance of the transactions to the nearest public transport station, school, public park, lake or river, the central business district, the nearest listed building, and the nearest main street, and the street noise level. To control for time-varying implicit prices of proximity to consumption amenities, we generate a kernel-smoothed density surface based on the 2012 location of bars, coffee shops, restaurants, nightclubs, and hostels. We use a kernel radius of 2,000 meters and a quadratic kernel function (Silverman, 1986). The data are obtained from the open street map project, where users submit
data to generate a publicly accessible street map.\textsuperscript{18} While these data are user-generated and thus not official, they should provide a reasonable approximation of the actual distribution as long as the reporting probability does not vary systematically across space. The full list of considered variables is provided in Table A3 in the web-based appendix.

From the Berlin Senate Department, we obtained maps showing the exact locations and boundaries of the 39 initial investigation areas as well as the fractions that were subsequently designated in three waves in 1993, 1994, and 1995. Of the originally proposed 39 investigation areas, 17 remained entirely unconsidered in the eventual selection. From the remaining 22 areas a total of 69\% of the land area entered the program. The fragmentation of some of the 39 initial investigation areas results in 22 self-contained zones that were treated as well as another 22 zones that remained untreated. We have digitally processed the maps and converted them to a shape file to merge the information with the other spatial data in GIS. The 22 renewal areas have a mean size of approximately 0.37 square kilometers (median 0.35). The investigation areas have an average area of 0.43 square kilometers (median 0.36).

Figure 1 shows the spatial distribution of the renewal/investigation areas along with our estimated smoothed kernel density surface and our matched control group (control group IV). Renewal areas and investigation areas are typically located in central areas and in amenity clusters in the eastern part of the city. Our matched control group (red dots) consists of transactions that are either close to renewal or investigation areas or are in areas of high amenity densities, which lends some confidence to the selection process.

Table 1 compares key characteristics across the renewal areas and the various control groups. We report differences in means between the treated group and each control group normalized by standard deviation of the treated group (standardized bias) in brackets. Clearly, transactions in investigation areas and matched transactions are more similar to transactions within renewal areas than an average transaction in Berlin.\textsuperscript{19} The housing stock is much older than in the rest of Berlin, and the floor space index, which measures the density of development, is higher. The reason is, in part, that single-family houses are rare in the centrally located renewal and investigation areas, while they are naturally abundant in the peripheral parts of the rest of the city. Renewal areas and the considered control areas are relatively homogeneous areas dominated by buildings constructed

\textsuperscript{18} Data are available at www.openstreetmap.org.

\textsuperscript{19} Table A1 in the web-based appendix lists the renewal areas and some stylized facts per area.
around the turn of the 19th and 20th centuries (the so-called founding period/”Gründerzeit”). These are primarily apartment blocks, often with some commercial units on the ground floor.

– Figure 1 about here –

– Table 1 about here –

4 Empirical results

4.1 Policy effects on building condition

We begin by analyzing the effect of the policy on the propensity that a transacted property is in poor physical condition. Table 2 presents parametric estimates of equation 1 by varying control groups using an indicator variable that takes a value of one if the external building condition is poor and zero otherwise (average or good condition) as the dependent variable. To keep the presentation compact, only the coefficients of primary interest are reported. In all models the short-run effect reflected by the coefficient on $TxPOST (\beta_0)$ turns out to be small compared to the long-run effect implied by the coefficient on $TxV (\beta_1)$ multiplied by 20 years. This is the expected result as it takes time for applications to be filed and renovation works to be completed. Column (1) results imply that compared to the rest of Berlin the propensity (on a 0,1 interval) of a building being in poor condition at the time of transaction in a renewal area changed by $(\beta_0 = -0.030 + (\beta_1 = 0.02 \times 20 = -0.43)$. Normalization by the initial share of buildings in poor condition in renewal areas of 52.11% results in a cumulated (percentage) effect after 20 years of $-0.43/52.11\% = -82.37\%$ or a compound annual growth rate of $(1 + 82.37\%)^{1/20} - 1 = -8.3\%$.

– Table 2 about here –

As we increase the strength of the counterfactual using spatially proximate properties (2), the investigation areas (3), the same with observations weighted by distance from the designation cut-off (4), or the matched properties (5) as a control group, the cumulative effect drops significantly, but remains relatively large with at least -39.88% (5). Restricting the identifying variation to areas near the designation cut-off hardly affects the estimated effect, which is not in line with a selection problem. Combining our preferred control group, the investigation areas (group III) or the matched control group (group IV) with the time-varying effects (6 and 7) consistently results in substantially lower (approx. $-21.5\%$) and insignificant estimated cumulative effects. One interpretation of this reduction in the treatment effect is that the renewal effect is to a significant extent driven by the
favorable location of renewal areas. In other words, to a large extent renovations would likely have happened even in the absence of the policy. Another interpretation is that the time-varying effects are absorbing some variation that is genuinely attributable to the policy. This would be the case if the increase in attractiveness of central amenity locations was the result of the policy instead of other factors. 20 To this extent, the results in columns (6) and (7) represent lower-bound estimates of the policy impact and those in columns (3) and (5) indicate upper bounds. We view model (4) primarily as a means to evaluate whether the results in column (3) are driven by a selection problem. Throughout the paper we prefer to highlight the range of estimates across the models reported in columns (3–5) and (6–7) rather than an individual point estimate. Accordingly, we conclude that the policy effect on the propensity of a building being in poor condition is between an insignificant -1.2% and a significant -3% per year.

