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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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JEL: *D62, H23, R21, R31*

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

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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 urban development programs ("Städtebauförderung"), which are typically jointly financed by the

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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.

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 areas[4] 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

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.[6] 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,

[6] 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).[7] 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 polices such as historic preservation may contribute to the development of targeted neighborhoods and thus promote gentrification.[8]

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).[9] 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.

[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.[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).[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.[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.[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:

$$Y_{isnt} = \alpha_1 T_i + f(T_i \times V_{st}) + \delta(T_i \times A_{st}) + X_{it} b + G_{it} c_t + \varphi_t + \mu_n + \varepsilon_{isnt} \quad (1)$$

[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).

[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.

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

[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_{isnt} 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_i, 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 $\hat{\delta}$ because usually there are few observations after an area has been released from designation status. X_{it} 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 μ_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 φ_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

[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.[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.[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.

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.

$$f(T_i \times V_{st}) = \beta_0 T_i \times POST_{st} + \beta_1 T_i \times V_{st} \times POST_{st}, \qquad (2)$$

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[15] An anonymous referee is acknowledged for pointing us in this direction.

[16] We weight transactions using the following area-specific weight : $w_s = \frac{1}{\lambda\sqrt{2\pi}} \exp\left(-\frac{1}{2}\left(\frac{S_S - \bar{S}}{\lambda}\right)^2\right)$, where λ 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).[17]

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:

$$f(T_i \times V_{st}) = \sum\nolimits_{u \neq 0} \beta_u (T_i \times VD_{ust}) \qquad (3)$$

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

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[17] This alternative specification would take the following form : $f(T_i \times V_{st}) = \beta_P T_i \times V_{st} + \beta_0 T_i \times POST_{st} + \beta_1 T_i \times V_{st} \times POST_{st}$. In robustness checks reported in the appendix we also use the following treatment function : $f(T_i \times V_{st}) = \beta_P T_i \times V_{st} \times (1 - POST_{st}) + \beta_0 T_i \times POST_{st} + \beta_1 T_i \times V_{st} \times POST_{st}$. In this model β_P 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.[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.[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

[18] Data are available at www.openstreetmap.org.

[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 $T\text{x}POST$ (β_0) turns out to be small compared to the long-run effect implied by the coefficient on $T\text{x}V$ (β_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 about here –

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 analogical 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).

– Figure 2 about here –

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.[22] 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.

[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 conditon 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.[23] 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

[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.

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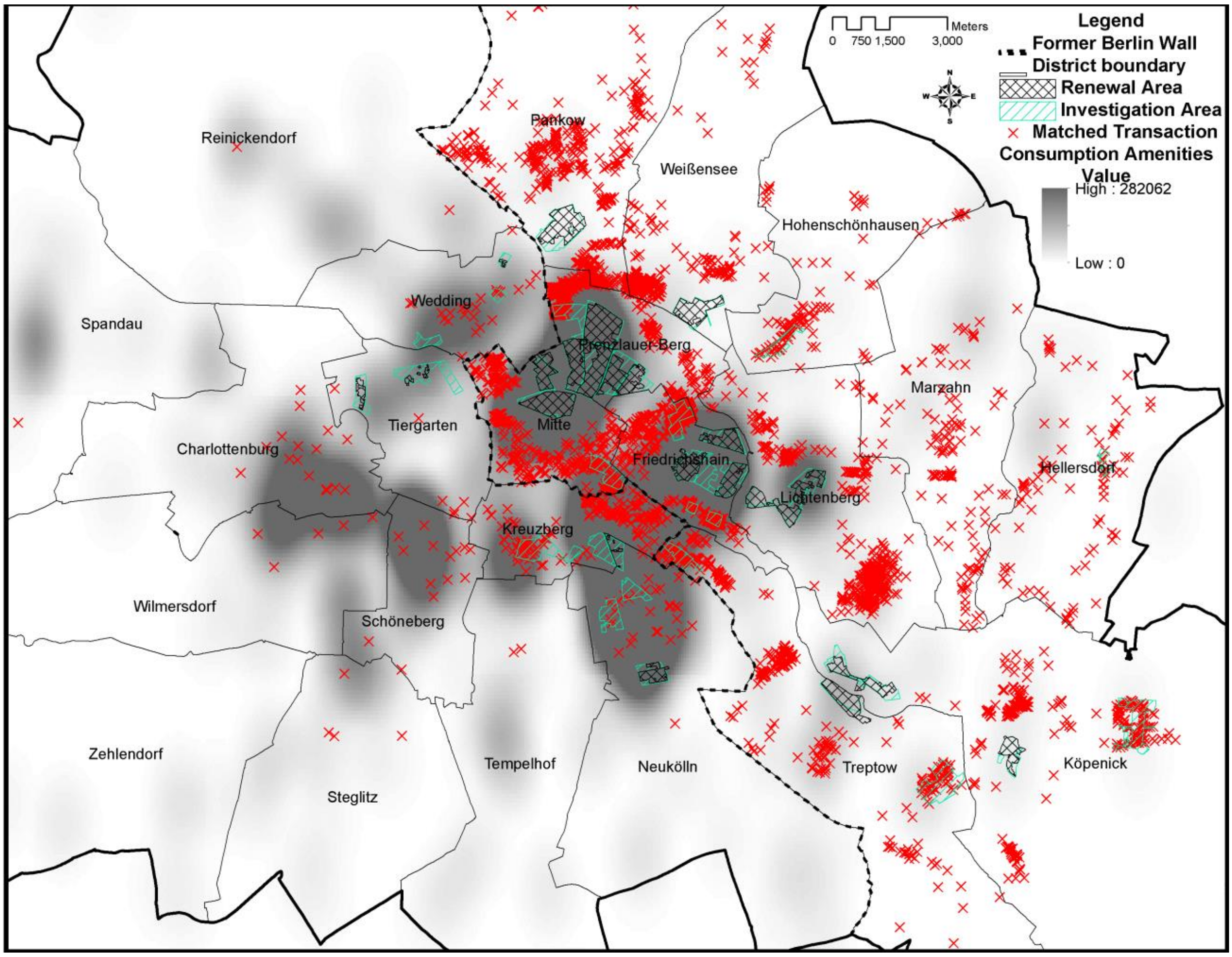
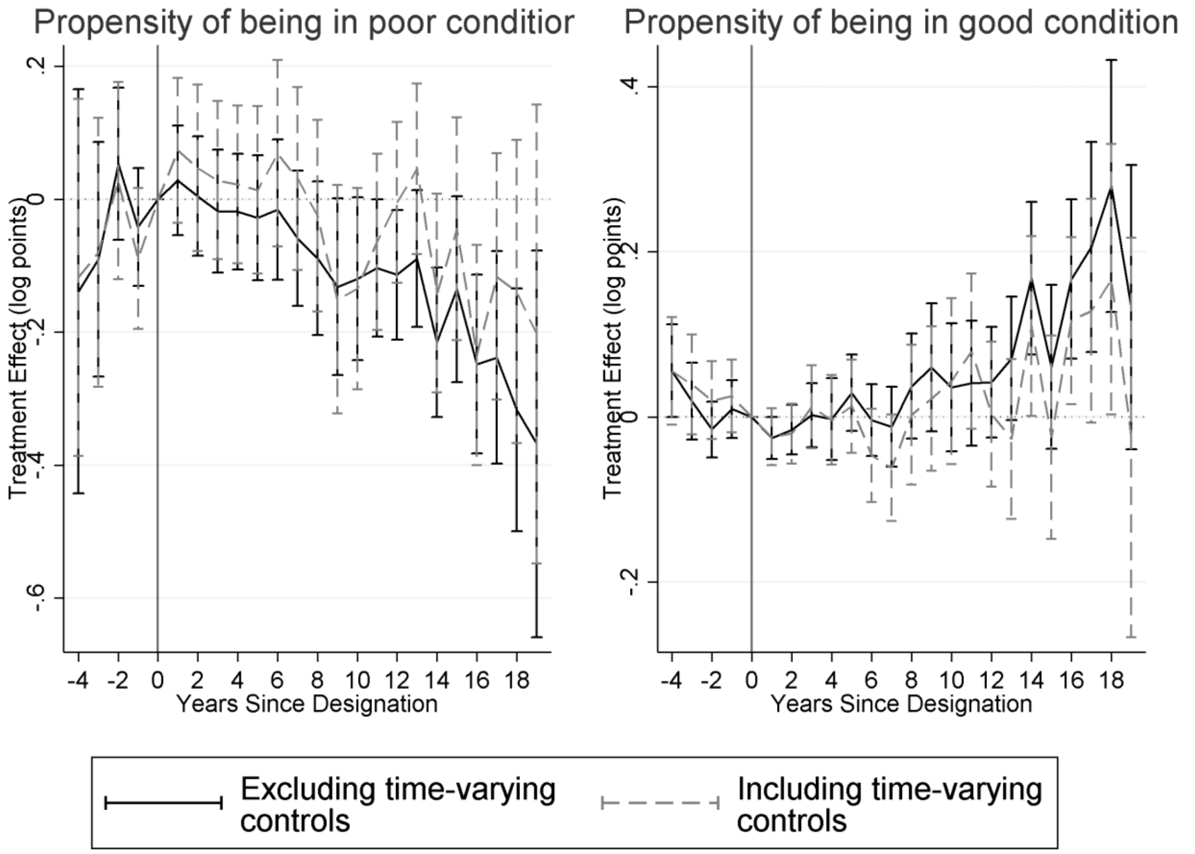
Figures

Fig. 1. Study area



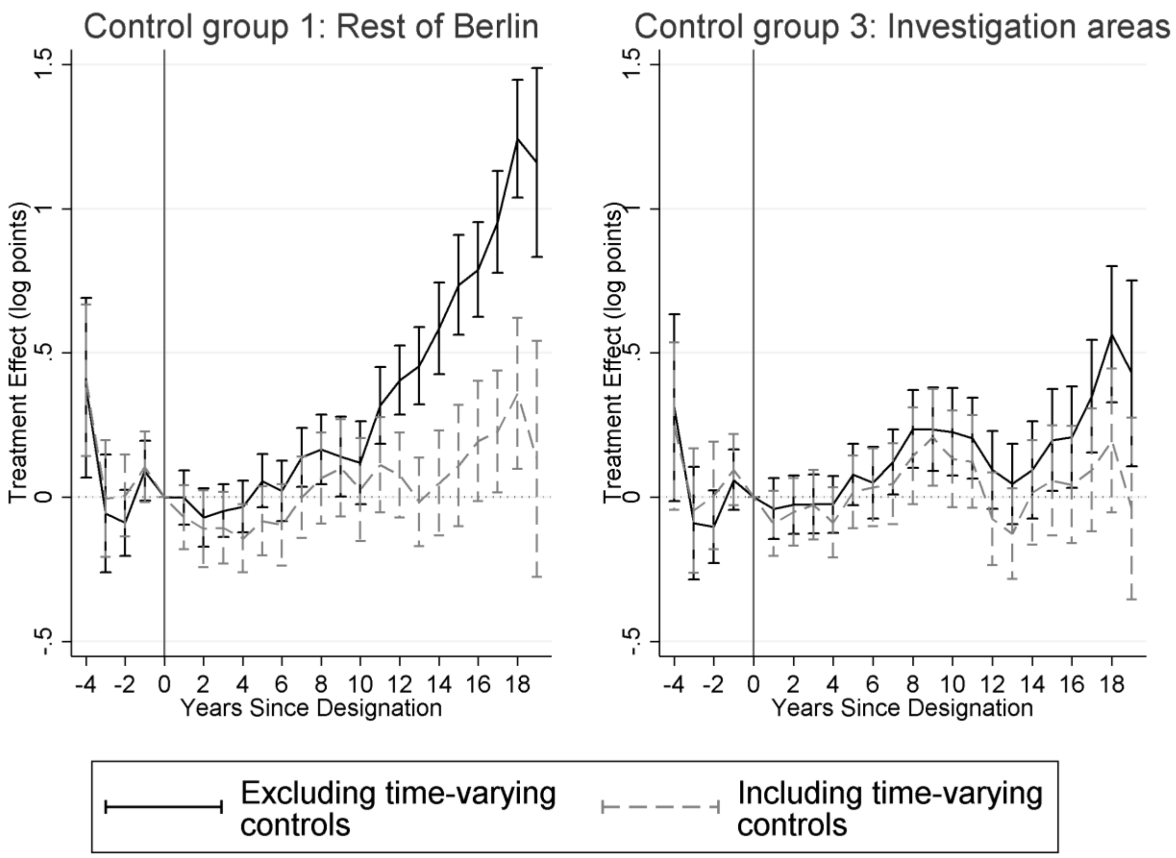
Notes: Own illustration based on the urban and environmental information system (Senatsverwaltung für Stadtentwicklung Berlin, 2006). Crosshatched (hatched) areas indicate renewal (investigation) areas. Crosses are the matched transactions in control group IV. Smoothly shaded areas represent the consumption amenity density.

Fig. 2. Renewal effect on propensity of transacted buildings being in poor or good condition



Notes: Figure illustrates the renewal treatment effect β_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

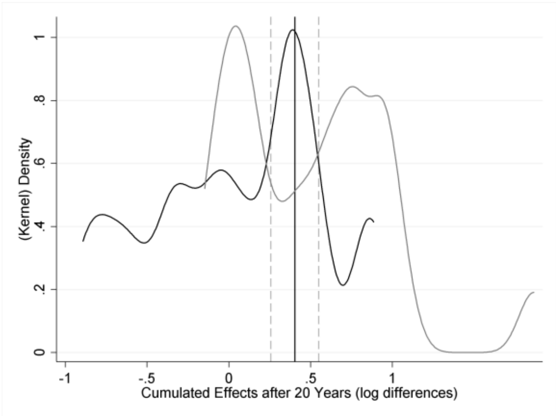


Notes: Figure illustrates the renewal treatment effect β_u as defined in equation 3. Estimates are based on equation (1), including (light dashed lines) or excluding (dark solid lines) time-varying treatment effects G_{it} and using the log of property price as the dependent variable. The parametric 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.

