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## Urban renewal after the Berlin Wall

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# Urban renewal after the Berlin Wall<sup>\*</sup>

**Abstract:** Urban renewal areas are popular but empirically understudied spatial planning instruments designed to prevent urban decline and induce renewal. 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. Our results suggest that the policy has helped reducing (increasing) the number of buildings in bad (good) condition by 25% (10%). Property prices increased at an annual rate of 0.4-1.7% according to our preferred estimates. Evidence is weak at best, however, for positive housing externalities.

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

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PRELIMINARY DRAFT

## 1 Introduction

Among the arguably most striking phenomena in contemporary cities is the simultaneous existence of urban decline and gentrification. One way to rationalize these apparently contradictory dynamics is to assume the existence of housing externalities, i.e. mutual quality related spillovers among properties in a neighborhood. If housing externalities exist, the attractiveness of a location depends on the simultaneous decisions of landlords and homeowner regarding maintenance and upkeep of their properties (Rossi-Hansberg, Sarte, & Owens, 2010). Since good design and poor maintenance of surrounding buildings make an area relatively more or less attractive to different household types and can lead

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to sorting, housing externalities can affect the profitability of investments into design and upkeep of the own building. As an example we may observe a downward spiral in which poor maintenance and flight of affluent households mutually reinforce themselves and lead to urban decline. In an otherwise similar neighborhood the investments into the building stock and the attraction of affluent households could become mutually reinforcing and lead to gentrification. As with many spatial outcomes there is a possibility that multiple spatial equilibrium configurations exist and privileged and disadvantaged neighborhoods result from a Krugman (1991, 1994) type historic accident. Given the potential for multiple equilibria and the fact that no economic market exists where housing externalities could be traded, it is easy to rationalize government intervention.

In fact, there are numerous sizable programs targeting neighborhoods in need in the U.S. As an example, the Community Development Block Grant (CDBG) provides between \$3 and \$10 Bill. each year to cities and local administrations to improve the situation in low income urban areas (Brooks & Phillips, 2007). Another example is the Home Investment Partnership (HOME) program, which supports affordable housing with about \$2 Bill. per year. Similar programs also operate outside the U.S. In Germany the budget for various urban development programs (“Städtebauforderung”), which are typically jointly financed by the federal government and the federal states, amounts to about €350 (\$453.1) Mio. to €500 (\$647.3) Mio. per year (Bundesinstitut für Bau-, Stadt-, und Raumforschung (2009)).<sup>1</sup> One justification for such public expenditures rests on anticipated positive and self-reinforcing housing externalities, i.e. the hope that subsidies into renovation of a property will benefit others than just the respective owner or landlord. Complementary arguments have been the preservation of cultural heritage or an increase in the consumption value of cities that helps attracting high skilled workers (Glaeser, Kolko, & Saiz, 2001).

To date, surprisingly little evidence is available on the impact these policies have on housing market outcomes. One reason for this lack of evidence might be that separating the effects of policies designed to promote housing externalities from other forces that (re)shape the structure and attractiveness of cities and neighbourhoods is a challenging task. For one thing, housing externalities are not tradable goods. As such, it is not possible to value them based on observable market prices. One established way of dealing with this

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<sup>1</sup> Aggregated renewal finance data on European level is not available.

problem is to assume a spatial equilibrium in which housing prices must offset for all locational advantages and disadvantages, including the policy in question (Roback, 1982; Rosen, 1974).<sup>2</sup> Various policies have been studied based on the associated capitalization effects in a long tradition that dates back to Oates (1969) at least.<sup>3</sup> For another thing, the task is a derivative of the broader problem in social sciences of separating the effect of treatments from correlated effects. In a spatial context the challenge is to separate the policy effect (the treatment) from a variety of other factors that affect the attractiveness of a location, many of which are unobserved. Quasi-experimental methods have recently gained popularity as a means to control for unobserved factors that impact on house price trends (Ahlfeldt & Kavetsos, 2013; Dachis, Duranton, & Turner, 2012; Gibbons & Machin, 2005). With this approach a counterfactual for a group of treated properties (exposed to the policy) is established via the comparison of house price trends with a control group. Key to the credibility of such approaches is the appropriateness of the identifying assumptions that a) the control group itself is unaffected by the treatment and b) the control group in the absence of an intervention would have followed exactly the same trend as the treated.

Our analysis makes use of such a quasi-experimental research design to identify the effects of some substantial urban renewal policy efforts in Berlin, Germany that have started in the early 1990s. The policy was aimed at promoting the recovery of neighbourhoods that were found to have suffered particularly severely during the long lasting period of division. We follow the evolution of these neighbourhoods over more than two decades, paying particular attention to establishing a valid counterfactual via appropriate control groups and isolating policy related housing externalities from other determinants of house prices.

To our knowledge we are the second to Rossi-Hansberg et al. (2010) [hereafter RH] only to provide a rigorous evaluation of revitalization policies. RH investigate property prices in and around four renewal areas<sup>4</sup> and one control area, which was initially considered

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<sup>2</sup> See Lucas and Rossi-Hansberg (2002) and Ahlfeldt et al (2012) for recent urban equilibrium models.

<sup>3</sup> See Celini, Ferreira and Rothstein (2010), Dehring, Depken, & Ward (2008), Brunner, Sonstelie, & Thayer (2001), Brunner, and Sonstelie (2003) for a recent examples.

<sup>4</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.

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, about 50% every 1000 feet (RH 2012, p. 487). Equivalently importantly, 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 invested dollar. 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 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 natural experiment over a longer period. We are able to draw from a larger pool of treated areas and potential control areas and try to make the most of the available variation to obtain credible estimates of the average effect across the treated 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 bad physical condition of the housing stock, especially so in the eastern part (Berlin, 1992), p. 16). In response to this situation 22 out of 39 originally proposed renewal zones were designated between 1993 and 1995 as target areas for a renewal program.<sup>5</sup> Until late 2009 (the period of the last official report on the renewal program) as much as €1.8 Bill. (\$2.34 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 17 runner-up areas ("Untersuchungsgebiete") not selected for the program as a control group for comparison, but also make use other control groups which are

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<sup>5</sup> The *First Berlin Renewal Program* (Erstes Gesamtberliner Stadterneuerungsprogramm).

close to the treated either in spatial or socio-economic terms. With these counterfactuals we establish a composite renewal effect, which consists of an increase in structural value of renovated properties and an increase in locational value due to renovation of adjacent properties, i.e. a housing externality. One attractive feature of our data set is an indication of a property's physical condition at the time of transaction. We make use of this feature to get to the housing externality effect by focusing on properties in good condition exclusively. The rationale is twofold. First, by holding internal quality constant, our estimated treatment effects only capture appreciation related to renovation of surrounding properties, i.e. an (housing) external(ity) effect. Second, we argue that properties which are in good condition by the time of transaction are unlikely to be renovated right after the transaction so 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 onto areas just outside the treated areas. Previewing our findings, our results indicate that the policy has led to a significant upgrade of the housing stock and a general appreciation of the properties in targeted areas, but not to a pure increase in site value due to housing externalities one may have hoped for.