Table 3 replicates Table 2, replacing the dependent variable with a 0,1 dummy variable that takes the value of one if a transacted building is in good condition and zero otherwise (average or poor condition). The interpretation of all results is thus analogous to Table 2. We find smaller point estimates throughout, but these need to be interpreted in light of the smaller share of the 17.79% of transacted buildings in good condition within renewal areas before designation. The increase in the cumulated effect in the weighted model (4) relative to the unweighted model (3) is, once again, not in line with an upward bias due to selection. The resulting cumulative percentage effects on the propensity of buildings being in good condition are in absolute and relative terms similar to the effects on the propensity of buildings being in poor condition, though pointing in the opposite direction, as expected. The policy effect on the propensity of buildings being in good condition ranges between an insignificant 0.6% and a significant 2.5% per year.

In Figure 2 we present the semi-non-parametric estimates of the temporal treatment function defined in equation (3). The results are qualitatively and quantitatively in line with the parametric estimates and even the lower-bound estimates are statistically significant at least in a number of consecutive years. Importantly, the relative trends we find during the years after designation do not represent continuations of pre-existing trends. If anything, Figure 2 points to a trend reversion around the time when the policy started.

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20 This problem is a variant of the “bad control problem” (Angrist and Pischke, 2009).
Overall, we conclude that evidence of a positive policy effect on the quality of the building stock is relatively strong.

4.2 Policy effects on property prices

We now turn our attention to the extent to which the increase in the quality of the building stock capitalizes into the market value of properties in the renewal areas. To estimate the capitalization effect of the policy we once more estimate equation (1), now using the log of property transaction prices as the dependent variable. The presentation of results in Table 4 and the order of the models is analogous to tables 2 and 3. For the sake of brevity, we again focus on the treatment estimates of primary interest. The complete estimates of the structural and location parameters are in line with the typical findings in similar studies and are reported in Table A4 in the web-based appendix. Given the logarithmic scale of the variable the cumulated percentage renewal policy effect for any given year since designation can be computed as \( \exp(\beta_0 + \beta_1 V_{it}) - 1 \).\(^{21}\)

Model (1) compares the evolution of property prices within the renewal areas to the rest of Berlin, our most general control group I. The results suggest that a positive long-run trend (approximately 4.9% per year) dominates a negative intercept (-15%). After \( V=20 \) years, sales prices in designated renewal areas, on average, have since appreciated by as much as 120.4% relative to the rest of the city. This corresponds to an average yearly appreciation rate of approximately \( (1 + 120.4\%)^{1/20} - 1 = 4.03\% \). As we increase the strength of the counterfactual using spatially proximate properties (2), the investigation areas (3), the same weighted by distance from the designation cut-off (4) or the matched properties (5) as a control group, the cumulative effect (average appreciation rate) drops to 95.25% (3.4%), 48.96%, 50.82 (2.08%), and 43.31% (1.82%), respectively. Weighting areas, once more, hardly affects the estimates.

The inclusion of time-varying effects in models (6) and (7) has a strong impact on the estimated policy capitalization effect. The cumulative effect is reduced to an insignificant 1.08% (9.36%) in

---

\(^{21}\) We make use of the conventional interpretation of dummy variables in semi-log models (Halvorsen and Palmquist, 1980).
model 6 (7). Following the same logic as discussed in the previous section the annualized percentage policy effect on property prices ranges from an insignificant 0.1% to a significant 2% in our preferred models.

--- Table 4 about here ---

Figure 3 illustrates our semi-non-parametric estimates of the temporal treatment function defined in equation (3). We present estimates excluding (light dashed lines) and including (dark solid lines) time-varying effects using all properties outside the renewal areas (left graphs) and properties in investigation areas (right graphs) as a control group. The semi-non-parametric estimates are generally in line with the parametric counterparts presented in Table 4. The cumulative effect on all properties inside the renewal areas relative to those outside the renewal areas is slightly larger than implied by the parametric estimates (left), but declines to approximately 50% when the trend is benchmarked against the investigation areas (right). The positive trend effects seem to capitalize with some delay (beginning after five years). The negative level shifts found in Table 4 thus appear to be primarily driven by parametric constraints and should not necessarily be taken as indicative of a significant decline in prices immediately following designation. We note that the cumulative effect after 20 years in the models with time-varying effects is within the same range as model (6) in Table 4 and is not statistically significant. We find little evidence of the existence of relative trends prior to the intervention taking place. The (placebo) treatment effects for the initial year ($u=-4$) tend to be large and positive and again, if anything, imply a trend reversion.