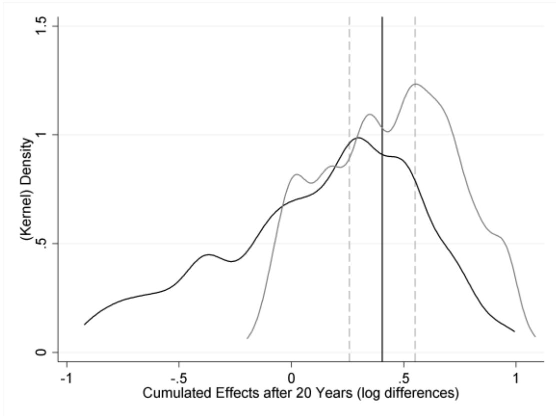
Fig. 4. Varying numbers of areas: Distribution of point estimates

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

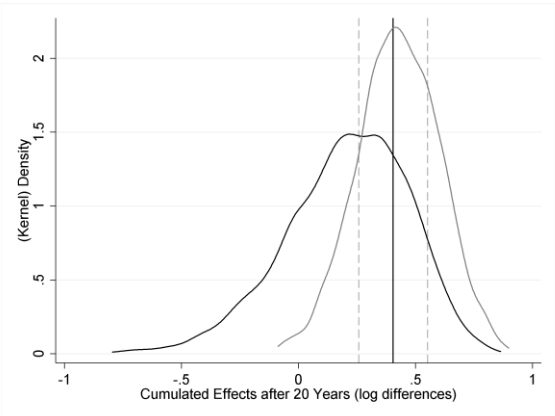
1 random area selected (1 treated vs 22 control)



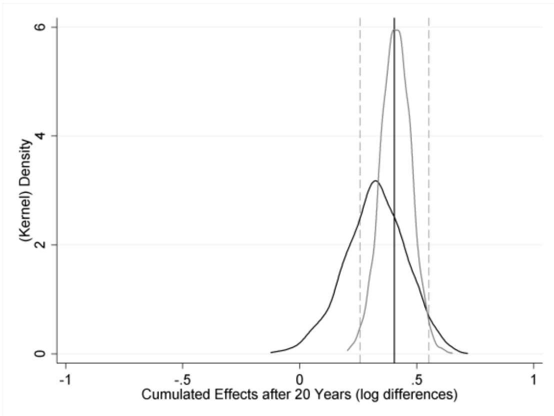
2 random areas selected (2 treated vs 22 control)



5 random areas selected (5 treated vs 22 control)

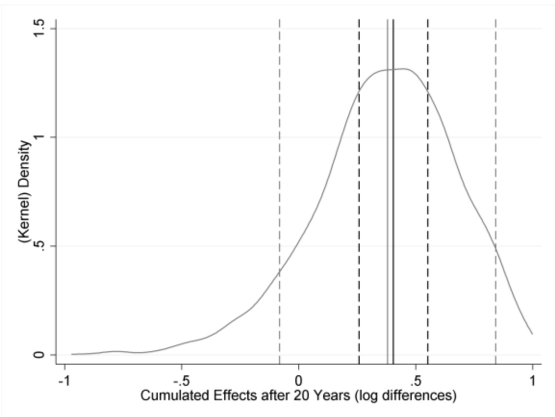


15 random areas selected (15 treated vs 22 control)

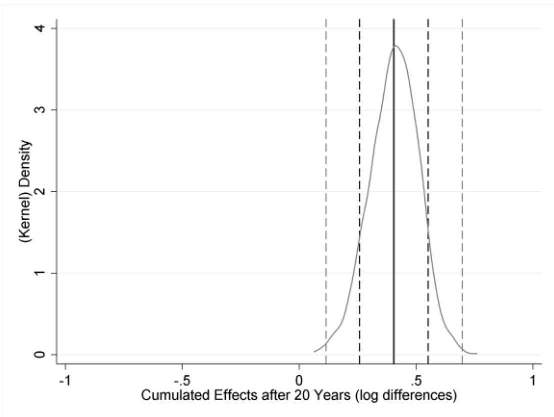


Simultaneously varying number of treatment & control areas

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

	Renewal areas	Invest. areas	Matched obs.	0.5-2k m buffer	Berlin (total)
	(Treated)	(Control group III)	(Control group IV)	(Control group II)	All transactions
Price [€, CPI adjusted]	1,166,478.7	1,320,897.2	1,513,634.6	1,317,781.4	994,908.1
	(1,614,568)	(1,553,772.5)	(3,959,344.1)	(2,763,671.4)	(2,711,511.8)
		[-9.564]	[-21.501]	[-9.371]	[10.626]
Building age	100.8	95.29	92.57	84.98	63.19
	(21.9)	(25.77)	(27.33)	(32.58)	(36.64)
		[25.160]	[37.580]	[72.237]	[171.735]
Condition good [%]	10.3	8.24	11	13.6	21.8
	(30.4)	(27.5)	(31.3)	(34.3)	(41.3)
		[6.776]	[-2.303]	[-10.855]	[-37.829]
Condition bad [%]	42	28.2	34	25.4	14.7
	(49.4)	(45)	(47.4)	(43.5)	(35.4)
		[27.935]	[16.194]	[33.603]	[55.263]
Floor space index (floor space / lot size)	2.664	2.707	2.063	2.153	1.214
	(0.998)	(1.238)	(1.303)	(1.341)	(1.292)
		[-4.309]	[60.220]	[51.202]	[145.291]
Lot size	863.7	919.4	1312.1	1077.9	1040.1
	(923.8)	(978.8)	(2941.6)	(2325.6)	(2746.7)
		[-6.029]	[-48.539]	[-23.187]	[-19.095]
Share of non-German population [%]	13.7	20.6	11.5	16.6	10.7
	(7.21)	(15.1)	(13.4)	(15)	(12.1)
		[-95.700]	[30.513]	[-40.222]	[41.609]
Single family home [%]	0.387	3.16	2.17	16.2	46.5
	(6.21)	(17.5)	(14.6)	(36.8)	(49.9)
		[-44.654]	[-28.712]	[-254.638]	[-742.560]
Apartment building [%]	33.9	40.5	31.6	32.4	20.2
	(47.3)	(49.1)	(46.5)	(46.8)	(40.2)
		[-13.953]	[4.863]	[3.171]	[28.964]
Mixed use building [%]	59.1	48.7	43.8	39.8	20.4
	(49.2)	(50)	(49.6)	(49)	(40.3)
		[21.138]	[31.098]	[39.228]	[78.659]
Commercial use building [%]	2.81	1.76	5.94	3.32	1.65
	(16.5)	(13.2)	(23.6)	(17.9)	(12.7)
		[6.364]	[-18.970]	[-3.091]	[7.030]

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

Control group	(1) 0.5 km - ∞ buffer (I)	(2) 0.5 - 2 km buffer (II)	(3) Investigation areas (III)	(4) Investigation areas (III)	(5) Matched observations (IV)	(6) Investigation areas (III)	(7) Matched observations (IV)
T x POST (β_0)	-0.030	-0.017	0.006	-0.015	0.031	0.053	-0.025
	(0.031)	(0.032)	(0.036)	(0.049)	(0.046)	(0.050)	(0.056)
T x V (β_1)	-0.020***	-0.016***	-0.012***	-0.011***	-0.012***	-0.008**	-0.004
	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Cumulated effect after 20 years (%)	-82.37***	-65.17***	-45.65***	-46.23***	-39.88***	-21.71	-21.36
	(8.68)	(9.07)	(10.82)	(14.57)	(13.18)	(14.18)	(18.49)
Compound annual growth rate (%)	-8.31	-5.14	-3.00	-3.06	-2.51	-1.22	-1.19
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	0.420	0.368	0.300	0.278	0.362	0.351	0.417
AIC	10986.0	12436.8	8002.9	8432.4	8468.6	7958.0	8216.3

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. *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 bad 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. 3. Policy effect on propensity of buildings being in good condition

Control group	(1) 0.5 km - ∞ buffer (I)	(2) 0.5 - 2 km buffer (II)	(3) Investiga-tion areas (III)	(4) Investiga-tion areas (III)	(5) Matched observa-tions (IV)	(6) Investiga-tion areas (III)	(7) Matched observa-tions (IV)
T x POST (β_0)	-0.004	-0.004	-0.030**	-0.012	-0.036*	-0.024	-0.022
	(0.013)	(0.014)	(0.014)	(0.019)	(0.019)	(0.017)	(0.019)
T x V (β_1)	0.009***	0.006***	0.007***	0.009***	0.004*	0.002	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Cumulated effect after 20 years (%)	95.99***	67.52***	62.56***	99.75***	25.20	12.72	16.88
	(16.36)	(17.02)	(18.30)	(23.48)	(22.28)	(22.67)	(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

Control group	(1) 0.5 km - ∞ buffer(I)	(2) 0.5 - 2 km buffer (II)	(3) Investigation areas (III)	(4) Investigation areas (III)	(5) Matched observations (IV)	(6) Investigation areas (III)	(7) Matched observations (IV)
T x POST (β_0)	-0.162***	-0.114***	-0.040	-0.032	-0.140***	-0.073	-0.027
	(0.036)	(0.037)	(0.040)	(0.050)	(0.053)	(0.048)	(0.061)
T x V (β_1)	0.048***	0.039***	0.022***	0.022***	0.025***	0.004	0.006
	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
Cumulated effect after 20 years (%)	120.37***	95.25***	48.96***	50.82***	43.31***	1.08	9.36
	(6.64)	(6.85)	(7.90)	(9.21)	(9.81)	(8.51)	(11.15)
Compound annual growth rate (%)	4.03	3.40	2.01	2.08	1.82	0.05	0.45
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	0.536	0.502	0.267	0.304	0.324	0.321	0.371
AIC	23573.1	1236.1	491.7	459.8	972.3	448.1	881.1

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

No of areas		Cumulated effect after 20 years					% within 2 S.E. length
Treat.	Control	Iterations	Mean	S.D.	Min	Max	of benchmark
Varying number of treated areas							
1	22	22	-0.02	0.63	-1.28	1.01	28.32%
2	22	462	0.09	0.47	-1.26	0.99	30.40%
5	22	2500	0.21	0.26	-0.79	0.86	37.48%
10	22	2500	0.26	0.18	-0.43	0.78	47.08%
15	22	2500	0.32	0.13	-0.12	0.72	66.80%
20	22	2500	0.32	0.13	-0.18	0.69	64.44%
Varying number of control areas							
22	1	22	0.51	0.47	-0.15	1.87	14.56%
22	2	462	0.44	0.30	-0.20	1.09	31.72%
22	5	2500	0.43	0.17	-0.09	0.90	58.88%
22	10	2500	0.41	0.10	0.08	0.75	85.08%
22	15	2500	0.41	0.07	0.20	0.65	97.32%
22	20	2500	0.41	0.06	0.17	0.61	98.00%
Varying number of treated and control areas							
1	1	2261	0.24	1.74	-14.05	19.46	12.20%
2	2	2500	0.32	0.60	-4.00	2.76	19.40%
5	5	2500	0.38	0.31	-0.97	1.45	37.96%
10	10	2500	0.40	0.17	-0.18	1.01	60.88%
15	15	2500	0.41	0.11	0.06	0.76	84.36%
20	20	2500	0.41	0.10	0.02	0.72	83.60%

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.

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Technical appendix to: "Urban renewal after the Berlin wall": A place-based policy evaluation

Version: *January 2015*

1 Introduction

This technical appendix complements the main paper by providing complementary evidence and additional details on the data used. The appendix is not designed to stand alone or replace the main paper. Section 2 adds to the empirical strategy and data section of the main paper, providing further details on the renewal areas, the control groups, and the data. Section 3 provides complementary evidence that extends the results in sections 4 and 5 of the main paper. Finally, section 4 contains our analysis of potential externality and spillover effects.

2 Data

This section provides additional information on the studied areas and descriptive evidence not reported in the main paper to save space.

2.1 Berlin – stylized facts

Our study area comprises the area of the Federal State of Berlin, Germany. The city in 2012 counted some 3.3 million inhabitants and approximately 1.9 million dwelling units. Approximately 14% of the population are non-German citizens. While there have recently been signs of economic recovery after a relatively long period of economic struggle since unification, the unemployment rate has remained relatively high at approximately 13%. The overall area is approximately 892

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square kilometers (344 square miles). The center is densely populated, the overall building structure is a mix of historic buildings (aged approximately 100 to 130 years), buildings constructed after World War II to substitute the destroyed building stock (aged approximately 50–60 years), and newer buildings.

2.2 Renewal and investigation areas

This subsection presents the studied areas in greater detail. To convey an understanding of the size and form of the relevant space, Figure 1 in the main paper depicts the renewal and investigation areas. Figure A1 provides a more detailed picture of a cluster of renewal areas (gray) in East Berlin. Those parts of the investigation areas that were not transformed into renewal areas are hatched (diagonal parallel lines). It is reassuring that the matched observations (red X) and the investigation areas cover similar areas (if outside the 500m buffer), while geographically proximate but structurally different areas (for example, Wedding) are underrepresented. Some technical details on the matching technique are discussed in the following subsection.

In Table A1, we present some additional descriptive statistics on the renewal areas, including exact dates of the beginning and end of the designation, and the number of housing units, properties, and population.

– Figure A1 about here –

– Table A1 about here –

2.3 Renovation subsidies vs. capitalization effects

To put the €1.94 billion invested in renewal areas into context we approximate the total value of the housing stock in these areas. We compute the average property value as the average price in the renewal areas, discounted by a repeated-sales index that we normalize to a period ranging from 1998 to 2002, which is roughly the midpoint of the renewal program period. To construct a repeated sales index we estimate the following regression model:

$$P_{it} = \alpha + X_{it} + \sum\nolimits_{t \neq (1998,\ldots,2002)} \varphi_t + \theta_i + \varepsilon_{it}$$

, where P_{it} is the price at which a property *i* is sold at time *t.* We exclude single family homes as they are practically non-existent in the renewal areas. *X* are observable property characteristics discussed in the main paper, and θ_i is a set of property fixed effects holding all time-invariant location effects constant. The time effects φ_t form the repeated sales price index, which we use to discount the 2012 mean property price in renewal areas to the base value (1998–2002). We then multiply the resulting property value of €927,908 by the total number of properties in the renewal areas (5,844), which results in a total value of €5.42bn. The total expenditures attributable to the renewal policy thus amount to as much as 35.7% of the property value in the targeted areas.

