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Felix J. Richter

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Thesis Committee

Chairman:	Professor Armin Rott
First Examiner:	Professor Wolfgang Maennig
Second Examiner:	Professor Thomas Straubhaar

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to my daughter

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"Everything is related to everything else,
but near things are more related than distant things."

Waldo R. Tobler (1970, *Economic Geography*, 46(2): p.234-240)

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Chapter 1

Introduction

Economic relationships depend on the geographical distribution of the subjects. As Waldo R. Tobler famously put it in his *first law of geography*: "everything is related to everything else, but near things are more related than distant things." (Tobler, 1970, p. 236) The rationale is that economic interactions are to some extent influenced by the geographical or economic distance between observations. This influence ususally increases with decreasing distance of the observations to one another. This interrelation between subjects is often referred to as spatial correlation in the spatial economics literature.

The field of spatial economics aims at estimating economic relationships while accounting for spatial correlation. While it emerged from the geography and statistics literature it has evolved into a developing strand of the economics literature, with numerous approaches and applications. The versatility of the developed methods make them valuable for many fields, including the urban, regional, and real estate economics literature.

This thesis sheds light on several distinct features of quantitative spatial economics. It presents different empirical applications which take the geographic relationship between the observations into account. While the applications analyze a wider range of topics, each application combines several approaches to account for the spatial dependency of the data and demonstrates the differences between the spatially adjusted estimates and the conventional estimates.

The central aspect which motivates the spatial economics literature is the study of the influence of space on economic relationships. The basic idea is that certain attributes of the observation units - which can be countries, regions, districts, or street addresses, among others - are correlated with observations nearby. Neglecting this spatial correlation can have important implications: theoretical models might omit important aspects of the real world processes and empirical applications might suffer from biased results.

Although applications of quantitative spatial approaches are nowadays found in various fields, the development of the literature body can be condensed to but a few seminal advances which could be classified into three groups, namely early contributions, the new economic geography, and spatial econometrics.

One of the first scholars who incorporated spatial context into economic modeling was the German economist Johann Heinrich von Thünen. In the early nineteenth century, he developed a model of regional land use depending on productivity, transport costs, and the distance to the consumption center (Von Thünen and Hall, 1966). He is often regarded as one of the originators of the location theory in economics (Frambach, 2012). Another early contribution is the gravity model of international trade which was developed based on influential work by Tinbergen (1962) and Pöyhönen (1963). It is one of the first empirical applications which directly incorporates the geographic distance as economic distance, interpreting it as trade costs. Despite its astonishing empirical fit, a profound theoretical justification is still discussed and has been refined until recently (see e.g. Anderson and van Wincoop (2003)). The literature which emerged based on these contributions is known as the economic geography literature.

In the 1990s, notably Paul Krugman[1] coined the term *new economic geography* with a series of publications with a focus differing from the established models. He relaxed the concepts of perfect competition and Ricardian comparative adavantage. In his view, further factors such as spatial agglomeration processes, path dependent locational advantages, external effects, and increasing returns to scale are as fundamental as comparative advantages in determining international and regional trade patterns (Krugman, 1991a,b,c, 1993, 1994; Krugman and Venables, 1996). These advances, although not undisputed, are often regarded as the first contributions which substantiated the incorporation of spatial aspects into the general interest economic literature (Ron and Sunley, 1996).

In the meantime, also the methodological advancements in the last decades have been substantial. Since the seminal contribution of Paelinck and Klaassen (1979), a whole spatial econometrics literature has emerged. Among many others, notably Luc Anselin has contributed to the literature significantly (Anselin, 1988, 1995, 2003; Anselin et al., 1996, 1997). The classical spatial lag model and the spatial error model

[1] Many of the models were developed together with Tony Venables.

are nowadays accompanied by various approaches which are able to cope with spatial correlation at different stages of the data generating process. Recently, alternative ways to correct standard errors for spatial autocorrelation have attracted increased attention.[2] For an excellent overview overview of the formation of the field of spatial econometrics see for example Anselin (2010).

These developments also facilitated a further dissemination of spatial econometric concepts into many fields of the applied economics literature. Seminal contributions in the context of this thesis include among others studies on the regional and economic effects of large events and natural distasters (e.g. Carroll et al., 2009; Deschênes and Greenstone, 2007; Luechinger and Raschky, 2009; Rose and Spiegel, 2011), the econometric policy evaluation in general (e.g. Busso et al., 2013; Ham et al., 2011; Kline and Moretti, 2013), and more specifically the analysis of place-based policies in the housing sector (e.g. Autor et al., 2014; Baum-Snow and Marion, 2009; Eriksen and Rosenthal, 2010).

This thesis comprises four self-contained articles which can be read separately and revolve around some central aspects of the empirical spatial economics literature. After this introduction, the article in Chapter 2, titled "Exports and Olympic Games: Is There a Signal Effect?", deals with the potential effects of hosting the Olympic Games on countries' exports. In contrast to earlier contributions, the article shows that hosting or applying for the Olympic Games does not necessarily has a positive and lasting effect on countries' exports. Specifically, this *Olympic effect* vanishes, once the Olympic hosts are compared to appropriate control groups such as the OECD countries, and not to all remaining countries of the world.

The article in Chapter 3, titled "Nuclear Accidents and Policy: Notes on Public Perception", analyzes the effects of the nuclear accident in Fukushima in 2011 and the subsequent nuclear phase-out decision on the subjective perception in Germany. Subjective perception is captured through three independent items from the German Socio-Economic Panel (SOEP), including concerns about the environment, concerns about the reliability of energy supply, and concerns about the security of nuclear power plants. While the accident increases the probability to be worried about the environment, the phase-out decision decreases the worries about the security of nuclear energy. These effects are interrelated with the distance between the respondents' place of residence and the nearest nuclear facility.

In Chapter 4 the article titled "Urban Renewal after the Berlin Wall: a place-

[2] See e.g. Conley (1999).

based Policy Evaluation" evaluates a $2.3 Bn. urban renewal program designed to promote the recovery of 22 neighborhoods in Berlin, Germany. Such programs have become established instruments to mitigate the negative effects of urban decline. The study employs a quasi-experimental research design by comparing housing prices in the target areas over 20 years to various control groups, including areas with similar preconditions which were ultimately not selected for the policy and structurally similar transactions based on propensity score matching.

The results show, that the policy was effective in increasing the housing stock quality in the target areas. Compared to similar areas not targeted by the policy, the share of building in bad condition decreased by 25% over the program period, and the value increased by over 50%. However, there is no evidence that this is a causal effect. Also, there is no evidence for any external effects, which is astonishing given that such housing externalities are often used to justify the expenses for similar policies. Finally, there is evidence that the evaluation of place-based policies is sensitive to unobserved local differences, especially when there are but a few treatment or control areas.

The article in Chapter 5, titled "Winner Picking in Urban Revitalization Policies - Empirical Evidence from Berlin", evaluates whether local authorities strategically pick winners when selecting the targets for urban revitalization policies. The chapter analyzes the selection process leading to the designation of five large urban revitalization areas in Berlin, Germany. The article estimates the influence of long-term trends in two key attributes – the unemployment rate and the share of residents of immigrant background – on the probability of being selected as a target area, while holding the current levels of these attributes constant. The results are as expected: local authorities, while choosing from a pool of areas with high levels of unemployment, prefer areas which show first signs of a recovery or a gentrification process. This effect is interpreted as winner picking.

Although all chapters contain empirical applications incorporating several spatial aspects, there are various employed approaches suitable to a wide range of research environments. The chapters exhibit many interrelations between each other. It is therefore a convenient way to summarize and discuss the results by classifying the chapters into temporary categories, which change depending on the subject. At a first glance, the chapters can be classified into two groups: while Chapter 2 evaluates the effect of mega events on exports volumes and falls into the international trade or sports economics literature, chapters 3, 4, and 5 evaluate public policies.[3]

[3]While the nuclear accident in Fukushima was a disaster, the subsequent phase-out decision can

The chapters also differ in the way, the empirical strategy incorporates spatial aspects. In chapters 2 and 3, the distance between certain observations plays a central role. In Chapter 2, the distance between two countries is directly interpretetd as economic distance, or as a proxy for trade costs between these countries. In Chapter 3, one central finding is that the effect of the Fukushima incident on the worries about the safety of nuclear facility depends on the distance between the respondents place of residence and the location of the nearest nuclear facility. In the chapters 4 and 5, spatial aspects are incorporated more subtle via area fixed effects and clustering of the standard errors on varying spatial aggregation levels. A further crucial aspect in these chapters is the division of observations into target and control groups based on geocoding.

In chapters 2 and 4, a special emphasis is put on the identification strategy. Implementing a quasi-experimental research design, the empirical strategy aims at comparing the observations which have been exposed to a policy or an event (the treatment), to control groups, which are as similar as possible, but have not been exposed to the policy. These control groups can be based on economic theory (e.g. the OECD countries in Chapter 2 or the investigation areas in Chapter 4), or on synthetic matching approaches (e.g. propensity score matching). Based on certain covariates, this approach identifies control observations which are as similar as possible to the treated observations. These strategies lead in both cases to attenuated and less significant coefficients, which would have been regarded as valid results using conventional estimates. The findings in chapters 4 and 5 additionally present alternative ways to account for spatial autocorrelation of the error terms. Adopting an approach put forth by Conley (1999), the procedure allows to calculate standard errors corrected for spatial autocorrelation, serial correlation and heteroscedasticity.

In summary, the results of this thesis contribute to the spatial economics literature in several ways. They show, that it is crucial to take spatial aspects into account when evaluating economic effects, especially in an urban or regional context. It also becomes obvious repeatedly, that conventional estimates might be biased in face of spatial dependence, and that spatial econometric approaches can help reducing this bias. The results also show, that observations in many empirical research environments depend to at least some extend on the locational particularities they can be related to, which raises the impression that spatial dependence is not a particularity, but rather a normality. The results also show the importance of a convincing identification strategy

be seen as a public policy.

to isolate causal effects, which is particularly important for the chapters on policy evaluation. Combining spatial econometric approaches, synthetic matching methods, and economic theory to construct credible counterfactuals has proven very valuable in this context.

However, quantitative spatial economics is a very broad and complex research field, which has only started to attract the attention of a wider public. There is plenty of space for further research and the literature is far away from having converged on many issues. So far, the insights derived from the spatial economics literature in general, show that the impact of spatial economic approaches can be substantial. As spatially dependent data occurs frequently, spatial economic methods can be expected to become equally important as, for example, time series econometrics and should be included in any standard econometrics toolbox. The ongoing dissemination of spatial economic concepts into the main applied economics literature supports this view. As these insights are also in line with the findings from this thesis they give rise to the hope that this thesis makes a significant contribution to the spatial economics literature.

Chapter 2

Exports and Olympic Games: Is There a Signal Effect?*

Abstract: A recent study finds that Olympic Games host countries experience significant positive, lasting effects on exports. They interpret their results as an indication that countries use the hosting of such events to signal openness and competitiveness. The authors challenge these empirical findings on the grounds that a comparison of structurally different and nonmatching groups of countries might suffer from a selection bias. The authors demonstrate that with an appropriate matching and treatment methodology, the significant Olympic effect disappears.

Keywords: *export, Olympic Games, international trade, treatment, matching*
JEL: *F1, L83*

*Coauthored with Wolfgang Maennig (University of Hamburg). Published as Maennig and Richter (2012) in the *Journal of Sports Economics*.

2.1 Introduction

Rose and Spiegel (2011) [RS] find that Olympic Games host countries experience significant positive, lasting effects on exports.[2] Their results do not only hold for the actual hosts but also for countries that unsuccessfully bid for the Olympic Games. RS interpret their results as an indication that countries use such events to signal openness and economic competitiveness (i.e., a signal effect). We challenge the empirical findings of RS because they compare Olympic nations such as the United States, Japan, Germany, Canada, Italy, Spain, and Australia, which have been among the leading export nations for centuries, to all other nations. Their comparison of structurally different, nonmatching groups might suffer from a selection bias. We demonstrate that with an appropriately applied matching and treatment methodology, the RS Olympic export effect disappears.

To illustrate the structural differences between the subsamples, Figure 2.1 displays indices (1950 = 100) of the logarithms of real exports. The solid line depicts the average exports of the summer Olympics host countries, which clearly outperforms the dashed line depicting the average exports of nonhosts.[3] The dotted line shows the average exports of the Organization for Economic Cooperation and Development (OECD) member states of 2006, excluding Olympic hosts. Note that the export development of the founding members of the OECD (1961) does not significantly differ.[4]

2.2 Empirical Strategy

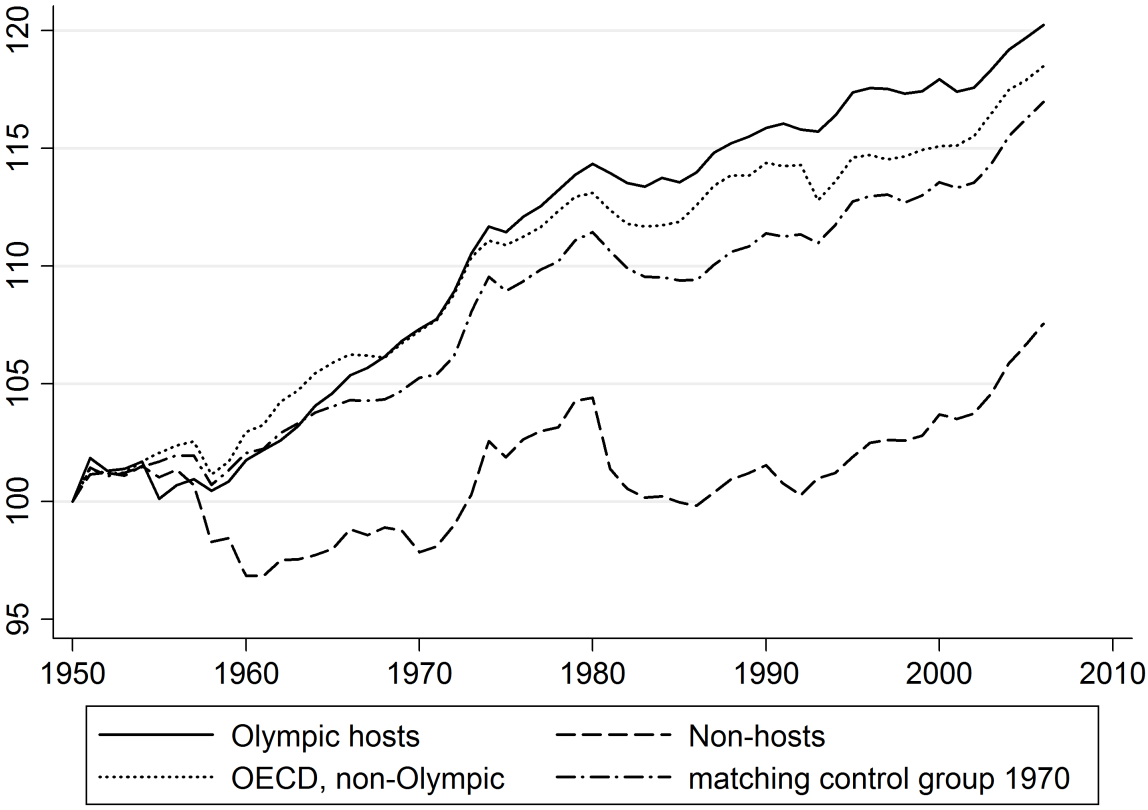
Overall, it seems plausible that Olympic host countries are structurally different from the majority of the rest of the world. To overcome this problem, we employ the matching strategy of Rosenbaum and Rubin (1983) and estimate propensity scores, that is, the probability of being part of a treatment group given a set of covariates. We use these estimations to systematically discriminate between Summer Olympic Games host countries (i.e., the treatment group) and nonhost countries (i.e., the control group). Only countries that are otherwise structurally similar are included in the subsequent analysis. The covariates included in the propensity score estimation should

[2] The authors thank Andrew K. Rose for providing the data as well as the STATA code for the base case regressions.

[3] As in RS, we focus our analysis on the Summer Olympic Games.

[4] Details are available from the authors upon request.

Fig. 2.1 Indexed real log exports



Notes: Own illustration based on aggregated export data. 1950 = 100.

affect both the outcome variable (i.e., exports) and the participation in the treatment (i.e., Olympic hosts), and they should either be measured before the treatment or be time-invariant (Caliendo and Kopeinig, 2008, p. 38). Note also that matching would not be possible if these covariates perfectly predicted the assignment into the treatment or the control group.[5] In our case, we aggregate the RS data to obtain a single export observation for each country i in year t.[6] We estimate the propensity scores using the logs of both the output and the population of the exporting country as covariates, fulfilling the balancing property.[7]

[5]Heckman et al. (1997, p. 637) emphasize that the covariates should not be "‘too good"’ but do not specify quantitative maximum requirements.

[6]The RS data set is an unbalanced sample of 707,519 observations, containing country i to country j export pairs for 196 countries between 1950 and 2006. Our aggregated sample includes 7,755 observations for country i exports for the same data period. The data sources are described in Rose and Spiegel (2011).

[7]The balancing property ensures that the treatment and the nontreatment groups differ only in the error term in the propensity score equation (Becker and Ichino, 2002). RS also apply a

We first estimate propensity scores for 1950; this is the first year of the RS data sample, which ranges from 1950 to 2006. This is also before the competition dates of the first Olympic Games included in the RS investigation (Olympic Summer games of 1952 in Helsinki, Finland). Thus, no treatment effects should be incorporated. For $t = 1950$, the values in the data set for four Olympic hosts are missing (namely, Union of Soviet Socialist Republics [USSR], Germany, Korea, and Greece), and the number of available nonhost countries is 44. Nineteen countries fulfill our common support condition, including the eight Olympic host countries.[8] We repeat the procedure for two further reference years, where data on more countries are available. For 1970, there are observations for all hosts except for the USSR. The nonhost group includes 106 countries, and 34 countries fulfill the common support condition. For 2000, data on all hosts are available. In that year, the nonhost group consists of 163 countries, while the common support condition is fulfilled by 37 countries.[9]

Apart from restricting our analysis to different subsamples of matching countries, we use the same investigation strategy as RS by employing an augmented version of the gravity model. Using RS's data set of single observations for each country i's exports to country j at each year t, we regress the logs of distance and output, an additional set of covariates, and an Olympic effect variable on the logarithms of exports of the country. The covariates include the log of the populations of both countries and a set of dummy variables that control, among other things, for common borders, common language, regional trade agreements, and common currency. The Olympic effect variable is a dummy variable that takes a value of 1 for the exporting country starting in the year it hosted the Olympic Games. For sensitivity analysis, we follow RS by alternatively estimating different combinations of year, dyadic, and country-specific fixed effects, and country-specific linear time trends.

matching strategy to evaluate the robustness of their results; however, their log file indicates that their covariates are not balanced.

[8]The common support region is [0.103, 0.946] for the $t = 1950$ subsample, [0.069, 0.953] for the $t = 1970$ subsample, and [0.056, 0.991] for the $t = 2000$ subsample.

[9]Nonhosts fulfilling the common support, $t = 1950$: Austria, Brazil, Denmark, France, India, the Netherlands, New Zealand, Norway, Sweden, Switzerland, and the United Kingdom. Additional hosts fulfilling the common support, $t = 1970$ (compared to $t = 1950$): Germany, Greece, and Korea. Additional nonhosts fulfilling the common support, $t = 1970$ (compared to $t = 1950$): Argentina, Chile, Colombia, Hungary, Indonesia, Peru, Philippines, Poland, Portugal, Saudi Arabia, Turkey, and Venezuela. Additional hosts fulfilling the common support, $t = 2000$ (compared to $t = 1950$): Germany, Greece, Korea, and Russia. Additional nonhosts fulfilling the common support, $t = 2000$ (compared to $t = 1950$): Argentina, Belgium, China, Hong Kong, Indonesia, Ireland, Israel, Malaysia, Poland, Portugal, Saudi Arabia, South Africa, Singapore, Thailand, and the United Arab Emirates.

Table 2.1 The Olympic effect, diverging control groups and methods

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Rose Spiegel (2011)	0.33**	0.24**	0.30**	0.19**	0.16**	0.34**	0.35**
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)
Common Support t = 1950	-0.20**	-0.01	0.01	0.15**	0.07	-0.19	0.01
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	-[a]	(0.04)
Common Support t = 1970	0.01	-0.02	0.01	0.10*	0.04	0.04	0.03
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)
Common Support t = 2000	-0.07	0	0.01	0.11**	0.01	-0.03	0.02
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)
OECD 2006	-0.03	-0.08*	-0.05	0.06	-0.04	0	-0.06
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Year Effects	YES	YES	YES	YES	YES	YES	NO
Dyadic Fixed Effects	NO	YES	NO	NO	NO	NO	NO
Exporter Fixed Effects	NO	NO	YES	NO	NO	NO	YES
Importer Fixed Effects	NO	NO	YES	NO	YES	NO	NO
Exporter*Time Fixed Effects	NO	NO	NO	YES	YES	NO	NO
Importer*Time Fixed Effects	NO	NO	NO	NO	NO	YES	YES

Notes: Significance: * (**) at 0.05 (0.01). Robust standard errors are in parentheses. [(a)]: Highly singular variance matrix. No standard deviations available.

