The London School of Economics and Political Science

Essays on the urban economics of housing and land markets

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Declaration

I certify that the thesis I have presented for examination for the MPhil/PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

I declare that my thesis consists of 69,218 words.

London, 30th September 2014

Sevrin Waights

Statement of conjoint work

I confirm that Chapter IV Game of zones: the political economy of conservation areas was jointly co-authored with Gabriel Ahlfeldt, Kristoffer Moeller and Nicolai Wendland.

This paper was based on work done for an LSE Enterprise research project for English Heritage, which was conducted with the same co-authors plus Nancy Holman. The original project involved data collection and a combined qualitative and quantitative report with some basic hedonic regression analysis.

Our subsequent research paper significantly developed on the original report both theoretically and empirically. My contributions to this research were as follows. I made significant contributions to the theoretical model. I was responsible for the initial wave of designation regressions. I was responsible for large parts of the data work in ArcGIS and Stata. I was responsible for estimating the policy treatment effect in the hedonic regressions. I contributed towards the development of our empirical strategies, in particular to the combined Regression Discontinuity Design and Difference-in-Difference (RDD-DD). I contributed towards the final write-up.

This statement is to confirm that I contributed a minimum of 25% to chapter IV as agreed by the undersigned.

Gabriel Ahlfeldt   Kristoffer Moeller   Sevrin Waights   Nicolai Wendland

Abstract

This thesis is comprised of four main chapters. Although the chapters are distinct works, they are related by their focus on housing and land markets and their reliance on urban economic theory and methods. They aim to contribute to the understanding of how these spatial markets function in order to work towards an improved implementation of urban policy. In particular this thesis tries to understand how house prices are determined by demand- and supply-side factors across different scales. It provides support for the idea that at a local level prices are determined by demand, in that they compensate for differences in locational amenities. It also investigates some of the consequences of price determination such as displacement of original residents from gentrifying neighbourhoods and welfare losses as a result of planning restrictions to development. The overall message that emerges from the body of work is that urban policy should pay close attention to the way that supply and demand interact to determine prices in markets for housing and land.

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CONTENTS

CHAPTER I

INTRODUCTION

1. Urban economics

According to the United Nations (2014), more than 54% of the world's inhabitants live in urban areas. However, urban areas cover just 0.5% of the word's land area (Schneider et al., 2009). Putting these estimates together suggests that each person living in a rural area has an average of 45,000m^2 of space, whereas each person in an urban areas has just 200m^2 (i.e. 14m × 14m).[1] Despite the apparent roominess of rural compared with urban life, people all over the world continue to cram into cities. Whilst the share that lived in cities was just 30% in 1950, it is predicted to reach 66% by 2050 (United Nations, 2014). It is clear that urban life provides something valuable, such that it is becoming the standard mode of living for human society. As such, understanding urban areas and urban issues is increasingly important.

Urban economics is a discipline that tries to understand cities using the methods and theories of economics. Questions such as 'what makes some cities more successful than others?' and 'how does urban policy impact on urban economic performance?' are typical in urban economics. To provide theoretical answers to such questions, urban economists typically start out by thinking about the location decision of individuals and firms. How individuals and firms behave is crucial to understanding differences in urban performance and the potential effects of urban policy. In an urban economic model, individuals and firms are assumed to behave rationally, choosing locations based on costs and benefits. Urban models often involve agents interacting in markets for locations (i.e. housing, commercial space or land). These are assumed to be in a spatial equilibrium where prices adjust to ensure that supply equals demand for each location. Such models are then used to generate theoretical predictions about how urban areas function.

[1] Taking 148,300,000 km^2 as total land surface are and 7.125 billion as the population of the world.

Empirical urban economics tests theoretical predictions by examining data on economic agents and spatial market prices. Data on factors such as house prices, wages, and firm performance are analysed in the urban context to discover the costs and benefits that determine the success of cities and the impact of urban economic policy. On a methodological level, such analysis must pay careful attention to econometric issues. Comparing differences in outcomes across locations can be problematic since correlation and causality are not the same thing. Therefore, 'identification' of effects plays a key role in urban economics.