The approximated total value of the housing stock can also serve to shed some light on the return on investment of the policy instrument. Drawing on the results in Table 4 in the main paper (columns (3) and (6)), the cumulated increase in housing value corresponds to a(n) (upper-) lower-bound estimate of an insignificant 1.08% (significant 48.96%) of the total housing value (€5.42bn). Setting these figures in relation to the overall investment of €1.94bn results in a return of €0.03–€1.37 per invested Euro which implies limited multiplier effects. While the windfall capital gains to landlords might have to some extent capitalized into housing value, the results do not indicate the presence of a strong housing externality, i.e., an effect of renovations on the value of buildings other than those renovated.

2.4 Control groups

This section discusses the different control groups and presents some technical details on the creation of control group IV. Overall, we observe approximately 71,000 transactions between 1990 and 2012 in Berlin. Of these transactions, 4,500 occurred inside our renewal areas. The transactions are compared to varying control groups. Control group I comprises all other transactions (outside a 500m buffer) and control group II all transactions in a 500–2,000 meter radius around the renewal areas. Control group III consists of the investigation areas and includes approximately 7,800 transactions. The matching procedure discussed below results in about 4,800 transactions that are matched to our renewal area transactions (control group IV).

We generate control group IV using a synthetic matching technique: We use the propensity score matching methodology advanced by Rosenbaum and Rubin (1983) to find observations that are structurally similar to the transactions in the renewal areas. For the estimation of models 4 and 6 in tables 2–4 in the main paper we include the following covariates: age of the building, building type, location quality, typical area floor space index, distance to the nearest park, main street, playground, waterway, and public transport station, latitude and longitude, and a set of dummies controlling for land use and east/west location. We match the treatment group to the control group using nearest-neighbor matching. The matching process creates subsamples, where the difference in means between the treatment and the control group is substantially reduced. Table A2 reports measures of the balance of the covariates for the control group IV.

– Table A2 about here –

3 Baseline models: complementary evidence

This section complements sections 4 and 5 of the main paper. The first subsection provides an overview of the variables and presents some of the estimation results omitted in the main paper. Section 3.2 discusses in detail how we approximate the latent deprivation variable and the cut-off value that determines designation. Section 3.3 discusses the potential problem of sample selection. Section 3.4 evaluates the robustness of our results to the inclusion of pre-treatment trends. Section 3.5 presents results for alternating combinations of the time-varying effects. Section 3.6 discusses changes in the spatial structure of the city to put our renewal estimates into some context. Section 3.7 considers alternative ways to account for the spatial autocorrelation of the standard errors in our model. Section 3.8 evaluates possible designation effects on the runner-up areas that remained unconsidered. In section 3.9, we replicate our benchmark results using an urban amenity density measure based on historic data. Finally, section 3.10 presents the results of the sensitivity analysis using the expanded model including time varying effects.

3.1 Complete results

Table A3 provides descriptive statistics of all structural and locational variables. Table A4 extends Table 4 in the main paper by presenting the implicit hedonic prices of the structural characteristics.

– Table A3 about here –

– Table A4 about here –

Most coefficients are as expected: To mention some examples, plot area and floor space significantly increase log prices. The land use indicators show, if significant, a positive influence of residential and commercial areas on log prices (relative to manufacturing sites). The age of a building tends to depreciate its (log) price.

3.2 Weighted estimates using control group III

To form a strong counterfactual, the selection of renewal areas from the pool of considered investigation areas has to be random. We conducted an extensive review of the official documentation on the selection process. We found no evidence of a ranking of the investigation areas according to deprivation or need. Instead, there exist several records stating that the considered investigation areas all fulfill the necessary criteria to be designated as renewal areas, but that budget constraints do not allow for further designations (Senatsverwaltung für Stadtentwicklung Berlin, 1995, 1997). The official documentation suggests that selection was random with respect to deprivation levels and, if anything, was guided by a need to achieve a balanced geographical distribution across districts.

Yet, it seems possible that the selection was guided by a needs assessment at least implicitly.[1] In a complementary approach we 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 beyond which no further designation was viable. 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.

The implementation of this strategy was complicated by the fact that the official reports are not or are no longer available for all originally considered investigation areas. Thus, to compile a data set describing the initial socio-economic conditions of the considered investigation areas we had to rely on our own data collection. We compiled a data set using GIS (the number of parks and playgrounds per square kilometer, the number of schools and kindergartens per square kilometer, the distance to the nearest public transport station), historic land value assessments (Bodenrichtwerte), property transaction data (floor area ratio, share of stove heating, outside wc, outside bathroom, all from 1992), and demographic information (age structure at block level 1992).

To validate our measures and to ensure that the data we process is consistent with the information that was available to decision-makers, we extracted similar information from the investigation area reports that were available at State Archive Berlin (Landesarchiv Berlin). We report the correlation between our self-constructed measures and those extracted from the official documents (where available) in Table A5. We find a strong correlation between our self-constructed data (left column) and the archived records (right column) for most of the variables, indicating that the collected data set describes the investigation areas in 1992 sufficiently well.

– Table A5 about here –

These self-constructed variables are input into an auxiliary OLS regression where the dependent variable is a 0,1 dummy variable indexing areas that were designated and became renewal areas. The predicted values from this regression are recovered as a proxy for the propensity to be designated as a renewal area, given the covariates. Given that 22 out of 44 investigation areas were

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[1] We thank an anonymous referee for this suggestion.

designated we unsurprisingly find that a cut-off point of 0.5 best predicts whether or not an area was designated or not. Nineteen of the 22 renewal areas that have been designated have a propensity score that exceeds this cut-off value, suggesting that we have predicted the selection reasonably well. The left panel of Figure A2 plots the propensity score against the actual designation outcome and the cut-off point.

To restrict the identifying variation to the fraction that is most plausibly exogenous we weight all observations in our baseline difference-in-differences specification according to their distance from the threshold using a Gaussian kernel. In particular, we weight transactions in area s using the following area-specific weight:

$$w_s = \frac{1}{\lambda\sqrt{2\pi}} \exp\left(-\frac{1}{2}\left(\frac{S_s - \bar{S}}{\lambda}\right)^2\right)$$

, where λ is the bandwidth which we set according to the Silverman (1986) rule: $\lambda = 1.06 \times \sigma N^{-\frac{1}{5}}$. The right panel of Figure A2 illustrates the resulting weight distribution.

– Figure A2 about here –

As an alternative we consider a binary weighting scheme where all observations receive a weight of one if they fall within a window around the cut-off value, and zero otherwise. Within the smallest window we consider of 0.05, four out of eight investigation areas were selected as renewal areas.

In columns (1–3) of Table A6 we explore how the estimated effects on the propensity of a building being in poor condition depends on the choice of the weighting scheme. In columns (1–2) we halve and double the optimal bandwidth used in Table 2, column 4. In column (3) we use the binary weighting scheme with the smallest bandwidth. The results fall within a very narrow range and are close to the baseline in Table 2 in the main paper. In columns (4–6) we similarly alter the weighting scheme of the baseline model in Table 3, column 4, in the main paper, which analyzes the policy effects on the propensity of a building being in good condition. The variation is somewhat larger than in columns (1–3), but is still within a relatively small range. More local estimates tend to produce larger policy effects, which is in line with the results presented in the main paper.

In Table A7 we similarly alter the weighting scheme and explore the sensitivity of the estimated price effects. We, again, halve and double the optimal bandwidth (1 and 3) and present the results with the optimal bandwidth for comparison (2). We also present the results using the binary weighting scheme and a window bandwidth of 0.05, 0.1, and 0.2. The policy effect remains roughly within the same range across all specifications. There is no clear correlation between the size of the bandwidth and the magnitude of the estimated policy effect.

– Table A6 about here –

– Table A7 about here –

3.3 Sample selection

One concern with our data is that we only observe the price and the quality of a property if and when it is transacted. If properties of higher quality sell quicker it is theoretically possible that the policy will increase the number of transactions in the treated areas. In this case the marginal transacted home in the treated areas could be of lower quality, which could affect the estimated treatment effect.[2] To test the effect of the policy on the number of transactions we collapse our data set to observational year-block units and compute the number of transactions within each unit. We then replicate our standard set of estimations (tables 2–4 in the main paper) using the log of the number of transactions as dependent variables. The results presented in Table A8 reveal that in relative terms the number of transactions in the treated areas declined over time, suggesting that landlords hold longer to their properties. Thus, if anything, the selection effect will increase the estimated policy effect (because the marginal property is of higher quality), once more suggesting that the effects of the policy are likely moderate.

– Table A8 about here –

To substantiate this interpretation we replicate the analysis using so-called standard land values (Bodenrichtwerte) from the Berlin Committee of Valuation Experts (Gutachterausschuss für Grundstückswerte). Land values assessed by such committees, which exist throughout Germany, capture the fair market value of a square metre of land if it was undeveloped. These assessed land values are, thus, supposed to abstract entirely from structural values. The assessment by these committees is based on recent market transactions and is generally considered reliable (Weiss, 2004). Ahlfeldt et al. (2015), who use similar data, show that the standard land values tend to closely follow market prices. The results presented in Table A9 are below the estimated treatment effects on property prices, which is in line with the sample-selection interpretation above and provides further evidence that the policy effects, if anything, were small.

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[2] We owe this idea to an anonymous referee.

– Table A9 about here –

3.4 Pre-trends

As with every difference-in-differences estimation strategy we assume that in the absence of designation the renewal areas and the control areas would have followed similar trends. This assumption can generally not be tested, but it is at least possible to evaluate whether the respective areas followed similar trends prior to the intervention. To allow for pre-trends we expand our baseline treatment function as follows:

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

, where V_{st} denotes the years since the designation of a renewal area during the pre-period. V_{st} takes negative values for the period pre-designation and a zero value afterwards (e.g., -1 for one year before designation and -2 for two years before designation). $POST_{st}$ indicates observations after designation ($V_{st} > 0$). T similarly indicates a location inside a renewal area. Plugging this treatment function into the baseline estimation equation (1) leads to the results presented in Table A10.

We report results for three different dependent variables, the propensity of a property being in poor condition, the propensity of a property being in good condition, and the log of sales prices. In each case we report the results excluding and including time-varying effects. The results reported thus correspond to columns (3) and (6) in tables 2, 3, and 4 in the main paper. In five out of these six models we find no significant difference in the trends between treated and control areas prior to the treatment, which makes it less likely that our estimated policy effects are driven by unrelated trends. In case of the price regression including time-varying effects (column (6)) we find a significantly negative pre-trend which, if the extrapolation were to be taken as the counterfactual, would imply that we have underestimated the policy effect. While we prefer to avoid an extrapolation of the pre-trend because the pre-period is short relative to the post period, we argue that all of the six models suggest that the results reported in the main paper are unlikely to be driven by long-run trends that are correlated with the treatment.

– Table A10 about here –

3.5 Time-varying effects

In columns (6) and (7) of tables (2–4) in the main paper we control for unobserved trends that are correlated with observable locational characteristics using a relatively extensive set of time-varying effects. We interact distance to the CBD, an amenity density measure, and a full set of district effects with year fixed effects. One concern with this approach is that changes in the implicit prices of these variables (e.g., distance to the CBD) could be driven by the policy, in which case the time-varying controls would be absorbing variation that is genuinely attributable to the policy.

To address this concern we replicate the baseline models using a number of less extensive combinations of time-varying effects. Table A11 displays the specification from Table 4 (columns (6) and (7)) from the main paper exclusively using the year x district effects (columns (1) and (2)), exclusively the consumption amenity x year effects (columns (3) and (4)), and the consumption amenity effects combined with an interaction of year effects and a dummy variable distinguishing between East/West Berlin (columns (5) and (6)). The evaluated policy effects on prices tend to react differently to the inclusion of the different time-varying effects, depending on whether we use as a control group the investigation areas (control group III, columns (1), (3), and (5)) or the matched observations (control group IV, columns (2), (4), and (6)). With district x year effects exclusively, the policy effect becomes insignificant if investigation areas are used as a control group (column (1)). District trends thus explain much of the heterogeneity in trends between renewal and investigation areas. In contrast, the policy effect remains relatively large and significant if the control group of matched observations is used (column (2)). Because we matched on the general location in the city using x- and y- coordinates as PSM covariates, the insensitivity to the inclusion of district x year effects when control group 4 is used does not come as a surprise.

Using the matched control group the results are sensitive to the inclusion of controls for amenity trends (column (4)), which is conclusive given that we did not use this variable as a covariate in the PSM procedure. Interestingly, the effect remains relatively large if we control for amenity trends but not for district x year effects, and use the investigation areas as a control group (column (3)).

The vast majority of renewal areas were designated in former East Berlin. We expect heterogeneous trends as the two parts of the city reintegrated into a common housing market area over our study period (Ahlfeldt et al., 2015). In columns (5) and (6) we therefore allow for trend heterogeneity with respect to the amenity density and a location within former East Berlin. This specification is significantly less demanding than the expanded specifications in columns (6) and (7) of Table 4 of the main paper. Yet both specifications produce insignificant and near to zero treatment effects.

– Table A11 about here –

3.6 Urban spatial structure

To put the magnitude of the policy effects into perspective we provide an analysis of the change in general spatial structure of Berlin over the course of the study area. We allow implicit prices of selected attributes to change by using the same approach as in the extended baseline model (including time-varying effects).

The arguably most popular summary statistic for within-urban spatial structure in urban economics is the distance from CBD gradients, a measure of property prices change as one moves away from the center of the city. We allow the effect of the proximity to the CBD to vary over time by means of distance from CBD x year interaction effects. Normalized to the initial value in 1991, we plot how the implicit price of centrality varies over time in the left panel of Figure A3.

The study period we analyze covers the period during which the two parts of Berlin that were separated during the Cold War period integrated to a unified city. To gain insights into the speed of the convergence process we estimate East x year interactions effects, where East is a dummy variable indexing transactions within former East Berlin. The estimates are plotted in the right panel of Figure A3, again normalized by the initial value in 1991.

The two panels reveal striking changes in the spatial structure of the city over the two post-unification decades. From the mid-2000s onwards central parts of the city steadily appreciated relative to the rest of the city. 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. This is in line with the resurgence of central cities that has been debated in the consumer city literature (Glaeser et al., 2001). The appreciation process of East Berlin relative to West Berlin is even more impressive. Properties in East Berlin, on average, appreciated by about 145% relative to properties in West Berlin. The convergence process is concave, that is, the gap in growth rates between the two city parts gradually declines over time.