--- Figure 3 about here ---

It seems important to note that the inclusion of time-varying effects has an even stronger effect on the estimated capitalization effects than on the estimated policy effects on the quality of the building stock, in particular in the models controlling for time-varying effects. The case for a positive capitalization effect is, therefore, arguably weaker. Furthermore, our range of capitalization effects, from an insignificant 0.1% to a significant 2%, is substantially lower than the 2% to 5% (both significant) range reported by RH. Combining our estimated policy capitalization effects with the average property value and the number of properties in the renewal areas, back-of-the-envelope calculations suggest that the total property value increased by €0.03–€1.37 for each Euro spent on the program. Notably, the lower bound is not only economically small, but is also based on an estimate effect that is statistically not distinguishable from zero (see the appendix, section 2.3 for details). This is, again, significantly lower than the 2–6 multiplier range reported by RH.
4.3 Robustness and extensions

In this section we summarize the results of a number of alterations to the models reported here that are discussed in more detail in the appendix. First, we compute standard errors clustering on a wider neighborhood level and account for spatial autocorrelation, serial correlation, and heteroscedasticity, following Conley (1999) and using various cut-off distances (see appendix, section 3.7). Second, we check parametrically that no significantly differing trends in house prices and building stock quality existed in the treatment and control areas prior to the policy being introduced (see appendix, section 3.4). Third, we test for the possibility that the designation of renewal areas represented a negative signal to the remaining investigation areas, which could invalidate the counterfactual provided by control group 3 (see appendix, section 3.8). Fourth, we replace the contemporary amenity density with an analogically constructed variant that uses bars and restaurants as reported in the 1995/96 edition of the yellow pages (Gelbe Seiten), which should predate the impact of the designation of renewal areas (see appendix, section 3.9). Fifth, we experiment with Gaussian and binary kernels of varying bandwidths in the models using control group III (section 3.2). The results support the interpretations and conclusions presented in this document.

In a series of further alterations we replicate our preferred capitalization models, allowing for fewer time-varying controls, to address the concern that these absorb variation that is (partially) attributable to the policy. We find that even with more conservative controls for correlated trends the estimated treatment effects frequently tend to be close to our lower-bound estimates, which further indicates that the policy’s impact on property prices was limited (see appendix, section 3.5). In a further set of auxiliary regressions we find that the number of transactions in relative terms tends to decline in renewal areas. The marginal quality of transacted properties in renewal areas might be increasing, implying a section effect that would increase the estimated policy effect and, once more, suggesting that the effects of the policy are likely moderate. In line with this interpretation we find lower treatment effects on assessed land values, which are supposed to be independent of structural housing quality (see appendix, section 3.3).

We also conduct several empirical exercises to detect potential housing externalities, that is, increases in housing values due to the renovation of nearby buildings. To separate the effect of the (subsidized) renovation of buildings on their own value from the effects of increased nearby renovation activity within renewal areas we restrict the sample exclusively to buildings that were in
good condition at the time of transaction. Keeping the internal housing quality constant we interpret the treatment effect as reflective of externality effects. Unlike in the baseline models we find cumulated treatment effects near to and not statistically distinguishable from zero when the comparison is made to the rest of Berlin or to the 0.5–2km buffer area. Using investigation areas and matched transactions as control groups we find somewhat larger, but qualitatively inconsistent and insignificant treatment effects (see Table A17 in the appendix).

In an alternative approach we focus on spillover effects onto property prices in a 0–0.5 km buffer area around the renewal areas. Those areas were not exposed to the policy but would benefit from nearby improvements if housing externalities played a significant role. Using the naïve control groups comparing trends within the spillover areas to the rest of Berlin (excluding renewal areas) or the 0.5–2km buffer, the treatment effects are reduced by about two-thirds compared to the baseline models. In the most demanding models, which use investigation areas or matched observations as control groups and control for time-varying effects, the treatment effects are insignificant and qualitatively inconsistent. The models using the same control groups, but excluding time-varying effects, stand out in the sense that they yield treatment estimates for the spillover areas that are very similar to the corresponding estimates for the renewal areas. This result runs counter to expectations in that even in the presence of housing externalities the typically relatively steep spatial decay in the externality (see Rossi-Hansberg et al., 2010) should imply a significantly smaller treatment effect within spillover areas than within renewal areas. It seems therefore likely that our upper-bound treatment effects are at least partially driven by some heterogeneity in counterfactual trends that is not accounted for in the models excluding time-varying effects. This result further adds to the notion that our lower bound might be the more credible estimate of the policy effect (see Table A18 in the appendix).

Overall, our additional robustness checks, using a different type of variation than in the baseline models, support the view that the evidence for housing externalities induced by the renewal program is weak at best.

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22 Compared to a restriction to buildings in poor (or normal) condition, our choice has the advantage that tax abatements or renovation subsidies are less likely capitalized into the sales prices of properties in good condition since these are unlikely to be renovated in the near future.
5 Sensitivity analysis

Compared to RH, the somewhat puzzling result of our analysis is that we find smaller effects of the policy on property prices and weaker evidence of housing externalities, despite analyzing a significantly larger program. Naturally, the question arises as to which factors may account for the unexpected pattern of results. In terms of the institutional setting, it is notable that the share of owner-occupancy in Berlin is very low, which may complicate coordination and reduce housing externalities. Naturally, the question arises as to which factors may account for the unexpected pattern of results. In terms of the empirical setting, a notable difference is that in Berlin both the number of areas that were included (22 vs. 4) in the program and the number of areas that remained excluded (22 vs. 1) are significantly larger than in Richmond, reflecting the larger size of the program. Successful identification in difference-in-differences analyses rests on the assumption that the treated and control areas are subject to the same macro-economic shocks. The relatively large number of treatment and control areas available in Berlin arguably helps with the identification because idiosyncratic year-area specific shocks are more likely to cancel each other out within larger groups of treated or control areas.