Section 1 of this introductory chapter continues as follows. Section 1.1 gives a quick tour of some of the key ideas is urban economics, placing emphasis on explanations for differences in urban economic performance. Then in Section 1.2 I very briefly explain the importance of housing and land markets in urban economics. These spatial markets are one of the key focuses of this dissertation. In Section 1.3, I examine a common methodological problem in urban economics, focussing on panel fixed effects as a standard solution. This method is used throughout the chapters of this thesis. Section 2 gives a summary of the individual chapters (Section 2.1) and a synthesis of the overall findings drawing some policy implications (Section 2.2).

1.1 A lighting tour of ideas in urban economics

A pre-occupation of urban economics is understanding the determinants of urban economic performance. Probably the most fundamental reason why some cities are considered successful and others not, is to do with the wages they offer. In general, high wages are one of the major attractors of people to cities. This can help explain the growth of a particular city and the general trends in urbanisation across all cities.

Cities pay higher wages, predominantly because they have greater levels of productivity. A whole branch of urban economics looks at 'agglomeration economies' that describe how firms are more productive when they are more densely located (e.g. Henderson, 2003; Rosenthal & Strange, 2001). This idea goes back to Marshall (2009), originally published in 1890, who developed a theory of knowledge spillovers. Here, proximity of firms in the same industry facilitates the exchange of ideas and techniques and boosts productivity, innovation and growth. Agglomeration economies also come about when density reduces transport costs between firms allowing for more efficient inter-industry trade. This is shown to the most important determinant of co-agglomeration by Ellison et al. (2007). Another source of productivity gain related to agglomeration is through enhanced forms of collaboration enabled by frequent face-to-face interaction (Storper & Venables, 2004).

Beyond agglomeration, another important determinant of productivity in cities is human capital. The most productive and most successful cities have the most highly skilled labour force. This may be because individuals become more skilled from living in cities (Glaeser, 1999; Gould, 2007). However, there is mounting evidence that the mostly highly skilled worker 'sort' themselves into the most productive cities (Combes et al., 2008; Yankow, 2006). It is thought that the best labour is attracted to places that offer the best quality of life amenities. Therefore, amenities are a major explanation for different economic performances of cities.

This relates to the emerging concept is that cities as not just centres for production but centres for consumption (Florida, 2002b; Glaeser et al., 2001). The argument goes that people do not choose to live in cities just because of higher wages but because in cities there are more ways and better ways to spend time and money. From cultural amenities, like theatre and art, to the variety of consumption opportunities, like restaurants and shopping experiences, there is just more 'stuff to do' in cities. Indeed in both an inter-urban and intra-urban context, a massive literature document people are willing to pay more for locational amenities (e.g. Albouy, 2009; Black, 1999; Chay & Greenstone, 2005; Gibbons et al., 2011; Linden & Rockoff, 2008). The idea that cities are becoming centres of consumption is also consistent with the documentation of the trend of 'gentrification', where middle-class households return to the urban cores that they had deserted in previous decades (Ellen & O'Regan, 2008). Urban areas are no longer just a place for work that are to otherwise be avoided but a place where people wish to live in and spend time in. All this means that if agglomeration economies disappeared tomorrow, we might all still live in cities the next day.

Public policy interventions are also an important determinant of city performance. Transport investment, in particular, plays a major role in urban economic thought. The monocentric city model, for example, describes how city size is determined by the speed by which residents can travel to the city centre (Alonso, 1964; Brueckner, 1987; Mills, 1969; Muth, 1969). A larger, faster transport network allow resident to locate further from the city centre and the city to grow in size or population. There has been much research that documents the willingness to pay for transport improvements (e.g. Gibbons & Machin, 2005), the effect of transport on urban structure e.g. suburbanisation (e.g. Baum-Snow, 2007) and whether development follows transport or the other way around (Ahlfeldt et al., 2014b). What is clear is that the development of the transport network is intertwined with the urban performance of cities.