These results are important to understanding the nature of the policy effect. Since most of the renewal areas are located in former East Berlin the renewal policy effects come in addition to an already very substantial increase in property prices across, in particular, the central areas of East Berlin. These results also explain why the inclusion of time-varying effects substantially reduces the renewal area effect.

– Figure A3 about here –

3.7 Alternative standard errors

In our benchmark specification reported in the main paper we allow for unobserved time-invariant effects at the block level. Standard errors are clustered at the same level. Because statistical blocks are relatively small we flexibly allow for a relatively complex (cross-sectional) spatial structure in the error terms at the expense of having relatively few observations within a block cell. We therefore expect relatively large standard errors, which leads to the concern that we may be raising the bar for rejecting the null-hypothesis (of no renewal effect) too high. In this section we consider alternative methods to compute standard errors. We focus on the lower-bound estimates from the models, including time-varying effects, since the models excluding time-invariant effects generally produce highly significant results no matter how we compute standard errors.

The results are reported in Table A12 where we also present the benchmark point estimates and standard errors (clustered on blocks) in column (1) for comparison. In column (2) we alter the clustering level to traffic cells (Verkehrszellen). Our sample of property transactions is distributed across 324 traffic cells which, on average, consist of about 40 blocks forming neighborhoods that are just about walkable. With this specification we assume independency of standard errors between much larger groups of transactions. In column (3) we further increase the size of the clustering cells by using the 190 Berlin zip codes. As with the block level clustered standard errors the cumulated effect remains statistically insignificant. We note that the estimates also remain insignificant if we alter the location fixed effects to the traffic or zip code level, despite a substantial increase in degrees of freedom.

In an alternative approach to controlling for spatial dependence of the error, we adopt the procedure suggested by Conley (1999). Using varying distance cut-offs, we calculate standard errors corrected for spatial autocorrelation, serial correlation, and heteroscedasticity adapted for panel data as in Hsiang (2010). The resulting standard errors are reported in columns (4) to (7) of Table A8. With a 50km cut-off, which essentially implies that correlation among all observations is allowed for, we find standard errors that are marginally smaller than with clustered standard errors. As we decrease the distance cut-off we tend to get smaller standard errors. However, even with cut-off distances that are way below conventional thresholds the estimated policy effect remains insignificant.[3] We conclude that the lower-bound treatment effects are statistically not distinguishable from zero.

– Table A12 about here –

3.8 Designation effects on investigation areas

One of the identifying assumptions of quasi-experimental research designs is that the control group used to establish a counterfactual must not be affected itself by the analyzed treatment. A control group formed by runner-ups in a selection process could violate this assumption if the selection of those being treated changes the expectation regarding the prospect of those remaining untreated. If a positive signal to the treated areas represents a negative signal to the runner-up areas, the estimated treatment effect would be positively biased. To avoid the potentially problematic direct comparison of the selected renewal areas to the runner-up areas, we benchmark both areas against the matched transactions (control group IV) discussed in section 2. We define the renewal areas and the "runner-up" investigation areas that remained undesignated as two separate treatment groups and assign all matched transactions outside the investigation areas to the control group. In Table A13, we report the results of two models that are analogous to (4) and (7) in Table 4 in the main paper, except for the added second treatment group (investigation areas). We choose 1995 as a (placebo) treatment date for the investigation areas that were not designated because the last wave of designation occurred in that year and the decision not to include these areas into the program became definitive.[4]

For the investigation areas, the cumulated effects after 20 years are not statistically different from zero, no matter whether we allow for selected time-varying effects or not. This finding is consistent with the results in Table 4 in the main paper, where the comparison of trends in renewal areas to either the remaining investigation areas or the matched transactions led to similar results. Taken together, the evidence does not indicate that the runner-up areas provide an invalid counterfactual.

– Table A13 about here –

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[3] For US census data, distance cut-offs are often set at approximately 10 miles (Boarnet et al., 2005; Jeanty et al., 2010).

[4] Setting the placebo designation date to the date of the nearest renewal area changes the results only marginally.

3.9 Historic amenity density

As outlined in the main paper, we employ a kernel-smoothed density surface interacted with year dummies based on the geographic location of bars, pubs, and nightclubs to account for the change in valuation over time for these urban amenities. The rationale behind this approach is that particular districts with great centrality, and many urban amenities, could have increased in value anyway and that this increase cannot be attributed to the designation of the renewal areas. The data stem from the open street map project. One concern is the potential endogeneity of the current (2012) distribution of amenities to the designation of the renewal areas. To address this concern, we provide an alternative approach as a robustness check: we collected data for the distribution of urban amenities for the years 1995/96, the first year in which the yellow pages for Berlin reported zip codes in a new format that applies to both parts of the formerly divided city and allows for precise geocoding. Figure A4 compares the resulting 1995/96 kernel-smoothed density surface (left panel) with the existing current surface displayed in Figure 1 in the main paper (right panel): While there is a shift in amenity gravity from the south and north western to the eastern downtown areas, the overall centralized spatial pattern has remained remarkably stable over more than 15 years of convergence to a new post-Berlin Wall equilibrium.

– Figure A4 about here –

Table A14 replicates our expanded property price models (columns (6) and (7) in Table (4) using the consumption amenity density depicted in the left panel of Figure A2. Columns (1) and (2) report the effects within renewal areas when compared to the investigation areas and the matched observations. The differences from our primary results are not substantial. As in our main results, no effects are significantly different from zero.

– Table A14 about here –

3.10 Simulation including time-varying effects

This section replicates the sensitivity analysis presented in section 5 of the main paper for the model including time-varying effects (Table 4 column (6)). Similar to Table 5 in the main paper Table A15 summarizes the distributions and the share of point estimates. Corresponding to Figure 3 in the main paper Figure A5 displays some selected distributions graphically. The implications are similar to those presented in the main paper.

– Table A15 about here –

– Figure A5 about here –

4 Externalities and spillover effects

One justification for public expenditure on urban renewal policies rests on anticipated positive and self-reinforcing housing externalities, i.e., the hope that subsidies for the renovation of a property will benefit others in addition to the respective building or owner. With our baseline empirical models we establish a composite renewal effect which consists of an increase in the structural value of renovated properties and an increase in locational value due to the renovation of adjacent properties, i.e., a housing externality. In this section we aim at separating the effect of the (subsidized) renovation of buildings on their own value from the effects of increased nearby renovation activity.

One attractive feature of our data set is an indication of a property's physical condition at the time of transaction. We exploit this feature to determine the housing externality effect by exclusively focusing on properties in good condition. The rationale is twofold. First, by holding internal quality constant, our estimated treatment effects only capture appreciation related to the renovation of surrounding properties, i.e., (a housing) an external(ity) effect. Second, we argue that properties in good condition at the time of the transaction are unlikely to be renovated immediately following the transaction, and hence that renovation incentives (subsidies and tax deductions) do not (or only to a limited extent) capitalize into transaction prices. We complement this approach to measuring housing externalities with an analysis of spatial spillovers into areas just outside the treated areas. Before we present our actual empirical specification, we introduce the basic nature of the treatment effect we estimate.

4.1 Identification

Let us assume we observe a property, the maintenance levels of which are constant within a neighborhood and depend on a housing subsidy *S*. Within a neighborhood, the housing subsidy policy is uniform.

At any given location, the value of a property *(P)* depends on the maintenance level *(I)*, a (housing) externality *(E)*, which depends on the maintenance level in the neighborhood and the amenity level *(L)* of the neighborhood, and the overall macroeconomic conditions that are invariant across neighborhoods *(Y)*. For now, we assume that the policy does not impact neighborhood quality except through a housing externality:

$$P = f\big(I(S), E\big(I(S)\big), L, Y\big)$$

For simplicity, we assume that the externality is simply the aggregate of individual maintenance levels at all locations within the neighborhood, i.e., there is no spatial decay within the neighborhood. In a linear neighborhood aligned along one dimension D from zero to one, we can then simply write:

$$E(D) = \int_0^1 I(D)d(D) = I$$

Taking the total derivative we can rewrite the price equation as follows:

$$dP = \left(\frac{\partial P}{\partial I(S)} + \frac{\partial P}{\partial E(S)}\right) dI(S) + \frac{\partial P}{\partial L} dL + \frac{\partial P}{\partial Y} dY$$

Or:

$$dP = \left(\frac{\partial P}{\partial I} + \frac{\partial P}{\partial E}\right) \frac{\partial I}{\partial S} dS + \frac{\partial P}{\partial L} dL + \frac{\partial P}{\partial Y} dY$$

To identify the effect of the policy on property values, we essentially employ the difference-in-differences methodology that compares the value of properties at different points in time (first difference Δ) and at different locations (second difference d). We assume that a change in policy ΔS has an effect only on properties in a treatment neighborhood *(T)* but not in an otherwise comparable control neighborhood *(C)* that is subject to the same macroeconomic shocks ($\Delta Y^T = \Delta Y^C$).[5]

Our treatment effect can be described as follows:

$$\beta = (P(S = 1)^{POST} - P((S = 0)^{PRE})^T - (P(S = 0)^{POST} - P(S = 0)^{PRE})^C$$

Or:

$$\beta = \Delta P^T - \Delta P^C$$

5 In the empirical implementation, we introduce a buffer around the treated areas to ensure that the control group is not affected by the treatment through spillover effects.

If we assume L to be time-invariant at any location, i.e., $\Delta L=0$, our treatment effect is defined as follows:

$$\beta = \left(\left(\frac{\partial P}{\partial I} + \frac{\partial P}{\partial E}\right)\frac{\partial I}{\partial S}\Delta S + \frac{\partial P}{\partial Y}\Delta Y\right)^T - \left(\left(\frac{\partial P}{\partial I} + \frac{\partial P}{\partial E}\right)\frac{\partial I}{\partial S}\Delta S + \frac{\partial P}{\partial Y}\Delta Y\right)^C$$

Or:

$$\beta = \left(\frac{\partial P}{\partial I} + \frac{\partial P}{\partial E}\right)\frac{\partial I}{\partial S}, \qquad \text{where } \Delta S = \begin{cases} 1 \text{ if treated} \\ 0 \text{ if control} \end{cases}$$

There are important implications for our empirical strategy that aims to estimate β. Given an appropriately defined control group, the difference-in-differences coefficient identifies a composite effect determined by the impact of the policy on maintenance levels in the neighborhood ($(\partial I/\partial S)\,\Delta S$), and the valuation of *internal quality* ($\partial P/\partial I$) and the *housing externality* ($\partial P/\partial E$) if the effect of internal housing quality is not held constant in an empirical model. To the extent that the interior quality effect can be held constant empirically ($(\partial P/\partial I)\Delta S = 0$), the treatment reflects the externality effect caused by the policy ($\beta = (\partial P/\partial E)\,(\partial I/\partial S)$). With the data we have at hand, we are able to hold the interior quality effect constant by restricting the transactions sample to properties in good condition.

4.2 Empirical strategy

For a given year since designation, our baseline treatment estimate reflects the cumulative effect of the improvement in the maintenance condition of a sold property *i* on the price of *i* and the external effect of the improvements in all other properties *j* in the same neighborhood as *i* on the price of *i*. Unlike the theoretical example, the externality of buildings *j* and *i* is discounted by distance D_{ij} and may include the social externality of new residents moving into upgraded buildings:

$$\beta_V = \frac{\partial P}{\partial I}\frac{\partial I_{iV}}{\partial S_{iV}} + \frac{\partial P}{\partial E}\sum_j \frac{\partial I_{jV}}{\partial S_{jV}}\tau(D_{ij}), \qquad \text{where } \tau(D) > 0 \text{ and } \tau'(D) < 0$$

Building quality

In a first alternation to the baseline specification presented in the main paper, we only consider buildings in good condition to hold the quality of the traded buildings constant ($dI_i = \partial I_i/\partial S_i = 0$). Hence the estimated treatment effect collapses to $\beta = (\partial P/\partial E)\sum_j \partial I_j/\partial S_j\, \tau(D_{ij})$. We choose to restrict the sample to properties in good condition (as opposed to poor condition), as it is less likely that these buildings will be renovated shortly after the transaction. It is therefore also less likely that anticipated tax abatements or renovation subsidies are capitalized in the sales price. With this approach, theoretically, we only capture the effects of improvements in the quality of buildings *j* on the price of a sold building *i* and, hence, a housing externality promoted by the policy. In practice, this approach to separating the internal and the external maintenance effect comes with some limitations. First, our data set offers information on whether a property, at the time of the transaction, was in a particularly good or poor condition. While this is significantly more information than available in most comparable data sets, this is also evidently a highly aggregated data. Second, we have assumed that there are no policy effects on neighborhood quality other than through housing externalities. If there are significant direct investments in the quality of local public goods, e.g., the renovation of schools or playgrounds, these location features become a function of the policy. Adding these features $Q_q(S)$ to the original price equation results in an additional component in the treatment effect we measure:

$$\beta_V = \left(\frac{\partial P}{\partial I}\frac{\partial I_{iV}}{\partial S_{iV}} + \right)\frac{\partial P}{\partial E}\sum\nolimits_j \frac{\partial I_{jV}}{\partial S_{jV}}\tau\left(D_{ij}\right) + \sum\nolimits_q \frac{\partial P}{\partial Q_q}\frac{\partial Q_{jq}}{\partial S_{jq}}$$

As such improvements in $Q_q(S)$ are difficult to observe, it is difficult to separate them from the housing externalities. We employ an alternative approach to measuring housing externalities, focusing on spillovers into areas just outside renewal areas. This approach, which is described next, is closer to RH. It suffers, however, from a similar problem in that it is difficult to separate the housing externality spillover effect from an accessibility effect to improved local public goods in nearby areas. In practice, this interpretation problem is mitigated by the fact that both approaches consistently indicate that the joint neighborhood effect (housing externality and local public goods effect) are fairly limited. Irrespective of this problem, a significant reduction in the treatment effect when holding building quality constant indicates the presence of a significant internal capitalization effect.

