

Felix, Richter; Ahlfeldt, Gabriel; Maennig, Wolfgang

**Conference Paper**

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

Beiträge zur Jahrestagung des Vereins für Socialpolitik 2014: Evidenzbasierte Wirtschaftspolitik - Session: Agglomeration, Policy und Persistence, No. B14-V2

**Provided in Cooperation with:**

Verein für Socialpolitik / German Economic Association

*Suggested Citation:* Felix, Richter; Ahlfeldt, Gabriel; Maennig, Wolfgang (2014) : Urban renewal after the Berlin Wall - A place-based policy evaluation, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2014: Evidenzbasierte Wirtschaftspolitik - Session: Agglomeration, Policy und Persistence, No. B14-V2, ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft, Kiel und Hamburg

This Version is available at:

<https://hdl.handle.net/10419/100467>

**Standard-Nutzungsbedingungen:**

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

**Terms of use:**

*Documents in EconStor may be saved and copied for your personal and scholarly purposes.*

*You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.*

*If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.*

Gabriel M. Ahlfeldt<sup>\*</sup>, Wolfgang Maennig<sup>^</sup> & Felix J. Richter<sup>♦</sup>

# Urban renewal after the Berlin Wall: A place-based policy evaluation<sup>\*</sup>

**Abstract:** We use a quasi-experimental research design to study the effects of 22 renewal areas implemented in Berlin, Germany, to increase housing and living quality in the aftermath of the city's division during the Cold War period. We find that compared to areas considered but not selected for the program, the number of buildings in poor (good) condition decreased (increased) by 25% (10%) and property value increased by about 50% over a near to 20 years period. The effect, however, does not seem causal and largely attributable to trends correlated with locational characteristics. More generally, our findings suggest that estimated place-based policy effects can be sensitive to unobserved local shocks if a limited number of treatment and control areas are considered in the analysis.

*Keywords:* Urban, renewal, revitalization, redevelopment, hedonic regression, quasi-experiment, policy evaluation

*Version:* June 2014

*JEL:* D62, H23, R21, R31

## 1 Introduction

Evidence-based policy-making, i.e. the idea that public policies must be based on rigorous and objective evidence, has rapidly gained popularity during the 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 poli-

---

<sup>\*</sup> London School of Economics (LSE), Department of Geography and Environment & Spatial Economics Research Centre (SERC), Houghton Street, London WC2A 2AE, [g.ahlfeldt@lse.ac.uk](mailto:g.ahlfeldt@lse.ac.uk), [www.ahlfeldt.com](http://www.ahlfeldt.com)

<sup>^</sup> University of Hamburg, Department of Economics, Von Melle Park 5, 20146 Hamburg, [wolfgang.maennig@wiso.uni-hamburg.de](mailto:wolfgang.maennig@wiso.uni-hamburg.de), [www.uni-hamburg.de/economicpolicy](http://www.uni-hamburg.de/economicpolicy)

<sup>♦</sup> University of Hamburg, Department of Economics, Von Melle Park 5, 20146 Hamburg, [felix.richter@wiso.uni-hamburg.de](mailto:felix.richter@wiso.uni-hamburg.de), [www.uni-hamburg.de/economicpolicy](http://www.uni-hamburg.de/economicpolicy)

<sup>\*</sup> We thank the Berlin Committee of Valuation Experts and the Senate Department for Urban Development and the Environment for data provision. We also thank the conference participants of the 2013 SERC annual conference, the 2013 conference of the Verein fuer Sozialpolitik, and the 2013 ERSA conference and especially Paul Cheshire and Henry Overman for helpful comments and suggestions.

cy outcome can be compared. Truly experimental methodologies like randomized control trials, where randomly selected treated subjects can be followed over time and compared to similar non-treated subjects are not feasible in many fields of policy evaluation. Researchers have responded to this limitation by applying quasi-experimental research designs to ex-post outcomes of existing policies, which, however, are typically implemented non-randomly for good reasons. One policy area where the application of program evaluation techniques is severely complicated by the non-random nature of the analyzed policies are spatially targeted policies that aim at local economic growth. Because 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 not exposed to the policy. As a result compelling empirical evidence on the effects of place-based policies is often difficult to find.

With this contribution we aim at providing evidence on 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. Our objectives are two-fold: Firstly, we aim at estimating the causal economic effect of a major renewal policy implemented in the aftermath of Berlin's (Germany) unification. The empirical question is whether the policy has sustainably increased the attractiveness of the targeted locations and, if so, whether the generated value exceeds 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 U.S. the Community Development Block Grant (CDBG) provides between \$3 and \$10 bill. each year to cities and local administrations to improve conditions in low income urban areas (Brooks & Phillips, 2007). Another example is the Home Investment Partnership (HOME) program, which supports affordable housing with approximately \$2 bill. per year. In Germany, the budget for various urban development programs ("Städtebauförderung"), which are typically jointly financed by the federal government and the federal states, amounts to approximately €350 (\$453.1) mill. to €500 (\$647.3) mill. per year (Bundesin-

stitut für Bau-, Stadt-, und Raumforschung (2009)).<sup>1</sup> To our knowledge, we are only the second, after Rossi-Hansberg et al. (2010) [hereafter RH], to provide a rigorous evaluation of revitalization policies that are directly targeted at the quality of local housing stock.<sup>2</sup> RH investigate property prices in and around four renewal areas<sup>3</sup> and one control area, which was initially considered but ultimately excluded from the program in Richmond, Virginia to detect housing externalities. Their results indicate that housing externalities exist but diminish relatively steeply in distance, approximately 50% every 1,000 feet (RH 2012, p. 487). Equally important, they estimate that house prices in the designated areas rose between 2 and 5% per year during the renewal period, which equates to a return of 2 to 6 USD per dollar invested. Their results, thus, strongly indicate that urban renewal programs promote positive housing externalities and might be efficient instruments to increase welfare in neighborhoods in need. Despite the methodological rigor of their analysis, there is an evident need for complementary evidence to conclude on the generalizability of the case. This is especially true given that RH establish their counterfactual via a singular control area. As such, their finding might be sensitive to idiosyncratic characteristics of that area, which could influence the counterfactual price trend, but are difficult to anticipate. In short, we complement RH's findings by analyzing a larger policy experiment over a longer period. We make use of a relatively large pool of treated areas and potential control areas to obtain credible estimates for the average effect across the treated areas and to evaluate the sensitivity of the estimates to the selection of a more limited number of treated and control areas.

Berlin offers a unique institutional setting for an analysis of revitalization policies due to the 20<sup>th</sup> 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 & Sturm, 2008) or transformation into a non-market economy (East-Berlin), both of which severely affected the economic health of the city. After reunification in 1990, the adverse economic performance was mirrored by a poor physical condition of the housing stock,

---

<sup>1</sup> Aggregate renewal financing data at the European level are not available.

<sup>2</sup> Kline & Moretti (2014b) provide an introduction into the welfare economics of place-based policies more generally along with a recent survey of the empirical literature .

<sup>3</sup> Definitions and notation vary. Throughout this article, we will try to stick to the term renewal area, however, the terms redevelopment or revitalization area are often used interchangeably.

especially so 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.<sup>4,5</sup> Until late 2010 (the period of the last official report on the renewal program), as much as €1.94 Bill. (\$2.62 Bill.) had been spent on these areas. Our quasi-experimental research design compares property price trends within these 22 selected conservation areas over the period from 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.

Previewing our findings, our results indicate that the policy led to a significant upgrade of the housing stock. Property prices in the targeted areas increased at an above-average rate, but a closer inspection reveals that much of this trend can be attributed to favorable locational attributes. We do not find strong evidence for the existence of housing externalities, i.e. multiplier effects of the policy, and therefore keep the presentation of the analysis to the appendix. Importantly, our sensitivity analysis suggests that estimated place-based policy effects become sensitive to unobserved local shocks if very few subject or control areas are available.

In addition to adding important evidence to the literature on the economic effects of revitalization policies (e.g. Clay, 1979; Noonan, 2014; Rossi-Hansberg et al., 2010) and housing externalities (e.g. Ahlfeldt & Maennig, 2010; Autor, Palmer, & Pathak, 2014; Ellen, Schill, Susin, & Schwartz, 2001; Helms, 2012; Ioannides, 2002; Koster & Van Ommeren, 2013; Rossi-Hansberg et al., 2010; Schwartz, Ellen, Voicu, & Schill, 2006), we contribute to a literature that has assessed the impact of various local public policies via capitalization effects (e.g. Ahlfeldt & Kavetsos, 2014; Cellini, Ferreira, & Rothstein, 2010; Dachis, Duranton, & Turner, 2012; Dehring, Depken, & Ward, 2008; Eriksen & Rosenthal, 2010; Gibbons & Machin, 2005; Oates, 1969; Santiago, Galster, & Tatian, 2001) and economic effects of spatially targeted policies more generally (Baum-Snow & Marion, 2009; Boarnet & Bogart,

---

<sup>4</sup> The *First Berlin Renewal Program* (Erstes Gesamtberliner Stadterneuerungsprogramm).

<sup>5</sup> 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.

1996; Busso, Gregory, & Kline, 2013; Freedman, 2012, 2014; Freedman & Owens, 2011; Gobillon, Magnac, & Selod, 2012; Ham, Swenson, İmrohoroğlu, & Song, 2011; Kline, 2010; Kline & Moretti, 2013, 2014a; Murray, 1999; Neumark & Kolko, 2010; Sinai & Waldfoegel, 2005).<sup>6</sup>

Our analysis also connects to a more general research strand in urban economics that examines the amenity value of cities (e.g. Albouy, 2009, 2012; Blomquist, Berger, & Hoehn, 1988; Gabriel & Rosenthal, 2004; Gyourko & Tracy, 1991; Tabuchi & Yoshida, 2000) or neighborhoods within cities (e.g. Brueckner, Thisse, & Zenou, 1999; Carlino & Coulson, 2004; Cheshire & Sheppard, 1995; Ioannides, 2003).<sup>7</sup> This literature has argued that there has been a re-orientation towards attractive central cities, especially among high-skilled young professionals, the so called 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 & Saiz, 2008; Glaeser, Kolko, & Saiz, 2001).

Our findings inform this literature on whether revitalization policies and other neighborhood policies such as historic preservation may contribute to the development of targeted neighborhoods and promote gentrification.<sup>8</sup> Our results also complement the analysis by Ahlfeldt et al. (2012), who estimate a general equilibrium model of simultaneous household and firm location using exogenous variation that stems from the rise and fall of the Berlin Wall. Our results provide further evidence that the fundamental re-orientation to the pre-WW II equilibrium the city experienced after the fall of the Berlin Wall is unlikely to be explained by the renewal policies and likely attributable to economic agglomeration and dispersion forces. Finally, our results inform the program evaluation literature more

---

<sup>6</sup> A related body of literature has investigated the capitalization effects of historic designation, both on designated buildings and properties near designated buildings (e.g. Asabere, Huffman, & Mehdiian, 1994; Clark & Herrin, 1997; Coulson & Lahr, 2005; Coulson & Leichenko, 2004; Koster, Van Ommeren, & Rietveld, 2012; Lazrak, Nijkamp, Rietveld, & Rouwendal, 2010; Leichenko, Coulson, & Listokin, 2001; Listokin, Listokin, & Lahr, 1998; Noonan & Krupka, 2011; Schaeffer & Millerick, 1991).

<sup>7</sup> This study complements research examining the effects of spatial density on the productivity of workers and firms (e.g. Ahlfeldt, Redding, Sturm, & Wolf, 2012; Ciccone, 2002; Ciccone & Hall, 1996; Glaeser, Hedi, Jose, & Andrei, 1992; Glaeser & Mare, 2001; Rauch, 1993; Rosenthal & Strange, 2001)

<sup>8</sup> Alternative determinants include transport affordability (LeRoy & Sonstelie, 1983), housing cycles (Brueckner & Rosenthal, 2009), housing demand shocks (Guerrieri, Hartley, & Hurst, 2013), or natural amenities (Lee & Lin, 2012).

generally in that successful identification of place-based policy effect using quasi-experimental methods may critically depend on sufficiently large number of treatment and control areas.

The remainder of the article is organized as follows: Section 2 introduces into the institutional setting. Sections 3 and 4 present the empirical strategy and results. The final section summarizes our findings and concludes.

## 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 until 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 were 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 after unification in 1990.

The main instrument to overcome these problems was the initiation of the *First Berlin Renewal Program* which identified a group of urban renewal areas eligible for public funding and support according to specific rules. The location, shape, and structure of a renewal area are determined in a political decision process that involves several steps: First, the districts of Berlin and the Senate initiate a search for hotspots of urban decline, the so called ‘investigation areas’, to identify potential renewal areas. In depth analyses of the social structure in the respective areas are then commissioned, which encompass possible revitalization concepts and recommendations on size and position of the potential renewal areas. Finally, the Senate of Berlin officially designates the renewal areas (Maennig, 2012).