To evaluate the sensitivity of the identified treatment effect to the number of treated or control areas considered, we replicate our benchmark model using various combinations of 1, 2, 5, 10, 15, 20, or all treatment or control areas. For each combination considered, we run 2,500 iterations with randomly selected areas (unless the total number of combinations is exhausted at a lower number, in which case we simply run all combinations). Assuming that the estimate in Table 4, column 3, reflects the true causal policy effect, the distribution of point estimates gives an indication of how likely it is that the policy evaluation would have yielded a biased result should fewer treatment or control areas have been available.

Table 5 summarizes the distributions of the cumulated treatment effects by varying the number of randomly selected treatment and control areas. As expected, the means of the distributions of point estimates tend to be close to our baseline result of $-0.04 + 0.022 \times 20 = 0.40$ log-points in Table 4, column (3), especially in the experiments where we alter the number of control areas. The variation in point estimates is large, however. The standard deviation exceeds the mean of the point estimate if five or less treatment areas are sampled. Less than 40% of the point estimates fall within a two standard error length of our benchmark result. With only one control area sampled the point

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23 In central city districts where most of the renewal areas are situated, the owner-occupancy share is frequently below 10% (IBB, 2008).
estimates are within the same window in less than 15% of the cases. The variation decreases rapidly as the number of control areas increases. With five sampled control areas the standard deviation is already less than half of the mean and the share of point estimates within two standard error lengths of the benchmark result increases to close to 60%. A similar pattern emerges if both treatment and control areas are sampled at the same time.

Figure 4 displays a selection of distributions summarized in Table 5. With only one randomly selected treatment (control) area compared to all control (treatment) areas, there is little clustering of the point estimates, indicating a significant degree of area-specific shocks and/or heterogeneity for the policy effect across the treated areas (upper-left). With two randomly drawn treatment or control areas, the distribution of the probability of obtaining a point estimate near to the average treatment effect significantly increases even though only a small proportion of the estimates falls within two standard error lengths of the benchmark estimate (upper-right). With five or more treatment or control groups there is a relatively well-behaved probability distribution centered around the average treatment effect (middle panels).

When treatment and control areas are randomly drawn simultaneously, the probability distributions start to exhibit a reasonable shape once at least five treatment and control areas are considered (bottom-left), although the results still show a remarkable degree of variation across the iterations. The variation decreases substantially as the number of treatment and control areas is increased. With 15 treatment and control areas, the mean of the point estimates is very close to the benchmark model (using all 22 treatment and 22 control areas).

As we cannot draw large numbers of treatment and/or control areas independently it is not surprising that the variation across point estimates generally declines in the number of areas considered. Yet, the degree of variability in the treatment estimates across the series where relatively few treatment or control areas are used is an interesting finding in its own right. It seems important to acknowledge that the inference of causal policy effects in similar settings is particularly challenging. We note that we find a very similar pattern of results when replicating the sensitivity analysis using the expanded model including time-varying effects (Table 4, column 6). The results are presented in the appendix (section 3.10).

– Table 5 about here –

– Figure 4 about here –
6 Conclusion

Given the expectations that have motivated the renewal program in question and other similar programs, our results are simultaneously encouraging and disillusioning.

On the positive side, our results indicate that the policy led to increased renovation work and improved the maintenance condition of buildings in the target areas. In the course of 20 years the policy helped to reduce (increase) the propensity of buildings being in poor (good) condition within the targeted areas by on average 1.2–3% (0.6–2.5%) per year. This improvement in the stock of buildings has been accompanied by an increase in property prices in the range of 0.1–2% per year. In each case the lower bound is not statistically distinguishable from zero. Considering the full range of estimates, there is somewhat stronger evidence of a positive policy effect on building condition than on property prices.

On the negative side, our results do not point to the self-reinforcing effect operating through housing externalities for which one may have hoped. The increase in property value seems largely attributable to the upgrade of internal quality. Back-of-the-envelope calculations suggest that total property value increased by €0.06–€1.35 for each Euro spent on the program. The lower bound is not only economically small but is also statistically not distinguishable from zero. While the policy seemed to have sped up the renovation of significant fractions of the urban fabric and, as such, helped to eliminate the visible traces of the division period, it has also primarily been a cash transfer to those landlords participating in the program.

Our results look less favorable than those previously presented by RH, who find positive and large effects on property values in four renewal areas that exceed the investments by a factor of two to six and significant spillovers into adjacent areas. This is a surprising result given that the Berlin renewal program in terms of public investment was substantially larger than the Neighborhoods in Bloom program in Richmond, Virginia, analyzed by RH.