Spillover effects

One of the advantages of the above approach is that we aim at measuring policy-induced housing externalities where they are presumably strongest, i.e., within renewal areas. One of the problems with this approach, as discussed, is that the information we use on building maintenance is imperfect. We therefore employ an alternative approach in which we focus on areas just outside the designated renewal areas. While attenuated, housing externalities should still be present in these areas. Moreover, any price effect will not be confounded with the policy effect on the internal quality of buildings because the respective areas did not qualify for subsidies. The treatment effect we estimate, hence, depends purely on the valuation of the housing externality and the policy effect on the maintenance level of buildings *j* in a nearby renewal area, discounted by distance *D*:

$$\beta_V = \frac{\partial P}{\partial E}\sum_j \frac{\partial I_{jV}}{\partial S_{jV}}\tau(D_{ij}), \qquad \text{where } \tau(D) > 0 \text{ and } \tau'(D) < 0$$

This approach also mitigates another concern, namely, that the authorities reserve the right to levy the increase in land value generated by the policy ("Ausgleichsabgabe"). By the end of 2011, local authorities had generated €68 million ($93.3 million) in levies. The total expected levies estimated by the local administration amounted to €211m ($285.3m) based on an estimated average increase in land value of €45 ($60.8) per m^2 (Senatsverwaltung für Stadtentwicklung Berlin, 2012), which are strikingly low figures compared to the above-mentioned investment volumes.[6] Property prices could be negatively affected.

To detect spillovers, we alter the definition of the treatment *T* measure and the control groups relative to the benchmark specification (see equation 1 in the main paper). In the first alteration, we redefine our treatment measure as a binary variable that takes the value of $T_{S1i}=1$ if a property falls within a 500m buffer area and zero otherwise. We run this specification using the two treatment functions introduced above and varying control groups.

Control groups

For the spillover models described above we define a second set of control groups (A-I to A-IV), where we employ 6,600 transactions that are located in a 500 meter radius around the renewal areas as a treatment, and compare them to all other transactions (A-I), to all transactions in a 500 to 2,000 meter radius around the renewal areas (A-II, includes 12,800 obs.), to the investigation areas plus a 1,000 meter buffer around them (A-III, includes 14,700 obs.), and to a matched group (A-IV, includes 6,000 obs.). We use the same PSM matching technique as described in section 2.3 in the main paper to find matched pairs for the transactions within a 500m buffer around the renewal areas. Transactions inside the renewal areas are completely excluded from the sample for the estimations of the spillover effects. Table A16 reports measures of the balance of the covariates for the matched control group.

– Table A16 about here –

[6] All income generated through this source is reinvested in the district's infrastructure or in neighborhood improvements.

4.3 Empirical results

Building quality

Table A17 replicates our baseline approach (Table 4 in the main paper) using only buildings in good physical condition. When using control groups I (all properties outside renewal areas) and II (properties outside and within 2km of renewal areas) we find effects that are economically close to zero and statistically not distinguishable from zero. This is in stark contrast to the large and significant effects found for all properties (reported in Table 4, columns (1) and (2) in the main paper). With our preferred control groups III (investigation areas) and IV (matched properties) the standard errors increase substantially as the number of observations decrease. The point estimates are, again, not statistically distinguishable from zero, but given the large magnitudes and inconsistent directions this seems primarily attributable to limited degrees of freedom. Given the already limited degrees of freedom we also do not estimate the demanding model with time-varying effects on this sample. Still, a comparison of columns (1) and (2) in particular to the respective Table 4 in the main paper suggests that the benchmark results are not primarily driven by externality effects.

– Table A17 about here –

Spillover effects

Table A18 and Figure A6 replicate the benchmark analysis for the spillover areas, i.e., the 500m buffer adjacent to the renewal areas. As the external areas have not been targeted by the policy, housing externalities can be identified using all buildings irrespective of their maintenance condition. The 500m buffer area previously excluded due to the presence of spillovers now serves as a treatment group to detect spillover effects. Lower thresholds generally yield similar results but suffer from a loss of degrees of freedom.

For control groups (A-I and A-III), we find effects that correspond to about one-fourth (3) to slightly less than one half (1) of the corresponding renewal area effects reported in Table 4 in the main paper. While statistically significant, the effect on the renewal area buffer with our preferred control group III is no more than 0.7% per year. Moreover, the time-varying buffer area results reported in Figure 6 cast some doubt on the nature of the treatment effect captured by model (1). As long as 10 years after designation the treated follow exactly the same trend as the control group, which is remarkable given the naïve definition of control group I. After 10 to 12 years, the trends suddenly start to diverge. This divergence coincides with the appreciation of central areas, which started in 2004 as revealed by Figure A3, which indicates that the treatment effect could be spurious. This impression is substantiated by the significant reduction in the long-run treatment effect once we control for time-varying effects.

The outlier in Table 18 is model (4) which, using group IV, yields the surprising result of a treatment effect on the renewal area buffer which is almost as large as the effect on the renewal area itself (Table 4 in the main paper). This is not a conclusive result because even in the presence of strong housing externalities the typically steep spatial decay should imply a significantly smaller treatment effect on the buffer area (Rossi-Hansberg et al., 2010). It thus seems likely that the effects in column (4) – and therefore also in the respective model in Table 4 in the main paper – are driven by some unobserved heterogeneity that affects property price trends over time. This interpretation is supported by the model controlling for such trends by means of time-varying effects in column (6), which yields insignificant results. Controlling for time-varying treatment effects we even find a small but significant negative effect when using control group III (column 5). Taken together, we do not find conclusive evidence of the presence of spillover effects, which is in line with the limited multiplier effects of the policy and the insignificant capitalization effects for properties in good condition.

– Table A18 about here –

– Figure A6 about here –

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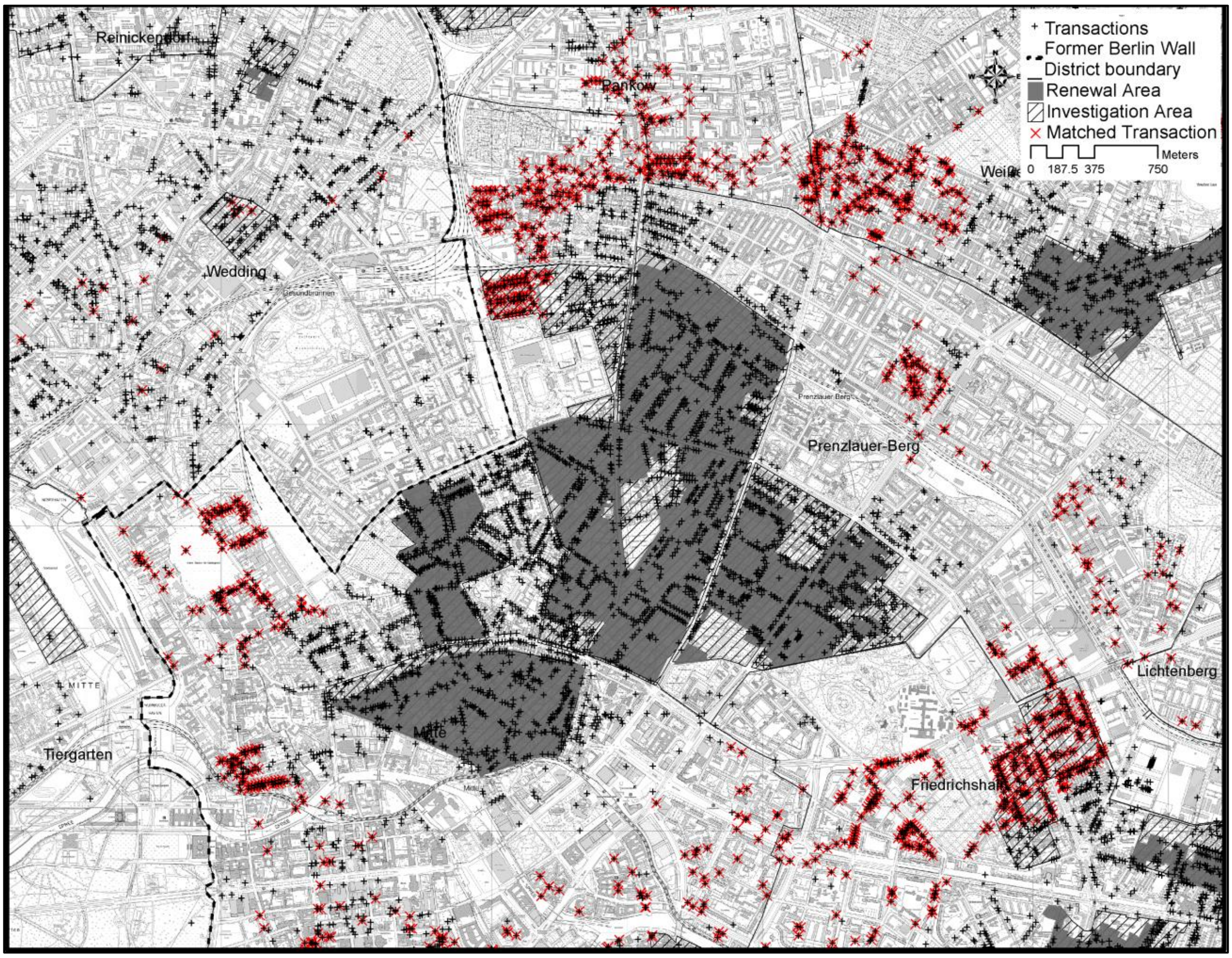
Figures

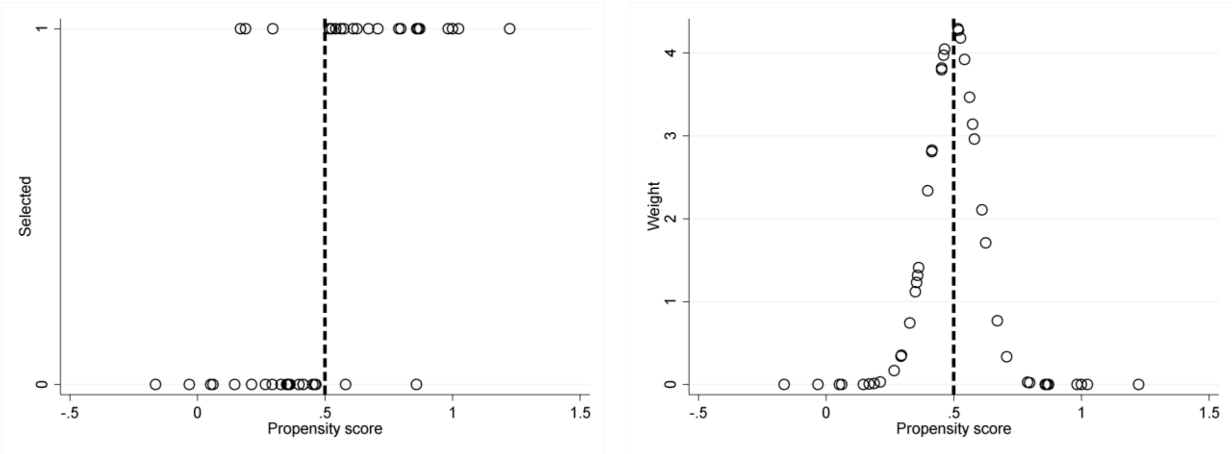
Fig. A1. Snapshot renewal areas



Notes: Own illustration based on the urban and environmental information system (Senatsverwaltung für Stadtentwicklung Berlin, 2006). Dark shaded (hatched) areas indicate renewal (investigation) areas. Black (red) crosses indicate (matched) transactions (in control group IV).

Fig. A2. Designation propensity and Gaussian kernel weights

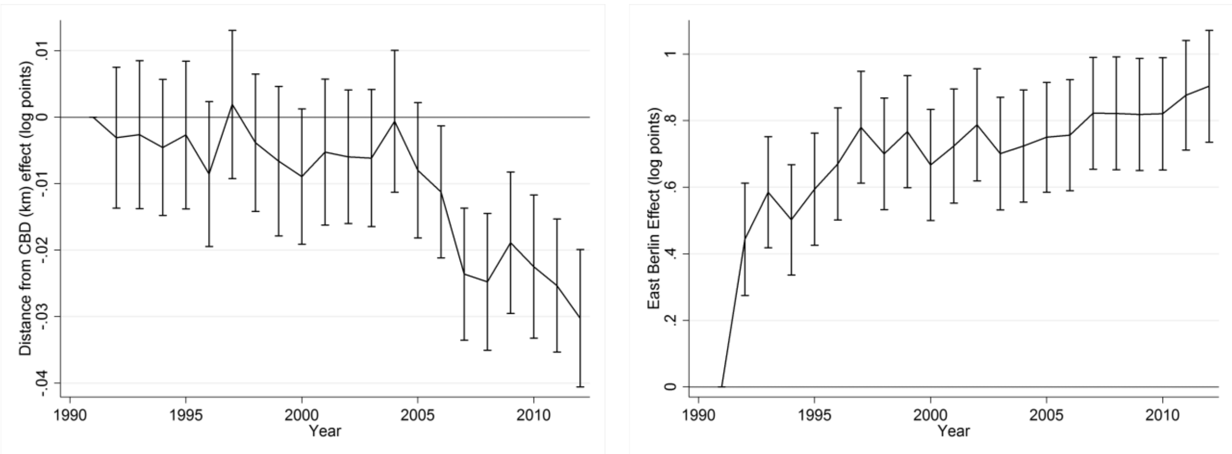
Prediction and cut-off point *Weight distribution*



Notes: The propensity score are the predicted values from regressions of a 0,1 selection variable (being selected or not being selected as a renewal area) on a range of area characteristics capturing the socio-economic conditions in 1992. The weight function uses a Gaussian kernel based on distance from the 0.5 cut-off value (dashed lines) and a bandwidth selected according to the Silverman-rule.