2.3 Results

Table 2.1 reports the regression results for the Olympic effect coefficient if we restrict the RS method to the countries that fulfill the common support condition in 1950 (row 2), 1970 (row 3), and 2000 (row 4). For ease of comparison, row 1 displays the RS results, which we were able to replicate. As the dependent variable is estimated in logarithms, the RS estimate of 0.33 in row 1, column (1) would translate into a permanent Olympic effect on exports of about $exp(0.33) - 1 = 39\%$. However, with the single exception of specification (4) (i.e., fixed-year effects and country-specific export trends), no significant positive effects are measurable if the Olympic hosts are compared to matching groups of countries, avoiding a selection bias. For the sample restricted to those countries on the common support in $t = 1950$ and the specifications (1) and (6), even significant negative effects can be found. If the analysis is restricted to those countries on the common support in $t = 1970$ (row 3) and $t = 2000$ (row 4), where the data are the most complete, the majority of the effects is insignificant and around zero, with coefficients often below one standard deviation. Specification (4) is again an exception.

For readers who mistrust complex data selection methods as treatment and matching procedures, we alternatively compare the Olympic OECD countries with the non-

Olympic OECD countries, which can be reasonably assumed to be structurally alike. Again, no significant, positive effects on exports are found (row 5). Figure 2.1 might help clarify the striking difference between the results. RS compare Olympic countries (solid line) to all other countries (dashed line). As mentioned above, this implies a comparison of Olympic nations, such as the United States, Japan, Germany, Canada, Italy, Spain, and Australia, to some of the world's most disadvantaged nations. Instead, we compare Olympic countries to structurally similar countries, such as other OECD countries or control groups identified by empirical matching strategies. As mentioned above, RS find that their results do not only hold for actual hosts but also for countries that unsuccessfully bid for Olympic Games, leading them to the interpretation that countries use the Games (and similar events) to signal openness and increasing economic competitiveness (signal effect). However, when controlling for the structural similarities/dissimilarities of countries, again we did not find any systematically significant, positive effects for the bidding countries.[10]

RS regress,among other variables,Olympic dummies on export performance,which implies the test 'Olympic Games $\rightarrow$ competiveness'. RS interpret their results as a signal effect, which implies a reverse hypothesis of 'competiveness $\rightarrow$ (bidding for) Olympic Games', which is debatable because these results would be based on tests that regress export performance and other determinants on (the probability of) bidding for the Olympic Games. On the basis of the RS results, policy makers might thus believe that they can increase their country's exports by organizing the games or by bidding for them. There might be good reasons to bid for the Olympic Games, but our results provide a warning that the hopes for export growth should not part of rational motivations.

[10]Details are available from the authors upon request.

Chapter 3

Nuclear Accidents and Policy: Notes on Public Perception*

Abstract: Major nuclear accidents as recently in Fukushima set nuclear power plant security at the top of the public agenda. Using data of the German Socio-Economic Panel we analyze the effects of the Fukushima accident and a subsequent government decision on nuclear power phase-out on several measures of subjective perception in Germany. In the light of current political debates about the strategic orientation of this energy turnaround, such an analysis is of particular interest since non-pecuniary gains in measures of subjective perception might provide further aspects to be taken into consideration when evaluating the economic costs of the policy. We find that the Fukushima accident increases the probability to report greater worries about the environment. Furthermore, we find evidence for a decrease in the probability to be very worried about the security of nuclear power plants following the government's resolution on nuclear phase-out. Finally we find that the probabilities of reporting very high concerns are related to the distance between the respondents' place of residence and the nearest nuclear power station.

Keywords: *Environment, Fukushima, nuclear accident, nuclear energy, nuclear phase-out, subjective perception*
JEL: *I3, Q4, R1*

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3.1 Introduction

Access to reasonably priced energy is often regarded as a major determinant for the competitiveness of an economy. With many fossil resources such as coal being criticized in terms of their sustainability and renewable energy sources still being expensive and not yet fully established, many countries worldwide regard nuclear energy as a key technology in the struggle for affordable electricity. However, major nuclear accidents as recently in Fukushima set nuclear power plant security on top of the public agenda and increase pressure on policy makers to provide adequate reactions. In the case of Germany, the origins of these discussions and the formation of an anti-nuclear movement can be traced back to the 1970s. Following the 1986 Chernobyl nuclear disaster with large areas of Germany being affected by radioactive fallout, public opinion increasingly turned against this source of energy generation. A first act on nuclear phase-out passed by the Social Democratic/Green party coalition in 2002 was dismissed by the Christian Democratic/Liberal coalition in September 2010. However, increasing opposition towards nuclear energy after the Fukushima catastrophe in March 2011 resulted in a sudden change in policy. On June 6th 2011, the Christian Democratic/Liberal German government decided on a new accelerated phase-out with the final shutdown of eight power plants in August 2011 and a complete abandoning of nuclear energy by 2022.

In the light of substantial public opposition against the use of nuclear energy the question arises as to what extend far-reaching events such as nuclear accidents or changes in nuclear policy are reflected in subjective assessment. Regarding ongoing public discussions in Germany, such an analysis is of particular interest since nonmonetary gains in measures of subjective perception might provide further aspects to be taken into consideration when evaluating the economic costs of the energy turnaround. Using data of the German Socio- Economic Panel (SOEP), we investigate the impact of the Fukushima accident and the subsequent decision on nuclear phase-out on reported concerns about the environment. Taking advantage of a set of new variables included in the SOEP directly after the Fukushima accident, we further analyze the effects of the phase-out on concerns about nuclear power plant security as well as on the reliability of energy supply without the use of nuclear energy. In order to control for personal involvement, we complement our analysis by additional models that account for the distance from the respondents' place of residence to the nearest active nuclear power plant.

The remainder of the article is organized as follows: section 3.2 looks into the relevant literature followed by a presentation of the data source and empirical strategy in section 3.3. Section 3.4 discusses the results of our baseline models whereas section 3.5 presents the findings of the extended models including regional characteristics. The paper closes with a conclusion.

3.2 Literature

There is a growing field of economic literature looking into potential effects of disasters such as the attacks of 9/11 in the United States, Hurricane Katrina, the earthquake 2005 in Pakistan, and the nuclear accidents in Chernobyl or in Fukushima on measures of subjective perception. Most of the existing studies focus on individual life satisfaction but there are also a number of studies that focus on subjective concerns.

Berger (2010) analyzes effects of the 1986 Chernobyl accident on happiness and environmental concerns in Germany. While her results support the thesis that environmental concerns are affected by nuclear accidents, no such evidence is found concerning an impact on reported happiness. Danzer and Danzer (2011) test the long run influence of the Chernobyl accident on subjective life satisfaction in the Ukraine. As expected they find a negative impact on happiness for individuals exposed to the catastrophe. Remennick (2002) analyzes the health of Chernobyl survivors that immigrated into Israel whereas Bromet et al. (2000) focus on the happiness of local children that were infants or unborn at the time of the accident. Further economic or socioeconomic literature on the relationships between subjective life satisfaction respectively concerns about the environment and nuclear accidents is on the rise.

Hommerich (2012) investigates the effects of the Fukushima accident on trust and happiness in two Japanese regions and Rehdanz et al. (2013) use Japanese panel data in combination with regional information about the respondents' place of residence to analyze the effects of the Fukushima nuclear catastrophe on individual well-being in Japan. They find that individual well-being declined after the catastrophe with increasing proximity to the site of the accident. Using US data, Greenberg (2009) examines differences between people who live near nuclear facilities and a control group from other regions. The findings suggest that people who live near reactors have greater concerns about nuclear issues than the control group. Poortinga et al. (2013) look into the differences between the UK and Japan regarding the public perception of future energy use before and after the Fukushima incident. Furthermore, there exists a body of literature evaluating the effects of nuclear accidents on the public attitude towards nuclear energy and the risk of nuclear accidents (Eiser et al., 1989; Lindell and Perry, 1990; Verplanken, 1989; Visschers and Siegrist, 2013) and on energy policy (Csereklyei, 2013).

Thematically related, Luechinger and Raschky (2009) analyze the effect of natural disasters on life satisfaction, but focus on flood catastrophes. Their findings point out that flood catastrophes are negatively related with life satisfaction. Carroll et al. (2009) estimate the effects of droughts on happiness in Australia in order to quantify arising costs.

Hinman et al. (1993) and Hinman et al. (1993) assemble a list of environmental risks. By means of international data they find out that risks about nuclear issues such as nuclear accidents are top ranked. The studies of Kimball et al. (2006) and Metcalfe et al. (2011) investigate the impact of catastrophes on happiness in the country of the accident and in other countries. The former show, amongst others, that the earthquake in Pakistan in the year 2005 has an impact on life satisfaction in America, while the latter provide evidence that the terror attacks of 9/11 have a significant impact on people´s life satisfaction in the UK. For an overview about socio-economic determinants of environmental concerns in general see, e.g. Berger (2010) or Saijo and Shen (2007).

3.3 Data and empirical strategy

We model the effects of the Fukushima nuclear accident and the subsequent change in nuclear policy on concerns about the environment as well as on concerns about the reliability of energy supply without the use of nuclear energy and on concerns about the safety of nuclear power plants. Our working hypothesis is that the accident has a significant impact on environmental concerns, i.e. leads to an increase in concerns. In contrast, the nuclear phase-out could increase worries about reliable energy supply but lead to a decrease in concerns about nuclear power plant security.

We use data from the SOEP v28-edition (SOEP, 2012), a population-representative panel survey conducted in Germany (Wagner et al., 2007). Our constructed data set comprises the year 2011. To operationalize subjective perception we use three different single-item measurements included in the SOEP: worries about environmental protection, about the reliability of energy supply without the use of nuclear energy, and about the security of nuclear power plants. All items are captured on an ordinal three category scale, originally coded "very worried", "slightly worried" and "not worried". For ease of interpretation, all variables are mirrored.[2]

While the variable concerning environmental protection is available for all waves of the panel, both the question on worries about the reliability of energy supply and the question on security of nuclear power plants have been included in the SOEP surveys since April 2011.

The main independent variables consist of dummy structures describing the various time periods of interest. For the models on environmental concerns, we divide the observation period into three sub-periods. The questionnaires completed before the Fukushima accident are considered as control group, and the effect periods include (1) the weeks after the Fukushima catastrophe until the day before the decision on nuclear phase-out (03/11/2011 – 06/05/2011) and (2) the months after the government resolution (06/06/2011 – 09/30/2011). Regarding the questions on concerns about the reliability of energy supply and about nuclear power-station safety we use a modified layout in the corresponding models, where the period from April 1^{st} until June 5^{th} is used as reference period. The effect period between June 6^{th} and September 30^{th} should thus reflect the effects of the government resolution on nuclear phase-out. Additionally we control for a set of common socioeconomic variables including age, age squared, gender, log of monthly household income, marital status, children in household, educational level, and labor market status in addition to regional dummies. Table 3.4 in the appendix provides summary statistics.

The empirical strategy consists of the following steps: Throughout the model setup described above, we assume that both the Fukushima accident as well as the subsequent decision on nuclear phase-out can be regarded as quasi-exogenous shocks that should be reflected in changes in our measures of subjective perception. In order to verify this assumption and as an initial test for the presence of the suspected effects, we pool the 2011 data for each sub period with the observations for the same time span in 2010. We then perform separate ordered logit regression for each of the three time-subsamples, using a year dummy variable as an indicator for potential differences between the same time periods in 2010 and 2011.[3] If the assumption of quasi-exogenous shocks holds true, one would expect to see statistical significant

[2]The exact passages in the questionnaire are: "What is your attitude towards the following areas - are you concerned about them (Environmental Protection; Security of Nuclear Power Plants; Reliability of Energy Supply Without the Use of Nuclear Energy)?" Possible answers are “Very worried”, “Slightly worried” and “Not worried”.

[3]Since the question on environmental concern is the only of our three measures of interest included in the 2010 SOEP questionnaire, we rely on this variable in our initial annual comparison.

differences between the 2010 and 2011 data for the post-Fukushima and post-nuclear phase-out time spans whereas no such difference should be present for the reference period. Given the existence of such differences in between the two years, this could also be seen as evidence that potential in-year effects in the subsequent analyses are indeed caused by exogenous events instead of being a consequence of recurring seasonal variation.

After this initial year-over-year comparison for each of the three time periods, we focus on the data for the year 2011. Depending on the variable at question we employ the corresponding dummy structures described above to test for changes in the self-reported worries throughout the year 2011, in particular following the Fukushima accident and the subsequent decision on nuclear phase out. Given the ecological impact of a major nuclear accident, one would expect that environmental concerns increase during the weeks after the Fukushima accident. On the other hand, the government decision on nuclear phase out with the immediate shut down of eight nuclear reactors and the complete abandoning of nuclear energy by the year 2022 could be anticipated by a decrease in respective subjective worries. Due to the ordinal character of the dependent variables, we stick to the ordered logit estimation procedure throughout the analyses. In order to control for a possible bias due to differently sized subsamples of the SOEP, all observations are included taking into account their cross-sectional weights.

Finally one might argue that the size of potential effects depends on regional differences, especially the distance to the nearest nuclear power plant. To account for this possibility, we extend the preceding analysis by including a distance measure and the interaction between our effect variables and the distance indicator. In the SOEP, access to the respondents' geographical location is limited for privacy protection. However, the data at hand for this analysis allows regional identification on a Raumordnungsregion (ROR) level – planning units that divide Germany into 96 regions of an average size of 3,720 square kilometres (1,436 square miles) and an average population of 852,539. Hence, for each ROR z with a population of POP_z, we calculate a population-weighted average distance to the nearest active nuclear power station as

$$DIST_z = \sum_{i=1}^{n} \left(\frac{POP_i}{POP_z} \right) \cdot DIST_i, \tag{3.1}$$

where POP_i is the population in community i of ROR z with a distance of $DIST_i$

to the nearest active power station. The population data is obtained from the Statistisches Bundesamt (2012). We also take into account active nuclear power plants in directly neighbouring countries within a 100-km radius around Germany. Figure 3.1 shows the various RORs and the location of the nuclear power sites included in the subsequent analyses.

3.4 Baseline models

Table 3.1 shows the results for our initial tests on the validity of the assumption that both the Fukushima accident and the subsequent nuclear power phase out can be seen as quasi-exogenous shocks which should be reflected in a change in the measures of subjective perception. In order to ensure that potential in-year effects detected in the latter analyses are not caused by recurring seasonal trends, we pool the 2011 data for each of the three periods described above with data from the same time periods in 2010 and test whether there are statistical significant differences in between the two years. As only the question on worries about the environmental protection is available for both years, we run these tests using this variable as our dependent measure.

The upper panel of Table 3.1 shows the marginal effects of the 2011 year dummy for each of the three outcomes of the variable on environmental worries during the first few weeks of the year. As mentioned before, this time span will serve as our reference period in the latter in-year analyses for 2011. Since all three corresponding marginal effects proof to be insignificant, our working hypothesis that the first few weeks of 2011 should not differ significantly from previous years appears to hold true. However, as expected, this changes for the time period after the Fukushima accident. Following the events of March 11^{th}, 2011, people appear to be significantly more worried about the environment compared to the same time period in the year before. In particular, the probability of being very concerned increases by 2.81 percentage points whereas both, the probabilities of being somewhat worried and not worried at all show a highly significant decrease. Systematic differences in self-reported environmental concerns in between the two years are also present for the time following the government's decision on nuclear phase-out (06/06/2011 - 09/30/2011). Though on a less significant level, environmental worries appear to be lower in 2011 than during the same time of the previous year.

As shown in Table 3.1, there appear to be significant year-over-year differences in the level of environmental concerns for both the weeks following the Fukushima

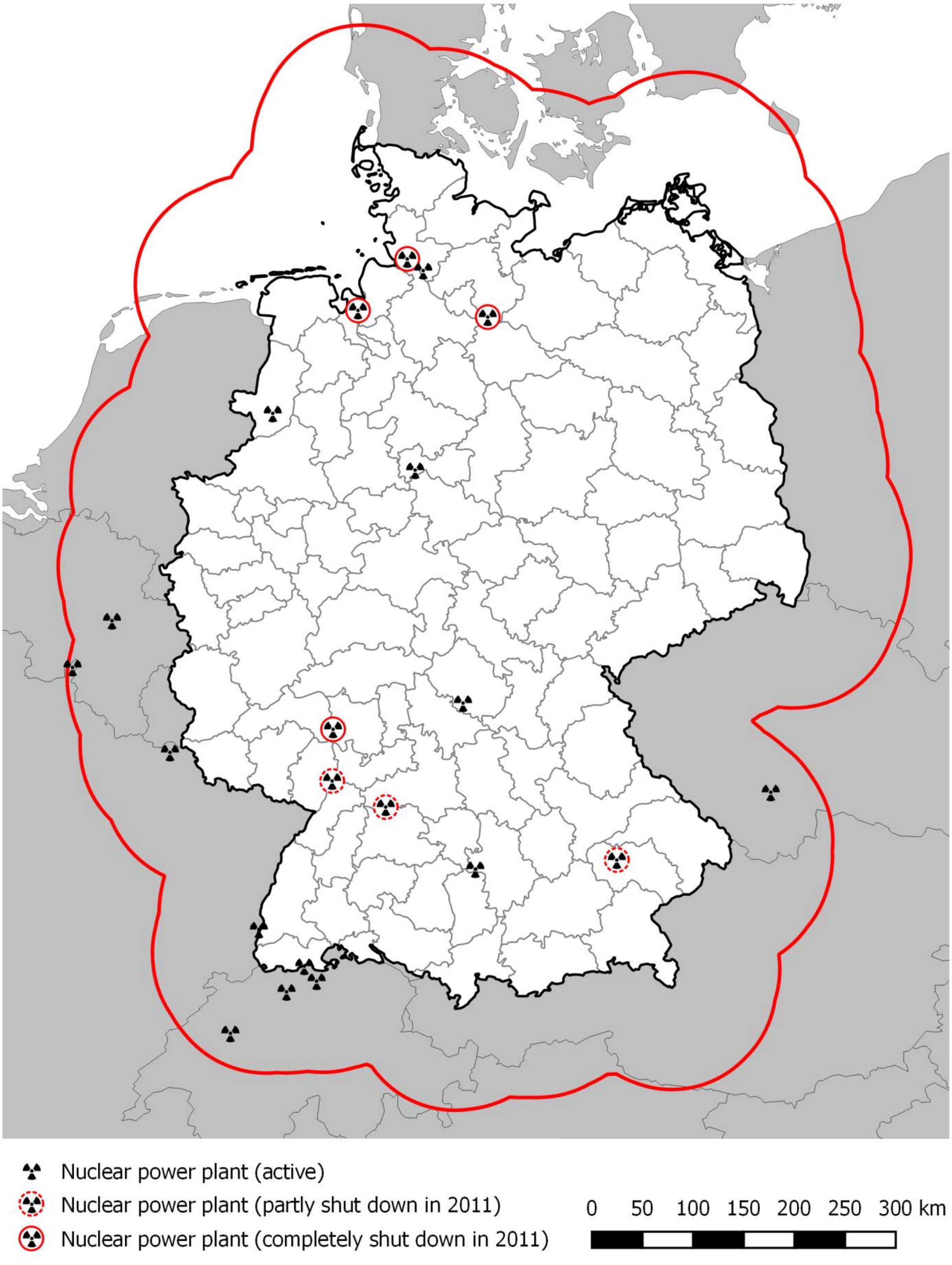
Table 3.1 Fukushima accident and nuclear power phase-out in Germany: Time period subsamples

	$\frac{\partial Pr(y_i=not\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=slightly\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=very\ worried)}{\partial x_i^j}$
Reference period (02/01 – 03/10)			
Year 2010	*(Ref.)*	*(Ref.)*	*(Ref.)*
Year 2011	0.0049	0.0050	-0.0099
	(0.0054)	(0.0055)	(0.0108)
Observations		12276 (6409/5867)	
Pseudo R^2		0.0139	
Fukushima accident (03/11 - 06/05)			
Year 2010	*(Ref.)*	*(Ref.)*	*(Ref.)*
Year 2011	-0.0121***	-0.0160***	0.0281***
	(0.0047)	(0.0061)	(0.0108)
Observations		13997 (6070/7927)	
Pseudo R^2		0.0163	
Nuclear power phase-out (06/06 - 09/30)			
Year 2010	*(Ref.)*	*(Ref.)*	*(Ref.)*
Year 2011	0.0169*	0.0130	-0.0299*
	(0.0101)	(0.0081)	(0.0181)
Observations		4645 (1234/3411)	
Pseudo R^2		0.0227	

Notes: Ordered logit estimates; dependent variable: Worries about the reliability of energy supply without the use of nuclear energy (coded 1 – 3); marginal effects; robust standard errors in brackets, error probabilities in parentheses: ***p<0.01 - **p<0.05 - *p<0.1; cross section weights; other exogenous variables: Gender, age, age (squared), log household income, child in household, marital status, employment status, education, state dummies and regional dummy (east).

accident and the subsequent resolution on nuclear phase-out. Building upon this initial evidence we now turn to our in-year analysis for the year 2011, using the dummy structures described above. Table 3.2 shows the main results for the our baseline specification in which we assess the effects on reported worries about the environment, the reliability of energy supply without the use of nuclear energy and the security of nuclear power plants. All reported parameters are marginal effects calculated from pooled cross-section ordered logit regressions for the year 2011. For clarity, we report the marginal effects for all outcomes as “not worried”, “slightly worried” and “very worried”. Based on the previous findings, it is expected that any changes in concerns related to the nuclear accident would predominantly appear in the category “very worried”.

Fig. 3.1 Regional planning units (ROR) and nuclear power plant sites



Notes: Own illustration. Following the decision on nuclear phase-out, a total of eight nuclear reactors were denominated for immediate shut down (Biblis A, Biblis B, Brunsbüttel, Isar 1, Krümmel, Neckarwestheim 1, Philippsburg 1, Unterweser). However, at three of these sites other reactors remain operational for the time being (Isar 2, Neckarwestheim 2 and Philippsburg 2).