Besides adding important evidence to the sparsely developed literature on the economic effects of revitalization policies (e.g. Clay, 1979; Rossi-Hansberg et al., 2010) and housing externalities (e.g. Ahlfeldt & Kavetsos, 2013; Ahlfeldt & Maennig, 2010; Rossi-Hansberg et al., 2010), our analysis connects to a more general research strand in urban economics that looks into 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. Ahlfeldt, 2011; Brueckner, Thisse, & Zenou, 1999; Carlino & Coulson, 2004; Cheshire & Sheppard, 1995).<sup>6</sup> In this literature it has been argued that there has been a re-orientation towards attractive central cities, especially by high-skilled young professionals, the so called creative class (Florida, 2002). The consumption value of cities has therefore becomes increasingly important for the attraction of a highly skilled labor force and, hence, the economic success of cities (Carlino & Saiz, 2008;

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<sup>6</sup> This research complements research that has looked into the effects of spatial density on productivity of workers and firm (e.g. Ahlfeldt et al., 2012; Ahlfeldt & Wendland, 2013; Ciccone, 2002; Ciccone & Hall, 1996; Glaeser, Hedi, Jose, & Andrei, 1992; Glaeser & Mare, 2001; Rauch, 1993; Rosenthal & Strange, 2001)

Glaeser et al., 2001). Our findings inform this literature on whether revitalization policies and other neighborhood policies like historic preservation may contribute to the surge of targeted neighborhoods,<sup>7</sup> or whether other factors like transport affordability (LeRoy & Sonstelie, 1983), housing cycles (Brueckner & Rosenthal, 2009) or natural amenities (Lee & Lin, 2012) are more important determinants of gentrification. Last but not least our results 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.

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 the results. The final section summarizes and concludes our findings.

## 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 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 since private real estate property owning was not encouraged in the GRD, and rents were freezed on a low level since 1945. These developments resulted in an overall bad condition of the building substance of original housing stock and inner city district centers, including massive vacancies, and in an increased need for renovation after the unification in 1990. At the same time, the political mood after the unification of East and West Germany facilitated large scale public funding. Among other programs, these developments resulted in the *First Berlin Renewal Program*.

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<sup>7</sup> A growing body of literature has investigated capitalization effects of historic designation, both on designated buildings as well as on properties near to designated buildings (e.g. Asabere, Huffman, & Mehdian, 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).

The main instrument to overcome these problems was the initiation of a group of urban renewal areas, which are eligible for public funding and support according to specific rules. The location, shape, and structure of a renewal area are determined in 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, which are 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 about 8,100 square kilometers, 5,723 plots, and about 81,500 dwelling units, with an average population of 5,000 residents per renewal area (Senat Berlin, 2001).<sup>8</sup>

The Berlin program is characterized by two main phases: In the post-unification phase between 1992 and 2002, massive vacancies, and very bad building substance have been the driving factors of the renewal program. By 2000, already more than 50 % of the housing units in the renewal areas had been modernized (Berlin, 2005). Private investments in the building stock have been supported through tax reductions, loans, cash advances and further financial support. In the post-2002 phase, due to the progress made during the post-unification phase and an increasingly tight public budget, the focus changed: It 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>9</sup>

As indicated above, most of the designated renewal areas are located in the former eastern part of Berlin. Additionally, the 5 renewal areas in former West Berlin are much smaller than their eastern counterparts. Only about 6% of the housing units inside the renewal

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<sup>8</sup> In Richmond, the object of the RH (2012) analysis, the 4 targeted areas had an average population of 1900 residents and on average 1,000 housing units.

<sup>9</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 code of building law (BauGB), and § 7h, 10f, and 11a of the code of income tax law (EStG).



areas were located in the western part of Berlin reflecting the fact that West Berlin was in a significantly better economic situation after the fall of the Berlin Wall.

Until 2009, the expenses comprised more than 1.8 billion € (2.3 billion US\$) of public investments, amounting to about 880 million € (1.13 billion US\$) for modernization and reinstatement, and about 546 million € (730 million US\$) for expenses in infra-structure and social environment. The remaining disbursements consist of preparation costs (€75 Mio. / \$97 Mio.), allowances (€115 Mio. / \$150 Mio.), other regulatory measures including compensations (€181 Mio. / \$235 Mio.), and other building measures (€63 Mio. / \$81 Mio.).<sup>10</sup> On single renewal area level, the average expenses are about 80 million € (102 million US\$), translating into per capita expenses of €16,000 (\$20,600) distributed within a period of some 15 years. This compares to per area payments of \$3.5 Mio. 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 location of the renewal and investigation areas in Berlin.<sup>11</sup>

### 3 Empirical Strategy

While assessing whether significant price trends exist within renewal areas relative to other areas is empirically straightforward, separating the causal effects of the policy from correlated effects and distinguishing between “internal” and “external” (via the housing externality) capitalization effects is more challenging. First, the locations where these policies operate are not random and likely correlated with specific location characteristics. Second, the characteristics, when unobserved, may not only affect the level of property prices at a given locality, but also the trends they follow. Third, housing quality and especially exterior housing quality is difficult to observe. Our identification strategy engages with these challenges and makes use of housing quality indicators which are typically difficult to obtain. Before we present our actual empirical specification we abstract from

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<sup>10</sup> Compare (Berlin, 2010), where the local administration (Senatsverwaltung Berlin) 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>11</sup> Compare Table A1 in the technical appendix for details on designation date, district, and resignation of the renewal areas. An overview over the area is shown in Figure 1, a snapshot providing more detailed graphical information can be found in Figure A1 in the appendix.

some of the identification issues to introduce basic nature of the treatment effect we estimate.