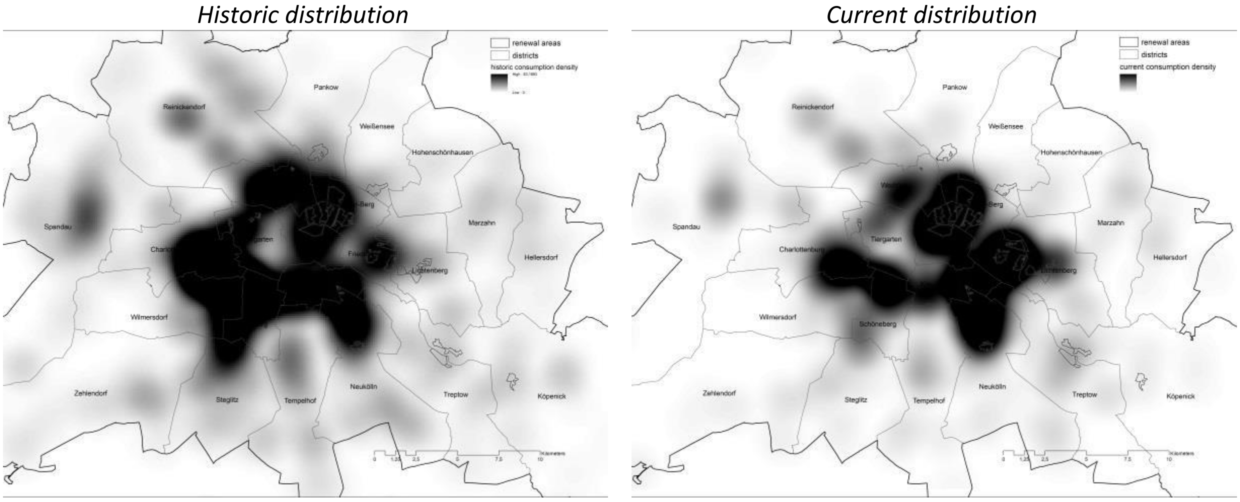
Fig. A3. Time-varying distance from CBD and East Berlin effects

Distance to CBD *East Berlin*



Notes: Figures plot interaction effects of distance from CBD (left) and an East Berlin dummy (right) and year effects, normalized to 1991. The estimates are from a regression similar to the model reported in Table (column 6) in the main paper, where we replace the district x year effects with east x year effects.

Fig. A4. Kernel-smoothed density surfaces comparison

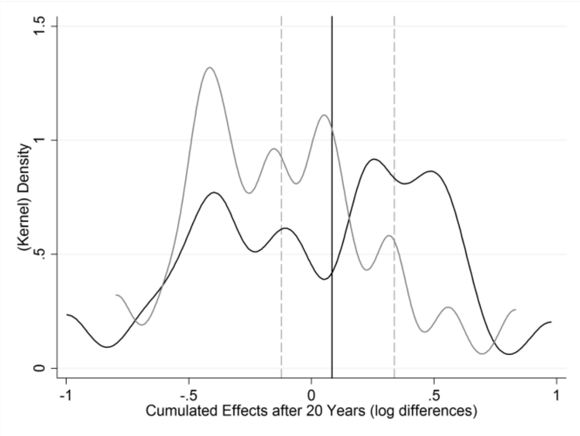


Notes: Notes: Own illustration based on the urban and environmental information system (Senatsverwaltung für Stadtentwicklung Berlin, 2006). Smooth gray-shaded areas represent the consumption amenity density in 1995/96 (left panel) and 2012 (right panel).

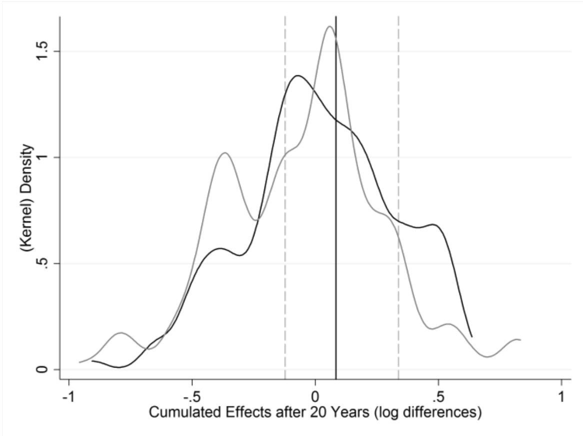
Fig. A5. Distribution of simulated point estimates – including time-varying effects

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

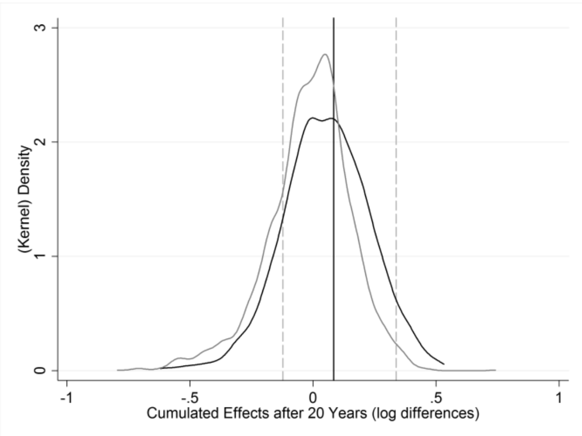
1 random area selected (1 treated vs 22 control)



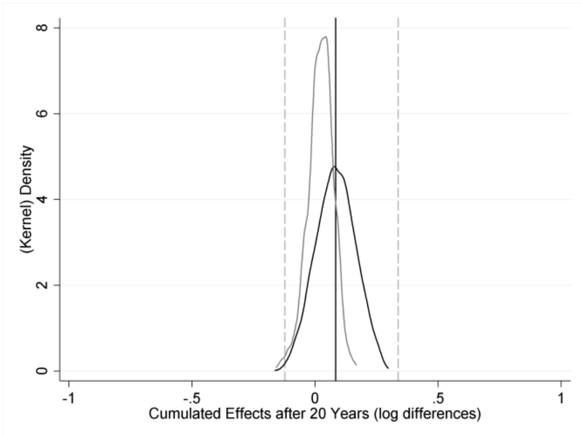
2 random areas selected (2 treated vs 22 control)



5 random areas selected (5 treated vs 22 control)

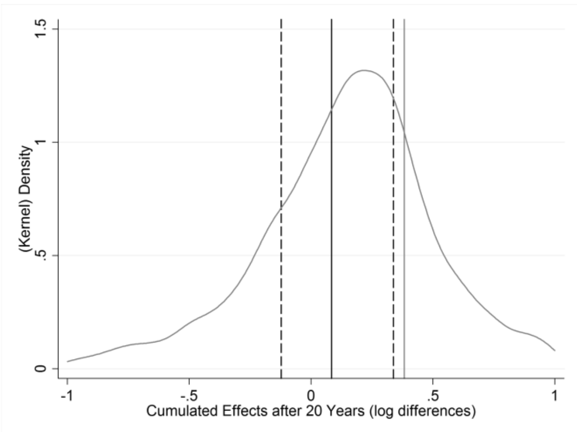


15 random areas selected (15 treated vs 22 control)

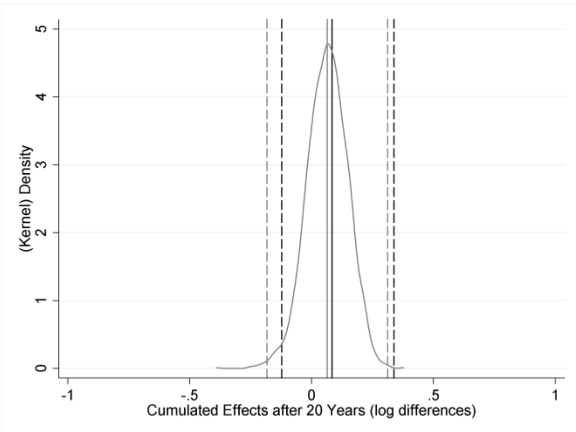


Simultaneously varying number of treatment & control areas

5 random areas selected (5 treated vs 5 control)

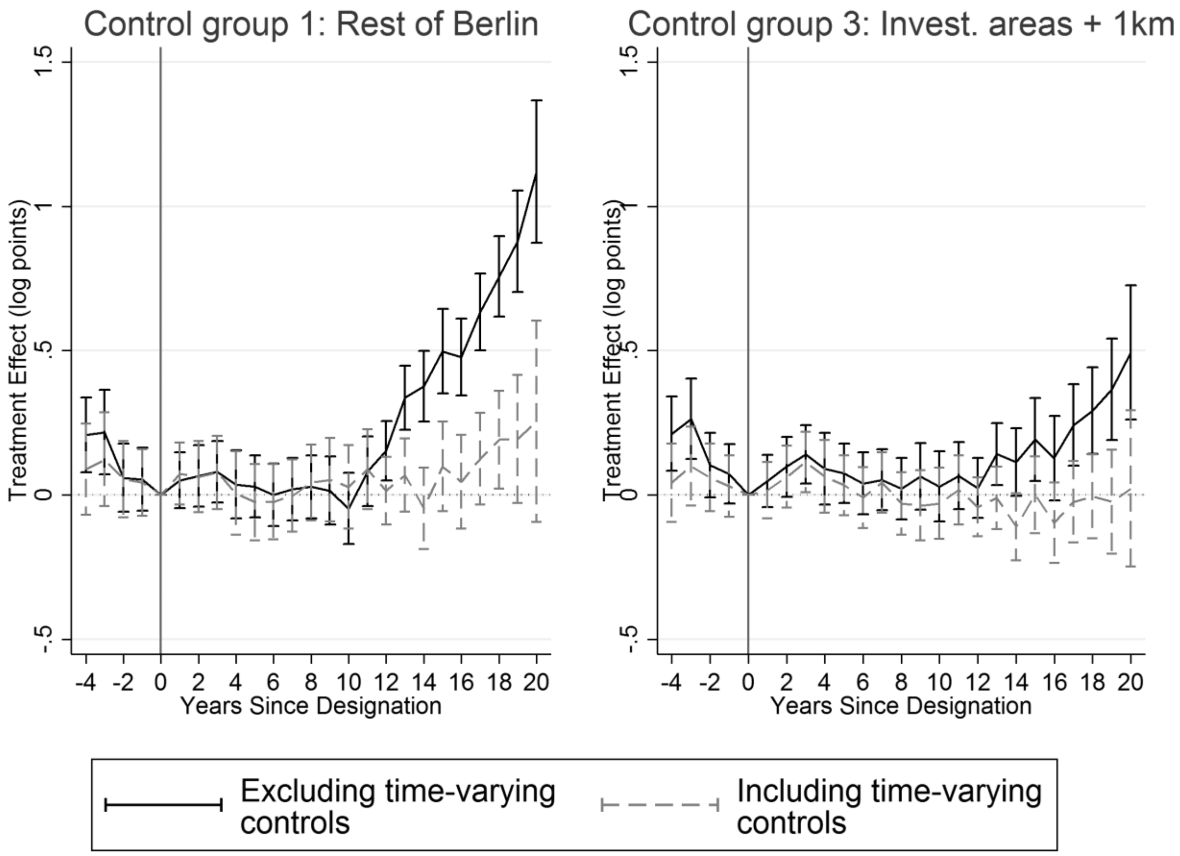


15 random areas selected (15 treated vs 15 control)



Notes: 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).

Fig. A6. Price trends in spillover areas, relative to varying control groups



Notes: Figure illustrates the renewal spillover effects on a 500m buffer area surrounding renewal areas. Estimates are based on the equation described in section (4), 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 A18, column 1 (solid left), column 3 (solid right), and, column 5 (dashed-right). The parametric equivalent to the light dashed line in the left panel is not reported in Table 18 to save space. Error bars indicate the 95% confidence intervals.

Tables

Tab A1. Descriptive statistics renewal areas

Name	Start	End	Area (km2)	Properties	Dwelling units	Residents
Samariterviertel	09.10.1993	10.02.2008	0.339	263	5,302	8,324
Warschauer Strasse	04.12.1994	28.04.2011	0.381	227	5,110	8,599
Traveplatz Ostkreuz	04.12.1994	11.07.2010	0.351	204	4,380	6,964
Kaskelstrasse	04.12.1994	10.02.2008	0.221	248	1,665	3,394
Weitlingstrasse	04.12.1994	28.01.2009	0.503	331	4,214	5,337
Spandauer Vorstadt	09.10.1993	10.02.2008	0.671	632	5,809	8,771
Beusselstrasse	04.12.1994	21.02.2007	0.106	93	2,314	3,045
Rosenthaler Vorstadt	04.12.1994	28.01.2009	0.376	373	4,809	6,794
Stephankiez	10.11.1995	21.02.2007	0.063	54	1,288	1,860
Soldiner Strasse	10.11.1995	21.02.2007	0.019	11	447	661
Wederstrasse	10.11.1995	11.07.2010	0.246	233	1,341	2,079
Kottbusser Damm Ost	10.11.1995	21.02.2007	0.025	21	380	522
Kollwitzplatz	09.10.1993	28.01.2009	0.607	476	6,519	11,412
Helmholtzplatz	09.10.1993	09.12.2014	0.819	560	13,338	21,211
Winsstrasse	04.12.1994	28.04.2011	0.348	219	4,850	8,568
Wollankstrasse	04.12.1994	28.04.2011	0.685	338	3,386	7,719
Teutoburger Platz	04.12.1994	12.02.2013	0.498	316	4,432	7,950
Komponistenviertel	04.12.1994	11.07.2010	0.339	477	3,443	7,400
Boetzowstrasse	10.11.1995	28.04.2011	0.381	191	3,072	6,211
Altstadt Kiez Vorstadt	09.10.1993	21.02.2007	0.351	225	1,105	2,115
Niederschöneweide	04.12.1994		0.221	97	799	1,368
Oberschöneweide	10.11.1995	11.07.2010	0.503	255	3,465	5,375

Notes: The data for area, properties, dwelling units, and residents are from the Berlin administrative unit for urban development and environment (Senatsverwaltung für Stadtentwicklung und Umwelt, 2007). The Renewal Area "Teutoburger Platz" was deregulated at the end of our observation period (August 2012). The data for the areas "Komponistenviertel" and "Niederschöneweide" are from 2010.