Table 3.2 Fukushima accident and nuclear power phase-out in Germany: ordered logit estimates

	$\frac{\partial Pr(y_i=not\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=slightly\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=very\ worried)}{\partial x_i^j}$
Worries about environmental protection			
Before Fukushima accident: 02/01/2011 – 03/10/2011	*(Ref.)*	*(Ref.)*	*(Ref.)*
Fukushima accident: 03/11/2011 – 06/05/2011	-0.0224*** (0.0049)	-0.0248*** (0.0055)	0.0471*** (0.0103)
Nuclear power phase-out: 06/06/2011 – 09/30/2011	0.0094* (0.0056)	0.0104* (0.0063)	-0.0198* (0.0119)
Observations		17205	
Pseudo R^2		0.0164	
Worries about the reliability of energy supply without the use of nuclear energy			
Fukushima accident 04/01/2011 – 06/05/2011	*(Ref.)*	*(Ref.)*	*(Ref.)*
Nuclear power phase-out: 06/06/2011 – 09/30/2011	0.0226 (0.0162)	-0.0113 (0.0081)	-0.0113 (0.0081)
Observations		4269	
Pseudo R^2		0.0190	
Worries about the security of nuclear power plants			
Fukushima accident 04/01/2011 – 06/05/2011	*(Ref.)*	*(Ref.)*	*(Ref.)*
Nuclear power phase-out: 06/06/2011 – 09/30/2011	0.0742*** (0.0118)	0.0163*** (0.0036)	-0.0906*** (0.0144)
Observations		4278	
Pseudo R^2		0.0384	

Notes: Dependent variable: Worries about environmental protection (coded 1 – 3); worries about the reliability of energy supply without the use of nuclear energy (coded 1 – 3), worries about the security of nuclear power plants (coded 1 – 3); marginal effects; robust standard errors in brackets; coefficients of the models, with error probabilities in parentheses: ***p<0.01 - **p<0.05 - *p<0.1; cross section weights; other exogenous variables: Gender, age, age (squared), log household income, child in household, marital status, employment status, education, state dummies and regional dummy (east).

Concerning the worries about environmental protection, highly significant effects can be observed for the weeks immediately after the Fukushima catastrophe. In particular, as reported in the upper panel of Table 3.2, the probability of reporting very high concerns about environmental protection increases by up to 4.7 percentage points compared to the reference period. A closer inspection of the estimated probabilities for the other two outcomes further reveals that this increase in very high concerns does not just rely on answers by respondents with some already-existing ecological sensitivity (-2.5 percentage points) but also seems to be a result of a changed perception among people who previously reported no worries about environmental protection (-2.2 percentage points).

Regarding the months after the government resolution on nuclear phase-out, there are some indications of a decrease in environmental concerns among the German population. Whereas the probability that people report very high concerns decreases by about 2 percentage points compare to the reference period, both other categories become respectively more likely. However, the effects for the time after the decision on nuclear phase-out are only slightly significant and should therefore be interpreted with caution. Overall the in-year effects for concerns about the environmental protection correspond nicely to the previous findings from the year-over-year comparison. We note that the ecological awareness among the German public is sensitive about international environmental disasters such as the one in Fukushima, potentially leading to non-pecuniary costs for the German public (Berger, 2010, e.g.).

The middle panel of Table 3.2 presents the findings on self-reported concerns about the reliability of energy supply without the use of nuclear energy. Since this variable was only included in the SOEP questionnaires following the Fukushima events, we only observe the period from April 1^{st}, 2011 until the end of 2011, using the weeks before the decision on nuclear phase-out as reference. Somewhat expectantly, no significant effects appear to be present after the announcement on June 6^{th}, 2011. Nonetheless, following weeks of public discussion with ongoing debates and arguments over the necessity of nuclear power in the German energy market, the fact that the actual decision on immediate permanent shut down of seven nuclear reactors and complete phase-out by the year 2022 does not reflect in related worries can be seen as a pronounced sign of confidence in the compensability of nuclear energy.

The lower panel of Table 3.2 shows the results of our baseline models on concerns about the security of nuclear power plants. As before, data availability restricts our analysis to the post Fukushima periods, using the same dummy structure as described in the previous paragraph. Yet, contrary to the findings concerning the reliability of energy supply, in this case highly significant effects appear to be present following the announcement of nuclear phase-out. In particular, the probability of being very worried drops by up to 9.1 percentage points. This surprisingly strong decrease is accompanied by respective increases in the probabilities of being slightly worried (1.6 percentage points) and of being not concerned at all (7.4 percentage points). We conclude that the phase-out decision reduces the probability to report greater worries about nuclear power plant security. Considering the fact that the complete phase-out will not be completed until the year 2022, both the magnitude of the changes and the pattern of deferrals in the two extreme outcomes can be considered as somewhat surprising. Yet, these findings provide some evidence for a rather strong relief in the German public caused by the anticipated phase-out. The results are in line with our previous findings, indicating nonmonetary gains generated by the phase-out decision.

3.5 Regional models

As shown in the preceding analyses, both the Fukushima accident and the announcement of nuclear phase-out appear to have a significant influence on subjective perception in the German public. One might argue, however, that the effects are predominantly determined by regional influences, especially the varying proximity to active nuclear power plants, resulting in different levels of sensitivity. To account for this possibility, we alter our models by including the population weighted distance proxy described in Section 3.3 and considering possible interactions with our effect variables. Table 3.3 reports the estimated results of all regional interaction models for each endogenous variable used in the previous analyses. As before, we use ordered logit models and present marginal effects for all variables of interest. The interaction effects are reported at means.[4]

Concerning the worries about environmental protection, the corresponding marginal effects presented in the upper panel of Table 3.3 pretty much resemble those of the baseline specifications shown in previous section (compare Table 3.2). In particular,

[4]The use of interaction terms in non-linear models might lead to biased estimates in both, marginal effects and standard errors. As a robustness check of our ordered logit results, we also replicate the models shown in Table 3.3 using a standard logit approach, thus allowing for the application of the procedure suggested by Norton et al. (2004) for the calculation of corrected interaction term values in logit and probit specifications. In each case, the results of the ordered logit specifications are generally confirmed. For details see Table 3.6 in the appendix.

the results of the regional model suggest that, when living at a mean distance to an active nuclear power station, the sole probability of being very concerned about the environment increases by up to 4.5 percentage points following the Fukushima accident. Whereas the absolute distance to the nearest power plant appears to be statistically insignificant, the highly significant interaction term between the variable on the Fukushima accident and the population weighted distance measure indicates that there is a conditional relationship between the two variables.

Each additional kilometer in between a respondent's place of residence and the nearest active reactor has an average compensatory effect of about 0.04 percentage points, meaning that the increase in the probability of reporting very high concerns is of less magnitude, the further one lives away from an active nuclear power plant. In contrast, the Fukushima accident leads to a drop in the probabilities of reporting slight (no) concerns about the protection of the environment or no environmental worries by about 2.4 (2.2) percentage points. Each additional kilometer to the nearest active power plant increases the probability of reporting one of these two outcomes by about 0.2 percentage points.

Consistent with our earlier findings, neither the decision on the nuclear power phase-out nor the absolute distance to an active nuclear power plant appears to be statistically significant in the regional specification on the concerns about the reliability of energy supply without the use of nuclear energy. However, as shown in the lower panel of Table 3.3, highly significant effects are present in the regional model using the concerns about the security of nuclear power plants as the dependent measure. In line with our earlier findings from the baseline specifications (compare Table 3.2), the probability of reporting very high concerns about the security of nuclear power plants drops noticeably following the decision on nuclear phase-out with the immediate closure of seven power plants and the anticipated shut down off all German nuclear power stations by the year 2022.

However, as indicated by the significant interaction term, the actual magnitude of this effect appears to depend crucially on the distance between the respondent's place of residence and the nearest active power station. Whereas concerns decrease by about 9 percentage points at a mean distance, each additional kilometer has an additional reinforcing effect of 0.04 percentage points, i.e. the probability of being very worried about the security of nuclear power plants decreases the more, the further one lives away from an active nuclear power plant. Naturally, this relief is also nicely reflected in the corresponding effects on the other two outcomes, in particular in the marginal effect of not being worried at all: Following the government's resolution on nuclear phase out, the probability of not being worried increases by about 7.4 percentage point with each additional kilometer adding another 0.04 percentage points.

While the regional specifications generally confirm the results of our baseline models in both size and significance, they add to the earlier findings by indicating that the magnitudes of the detected effects depend conditionally on a respondent's distance to an active nuclear power station. On the one hand, the increase in worries about the environmental protection following the Fukushima accident appears to be the more pronounced the nearer one lives to an active nuclear power plant. On the other hand, people living in the periphery of an active reactor show less relief concerning the security of nuclear power stations following the decision on nuclear phase-out than individuals that live at a greater distance. Taking into account that the process of complete nuclear phase-out in Germany is supposed to last until the year 2022, these results can also be interpreted as a reflection of a higher general sensitivity towards atomic energy when one lives in the vicinity of a nuclear power station.

3.6 Conclusion

The use of nuclear power is often controversially discussed. While widely accepted as a civil power source in many countires throughout the world, it also faces strong public opposition in other nations. Major nuclear accidents as in Chernobyl or recently in Fukushima set nuclear power plant security on top of the public agenda. In Germany, facing public pressure, a rather sudden nuclear power phase-out plan was passed by the government in the aftermath of Fukushima 2011.

In this article, we analyze the effects of the Fukushima nuclear accident and the subsequent phase-out on subjective perceptions in Germany, using three single item measurements from the SOEP: concerns about the environmental protection, worries about the reliability of energy supply without the use of nuclear energy and concerns about the safety of nuclear power stations.

Our findings suggest that the Fukushima accident itself led to an increase in the probability of reporting high concerns about environmental protection of about 4.7 percentage points. Moreover, worries about the safety of nuclear power plants are strongly affected by the governments decision on nuclear power phase-out, resulting in a decline in the probability of being very worried about power plant security of 9.1 percentage points. It is also shown that the magnitude of the detected effects for environmental concerns as well as worries about nuclear power plant safety depends on regional characteristics with people living nearby an active nuclear power station generally showing a more sensible reaction than those living at greater distance.

In summary, our results are conclusive that catastrophes and changes in policies can have an immediate impact on public perception. While these results are consistent with reasonable prior beliefs, this study adds to the literature that provides empirical evidence, and provides an approximation of the magnitude of such effects. Moreover, one can conclude that the German government's decision on an energy turnaround in the weeks after the Fukushima accident had an immediate significant positive influence on the German public perception. Even though the corresponding effects are of non-monetary nature and are thus difficult to compare with the classical monetary costs associated with the accelerated nuclear phase-out, they should probably still be taken into consideration when evaluating the total economic welfare effect of this change in policy.

These positive subjective externalities appear even more pronounced when taking into consideration that no evidence was found for an increase in concerns about the reliability of energy supply without the use of nuclear energy during the weeks after the actual government resolution. However, it should be noted that the analysis presented here focuses on a short to medium time horizon after the actual events. It is up to future research to look into longer term effects that could for instance be caused by continuously rising energy prices as observed in recent years.

Table 3.3 Fukushima accident and nuclear power phase-out in Germany – distance to the nearest active power plant: ordered logit estimates

	$\frac{\partial Pr(y_i=not\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=slightly\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=very\ worried)}{\partial x_i^j}$
Worries about environmental protection			
Before Fukushima accident: 02/01/2011 – 03/10/2011	*(Ref.)*	*(Ref.)*	*(Ref.)*
Fukushima accident:	-0.0215***	-0.0239***	0.0454***
03/11/2011 – 06/05/2011	(0.0049)	(0.0055)	(0.0103)
Nuclear power phase-out:	0.0093	0.0103	-0.0195
06/06/2011 – 09/30/2011	(0.0057)	(0.0064)	(0.0121)
Distance to the nearest active	-0.0001	-0.0001	0.0002
power plant (weighted)	(0.0001)	(0.0001)	(0.012)
*Fukushima accident * Distance*	0.0002**	0.0002**	-0.0004**
(weighted)	(0.0001)	(0.0001)	(0.0002)
*Nuclear power phase-out **	0.0002**	0.0002**	-0.0004**
Distance (weighted)	(0.0001)	(0.0001)	(0.0002)
Observations		17205	
Pseudo R^2		0.0169	
Worries about the reliability of energy supply without the use of nuclear energy			
Fukushima accident 04/01/2011 – 06/05/2011	*(Ref.)*	*(Ref.)*	*(Ref.)*
Nuclear power phase-out:	0.0246	-0.0123	-0.0123
06/06/2011 – 09/30/2011	(0.0166)	(0.0083)	(0.0083)
Distance to the nearest active	-0.0002	0.0001	0.0001
power plant (weighted)	(0.0004)	(0.0002)	(0.0002)
*Nuclear power phase-out **	0.0006**	-0.0003**	-0.0003**
Distance (weighted)	(0.0002)	(0.0001)	(0.0001)
Observations		4269	
Pseudo R^2		0.0197	
Worries about the security of nuclear power plants			
Fukushima accident 04/01/2011 – 06/05/2011	*(Ref.)*	*(Ref.)*	*(Ref.)*
Nuclear power phase-out:	0.0744***	0.0164***	-0.0908***
06/06/2011 – 09/30/2011	(0.0120)	(0.0036)	(0.0147)
Distance to the nearest active	-0.0001	-0.00002	-0.0001
power plant (weighted)	(0.0002)	(0.0001)	(0.0003)
*Nuclear power phase-out **	0.0004**	0.0001**	-0.0004**
Distance (weighted)	(0.0002)	(0.0000)	(0.0002)
Observations		4278	
Pseudo R^2		0.0391	

Notes: Dependent variable: Worries about environmental protection (coded 1 – 3); worries about the reliability of energy supply without the use of nuclear energy (coded 1 – 3), worries about the security of nuclear power plants (coded 1 – 3); marginal effects; robust standard errors in brackets; coefficients of the models, with error probabilities in parentheses: ***p<0.01 - **p<0.05 - *p<0.1; cross section weights; other exogenous variables: Gender, age, age (squared), log household income, child in household, marital status, employment status, education, state dummies and regional dummy (east).

Appendix 3.A Technical Appendix

3.A.1 Descriptive statistics

Table 3.4 shows summary statistics for all variables used in the analyses. Percentage shares are displayed for all categorical variables whereas mean and standard deviation values are presented for metric variables. If not stated otherwise, all data shown for the period of the Fukushima accident relates to the period between March 11^{th} and June 5^{th}, 2011 as used in the specification on worries about environmental protection. Due to data availability this time span varies in the models concerning energy supply and the security of nuclear power plants.

3.A.2 Complete results

Tables 3.5, 3.6, and 3.7 correspond to the the upper, middle, and lower panel of Table 2 in the main paper and present the full results of our ordered logit baseline specifications. Most of the covariates show the expected signs and magnitudes well established in the literature.

Table 3.4 Descriptive Statistics

	Before Fukushima accident: 02/01/2011 – 03/10/2011	Fukushima accident: 03/11/2011 – 06/05/2011	Nuclear power phase-out: 06/06/2011 – 09/30/2011
Worries about environmental protection:			
Not Worried	12.58 %	10.31 %	13.37 %
Slightly Worried	60.17 %	56.18 %	58.19 %
Very Worried	27.25 %	33.52 %	28.44 %
*Worries about the reliability of energy supply without the use of nuclear energy:**			
Not Worried	-	41.21 %	43.42 %
Slightly Worried	-	42.99 %	41.20 %
Very Worried	-	15.80 %	15.38 %
*Worries about the security of nuclear power plants:**			
Not Worried	-	21.12 %	26.30 %
Slightly Worried	-	40.53 %	43.27 %
Very Worried	-	38.35 %	30.43 %
Female:			
Mean	0.528	0.523	0.529
SD	(0.499)	(0.500)	(0.499)
Age:			
Mean	54.893	51.952	51.342
SD	(17.096)	(16.296)	(16.907)
Household Income:			
Mean	3104.797	3334.208	3025.248
SD	(2,032.408)	(2,257.719)	(2,249.317)
East:			
Mean	0.293	0.236	0.175
SD	(0.455)	(0.425)	(0.380)
Child in Household:			
Mean	0.217	0.276	0.290
SD	(0.413)	(0.447)	(0.454)
Distance to nearest active nuclear power plant:			
Mean	106.13	99.288	104.313
SD	-66.79	-63.812	(65.864)
Labor Status:			
Full-Time Employment	32.20%	36.99%	35.12%
Part-Time Employment	10.12%	12.25%	11.32%
Not Employed	6.32%	6.27%	7.86%
Unemployed	4.40%	4.28%	5.66%
Retired	36.41%	27.43%	27.97%
Self-Employed	4.74%	6.84%	6.24%
Irregular Employment	4.06%	4.28%	3.87%
In Education	1.76%	1.68%	1.96%
Marital Status:			
Married	67.46%	68.05%	63.41%
Single	17.91%	19.26%	21.81%
Divorced	5.98%	6.26%	8.53%
Widowed	8.64%	6.43%	6.24%
Educational Level (ISCED):			
ISCED 1 & 2	15.36%	13.60%	16.36%
ISCED 3 & 4	54.00%	54.69%	56.93%
ISCED 5 & 6	30.65%	31.71%	26.71%
Observations	5867	7927	3411

Notes: * The variables worries about the reliability of energy supply without the use of nuclear energy and worries about the security of nuclear power plants are available for the period from April 2011 to December 2011. We divide the observation period into two sub periods: Fukushima accident: 04/01/2011 – 06/05/2011, Nuclear power phase-out: 06/06/2011 – 09/30/2011.

Table 3.5 Fukushima accident and nuclear power phase-out in Germany: ordered logit estimates Worries about environmental protection: full results

	$\frac{\partial Pr(y_i=not\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=slightly\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=very\ worried)}{\partial x_i^j}$
Worries about environmental protection			
Before Fukushima accident: 02/01/2011 – 03/10/2011	(Ref.)	(Ref.)	(Ref.)
Fukushima accident: 03/11/2011 – 06/05/2011	-0.0224*** (0.0049)	-0.0248*** (0.0055)	0.0471*** (0.0103)
Nuclear power phase-out: 06/06/2011 – 09/30/2011	0.0094* (0.0056)	0.0104* (0.0063)	-0.0198* (0.0119)
Exogenous Variables:			
Female	-0.0344*** (0.0048)	-0.0381*** (0.0052)	0.0724*** (0.0097)
Age	-0.0064*** (0.0010)	-0.0071*** (0.0010)	0.0136*** (0.0019)
Age (squared)	0.0001*** (0.0000)	0.0001*** (0.0000)	-0.0001*** (0.0000)
Log Household Income	0.0031 (0.0044)	0.0035 (0.0049)	-0.0066 (0.0092)
East	0.0012 (0.0210)	-0.0013 (0.0233)	0.0024 (0.0443)
Labor Status			
Full-Time Employment	(Ref.)	(Ref.)	(Ref.)
Part-Time Employment	-0.0161** (0.0075)	-0.0178** (0.0083)	0.0338** (0.0159)
Not Employed	0.0113 (0.0089)	0.0125 (0.0099)	-0.0239 (0.0188)
Unemployed	-0.0209* (0.0112)	-0.0232* (0.0124)	0.0441* (0.0236)
Retired	-0.0126 (0.0078)	-0.0139 (0.0087)	0.0265 (0.0165)
Self-Emloyed	0.0147 (0.0099)	0.0163 (0.0110)	-0.0310 (0.0209)
Irregular Employment	-0.0180 (0.0114)	-0.0199 (0.0127)	0.0379 (0.0240)
In Education	0.0058 (0.0214)	0.0064 (0.0237)	-0.0121 (0.0451)
Marital Status			
Married	(Ref.)	(Ref.)	(Ref.)
Single	-0.0114 (0.0072)	-0.0126 (0.0080)	0.0239 (0.0151)
Divorced	0.0076 (0.0086)	0.0084 (0.0095)	-0.0161 (0.0181)
Widowed	0.0143* (0.0084)	0.0158* (0.0093)	-0.00053277
Child in household	-0.0022 (0.0058)	-0.0024 (0.0064)	0.0047 (0.0122)
Educational Level (ISCED)			
ISCED 1 & 2	(Ref.)	(Ref.)	(Ref.)
ISCED 3 & 4	-0.0097 (0.0064)	-0.0107 (0.0070)	0.0204* (0.0134)
ISCED 5 & 6	-0.0215*** (0.0074)	-0.0238*** (0.0082)	0.0453*** (0.0155)
State Dummies	Yes	Yes	Yes
Observations		17205	
Pseudo R2		0.0164	

Notes: Dependent variable: Worries about environmental protection (coded 1 – 3); marginal effects; robust standard errors in brackets; coefficients of the models, with error probabilities in parentheses: ***p<0.01 - **p<0.05 - *p<0.1; cross section weights.