There are some institutional factors that may account for the large discrepancy in the findings of Richmond and Berlin. For one thing, the Richmond program was based more on community volunteering and local non-profit organizations, while Berlin adopted a top-down approach implemented by official state authorities. For another thing, and perhaps more important, German cities, and especially Berlin, are not directly comparable to a US city like Richmond. In Berlin, much of the downtown housing stock is owned by landlords and occupied by renters. Absentee landlords, however, are often argued to spend less on maintenance than owner-occupiers (Galster, 1983). Similarly,
owner-occupiers have been demonstrated to invest more in social capital (DiPasquale and Glaeser, 1999; Hilber, 2010) and tend to use neighborhood policies as a framework to coordinate their behavior to internalize externalities (Holman and Ahlfeldt, 2014), as such, they may also be more receptive to renovation subsidies. A within-neighborhood contagion effect (Towe and Lawley, 2013) in renovation activity is, thus, less likely in Berlin.

Another notable difference to RH is that, as collateral of the size of the program, we are able to establish a counterfactual based on a large number of areas which were initially considered but eventually not selected for the program. Due the smaller size of the Neighborhood in Bloom program, RH rely on a singular neighborhood that was similarly considered but eventually not selected for treatment. The results of our sensitivity analysis indicate that some care is warranted when interpreting the results of quasi-experimental place-based policy evaluations based on small numbers of treatment or control areas.

Overall, our results are in line with some previous analyses that have found moderate and ambiguous effects of similar renewal policies (Ding et al., 2000; Santiago et al., 2001), suggesting that the very positive policy effect found by RH are likely specific to the case of Richmond, Virginia. We conclude that spatially targeted renewal area policies may well have a positive impact on the built environment, but it is not clear that they are necessarily welfare-enhancing.

**Literature**


Figures

Fig. 1. Study area

Notes: Own illustration based on the urban and environmental information system (Senatsverwaltung für Stadtentwicklung Berlin, 2006). Crosshatched (hatched) areas indicate renewal (investigation) areas. Crosses are the matched transactions in control group IV. Smoothly shaded areas represent the consumption amenity density.
Fig. 2. **Renewal effect on propensity of transacted buildings being in poor or good condition**

![Graph showing renewal effect on propensity](image)

Notes: Figure illustrates the renewal treatment effect $\beta_u$ as defined in equation 3. Estimates are based on equation (1), including (light dashed lines) or excluding (dark solid lines) time-varying treatment effects $G_{it}$ and using a 0,1 indicator variable denoting buildings in poor (left panel) or good (right panel) condition as the dependent variable. The control group are the non-designated investigation areas in all models. The parametric equivalents are in Table 2, column 3 and 6 (left) and Table 3, column 3 and 6 (right). Error bars indicate the 95% confidence intervals.
Fig. 3. Renewal effect on property transaction prices

Notes: Figure illustrates the renewal treatment effect $\beta_u$ as defined in equation 3. Estimates are based on equation (1), including (light dashed lines) or excluding (dark solid lines) time-varying treatment effects $G_{it}$ and using the log of property price as the dependent variable. The parametric equivalents are in Table 4, column 1 (solid left), column 3 (solid right), and, column 6 (dashed-right). The parametric equivalent to the light dashed line in the left panel is not reported in Table 4 to save space. Error bars indicate the 95% confidence intervals.
Fig. 4.  Varying numbers of areas: Distribution of point estimates

Separately varying number of treatment (black) & control areas (gray)

1 random area selected (1 treated vs 22 control)  
2 random areas selected (2 treated vs 22 control)

5 random areas selected (5 treated vs 22 control)  
15 random areas selected (15 treated vs 22 control)

Simultaneously varying number of treatment & control areas

5 random areas selected (5 treated vs 5 control)  
15 random areas selected (15 treated vs 15 control)

Notes: Figure shows distributions of point estimates in series of estimations of the baseline model (Table 4, column 3) with randomly drawn treatment and/or control areas. In the upper two rows, black (gray) solid lines depict the kernel density of cumulated effects when varying the number of renewal (investigation) areas and comparing them to all investigation (treatment) areas. The black vertical lines depict the cumulated effect of our benchmark model (solid) plus/minus two standard error lengths (dashed).
### Tables