Tab A2. Descriptive statistics: Matched control group IV

Balancing of the covariates

Variable	Sample	Mean Treated	Control	standardized bias (%)	% reduction in abs. bias
Age	Unmatched	100.81	59.704	136.9	
	Matched	101.5	92.186	31	77.3
East / west	Unmatched	0.04238	0.64936	-165.7	
	Matched	0.04264	0.07909	-10	94
Longitude	Unmatched	27282	23782	55.4	
	Matched	27271	28781	-23.9	56.8
Latitude	Unmatched	21874	19423	43.9	
	Matched	21900	21080	14.7	66.5
Index of locational quality	Unmatched	2.5171	3.7574	-61.4	
[1, poor to 6, very good]	Matched	2.7627	3.1517	-19.2	68.6
Typical floor space index	Unmatched	2.2635	1.0455	163.9	
	Matched	2.263	1.7998	62.3	62
Land use: residential	Unmatched	0.86687	0.84722	5.6	
	Matched	0.9545	0.90186	15	-167.9
Land use: commercial	Unmatched	0.02761	0.01855	6	
	Matched	0.02978	0.06051	-20.5	-239.1
Distance to CBD [m]	Unmatched	4705.4	9250.6	-120.3	
	Matched	4697	6698.9	-53	56
Distance to park [m]	Unmatched	2138.2	1695.4	39	
	Matched	2132.2	1801.8	29.1	25.4
Distance to main street	Unmatched	127.43	198.62	-40.4	
[m]	Matched	127.63	125.6	1.2	97.1
Distance to water [m]	Unmatched	1406.7	1594.7	-16.6	
	Matched	1399.3	1192.6	18.3	-10

Notes: The propensity scores are computed using nearest neighbor matching. Following Rosenbaum & Rubin (1985) and Leuven & Sianesi (2003), the standardized bias is the difference between the sample means in the subsamples (treated and control), computed as the percentage of the square root of the average of the sample variances in the treated and control groups. Imposing a common support restriction does not substantially change the results of the later analyses where we use the matched observations as a control group

Tab A3. Descriptive statistics

Variable	Mean	Std. Dev.	Min	Max
Price [constant 2012 €]	1,495,060	5,356,786	16,631	299,000,000
Plot area [m²]	1682.062	6086.808	150	205222
Floor space index [floor space/plot area]	1915.712	6105.441	65	191375
Age [years]	65.51245	37.42346	0	294
West / east indicator	0.6099851	0.4877562	0	1
Residential area indicator	0.8415044	0.3652073	0	1
Commercial area indicator	0.0287225	0.1670266	0	1
industrial area indicator	0.0323323	0.1768823	0	1
Distance to main street [m]	182.3591	207.0289	0	2140.739
Distance to public rail transport [m]	980.7227	988.5591	10.0361	9381.628
Distance to open water [m]	1515.542	1297.361	0	8316.602
Distance to park [m]	1786.17	1377.644	0	5972.606
Distance to playground [m]	325.5659	318.355	10.34	6209.051
Distance to listed building [m]	230.9044	270.7093	0.2341669	2829.887
Street noise level [db]	57.42288	9.529247	15.0819	94.5513
Location within block				
Building at street front [%]	73.34			
Building at a corner [%]	13.98			
Building with multiple fronts [%]	3.89			
Hammer type building [%]	1.41			
Building in inner block loc. [%]	6.66			
Other [%]	4.61			

Tab A4. Policy effects on property prices: Complete results

Control group	(1) 0.5 km - ∞ buffer(I)	(2) 0.5 - 2 km buffer (II)	(3) Investigation areas (III)	(4) Investigation areas (III)	(5) Matched observations (IV)	(6) Investigation areas (III)	(7) Matched observations (IV)
T x POST (β_0)	-0.162***	-0.114***	-0.040	-0.032	-0.140***	-0.073	-0.027
	(0.036)	(0.037)	(0.040)	(0.050)	(0.053)	(0.048)	(0.061)
T x V (β_1)	0.048***	0.039***	0.022***	0.022***	0.025***	0.004	0.006
	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
Building age (10 years)	-0.144***	-0.122***	-0.129***	-0.168***	-0.113***	-0.122***	-0.106***
	(0.006)	(0.015)	(0.018)	(0.024)	(0.024)	(0.018)	(0.025)
Building age (10 years),	0.001***	0.001***	0.001***	0.001***	0.000***	0.000***	0.000**
Squared	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Industrial	0.127*	-0.115	0.190	0.177	0.252	0.251	0.408*
area indicator	(0.072)	(0.106)	(0.206)	(0.395)	(0.222)	(0.214)	(0.244)
Residential	0.022	-0.084	-0.038	-0.085	0.031	0.027	0.162
area indicator	(0.048)	(0.079)	(0.129)	(0.209)	(0.148)	(0.133)	(0.160)
Commercial	0.358***	0.222**	0.212	0.154	0.333*	0.217	0.427**
use indicator	(0.061)	(0.090)	(0.159)	(0.235)	(0.173)	(0.164)	(0.185)
Plot area (100 sqm)	0.002**	0.003	0.000	-0.000	0.011**	-0.001	0.011*
	(0.001)	(0.003)	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)
Floor space (100 sqm)	0.009***	0.014***	0.023***	0.018***	0.011**	0.023***	0.011**
	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.003)	(0.005)
Building at a	0.207***	0.233***	0.201*	0.268	0.318***	0.204**	0.308***
Corner	(0.041)	(0.066)	(0.106)	(0.205)	(0.105)	(0.103)	(0.103)
Build. with	0.346***	0.112	0.185	0.351	0.070	0.211	0.088
mult. fronts	(0.065)	(0.102)	(0.166)	(0.245)	(0.162)	(0.160)	(0.159)
Hammer type	-0.108**	-0.209	0.064	0.337	-0.448	0.126	-0.445
Building	(0.043)	(0.133)	(0.236)	(0.428)	(0.338)	(0.280)	(0.311)
Build. in inner	-0.133***	-0.270***	-0.398***	-0.588**	-0.421***	-0.479***	-0.472***
block loc.	(0.042)	(0.085)	(0.153)	(0.281)	(0.146)	(0.151)	(0.147)
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	0.536	0.502	0.267	0.304	0.324	0.321	0.371
AIC	23573.1	1236.1	491.7	459.8	972.3	448.1	881.1

Notes: Expanded version of Table 4 in the main paper. 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 A5. Correlations between collected and archived data

Data available for all invest. areas	Data from Berlin Archive	Correlation
No. green areas per km2	Endowment index green areas	0.503***
No. schools/playgrounds per km2	Endowment index schools / playgrounds	0.561***
Distance to public transport	Endowment index public transport	-0.588***
Share oven heating	Share oven heating	0.685***
Share without restroom	Share without restroom	-0.340
Share with WC outside	Share with WC outside	0.0720
Average floor space index	Average floorspace index	0.684***
Share aged 15-45	Share aged 15-45	0.177

Notes: The correlations are calculated for the subset of the investigation areas, which are described in the records available from the Berlin Stata Archive. This includes data for 33 of the 44 investigation areas. The endowment index is a 4 scale index ranging from 1=very bad to 4=very good.

Tab A6. Policy effects on propensity of buildings being in poor and good condition: Varying kernel weights

	(1)	(2)	(3)	(4)	(5)	(6)
	Propensity: Poor	Propensity: Poor	Propensity: Poor	Propensity: Good	Propensity: Good	Propensity: Good
Control group	Investigation areas (III)	Investigation areas (III)	Investigation areas (III)	Investigation areas (III)	Investigation areas (III)	Investigation areas (III)
T x POST (β_0)	-0.025	0.002	-0.005	0.013	-0.024	0.015
	(0.062)	(0.041)	(0.068)	(0.022)	(0.016)	(0.024)
T x V (β_1)	-0.010**	-0.011***	-0.011*	0.008***	0.009***	0.009***
	(0.005)	(0.003)	(0.005)	(0.003)	(0.002)	(0.003)
Cumulated effect after 20 years (%)	-44.97**	-42.65***	-42.32*	94.29***	86.49***	104.81***
	(18.90)	(12.22)	(21.71)	(27.36)	(20.79)	(31.20)
Compound annual growth rate (%)	-2.94	-2.74	-2.71	3.38	3.17	3.65
T (ever designated)	YES	YES	YES	YES	YES	YES
T x A (released from program)	YES	YES	YES	YES	YES	YES
Hedonic controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Block x area fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Weights	Gaussian	Gaussian	Binary	Gaussian	Gaussian	Binary
Bandwidth	Optimal x 0.5	Optimal x 2	0.05	Optimal x 0.5	Optimal x 2	0.05
Observations	7,420	7,841	2,034	7,420	7,841	2,034
R^2	0.266	0.284	0.258	0.330	0.290	0.340
AIC	8281.3	8326.4	2392.4	-249.9	735.4	-70.5

Notes: Dependent variable is a 0,1 dummy variable that is one if at the time of transaction a property was in poor (1–3) or good (4–6) 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: $\exp(\beta_0 + \beta_1 \times 20) - 1$. 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. 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. Block x area fixed effects are statistical block x renewal area x investigation area fixed effects. Gaussian weighted models are weighted by the distance from the designation cut-off along a latent deprivation variable. Optimal bandwidth selected according to the Silverman-rule. In binary weighted models weights are zero if the designation propensity is within the bandwidth of the cut-off value (0.5). Standard errors in parentheses are clustered on block x area effects level in all models. * p < 0.1, ** p < 0.05, *** p < 0.01.

Tab A7. Policy effects on property transaction prices: Varying kernel weights

	(1)	(2)	(3)	(4)	(5)	(6)
Control group	Investigation areas (III)	Investigation areas (III)	Investigation areas (III)	Investigation areas (III)	Investigation areas (III)	Investigation areas (III)
T x POST (β_0)	-0.035	-0.032	-0.047	-0.058	-0.028	-0.019
	(0.057)	(0.050)	(0.045)	(0.064)	(0.054)	(0.050)
T x V (β_1)	0.019***	0.022***	0.021***	0.018***	0.022***	0.020***
	(0.006)	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)
Cumulated effect after 20 years (%)	40.22***	50.82***	45.59***	34.93***	50.28***	46.50***
	(10.09)	(9.21)	(8.74)	(11.62)	(10.87)	(10.29)
Compound annual growth rate (%)	1.70	2.08	1.90	1.51	2.06	1.93
T (ever designated)	YES	YES	YES	YES	YES	YES
T x A (released from program)	YES	YES	YES	YES	YES	YES
Hedonic controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Block x area fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Weights	Gaussian	Gaussian	Gaussian	Binary	Binary	Binary
Bandwidth	Optimal x 0.5	Optimal	Optimal x 2	.05	.1	.2
Observations	7,420	7,841	7,841	2,034	2,787	4,775
R^2	0.642	0.635	0.630	0.656	0.640	0.635
AIC	9152.0	10895.7	11403.7	2478.1	3547.4	7127.0

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. 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. Block x area fixed effects are statistical block x renewal area x investigation area fixed effects. Gaussian weighted models are weighted by the distance from the designation cut-off along a latent deprivation variable. Optimal bandwidth selected according to the Silverman-rule. In binary weighted models weights are zero if the designation propensity is within the bandwidth of the cut-off value (0.5). Standard errors in parentheses are clustered on block x area effects in all models. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Tab A8. Policy effects on number of transactions

	(1)	(2)	(3)	(4)	(5)	(6)
Control group	All (I)	1.5- 2 km buffer (II)	Investigation areas (III)	Matched observations (IV)	Investigation areas (III)	Matched observations (IV)
T x POST (β_0)	0.121***	0.119***	0.084*	-0.005	-0.139**	-0.130
	(0.040)	(0.043)	(0.050)	(0.068)	(0.068)	(0.085)
T x V (β_1)	-0.025***	-0.023***	-0.015***	-0.015***	-0.003	-0.007*
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Cumulated effect after 20 years (%)	-31.4***	-28,8***	-20.2***	-25.7***	-18.71**	-24.0**
	(4.9)	(5.4)	(6.31)	(8.60)	(8.45)	(11.2)
Compound annual growth rate (%)	-1.90	-1.7	-1.11%	-1.47	-1.03	-1.36
T (ever designated)	YES	YES	YES	YES	YES	YES
T x A (released from program)	YES	YES	YES	YES	YES	YES
Hedonic controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Block x area fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Time-varying effects	-	-	-	--	YES	YES
Observations	45,600	10,781	4,527	5,180	4,527	5,180
R^2	0.333	0.341	0.278	0.363	0.356	0.416
AIC	31,922.1	10,185.1	5,175.2	4,500.1	5,195.4	4,572.9

Notes: Dependent variable is log of number of transactions within a block-year cell in all models. Estimation method is 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. Standard errors in parentheses are clustered on Block x area fixed effects level in all models. * p < 0.1, ** p < 0.05, *** p < 0.01.

Tab A9. Policy effects on standard land values

	(1)	(2)	(3)	(4)	(5)	(6)
Control group	All (I)	1.5- 2 km buffer (II)	Investiga-tion areas (III)	Matched observations (IV)	Investigation areas (III)	Matched observations (IV)
T x POST (β_0)	0.528***	0.364***	0.190***	0.0311	0.00564	0.0545**
	(0.0284)	(0.0287)	(0.0346)	(0.0351)	(0.0189)	(0.0228)
T x V (β_1)	0.00207	0.0104***	0.0128***	0.00607***	-0.00353**	-0.00652***
	(0.00142)	(0.00163)	(0.00198)	(0.00194)	(0.00141)	(0.00178)
Cumulated effect after 20 years (%)	76.78***	77.09***	56.4***	16.47***	-6.3**	-7.31
	(4.47)	(4.62)	(5.92)	(5.84)	(2.97)	(4.54)
Compound annual growth rate (%)	2.89	2.90	2.26	0.77	-0.32	-0.38
T (ever designated)	YES	YES	YES	YES	YES	YES
T x A (released from program)	YES	YES	YES	YES	YES	YES
Hedonic controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Block x area fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Time-varying effects	-	-	-	-	YES	YES
Observations	63,975	17,301	7,831	8,853	7,831	8,853
R^2	0.948	0.917	0.866	0.937	0.952	0.963
AIC	-58,763.9	-8,231.9	-3,327.8	-7,186.7	-10,753.7	-11,446.6

Notes: Dependent variable is log of assessed land value in all models. Estimation method is 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. Standard errors in parentheses are clustered on block x area fixed effects in all models. * p < 0.1, ** p < 0.05, *** p < 0.