Table 3.6 Fukushima accident and nuclear power phase-out in Germany: ordered logit estimates Worries about the reliability of energy supply without the use of nuclear energy: full results

	$\frac{\partial Pr(y_i=not\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=slightly\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=very\ worried)}{\partial x_i^j}$
Worries about the reliability of energy supply without the use of nuclear energy			
Fukushima accident: 04/01/2011 – 06/05/2011	(Ref.)	(Ref.)	(Ref.)
Nuclear power phase-out: 06/06/2011 – 09/30/2011	0.0226 (0.0162)	-0.0113 (0.0081)	-0.0113 (0.0081)
Exogenous Variables:			
Female	-0.0107 (0.0182)	0.0054 (0.0091)	0.0054 (0.0091)
Age	0.0015 (0.0034)	-0.0008 (0.0017)	-0.0008 (0.0017)
Age (squared)	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Log Household Income	0.0241 (0.0188)	-0.0120 (0.0094)	-0.0120 (0.0094)
East	-0.1461 (0.969)	0.0730 (0.0485)	0.0731 (0.0485)
Labor Status			
Full-Time Employment	(Ref.)	(Ref.)	(Ref.)
Part-Time Employment	0.0092 (0.0288)	-0.0046 (0.0144)	-0.0046 (0.0144)
Not Employed	-0.0406 (0.0354)	0.0203 (0.0177)	0.0203 (0.0177)
Unemployed	-0.0320 (0.0454)	0.0160 (0.0226)	0.0160 (0.0228)
Retired	-0.0233 (0.0322)	0.0117 (0.0161)	0.0117 (0.0161)
Self-Emloyed	0.0419 (0.0404)	-0.0209 (0.0203)	-0.0209 (0.0202)
Irregular Employment	-0.0119 (0.0450)	0.0060 (0.0225)	0.0060 (0.0225)
In Education	0.0399 (0.0724)	-0.0200 (0.0362)	-0.0200 (0.0362)
Marital Status			
Married	(Ref.)	(Ref.)	(Ref.)
Single	0.0906*** (0.0275)	-0.0453*** (0.0141)	-0.0453*** (0.0136)
Divorced	0.0623** (0.0303)	-0.0312** (0.0153)	-0.0312** (0.0151)
Widowed	0.0548* (0.0329)	-0.0274* (0.0165)	-0.0274* (0.0164)
Child in household	0.0480** (0.0228)	-0.0240** (0.0115)	-0.0240** (0.0114)
Educational Level (ISCED)			
ISCED 1 & 2	(Ref.)	(Ref.)	(Ref.)
ISCED 3 & 4	0.0422* (0.0230)	-0.00024265	-0.00024265
ISCED 5 & 6	0.1367*** (0.0282)	-0.0684*** (0.0144)	-0.0683*** (0.0142)
State Dummies	Yes	Yes	Yes
Observations		4269	
Pseudo R2		0.019	

Notes: Dependent variable: Worries about the reliability of energy supply without the use of nuclear energy (coded 1 – 3), marginal effects; robust standard errors in brackets; coefficients of the models, with error probabilities in parentheses: ***p<0.01 - **p<0.05 - *p<0.1; cross section weights.

Table 3.7 Fukushima accident and nuclear power phase-out in Germany: ordered logit estimates Worries about the security of nuclear power plants: full results

	$\frac{\partial Pr(y_i=not\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=slightly\ worried)}{\partial x_i^j}$	$\frac{\partial Pr(y_i=very\ worried)}{\partial x_i^j}$
Worries about the security of nuclear power plants			
Fukushima accident: 04/01/2011 – 06/05/2011	(Ref.)	(Ref.)	(Ref.)
Nuclear power phase-out: 06/06/2011 – 09/30/2011	0.0742*** (0.0118)	0.0163*** (0.0036)	-0.0906*** (0.0144)
Exogenous Variables:			
Female	-0.0683*** (0.0132)	-0.0150*** (0.0034)	0.0833*** (0.0158)
Age	-0.0161*** (0.0026)	-0.0035*** (0.0007)	0.0196*** (0.0032)
Age (squared)	0.0001*** (0.0000)	0.0000*** (0.0000)	-0.0001*** (0.0000)
Log Household Income	0.0265* (0.0138)	0.0058* (0.0032)	-0.0323* (0.0169)
East	-0.0497 (0.0577)	-0.0109 (0.0128)	0.0606 (0.0704)
Labor Status			
Full-Time Employment	(Ref.)	(Ref.)	(Ref.)
Part-Time Employment	-0.0203 (0.0212)	-0.0044 (0.0047)	0.0247 (0.0258)
Not Employed	-0.0107 (0.0245)	-0.0024 (0.0054)	0.0131 (0.0298)
Unemployed	-0.0030 (0.0307)	-0.0007 (0.0067)	0.0037 (0.0374)
Retired	0.0238 (0.0236)	0.0052 (0.0052)	-0.0290 (0.0288)
Self-Emloyed	0.0376 (0.0278)	0.0083 (0.0062)	-0.0458 (0.0339)
Irregular Employment	0.0403 (0.0319)	0.0089 (0.0071)	-0.0492 (0.0389)
In Education	0.0694 (0.0512)	0.0152 (0.0115)	-0.0847 (0.0624)
Marital Status			
Married	(Ref.)	(Ref.)	(Ref.)
Single	-0.0190 (0.0199)	-0.0042 (0.0044)	0.0232 (0.0243)
Divorced	0.0252 (0.0221)	0.0055 (0.0049)	-0.0307 (0.0269)
Widowed	0.0158 (0.0268)	0.0035 (0.0059)	-0.0192 (0.0327)
Child in household	-0.0159 (0.0160)	-0.0035 (0.0036)	-0.0194 (0.0196)
Educational Level (ISCED)			
ISCED 1 & 2	(Ref.)	(Ref.)	(Ref.)
ISCED 3 & 4	-0.0085 (0.0166)	-0.0019 (0.0037)	0.0103 (0.0202)
ISCED 5 & 6	0.0159 (0.0201)	0.0035 (0.0044)	-0.0194 (0.0245)
State Dummies	Yes	Yes	Yes
Observations		4278	
Pseudo R2		0.0384	

Notes: Dependent variable: Worries about the security of nuclear power plants (coded 1 – 3); marginal effects; robust standard errors in brackets; coefficients of the models, with error probabilities in parentheses: ***p<0.01 - **p<0.05 - *p<0.1; cross section weights.

3.A.3 Logit estimates of regional models

The use of interaction terms in nonlinear models bears the potential risk of biased results in both marginal effects and standard errors. Table 3.8 therefore replicates the regional specifications shown in in-text Table 3.3, using a standard logit approach with the dependent dummy variables being recoded as "very worried" (1) vs. all other outcomes (0). The use of a binary dependent measure allows for the application of the procedure suggested by Norton et al. (2004) for the calculation of corrected marginal effects and standard errors in nonlinear models.[5] When comparing the standard and the corrected marginal effects and standard errors, one finds that for our data both procedures lead to near identical results. Given the expected tolerance in between the more differentiated ordered logit estimates presented in in-text Table 3 and the pooled logit estimates shown above, all alternatives lead to very similar results. Thus it seems fair to assume that one can rely on the standard procedure for the calculation of marginal effects and standard errors in the ordered logit setup shown in Table 3.3.

Figure 3.2 visualizes the influence of the respondents' distance to the nearest active power plant on worries about the environment and on worries about the security of nuclear power plants. Both models are visualized because of detected regional influences an all graphs are based on the logit results shown in Table 3.8. In detail, Figures 3.2a, 3.2c, and 3.2e correspond to the main effects of the estimates reported in Table 3.8, columns 2 and 6. The corresponding interaction terms can be located as the difference of the slopes of the effect lines minus the slopes of the reference lines (Mitchell and Chen, 2005). Figures 3.2b, 3.2d, and 3.2f show the distribution of each corresponding interaction effect. To avoid possible biased estimates arising from the use of interaction terms in nonlinear models, the visualizations also take into account the corrected marginal effects and standard errors calculated according to the procedure suggested by Norton et al. (2004).

[5]According to Norton et al. (2004) the correct magnitude of an interaction term in binary logit and probit models should be calculated as the cross derivate of the dependent variable's expected value whereas the corresponding test for statistical significance should be based on the estimated cross-partial derivate rather than on the coefficient of the interaction term.

Table 3.8 Fukushima accident and nuclear power phase-out in Germany – distance to the nearest active power plant: logit estimates

	Worries environment		Worries energy supply		Worries power plant security	
	Standard Interaction Terms	Corrected Interaction Terms	Standard Interaction Terms	Corrected Interaction Terms	Standard Interaction Terms	Corrected Interaction Terms
Before Fukushima accident: 02/01/2011 – 03/10/2011	(Ref.)	(Ref.)	-	-	-	-
Fukushima accident:	0.0505***	0.0505***	(Ref.)^b	(Ref.)^b	(Ref.)^b	(Ref.)^b
03/11/2011 – 06/05/2011	(0.0115)	(0.0115)				
Nuclear power phase-out	-0.0098	-0.0098	-0.0135	-0.0135	-0.1065***	-0.1065***
06/06/2011 – 09/30/2011	(0.0137)	(0.0137)	(0.0120)	(0.0120)	(0.0167)	(0.0167)
Dist. to nearest	0.0003	0.0003	0.0003	0.0003	0.0002	0.0002
active reactor (weighted)	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0004)	(0.0004)
Fukushima accident *	-0.00042**	-0.00041**	-	-	-	-
Distance (weighted)	(0.00018)	(0.00018)	-	-	-	-
Nuclear power phase-out *	-0.00035*	-0.00035*	-0.00033*	-0.00035*	-0.00057*	-0.00055**
Distance (weighted)	(0.00020)	(0.00020)	(0.00019)	(0.00021)	(0.00026)	(0.00027)
Observations	17205	17205	4269	4269	4278	4278
Pseudo R2	0.0159	0.0159	0.0305	0.0305	0.0477	0.0477

Notes: (b) Reference Period: (04/01/2013 – 06/05/2013). Corrected marginal effects and standard errors of the interaction terms according to Norton et al. (2004). Dependent variables: Worries about environmental protection (0/1), worries about the reliability of energy supply without the use of nuclear (0/1), worries about the security of nuclear power plants (0/1); marginal effects: Probability of being "very worried"; robust standard errors in brackets; coefficients of the models, with error probabilities in parentheses.

Fig. 3.2 Regional models: logit estimates and correction in interaction terms

Fig. 3.2a: Fukushima accident: Predicted probability of being "very worried" about the environment

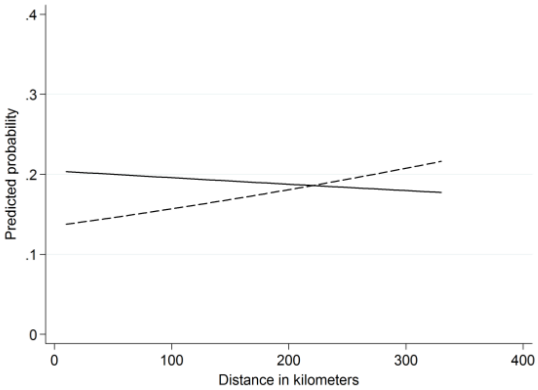


Fig. 3.2b: Interaction effect: Fukushima accident * distance – probability "very worried" about the environment

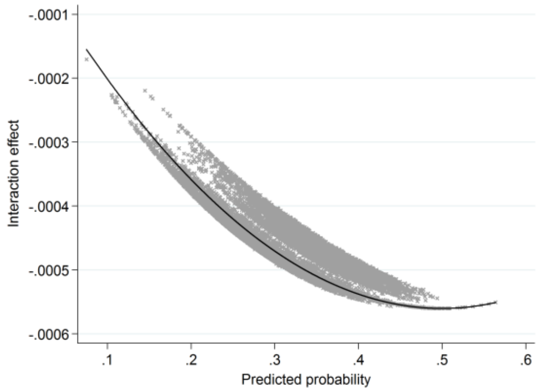


Fig. 3.2c: Phase-out: Predicted probability of being "very worried" about the environment

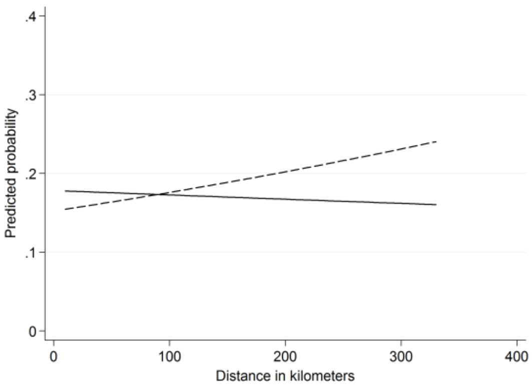


Fig. 3.2d: Interaction effect: Phase-out * distance – probability "very worried" about the environment

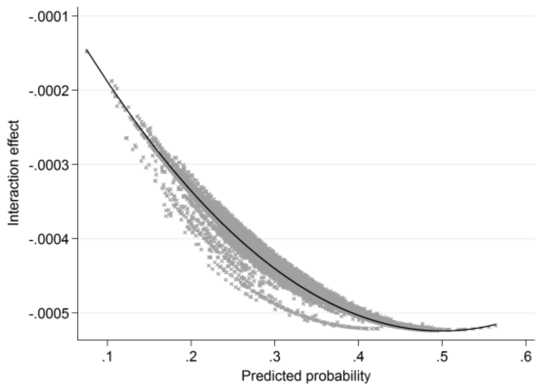


Fig. 3.2e: Phase-out: Predicted probability of being "very worried" about the security of nuclear plants

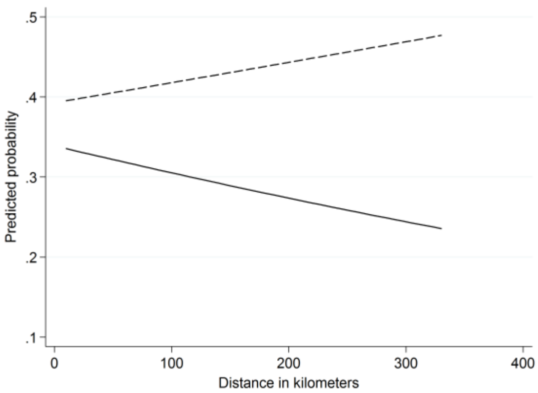
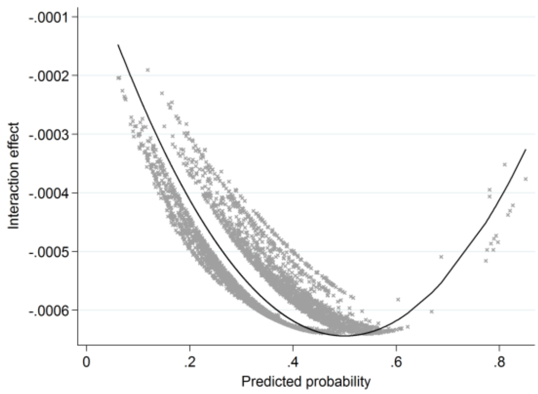


Fig. 3.2f: Interaction effect: Phase-out * distance – probability "very worried" about the security of nuclear plants



Notes: All Fig.: logit estimations (controlled for correct marginal effects in non-linear equations with interaction terms). Fig. 3.2a: Dashed line, reference period (before Fukushima accident) – solid line, effect period (Fukushima accident). Fig. 3.2c: Dashed line, reference period (before Fukushima accident) – solid line, effect period (nuclear phase-out). Fig: 3.2e: Dashed line, reference period (Fukushima accident) – solid line, effect period (nuclear phase-out). Fig: 3.2b, 3.2d, 3.2f: Solid line – standard marginal effects, crosses – correct marginal effects.

Chapter 4

Urban Renewal after the Berlin Wall - a place-based Policy Evaluation*

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*
JEL: *D62, H23, R21, R31*

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4.1 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 policy outcome can be compared. Truly experimental methodologies like randomized control trials, where randomly selected treated subjects can be followed over time and compared to similar non-treated subjects are not feasible in many fields of policy evaluation. Researchers have responded to this limitation by applying quasi-experimental research designs to ex-post outcomes of existing policies, which, 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 and 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 (*Städtebauforderung*), 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 (Bundesinstitut für Bau, Stadt- und Raumforschung, 2009).[2]

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.[3] RH investigate property prices in and around four renewal areas and one control area, which was initially considered but ultimately excluded from the program in Richmond, Virginia to detect housing externalities.[4] Their results indicate that housing externalities exist but diminish relatively steeply in distance, approximately 50% every 1,000 feet (Rossi-Hansberg et al., 2010, 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^{th} century history of the city. For several decades, the former capital

[2]Aggregate renewal financing data at the European level are not available.

[3]Kline and Moretti (2014b) provide an introduction into the welfare economics of place-based policies more generally along with a recent survey of the empirical literature.

[4]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.

of Germany suffered from either economic isolation (West Berlin) and loss of market access (Redding and Sturm, 2008) or transformation into a non-market economy (East-Berlin), both of which severely affected the economic health of the city. After reunification in 1990, the adverse economic performance was mirrored by a poor physical condition of the housing stock, 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.[5, 6] 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 renewal 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 and Maennig, 2010b; Autor et al., 2014; Ellen et al., 2001; Helms, 2012; Ioannides, 2002; Koster and Van Ommeren, 2013; Rossi-Hansberg et al., 2010; Schwartz et al., 2006), we contribute to a literature that has assessed the impact of various local public policies via capitalization effects (e.g. Ahlfeldt and Kavetsos, 2013; Cellini et al., 2010; Dachis et al., 2012; Dehring et al., 2008; Eriksen and Rosenthal, 2010; Gibbons and Machin, 2005; Oates, 1969; Santiago et al., 2001) and economic effects of spatially targeted policies more generally (e.g. Baum-Snow

[5] The first general urban renewal program of Berlin (*Erstes Gesamtberliner Stadterneuerungsprogramm*).

[6] 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.

and Marion, 2009; Boarnet and Bogart, 1996; Busso et al., 2013; Freedman, 2012, 2014; Freedman and Owens, 2011; Gobillon et al., 2012; Ham et al., 2011; Kline, 2010; Kline and Moretti, 2013, 2014a; Murray, 1999; Neumark and Kolko, 2010; Sinai and Waldfogel, 2005).[7]

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 et al., 1988; Gabriel and Rosenthal, 2004; Gyourko and Tracy, 1991; Tabuchi and Yoshida, 2000) or neighborhoods within cities (e.g. Brueckner et al., 1999; Carlino and Coulson, 2004; Cheshire and Sheppard, 1995; Ioannides, 2003).[8] This literature has argued that there has been a reorientation 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 and Saiz, 2008; Glaeser et al., 2001).

Our findings inform this literature on whether revitalization policies and other neighborhood polices such as historic preservation may contribute to the development of targeted neighborhoods and promote gentrification.[9] 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 reorientation 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 generally 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 4.2 introduces into the

[7]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 et al., 1994; Clark and Herrin, 1997; Coulson and Lahr, 2005; Coulson and Leichenko, 2004; Koster et al., 2012; Lazrak et al., 2010; Leichenko et al., 2001; Listokin et al., 1998; Noonan and Krupka, 2011; Schaeffer and Millerick, 1991).

[8]This study complements research examining the effects of spatial density on the productivity of workers and firms (e.g. Ahlfeldt et al., 2012; Ciccone, 2002; Ciccone and Hall, 1996; Glaeser et al., 1992; Glaeser and Mare, 2001; Rauch, 1993; Rosenthal and Strange, 2001).

[9]Alternative determinants include transport affordability (LeRoy and Sonstelie, 1983), housing cycles (Brueckner and Rosenthal, 2009), housing demand shocks (Guerrieri et al., 2013), or natural amenities (Lee and Lin, 2012).

institutional setting. Sections 4.3 and 4.4 present the empirical strategy and results. The final section summarizes our findings and concludes.

4.2 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 general urban renewal program of Berlin (*Erstes Gesamtberliner Stadterneuerungsprogramm*) 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, 2012).[10] 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 though tax reductions, loans, cash advances and further financial support. After 2002 the focus was set to improve-

[10]In Richmond, the object of the RH (2012) analysis, the four targeted areas had an average population of 1,900 residents and on average 1,000 housing units.

ments 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.[11]

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.).[12] 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.[13] This compares to per area payments of $3.5 mill. and per capita expenses of $1,800 in Richmond over a period of four years. Currently, 19 of the 22 considered renewal areas have been released from their renewal status; Figure 4.1 in the data section shows the geographic locations of the renewal and investigation areas in Berlin.[14]

4.3 Empirical Strategy

4.3.1 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 β_V parameters, where V indicates the number of years that have passed since designation. To estimate these

[11] 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).

[12] 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.

[13] The total investment amounts to about 35% of the housing stock value. See for further detail section 4.A.2 in the technical appendix.

[14] See Table 4.3 in the technical appendix for details on designation date, district, and expiration of the renewal areas. An overview of the area is shown is in Figure 4.1; a snapshot providing more detailed graphical information can be found in Figure 4.5 in the appendix.

parameters of interest, we estimate the following empirical specification:

$$\begin{aligned} logT_{it} = \alpha_1 T_i + f(T_i \times V_{it}) + \delta(T_i \times A_{it}) + \sum_k \gamma_k X_{kit} \\ + \sum_l \gamma_l L_{li} + \sum_t \sum_g (\gamma_{gt} G_i \times \varphi_t) + \sum_t \varphi_t + \sum_n \mu_n + \epsilon_{it}, \end{aligned} \tag{4.1}$$

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 the 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 γ_k and γ_l are the respective implicit prices. We control for otherwise unobserved time-invariant location characteristics via a fixed effects μ_n defined for 9,718 statistical blocks.[15] Standard errors ($\epsilon_i t$) are clustered at the same level and, thus, accommodate a spatial structure in a relatively flexible manner. Macroeconomic factors that are assumed to be invariant across the treatment and control groups are captured by year fixed effects φ_t.

In addition to controlling for year effects and time-invariant location characteristics, we further allow for time-variant implicit prices γ_{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.

[15] 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).

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 × 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 and 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 in-variant are considered (Caliendo and Kopeinig, 2008). These covariates include a range of internal property and external location characteristics and are discussed in greater detail in the technical appendix, where we also present some descriptive statistics for the resulting sample.

Treatment functions

To capture the time-varying treatment effects β_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:

$$f(T_i \times V_{it}) = \beta_0 T_{it} \times POST_{it} + \beta_1 T_{it} \times V_{it}, \tag{4.2}$$

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 and 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., $VD_{1it} = 1$ 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:

$$f(T_i \times V_{it}) = \sum_V \beta_v (T_{it} \times VD_{Vit}) \tag{4.3}$$

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.