**Tab. 1. Comparative statistics**

<table>
<thead>
<tr>
<th></th>
<th>Renewal areas</th>
<th>Invest. areas</th>
<th>Matched obs.</th>
<th>0.5-2k m buffer</th>
<th>Berlin (total)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(Treated)</td>
<td>(Control group III)</td>
<td>(Control group IV)</td>
<td>(Control group II)</td>
<td>All transactions</td>
</tr>
<tr>
<td><strong>Price [€, CPI adjusted]</strong></td>
<td>1,166,478.7 (1,614,568)</td>
<td>1,320,897.2 (1,553,772.5)</td>
<td>1,513,634.6 (1,959,344.1)</td>
<td>1,317,781.4 (2,763,671.4)</td>
<td>994,908.1 (2,711,511.8)</td>
</tr>
<tr>
<td><strong>Building age</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100.8</td>
<td>95.29</td>
<td>92.57</td>
<td>84.98</td>
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</tr>
<tr>
<td>(21.9)</td>
<td>(25.77)</td>
<td>(27.33)</td>
<td>(32.58)</td>
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<td></td>
<td></td>
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</tr>
<tr>
<td><strong>Condition good [%]</strong></td>
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<td>8.24</td>
<td>11</td>
<td>13.6</td>
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<td></td>
<td>(304)</td>
<td></td>
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<td></td>
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<tr>
<td><strong>Condition bad [%]</strong></td>
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<td>28.2</td>
<td>34</td>
<td>25.4</td>
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<td>(49.4)</td>
<td></td>
<td></td>
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<td><strong>Floor space index</strong></td>
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<td>2.707</td>
<td>2.063</td>
<td>2.153</td>
<td>1.214</td>
</tr>
<tr>
<td>(floor space / lot size)</td>
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<td>(1.238)</td>
<td>(1.303)</td>
<td>(1.341)</td>
<td>(1.292)</td>
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<td></td>
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<tr>
<td><strong>Lot size</strong></td>
<td>863.7 (923.8)</td>
<td>919.4 (978.8)</td>
<td>1312.1 (2941.6)</td>
<td>1077.9 (2325.6)</td>
<td>1040.1 (2746.7)</td>
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<td><strong>Share of non-German population [%]</strong></td>
<td>13.7</td>
<td>20.6</td>
<td>11.5</td>
<td>16.6</td>
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<td></td>
<td>(7.21)</td>
<td>(15.1)</td>
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<td><strong>Single family home [%]</strong></td>
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<td></td>
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<td>(14.6)</td>
<td>(36.8)</td>
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<td>(49.1)</td>
<td>(46.5)</td>
<td>(46.8)</td>
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<td><strong>Mixed use building [%]</strong></td>
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<td>43.8</td>
<td>39.8</td>
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<td></td>
<td>(49.2)</td>
<td>(50)</td>
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<tr>
<td><strong>Commercial use building [%]</strong></td>
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<td>1.76</td>
<td>5.94</td>
<td>3.32</td>
<td>1.65</td>
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<td></td>
<td>(16.5)</td>
<td>(13.2)</td>
<td>(23.6)</td>
<td>(17.9)</td>
<td>(12.7)</td>
</tr>
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</table>

Notes: Prices are in 2012 Euros. Standard deviations in parentheses. The percentage standardized bias [in brackets] is the difference between the means of the treated group and a control group normalized by the standard deviation of the treated group.
### Tab. 2. Policy effects on propensity of buildings being in poor condition

<table>
<thead>
<tr>
<th>Control group</th>
<th>(1) 0.5 km -∞ buffer</th>
<th>(2) 0.5 - 2 km buffer (I)</th>
<th>(3) Investigation areas (II)</th>
<th>(4) Investigation areas (III)</th>
<th>(5) Matched observations (IV)</th>
<th>(6) Investigation areas (V)</th>
<th>(7) Matched observations (VI)</th>
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</thead>
<tbody>
<tr>
<td><strong>T x POST (β₀)</strong></td>
<td>-0.030 (0.031)</td>
<td>-0.017 (0.032)</td>
<td>0.006 (0.036)</td>
<td>-0.015 (0.049)</td>
<td>0.031 (0.046)</td>
<td>0.053 (0.050)</td>
<td>-0.025 (0.056)</td>
</tr>
<tr>
<td><strong>T x V (β₁)</strong></td>
<td>-0.020*** (0.002)</td>
<td>-0.016*** (0.002)</td>
<td>-0.012*** (0.003)</td>
<td>-0.011*** (0.004)</td>
<td>-0.012*** (0.004)</td>
<td>-0.008*** (0.004)</td>
<td>-0.004 (0.004)</td>
</tr>
<tr>
<td>Cumulated effect after 20 years (%)</td>
<td>-82.37*** (8.68)</td>
<td>-65.17*** (9.07)</td>
<td>-45.65*** (10.82)</td>
<td>-46.23*** (14.57)</td>
<td>-39.88*** (13.18)</td>
<td>-21.71 (14.18)</td>
<td>-21.36 (18.49)</td>
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<tr>
<td>Compound annual growth rate (%)</td>
<td>-8.31</td>
<td>-5.14</td>
<td>-3.00</td>
<td>-3.06</td>
<td>-2.51</td>
<td>-1.22</td>
<td>-1.19</td>
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<tr>
<td><strong>T (ever designated)</strong></td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
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<tr>
<td><strong>T x A (released from program)</strong></td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Hedonic controls</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Location controls</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Block x area fixed effects</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Year fixed effects</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Time-varying effects</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Weighted</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>YES</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Observations</td>
<td>64,677</td>
<td>17,447</td>
<td>7,841</td>
<td>7,841</td>
<td>8,860</td>
<td>7,841</td>
<td>8,860</td>
</tr>
<tr>
<td>R²</td>
<td>0.420</td>
<td>0.368</td>
<td>0.300</td>
<td>0.278</td>
<td>0.362</td>
<td>0.351</td>
<td>0.417</td>
</tr>
<tr>
<td>AIC</td>
<td>10986.0</td>
<td>12436.8</td>
<td>8002.9</td>
<td>8432.4</td>
<td>8468.6</td>
<td>7958.0</td>
<td>8216.3</td>
</tr>
</tbody>
</table>