### 3.1 Identification

Let's assume we observe a property, whose maintenance levels 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 as well as 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 on the neighborhood quality except through a housing externality.

$$P = f(I(S), E(I(S)), L, Y) \quad (1)$$

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:

$$E(D) = \int_0^1 I(D)d(D) = I \quad (2)$$

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

$$dP = \left( \frac{\partial P}{\partial I(S)} + \frac{\partial P}{\partial E(S)} \right) dI(S) + \frac{\partial P}{\partial L} dL + \frac{\partial P}{\partial Y} dY \quad (3)$$

Or

$$dP = \left( \frac{\partial P}{\partial I} + \frac{\partial P}{\partial E} \right) \frac{\partial I}{\partial S} dS + \frac{\partial P}{\partial L} dL + \frac{\partial P}{\partial Y} dY \quad (4)$$

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>12</sup>

Our treatment effect can be described as follows:

$$\beta = (P(S = 1)^{POST} - P((S = 0)^{PRE})^T - (P(S = 0)^{POST} - P(S = 0)^{PRE})^C \quad (5)$$

Or

$$\beta = \Delta P^T - \Delta P^C \quad (6)$$

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

$$\beta = \left( \left( \frac{\partial P}{\partial I} + \frac{\partial P}{\partial E} \right) \frac{\partial I}{\partial S} \Delta S + \frac{\partial P}{\partial Y} \Delta Y \right)^T - \left( \left( \frac{\partial P}{\partial I} + \frac{\partial P}{\partial E} \right) \frac{\partial I}{\partial S} \Delta S + \frac{\partial P}{\partial Y} \Delta Y \right)^C \quad (7)$$

or

$$\beta = \left( \frac{\partial P}{\partial I} + \frac{\partial P}{\partial E} \right) \frac{\partial I}{\partial S}, \quad \text{where } \Delta S = \begin{cases} 1 & \text{if treated} \\ 0 & \text{if control} \end{cases} \quad (8)$$

There are important implications for our empirical strategy which aims at estimating  $\beta$ . First, as in any quasi-experimental policy evaluation, the treatment effect only reveals the unbiased policy effect operating via  $I$  under the assumption that the control group in the absence of the policy would follow the same trend ( $\Delta Y^T = \Delta Y^C$ ). An appropriate definition of a control group is therefore crucial for the identification. Second, 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. Third, to the extent that 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 ( $\beta =$

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<sup>12</sup> In the empirical implementation we introduce a buffer around the treated areas to ensure that the control group is not effected by the treatment through spillover effects.

cy ( $\beta = (\partial P/\partial E) (\partial I/\partial S)$ ). 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.

### 3.2 Renewal Effects

#### *Baseline specification*

We use a combination of hedonic (Rosen, 1974) and differences-in-differences methods to estimate the treatment effect discussed above. 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:

$$\begin{aligned} \log P_{it} = & \alpha_1 T_i + f(T_i \times V_{it}) + \delta(T_i \times A_{it}) \\ & + \sum_k \gamma_k X_{kit} + \sum_l \gamma_l L_{li} + \sum_t \sum_g (\gamma_{gt} G_i \times \varphi_t) + \sum_t \varphi_t + \sum_n \mu_n + \varepsilon_{it}, \end{aligned} \quad (9)$$

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 functional forms we use in depth later in the text after providing a description of and a rationale for the control variables used.

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 not observed time-invariant location characteristics via a fixed effects  $\mu_n$  defined for 323 traffic cells.<sup>13</sup> Standard errors ( $\varepsilon_{it}$ ) are clustered on the same level. Macroeconomic factors

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<sup>13</sup> Traffic Cells (Verkehrszellen) are statistical areas originally used by the local administration to analyze traffic. There exist 323 traffic cells in Berlin, the average size is 2.7 square kilometers (1,05 square miles).

that are assumed to be invariant across the treatment and control groups are captured by year fixed effects  $\varphi_t$ .

Besides 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 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 try to address at least 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, night-clubs and hotels. We also add a full set of 23 city districts x year fixed effects to capture variation across district-year cells. We note that all of the variables we interact with the year dummies are time-invariant to avoid problems of circular causation.

With the strong controls for time-invariant location features, the flexibility in time-varying implicit location attribute prices and a careful match between treated and non-treated properties discussed in section 3.3 we hope to establish a sufficiently well-defined counterfactual to benchmark the effect of the treatment over almost two decades. The two specifications of the treatment function we use produce treatment estimates that vary in the years  $V$  since designation of a renewal area has taken place. For a given year since designation the treatment estimate then reflects the cumulated effect of the improvement in the maintenance condition of a sold properties  $i$  on the price of  $i$  as well as 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 building  $j$  and  $i$  is discounted by distance  $D_{ij}$  and may include the social externality of new residents moving into upgraded buildings.

$$\beta_V = \frac{\partial P}{\partial I} \frac{\partial I_{iV}}{\partial S_{iV}} + \frac{\partial P}{\partial E} \sum_j \frac{\partial I_{jV}}{\partial S_{jV}} \tau(D_{ij}), \quad \text{where } \tau(D) > 0 \text{ and } \tau'(D) < 0 \quad (10)$$

### *Treatment functions*

To get at 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.

$$f(T_i \times V_{it}) = \beta_0 T_{it} \times POST_{it} + \beta_1 T_{it} \times V_{it}, \quad (11)$$

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 is defined as  $\beta_V = \beta_0 + \beta_1 V_{it}$ . The second approach follows Ahlfeldt & Kavetsos (2013) 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.  $VD_{it}=1$  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 group.

$$f(T_i \times V_{it}) = \sum_V \beta_v (T_{it} \times VD_{vit}) \quad (12)$$

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. Besides using these two distinct treatment functions we vary the model along three more dimensions.