In July 1992, the Senate of Berlin initiated 39 investigation areas. In 1993, 1994, and 1995, 22 renewal areas were officially designated, with 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).<sup>9</sup> 94% of the housing units inside the renewal areas were located in the eastern part of Berlin. Within these renewal areas, private investments in the building stock have been supported through tax reductions, loans, cash advances and further financial support. After 2002 the focus was set to improvements of the social infrastructure and living quality of the neighborhood. Private modernizations are no longer co-financed through public investments, but significant tax abatements remain as an implicit subsidy.<sup>10</sup>

Until late 2010, the expenses comprised about €1.94 bill. (\$2.62 bill.) in public investments, amounting to approximately €880 mill. (\$1.19 bill.) for modernization and reinstatement, and approximately €645 mill. (\$873 mill.) for expenses on infrastructure and social environment. The remaining disbursements consist of preparation costs (€77 mill. / \$104 mill.), allowances (€123 mill. / \$166 mill.), other regulatory measures including compensations (€143 mill. / \$193 mill.), and other building measures (€63 mill. / \$85 mill.).<sup>11</sup> The average expenses are approximately €88 mill. (\$119 mill.) per renewal area, translating into per capita expenses of €17,500 (\$23,700) distributed over a period of some 15 years.<sup>12</sup> This compares to per area payments of \$3.5 mill. and per capita expenses of \$1,800 in Richmond in 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.<sup>13</sup>

---

<sup>9</sup> In Richmond, the object of the RH (2012) analysis, the four targeted areas had an average population of 1900 residents and on average 1,000 housing units.

<sup>10</sup> 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).

<sup>11</sup> See Senatsverwaltung für Stadtentwicklung Berlin (2012), where the local administration provides detailed budget accounting information for the different time periods. More up-to-date figures are not yet available to the best of our knowledge.

<sup>12</sup> The total investment amounts to about 35% of the housing stock value. See for further detail section 2.3 in the technical appendix.

<sup>13</sup> 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.



### 3 Empirical strategy

#### 3.1 Baseline specification

We use a combination of hedonic (Rosen, 1974) and difference-in-difference methods to estimate the causal effect the renewal policy on property prices in the targeted areas. Specifically, we aim at estimating a series of time specific  $\beta_V$  parameters, where  $V$  indicates the number of years that have passed since designation. To estimate these parameters of interest, we estimate the following empirical specification:

$$\begin{aligned} \log P_{it} = & \alpha_1 T_i + f(T_i \times V_{it}) + \delta(T_i \times A_{it}) \\ & + \sum_k \gamma_k X_{kit} + \sum_l \gamma_l L_{li} + \sum_t \sum_g (\gamma_{gt} G_i \times \varphi_t) + \sum_t \varphi_t + \sum_n \mu_n + \varepsilon_{it}, \end{aligned} \quad (1)$$

where  $P_{it}$  is the price at which a property  $i$  is sold at time  $t$ . The central elements of this specification are an indicator variable  $T$ , which denotes whether a property falls within one of the renewal areas we investigate ( $T=1$ ) or into the control area ( $T=0$ ), and the function  $f(T_i \times V_{it})$ , which captures interaction effect of being located within one of the renewal areas and the number of years this area has been designated ( $V$ ). We discuss the employed functional forms in depth 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 ( $A_{it} = 1$ ). We control for a potential capitalization effect via the interaction term ( $T_i \times A_{it}$ ).  $X_k$  and  $L_l$  are observable property and location characteristics discussed in the data section and  $\gamma_k$  and  $\gamma_l$  are the respective implicit prices. We control for otherwise unobserved time-invariant location characteristics via a fixed effects  $\mu_n$  defined for 9,718 statistical blocks.<sup>14</sup> Standard errors ( $\varepsilon_{it}$ ) are clustered at the same level and, thus, accommodate a spatial structure in a relatively flexible manner. Macroeconomic factors that are assumed to be invariant across the treatment and control groups are captured by year fixed effects  $\varphi_t$ .

---

<sup>14</sup> Statistical blocks are the smallest geographic statistical unit in Berlin. There are close to 16,000 blocks in Berlin, of which about 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).

In addition to controlling for year effects and time-invariant location characteristics, we further allow for time-variant implicit prices  $\gamma_{gt}$  for some time-invariant location characteristics  $G_i$  by means of interaction terms with the year effects. The rationale for including these variables is that, 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 differ experience a change in valuation, this will affect the counterfactual. The problem can be remedied by allowing the implicit price of the respective attribute to vary over time. We attempt to at least address the most obvious candidate, the gentrification of central neighborhoods, especially those with an attractive endowment of consumption amenities (Glaeser et al., 2001). We therefore interact the year dummies with the distance to the central business district and a kernel smoothed density surface of bars, pubs, nightclubs and hotels. We also add a full set of 23 city district  $\times$  year fixed effects to capture variation across district-year cells. We note that all the variables we interact with the year dummies are time-invariant to avoid problems of circular causation.

### *Control groups*

We use several definitions of control groups to establish the counterfactual. For all control groups, we exclude a 500 m buffer area around the renewal areas, to rule out a treatment effect on the control groups due to potential spillovers. Control group I includes all observations outside the urban renewal areas and the 500 m buffer. In control group II, we impose a geographical limit by considering transactions that lie within a 500 to 2,000 meter (approx. 6,000 ft.) distance from the renewal areas. Control group III consists of the fractions of investigation areas outside the 500 m buffer that were not transformed into renewal areas – similar to RH. As a further alternative, control group IV is created based on the propensity score matching procedure proposed by Rosenbaum & Rubin (1983).

For the synthetic matched control group IV, 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 before the treatment or are time invariant are considered (Caliendo & Kopeinig, 2008). These covariates include a range of internal property and external location characteristics and are discussed in greater detail in the technical appendix, where we also present some descriptive statistics for the resulting sample.

### *Treatment functions*

To capture the time-varying treatment effects  $\beta_v$ , we define two versions of  $f(T_i \times V_{it})$ . The first is a relatively restrictive parametric variant designed to allow for a level and a trend shift following designation:

$$f(T_i \times V_{it}) = \beta_0 T_{it} \times POST_{it} + \beta_1 T_{it} \times V_{it}, \quad (2)$$

where  $POST$  is an indicator variable taking the value of one if a property is sold after the respective renewal area has been designated. The year specific treatment effects are defined as  $\beta_v = \beta_0 + \beta_1 V_{it}$ . The second approach follows Ahlfeldt & Kavetsos (2014) and is more flexible. We group the treated observations into cohorts depending on  $V_{it}$ . For each cohort, we then define an indicator variable  $VD_{vit}$  describing whether transactions fall into the cohort, e.g.,  $VD_{1it}=1$  for all observation transacted one year after designation of the respective renewal area. Interacting all cohort indicator variables with the treatment indicator  $T$ , we estimate a series of difference-in-difference treatment effects that compare how prices have changed since designation in the treatment and control groups:

$$f(T_i \times V_{it}) = \sum_v \beta_v (T_{it} \times VD_{vit}) \quad (3)$$

The estimated  $\hat{\beta}_v$  coefficients, hence, form a mix-adjusted hedonic price index that flexibly reflects the evolution of the treatment group relative to the control group. 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 also larger confidence bands due to the relatively smaller number of observations per  $VD_{vit}$  cohort.

### 3.2 Sensitivity analysis

One favorable feature of our empirical setting is the availability of a relatively large number of treatment (22 designated renewal areas) and control areas (22 self-contained zones out of 39 areas initially considered). Our empirical models control for unobserved time-invariant spatial heterogeneity, unobserved shocks at the city district level and unobserved shocks that are correlated with distance to the CBD and the spatial distribution of consumption amenities. Successful identification rests on the assumption that the treated and control areas are subject to the same macro-economic shocks conditional on these controls. The relatively large number of treatment and control areas 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.

In other instances of place-based policy evaluations the number of available treatment and control areas may be more limited. 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 benchmark estimate reflects the true causal policy effect, the distribution of point estimates across these iterations will give an indication of how likely the policy evaluation would have yielded biased results should fewer treatment or control areas have been available.

### 3.3 Data and descriptive statistics

Our study area comprises the area of the Federal State of Berlin, Germany. Within this study area, we observe all transactions of developed land 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 are 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 (2<sup>nd</sup> order polynomial), land use, location within a block of houses (e.g., a corner lot), among other variables.

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.<sup>15</sup> While these data are not official, but user-generated, 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 A4 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. Out 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). As one would expect from the renewal and investigation areas having been chosen due to similar building, socio-demographic and geographic characteristics, the areas are also relatively similar in other observable characteristics.<sup>16</sup>

Figure 1 shows of the spatial distribution of the renewal / investigation areas along with our estimated smoothed kernel density surface and our synthetic control group (control group IV). Renewal areas and revitalization areas are typically located in central areas and

---

<sup>15</sup> [www.openstreetmap.org](http://www.openstreetmap.org)

<sup>16</sup> Table A1 in the web based appendix lists the renewal areas and some stylized facts per area, while Table A2 compares key characteristics across the renewal areas, the investigation areas, and the rest of Berlin.

in amenity clusters in the eastern part of the city. Our synthetic control group (red dots) consists of transactions that are either close to renewal or investigation areas or in areas of high amenity densities, which lends some confidence to the selection process.

– Figure 1 about here –

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, who undertake on-site examinations for each transaction of developed land that takes place. In Figure 2, we plot how the conditional mean shares of transacted properties in either good or poor condition evolved in the renewal areas relative to the investigation areas over time. The indices are generated using auxiliary regressions described in the figure notes. From the mix-adjusted quality trends, it is evident that the quality of the housing stock in the renewal areas improved significantly over time. In 1990, the fraction of buildings in poor condition in renewal areas was significantly larger than in the investigation areas, possibly a reason for their selection. The difference steadily declines over time. By the end of the observation period, the relationship is at the margin of becoming negative and statistically significant. While the conditional mean shares of properties in good maintenance were virtually the same in 1990, the proportion was significantly larger in the renewal areas by the end of the period. Figure 2 demonstrates this development and indicates that the renewal program accelerated the upgrading of the housing stock that was left behind during the division period.

– Figure 2 about here –

## 4 Empirical results

### 4.1 Baseline specification

Table 1 summarizes our parametric estimates (see equation 1) of renewal area capitalization effects by varying control groups. For the sake of brevity, we 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 reported in Table A5 in the web based appendix. The parameter on  $TxPOST$  ( $\beta_0$ ) indicates a shift in log prices at the time of designation, while the parameter on  $TxV$  ( $\beta_1$ ) reveals the yearly percentage appreciation within the renewal areas relative to the control areas in the post designation

period. Based on the two estimated parameters, the cumulated percentage renewal policy effect for any given year since designation can be computed as  $(\exp(\beta_0 + \beta_1 V_{it}) - 1)$ .<sup>17</sup>

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 4.7% per year) dominates a negative intercept (-16.2%). After  $V=20$  years, sales prices in designated renewal areas, on average, have appreciated by as much as 119.4% relative to the rest of the city. This corresponds to an average yearly appreciation rate of approximately  $(1 + 119.4\%)^{1/20} - 1 = 4.01\%$ . As we increase the strength of the counterfactual using spatially proximate properties (2), the investigation areas (3) or the matched properties (4) as a control group, the cumulative effect (average appreciation rate) drops to 94.49% (3.3%), 49.79% (2.04%), and 42.7% (1.79%), respectively. Most notable are the effects of the inclusion of time-varying effects in models (5) and (6), which compared to the baseline models (3) and (4), reduce the cumulative effect to – a non-significant - 7.33% (8.3%) in model 5 (6). This corresponds to a – non-significant - average annual appreciation of approximately 0.375%. One interpretation of this remarkable decline is that the relative appreciation of the renewal areas is to a significant extent driven by their favorable location with respect to distance to the CBD, consumption amenity endowment and the districts they fall in, i.e., they would have appreciated even in the absence of the policy.

– Table 1 about here –

Figure 3 illustrates our semi-non-parametric estimates of the temporal treatment function according to equation 2. We present estimates excluding (upper row) and including (lower row) time varying effects using all properties outside the renewal areas (left column) and properties in investigation areas (right column) as control group. The semi-non-parametric estimates are generally in line with the parametric counterparts presented in Table 1. The cumulative effect on all properties inside renewal areas relative to those outside the renewal areas is slightly larger than implied by the parametric estimates (upper left), but declines to approximately 50% when the trend is benchmarked against the investigation areas (upper right). The positive trend effects seem to capitalize with some delay

---

<sup>17</sup> We make use of the conventional interpretation of dummy variables in semi-log models (Halvorsen & Palmquist, 1980).

(beginning after approximately 5 years). The negative level shifts found in Table 1, 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 (5) in Table 1 and is neither statistically significant. As with the parametric model, we are not able to affirm the existence of significant revitalization effects based on the most demanding models.