**Notes:** Dependent variable is a 0,1 dummy variable that is one if at the time of transaction a property was in poor physical condition and zero otherwise. Estimation method is (weighted) OLS. $T$ is a 0,1 dummy variable denoting a property location within a renewal area. $POST$ similarly denotes that the respective renewal area has been designated. $V$ is the number of years that have gone by since designation. The cumulated effect after 20 years is the combination of the short-run level shift and the long-run effect of the trend shift on the share of transacted buildings in bad condition normalized by the share before the policy was implemented ($\beta₀ + 20 \times \beta₁$)/$S^{PREF}$, where $S^{PREF}$ is the share of buildings in bad condition transacted in 1993 and earlier. Standard errors of the cumulated effect are similarly normalized by $S^{PREF}$. Hedonic and location controls consist of covariates controlling for internal property and external location characteristics described in greater detail in the data section and the appendix. Block x area fixed effects are statistical block x renewal area x investigation area fixed effects. Time-varying controls are sets of interaction effects of year effects and distance to the CBD, district effects, and a consumption amenity measure described in the data section. Weighted model is weighted by the distance from the designation cut-off along a latent deprivation variable (Gaussian kernel, bandwidth according to Silverman-rule). Standard errors in parentheses are clustered on block x area fixed effects in all models. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 
### Tab. 3. Policy effect on propensity of buildings being in good condition

<table>
<thead>
<tr>
<th>Control group</th>
<th>(1) 0.5 km buffer</th>
<th>(2) 0.5 - 2 km buffer</th>
<th>(3) Investigation areas (I)</th>
<th>(4) Investigation areas (II)</th>
<th>(5) Matched observations (IV)</th>
<th>(6) Investigation areas (III)</th>
<th>(7) Matched observations (IV)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>T x POST</strong> ((\beta_0))</td>
<td>-0.004 (0.013)</td>
<td>-0.004 (0.014)</td>
<td>-0.030** (0.019)</td>
<td>-0.012 (0.019)</td>
<td>-0.036* (0.017)</td>
<td>-0.024 (0.019)</td>
<td>-0.022 (0.019)</td>
</tr>
<tr>
<td><strong>T x V</strong> ((\beta_1))</td>
<td>0.009*** (0.002)</td>
<td>0.006*** (0.002)</td>
<td>0.007*** (0.002)</td>
<td>0.009*** (0.002)</td>
<td>0.004* (0.002)</td>
<td>0.002 (0.002)</td>
<td>0.003 (0.003)</td>
</tr>
</tbody>
</table>

| **Cumulated effect after 20 years (%)** | 95.99*** (16.36) | 67.52*** (17.02) | 62.56*** (18.30) | 99.75*** (23.48) | 25.20 (22.28) | 12.72 (22.67) | 16.88 (27.37) |
| **Compound annual growth rate (%)** | 3.42 | 2.61 | 2.46 | 3.52 | 1.13 | 0.60 | 0.78 |
| **T (ever designated)** | YES | YES | YES | YES | YES | YES | YES |
| **T x A (released from program)** | YES | YES | YES | YES | YES | YES | YES |
| **Hedonic controls** | YES | YES | YES | YES | YES | YES | YES |
| **Location controls** | YES | YES | YES | YES | YES | YES | YES |
| **Block x area fixed effects** | YES | YES | YES | YES | YES | YES | YES |
| **Year fixed effects** | YES | YES | YES | YES | YES | YES | YES |
| **Time-varying effects** | - | - | - | - | - | YES | YES |
| **Weighted** | - | - | - | - | YES | - | - |
| **Observations** | 64,677 | 17,447 | 7,841 | 7,841 | 8,860 | 7,841 | 8,860 |
| **\(R^2\)** | 0.802 | 0.773 | 0.635 | 0.635 | 0.711 | 0.682 | 0.736 |
| **AIC** | 79823.3 | 25239.9 | 11273.1 | 10895.7 | 13456.0 | 10744.4 | 13204.3 |

**Notes:** Dependent variable is a 0,1 dummy variable that is one if at the time of transaction a property was in good physical condition and zero otherwise. Estimation method is (weighted) OLS. \(T\) is a 0,1 dummy variable denoting a property location within a renewal area. \(POST\) similarly denotes that the respective renewal area has been designated. \(A\) similarly denotes whether the nearest area has been released from the program. \(V\) is the number of years that have gone by since designation. The cumulated effect after 20 years is the combination of the short-run level shift and the long-run effect of the trend shift on the share of transacted buildings in bad condition normalized by the share before the policy was implemented \((\beta_0 + 20 \times \beta_1)/S_{PRE}, where S_{PRE} is the share of buildings in good condition transacted in 1993 and earlier. Standard errors of the cumulated effect are similarly normalized by \(S_{PRE}\). Hedonic and location controls consist of covariates controlling for internal property and external location characteristics described in greater detail in the data section and the appendix. Block x area fixed effects are statistical block x renewal area x investigation area fixed effects. Time-varying controls are sets of interaction effects of year effects and distance to the CBD, district effects and a consumption amenity measure described in the data section. Weighted model is weighted by the distance from the designation cut-off along a latent deprivation variable (Gaussian kernel, bandwidth according to Silverman-rule). Standard errors in parentheses are clustered on block x area fixed effects in all models. * \(p < 0.1, ** p < 0.05, *** p < 0.01.\)
Tab. 4. Policy effects on property transaction prices