4.3.2 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.

4.3.3 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 (2nd 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.[16] 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 4.6 in the 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.[17]

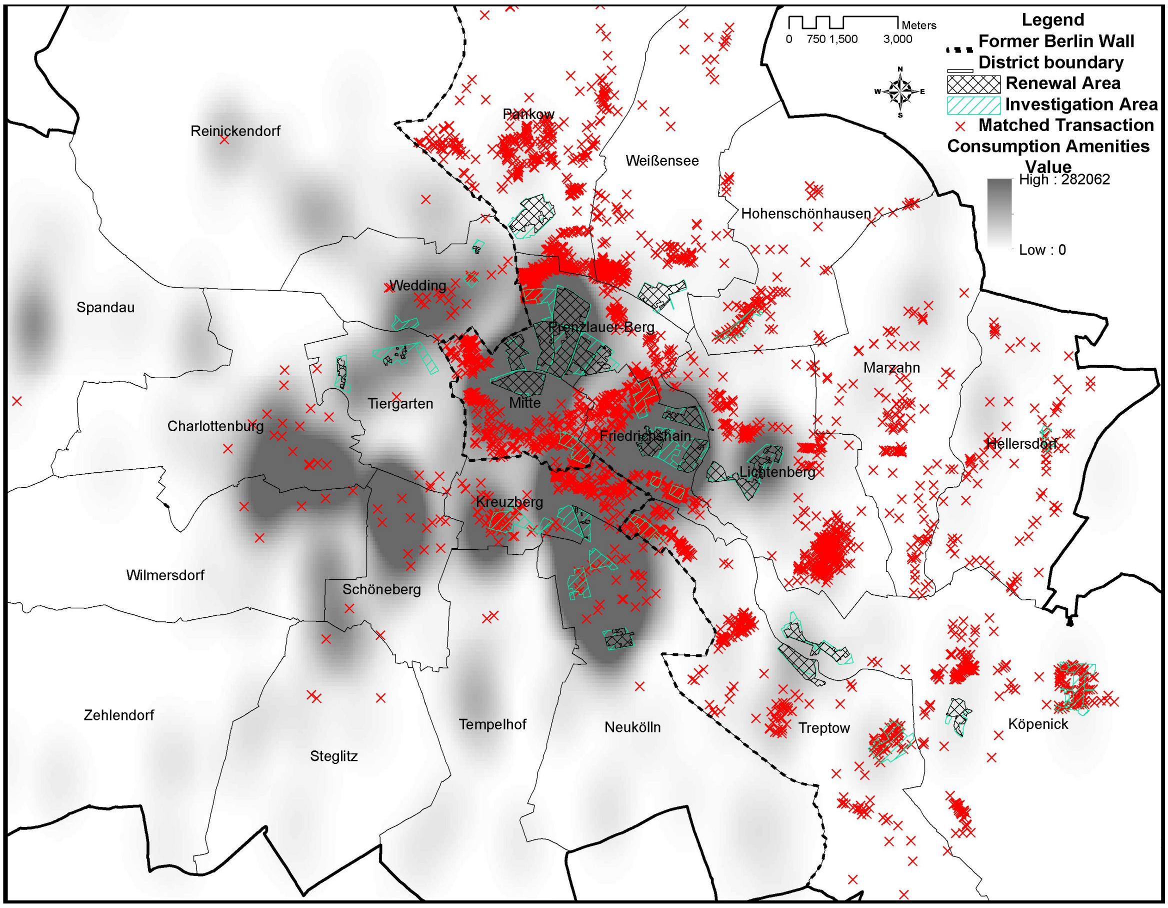
Figure 4.1 shows of the spatial distribution of the renewal / investigation areas along with our estimated smoothed kernel density surface and our synthetic control group (control group IV). Renewal areas and revitalization areas are typically located in central areas and in amenity clusters in the eastern 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.

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

[16] www.openstreetmap.org

[17] Table 4.3 in the appendix lists the renewal areas and some stylized facts per area, while Table 4.4 compares key characteristics across the renewal areas, the investigation areas, and the rest of Berlin.

Fig. 4.1 Study area

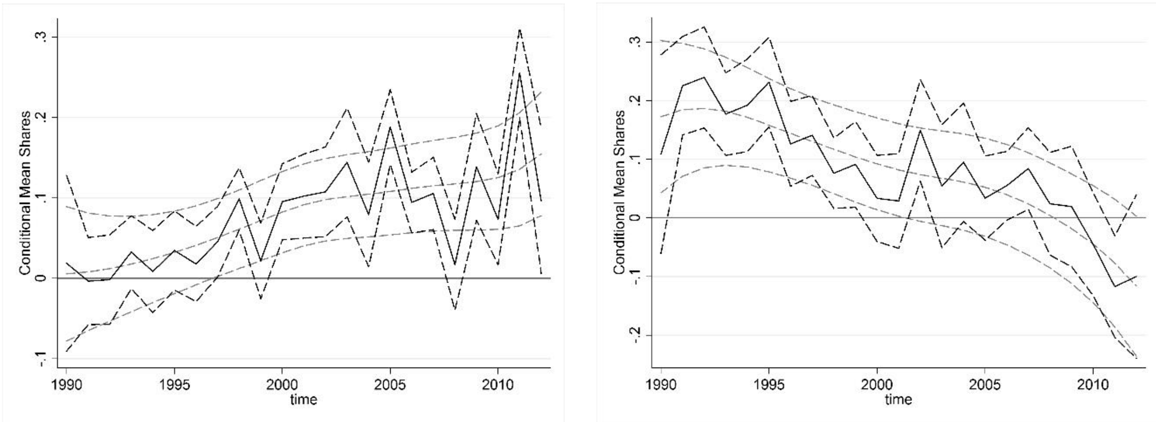


Notes: Own illustration based on the urban and environmental information system (Senatsverwaltung für Stadtentwicklung Berlin, 2006). Crosshatched (hatched) areas indicate renewal (investigation) areas. Crosses are the matched transactions in control group IV. Smoothly shaded areas represent the consumption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

of developed land that takes place. In Figure 4.2, 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 4.2 demonstrates this development and indicates that the renewal program accelerated the upgrading of the housing stock that was left behind during the division period.

Fig. 4.2 Maintenance trends in renewal areas

Share of buildings in "Condition good" *Share of buildings in "Condition bad"*



Notes: Year specific differences in mean shares are estimated in two separate regressions of the following type: $Y_{it} = \sum \lambda_t T_i \times \varphi_t + \sum X_{ot} + \varphi_t + \epsilon_{it}$, 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 λ_t point estimates (95% confidence intervals). Grey dashed lines are lowess smoothes of the parameters.

4.4 Empirical results

4.4.1 Baseline specification

Table 4.1 summarizes our parametric estimates (see equation 4.1) 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 4.7 in the appendix. The parameter on T × POST (β_0) indicates a shift in log prices at the time of designation, while the parameter on T × V (β_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 $(\exp(\beta_0 + \beta_1 V_{it}) - 1)$.[18]

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 $(1 + 119.4\%)$ ˆ $(1/20) - 1 = 4.01\%$. 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.

Figure 4.3 illustrates our semi-non-parametric estimates of the temporal treatment function according to equation 4.3. 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 4.1. The cumulative effect on all properties inside renewal areas relative to those out-side 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 (beginning after approximately 5 years). The negative level shifts found in Table 4.1, 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

[18] We make use of the conventional interpretation of dummy variables in semi-log models (Halvorsen and Palmquist, 1980).

Table 4.1 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)
T x POST	-0.162***	-0.115***	-0.060	-0.139***	-0.120***	-0.026
(within renewal)	(0.036)	(0.037)	(0.039)	(0.052)	(0.045)	(0.061)
T x V (years since	0.047***	0.039***	0.023***	0.025***	0.010***	0.005
designation)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Cum. effect after	119.4%***	94.49%***	49.79%***	42.7%***	7.33%	8.3%
20 years	(6.62%)	(6.84%)	(7.32%)	(9.79%)	(7.32%)	(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^2	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.

(5) in Table 4.1 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.

4.4.2 Sensitivity analysis

Table 4.2 summarizes the distributions of the cumulated treatment effects that are obtained from several series of applications of our parametric benchmark model (Table 4.1, 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 4.1, 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 4.4 displays some of the distributions summarized in Table 4.2. 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.

4.4.3 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

Table 4.2 Renewal area treatment effects

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

Notes: Each row describes the distribution of the cumulated effects after 20 years derived from a series of estimations of the benchmark specification (equations 4.1 and 4.2). 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.

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 III. Fourth, we replace the contemporary amenity density with an analogically constructed variant that uses bars and restaurants as reported in the 1995/96 edition of the yellow pages (Gelbe Seiten), which should predate the impact of the designation of renewal areas. 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.

4.5 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.

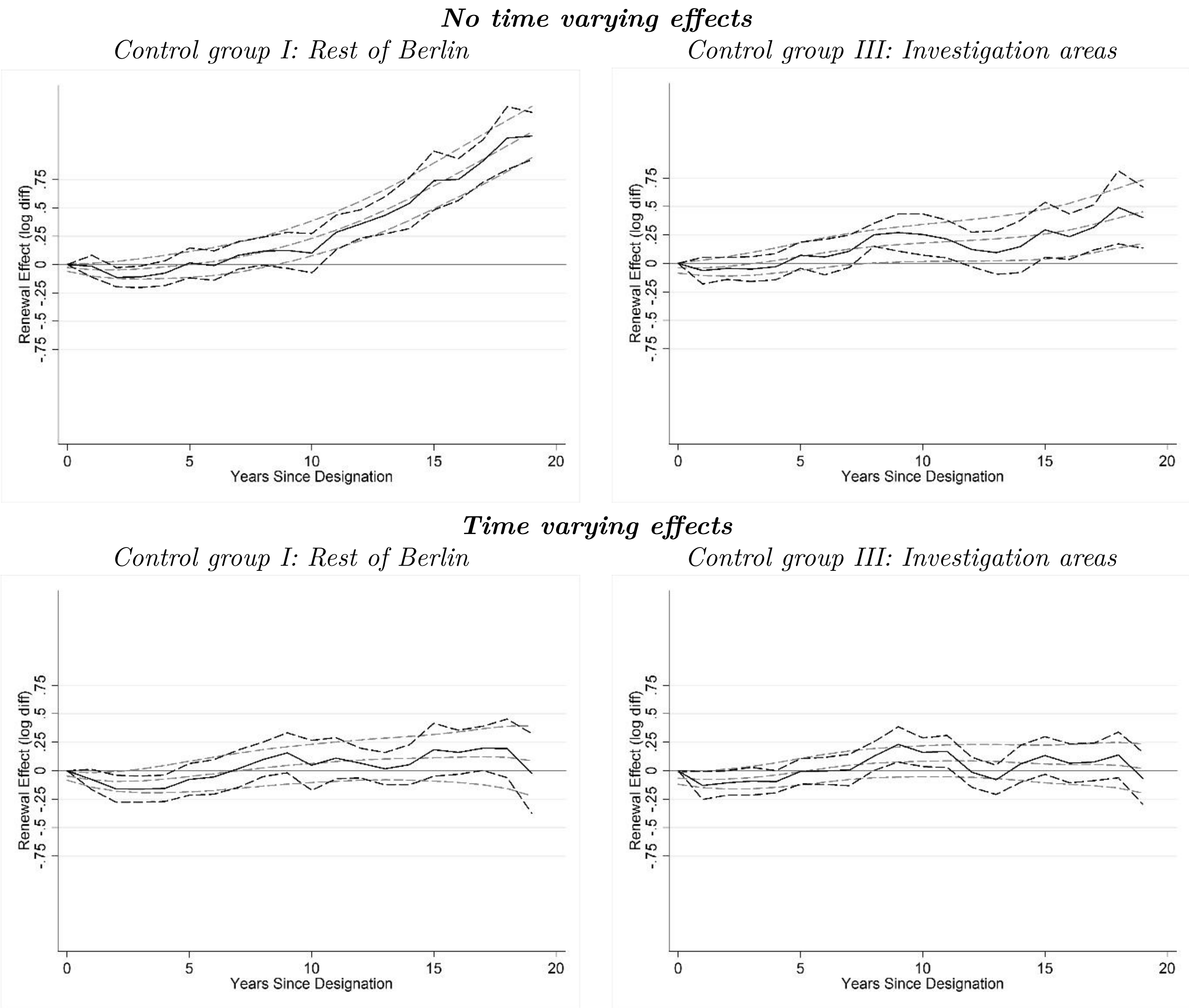
Our results, however, also suggest that the renewal effect is not causally related to the policy as of the appreciation is likely attributable to trends correlated with locational characteristics. The selected areas primarily locate in amenity-rich central areas in former East-Berlin. Controlling for 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 and Glaeser, 1999; Hilber, 2010) and tend to use neighborhood policies as a framework to coordinate their behavior to internalize externalities (Holman and 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.

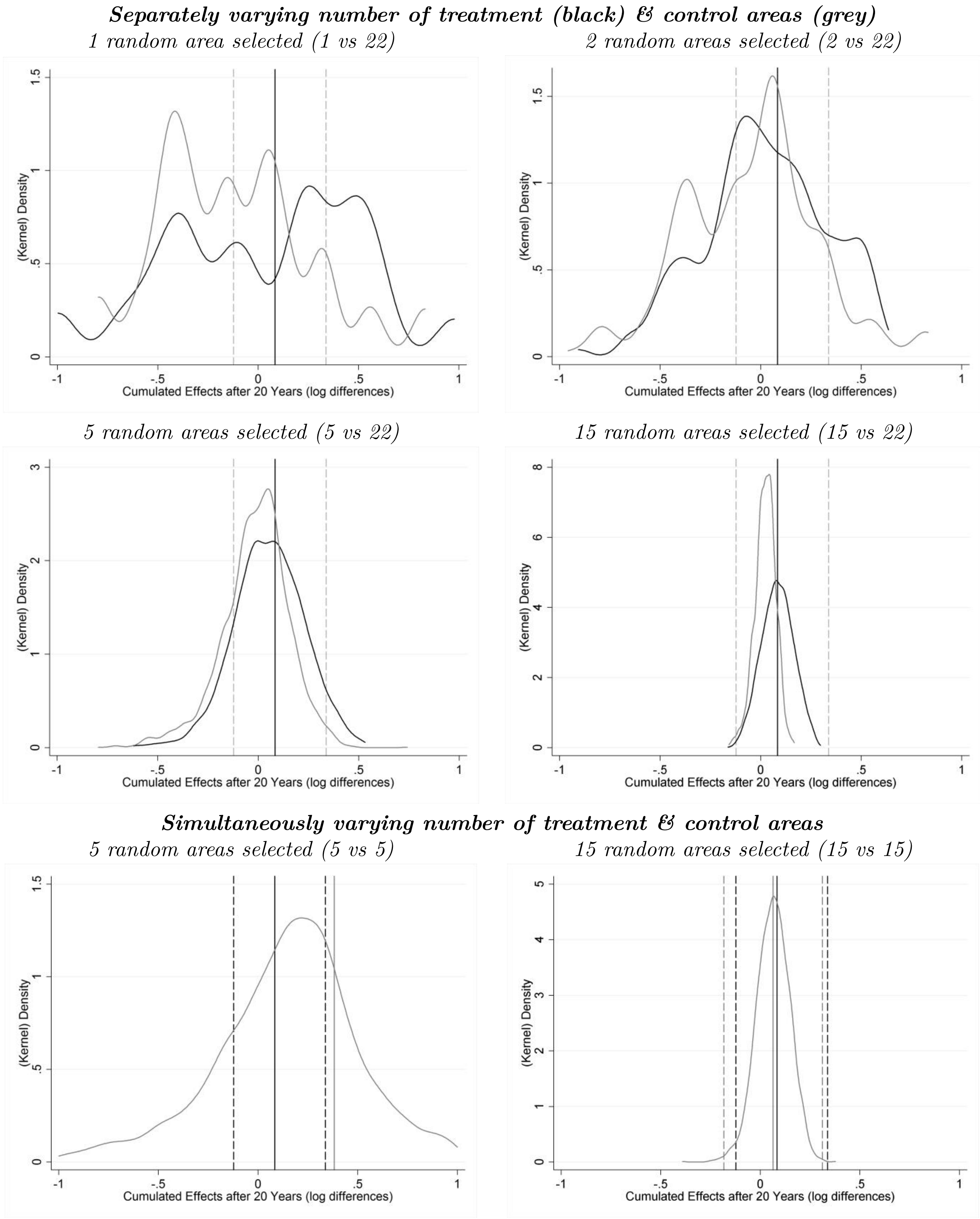
More generally, our results show critically that the outcome of place-based policy evaluations can depend on the empirical specifications used. In our case more credible control groups yield significantly lower treatment estimates than naive comparisons to nearby areas or even the rest of the city. Controlling 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.

Fig. 4.3 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 lowess smoothes of the parameters.

Fig. 4.4 Varying numbers of treated and control areas: Distribution of point estimates



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).

Appendix 4.A Technical Appendix

4.A.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 4.A.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 4.A.3 provides complementary evidence that extends the results in section 4.4 of the main paper. Finally, section 4.A.4 contains our analysis of potential externality and spillover effects.

4.A.2 Data

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

Berlin - stylized facts

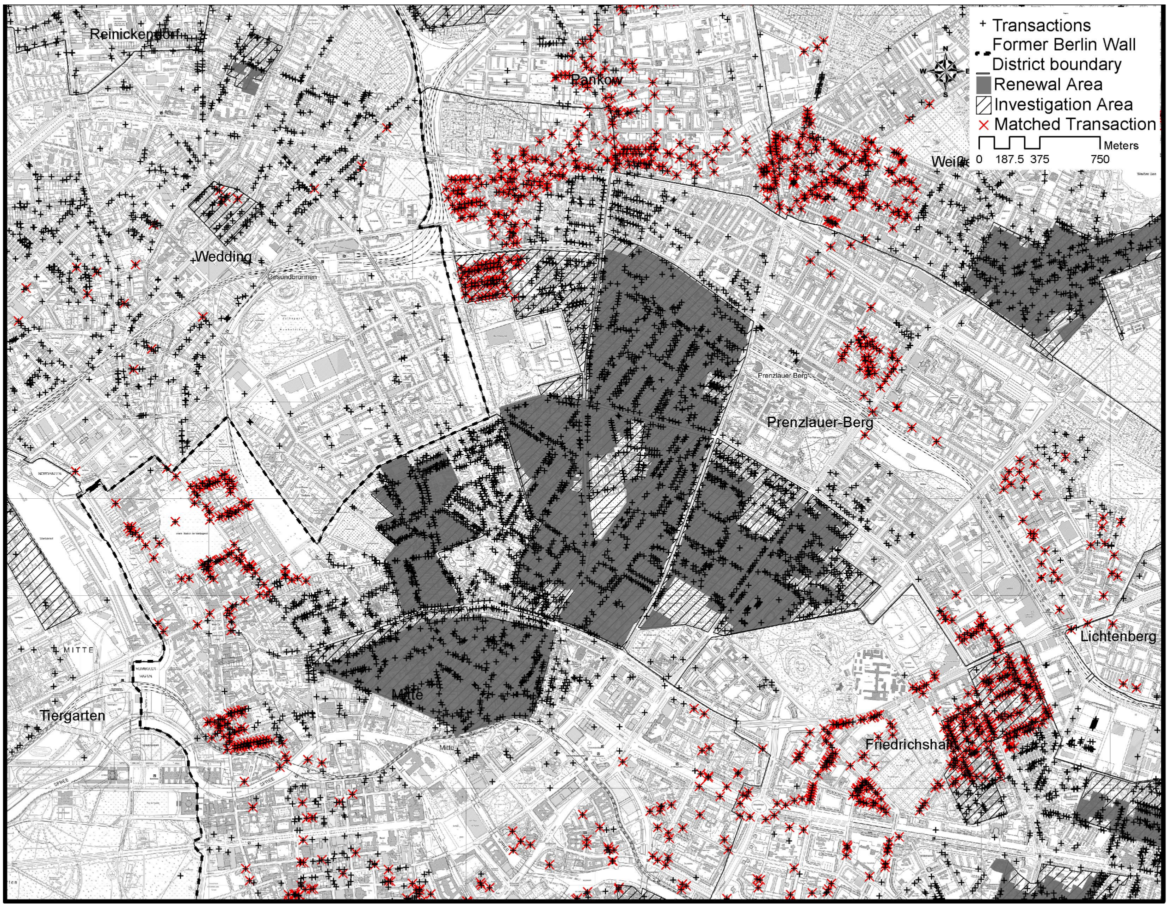
Our study area comprises the area of the Federal State of Berlin, Germany. The city in 2012 counted some 3.3 mill. inhabitants and approximately 1.9 mill. dwelling units. Approximately 14% of the population is non-German citizens. While there have recently been signs of economic recovery after a relatively 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.

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 4.1 in the main paper depicts the renewal and investigation areas. Figure 4.5 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 section 4.A.2.

In Table 4.3, 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.

Fig. 4.5 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).