– Figure 3 about here –

## 4.2 Sensitivity analysis

Table 2 summarizes the distributions of the cumulated treatment effects that are obtained from several series of applications of our parametric benchmark model (Table 1, column 5) to varying numbers of randomly selected treatment and control areas. One notable finding is that in all series, the mean of the estimated cumulated policy effects after 20 years is within approximately one standard deviation of zero, which is consistent with the policy not having a statistically significant impact. Equally important, the estimates tend to fall into a narrower range as the number of areas considered is increased. The percentage of individual estimates falling within two standard error lengths of our benchmark result (Table 1, column 5) increase from 36 (32) to near 100 percent as we increase the number of treatment (control) areas from 1 to 15, holding the number of control (treated) areas constant. The effect is even larger if the number of treatment and control areas considered is increased at the same time. The estimates become reasonably precise once ten treated and control areas are selected.

Figure 4 displays some of the distributions summarized in Table 2. With only one randomly selected treatment (control) area compared to all control (treatment) areas, there is no apparent 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 treatment or control groups there is a relatively well-behaved probability distribution centered around the average treatment



effect, with the majority of individual estimates being within two standard error lengths (middle left).

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 fifteen 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). Also, the standard deviation of the estimates is very close to the standard error estimated in the benchmark model.

Because 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.

*– Table 2 about here –*

*– Figure 4 about here –*

### **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 replicate our preferred models allowing for fewer time-varying controls to address the concern that these absorb variation that is (partially) attributable to the policy. Second, we compute standard errors that account for spatial autocorrelation, serial correlation, and heteroscedasticity following Conley (1999) and using various cutoff distances. 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. Fourth, we replace the contemporary amenity density with an analogically constructed variant that uses bars and restaurants as reported in the 1995/6 edition of the yellow pages (Gel-

be Seiten), which should predate the impact of the designation of renewal areas. The results support the interpretations and conclusions presented in this document.

We also conduct several empirical exercises to detect potential housing externalities, i.e. increases in housing values due to renovations 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. In an alternative approach we focus on spillover effects onto nearby areas that were not exposed to the policy but would benefit from nearby improvements if housing externalities played a significant role. Our results do not indicate the presence of significant housing externalities.

## 5 Conclusion

Given the expectations that have motivated the renewal program in question and similar programs, our results are simultaneously encouraging and disillusioning. On the one hand, our results indicate that the policy led to increased renovation work, improved maintenance, and an appreciation of the renovated buildings in the targeted neighborhoods. Over approximately 20 years, the share of buildings in poor (good) condition declined (increased) by approximately 25% (10%) relative to similar untargeted areas. Compared to similar areas considered, but not selected for the program, property prices, on average, after 20 year of operation of the program increased by approximately 50%, which equates to a yearly appreciation rate of 2%. The appreciation is even larger compared to the city average.

Our results, however, also suggest that the renewal effect is not causally related to the policy as of the appreciation is likely attributable to trends correlated with locational characteristics. The selected areas primarily locate in amenity-rich central areas in former East-Berlin. Controlling for these effects our preferred estimates point to a statistically insignificant cumulative effect of less than 10%, which corresponds to an (insignificant) annual appreciation of less than 0.5%. Equally important, our results, on average, do not point to the self-reinforcing effect operating through housing externalities for which one may have hoped.

Our results, thus, look less favorable than those previously presented by RH for the Neighborhoods in Bloom program in Richmond, Virginia. Analyzing a much smaller program, RH 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. There are some explanations that may account for the large discrepancy in the findings for Richmond and Berlin. The first are the different structures of the two local communities. The Richmond program was more based on community volunteering and local non-profit organizations, while Berlin adopted a top-down approach implemented by official state authorities. Second, and perhaps more important, German cities, and especially in Berlin, are not directly comparable to the average US city in that many residents choose to rent apartments. As a result, 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, owners have been demonstrated to invest more in social capital (DiPasquale & Glaeser, 1999; Hilber, 2010) and tend to use neighborhood policies as a framework to coordinate their behavior to internalize externalities (Holman & Ahlfeldt, 2013), as such, they may also be more receptive to renovation subsidies. Third, there is some indication that the impact of the policy varied across targeted neighborhoods in Berlin, which may indicate that some areas are more responsive to renewal policies than others.

Future research into the long-run effects of renewal policies across different institutional settings is needed to fully reconcile the evidence. Understanding the factors that determine how incentivizing private investment in building maintenance can lead to positive spillovers is key to deciding where such programs should be implemented and where the focus should be on improvements of fundamental location factors in the first place.

More generally, our results show critically that the outcome of place-based policy evaluations can depend on the empirical specifications used. In our case more credible control groups yield significantly lower treatment estimates than naïve comparisons to nearby areas or even the rest of the city. Controlling for trends that are correlated with (favorable) locational attributes further brings down the treatment effect substantially. Moreover, 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. While in practice, little can be done to overcome the

limitation of a policy (quasi)experiment that offers only a small number of targeted areas, the matching approach used in the construction of the synthetic control group can be considered as an alternative or a robustness check when only a few obvious candidate areas exist to establish a counterfactual.

## Literature

- Ahlfeldt, G. M., & Kavetsos, G. (2014). Form or Function? The impact of new sports stadia on property prices in London. *Journal of the Royal Statistical Society A*, 177(1), 169-190.
- Ahlfeldt, G. M., & Maennig, W. (2010). Substitutability and Complementarity of Urban Amenities: External Effects of Built Heritage in Berlin. *Real Estate Economics*, 38(2), 285-323.
- Ahlfeldt, G. M., Redding, S. J., Sturm, D. M., & Wolf, N. (2012). The Economics of Density: Evidence from the Berlin Wall. *CEP Discussion Paper No 1154*.
- Albouy, D. (2009). What are cities worth? Land rents, local productivity, and the capitalization of amenity values. *NBER Working Paper 14981*.
- Albouy, D. (2012). Are Big Cities Bad Places to Live? Estimating Quality of Life across Metropolitan Areas. *Working Paper*.
- Asabere, P. K., Huffman, F. E., & Mehdiian, S. (1994). The Adverse Impacts of Local Historic Designation: The Case of Small Apartment Buildings in Philadelphia. *Journal of Real Estate Finance & Economics*, 8(3), 225-234.
- Autor, D. H., Palmer, C. J., & Pathak, P. A. (2014). Housing Market Spillovers: Evidence from the end of Rent Control in Cambridge Massachusetts. *Journal of Political Economy*, forthcoming.
- Baum-Snow, N., & Marion, J. (2009). The effects of low income housing tax credit developments on neighborhoods. *Journal of Public Economics*, 93(5-6), 654-666.
- Blomquist, G. C., Berger, M. C., & Hoehn, J. P. (1988). New Estimates of Quality of Life in Urban Areas. *The American Economic Review*, 78(1), 89-107.
- Boarnet, M. G., & Bogart, W. T. (1996). Enterprise Zones and Employment: Evidence from New Jersey. *Journal of Urban Economics*, 40(2), 198-215.
- Brooks, L., & Phillips, J. (2007). Inside the Gift Horse's Mouth: City Spending, Political Institutions and the Community Development Block Grant Program. *McGill University Working Papers*.
- Brueckner, J. K., & Rosenthal, S. S. (2009). Gentrification and Neighborhood Housing Cycles: Will America's Future Downtowns be Rich? *The Review of Economics and Statistics*, 91(4), 725-743.
- Brueckner, J. K., Thisse, J.-F., & Zenou, Y. (1999). Why Is Central Paris Rich and Downtown Detroit Poor? An Amenity-Based Theory. *European Economic Review*, 43(1), 91-107.
- Bundesinstitut für Bau, S.-u. R. (2009). *Die Städtebauförderungsdatenbank des BBSR*.
- Busso, M., Gregory, J., & Kline, P. (2013). Assessing the Incidence and Efficiency of a Prominent Place Based Policy. *American Economic Review*, 103(2), 897-947.
- Caliendo, M., & Kopeinig, S. (2008). Some practical Guidance for the Implementation of Propensity Score Matching. *Journal of Economic Surveys*, 22(1), 31-72.
- Carlino, G. A., & Coulson, N. E. (2004). Compensating Differentials and the Social Benefits of the NFL. *Journal of Urban Economics*, 56(1), 25-50.
- Carlino, G. A., & Saiz, A. (2008). City Beautiful. *Federal Reserve Bank for Philadelphia Working Papers*, 08-22, 1-61.
- Cellini, S. R., Ferreira, F., & Rothstein, J. (2010). The value of school facility investments: Evidence from a dynamic regression discontinuity design. *The Quarterly Journal of Economics*, 125(1), 215-261.
- Cheshire, P. C., & Sheppard, S. (1995). On the Price of Land and the Value of Amenities. [Article]. *Economica*, 62(246), 247-267.
- Ciccone, A. (2002). Agglomeration effects in Europe. *European Economic Review*, 46(2), 213-227.
- Ciccone, A., & Hall, R. E. (1996). Productivity and the Density of Economic Activity. *American Economic Review*, 86(1), 54-70.
- Clark, D. E., & Herrin, W. E. (1997). Historical preservation districts and home sale prices: evidence from the Sacramento housing market. *The Review of regional studies*, 27, 29-48.
- Clay, P. L. (1979). *Neighborhood renewal. Middle-class Resettlement and Incumbent Upgrading in American Neighbourhood*. Lexington, MA: Lexington Books.

- Conley, T. G. (1999). GMM estimation with cross sectional dependence. *Journal of Econometrics*, 92(1), 1-45.
- Coulson, N. E., & Lahr, M. L. (2005). Gracing the Land of Elvis and Beale Street: Historic Designation and Property Values in Memphis. *Real Estate Economics*, 33(3), 487-507.
- Coulson, N. E., & Leichenko, R. M. (2004). Historic preservation and neighbourhood change. *Urban Studies*, 41(8), 1587-1600.
- Dachis, B., Duranton, G., & Turner, M. A. (2012). The effects of land transfer taxes on real estate markets: evidence from a natural experiment in Toronto. *Journal of Economic Geography*, 12(2), 327-354.
- Dehring, C. A., Depken, C. A., & Ward, M. R. (2008). A direct test of the homevoter hypothesis. *Journal of Urban Economics*, 64(1), 155-170.
- DiPasquale, D., & Glaeser, E. L. (1999). Incentives and Social Capital: Are Homeowners Better Citizens? *Journal of Urban Economics*, 45(2), 354-384.
- Ellen, I. G., Schill, M. H., Susin, S., & Schwartz, A. E. (2001). Building homes, reviving neighborhoods: Spillovers from subsidized construction of owner-occupied housing in New York City. *Journal of Housing Research*, 12(2), 185-216.
- Eriksen, M. D., & Rosenthal, S. S. (2010). Crowd out effects of place-based subsidized rental housing: New evidence from the LIHTC program. *Journal of Public Economics*, 94(11-12), 953-966.
- Florida, R. (2002). *The Rise of the Creative Class - and how it's Transforming Work, Leisure, Community and Everyday Life*. New York: Basic Books.
- Freedman, M. (2012). Teaching new markets old tricks: The effects of subsidized investment on low-income neighborhoods. *Journal of Public Economics*, 96(11), 1000-1014.
- Freedman, M. (2014). Tax Incentives and Housing Investment in Low-Income Neighborhoods.
- Freedman, M., & Owens, E. G. (2011). Low-income housing development and crime. *Journal of Urban Economics*, 70(2), 115-131.
- Gabriel, S. A., & Rosenthal, S. S. (2004). Quality of the Business Environment Versus Quality of Life: Do Firms and Households Like the Same Cities? *The Review of Economics and Statistics*, 86(1), 483.
- Galster, G. C. (1983). Empirical Evidence on Cross-Tenure Differences in Home Maintenance and Conditions. *Land Economics*, 59(1), 107-113.
- Gibbons, S., & Machin, S. (2005). Valuing rail access using transport innovations. *Journal of Urban Economics*, 57(1), 148-169.
- Glaeser, E. L., Hedi, D. K., Jose, A. S., & Andrei, S. (1992). Growth in Cities. *Journal of Political Economy*, 100(6), 1126-1152.
- Glaeser, E. L., Kolko, J., & Saiz, A. (2001). Consumer city. *Journal of Economic Geography*, 1(1), 27-50.
- Glaeser, E. L., & Mare, D. C. (2001). Cities and Skills. *Journal of Labor Economics*, 19(2), 316-342.
- Gobillon, L., Magnac, T., & Selod, H. (2012). Do unemployed workers benefit from enterprise zones? The French experience. *Journal of Public Economics*, 96(9-10), 881-892.
- Guerrieri, V., Hartley, D., & Hurst, E. (2013). Endogenous gentrification and housing price dynamics. *Journal of Public Economics*, 100(0), 45-60.
- Gyourko, J., & Tracy, J. (1991). The Structure of Local Public Finance and the Quality of Life. *Journal of Political Economy*, 99(4), 774-806.
- Halvorsen, R., & Palmquist, R. (1980). The Interpretation of Dummy Variables in Semilogarithmic Equations. *American Economic Review*, 70(3), 474-475.
- Ham, J. C., Swenson, C., İmrohoroğlu, A., & Song, H. (2011). Government programs can improve local labor markets: Evidence from State Enterprise Zones, Federal Empowerment Zones and Federal Enterprise Community. *Journal of Public Economics*, 95(7-8), 779-797.
- Helms, A. C. (2012). Keeping up with the Joneses: Neighborhood effects in housing renovation. *Regional Science and Urban Economics*, 42(1), 303-313.
- Hilber, C. A. L. (2010). New housing supply and the dilution of social capital. *Journal of Urban Economics*, 67(3), 419-437.