Tab A10.Policy effects on property prices: Pre-trend evaluation

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Condition poor		Condition good		Log of price	
Control group	Invest. areas (III)	Invest. areas (III)	Invest. areas (III)	Invest. areas (III)	Invest. areas (III)	Invest. areas (III)
T x POST	-0.029	0.023	-0.022	0.003	-0.052	-0.053
	(0.050)	(0.067)	(0.015)	(0.015)	(0.056)	(0.068)
T x V	-0.012***	-0.009**	0.007***	0.003	0.022***	0.004
	(0.003)	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)
T x V_PRE	0.021	0.019	-0.005	-0.017**	0.007	-0.013
	(0.022)	(0.026)	(0.007)	(0.008)	(0.026)	(0.031)
Cumulated effect after 20 years (%)	-24.26***	-13.76	12.76***	5.54	46.85***	3.54
	(7.19)	(9.80)	(3.63)	(4.23)	(9.54)	(10.04)
Compound annual growth rate (%)	-1.38	-0.74	0.60	0.27	1.94	0.17
T (ever designated)	YES	YES	YES	YES	YES	YES
T x A (released from program)	YES	YES	YES	YES	YES	YES
Hedonic controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Block x area fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Time-varying effects	-	-	-	-	-	YES
Observations	7,841	7,841	7,841	7,841	7,841	7,841
R^2	0.300	0.351	0.267	0.321	0.635	0.682
AIC	8,003.6	7,959.4	493.6	450.7	11,275.0	10,746.2

Notes: Dependent variable is log of property price in all models. Estimation method is OLS. T is a 0,1 dummy variable denoting a property location within a renewal area. A similarly denotes whether the nearest area has been released from the program. *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: $\exp(\beta_0 + \beta_1 \times 20) - 1$. Hedonic and location controls consist of covariates controlling for internal property and external location characteristics described in greater detail in the data sections of the main paper 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 district effects, East Berlin effects and a consumption amenity measure described in the data section of the main paper. Except for adding *T x V_PRE,* columns (1) and (2) are identical to columns (3) and (6) of Table 2, columns (3) and (4) identical to columns (3) and (6) of Table 3, and columns (5) and (6) identical to columns (3) and (6) of Table 4. Standard errors in parentheses are clustered on block x area fixed effects. * p < 0.1, ** p < 0.05, *** p < 0.01. * p < 0.1, ** p < 0.05, *** p < 0.01.

Tab A11.Renewal area effects and the influence of modeling time-varying effects

	(1)	(2)	(3)	(4)	(5)	(6)
Control group	Investi-gation areas (III)	Matched observa-tions (IV)	Investiga-tion areas (III)	Matched observa-tions (IV)	Investiga-tion areas (III)	Matched observa-tions (IV)
T x POST	-0.108**	-0.085	-0.009	-0.049	-0.069	-0.057
	(0.048)	(0.056)	(0.040)	(0.053)	(0.043)	(0.054)
T x V	0.009**	0.021***	0.019***	0.005	0.003	0.001
	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)
Cum. effect after	7.0	40.48***	43.80***	5.43	-1.31	-2.85
20 years (%)	(8.72)	(12.21)	(7.59)	(10.13)	(7.78)	(9.88)
Av. appr. rate (%)	0.34	1.71	1.83	0.26	-0.07	-0.14
Observations	7,841	8,860	7,841	8,860	7,841	8,860
R2	0.678	0.732	0.644	0.718	0.655	0.721
AIC	10,764.0	13,232.4	11,115.8	13,284.1	10,911.4	13,223.0
T (ever designated)	YES	YES	YES	YES	YES	YES
T x A (released from program)	YES	YES	YES	YES	YES	YES
Hedonic controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Block x area fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Time-varying effects:						
District x year effects	YES	YES	-	-	-	-
Consumption amenities x year effects	-	-	YES	YES	YES	YES
East Berlin x year effects	-	-	-	-	YES	YES

Notes: Dependent variable is log of property price in all models. Estimation method is OLS. *T* is a 0,1 dummy variable denoting a property location within a renewal area. *A* similarly denotes whether the nearest area has been released from the program. *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: $\exp(\beta_0 + \beta_1 \times 20) - 1$. Hedonic and location controls consist of covariates controlling for internal property and external location characteristics described in greater detail in the data sections of the main paper 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 district effects, East Berlin effects or a consumption amenity measure described in the data section of the main paper. 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 A12. Alternative methods to compute standard errors

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Point esti-mates	Standard errors						
		Clustered (block)	Clustered (traffic cells)	Clustered (post code)	Spatial HAC	Spatial HAC	Spatial HAC	Spatial HAC
Distance cutoff					2km	5km	10km	50km
T x POST	-0.027	(0.061)	(0.065)	(0.063)	(0.040)	(0.062)	(0.061)	(0.064)
T x V	0.006	(0.005)	(0.006)	(0.007)	(0.003)*	(0.004)	(0.004)	(0.003)*
Cum. effect (%)	9.36	(11.15)	(11.66)	(16.44)	(9.14)	(11.30)	(11.44)	(10.89)
Observations	8,860							
R^2	0.736							

Notes: Baseline model is model (6) in Table 4 in the main text. *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: $\exp(\beta_0 + \beta_1 \times 20) - 1$. All models include hedonic and location controls, block x area fixed effects, a target area indicator, after-designation effects, year effects, and time-varying effects as described, for example, in the data section of the main paper. Spatial HAC denotes non-parametric heteroscedasticity-autocorrelation consistent standard errors accounting for spatial autocorrelation and serial correlation allowing for a lag length of 23 years (our observation period). Standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

**Tab A13.Placebo designation effects on investigation areas:
Renewal and investigation areas vs. matched control group**

	(1)		(2)	
Control group	Matched observations (IV)			
Treatment effect on renewal areas:				
T^{REN} x $POST^{REN}$ (within renewal)	-0.036	(0.046)	-0.116**	(0.063)
T^{REN} x V^{REN} (years since designation)	0.021***	(0.005)	0.009**	(0.004)
Cum. effect after 20 years (%)	46.21***	(8.07)	7.01*	(7.49)
Av. appr. rate (%)	1.92		0.34	
Placebo treatment effect on investigation areas:				
T^{INV} x $POST^{INV}$ (within investigation)	-0.078*	(0.047)	0.035	(0.050)
T^{INV} x V^{INV}) (years since designation)	0.007*	(0.004)	0.002	(0.004)
Cum. effect after 20 years (%)	7.01	(8.33)	7.33	(9.06)
Av. appr. rate (%)	0.34		0.35	
Observations	12,121		12,121	
R^2	0.700		0.725	
AIC	17,754.0		17,332.9	
T (ever designated)	YES		YES	
T x A (released from program)	YES		YES	
Hedonic controls	YES		YES	
Location controls	YES		YES	
Block x area effects	YES		YES	
Year effects	YES		YES	
Time-varying effects	-		YES	

Notes: Dependent variable is log of property price in all models. Estimation method is OLS. *T* is a 0,1 dummy variable denoting a property location within a treatment area. *A* similarly denotes whether the nearest area has been released from the program. *POST* similarly denotes that the respective treatment 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: $\exp(\beta_0 + \beta_1 \times 20) - 1$. Location controls consist of covariates controlling for external location characteristics described in greater detail in the data sections of the main paper 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 of the main paper. Standard errors in parentheses are clustered on block x area fixed effects. * p < 0.1, ** p < 0.05, *** p < 0.01

Tab A14. Renewal area effects with historic amenities

	(1)	(2)
Control group	Investigation areas (III)	Matched observations (IV)
T x POST	-0.065	-0.016
	(0.048)	(0.062)
T x V	0.004	0.006
	(0.004)	(0.005)
Cum. effect after 20 years (%)	1.94	10.13
	(8.14)	(10.76)
Av. appr. rate (%)	0.10	0.48
Observations	7,841	8,860
R^2	0.681	0.736
AIC	10,757.2	13,192.2
T (ever designated)	YES	YES
T x A (released from program)	YES	YES
Hedonic controls	YES	YES
Location controls	YES	YES
Block x area effects	YES	YES
Year effects	YES	YES
Time-varying effects	YES	YES

Notes: Dependent variable is log of property price in all models. Estimation method is 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. Standard errors in parentheses are clustered on block x area fixed effects in all models. * p < 0.1, ** p < 0.05, *** p < 0.

Tab A15. Varying groups of treated and controls – including time-varying effects

No of areas		Cumulated effect after 20 years					% within 2 S.E. length
Treat.	Control	Iterations	Mean	S.D.	Min	Max	of bench.
			Varying number of treated areas				
1	22	22	0.04	0.48	-1.00	0.98	36.36%
2	22	462	0.02	0.30	-0.91	0.64	52.81%
5	22	2500	0.05	0.18	-0.62	0.53	80.00%
10	22	2500	0.07	0.11	-0.30	0.42	96.44%
15	22	2500	0.08	0.08	-0.16	0.30	99.80%
20	22	2500	0.09	0.08	-0.18	0.30	99.64%
			Varying number of control areas				
22	1	22	-0.12	0.34	-0.67	0.69	31.82%
22	2	462	-0.07	0.30	-0.78	0.69	53.68%
22	5	2500	-0.01	0.17	-0.79	0.74	76.56%
22	10	2500	0.01	0.09	-0.38	0.31	92.44%
22	15	2500	0.02	0.05	-0.16	0.17	99.08%
22	20	2500	0.02	0.05	-0.22	0.18	99.32%
			Varying number of treated and control areas				
1	1	2261	44.3	449	-1242	10063	0.97%
2	2	2500	7.14	131	-1054	3407	8.72%
5	5	2500	0.40	3.61	-22.56	85.65	42.56%
10	10	2500	0.10	0.16	-0.58	0.72	84.48%
15	15	2500	0.06	0.08	-0.39	0.38	98.08%
20	20	2500	0.07	0.08	-0.31	0.36	98.48%

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.

Tab A16. Descriptive statistics of matched control group A-IV

Balancing of the covariates

Variable	Sample	Mean Treated	Control	standardized bias (%)	% reduction in abs. bias
Age	Unmatched	87.676	56.839	90.2	
	Matched	88.573	80.826	22.7	74.9
Index of locational quality	Unmatched	2.7652	3.8524	-51.8	
[1, poor to 5, very good]	Matched	2.9681	3.4993	-25.3	51.1
Typical floor space index	Unmatched	2.1696	0.9391	149.7	
	Matched	2.1696	2.0385	16	89.3
Residential area indicator	Unmatched	0.8112	0.8462	-9.3	
	Matched	0.8971	0.8568	10.7	-15.3
Commercial area indicator	Unmatched	0.0393	0.0219	10.1	
	Matched	0.0430	0.0674	-14.2	-40.2
Distance to CBD [m]	Unmatched	5006.4	9667.8	-121.5	
	Matched	4980.7	5402.4	-11	91
Distance to park [m]	Unmatched	2302.5	1595.4	55.4	
	Matched	2293.1	1973	25.1	54.7
Distance to main street [m]	Unmatched	125.19	208.07	-45.2	
	Matched	125.12	115.84	5.1	88.8
Distance to water [m]	Unmatched	1245.7	1626.5	-32.4	
	Matched	1245.9	1200.5	3.9	88.1

Notes: The propensity scores are computed using nearest neighbor matching. Following Rosenbaum and Rubin (1985) and Leuven and Sianesi (2003), the standardized bias is the difference in the sample means in the subsamples (treated and control) as the percentage of the square root of the average of the sample variances in the treated and control groups. Imposing a common support restriction does not substantially change the results of the later analyses where we use the matched observations as a control group

Tab A17. Renewal area treatment effects – buildings in good quality

	(1)	(2)	(3)	(4)
Control group	All (I)	0.5-2 km (II)	Investigation areas (III)	Matched observations (IV)
T x POST	0.010	-0.067	-2.148***	0.512
	(0.338)	(0.416)	(0.402)	(1.226)
T x V	-0.000	-0.002	-0.026	-0.002
	(0.012)	(0.014)	(0.020)	(0.019)
Cum. effect after 20 years (%)	0.57	-10.0	-93.1	60.81
	(39.47)	(50.26)	(75.7)	(243.58)
Av. appr. rate (%)	0.03	-0.53	-12.5	2.40
Observations	15,406	2,567	725	948
R^2	0.917	0.941	0.863	0.891
AIC	5,488.1	1,474.4	502.8	736.3
T (ever designated)	YES	YES	YES	YES
T x A (released from program)	YES	YES	YES	YES
Hedonic controls	YES	YES	YES	YES
Location Controls	YES	YES	YES	YES
Block x area effects	YES	YES	YES	YES
Year effects	YES	YES	YES	YES

Notes: Dependent variable is log of property price in all models. Estimation method is OLS. *T* is a 0,1 dummy variable denoting a property location within a renewal area AND in good condition. *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. Standard errors in parentheses are clustered on Block x area fixed effects in all models. * p < 0.1, ** p < 0.05, *** p < 0.

Tab A18.Renewal area spillover effects within 500m renewal area buffer

Model	(1)	(2)	(3)	(4)	(5)	(6)
Control group	All (A-I)	1.5-2 km renewal area buffer (A-II)	Invest. areas + 1 km buff-er (A-III)	Matched ob-servations (A-IV)	Invest. areas + 1 km buff-er (A-III)	Matched observa-tions (A-IV)
T x POST	-0.201***	-0.133***	-0.075**	-0.004	0.030	0.014
	(0.035)	(0.036)	(0.034)	(0.042)	(0.033)	(0.053)
T x V	0.032***	0.022***	0.009***	0.019***	-0.007***	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Cum. effect after	55.12***	35.31***	11.64**	46.22***	-11.04**	-0.297
20 years (%)	(5.27)	(5.43)	(5.35)	(6.53)	(4.75)	(8.03)
Av. appr. rate (%)	2.21	1.53	0.66	1.93	-0.62	-0.13
Observations	66,651	19,421	21,324	12,641	21,324	12,641
R^2	0.805	0.777	0.723	0.753	0.749	0.781
AIC	82,305.6	27,744.8	31,296.4	15,989.7	29,864.8	15,243.6
Hedonic controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Block x area effects	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES
Time-varying effects	-	-	-	-	YES	YES

Notes: Dependent variable is log of property price in all models. Estimation method is OLS. *T* is a 0,1 dummy variable denoting a property location within a spillover area. *A* similarly denotes whether the nearest renewal area has been released from the program. *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: $\exp(\beta_0 + \beta_1 \times 20) - 1$. Hedonic and location controls consist of covariates controlling for internal property and external location characteristics described in greater detail in the data sections of the main paper and the appendix. 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 of the main paper. Standard errors in parentheses clustered on block x area effects. * p < 0.1, ** p < 0.05, *** p < 0.01