### *Building quality*

In a first alternation, we only consider buildings in good condition to hold the quality of the traded buildings constant ( $dI_i = \partial I_i / \partial S_i = 0$ ) so that 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 bad condition) since it is less likely that these build-

ings 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 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. For one thing, our data set offers two binary variables denoting whether a property, at the time of the transaction, was in a particularly good or bad condition. While this is significantly more information than available in most comparable data sets, this is also evidently far from perfect. For another thing, we have assumed that there are no policy effects onto neighborhood quality other than through housing externalities. If there are significant direct investments into the quality of local public goods, e.g. renovation of schools, 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

$$\beta_V = \left( \frac{\partial P}{\partial I} \frac{\partial I_{iV}}{\partial S_{iV}} + \right) \frac{\partial P}{\partial E} \sum_j \frac{\partial I_{jV}}{\partial S_{jV}} \tau(D_{ij}) + \sum_q \frac{\partial P}{\partial Q_q} \frac{\partial Q_{jq}}{\partial S_{jq}} \quad (13)$$

Since such improvements in  $Q_q(S)$  are hard to observe, it is difficult to separate them from the housing externalities. We employ an alternative approach to measuring housing externalities focusing on spillovers onto 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. Irrespective of this problem, a significant reduction in the treatment effect when holding building quality constant indicates the presence of a significant internal capitalization effect.

### *Spillover effects*

One of the advantages of the 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. At the same time any price effect will not be confounded with the policy effect on internal quality of buildings since 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  $D$ .

$$\beta_V = \frac{\partial P}{\partial E} \sum_j \frac{\partial I_{jV}}{\partial S_{jV}} \tau(D_{ij}), \quad \text{where } \tau(D) > 0 \text{ and } \tau'(D) < 0 \quad (14)$$

This approach also helps with another concern which is that authorities reserve the right to levy the value generated by the policy (“Ausgleichsabgabe”). Until 2009, local authorities generated €28.38 Mio. (\$ 36.6 Mio.) and stipulated €46 Mio. (\$ 60 Mio.) (Senatsverwaltung Berlin (2009, 2010), which are strikingly low figures compared to the above mentioned investment volumes.<sup>14</sup> While these payments are in practice rare and 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 (1). In the first alteration we redefine our treatment measure as a binary variable that takes the value of  $T_{S1i}=1$  if a property falls within a 500m buffer area and zero otherwise. We run this specification using the two treatment functions introduced above and varying control groups. Focusing on the parametric specification and our preferred control group we then use an alternative treatment measure  $T_{S2i} = \beta_S T_{S1i} + \beta_{S2} DIST_{Si}$ , where  $DIST_{Si}$  is the distance to the nearest renewal area.

#### *Area specific effects*

The models discussed above produce an average effect of the treatment based on dozens of treatment and control areas. This setting has the advantage of being relatively insensitive to idiosyncratic factors that affect trends in individual renewal or control areas, but cancel out across areas. We complement the estimation of the average treatment effect by estimating individual effects by renewal area in separate regressions. To estimate degrees

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<sup>14</sup> All income generated through this source is to be reinvested into the districts infrastructure or into neighbourhood improvements.



of freedom in the individual models we use a simplified long-difference approach, in which we compare changes in mean prices before and 15 years after designation (the average designation time) in the treatment and control group of properties near to the treated area. We briefly discuss the results in the main paper and leave a detailed presentation of the model and the results to the technical appendix.

### **3.3 Control Groups**

We define a number of control groups (CG) for the benchmark (renewal area) models (CG 1a-4a) and the spillover models (CG 1b-4b).

For all control groups, we exclude a 500m buffer area around the renewal areas, to rule out a treatment effect on the control groups due to spillovers. CG 1a includes all observations outside the urban renewal areas and the 500m buffer. In CG 2a, we impose a geographical limit by considering transactions that lie within a 500 to 2,000 meter (approx. 6,000 ft.) distance to the renewal areas. CG 3a consists of the fractions of investigation areas outside the 500m buffer that were not transformed into renewal areas – similar to RH. As a further alternative, CG 4a is created based on the propensity score matching procedure proposed by Rosenbaum & Rubin (1983). For the spillover models that use the 0-500m buffer area around the renewal areas as a treatment group, CG1(b) and CG2(b) are defined exactly as before. CG 3b encompasses transactions in investigation areas outside the 500m buffer, plus a 500 meter buffer around the investigation areas. For CG 4b we again use the synthetic matching procedure to find suitable matches to the transactions in the buffer around the renewal areas.

For the synthetic matched control groups (4a and 4b) we match transactions inside and outside renewal areas (or buffer 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 the participation in the treatment and the outcome variable. To avoid anticipation effects 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 more detail in the technical appendix, where we also present some descriptive statistics for the resulting samples.

### 3.4 Data and descriptive statistics

Our study area comprises the area of the Federal State of Berlin, Germany. The city in 2012 counted some 3.3 Mio inhabitants and about 1.9 Mio dwelling units. About 14% of the population are non-German citizens. While recently there have been signs of economic recovery after a relatively long period of economic struggle since unification, the unemployment rate has remained relatively high at about 13%. The overall area is about 892 square kilometers (344 square miles). The center is densely populated, the overall building structure is a mix of historic building (aged about 100-130 years), and buildings put up after World War II to substitute for the destroyed building stock (age about 60 years) and newer buildings.

Within this study area we observe all transactions of developed land that took place between January 1990 and August 2012, which amounts to about 70,000 transactions. The data set includes price, transaction date, location, and a set of parameters describing building / plot characteristics. The data is obtained from the Committee of Valuation Experts Berlin 2012 (Gutachterausschuss Berlin). The transactions are geo-referenced (addresses and x/y coordinates), which allows integrating them 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 meter and a quadratic kernel function (Silverman, 1986). The data is 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 A5 in the web based appendix.