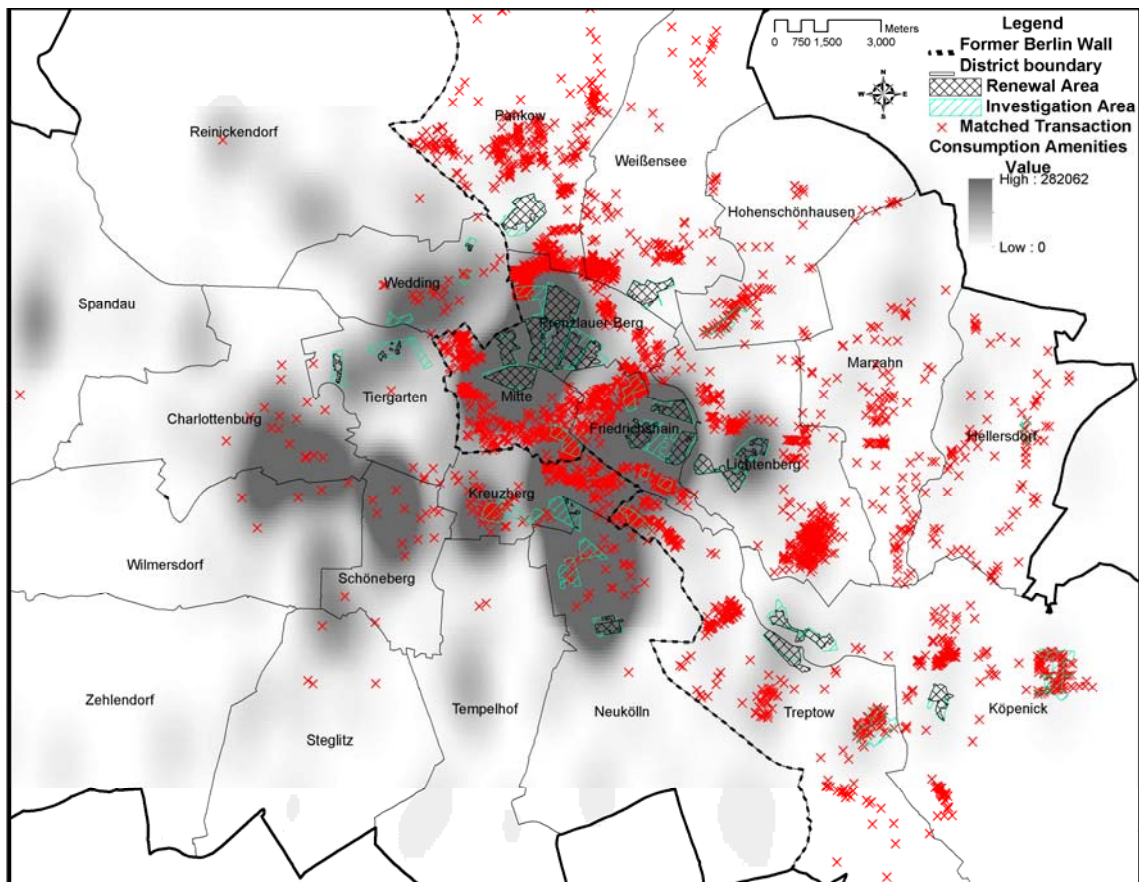
- Holman, N., & Ahlfeldt, G. M. (2013). No escape? The coordination problem in heritage preservation. *SERC Discussion Paper 145*.
- Ioannides, Y. M. (2002). Residential neighborhood effects. *Regional Science and Urban Economics*, 32(2), 145-165.
- Ioannides, Y. M. (2003). Interactive property valuations. *Journal of Urban Economics*, 53(1), 145-170.
- Kline, P. (2010). Place Based Policies, Heterogeneity, and Agglomeration. *The American Economic Review*, 100(2), 383-387.
- Kline, P., & Moretti, E. (2013). Place Based Policies with Unemployment. *American Economic Review*, 103(3), 238-243.
- Kline, P., & Moretti, E. (2014a). Local Economic Development, Agglomeration Economies, and the Big Push: 100 Years of Evidence from the Tennessee Valley Authority. *The Quarterly Journal of Economics*, 129(1), 275-331.
- Kline, P., & Moretti, E. (2014b). People, Places and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs. *forthcoming: Annual Review of Economics*.
- Koster, H. R. A., & Van Ommeren, J. N. (2013). *Spatial Externalities and Place-Based Policies: Evidence from the Netherlands*. Paper presented at the ERSA 2013 Conference.
- Koster, H. R. A., Van Ommeren, J. N., & Rietveld, P. (2012). Upscale Neighbourhoods: Historic Amenities, Income and Spatial Sorting of Households. *Mimeo, VU University Amsterdam*.
- Lazrak, F., Nijkamp, P., Rietveld, P., & Rouwendal, J. (2010). The market value of listed heritage: An urban economic application of spatial hedonic pricing. *VU University Amsterdam Working Paper*.
- Lee, S., & Lin, J. (2012). Neighborhood Dynamics and Natural Amenities. *Working Paper*.
- Leichenko, R. M., Coulson, N. E., & Listokin, D. (2001). Historic Preservation and Residential Property Values: An Analysis of Texas Cities. *Urban Studies*, 38(11), 1973-1987.
- LeRoy, S. F., & Sonstelie, J. (1983). Paradise lost and regained: Transportation innovation, income, and residential location. *Journal of Urban Economics*, 13(1), 67-89.
- Listokin, D., Listokin, B., & Lahr, M. (1998). The Contributions of Historic Preservation to Housing and Economic Development. *Housing Policy Debate*, 9(3), 431-478.
- Maennig, W. (2012). Monument Protection and Zoning: Regulations and Public Support from an International Perspective. In T. Just & W. Maennig (Eds.), *Understanding German Real Estate Markets* (pp. 181-192). Berlin Heidelberg: Springer
- Murray, M. P. (1999). Subsidized and unsubsidized housing stocks 1935 to 1987: crowding out and cointegration. *The Journal of Real Estate Finance and Economics*, 18(1), 107-124.
- Neumark, D., & Kolko, J. (2010). Do enterprise zones create jobs? Evidence from California's enterprise zone program. *Journal of Urban Economics*, 68(1), 1-19.
- Noonan, D. S. (2014). *U.S. Cultural Districts and Urban Renewal*.
- Noonan, D. S., & Krupka, D. J. (2011). Making—or Picking—Winners: Evidence of Internal and External Price Effects in Historic Preservation Policies. *Real Estate Economics*, 39(2), 379-407.
- Oates, W. E. (1969). The Effects of Property Taxes and Local Public Spending on Property Values: An Empirical Study of Tax Capitalization and the Tiebout Hypothesis. *Journal of Political Economy*, 77(6), 957-971.
- Rauch, J. E. (1993). Productivity Gains from Geographic Concentration of Human Capital: Evidence from the Cities. *Journal of Urban Economics*, 34(3), 380-400.
- Redding, S. J., & Sturm, D. M. (2008). The costs of remoteness: evidence from German division and reunification. *American Economic Review*, 98(5), 1766-1797.
- Rosen, S. (1974). Hedonic Prices and Implicit Markets: Product Differentiation in Pure Competition. *Journal of Political Economy*, 82(1), 34-55.
- Rosenbaum, P. R., & Rubin, D. B. (1983). The Central Role of the Propensity Score in Observational Studies for Causal Effects. *Biometrika*, 70(1), 41-55.
- Rosenthal, S. S., & Strange, W. C. (2001). The Determinants of Agglomeration. *Journal of Urban Economics*, 50(2), 191-229.

- Rossi-Hansberg, E., Sarte, P.-D., & Owens, R. (2010). Housing Externalities. *Journal of Political Economy*, 118(3), 485-535.
- Santiago, A. M., Galster, G. C., & Tatian, P. (2001). Assessing the property value impacts of the dispersed housing subsidy program in Denver. *Journal of Policy Analysis and Management*, 20(1), 65-88.
- Schaeffer, P. V., & Millerick, C. A. (1991). The impact of historic district designation on property values: An empirical study. *Economic Development Quarterly*, 5(4), 301-312.
- Schwartz, A. E., Ellen, I. G., Voicu, I., & Schill, M. H. (2006). The external effects of place-based subsidized housing. *Regional Science and Urban Economics*, 36(6), 679-707.
- Senatsverwaltung für Stadtentwicklung Berlin. (1992). 18. Bericht über Stadterneuerung.
- Senatsverwaltung für Stadtentwicklung Berlin. (2001). 22. Bericht über Stadterneuerung.
- Senatsverwaltung für Stadtentwicklung Berlin. (2006). *Urban and Environmental Information System*. Berlin.
- Senatsverwaltung für Stadtentwicklung Berlin. (2012). 28. Bericht über Stadterneuerung.
- Silverman, B. W. (1986). Density Estimation For Statistics and Data Analysis. *Monographs on Statistics and Applied Probability*.
- Sinai, T., & Waldfoegel, J. (2005). Do low-income housing subsidies increase the occupied housing stock? *Journal of Public Economics*, 89(11), 2137-2164.
- Tabuchi, T., & Yoshida, A. (2000). Separating Urban Agglomeration Economies in Consumption and Production. *Journal of Urban Economics*, 48(1), 70-84.