Table 4.4 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

Table 4.3 Descriptive statistics renewal areas

Name	Start	End	Area (km2)	Properties	Dwelling units	Residents
Samariterviertel	09.10.1993	10.02.2008	0.339	263	5302	8324
Warschauer Strasse	04.12.1994	28.04.2011	0.381	227	5110	8599
Traveplatz Ostkreuz	04.12.1994	11.07.2010	0.351	204	4380	6964
Kaskelstrasse	04.12.1994	10.02.2008	0.221	248	1665	3394
Weitlingstrasse	04.12.1994	28.01.2009	0.503	331	4214	5337
Spandauer Vorstadt	09.10.1993	10.02.2008	0.671	632	5809	8771
Beusselstrasse	04.12.1994	21.02.2007	0.106	93	2314	3045
Rosenthaler Vorstadt	04.12.1994	28.01.2009	0.376	373	4809	6794
Stephankiez	10.11.1995	21.02.2007	0.063	54	1288	1860
Soldiner Strasse	10.11.1995	21.02.2007	0.019	11	447	661
Wederstrasse	10.11.1995	11.07.2010	0.246	233	1341	2079
Kottbusser Damm Ost	10.11.1995	21.02.2007	0.025	21	380	522
Kollwitzplatz	09.10.1993	28.01.2009	0.607	476	6519	11412
Helmholtzplatz	09.10.1993		0.819	560	13338	21211
Winsstrasse	04.12.1994	28.04.2011	0.348	219	4850	8568
Wollankstrasse	04.12.1994	28.04.2011	0.685	338	3386	7719
Teutoburger Platz	04.12.1994	12.02.2013	0.498	316	4432	7950
Komponistenviertel	04.12.1994	11.07.2010	0.339	477	3443	7400
Boetzowstrasse	10.11.1995	28.04.2011	0.381	191	3072	6211
Altstadt Kiez Vorstadt	09.10.1993	21.02.2007	0.351	225	1105	2115
Niederschöneweide	04.12.1994		0.221	97	799	1368
Oberschöneweide	10.11.1995	11.07.2010	0.503	255	3465	5375

Notes: The data for area, properties, dwelling units, and residents are from the Berlin administrative unit for urban development and environment from the year 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.

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 homogenous areas dominated by buildings constructed around the turn of the 19^{th} and 20^{th} centuries (the so called founding period / "Gründerzeit"). These are primarily apartment blocks, often with some commercial units on the ground floor.

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 a period ranging from 1998 to 2002, which is roughly the midpoint of the renewal program period. To construct a repeated sales index we estimate the

Table 4.4 Tab A2. Comparative statistics

	Renewal areas	Investigation areas	Rest of Berlin (without RENEWAL / INVEST)
Price (cpi adjusted)	€ 1,490,795.00	€ 1,382,921.00	€ 1,503,588.00
	(€ 3,290,749.00)	(€ 1,548,053.00)	(€ 5,667,000.00)
Age	101.5	96.0	60.3
	(22.8)	(23.6)	(36.5)
Floor space index	2.609	2.902	1.127
	(0.981)	(1.074)	(1.230)
Average plot size	1058	1003	1798
	(1834)	(1481)	(6515)
Share of foreigners	0.14	0.17	0.11
	(0.14)	(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.

following regression model:

$$P_{it} = \alpha + \sum_k \gamma_k X_{kit} + \sum_{t \neq \{1998,...,2002\}} \varphi_t + \theta_i + \epsilon_i t, \tag{4.4}$$

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 θ_i is a set of property fixed effects holding all time-invariant location effects constant. The time effects φ_t form the repeated sales price index, which we use to discount the 2012 mean property price in renewal areas to the base value (1998-2002). We then multiply the resulting property value of €927,908 by the total number of properties in the renewal areas (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.

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 and Rubin (1983) to find observations that are structurally similar to the transactions in the renewal areas. For the estimation of models 4 and 6 in Table 4.1 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 4.5 reports the average treatment effect on the treated (ATT) and several measures of the balance of the covariates for the control group IV.

Table 4.5 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	Unmatched	2.5171	3.7574	-61.4	
(1, poor to 6, very good)	Matched	2.7627	3.1517	-19.2	68.6
Typical floor space index	Unmatched	2.2635	1.0455	163.9	
	Matched	2.263	1.7998	62.3	62
Land use: residential	Unmatched	0.86687	0.84722	5.6	
	Matched	0.9545	0.90186	15	-167.9
Land use: commercial	Unmatched	0.02761	0.01855	6	
	Matched	0.02978	0.06051	-20.5	-239.1
Distance to CBD (m)	Unmatched	4705.4	9250.6	-120.3	
	Matched	4697	6698.9	-53	56
Distance to park (m)	Unmatched	2138.2	1695.4	39	
	Matched	2132.2	1801.8	29.1	25.4
Distance to main street (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 and Rubin (1985) and Leuven and 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.

4.A.3 Baseline models: complementary evidence

This section complements section 4.4 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 4.A.3 presents results for alternating combinations of the time varying effects, while section 4.A.3 considers an alternative way to account for the spatial autocorrelation of the standard errors in our model. Section 4.A.3 evaluates possible designation effects on the runner-up areas that remained unconsidered. In section 4.A.3, we replicate our benchmark results using an urban amenity density measure based on historic data.

Complete results

Table 4.6 provides descriptive statistics for all structural and locational variables. Table 4.7 extends Table 4.1 in the main paper by presenting the implicit hedonic prices of the structural characteristics.

Table 4.6 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^2)	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
Location within block				
Building at street front (%)	73.34			
Building at a corner (%)	13.98			
Building with multiple fronts (%)	3.89			
Hammer type building (%)	1.41			
Building in inner block loc. (%)	6.66			
Other (%)	4.61			

Notes: Own calculations based on the complete sample.

Table 4.7 Complete results

Model	(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)
T x POST	-0.162***	-0.115***	-0.060	-0.139***	-0.120***	-0.026
(renewal)	(0.036)	(0.037)	(0.039)	(0.052)	(0.045)	(0.061)
T x V (years	0.047***	0.039***	0.023***	0.025***	0.010***	0.005
since des.)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Building age	-0.014***	-0.012***	-0.012***	-0.011***	-0.012***	-0.010***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Building age,	0.000***	0.000***	0.000***	0.000***	0.000***	0.000**
squared	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Industrial	0.128*	-0.112	0.127	0.257	0.147	0.408*
area indicator	(0.072)	(0.106)	(0.172)	(0.221)	(0.174)	(0.243)
Residential	0.022	-0.084	0.019	0.035	0.043	0.163
area indicator	(0.048)	(0.079)	(0.120)	(0.147)	(0.114)	(0.159)
Commercial	0.356***	0.221**	0.256*	0.336*	0.219	0.427**
use indicator	(0.061)	(0.089)	(0.151)	(0.172)	(0.147)	(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	0.208***	0.236***	0.219**	0.322***	0.223**	0.311***
corner	(0.041)	(0.066)	(0.104)	(0.106)	(0.102)	(0.104)
Build. with	0.348***	0.117	0.168	0.073	0.194	0.090
mult. fronts	(0.065)	(0.102)	(0.156)	(0.162)	(0.152)	(0.160)
Hammer type	-0.107**	-0.206	-0.087	-0.446	-0.098	-0.445
Building	(0.043)	(0.133)	(0.258)	(0.336)	(0.326)	(0.310)
Build. in inner	-0.131***	-0.270***	-0.381***	-0.420***	-0.432***	-0.470***
block loc.	(0.042)	(0.085)	(0.146)	(0.147)	(0.145)	(0.147)
Observations	64,677	17,447	8,623	8,860	8,623	8,860
R^2	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 4.1 in the main paper. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Location controls ans time varying control are decribed in detail in the data section of the main paper.

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.

Time-varying effects

In our preferred models (Table 4.1, 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.[19]

To address this concern we replicate the baseline models using a number of less extensive combinations of time-varying effects. Table 4.8 displays the specification from Table 4.1 (column (5) and (6)) from the main paper using only the year × district effects (columns (1) and (2)), only the consumption amenity × 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 × 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 insensitivity to the inclusion of district × year effects when control group IV is used does not come as a surprise.

An important insight arises from model (3) in Table 4.8. 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

[19]This problem is a variant of the "bad control problem" (Angrist and Pischke, 2009).

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 4.8 Renewal area effects with distinct time-varying effects

Model	(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)
T × POST	-0.140***	-0.084	-0.032	-0.048	-0.096**	-0.055
(within renewal)	(0.044)	(0.056)	(0.038)	(0.053)	(0.041)	(0.054)
T × V (years since	0.012***	0.021***	0.020***	0.005	0.007*	0.001
designation)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)
Cum. effect after	10.19%	39.52%***	44.51%***	4.94%	4.25%	-3.24%
20 years	(7.5%)	(12.19%)	(6.99%)	(10.13%)	(7.13%)	(9.88%)
Av. appr. rate	0.49%	1.68%	1.86%	0.24%	0.21%	-0.16%
Observations	8623	8860	8623	8860	8623	8860
R2	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
Time-var. effects:						
District × year effects	YES	YES	NO	NO	NO	NO
Cons. amenities × year effects	NO	NO	YES	YES	YES	YES
East Berlin × 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.

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 4.9 displays the point estimates from the OLS regression as in Table 4.1 (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.[20] We conclude that the treatment effects are not only economically small, but should also be viewed as statistically not distinguishable from zero.

Table 4.9 Robustness of the renewal area effects with SHAC standard errors

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Point estim.	Standard errors Clust. OLS	SHAC	SHAC	SHAC	SHAC	SHAC	SHAC
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							
R2	0.677							

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. SAN × POST, SAN × 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. SHAC 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).

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

[20]For US Census data, distance cutoffs are often set at approximately 10 miles (Boarnet et al., 2005; Jeanty et al., 2010).

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 4.A.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 4.10, we report the results of two models that are analogous to (4) and (6) in Table 4.1 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 4.1 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 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.

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

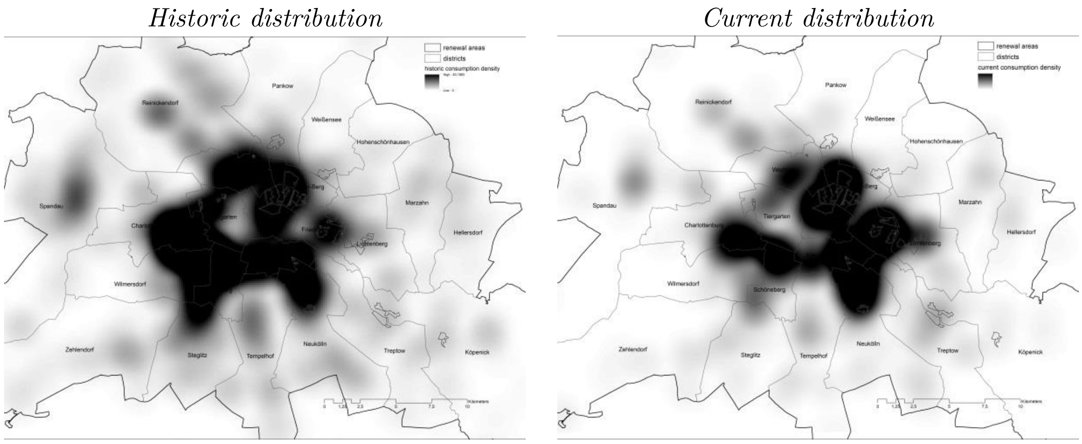
Table 4.10 Placebo designation effects on investigation areas: Renewal and investigation areas vs. matched control group

	(1)		(2)	
Control group	Matched observations (IV)			
Treatment renewal areas:				
$T^{REN} \times POST^{REN}$ (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%	
Investigation areas:				
$T^{INV} \times POST^{INV}$ (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^2	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.

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 4.6 compares the resulting kernel smoothed density surface (left panel) with the existing density surface displayed in Figure 4.1 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.

Fig. 4.6 Kernel smoothed density surfaces comparison



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).

Table 4.11 replicates our primary results using the consumption amenity density depicted in the left panel of Figure 4.6. 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.

4.A.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

Table 4.11 Renewal area effects with historic amenities

	(1)	(2)
Control group	Investigation areas (III)	Matched observations (IV)
T × POST	-0.113**	-0.015
(within renewal)	(0.044)	(0.062)
T × V (years	0.009***	0.005
since designation)	(0.004)	(0.005)
Cum. effect after	7.97%	9.19%
20 years	(7.15%)	(10.73%)
Av. appr. rate	0.38%	0.44%
Observations	8,623	8,860
R^2	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.

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.

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:

$$P = f(I(S), E(I(S)), L, Y). \tag{4.5}$$

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:

$$E(D) = \int_0^1 I(D)d(D) = 1. \tag{4.6}$$

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

$$dP = \left(\frac{\partial P}{\partial I(S)} + \frac{\partial P}{\partial E(S)}\right) dI(S) + \frac{\partial P}{\partial L} dL + \frac{\partial P}{\partial Y} dY, \tag{4.7}$$

or:

$$dP = \left(\frac{\partial P}{\partial I} + \frac{\partial P}{\partial E}\right) \frac{\partial I}{\partial S} dS + \frac{\partial P}{\partial L} dL + \frac{\partial P}{\partial Y} dY. \tag{4.8}$$

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 Δ) and at different locations (second difference d). We assume that a change in policy ΔS 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 ($\Delta Y^T = \Delta Y^C$).[21]

[21] 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.

Our treatment effect can be described as follows:

$$\beta = (P(S=1)^{POST} - P((S=0)^{PRE})^{T} - (P(S=0)^{POST} - P(S=0)^{PRE})^{C}, \quad (4.9)$$

or:

$$\beta = \Delta P^{T} - \Delta P^{C}. \quad (4.10)$$

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

$$\beta = \left(\left(\frac{\partial P}{\partial I} + \frac{\partial P}{\partial E}\right)\frac{\partial I}{\partial S}\Delta S + \frac{\partial P}{\partial Y}\Delta Y\right)^{T} - \beta = \left(\left(\frac{\partial P}{\partial I} + \frac{\partial P}{\partial E}\right)\frac{\partial I}{\partial S}\Delta S + \frac{\partial P}{\partial Y}\Delta Y\right)^{C}, \quad (4.11)$$

or:

$$\beta = \left(\frac{\partial P}{\partial I} + \frac{\partial P}{\partial E}\right)\frac{\partial I}{\partial S}, \text{ where } \Delta \begin{cases} 1, & \text{if treated} \\ 0, & \text{if control.} \end{cases} \quad (4.12)$$

There are important implications for our empirical strategy that aims to estimate β. 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 $(\beta = (\partial P/\partial E)(\partial I/\partial S))$. 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.

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:

$$\beta_V = \frac{\partial P}{\partial I}\frac{\partial I_{iV}}{\partial S_{iV}} + \frac{\partial P}{\partial E}\sum_j \frac{\partial I_{jV}}{\partial S_{jV}}\tau(D_i j), \text{ where } \tau(D) > 0 \text{ and } \tau'(D) < 0. \tag{4.13}$$

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 $\beta = (\partial P/\partial E)\sum_j \partial I_j/\partial S_j \tau(D_i j)$. 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:

$$\beta_V = \frac{\partial P}{\partial I}\frac{\partial I_{iV}}{\partial S_{iV}} + \frac{\partial P}{\partial E}\sum_j \frac{\partial I_{jV}}{\partial S_{jV}}\tau(D_i j) + \sum_q \frac{\partial P}{\partial Q_q}\frac{\partial Q_{jQ}}{\partial S_{jQ}}. \tag{4.14}$$

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. Irrespectively 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:

$$\beta_V = \frac{\partial P}{\partial E} \sum_j \frac{\partial I_{jV}}{\partial S_{jV}} \tau(D_i j), \text{ where } \tau(D) > 0 \text{ and } \tau'(D) < 0. \tag{4.15}$$

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^2 (Senatsverwaltung für Stadtentwicklung Berlin, 2012), which are strikingly low figures compared to the above mentioned investment volumes.[22] 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.

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

To detect spillovers, we alter the definition of the treatment T measure and the control groups relative to the benchmark specification (see equation 4.1 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 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 $T_{S2i} = \beta_S T_{S1i} + \beta_{S2} DISTS_i$, 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 section 4.A.2 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 4.12 reports the average treatment effect on the treated (ATT) and several measures of the balance of the covariates for the matched control group.

Empirical Results

Building quality

Table 4.13 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 4.1 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 4.1 in the main paper), the results are at least indicative that the benchmark results are not primarily driven by externality effects.

Table 4.12 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	Unmatched	2.7652	3.8524	-51.8	
(1, poor to 5, very good)	Matched	2.9681	3.4993	-25.3	51.1
Typical floor space index	Unmatched	2.1696	0.9391	149.7	
	Matched	2.1696	2.0385	16	89.3
Residential area indicator	Unmatched	0.8112	0.8462	-9.3	
	Matched	0.8971	0.8568	10.7	-15.3
Commercial area indicator	Unmatched	0.0393	0.0219	10.1	
	Matched	0.0430	0.0674	-14.2	-40.2
Distance to CBD	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 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.

Spillover effects

Table 4.14 and Figure 4.7 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

Table 4.13 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)
T × POST (within renewal)	0.008 (0.338)	-0.070 (0.416)	0.084 (1.063)	0.500 (1.232)
T × V (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% (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^2	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.

indistinguishable from zero once we control for independent appreciation trends by means of time-varying effects.

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 $POST \times DIST$ 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 $YSD \times DIST$, 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

Table 4.14 Renewal area spillover effects

Model	(1)	(2)	(3)	(4)	(5)	(6)
Control group	All (A-I)	< 2 km renewal area buffer (A-II)	Invest. areas + 1 km buffer (A-III)	Matched obser- vations (A-IV)	Invest. areas + 1 km buffer (A-III)	Matched obser- vations (A-IV)
T × POST	-0.124**	-0.145**	-0.149**	-0.095*	-0.052	-0.057
(within renewal)	(0.052)	(0.059)	(0.061)	(0.056)	(0.056)	(0.067)
T × V (years	0.020***	0.023***	0.025***	0.024***	-0.000	0.000
since designation)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Cum. effect after	31.6%***	38.25%***	42.9%***	46.46%***	-4.41%	-4.67%
20 years	(6.06%)	(6.54%)	(6.68%)	(7.06%)	(5.63%)	(8.41%)
Av. appr. rate	1.38%	1.63%	1.8%	1.93%	-0.23%	-0.24%
Observations	66,865	19,421	11,963	16,989	11,963	16,989
R^2	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
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

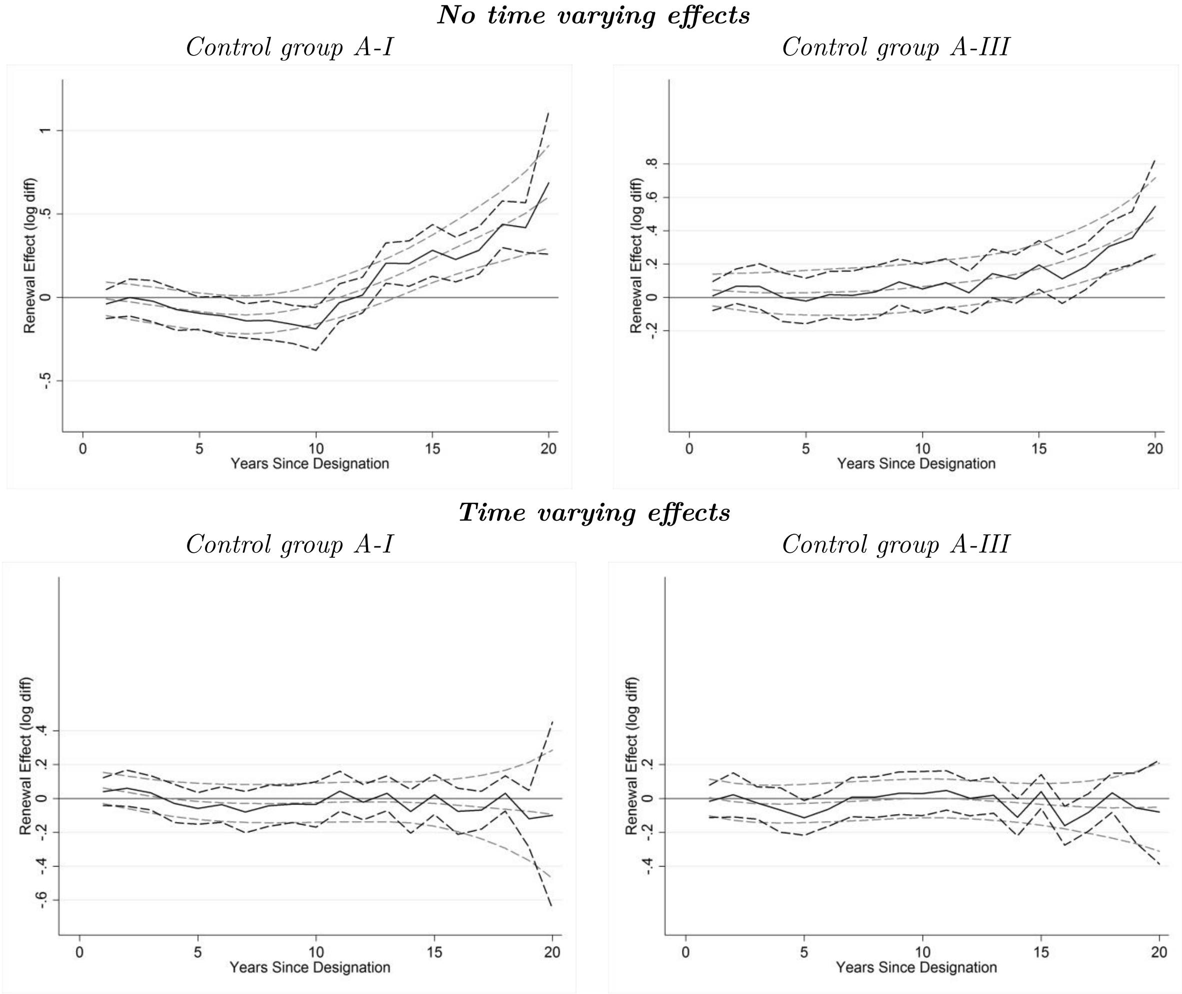
Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Hedonic and location controls consist of covariates controlling for internal property and external location characteristics described in greater detail in the data sections of the main paper and the appendix. Time-varying 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.

from the sample. The results for both specifications are presented in Table 4.15. As all relevant coefficients are insignificant, we conclude that that is no significant change in the spatial trend, neither directly after designation, not gradually emerging over the years.