<table>
<thead>
<tr>
<th>Control group</th>
<th>(1) 0.5 km - ∞ buffer(I)</th>
<th>(2) 0.5 - 2 km buffer (II)</th>
<th>(3) Investigation areas (III)</th>
<th>(4) Investigation areas (III)</th>
<th>(5) Matched observations (IV)</th>
<th>(6) Investigation areas (III)</th>
<th>(7) Matched observations (IV)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$T \times POST$ ($\beta_0$)</td>
<td>-0.162*** (0.036)</td>
<td>-0.114*** (0.037)</td>
<td>-0.040 (0.040)</td>
<td>-0.032 (0.050)</td>
<td>-0.140*** (0.053)</td>
<td>-0.073 (0.048)</td>
<td>-0.027 (0.061)</td>
</tr>
<tr>
<td>$T \times V$ ($\beta_1$)</td>
<td>0.048*** (0.003)</td>
<td>0.039*** (0.004)</td>
<td>0.022*** (0.004)</td>
<td>0.022*** (0.005)</td>
<td>0.025*** (0.004)</td>
<td>0.004 (0.004)</td>
<td>0.006 (0.005)</td>
</tr>
<tr>
<td>Cumulated effect after 20 years (%)</td>
<td>120.37*** (6.64)</td>
<td>95.25*** (6.85)</td>
<td>48.96*** (7.90)</td>
<td>50.82*** (9.21)</td>
<td>43.31*** (9.81)</td>
<td>1.08 (8.51)</td>
<td>9.36 (11.15)</td>
</tr>
<tr>
<td>Compound annual growth rate (%)</td>
<td>4.03</td>
<td>3.40</td>
<td>2.01</td>
<td>2.08</td>
<td>1.82</td>
<td>0.05</td>
<td>0.45</td>
</tr>
<tr>
<td>$T$ (ever designated)</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>$T \times A$ (released from program)</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Hedonic controls</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Location controls</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Block x area fixed effects</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Year fixed effects</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Time-varying effects</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Weighted</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
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<td>Observations</td>
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<td>7,841</td>
<td>7,841</td>
<td>8,860</td>
<td>7,841</td>
<td>8,860</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.536</td>
<td>0.502</td>
<td>0.267</td>
<td>0.304</td>
<td>0.324</td>
<td>0.321</td>
<td>0.371</td>
</tr>
<tr>
<td>$AIC$</td>
<td>23573.1</td>
<td>1236.1</td>
<td>491.7</td>
<td>459.8</td>
<td>972.3</td>
<td>448.1</td>
<td>881.1</td>
</tr>
</tbody>
</table>

Notes: Dependent variable is log of property price in all models. Estimation method is (weighted) OLS. $T$ is a 0,1 dummy variable denoting a property location within a renewal area. POST similarly denotes that the respective renewal area has been designated. A similarly denotes whether the nearest area has been released from the program. $V$ is the number of years that have gone by since designation. The cumulated effect after 20 years is the combination of the short-run level shift and the long-run effect: $\exp(\beta_0 + \beta_1 \times 20) - 1$. Hedonic and location controls consist of covariates controlling for internal property and external location characteristics described in greater detail in the data section and the appendix. Block x area fixed effects are statistical block x renewal area x investigation area fixed effects. Time-varying controls are sets of interaction effects of year effects and distance to the CBD, district effects and a consumption amenity measure described in the data section. Weighted model is weighted by the distance from the designation cut-off along a latent deprivation variable (Gaussian kernel, bandwidth according to Silverman-rule). Standard errors in parentheses are clustered on Block x area fixed effects in all models. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 
## Tab. 5. Varying groups of treated and controls

<table>
<thead>
<tr>
<th>No of areas</th>
<th>Cumulated effect after 20 years</th>
<th>% within 2 S.E. length of benchmark</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Treat.</td>
<td>Control</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Varying number of treated areas</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>22</td>
<td>22</td>
</tr>
<tr>
<td>2</td>
<td>22</td>
<td>462</td>
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<tr>
<td>5</td>
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<td>10</td>
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<td>15</td>
<td>22</td>
<td>2500</td>
</tr>
<tr>
<td>20</td>
<td>22</td>
<td>2500</td>
</tr>
<tr>
<td>Varying number of control areas</td>
<td></td>
<td></td>
</tr>
<tr>
<td>22</td>
<td>1</td>
<td>22</td>
</tr>
<tr>
<td>22</td>
<td>2</td>
<td>462</td>
</tr>
<tr>
<td>22</td>
<td>5</td>
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<tr>
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<tr>
<td>22</td>
<td>15</td>
<td>2500</td>
</tr>
<tr>
<td>22</td>
<td>20</td>
<td>2500</td>
</tr>
<tr>
<td>Varying number of treated and control areas</td>
<td></td>
<td></td>
</tr>
<tr>
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<td>1</td>
<td>2261</td>
</tr>
<tr>
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</tr>
<tr>
<td>20</td>
<td>20</td>
<td>2500</td>
</tr>
</tbody>
</table>

Notes: Each row describes the distribution of the cumulated effects after 20 years derived from a series of estimations of the benchmark specification (equations 1 + 2). The effects are expressed in units of log-differences. We consider all possible combinations of one or two treated vs. all (22) control areas and vice versa. For all other combinations we use 2,500 randomly drawn selections. All models estimated using (unweighted) OLS.