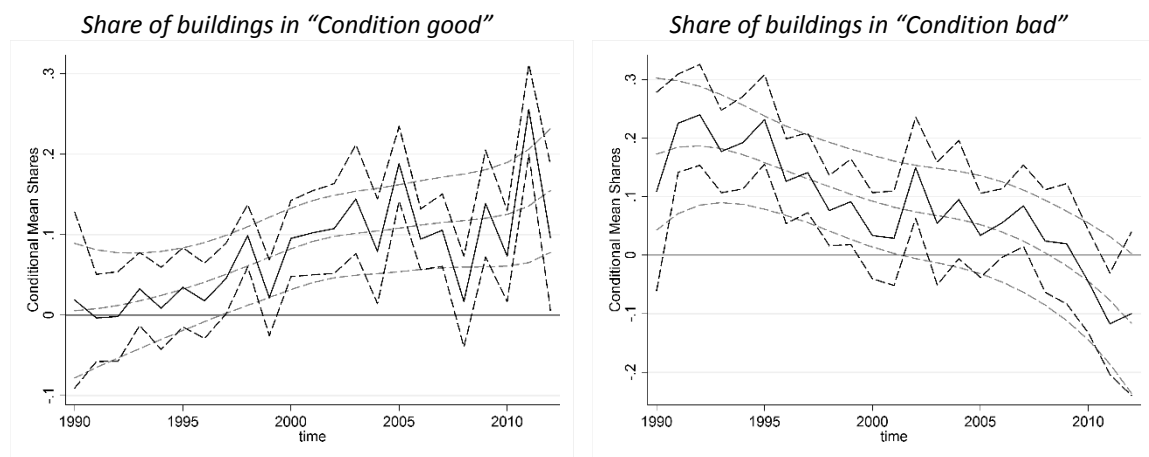
## Figures

Fig. 1. Study area



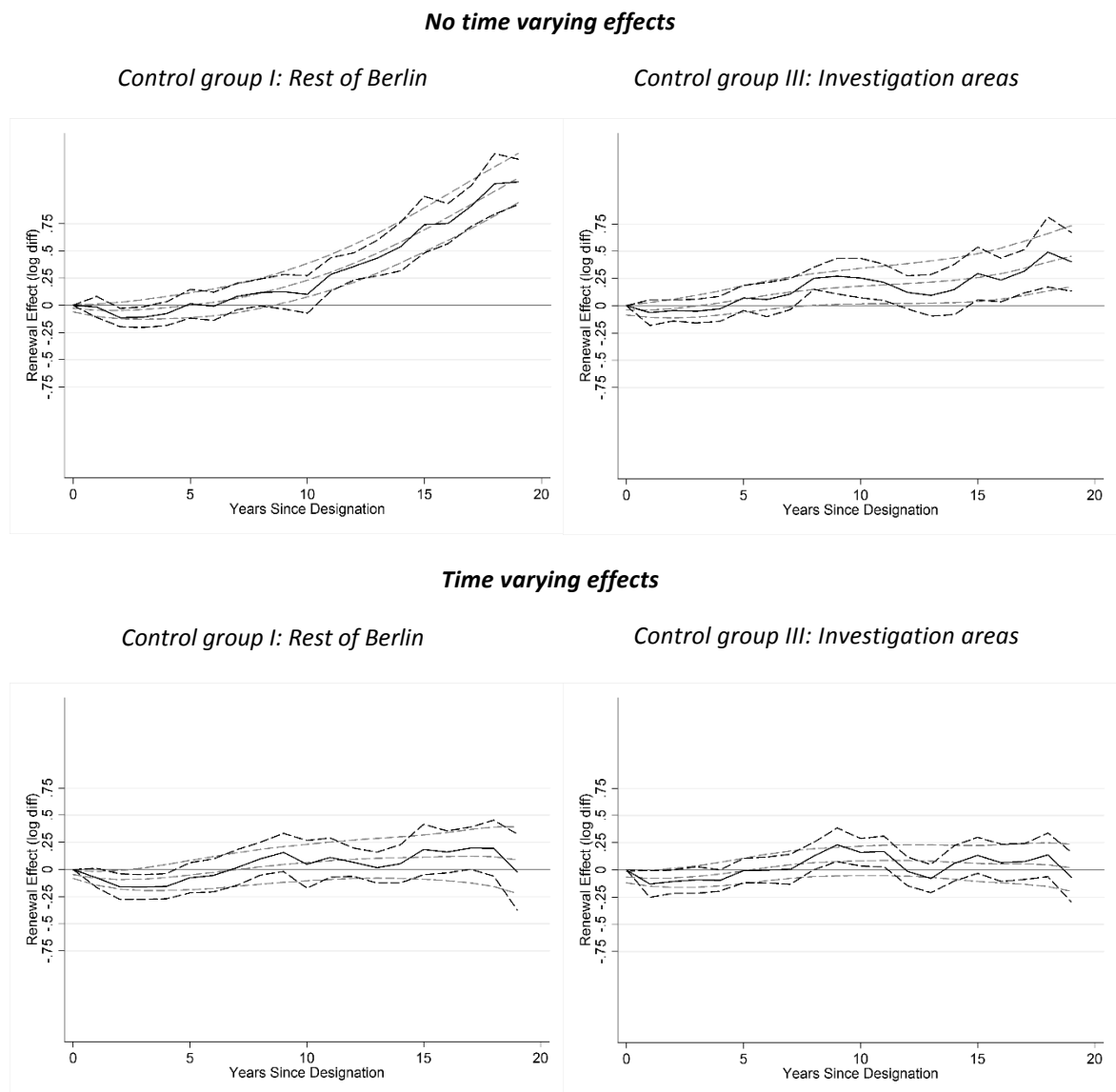
Notes: Own illustration based on the urban and environmental information system (Senatsverwaltung für Stadtentwicklung Berlin, 2006). Crosshatched (hatched) areas indicate renewal (investigation) areas. Crosses are the matched transactions in control group IV. Smoothly shaded areas represent the consumption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

**Fig. 2. Maintenance trends in renewal areas**



Notes: Year specific differences in mean shares are estimated in two separate regressions of the following type:  $Y_{it} = \sum_t \gamma_t T_i \times \varphi_t + \sum_o X_{ot} + \varphi_t + \epsilon_{it}$ , where  $Y_{it}$  indicates whether a property at time  $t$  was in good (left) or poor (right) maintenance and  $X_o$  controls for the following property features: age, plot area, and floor space index.  $T_i$  is an indicator variable discriminating between whether a property falls within a renewal area ( $T=1$ ) or within an investigation area ( $T=0$ ). Black solid (dashed) lines indicate  $\gamma_t$  point estimates (95% confidence intervals). Grey dashed lines are lowess smoothes of the parameters.

**Fig. 3. Price trends in renewal areas, relative to varying control groups**



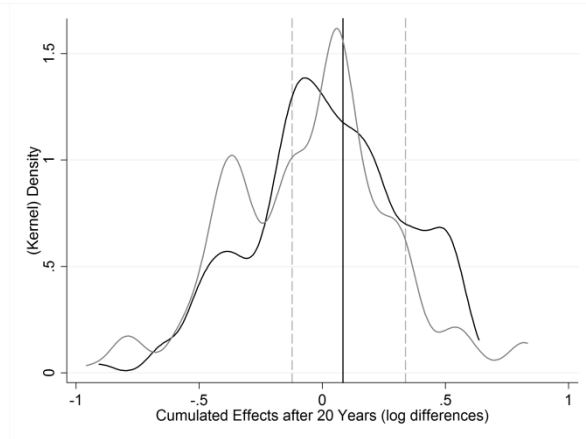
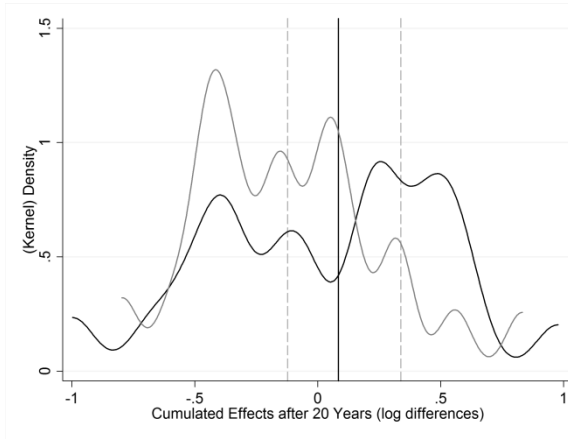
Notes: Black solid (dashed) lines indicate treatment point estimates (95% confidence intervals). Grey dashed lines are lowest smooths of the parameters.

**Fig. 4. Varying numbers of treated and control areas: Distribution of point estimates**

*Separately varying number of treatment (black) & control areas (grey)*

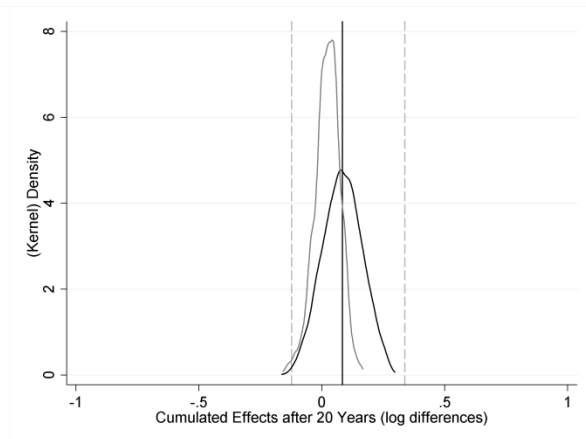
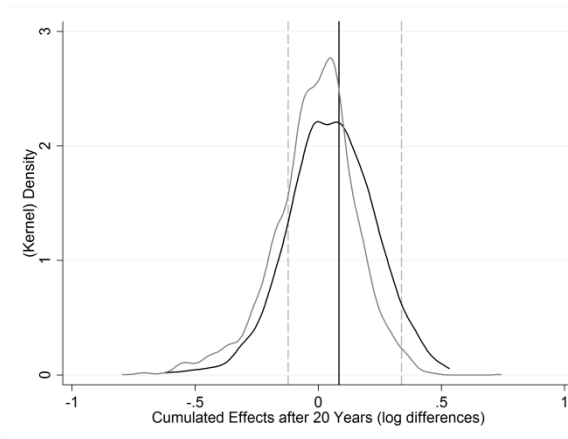
*1 random area selected (1 vs 22)*

*2 random areas selected (2 vs 22)*



*5 random areas selected (5 vs 22)*

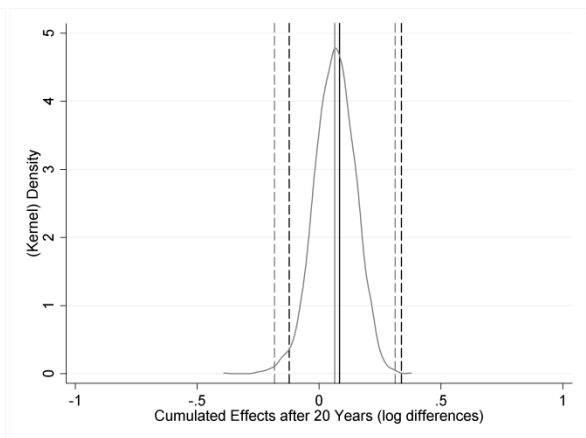
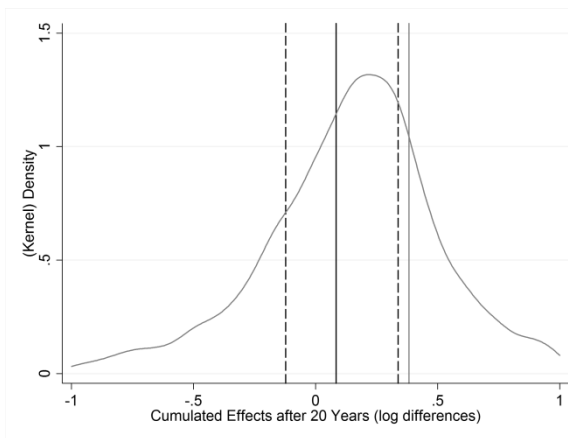
*15 random areas selected (15 vs 22)*



*Simultaneously varying number of treatment & control areas*

*5 random areas selected (5 vs 5)*

*15 random areas selected (15 vs 15)*



Notes: In the upper two rows, black (grey) 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. Renewal area treatment effects**

	(1)	(2)	(3)	(4)	(5)	(6)
Control group	All	All < 2 km	Investigation areas	Matched observations	Investigation areas	Matched observations
	(I)	(II)	(III)	(IV)	(III)	(IV)
<i>T x POST</i> (within renewal)	-0.162*** (0.036)	-0.115*** (0.037)	-0.060 (0.039)	-0.139*** (0.052)	-0.120*** (0.045)	-0.026 (0.061)
<i>T x V</i> (years since designation)	0.047*** (0.003)	0.039*** (0.004)	0.023*** (0.004)	0.025*** (0.004)	0.010*** (0.004)	0.005 (0.005)
Cum. effect after 20 years	119.4%*** (6.62%)	94.49%*** (6.84%)	49.79%*** (7.32%)	42.7%*** (9.79%)	7.33% (7.32%)	8.3% (11.12%)
Av. appr. rate	4.01%	3.38%	2.04%	1.79%	0.35%	0.4%
Observations	64,677	17,447	8,623	8,860	8,623	8,860
R <sup>2</sup>	0.802	0.772	0.632	0.710	0.677	0.735
AIC	79,932.8	25,276.8	12,347.3	13,477.5	11,778.3	13,226.6
Hedonic controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Block effects	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES
Time-varying effects	NO	NO	NO	NO	YES	YES

Notes: Standard errors in parentheses are clustered on statistical blocks in all models. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . 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.

**Tab. 2. Varying groups of treated and controls**

No of areas		Cumulated effect after 20 years					% within 2 S.E. length of bench.
Treat.	Control	Iterations	Mean	S.D.	Min	Max	
<i>Varying number of treated areas</i>							
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%
<i>Varying number of control areas</i>							
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%
<i>Varying number of treated and control areas</i>							
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 2500 randomly drawn selections.

Gabriel M. Ahlfeldt<sup>\*</sup>, Wolfgang Maennig<sup>^</sup> & Felix J. Richter<sup>♦</sup>

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

*Version: May 2014*

## 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 section 4 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 mill. inhabitants and approximately 1.9 mill. dwelling units. Approximately 14% of the population is non-German citizens. While there have recently been

---

<sup>\*</sup> London School of Economics (LSE), Department of Geography and Environment & Spatial Economics Research Centre (SERC), Houghton Street, London WC2A 2AE, [g.ahlfeldt@lse.ac.uk](mailto:g.ahlfeldt@lse.ac.uk), [www.ahlfeldt.com](http://www.ahlfeldt.com)

<sup>^</sup> University of Hamburg, Department of Economics, Von Melle Park 5, 20146 Hamburg, [wolfgang.maennig@wiso.uni-hamburg.de](mailto:wolfgang.maennig@wiso.uni-hamburg.de), [www.uni-hamburg.de/economicpolicy](http://www.uni-hamburg.de/economicpolicy)

<sup>♦</sup> University of Hamburg, Department of Economics, Von Melle Park 5, 20146 Hamburg, [felix.richter@wiso.uni-hamburg.de](mailto:felix.richter@wiso.uni-hamburg.de), [www.uni-hamburg.de/economicpolicy](http://www.uni-hamburg.de/economicpolicy)

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

## 2.2 Renewal and investigation areas

This (sub) section 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 (grey) 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 500 m buffer), while geographically proximate but structurally different areas (for example Wedding) are underrepresented. Some technical details on the matching technique are discussed in the next subsection.

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

*– Figure A1 about here –*

*– Table A1 about here –*

Table A2 provides a comparison of the renewal areas, the investigation areas, and the rest of Berlin. While there are some differences, the structural similarities between the renewal areas and the investigation areas are striking when compared to the rest of Berlin. The housing stock is much older than in the rest of Berlin, and the floor space index is higher. The reason is, in part, that single-family houses are practically not existent in the centrally located renewal and investigation areas, while naturally abundant in the peripheral parts of the rest of the city. Renewal areas and the investigation areas are relatively homogeneous areas dominated by buildings constructed around the turn of the 19<sup>th</sup> and 20<sup>th</sup> centu-



ries (the so called founding period / “Gründerzeit”). These are primarily apartment blocks, often with some commercial units on the ground floor.