Spillover effects – building quality and historic amenity density

For completeness, we also replicate the estimation of spillover effects including the building quality controls (Table 4.16) and the spillover effects including the historic amenity densities introduced in section 4.A.3 of the appendix (Table 4.17). 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

Fig. 4.7 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 lowess smoothes of the parameters.

in poor condition. Table 4.17 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 4.15 Spatio-temporal trends in spillovers

	(1)		(2)	
	log(price)		log(price)	
POST × DIST	0.053	(0.149)	-0.104	(0.199)
YSD × DIST			0.015	(0.014)
YSD			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	
R2	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.

Table 4.16 Spillover effects including quality controls

Model	(1)	(2)	(3)	(4)	(5)	(6)
Control group	All (A-I)	< 2 km renewal area buffer (A-II)	Invest. areas + 1 km buffer (A-III)	Matched obser- vations (A-IV)	Invest. areas + 1 km buffer (A-III)	Matched obser- vations (A-IV)
T × POST	-0.133***	-0.125**	-0.113**	-0.104*	-0.041	-0.094
(within renewal)	(0.050)	(0.056)	(0.056)	(0.054)	(0.049)	(0.066)
T × V (years	0.017***	0.019***	0.019***	0.019***	-0.002	0.003
since designation)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
Condition: good	0.307***	0.567***	0.598***	0.500***	0.571***	0.487***
	(0.015)	(0.036)	(0.036)	(0.034)	(0.034)	(0.031)
Condition: bad	-0.283***	-0.263***	-0.256***	-0.290***	-0.257***	-0.279***
	(0.013)	(0.021)	(0.021)	(0.024)	(0.021)	(0.023)
Observations	77,564	26,131	22,847	29,842	22,847	29,842
R^2	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.

Table 4.17 Spillover effects with historic amenities

	(1)	(2)
Control group	Invest. areas + 1 km buffer (A-III)	Matched observations (A-IV)
T × POST	-0.042	-0.034
(within renewal)	(0.050)	(0.064)
T × V (years	0.002	0.005
since designation)	(0.004)	(0.005)
Cum. effect after	-0.21%	6.76%
20 years	(5.07%)	(8.24%)
Av. appr. rate	-0.01%	-0.33%
Observations	11,963	16,989
R^2	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: 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.

Chapter 5

Winner Picking in Urban Revitalization Policies - Empirical Evidence from Berlin*

Abstract: This study addresses the question whether policy makers strategically pick winners when selecting the targets for place-based revitalization policies. It evaluates the influence of long-term trends of the unemployment rate and the share of residents of immigrant background on the probability of being selected as a target area, conditional on the current levels of these attributes. The empirical evidence is in line with the expectations: policy makers base their choice to some extent on the future performance expected of the areas. While high current levels of the unemployment rate increase the probability of being designated, an increase in the six year change of the unemployment rate, i.e. a negative development, decreases the probability of being selected designated. This effect is interpreted as winner picking: local authorities do not simply choose the areas which have the greatest need for revitalization, but instead prefer areas which show first signs of a gentrification process.

Keywords: *Winner picking, place-based policy, evaluation, urban, revitalization*
JEL: *R11, R21, R28, R58*

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*ERSA (European Regional Science Association) conference paper 2014

5.1 Introduction

Urban revitalization programs are widely used but understudied policy instruments designed to prevent urban decline. Recently, interest has increased in evaluating these pro-grams and providing in-depth analyses of potential costs and benefits. This study adds to the existing literature by evaluating the selection process that led to the designation of five large urban revitalization areas in Berlin, Germany in early 2010. In particular, the study addresses the question whether the choice of potential revitalization areas is influenced by a winner picking strategy to make the policy seem more successful. Policy makers might favor areas that have exhibited a gentrification process or a positive development perspective, and might prosper even in the absence of the policy in question. The study evaluates the influence of long-term trends in two key attributes (the unemployment rate and the share of residents of immigrant background) on the probability of being selected as a target area of the revitalization policy, conditional on the current levels of these at-tributes and a comprehensive set of control variables. Previewing the results, there is evidence that policy makers indeed seem to base their choices to some extent on the future performance expected of the areas. While there is no measurable effect related to the percentage of residents of immigrant background, policy makers seem to choose the target areas from a pool of areas characterized by high current levels of unemployment. From this pool, however, they prefer areas which have displayed a positive development in the past years. This effect is interpreted as winner picking, as the local authorities do not simply choose the areas which have the greatest need for revitalization, but prefer areas which show first signs of a revitalization or gentrification process.

The remainder of this introduction provides some background information and briefly surveys the related literature. Section 5.2 provides some facts about the data and the empirical strategy, section 5.3 presents the results. The final section summarizes the findings and provides a conclusion.

5.1.1 Background

After the German reunification in 1990, the hotspots in terms of socio-demographic development and building stock were located mostly in the eastern part of Berlin (Senatsverwaltung für Stadtentwicklung Berlin, 1992). The local authorities responded with a series of placed-based policies including different programs designed to target socio-demographically or economically disadvantaged neighborhoods and a broadly designed urban renewal program, the *first general Berlin urban renewal program*.[2] Until 2002, more than 40,000 dwelling units in the respective areas had been modernized (Senatsverwaltung für Stadtentwicklung Berlin, 2005). By then, the focus of the local administration had changed slightly: The less favorable financial situation of the federal state of Berlin and a less severe need for additional urban living space resulted in renovations no longer being subsidized directly. Urban planners shifted their focus to improving the social and cultural infrastructure and the quality of the overall living environment in disadvantaged areas all over Berlin, including the western part.

The present study evaluates the selection process of a place based policy consisting of five large target areas designated in early 2010 called *action areas plus* (Aktionsräume Plus), which comprise about 25% of the residents and roughly 10% of the area of Berlin (Senatsverwaltung für Stadtentwicklung Berlin, 2013b).[3] These areas are designed to concentrate the focus of public policy intervention in the areas of urban renewal, neighborhood management, and the support of disadvantaged urban spaces. The objectives of the policy are broadly formulated and include improvements of the overall living quality, improvements of education chances for the residents, and the avoidance – or the attenuation of the consequences – of urban decline (Senatsverwaltung für Stadtentwicklung Berlin, 2013a).

The official selection process is based on data collected in the context of the *Monitoring Social City Development* (Monitoring soziale Stadtentwicklung), which comprises socio-demographic attributes for Berlin on various geographic aggregation levels since 1998 (Senatsverwaltung für Stadtentwicklung Berlin, 2011).[4] Based on the reports from 2007/2008, local authorities identified five broad areas, which were particularly affected by socio-demographic disadvantages including unemployment, dependence on social benefits, social segregation, and a non-favorable housing stock condition. Moreover, these areas were claimed to exhibit a strongly negative perspective for the future (Senatsverwaltung für Stadtentwicklung Berlin, 2013a). The data survey also includes a *development index*, which is designed to comprise both past and future perspectives of each statistical area. The explicit construction of this index is documented in Senatsverwaltung für Stadtentwicklung Berlin (2011), however, it seems to take only short-term developments (the current year and the year before) into account. Also, as Berlin is a city with a complex structure of subsidies

[2] Erstes Gesamtberliner Staderneuerungsprogramm

[3] Throughout the paper, the *action areas plus* will be referred to as target areas.

[4] Data are available for the years 1999, 2000, 2002, 2004, and on a yearly base since 2006. The Section on data provides more details on the data and the geographic aggregation levels.

and place-based policies, there are several other programs in Berlin targeting disadvantaged areas, some of them were installed as early as in 1999. However, the *action areas plus* are specifically designed to strategically concentrate the focus of various policy instruments.

While the empirical strategy is explained in detail in section 5.2, the selection of the key attribute deserves some background information. Both attributes were selected based on the assumption, that they credibly mirror the general socio-demographic condition of an area. The unemployment rate is a straightforward indicator for the economic performance of an area. To understand the significance of the share of residents of immigrant background as a performance indicator, it is important to look at the dynamics of migration in Germany and Berlin: Many migrants came to Germany in the 1950 and 1960s when Germany needed additional workforce mainly for simple industry tasks (Kapphan, 2000). A large part of these migrants located themselves in disadvantaged urban areas. They were, on average, less educated and much poorer than the German average. These dynamics have turned out to be quite persistent. To this day, residents of immigrant background are poorer, less educated, and depend to a greater extent on welfare benefits, which explains why the share of residents of immigrant background can be regarded as a valid proxy for the socio-economic condition of an area (Bundesregierung, 2010; Gesemann, 2006).

5.1.2 Literature

The evaluation of urban revitalization policies is a relatively new field of research and the literature is thus not yet very developed. Existing studies mostly focus on identifying housing externalities in urban renewal policies, i.e. separating direct price effects from spillover effects on adjacent properties. A contribution looking into residential externalities is Rossi-Hansberg et al. (2010) who analyze a $14 mill. urban renewal program in Richmond, Virginia, consisting of four renewal areas. They compare housing prices in the selected areas to a runner up area that was considered beforehand but ultimately excluded from the program and find evidence for positive housing externalities, which decrease relatively fast with increasing distance. Compared to the control neighborhood, they find that properties in the targeted areas generate a yearly price premium of 2 to 5%.

Ahlfeldt et al. (2013a) challenge these findings. They evaluate a set of urban renewal areas designated in Berlin, Germany, in the aftermath of the German reunification between 1993 and 1995. Using a broader quasi-experimental research design they track housing prices in Berlin over 20 years and compare transactions in the renewal areas to various control groups including runner-up areas and transactions similar to those in the renewal areas based on matching techniques. They find that the housing stock condition in the targeted areas improved compared to similar areas, and that transactions in the renewal areas realize a yearly price premium compared to properties not targeted by the policy. In contrast to Rossi-Hansberg et al. (2010), however, they find no convincing evidence for housing externalities once appropriate control groups are in place. Furthermore, they find that the efficiency of place-based policy evaluations depends heavily on the number and quality of available control groups.

There is a broader strand in the literature analyzing the external effects of consumption amenities in cities, relying on influential work by Brueckner et al. (1999) identifying the crucial role of urban amenities for spatial sorting in cities and based on spatial equilibria dating back to Roback (1982).[5] As location specific advantages should in theory be completely reflected by property prices, various studies analyze the effects on property prices for example of train connection realignments (Ahlfeldt, 2011; Gibbons and Machin, 2005), the building of new sports stadiums (Ahlfeldt and Kavetsos, 2013; Ahlfeldt and Maennig, 2010a), neighborhood characteristics (Ioannides, 2003), places of worship (Brandt et al., ress), and place-based subsidized housing (Schwartz et al., 2006).

A related field of research evaluating policy measures is the literature analyzing the economic effects of historic preservation and conservation areas. One strand evaluates the effects of listed buildings or historic preservation areas on property prices in the US (Asabere et al., 1994; Clark and Herrin, 1997; Coulson and Lahr, 2005; Coulson and Leichenko, 2001, 2004; Koster and Van Ommeren, 2013; Leichenko et al., 2001; Noonan and Krupka, 2011), while a number of other studies focus on the developments in Europe (Ahlfeldt and Maennig, 2010b; Ahlfeldt et al., 2013b; Koster et al., 2012; Lazrak et al., 2010). Most of the studies find a positive net impact of the policies, i.e. the positive internal or external effects on property prices outweigh the negative effects including the restricted property rights. Noonan and Krupka (2011) also consider winner picking – or making – in their evaluation of historic preservation policies.

As indicated earlier, gentrification, i.e. the upgrading of formerly poor neighborhoods in terms of better economic performance and an increased share of wealthier

[5]See Ahlfeldt et al. (2012) or Lucas and Rossi-Hansberg (2002) for current more flexible versions of spatial equilibrium models of cities.

and better educated residents is closely intermingled with the evaluation of place-based policies, as both phenomena often have a similar desired effect; a neighborhood that is overall better off. As gentrification related effects have to be separated from effects attributed to the policy, it is important to review the various factors that can lead to or predate gentrification. While extensively discussed in the geography and social sciences literature, the economic literature on this field is less developed. Brueckner and Rosenthal (2009) identify the age of the housing stock as one of the main factors that lead to spatial sorting in U.S. metropolitan statistical areas, delivering new insights on the patterns of gentrification. Guerrieri et al. (2013) link gentrification to an increased housing demand in cities and find that gentrified areas grow incrementally at the borders, a process they refer to as endogenous gentrification. McKinnish et al. (2010) evaluate the population in- and outflows of various gentrifying census tracts in the US between 1990 and 2000. Leung and Tsang (2012) show that people dislike income inequality in their neighborhood, and that this effect can explain parts of the spatial sorting that is found in many cities.

5.2 Empirical Strategy

This section introduces into the empirical strategy. After providing some insights into the data sources and presenting some descriptive evidence in the first subsection the second subsection describes the identification strategy and the empirical specification.

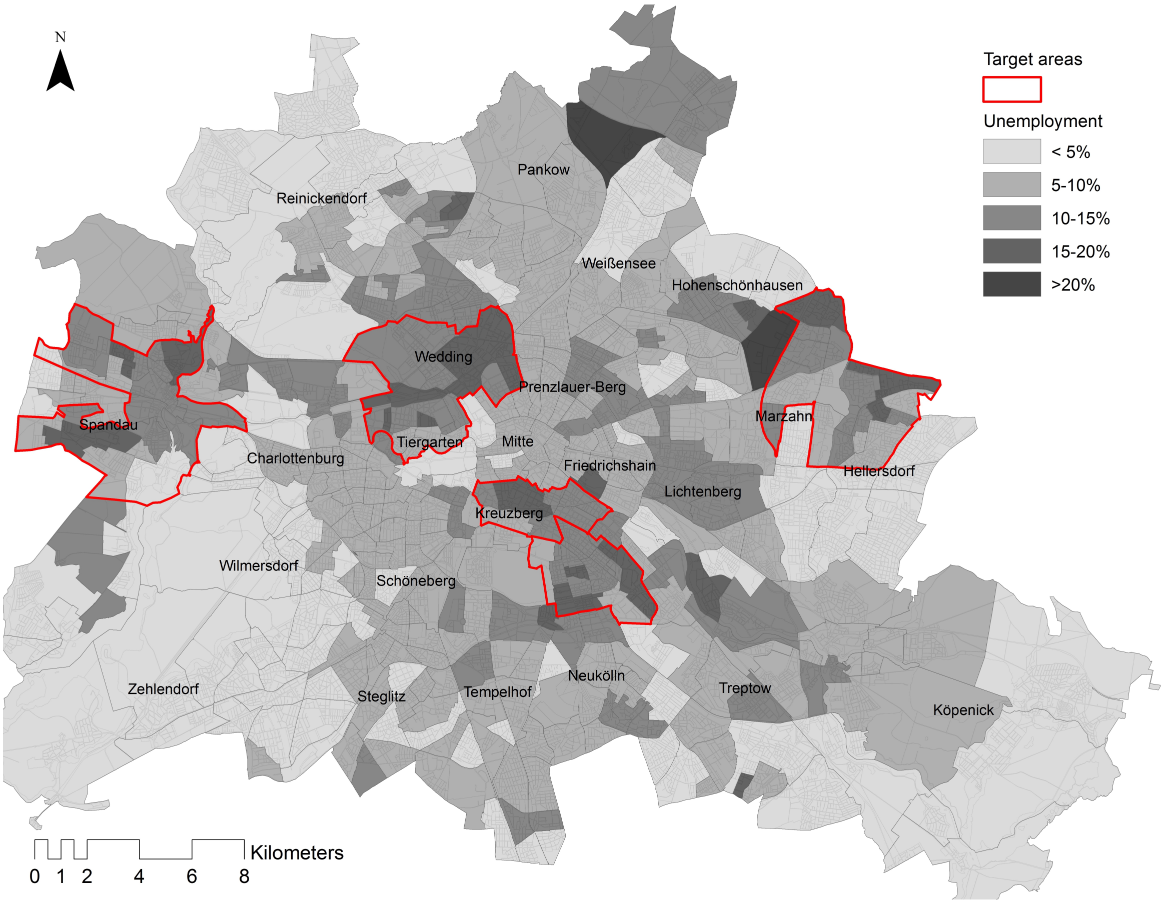
5.2.1 Data

Since 2006, Berlin is statistically divided into 447 *planning areas* (Planungsräume). These are statistical areas designed to comprise in practice separate urban living centers, taking into account building and social structure evolved over time (Senatsverwaltung für Stadtentwicklung Berlin, 2013a). Each has an average area of about two square kilometers and on average 8250 residents. The *Monitoring Social City Development* (Monitoring soziale Stadtentwicklung) provides continuous data on several socio-demographic indicators for the years 2006-2010. The data include the unemployment rate, the share of people of immigrant background, the share of foreigners from the European Union (EU 15), the overall migration volume, and the number of residents on planning area level.[6] Figure 5.1 provides an overview over Berlin, the

[6]The members of the European Union 15 (EU 15) are Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxemburg, Netherlands, Portugal, Spain, Sweden, and the

unemployment rate on planning area level in 2010, and the geographical location of the target areas.

Fig. 5.1 Target areas and unemployment rate (2010) in Berlin



Notes: Own illustration based on planning area level unemployment data (2010) and target area locations.

To construct a reliable long-term trend, the sample includes data on the (available) key attributes (the unemployment rate and the share of residents of immigrant background) lagged six years. The data are obtained from earlier (and not continuously collected) periods of the Monitoring Social City Development. Since the data were compiled on traffic cell / statistical area level (two former geographic administrative units) they were disaggregated on planning area level. The sample includes average values for the few cases where it was not possible to unambiguously relate the different administrative levels to one another. Moreover, no data were available for the years

UK.

2001 and 2003, so these values are calculated as the average between the circumjacent years respectively.

The sample also includes information describing the housing stock: Provided by the *Committee of Valuation Experts* (Gutachterausschuss) Berlin, the data comprises information on all property transactions in Berlin for the observed time period. The data includes a variety of attributes including plotarea, floorspace, typical area usage, indicators for the location of the building in the block, the condition of the building, and the year constructed. The transaction level data is aggregated yearly on planning area level. Obviously, averaged data of the transacted properties does not necessarily reflect the overall average of the housing stock, but it should provide a reasonable approximation.

Finally, the sample includes some time invariant location control variables including an east / west indicator, the distance to the nearest main street, school, playground, river or lake, and to the nearest public transport rail station, and a proxy for the level of consumption amenities on planning area level. This proxy consists of a kernel density surface based on the 2012 location of bars, pubs, nightclubs, hotels, and restaurants. The author employs a kernel radius of 2000m and a quadratic kernel function (Silverman, 1986), and the resulting kernel density surface is aggregated on planning area level and normalized between 0 and 1.[7] All observations included in the analysis and the locations of the target areas have been geocoded within a GIS framework.

Table 5.1 provides some descriptive evidence for the balanced sample of the 274 planning areas which were observed for all years between 2007-2010.[8] The sub samples exhibit the expected properties: attributes which are generally associated with disadvantaged areas as the unemployment rate, the share of people with immigration background, and the overall migration volume are higher in the target areas. Plot area and floor space are on average larger in the target areas, and the buildings in these areas are older and in a slightly worse condition.

[7] The data for the kernel density is from the open street map project (www.openstreetmap.org), and consists of user generated content. While there might be deviations from the actual distribution of consumption amenities, there is no need to fear that these deviations are structural.

[8] The sample comprises 59 of the 99 planning areas that were selected into one of the five target areas, and includes observations for all of these five areas. The excluded areas comprise all areas, which had less than 100 residents at some point in time (not reported by local authorities due to data protection) but also areas with missing observations for some of the years.

Table 5.1 Descriptive statistics on planning area level

	Target areas		Rest of Berlin	
Unemployment %	12.61	(3.449)	8.171	(3.846)
Immigration %	63.59	(21.03)	38.87	(21.38)
EU residents %	3.219	(2.078)	2.45	(2.054)
Migration volume %	33.34	(8.124)	26.76	(9.158)
Residents	9995.3	(5631.8)	8546.2	(4950.8)
Plotarea (m2)	1520.4	(1853.9)	1373.5	(1972.9)
Floorspace (m2)	2312.2	(2049.2)	1691.6	(2301.4)
Residential area %	90.8	(20.37)	90.82	(21.88)
Building facing the street %	0.775	(0.231)	0.715	(0.232)
Year constructed	1929.1	(29.92)	1942.3	(27.87)
Bad condition %	0.106	(0.172)	0.104	(0.185)

Notes: The displayed data are from 2010. Standard deviations in parentheses.

5.2.2 Empirical Specification

This subsection discusses policy makers' incentives and the identification strategy before illustrating the empirical specification. This study considers broadly speaking two different arguments which might influence the policy makers' selection decision: first, they might actually aim at choosing areas which have the greatest need for subsidies to attenuate the negative perspective of the disadvantaged areas. Second, they might favor areas which they expect to perform over proportionally well, to make the policy seem more successful.[9] The different departments of the local authorities are expected to face some kind of internal competition concerning the distribution of future funding. If this distribution depends to some extent on the success of former policies, such winner-picking behavior might be rational.