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

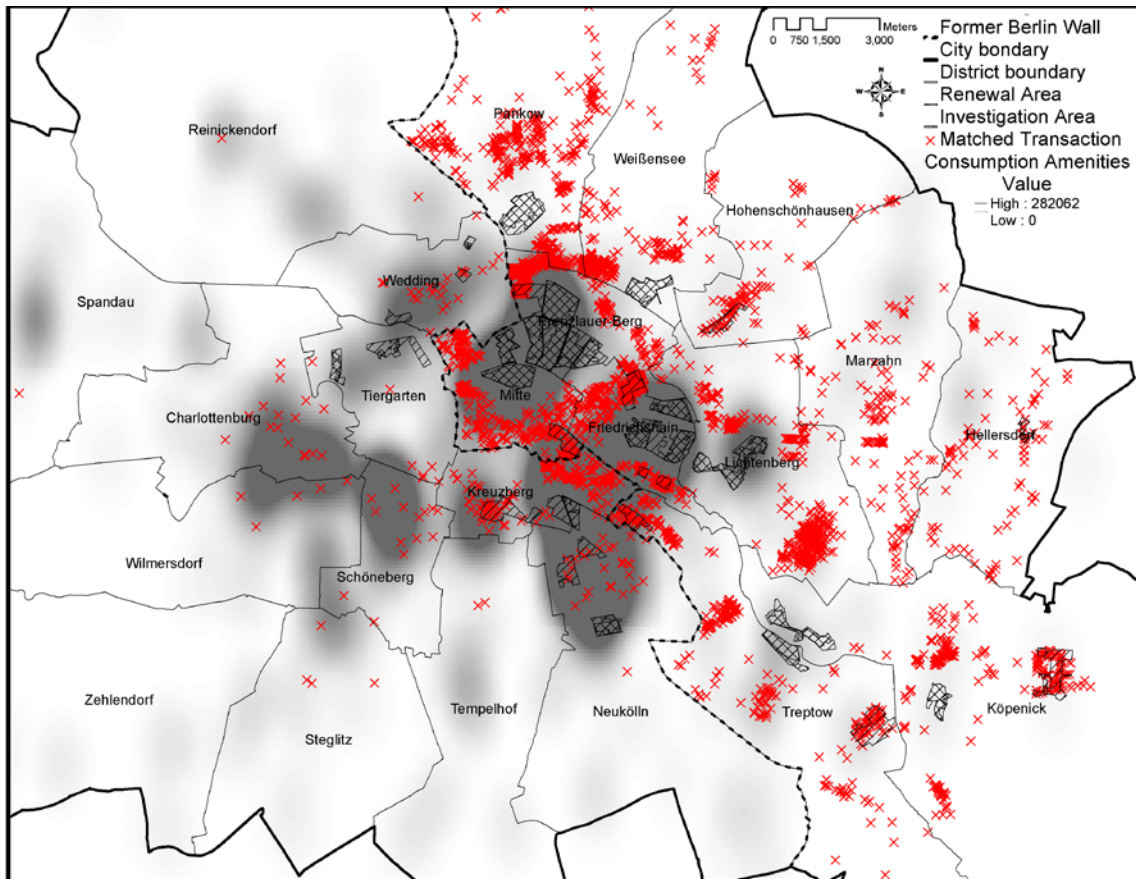
From the Berlin Senate Department we obtained maps showing the exact locations and boundaries of the 22 renewal areas designated in three waves in 1993, 1994, and 1995 as well as the 17 investigation areas discussed above. We have digitally processed the maps and converted them to a shape file to merge the information with the other spatial data in GIS. The 22 renewal areas have a mean size of about 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 due to the fact that renewal and investigation areas were 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 one of our synthetic control groups (CG 4a). From the figure, some notable stylized facts become evident. First, the majority of the renewal (17 out of 22) and investigation areas (31 of 39) are located in former East Berlin, which is not surprising given that the eastern part of the city had suffered even more severely during the period of division. Second, renewal areas and revitalization areas are typically located in central areas and in amenity clusters and, hence, areas that are typical candidates for gentrification. It is important to fully acknowledge this spatial pattern since failure to select sufficiently similar control groups or failure to account for a change in appreciation of these characteristics may result in a policy treatment effect that could be confounded with a general gentrification phenomenon. Third, our synthetic control groups (red dots) consist of transactions that are either close to renewal or investigation areas or in areas of high amenity densities, which lends some trust to the selection process.

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

**Fig. 1: Renewal geography**

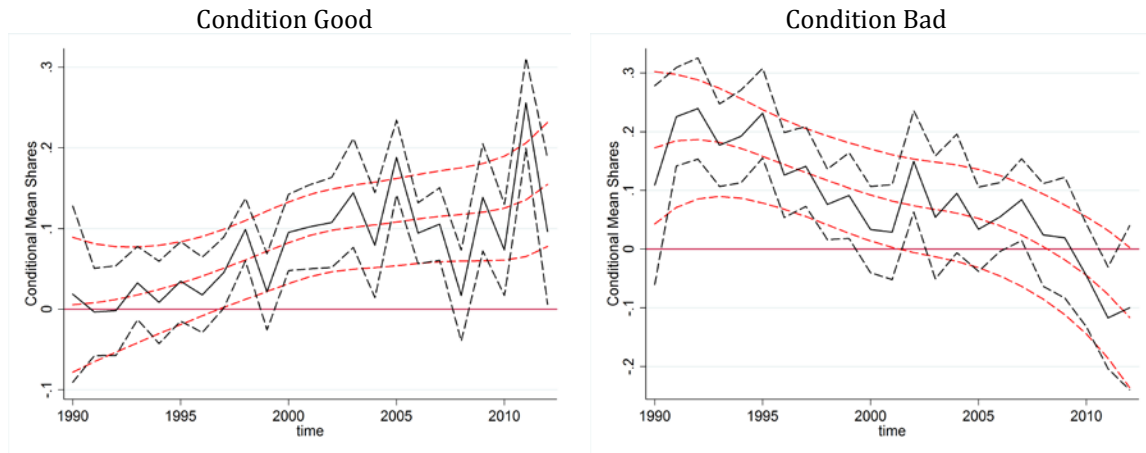


Notes: Own illustration based on the urban and environmental information system (Senatsverwaltung für Stadtentwicklung Berlin, 2006). Crosshatched (hatched) areas indicate renewal (investigation) areas. Red crosses are the matched transactions in CG 4a. Smoothly grey shaded areas represent the consumption amenity density.

As discussed above, the information on maintenance condition is a special feature of our property data set. 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 bad condition evolved in the renewal areas relative to the revitalization 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 quality of the housing stock in the renewal areas improved significantly over time. In 1990, the fraction of buildings in bad condition in renewal areas was significantly larger than in the investigation areas, possibly a reason for their selection. Over time the difference steadily declines. 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 achieved to accelerate the upgrade of the housing stock that was left behind during the division period.

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



Notes: Year specific differences in mean shares are estimated in two separate regressions of the following type:  $Y_{it} = \sum_t \Delta_t T_i \times \varphi_t + \sum_o X_{ot} + \varphi_t + \epsilon_{it}$ , where  $Y_{it}$  indicates whether a property at time  $t$  was in good (left) or bad (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 whether a property falls within a renewal area ( $T=1$ ) or within a revitalization area ( $T=0$ ). Black solid (dashed) lines indicate  $\Delta_t$  point estimates (95% confidence intervals). Red dashed lines are lowess smoothes of the parameters. Similar trends with alternative sets of covariates are presented in Figure A2 in the appendix.