– Table A2 about here –

### 2.3 Renovation subsidies vs. property value

To put the €1.94 bill. invested in renewal areas into some 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 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:

$$P_{it} = \alpha + \sum_k \gamma_k X_{kit} + \sum_{t \neq \{1998, \dots, 2002\}} \varphi_t + \theta_i + \varepsilon_{it}$$

, 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.  $X_k$  are observable property characteristics discussed in the main paper, and  $\theta_i$  is a set of property fixed effects holding all time-invariant location effects constant. The time effects  $\varphi_t$  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 (5844), which results in a total value of €5.42 bill. The total expenditures attributable to the renewal policy, thus, amount to as much as 35.7% of the property value in the targeted areas.

### 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, with between 2,200 and 6,000 observations per year. Of these transactions, 4,500 occurred inside our renewal areas. The transactions are compared to varying control groups, where the direct surroundings within a 500 m buffer of each renewal area are excluded from every control group. The rationale is to ensure that the counterfactual provided by the control groups is not contaminated by spillover effects. Control group I comprises all other transactions (outside the 500 m buffer) and control

group II all transactions in a 500-2,000 meter radius around the renewal areas. Control group III consists of the fractions of the investigation areas outside the 500 m buffer and includes approximately 4,000 transactions. The matching procedure discussed below results in 4,200 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 & 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 Table 1 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 control group is substantially reduced. Table A3 reports the average treatment effect on the treated (ATT) and several measures of the balance of the covariates for the control group IV.

*- Table A3 about here -*

### **3 Baseline models: complementary evidence**

This section complements section 4 of the main paper. The first sub-section provides an overview over the variables and presents some of the estimation results omitted in the main paper. Section 3.2 presents results for alternating combinations of the time varying effects, while section 3.3 considers an alternative way to account for the spatial autocorrelation of the standard errors in our model. Section 3.4 evaluates possible designation effects on the runner-up areas that remained unconsidered. In section 3.5, we replicate our benchmark results using an urban amenity density measure based on historic data.

#### **3.1 Complete results**

Table A4 provides descriptive statistics for all structural and locational variables. Table A5 extends Table 1 in the main paper by presenting the implicit hedonic prices of the structural characteristics.

– Table A4 about here –

– Table A5 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 logprices (relative to manufacturing sites). The age of a building significantly decreases its (log) price.

### 3.2 Time-varying effects

In our preferred models (Table 1, column 5 and 6 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 as well as 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.<sup>1</sup>

To address this concern we replicate the baseline models using a number of less extensive combinations of time-varying effects. Table A6 displays the specification from Table 1 (column (5) and (6)) from the main paper using only the year x district effects (columns (1) and (2)), only 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 investigation areas (control group III, columns (1), (3), and (5)) and the matched observations (control group IV, columns (2), (4), and (6)) react differently to the inclusion of the different time varying effects. While the district x year effects drive the results for the investigation areas down, the matched observations are strongly influenced by the consumption amenity effects. These results indicate that within districts renewal and investigation areas were located in areas with a similar amenity endowment. The matched control group does not account for trends related to the amenity endowment, which is conclusive given that we did not use this variable as a covariate in the PSM procedure. Because we matched on the general location in the city using x- and y- coordinates as PSM covariates, the insen-

---

<sup>1</sup> This problem is a variant of the “bad control problem” (Angrist & Pischke, 2009).

sitivity to the inclusion of district x year effects when control group 4 is used does not come as a surprise. An important insight arises from model (3) in Table A6. If the comparison between renewal and investigation areas is not restricted to the within district level, the estimated treatment effect is by orders of magnitude larger (3 vs. 1). This indicates significant heterogeneity in appreciation trends across districts. Given that the vast majority of renewal areas were designated in former East Berlin and the likely heterogeneous trends between the formerly separated parts of the city as these reintegrate to a common housing market area a respective control for such heterogeneity seems particularly important. In columns (5) and (6) we therefore allow for trend heterogeneity with respect to the amenity density and a location within former East Berlin exclusively. This specification is significantly less demanding than the benchmark specification. Yet, both specification produce insignificant and near to zero treatment effects, which increases our confidence in the benchmark models.

– Table A6 about here –

### **3.3 Heteroscedasticity-autocorrelation consistent 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 an alternative approach to controlling for spatial dependence of the error we adopt the procedure suggested by Conley (1999). Using varying distance cutoffs, we calculate standard errors corrected for spatial autocorrelation, serial correlation and heteroscedasticity adapted for panel data as in Hsiang (2010). Table A7 displays the point estimates from the OLS regression as in Table 1 (column (5)) in the main paper, the clustered standard errors (column (1)), and the HAC corrected standard errors for various distance cutoffs (columns (2) to (7)). With a 50km cutoff, 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 cutoff we tend to get smaller standard errors, pushing the treatment effect towards significance. For the treatment effect to be

statistically significant we need to reduce the cutoff distance to values that are way below conventional thresholds.<sup>2</sup> We conclude that the treatment effects are not only economically small, but should also be viewed as statistically not distinguishable from zero.

– Table A7 about here –

### 3.4 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 would violate this assumption if the selection of those being treated changed 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 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 A8, we report the results of two models that are analogous to (4) and (6) in Table 1 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. Setting the placebo designation date to the date of the nearest renewal area changes the results only marginally.

The cumulated effects after 20 years for the investigation areas 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 1 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. While a negative level shift with a compensating positive trend is found in the model (1), the effect is not robust to the inclusion of time-varying

---

<sup>2</sup> For US Census data, distance cutoffs are often set at approximately 10 miles (Boarnet, Chalermpong, & Geho, 2005; Jeanty, Partridge, & Irwin, 2010).

effects. Our preferred model (2) also suggests that the cumulated long-run effect of the renewal areas is not statistically distinguishable from the remaining investigation areas. Taken together, the evidence does not indicate that the runner-up areas provide an invalid counterfactual. To the contrary, the results provide further evidence that the policy had marginal impact only because the trends within the group of selected and remaining investigation areas are very similar.

– Table A8 about here –

### 3.5 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 for these urban amenities over time. 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 stems from the open street map project and provides a fairly good overview of the distribution of the urban amenities during the study period. One concern, however, 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 post codes in a new format that applies to both parts of the formerly divided city and allows for precise geocoding. Figure A2 compares the resulting kernel smoothed density surface (left panel) with the existing density surface displayed in Figure 1 in the main paper (right panel): While there is a slight but notable shift in amenity gravity from the south western to the eastern downtown areas, the overall spatial pattern has remained remarkably stable over more than 15 years of convergence to a new post-Berlin Wall equilibrium.

– Figure A2 about here –

Table A9 replicates our primary results 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 negligible (below 1 percentage point difference after 20 years). As in our main results, no effects are significantly different from zero.

– Table A9 about here –

## 4 Externalities and spillover effects

One justification for public expenditures 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., an (housing) 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 the 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:

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

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 value, we essentially employ the difference-in-difference methodology that compares the value of properties at different points in time (first difference  $\Delta$ ) and at different locations (second difference  $d$ ). We assume that a change in policy  $\Delta S$  only becomes effective in a treatment neighborhood ( $T$ ), but not in an otherwise comparable control neighborhood ( $C$ ) that is subject to the same macroeconomic shocks ( $\Delta Y^T = \Delta Y^C$ ).<sup>3</sup>

Our treatment effect can be described as follows:

---

<sup>3</sup> 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.



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

Or:

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

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:

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

There are important implications for our empirical strategy that aims to estimate  $\beta$ . Given an appropriately defined control group, the difference-in-difference 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 ( $\beta = \text{cycy} (\beta = (\partial P/\partial E) (\partial I/\partial S))$ ). 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 in 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:

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

### *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 are 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, we theoretically 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 two binary variables denoting 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 far from perfect. Further, 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:

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

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) was fairly limited. Irrespec-

tively 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 approach above 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 on building maintenance we use 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  $D$ :

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

This approach also mitigates another concern, namely, that authorities reserve the right to levy the increase in land value generated by the policy (“Ausgleichsabgabe”). Until the end of 2011, local authorities generated €68 mill. (\$93.3 mill.) in levies. The total expected levies estimated by the local administration amount to €211 mill. (\$285.3 mill.) based on an estimated average increase in land value of €45 (\$60.8) per m<sup>2</sup> (Senatsverwaltung für Stadtentwicklung Berlin, 2012), which are strikingly low figures compared to the above mentioned investment volumes.<sup>4</sup> While these payments are in practice small, property prices could be negatively affected, at least up to the point where the levy has actually been charged.

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 500 m buffer area and zero otherwise. We run this

---

<sup>4</sup> All income generated through this source is to be reinvested in the district’s infrastructure or neighborhood improvements.

specification using the two treatment functions introduced above and varying control groups. Focusing on the parametric specification and our preferred control group, we then use an alternative treatment measure  $T_{s2i} = \beta_S T_{S1i} + \beta_{S2} DISTSi$ , where  $DISTS$  is the distance to the nearest renewal area.

#### *Control groups*

For the spillover models just described 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 10,200 obs.), and a to a matched group (A-IV, includes 10,300 obs.). We use the same PSM matching technique as described in 2.3 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 A10 reports the average treatment effect on the treated (ATT) and several measures of the balance of the covariates for the matched control group.

– Table A10 about here –

### **4.3 Empirical results**

#### *Building quality*

Table A11 replicates our baseline approach using only buildings in good physical condition. The estimated effects tend to decline relative to the comparable benchmark models in Table 1 in the main paper. Moreover, the results are relatively unstable across varying control groups, and none of the effects are estimated at satisfying levels of statistical significance. While this may be partially driven by the reduction in observations and loss of degrees of freedom (which also leads us to not estimate the demanding model with time varying effects on this sample, compare columns 5 and 6 of Table 1 in the main paper), the results are at least indicative that the benchmark results are not primarily driven by externality effects.

– Table A11 about here –

### *Spillover effects*

Table A12 and Figure A3 replicate the benchmark analysis for the spillover areas, i.e., the 500 m buffer just outside 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 500 m 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. The results are easily summarized. For our preferred control groups (A-III and A-IV), we find results that are within the same range as the effects in the baseline model (columns 3 and 4). The revitalization effect, however, is statistically indistinguishable from zero once we control for independent appreciation trends by means of time-varying effects.

– Table A12 about here –

– Figure A3 about here –

### *Spatio-temporal trends*

One might be concerned that the non-significant spillovers we find are due to a relatively steep spatial decay and, hence, an impact area that is small relative to the 500 m spillover/buffer area used. We have therefore repeated our approach allowing for spatio-temporal trends. Restricting the sample to the 500 m buffer area around the renewal areas, we first use a *POSTxDIST* interaction term between an indicator variable denoting the period after designation (*POST*) and the distance to the renewal area (*DIST*) to allow for a change in the spatial trend after the designation. Second, we include *YSDxDIST*, an interaction between the distance to the nearest renewal area and the years since designation (*YSD*), allowing for the spatial trend to vary over time. Transactions in renewal areas and beyond the 500 m buffer are excluded from the sample. The results for both specifications are presented in Table A13. As all relevant coefficients are insignificant, we conclude that there is no significant change in the spatial trend, neither directly after designation, nor gradually emerging over the years.

– Table A13 about here –

*Spillover effects – building quality and historic amenity density*

For completeness, we also replicate the estimation of spillover effects including the building quality controls (Table A14) and the spillover effects including the historic amenity densities introduced in section 3.3. of the appendix (Table A15). The results are robust to both perturbations: The effects with maintenance indicators tend to have a similar magnitude as the main spillover effects and, as expected, we observe strong price effects associated with the physical condition of the building: Properties in good or normal condition generate a large price premium compared to buildings in poor condition. Table A15 shows the spillover effects compared to the respective control groups when we employ the historic amenity densities. The results differ only marginally from the original spillover results.