The identification strategy in this study rests on the construction of reliable long-term trends for two key attributes, the unemployment rate and the share of residents of immigrant background. Long-term changes in these two attributes are expected to mirror the overall performance of an area: a gentrification process is assumed to be mirrored by a decline of the unemployment rate and the share of residents of immigrant background over time. Along the same lines, an increase in the two key attributes might indicate a further downturn of the respective area. This setup enables us to test two competing hypotheses: Do policy makers target areas in decline, which actually need subsidies, or do they target potential winners, which are already

[9] To be precise, there might be further arguments not discussed in this study. E.g., policy makers might choose areas, where the impact of the subsidy relative to the investment is maximized.

gentrifying?

The empirical strategy aims at estimating the effects of these long-term changes in key attributes, while holding the current levels of these attributes constant. The rationale is that policy makers would ex ante only consider areas, which have relatively high levels in the unemployment rate and the share of residents of immigrant background. From this pool, they might however, favor areas that have undergone a positive development or some kind of gentrification process in the past years.[10] The change over six years in the unemployment rate ($UNEMP$) and the share of residents with immigration background (IMM) are computed as $UNEMP_{it} - UNEMP_{it-6} = \Delta UNEMP_{it}$ and $IMM_{it} - IMM_{it-6} = \Delta IMM_{it}$ respectively. The full specification of the employed linear probability model can be written as

$$\begin{aligned} T_{it} = \alpha_1 UNEMP_{it} + \alpha_2 IMM_{it} + \beta_1 \Delta UNEMP_{it} + \beta_2 \Delta IMM_{it} \\ + \vartheta_1 SOCIO_{it} + \vartheta_2 HOUSING_{it} + \sum_t \varphi_t + \sum_i \mu_i + \epsilon_{it}, \end{aligned} \tag{5.1}$$

where T_{it} is an indicator variable measuring the selection of the planning area i into a target area. It takes the value $T = 1$ if area i is a designated target area at time t and zero else. $SOCIO$ is a vector containing the socio-demographic attributes described in the data section and $HOUSING$ contains the building stock specific attributes. The specification also includes a set of year fixed effects φ_t and a set of area specific fixed effects μ_i defined for the planning areas. The standard errors ϵ_{it} are clustered on the planning area level as well.[11]

Following Conley (1999), the study also provides an alternative way to account for potential spatial autocorrelation in the error terms by calculating a spatial version of the non-parametric heteroskedasticity–autocorrelation consistent (HAC) standard errors adapted for panel data as in Hsiang (2010). The covariance matrix estimator calculates weighted averages of spatial autocovariances. The employed weights are

[10] An alternative (and rather implausible) explanation would be that policy makers unintentionally choose the wrong areas, because they base their decision on a short time horizon only. Actually, the development indicator mentioned in the introduction seems to only take into account the past and the current year.

[11] A further complication is the clustering of planning areas into the larger revitalization areas. Each of the five target areas consists of up to 28 connected planning areas. Urban planners most certainly avoid selecting various independent small target areas spread all over the city, but aim at obtaining a few broader connected target areas. To reach this goal, they possibly have to include some areas which do not perfectly fit their search criteria, and exclude some isolated areas which fulfill the criteria.

calculated using Bartlett kernels decreasing linearly in the two geographical dimensions and are set to zero once a predefined cutoff point has been reached (Conley, 1999; Deschênes and Greenstone, 2007).

Second, the study assesses the impact of the long-term trends on change in designation status (in 2010) of the planning areas in 2009 using a logit approach. The specification can be summarized as

$$ln\left(\frac{P}{1-P}\right) = \alpha_1 UNEMP_{it} + \alpha_2 IMM_{it} + \beta_1 \Delta UNEMP_{it} + \beta_2 \Delta IMM_{it} \\ + \vartheta_1 SOCIO_{it} + \vartheta_2 HOUSING + \vartheta_3 LOC_i + \epsilon_{it}, \tag{5.2}$$

where P is the probability that area i is designated as a target area in 2010 conditional on the given covariates. Additional to the covariates described in equation 5.1, the vector LOC contains the (time invariant) distance controls described in the data section. For this specification the sample is restricted to the year 2009 and the year effects and the area fixed effects are omitted from the model.

5.3 Empirical Results

This section summarizes the empirical results presented in Table 5.2. Columns (1-4) display the effects of the linear probability model with the point estimates in column (1), clustered standard errors in column (2), and the spatially corrected standard errors in columns (3-4). Columns (5-6) display the marginal effects and the robust standard errors from the logit estimation.

The coefficients of the covariates generally display the expected effects, although most of them do not significantly influence the probability of being selected as a target area: a larger share of buildings in bad condition and a larger share of buildings in residential areas increase the probability of being selected as a target area, as do a larger share of EU (15) residents and a higher migration volume. A modern housing stock and larger average floor space decreases the probability of being selected.

The main results are displayed in the first four rows: as expected, high levels of the unemployment rate increase the probability of selection into a target area significantly. A 10 percentage point increase in the unemployment rate increases the probability of selection by about 0.7 percentage points. However, the long-term trends reveal a different story: A 10 percentage points increase in the unemployment rate over

Table 5.2 Empirical results

	(1)	(2)	(3)	(4)	(5)	(6)
Model	OLS				LOGIT	
Standard errors		Clustered	Spatial HAC	Spatial HAC		Robust
Distance cutoff		-	2km	5km		-
Unemployment	0.0707	(0.0241)***	(0.0315)**	(0.0468)	0.0411	(0.00579)***
Immigration	-0.0108	(0.0123)	(0.0102)	(0.0099)	0.00522	(0.00457)
Δ Unemployment	-0.0552	(0.0139)***	(0.0162)***	(0.0156)***	-0.0332	(0.00743)***
Δ Immigration	-0.00753	(0.0117)	(0.00884)	(0.00749)	-0.00899	(0.00449)**
EU residents	0.0469	(0.0332)	(0.0357)	(0.029)	0.0111	(0.0137)
Migration Volume	0.00728	(0.00473)	(0.00349)**	(0.00393)*	0.0000636	(0.00381)
Residents	-0.0323	(0.0178)*	(0.0152)**	(0.0177)*	0.0024	(0.00255)
Plotarea	0.00908	(0.0655)	(0.05)	(0.0591)	0.188	(0.109)*
Floorspace	-0.0225	(0.0779)	(0.0595)	(0.0607)	-0.0071	(0.139)
Residential area	0.0266	(0.074)	(0.0539)	(0.0426)	0.0332	(0.0796)
Building facing the street	0.0341	(0.0396)	(0.0257)	(0.0222)	0.162	(0.0679)**
Year construct.	-0.000555	(0.000612)	(0.00042)	(0.000345)	-0.00103	(0.000831)
Bad condition	0.0427	(0.0595)	(0.0477)	(0.062)	0.0575	(0.0641)
Socio. controls		YES	YES	YES		YES
Housing controls		YES	YES	YES		YES
Location controls		NO	NO	NO		YES
Area fixed effects		YES	YES	YES		NO
Year effects		YES	YES	YES		NO
R^2	0.417				-	
AIC	-710.5				148.6	
Observations	1096				274	

Notes: Standard errors in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01. The results for the logit estimates (column 5) are marginal effects reported at means. To eliminate some left hand zeros, residents are measured in units of 1000, and plot area and floor space are measured in ha. Year effects are yearly fixed effects. Housing and distance controls consist of covariates controlling for property and location characteristics described in greater detail in the data section. Area effects consist of a set of the observed planning area level fixed effects.

six years, i.e. a negative development, actually decreases the probability of being designated significantly by about 0.55 percentage points. The level and trend effects of the share of residents of immigrant background cannot be estimated precisely and do not seem to have an impact on the selection probability. The effects seem small at a first glance, but they neglect the small average selection probability. In relation to the average selection probability in 2010, a 10 percentage points increase in the six year change of the unemployment rate decreases the designation probability by about 2.5%.

The results are robust to the inclusion of standard errors accounting for spatial autocorrelation and serial correlation in an alternative way: the presented SHAC standard errors (columns (3) and (4)) are estimated with a distance cutoff of two and five kilometers respectively, which tends to increase the standard errors of the level estimates but not of the trends estimates. Comparable results are also displayed by the logit estimates (columns (5) and (6)). A 10 percentage points increase in the unemployment rate increases the selection probability by about 0.4 percentage points, while an increase in the six year change in unemployment decreases the probability of selection by about 0.33 percentage points.

As a sensitivity analysis, the study replicates the main results, including either only the unemployment rate, or only the share of residents of immigrant background (and the lagged values respectively). The results are displayed in Table 5.3: the effect seems to be largely drive by the unemployment rate. While the parameters in columns (1) and (2) are similar to the effects in Table 5.2, including only the share of residents of immigrant back-ground yields only inconsistent and very small effects.

5.4 Conclusion

This study evaluates the selection process of a place-based policy leading to the designation of five urban revitalization areas, specifically addressing the question whether urban planners display winner picking. The empirical results support this hypothesis: while high levels of unemployment increase the probability of being selected as a target area, increases in the six year change of the unemployment rate, i.e. a negative development, decrease the probability of being selected. Neither the levels nor the long-term changes in the share of residents of immigrant background have a significant effect on the selection process. The results are stable across different specifications.

The effects can be interpreted as winner picking: high levels of unemployment increase the selection probability. This indicates that well-functioning areas are generally not selected (selecting these areas for a revitalization policy would be implausible). However, the negative effects of the six year change in the unemployment rate indicate that areas which exhibit negative perspectives, i.e. an increase in the long-term trend of the unemployment rate, have a decreasing chance of being subsidized. Instead, urban planners seem to prefer areas which demonstrated a positive development or a gentrification process. These results contrast the declared objectives of the policy, which was specifically designed to target disadvantaged areas expected to underperform in the future.

Table 5.3 Sensitivity: drivers of the effects

	Unemployment		Immigrant background	
	(1) OLS FE	(2) LOGIT	(3) OLS FE	(4) LOGIT
Unemployment	0.0558**	0.0383***	-	-
	(0.0256)	(0.00596)		
Δ Unemployment	-0.0467***	-0.0360***	-	-
	(0.0158)	(0.00636)		
Immigration	-	-	-0.0170*	0.0157***
			(0.00902)	(0.0045)
Δ Immigration	-	-	0.00177	-0.0126**
			(0.00768)	(0.00503)
EU residents	0.044	0.0154	0.0496	-0.0347**
	(0.0349)	(0.0135)	(0.0326)	(0.0165)
Migration volume	0.00653	0.00282	0.00821*	0.00313
	(0.00474)	(0.0025)	(0.00477)	(0.00321)
Residents	0.00442	0.00192	-0.0315**	0.00732**
	(0.00572)	(0.00263)	(0.0159)	(0.0031)
Plotarea	-0.000348	0.171	0.0184	0.109
	(0.0666)	(0.126)	(0.0623)	(0.13)
Floorspace	-0.0191	0.0409	-0.0383	-0.0486
	(0.08)	(-0.152)	(0.0765)	(0.19)
Residential area	0.0259	0.00282	0.0196	0.0504
	(0.0739)	(0.0813)	(0.0755)	(0.134)
Building facing	0.0308	0.145**	0.0437	0.149*
the street	(0.0403)	(0.0688)	(0.0396)	(0.0828)
Year constructed	-0.000326	-0.0011	-0.000306	-0.00188*
	(0.000616)	(0.000769)	(0.000619)	(0.000987)
Bad condition	0.0541	0.041	0.0293	-0.0603
	(0.0586)	(0.0641)	(0.0599)	(0.0885)
Socio-dem. Controls	YES	YES	YES	YES
Housing controls	YES	YES	YES	YES
Location controls	NO	YES	NO	YES
Area fixed effects	YES	NO	YES	NO
Year effects	YES	NO	YES	NO
R^2	0.402	-	0.399	-
AIC	-686.3	149.8	-680.1	188.7
Observations	1096	274	1096	274

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The results for the logit estimates (column 5) are marginal effects reported at means. To eliminate some left hand zeros, residents are measured in units of 1000, and plot area and floor space are measured in ha.

This study informs the economic policy evaluation literature on two important grounds. First, selection processes of placed-based policies might not be as quasi-exogenous (in the sense of being based on objective criteria) as one might expect. Urban planners might have an incentive to pick winners to make the policy seem more successful in retrospective. Other explanations for distorted selection results would include decisions based on in-complete data or potential corruption. Second, in the presence of winner picking, there are important implications for the evaluation of place-based policies. The selection process favors certain areas, which might prosper even in absence of the policy. Therefore, the construction of valid counterfactuals is especially important to effectively evaluate the effects of a place-based policy.

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SUMMARY:
Essays in Quantitative Spatial Economics

This thesis revolves around some central aspects of the empirical spatial economics literature, which studies the influence of space on economic relationships. The article in Chapter 2, titled "Exports and Olympic Games: Is There a Signal Effect?", deals with the potential effects of hosting the Olympic Games on countries' exports. In contrast to earlier contributions, the article shows that hosting or applying for the Olympic Games does not necessarily has a positive and lasting effect on countries' exports. Specifically, this *Olympic effect* vanishes, once the Olympic hosts are compared to appropriate control groups such as the OECD countries, and not to all remaining countries of the world.

The article in Chapter 3, titled "Nuclear Accidents and Policy: Notes on Public Perception", analyzes the effects of the nuclear accident in Fukushima in 2011 and the subsequent nuclear phase-out decision on the subjective perception in Germany. Subjective perception is captured through several independent items from the German Socio-Economic Panel (SOEP), including concerns about the environment and concerns about the reliability of nuclear energy. While the accident increases the probability to be worried about the environment, the phase-out decision decreases the worries about the security of nuclear energy. These effects are interrelated with the distance between the respondents' place of residence and the nearest nuclear facility.

In Chapter 4 the article titled "Urban Renewal after the Berlin Wall: a place-based Policy Evaluation" evaluates a $2.3 Bn. urban renewal program designed to promote the recovery of 22 neighborhoods in Berlin, Germany. Such programs have become established instruments to mitigate the negative effects of urban decline. The study employs a quasi-experimental research design by comparing housing prices in the target areas over 20 years to various control groups, including areas with similar preconditions which were ultimately not selected for the policy and structurally similar transactions based on propensity score matching.

The results show, that the policy was effective in increasing the housing stock quality in the target areas. Compared to similar areas not targeted by the policy, the share of building in bad condition decreased by 25% over the program period, and the value increased by over 50%. There is, however, no evidence that this is a causal effect. Also, there is no evidence for any external effects, which is astonishing given that such housing externalities are often used to justify the expenses for similar policies. Finally,

there is evidence that the evaluation of place-based policies is sensitive to unobserved local differences, especially when there are but a few treatment or control areas.

The article in Chapter 5, titled "Winner Picking in Urban Revitalization Policies - Empirical Evidence from Berlin", evaluates whether local authorities strategically pick winners when selecting the targets for urban revitalization policies. The chapter analyzes the selection process leading to the designation of five large urban revitalization areas in Berlin, Germany. The article estimates the influence of long-term trends in two key attributes – the unemployment rate and the share of residents of immigrant background – on the probability of being selected as a target area, while holding the current levels of these attributes constant. The results are as expected: local authorities, while choosing from a pool of areas with high levels of unemployment, prefer areas which show first signs of a recovery or a gentrification process. This effect is interpreted as winner picking.

In summary, the results of this thesis show that it is crucial to take spatial aspects into account when evaluating economic relationships, especially in an urban or regional context. It also becomes obvious, that conventional estimates might be biased in face of spatial dependence, and that quantitative spatial methods can help reducing this bias. In the light of the thematic broadness and the complexity of the quantitative spatial economics literature, these results help to explain the current dissemination of spatial methods into the applied economics literature.

KURZFASSUNG:

Essays in quantitativer räumlicher Ökonomie

Diese Dissertation diskutiert in vier Artikeln einige zentrale Aspekte der angewandten räumlichen Ökonomie, die den Einfluss von Raum auf ökonomische Zusammenhänge untersucht. Der Artikel in Kapitel 2 mit dem Titel "Exports and Olympic Games: Is There a Signal Effect?" diskutiert mögliche Effekte des Ausrichtens von olympischen Sommerspielen auf die Höhe der Exporte eines Landes. Im Gegensatz zu früheren Beiträgen zeigt er, dass das Ausrichten oder das Bewerben für olympische Spiele nicht notwendigerweise einen positiven und langfristigen Effekt auf die Exportmenge eines Landes hat. Dieser *olympische Effekt* verschwindet insbesondere dann, wenn man die ausrichtenden Länder nicht allen anderen Ländern gegenüber stellt, sondern eine angemessenere Kontrollgruppe wählt, wie etwa die OECD-Staaten.

Der Artikel in Kapitel 3 mit dem Titel "Nuclear Accidents and Policy: Notes on Public Perception" analysiert die Effekte des Nuklearunfalls in Fukushima in 2011 und der darauffolgenden Entscheidung über den Atomausstieg auf die individuelle Wahrnehmung in Deutschland. Die subjektive Wahrnehmung wird über verschiedene Items des deutschen sozio-ökonomischen Panels (SOEP) abgebildet, darunter die Sorgen über die Umwelt und die Sorgen über die Sicherheit von Nuklearkraftwerken. Während der Unfall zu einem signifikanten Anstieg der Sorgen über die Umwelt führt, senkt die darauffolgende Entscheidung über den Atomausstieg die Sorgen bezüglich der Sicherheit von Atomkraftwerken. Diese Effekte sind zu einem gewissen Grad abhängig von der Entfernung, die zwischen dem Wohnort der befragten Person und dem nächstgelegenen Atomkraftwerk liegt.

In Kapitel 4 evaluiert der Artikel mit dem Titel "Urban Renewal after the Berlin Wall: a place-based Policy Evaluation" ein $2.3 Mrd. teures Stadterneuerungsprogramm, welches die Aufwertung von 22 Vierteln in Berlin, Deutschland, zum Ziel hat. Derartige Programme sind heutzutage etablierte Politikmaßnahmen, um den negativen Effekten von Stadtverfall entgegenzuwirken. Die Studie greift auf ein quasiexperimentelles Forschungsdesign zurück, indem es Immobilienpreise in den Zielgebieten über 20 Jahre mit der Entwicklung in verschiedenen Kontrollgruppen vergleicht. Diese Kontrollgruppen beinhalten Untersuchungsgebiete, die ursprünglich auch als Zielgebiet vorgesehen waren, sowie strukturell ähnliche Gebiete, die auf Propensity Score Matching basieren.

Die Ergebnisse zeigen, dass die Politikmaßnahme zu einer erhöhten Qualität des Immobilienbestands in den Zielgebieten geführt hat. Im Vergleich zu ähnlichen Ge-

bieten, die nicht Ziel der Politikmaßnahme waren, hat sich der Anteil an Gebäuden in schlechtem Zustand über den Zeitraum der Studie um 25% verringert, während sich der Wert um bis zu 50% gesteigert hat. Die Ergebnisse deuten aber auch darauf hin, dass es sich nicht um einen kausalen Zusammenhang handelt. Außerdem gibt es keine Hinweise für externe Effekte, was vor allem angesichts der Tatsache verwundert, dass positive Externalitäten oft als Begründung für die hohen Ausgaben derartiger Politikmaßnahmen herangezogen werden. Schließlich gibt es Hinweise, dass die Evaluierung von ortsgebundenen Politikmaßnahmen zu einem großen Teil von unbeobachteten lokalen Unterschieden abhängt, besonders wenn es nur wenige Ziel- oder Kontrollgebiete gibt.

Der Artikel in Kapitel 5 mit dem Titel "Winner Picking in Urban Revitalization Policies – Empirical Evidence from Berlin" untersucht, ob lokale Verwaltungen strategisch potenzielle Gewinner bevorzugen, wenn sie die Zielgebiete für Stadterneuerungsprogramme bestimmen. Die Studie analysiert den Auswahlprozess, der zur Ausweisung von fünf großen Stadterneuerungsgebieten in Berlin, Deutschland, führt. Sie schätzt dafür den Effekt von langfristigen Trends von zwei zentralen Attributen – der Arbeitslosenquote sowie dem Anteil von Menschen mit Migrationshintergrund – auf die Wahrscheinlichkeit, als Zielgebiet ausgewählt zu werden. Aktuelle Level dieser zwei Attribute werden dabei konstant gehalten. Die Ergebnisse entsprechen den Erwartungen: Ausgehend von einer Gruppe mit relativ hoher aktueller Arbeitslosigkeit, werden Gebiete bevorzugt, die einen positiven Trend oder den Beginn eines Gentrifikationsprozesses vorweisen können. Dieser Effekt wird als *Winner Picking* interpretiert.

Zusammenfassend zeigen die Ergebnisse dieser Dissertation die Wichtigkeit, räumliche Aspekte bei der Analyse von ökonomischen Zusammenhängen nicht unberücksichtigt zu lassen, besonders in einem urbanen oder regionalen Kontext. Es wird offensichtlich, dass konventionelle Schätzergebnisse in Gegenwart von räumlicher Korrelation verzerrt sein können und dass quantitative räumliche Methoden helfen können, diese Verzerrung zu mindern. Angesichts der thematischen Breite und der Komplexität der quantitativen räumlichen Ökonomie helfen diese Ergebnisse, die immer größere Verbreitung von räumlichen Methoden in der allgemeinen angewandten ökonomischen Literatur zu erklären.

EIDESSTATTLICHE VERSICHERUNG

Hiermit erkläre ich, Felix J. Richter, an Eides statt, dass ich die Dissertation mit dem Titel: „Essays in Quantitative Spatial Economics“ selbständig und ohne fremde Hilfe verfasst habe. Andere als die von mir angegebenen Quellen und Hilfsmittel habe ich nicht benutzt. Es wurde keine kommerzielle Promotionsberatung genutzt. Die den herangezogenen Werken wörtlich oder sinngemäß entnommenen Stellen sind als solche gekennzeichnet.

Hamburg, den 10. Juli 2014,