Controlling for additional property features does generally not significantly alter the results, nor does using the other control groups. Alternative estimations using additional covariates and different control groups are presented and discussed in the appendix.

## 4 Empirical Results

### *Renewal area effects*

Table 1 summarizes our parametric estimates (see specification 11) of renewal area capitalization effects by varying control groups and samples. To keep the table compact 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 A6 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 aggregated re-

newal policy effect for any given year passed since designation can be computed as  $(\exp \beta_0 - 1) + \beta_1 V_{it}$ .<sup>17</sup>

Models (1-6) (upper panel of Table 1) estimate the gross-capitalization effect of the policy comprising both internal property as well as externality effects. Model 1 compares the evolution of property prices within the renewal areas to the rest of Berlin, our most general control group (1a). The results suggest that a positive long-run trend (about 4.7% per year) dominate a negative intercept (-16.2%). After  $V=20$  years sales prices in designated renewal areas, on average, have appreciated by as much as 79.82% relative to the rest of the city. This corresponds to an average yearly appreciation rate of of about  $(1 + 79.82\%)^{1/20} - 1 = 2.96\%$ . As we increase the strength of the counterfactual using spatially close properties (2), the investigation areas (3) or the matched properties (4) as a control group, the cumulated effect (average appreciation rate) drops to 67.15% (2.6%), 40.58% (1.7%), and 36.48% (1.59%) 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 cumulated effect to – non-significant - 7.76% (8.01%) in Model 5 (6). This implies a – non-significant - average annual appreciation of about 0.4%. One interpretation of this remarkable drop 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. If, however, the changes in the implicit prices (e.g. of distance to the CBD) were driven by the policy, e.g. central locations became generally more attractive because of the renewal efforts, the time-varying effects would be absorbing some variation that was genuinely attributable to the policy.<sup>18</sup> To this extent columns (5) and (6) results represent lower bound estimates of the policy impact as much as columns (3) and (5) results give upper bounds.

The estimated effects tend to come down relative to the comparable full models when the sample is restricted to properties in good physical condition (lower panel of Table 1). Moreover, the results are relatively unstable across varying control groups and none of the

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

<sup>18</sup> This problem is a variant of the „bad control problem“ (Angrist & Pischke, 2009).

effects is 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 not to estimate the demanding model with time varying effects on this sample), the results are at least indicative that the benchmark results are not driven by externality effects primarily.

**Tab. 1. Renewal Area Treatment Effects**

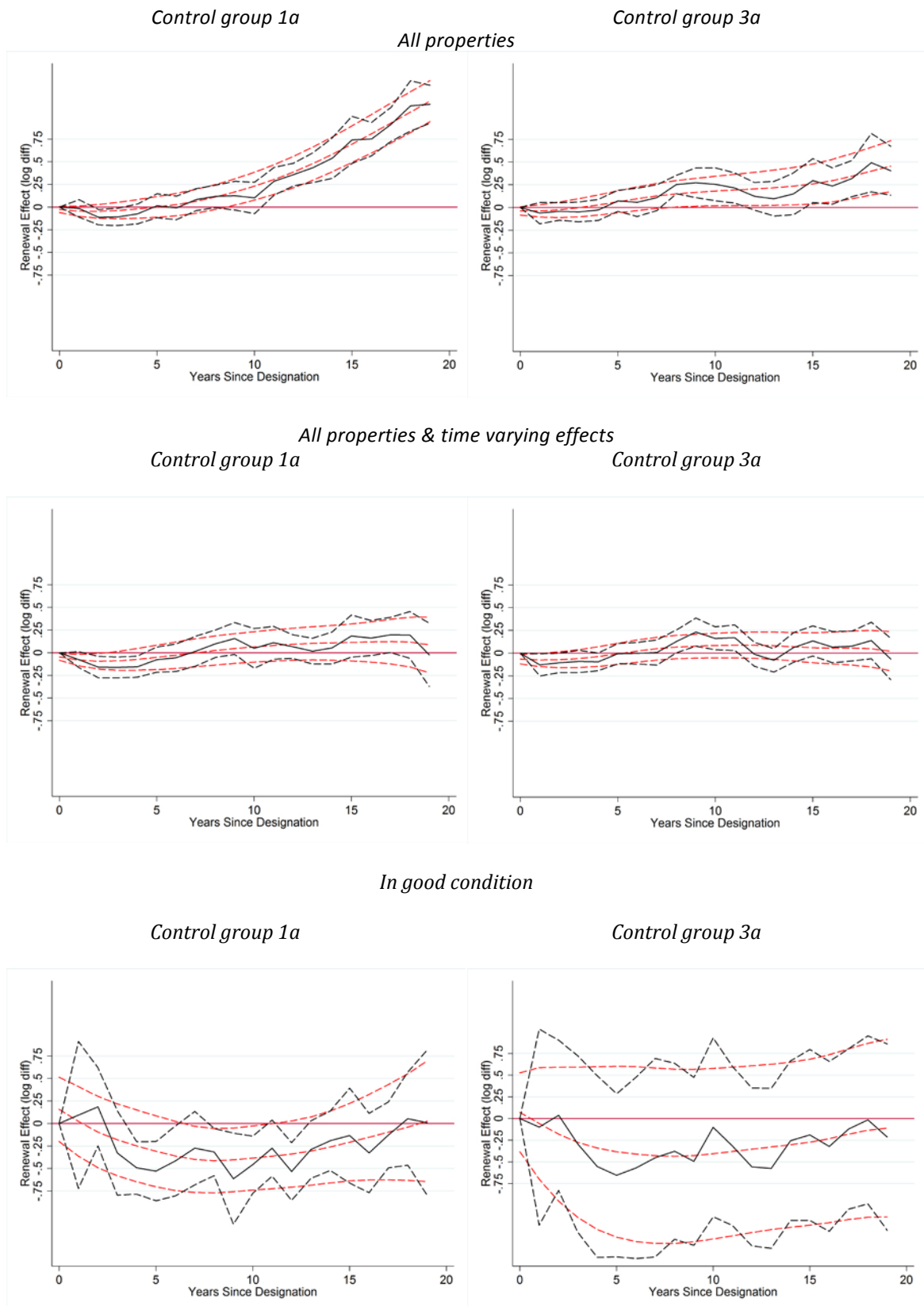
Control group	1a	2a	3a	4a	3a	4a
	All	All < 2km	Investigation Areas	Matched Observations	Investigation Areas	Matched Observations
<i>All properties</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
<i>T x POST</i> (within renewal)	-0.162*** (0.036)	-0.115*** (0.037)	-0.060 (0.039)	-0.139*** (0.052)	-0.120*** (0.045)	-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	79.82%*** (6.41%)	67.15%*** (6.62%)	40.58%*** (7.07%)	36.48%*** (9.43%)	7.76% (7.07%)	8.01% (10.55%)
Av. appr. rate	2.96%	2.6%	1.7%	1.59%	0.37%	0.39%
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
Traffic Cell E.	YES	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES
Time-Varying E.	NO	NO	NO	NO	YES	YES
<i>Buildings in good condition</i>						
	(7)	(8)	(9)	(10)		
<i>T x POST</i> (within renewal)	0.008 (0.338)	-0.070 (0.416)	0.084 (1.063)	0.500 (1.232)	-	-
<i>T x V</i> (years since designation)	-0.000 (0.012)	-0.002 (0.014)	-0.026 (0.019)	-0.001 (0.019)	-	-
Cum. effect after 20 years	0.67% (32.28%)	-10.12% (40.07%)	-44.17% (105.62%)	62.21% (123.99%)	-	-
Av. appr. rate	0.003%	-0.53%	-2.87%	-2.45%	-	-
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		
Traffic Cell E.	YES	YES	YES	YES		
Year Effects	YES	YES	YES	YES		
Time-Varying E.	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 more 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.