– Table A14 about here –

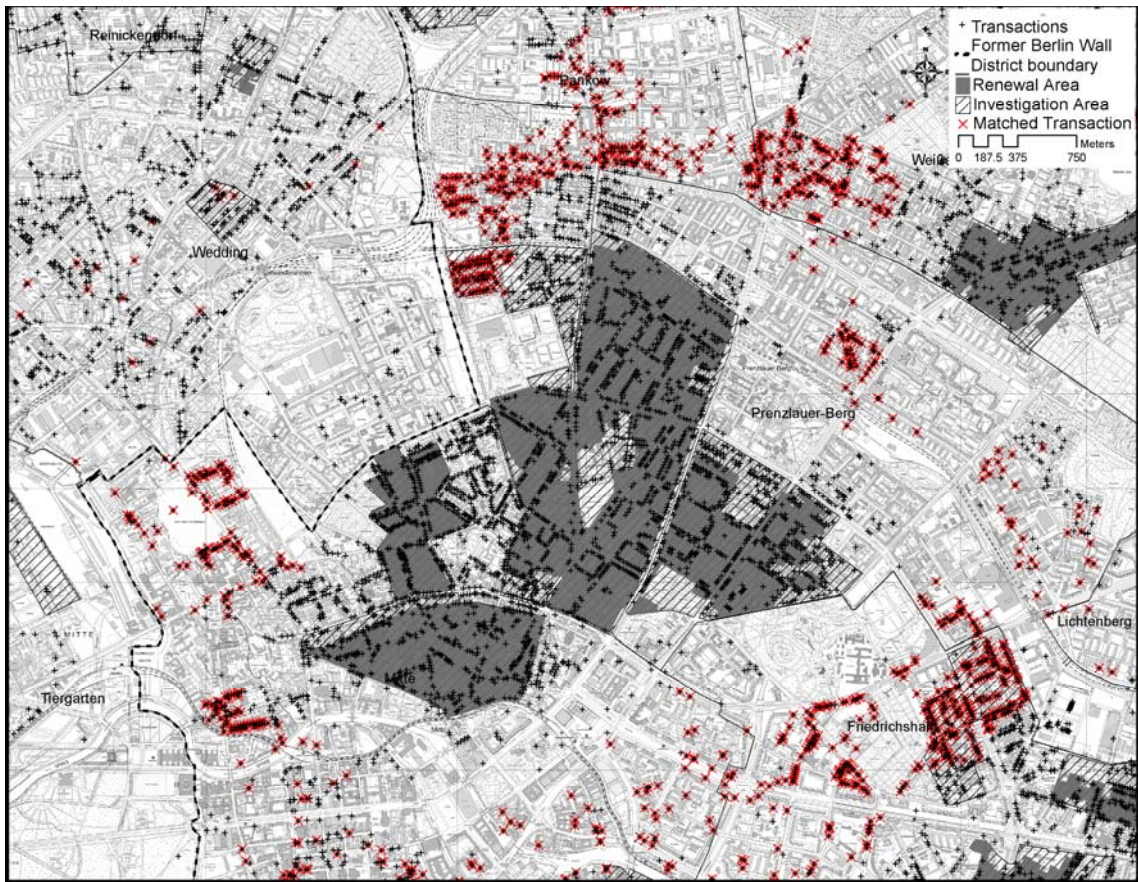
– Table A15 about here –

## Literature

- Angrist, J. D., & Pischke, J.-S. (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton, New Jersey: Princeton University Press.
- Boarnet, M. G., Chalermpong, S., & Geho, E. (2005). Specification issues in models of population and employment growth\*. *Papers in Regional Science*, 84(1), 21-46.
- Conley, T. G. (1999). GMM estimation with cross sectional dependence. *Journal of Econometrics*, 92(1), 1-45.
- Hsiang, S. M. (2010). Temperatures and cyclones strongly associated with economic production in the Caribbean and Central America. *Proceedings of the National Academy of Sciences*, 107(35), 15367-15372.
- Jeanty, P. W., Partridge, M., & Irwin, E. (2010). Estimation of a spatial simultaneous equation model of population migration and housing price dynamics. *Regional Science and Urban Economics*, 40(5), 343-352.
- Leuven, E., & Sianesi, B. (2003). PSMATCH2: Stata module to perform full Mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing (Version S432001): Boston College Department of Economics.
- Rosenbaum, P. R., & Rubin, D. B. (1983). The Central Role of the Propensity Score in Observational Studies for Causal Effects. *Biometrika*, 70(1), 41-55.
- Rosenbaum, P. R., & Rubin, D. B. (1985). Constructing a Control Group Using Multivariate Matched Sampling Methods That Incorporate the Propensity Score. *American Statistician*, 39(1), 33-38.
- Senatsverwaltung für Stadtentwicklung Berlin. (2006). *Urban and Environmental Information System*. Berlin.
- Senatsverwaltung für Stadtentwicklung Berlin. (2012). *28. Bericht über Stadterneuerung*.

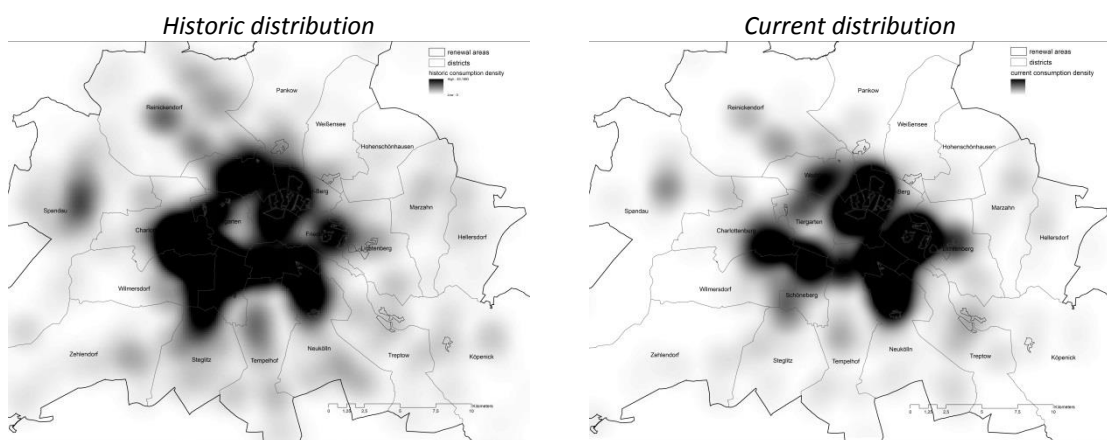
## Figures

**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).

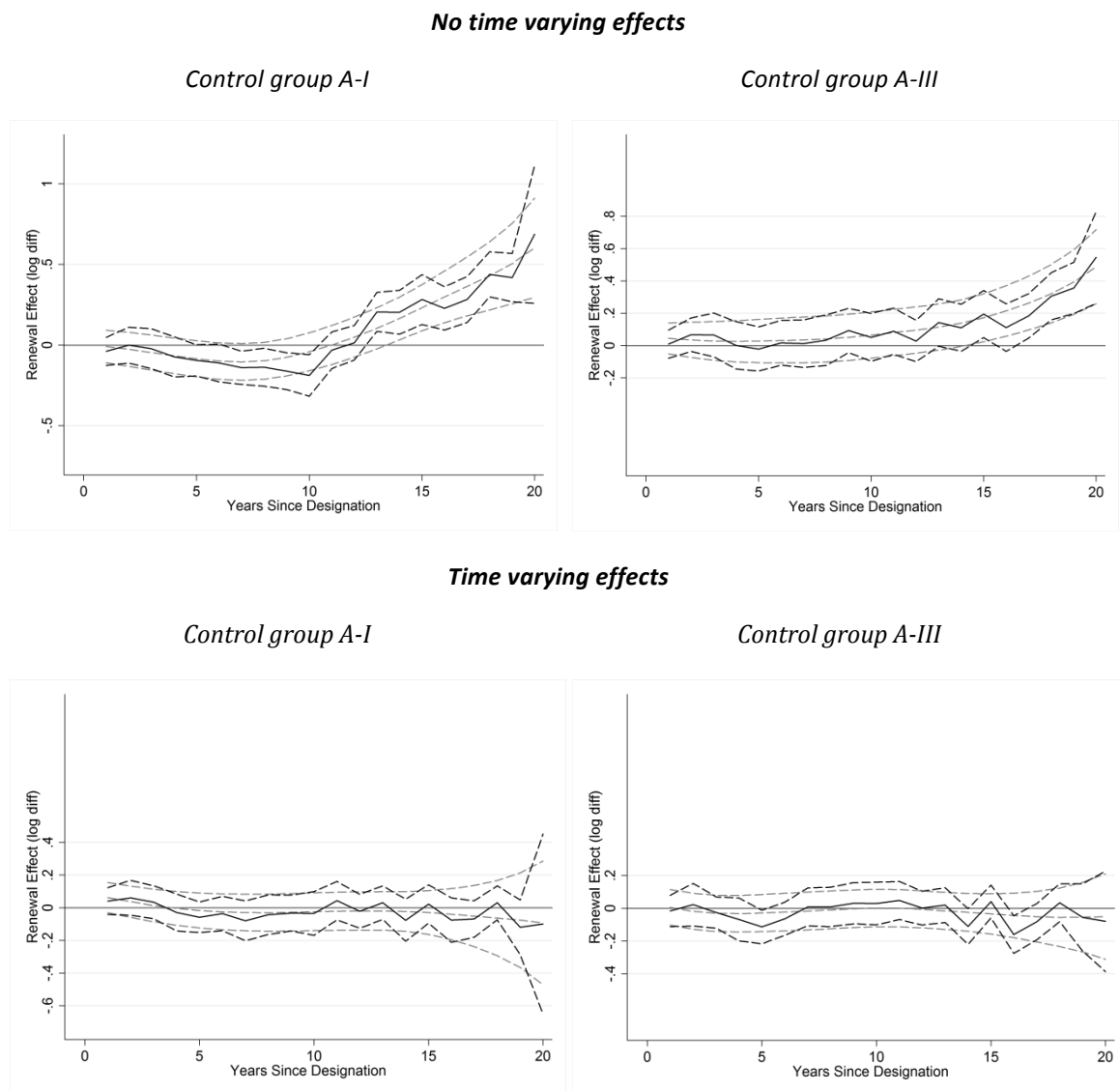
**Fig. A2. Kernel smoothed density surfaces comparison**



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



**Fig. A3. Price trends in spillover areas, relative to varying control groups**



Notes: Black solid (dashed) lines indicate treatment point estimates (95% confidence intervals). Grey dashed lines are lowest smoothed of the parameters.

## Tables

**Tab A1. Descriptive statistics renewal areas**

Name	Start	End	Area (km <sup>2</sup> )	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		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 after the end of our observation period (August 2012). The data for the areas “Komponistenviertel” and “Niederschöneweide” are from 2010.

**Tab A2. Comparative statistics**

	Renewal areas	Investigation areas	Rest of Berlin (without RENEWAL / INVEST)
Price (cpi adjusted)	€ 1,490,795.00 (€ 3,290,749.00)	€ 1,382,921.00 (€ 1,548,053.00)	€ 1,503,588.00 (€ 5,667,000.00)
Age	101.5 (22.8)	96.0 (23.6)	60.3 (36.5)
Floor space index	2.609 (0.981)	2.902 (1.074)	1.127 (1.230)
Average plot size	1058 (1834)	1003 (1481)	1798 (6515)
Share of foreigners	0.14 (0.14)	0.17 (0.11)	0.11 (0.13)
Single family home (%)	0.35	1.35	46.26
Apartment / buildings (%)	29.67	37.39	19.39
Mixed use buildings (%)	62.05	55.35	19.02
Commercial buildings (%)	3.69	1.68	2.35

Notes: Prices are in 2012 Euros. Standard deviations in parentheses.

**Tab A3. Descriptive statistics: matched control group IV**

Average treatment effect on the treated					
Sample	Treated	Controls	Difference	S.E.	T-stat
Unmatched	13.418	12.845	0.573	0.0159	35.9
Matched (ATT)	13.418	13.338	0.081	0.0210	3.82
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 (1, poor to 6, very good)	Unmatched	2.5171	3.7574	-61.4	
	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 (m)	Unmatched	127.43	198.62	-40.4	
	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 sub-samples (treated and control), computed as the percentage of the square root of the average of the sample variances in the treated and control groups.

**Tab A4. Descriptive statistics**

Variable	Mean	Std. Dev.	Min	Max
Price (constant 2012 €)	1,495,060	5,356,786	16,631	299,000,000
Plot area (m <sup>2</sup> )	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 transport (rail) (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 building (m)	230.9044	270.7093	0.2341669	2829.887
Street noise level (db)	57.42288	9.529247	15.0819	94.5513
<i>Location within block</i>				
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 A5. Complete results**

Model	(1)	(2)	(3)	(4)	(5)	(6)
	All	All < 2 km	Investigation areas	Matched observations	Investigation areas	Matched observations
Control group	(I)	(II)	(III)	(IV)	(III)	(IV)
<i>T x POST</i> (renewal)	-0.162*** (0.036)	-0.115*** (0.037)	-0.060 (0.039)	-0.139*** (0.052)	-0.120*** (0.045)	-0.026 (0.061)
<i>T x V</i> (years since des.)	0.047*** (0.003)	0.039*** (0.004)	0.023*** (0.004)	0.025*** (0.004)	0.010*** (0.004)	0.005 (0.005)
Building age	-0.014*** (0.001)	-0.012*** (0.001)	-0.012*** (0.002)	-0.011*** (0.002)	-0.012*** (0.002)	-0.010*** (0.002)
Building age, squared	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000** (0.000)
Industrial area indicator	0.128* (0.072)	-0.112 (0.106)	0.127 (0.172)	0.257 (0.221)	0.147 (0.174)	0.408* (0.243)
Residential area indicator	0.022 (0.048)	-0.084 (0.079)	0.019 (0.120)	0.035 (0.147)	0.043 (0.114)	0.163 (0.159)
Commercial use indicator	0.356*** (0.061)	0.221** (0.089)	0.256* (0.151)	0.336* (0.172)	0.219 (0.147)	0.427** (0.184)
Plot area	0.000** (0.000)	0.000 (0.000)	0.000 (0.000)	0.000** (0.000)	-0.000 (0.000)	0.000* (0.000)
Floorspace	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000** (0.000)	0.000*** (0.000)	0.000** (0.000)
Building at a corner	0.208*** (0.041)	0.236*** (0.066)	0.219** (0.104)	0.322*** (0.106)	0.223** (0.102)	0.311*** (0.104)
Build. with mult. fronts	0.348*** (0.065)	0.117 (0.102)	0.168 (0.156)	0.073 (0.162)	0.194 (0.152)	0.090 (0.160)
Hammer type Building	-0.107** (0.043)	-0.206 (0.133)	-0.087 (0.258)	-0.446 (0.336)	-0.098 (0.326)	-0.445 (0.310)
Build. in inner block loc.	-0.131*** (0.042)	-0.270*** (0.085)	-0.381*** (0.146)	-0.420*** (0.147)	-0.432*** (0.145)	-0.470*** (0.147)
Observations	64,677	17,447	8,623	8,860	8,623	8,860
R <sup>2</sup>	0.802	0.772	0.632	0.710	0.677	0.735
AIC	79,932.8	25,276.8	12,349.3	13,477.5	11,776.3	13,224.6
Hedonic controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Block effects	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES
Time-varying effects	NO	NO	NO	NO	YES	YES

Notes: Expanded version of Table 1 in the main paper. Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . 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. 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.