Figure 3 illustrates our semi-non-parametric estimates of the temporal treatment function according to equation 12. We present estimates for all properties (upper row), all properties including time varying effects (middle row), and properties in good condition only (bottom row) using control group 1a (all properties outside renewal areas) and 3a (properties in investigation areas). The semi-non-parametric estimates are generally in line with the parametric counterparts presented in Table 1. The cumulated effect on all properties inside renewal areas relative to those outside the renewal areas is even slightly larger than implied by the parametric estimates (upper left), but comes down to about 50% when the trend is benchmarked against the investigation areas (upper right). The positive trends effects seem to capitalize with some delay (starting after about 5 years). The negative level shifts found in Table 1, thus, appear to be mainly driven parametric constraints and should not necessarily be taken as indicative of a significant drop in prices immediately following designation. We note that the cumulated effect after 20 years in the models with time-varying effects is within the same range of model (5) in Table 1 but not statistically significant. Focusing on properties in good condition we, again, find that the cumulated effect after 20 years is not statistically distinguishable from zero, partly due to large standard errors. We are, thus, not able to affirm the existence of significant revitalization effects in based on the most demanding semi-parametric models.



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



Notes: Black solid (dashed) lines indicate treatment point estimates (95% confidence intervals). Red dashed lines are lowess smoothes of the parameters.

*Spillover effects*

Table 2 and Figure 4 replicate the analysis for the spillover areas, i.e. the 500m 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 irrespectively of their maintenance condition. The 500m buffer area previously excluded due to the presence of spillovers now serves as a treatment group to detect spillover effects. Lower thresholds generally yield similar results, but suffer from loss of degrees of freedom. The results are easily summarized. For our preferred control groups (3 and 4) we find results that are within the same range as the internal 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 that using the time-varying effect. The results are not particularly sensitive to the inclusion of controls for maintenance quality, even though the quality of the building stock is a sizable determinant of property value (see table A7 in the appendix).

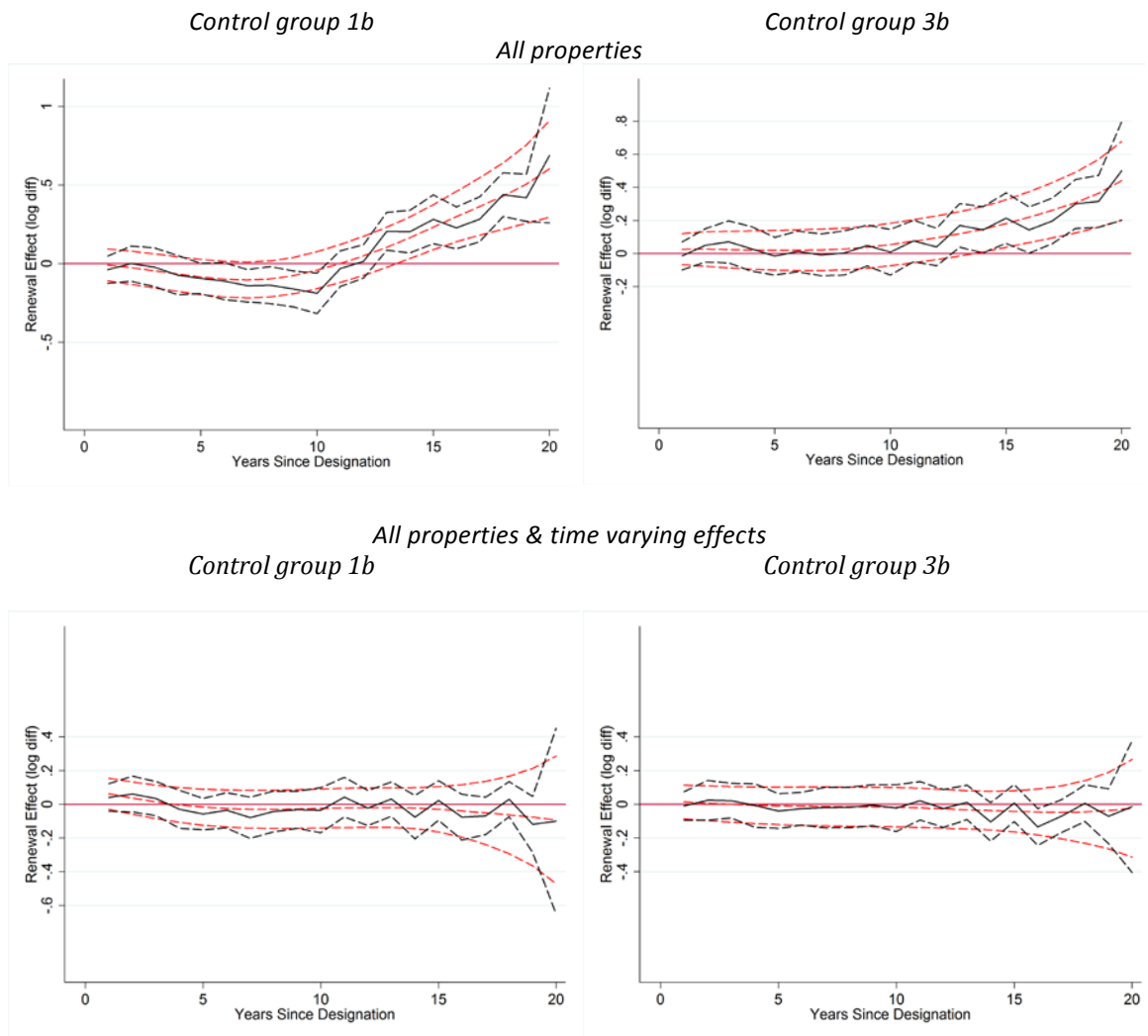
One explanation for the lack of evidence on positive spillovers in the models discussed might be a very steep spatial decay. Consequently, we have altered our strategy using distance to the renewal area boundary as a treatment measure. Even with a highly localized externality we would expect the distance gradient within a 1km buffer area to change after designation in the presence of significant spillovers. Since we do not find evidence for such distance gradient effect, neither immediately following designation, nor gradually emerging over time, we contain the resulting estimates to the appendix.

**Tab. 2. Renewal Area Spillover Effects**

Control group	1b	2b	3b	4b	3b	3b
	All	< 2km re- newal area buffer	Invest. Areas + 1 km buffer	Matched Observations	Invest. Areas + 1 km buffer	Matched Observations
<i>500 meter buffer</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
<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)
<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)
Cum. effect after 20 years	28.19%*** (5.88%)	33.39%*** (6.34%)	36.75%*** (6.46%)	38.59%*** (6.82%)	-4.38% (5.48%)	-4.62% (8.08%)
Av. appr. rate	1.25%	1.45%	1.58%	1.65%	-0.22%	-0.24%
Observations	66,865	19,421	11,963	16,989	11,963	16,989
R <sup>2</sup>	0.690	0.657	0.605	0.636	0.671	0.662
AIC	113,544.3	36,076.0	21,172.5	32,244.7	19,093.6	30,244.1
Hedonic Controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Traffic Cell E.	YES	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES
Time-Varying E.	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 more 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.

**Fig. 4: Price trends in spillover areas, compared to different control groups**



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

*Area specific effects*

We have replicated the main parametric models described above by renewal area to obtain area specific estimates. The distribution of individual effect is centered on zero, but slightly skewed towards positive effects. We are careful in the interpretation of individual estimates as the local focus makes it more difficult to separate the renewal effect from unrelated gentrification trends. A careful account of the estimates likely is that treatment heterogeneity seems to exist, which suggests that some areas are more responsive to external stimuli than others. A more detailed discussion is in the appendix.

## 5 Conclusion

Urban renewal programs have become a common instrument to mitigate and turn over the negative effects of urban decline, and to promote positive housing externalities. Public expenditures on such programs are justified on the grounds of a positive non-marketed externality building maintenance is anticipated to have on economic (and social) outcomes and the hope that the (temporary) stimulation of private investment can trigger an upward spiral of a neighborhood.

We contribute to a fresh strand of literature that looks into economic outcomes of urban renewal policies that has just been opened by RH. We add to their case by analyzing a \$2.3 Bn. neighborhood renewal program designed to promote the recovery of 22 neighborhoods in Berlin, Germany that had suffered particularly strongly during the period of division in the 20<sup>th</sup> century. We track the evolution of property prices in these neighborhoods over about 20 years and compare the trends to neighborhoods that were not selected for the program, but are otherwise similar.

Given the expectations that have motivated the renewal program in question and similar programs our results are both encouraging and disillusioning at the same time. On the one hand our results indicate that the policy has led to increased renovation work, improved maintenance, and an appreciation of the renovated buildings in the targeted neighborhoods. Over about 20 years, the share of buildings in bad (good) condition declined (increased) by about 25% (10%) relative to similar not targeted areas. Compared to similar areas considered, but not selected for the program, property prices, on average, after 20 year of operation of the program have increased by about 40%, which equates to a yearly appreciation rate of 1.7%. The appreciation is even larger compared to the city average.

On the other hand, our results also suggest that this appreciation is at least partially attributable to the favorable location of these areas. Controlling for trends related to proximity to the CBD, the distribution of consumption amenities and individual city districts, our most careful results point to a cumulated effect of less than 10%, which corresponds to an annual appreciation of less than 0.5%. Equivalently important, our results, on average, do not point to the self-reinforcing effect operating through housing externalities one may have hoped for. The increase in property value seems largely attributable to the upgrade of internal quality and there are no significant spillovers to adjacent areas. Back of

the envelope calculations suggest that total property value increased by only €0.35-€1.8 for €1 spent on the program taking the abovementioned lower and upper bound estimates as a benchmark (see the appendix for details). We conclude that the policy has speeded up the renovation of significant fractions of the urban fabric and, as such, helped eliminating the visible traces of the division period. But it has also been mainly a cash transfer to landlords participating in the program.

Despite the improvements in building stock likely caused by the policy our results look less favorable than those previously presented by RH for the Neighborhoods in Bloom program in Richmond, Virginia. Analysing a much smaller program of \$14Mio RH find positive and large effects on property value in four renewal areas that exceed the investments by a factor of two to six as well as significant spillovers onto adjacent areas. There are some explanations that may account for the large discrepancy in the findings for Richmond and Berlin. Firstly, the different structure in two local communities. The Richmond program was more based on community volunteering and on local non-profit organizations while Berlin took a top-down approach implemented by official state authorities with little community participation. Secondly, and perhaps more importantly, 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, it has been demonstrated that owners 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, 2012), so they may also be more receptive for subsidies for renovations. Third, there is some indication that the impact of the policy has varied across targeted neighborhoods in Berlin. While the individual effects by renewal area need to be interpreted with care, the heterogeneity may indicate that some areas are more responsive to external stimuli than others.

Future research into the long-run effects of renewal policies across different institutional settings is needed to fully reconcile the evidence. Understanding of the factors that determine how incentivizing private investment into 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.

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