**Tab A6. Renewal area effects with distinct time-varying effects**

	(1)	(2)	(3)	(4)	(5)	(6)
Control group	Investigation areas (III)	Matched observations (IV)	Investigation areas (III)	Matched observations (IV)	Investigation areas (III)	Matched observations (IV)
<i>T x POST</i> (within renewal)	-0.140*** (0.044)	-0.084 (0.056)	-0.032 (0.038)	-0.048 (0.053)	-0.096** (0.041)	-0.055 (0.054)
<i>T x V</i> (years since designation)	0.012*** (0.004)	0.021*** (0.005)	0.020*** (0.004)	0.005 (0.005)	0.007* (0.004)	0.001 (0.005)
Cum. effect after 20 years	10.19% (7.5%)	39.52%*** (12.19%)	44.51%*** (6.99%)	4.94% (10.13%)	4.25% (7.13%)	-3.24% (9.88%)
Av. appr. rate	0.49%	1.68%	1.86%	0.24%	0.21%	-0.16%
Observations	8623	8860	8623	8860	8623	8860
R <sup>2</sup>	0.674	0.732	0.642	0.717	0.652	0.721
AIC	11785.6	13256.1	12168.3	13305.9	11955.1	13245.4
Hedonic controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Block effects	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES
<i>Time-varying effects:</i>						
District x year effects	YES	YES	NO	NO	NO	NO
Consumption. amenities x year effects	NO	NO	YES	YES	YES	YES
East Berlin x year effects	NO	NO	NO	NO	YES	YES

Notes: Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . 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 district effects, East Berlin effects, or a consumption amenity measure described in the data section of the main paper.

**Tab A7. Robustness of the renewal area effects with HAC standard errors.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Point estimates	Standard errors						
	Clustered OLS	Spatial HAC	Spatial HAC	Spatial HAC	Spatial HAC	Spatial HAC	Spatial HAC
Distance cutoff		0.5km	1km	2km	5km	10km	50km
SAN x POST	-0.120 (0.045)***	(0.033)***	(0.036)***	(0.032)***	(0.035)***	(0.037)***	(0.043)***
SAN x years	0.010 (0.004)***	(0.002)***	(0.003)***	(0.002)***	(0.003)***	(0.004)***	(0.004)***
Cum. effect	7.33% (7.32%)	(3.17%)**	(4.09%)*	(3.37%)**	(5.47%)	(6.71%)	(6.98%)
Observations	8623						
R <sup>2</sup>	0.677						

Notes: Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . SAN x POST, SAN x year (since designation) and cum.(ulated) effect are defined exactly as in Table 1 in the main paper. All models include hedonic and location controls, block fixed effects, year effects, and time varying effects as described e.g. in the data section of the main paper. Spatial HAC denotes non-parametric heteroskedasticity–autocorrelation consistent standard errors accounting for spatial autocorrelation and serial correlation allowing for a lag length of 23 years (our observation period).

**Tab A8. Placebo designation effects on investigation areas:  
Renewal and investigation areas vs. matched control group**

Control group	(1)		(2)	
	Matched observations (IV)			
<i>Treatment renewal areas:</i>				
$T^{REN} \times POST^{REN}$ (within renewal)	-0.001	(0.059)	-0.084	(0.063)
$T^{REN} \times V^{REN}$ (years since designation)	0.021***	(0.007)	0.011**	(0.005)
Cum. effect after 20 years	51.50%***	(12.8%)	14.58%*	(8.28%)
Av. appr. rate	2.1%		0.68%	
<i>Investigation areas:</i>				
$T^{INV} \times POST^{INV}$ (within investigation)	-0.084	(0.055)	0.037	(0.047)
$T^{INV} \times V^{INV}$ (years since designation)	0.010*	(0.006)	0.003	(0.005)
Cum. effect after 20 years	12.34%	(10.32%)	10.31%	(9.68%)
Av. appr. rate	0.58%		0.49%	
Observations	12,121		12,121	
R <sup>2</sup>	0.578		0.609	
AIC	21,933.0		21,136.6	
Hedonic controls	YES		YES	
Location controls	YES		YES	
Block effects	YES		YES	
Year effects	YES		YES	
Time-varying effects	NO		YES	

Notes: Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . 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.

**Tab A9. Renewal area effects with historic amenities**

Control group	(1)	(2)
	Investigation areas (III)	Matched observations (IV)
$T \times POST$	-0.113**	-0.015
(within renewal)	(0.044)	(0.062)
$T \times V$ (years since designation)	0.009***	0.005
	(0.004)	(0.005)
Cum. effect after 20 years	7.97%	9.19%
	(7.15%)	(10.73%)
Av. appr. rate	0.38%	0.44%
Observations	8,623	8,860
R <sup>2</sup>	0.677	0.736
AIC	11,788.5	13,211.7
Hedonic controls	YES	YES
Location controls	YES	YES
Block effects	YES	YES
Year effects	YES	YES
Time-varying effects	YES	YES

Notes: Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . 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.

**Tab A10.Descriptive statistics of matched control group A-IV**

Average treatment effect on the treated					
Sample	Treated	Controls	Difference	S.E.	T-stat
Unmatched	13.531	12.790	0.741	0.011	66.84
Matched (ATT)	13.531	13.777	-0.247	0.015	-16.26
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 (1, poor to 5, very good)	Unmatched	2.7652	3.8524	-51.8	
	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	Unmatched	5006.4	9667.8	-121.5	
	Matched	4980.7	5402.4	-11	91
Distance to park	Unmatched	2302.5	1595.4	55.4	
	Matched	2293.1	1973	25.1	54.7
Distance to main street	Unmatched	125.19	208.07	-45.2	
	Matched	125.12	115.84	5.1	88.8
Distance to water	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 sub-samples (treated and control) as the percentage of the square root of the average of the sample variances in the treated and control groups.

**Tab A11.Renewal area treatment effects - buildings in good quality**

	(1)	(2)	(3)	(4)
Control group	All (I)	All < 2 km (II)	Investigation areas (III)	Matched observations (IV)
<i>T x POST</i>	0.008	-0.070	0.084	0.500
(within renewal)	(0.338)	(0.416)	(1.063)	(1.232)
<i>T x V</i> (years since designation)	-0.000	-0.002	-0.026	-0.001
	(0.012)	(0.014)	(0.019)	(0.019)
Cum. effect after 20 years	0.67% (39.49%)	-9.84% (50.23%)	-35.94% (187.56%)	62.55% (245.52%)
Av. appr. rate	0.003%	-0.52%	-2.2%	2.4%
Observations	15,406	2,567	787	948
R <sup>2</sup>	0.917	0.941	0.863	0.890
AIC	5,491.0	1,475.9	558.9	738.4
Hedonic controls	YES	YES	YES	YES
Location Controls	YES	YES	YES	YES
Block effects	YES	YES	YES	YES
Year effects	YES	YES	YES	YES
Time-varying effects	NO	NO	NO	NO

Notes: Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . 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.



**Tab A12. Renewal area spillover effects**

Model	(1)	(2)	(3)	(4)	(5)	(6)	
		< 2 km renewal area buff- er		Invest. areas + 1 km buffer	Matched observations	Invest. areas + 1 km buffer	Matched observa- tions
Control group	All (A-I)	(A-II)	(A-III)	(A-IV)	(A-III)	(A-IV)	(A-IV)
<i>T x POST</i> (within renewal)	-0.124** (0.052)	-0.145** (0.059)	-0.149** (0.061)	-0.095* (0.056)	-0.052 (0.056)	-0.057 (0.067)	-0.057 (0.067)
<i>T x V</i> (years since designation)	0.020*** (0.004)	0.023*** (0.004)	0.025*** (0.004)	0.024*** (0.004)	-0.000 (0.004)	0.000 (0.004)	0.000 (0.004)
Cum. effect after 20 years	31.6%*** (6.06%)	38.25%*** (6.54%)	42.9%*** (6.68%)	46.46%*** (7.06%)	-4.41% (5.63%)	-4.67% (8.41%)	-4.67% (8.41%)
Av. appr. rate	1.38%	1.63%	1.8%	1.93%	-0.23%	-0.24%	-0.24%
Observations	66,865	19,421	11,963	16,989	11,963	16,989	16,989
R <sup>2</sup>	0.690	0.657	0.605	0.636	0.671	0.662	0.662
AIC	113,544.3	36,076.0	21,172.5	32,244.7	19,093.6	30,244.1	30,244.1
Hedonic controls	YES	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES	YES
Block effects	YES	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES	YES
Time-varying effects	NO	NO	NO	NO	YES	YES	YES

Notes: Marginal effects; standard errors in parentheses. \* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01. 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.

**Tab A13. Spatio-temporal trends in spillovers**

	(1) log(price)	(2) log(price)
POSTxDIST	0.053	(0.149)
YSDxDIST		-0.104 (0.199)
YSD		0.015 (0.014)
		0.012 (0.029)
DIST	YES	YES
Hedonic controls	YES	YES
Location controls	YES	YES
Block effects	YES	YES
Year effects	YES	YES
Time-varying effects	YES	YES
Observations	6,636	6,636
R <sup>2</sup>	0.639	0.639
AIC	10,345.4	10,343.2

Notes: DIST is the distance to the nearest renewal area. Marginal effects; standard errors in parentheses. \* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01. 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.

**Tab A14. Spillover effects including quality controls**

	(1)	(2)	(3)	(4)	(5)	(6)
Control group	All (A-I)	< 2 km renewal area buff- er (A-II)	Invest. areas + 1 km buffer (A-III)	Matched observations (A-IV)	Invest. areas + 1 km buffer (A-III)	Matched observations (A-IV)
<i>T x POST</i> (within renewal)	-0.133*** (0.050)	-0.125** (0.056)	-0.113** (0.056)	-0.104* (0.054)	-0.041 (0.049)	-0.094 (0.066)
<i>T x V</i> (years since designation)	0.017*** (0.003)	0.019*** (0.003)	0.019*** (0.003)	0.019*** (0.004)	-0.002 (0.004)	0.003 (0.005)
Condition: good	0.307*** (0.015)	0.567*** (0.036)	0.598*** (0.036)	0.500*** (0.034)	0.571*** (0.034)	0.487*** (0.031)
Condition: bad	-0.283*** (0.013)	-0.263*** (0.021)	-0.256*** (0.021)	-0.290*** (0.024)	-0.257*** (0.021)	-0.279*** (0.023)
Observations	77,564	26,131	22,847	29,842	22,847	29,842
R <sup>2</sup>	0.724	0.704	0.687	0.655	0.729	0.696
AIC	141,269	48,362.4	41,278.9	56,782.8	38,084.7	53,474.5
Hedonic controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Block effects	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES
Time-varying effects	NO	NO	NO	NO	YES	YES

Notes: Marginal effects; standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Hedonic and location controls consist of covariates controlling for internal property and external location characteristics described in greater detail in the data section of the main paper. 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.

**Tab A15. Spillover effects with historic amenities**

	(1)	(2)
Control group	Invest. areas + 1 km buffer (A-III)	Matched observations (A-IV)
<i>T x POST</i> (within renewal)	-0.042 (0.050)	-0.034 (0.064)
<i>T x V</i> (years since designation)	0.002 (0.004)	0.005 (0.005)
Cum. effect after 20 years	-0.21% (5.07%)	6.76% (8.24%)
Av. appr. rate	-0.01%	-0.33%
Observations	11,963	16,989
R <sup>2</sup>	0.671	0.662
AIC	19,086.1	30,241.7
Hedonic controls	YES	YES
Location controls	YES	YES
Traffic cell effects	YES	YES
Year effects	YES	YES
Time-varying effects	YES	YES

Notes: Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . 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.