Planning policy plays a key role in the performance of cities. Planning has the potential to maintain the amenity level of cities. It can ensuring buildings are of a high architectural quality, keeping cities beautiful. It preserves public parks and open spaces that keep cities liveable. It can also prevent development to ensure cities are neither too dense nor too sprawling, improving the quality of life for residents and environmental impact of the city. However, overly restrictive planning regimes may stop a city from delivering sufficient housing for its current and future populations, resulting in increased housing costs (e.g. Albouy & Ehrlich, 2012; Cheshire & Hilber, 2008; Cheshire et al., 2011; Glaeser et al., 2003; Glaeser et al., 2005; Hilber & Vermeulen, In Press).

Of course it is only possible to cover here a fraction of the topics of urban economics but what has been covered gives an overview of some of the relevant literature that this thesis contributes to. Next I move on to highlight the importance and housing markets in urban economics.

1.2 The importance of housing and land markets

Housing and land markets are crucial to the study of urban economics because they reveal the costs and benefits of locations. Urban economists are not (in particular) interested in house price trends over time at a national level (e.g. bubbles, crashes) but in differences across locations. In the UK context, this means looking at prices across cities (London compared to Liverpool) or within cities (Soho compared to Hackney). These comparisons are useful because they contain so much information about the costs and benefits of different locations. Across cities, house prices tell us about wages and quality of life. For example, London is very expensive because it is in very high demand because it provides high wages, elite jobs and endless consumption opportunities. On the firm side, high business rents and commercial real estimate prices tell us directly about agglomeration economies and human capital. At the local level, price differences reveal amenity differences. Locations with high levels of amenities, such as transport access or good schools will have high prices after controlling for structural characteristics. This tells us about the things that are important to individuals and therefore why they choose to live in cities and what it is that makes cities successful.

1.3 Methodological issues

Urban empirical analysis that attempts to investigate the effect of some factor (or policy) X on some economic outcome Y could begin by running the following bivariate cross-sectional regression across locations i using OLS:

$$Y_i = \alpha + \beta X_i + \varepsilon_i \tag{1}$$

where β provides an estimate of the effect of the factor or policy. The error term ε_i is made up of potentially observable variables Z_i and inherently unobservable variables U_i. Some of these location variant factors may be time-invariant and some may vary over both locations and time. Specifically, $Z_i = z_i + z_{it}$ and $U_i = u_i + u_{it}$. If any of these factors are correlated across locations with X_i then the estimate of β will be biased.

A similar regression could be run using time-series data. This would be the same as above replacing the i locations with time periods t. In this case, bias is caused by any time-variant factors that impact on Y. These may be location-invariant or vary over both time and location i.e. $Z_t = z_t + z_{it}$ and $U_t = u_t + u_{it}$.

The collection of a panel dataset and inclusion of time and location effects can eliminate or reduce many of these sources of bias. The following fixed effects model can be estimated:

$$Y_{it} = \alpha + \beta X_{it} + f_i + y_t + \varepsilon_{it} \tag{2}$$

where f_i are location fixed effects and y_t are time, or 'year' effects. The major advantage of this model is that, even before thinking about control variables, all time-invariant and location-invariant observables and unobservables are controlled for:

$$Y_{it} = \alpha + \beta X_{it} + (z_i + u_i) + (z_t + u_t) + \varepsilon_{it} \tag{3}$$

That this controls for fixed or time-specific *unobservables* is a clear advantage. However, it should not be overlooked that model also controls for *all* fixed observables. Realistically, for several reasons, even a very thorough researcher will fail to find control variables for all *potentially observable* factors. Countless observable factors will not be thought of or may be too time consuming to collect. Controls will be subject to measurement error. Multicollinearity may preclude inclusion of all controls. But the inclusion of fixed and time effects deals bypasses all these issues where the factors are fixed over space or time. In the spatial context, this eliminates some huge sources of bias e.g. due to sorting of different individuals across locations. Temporally there are also likely to be important effects, e.g. where policies correlate with macroeconomic trends.

Of course, there remains the problem of time-location variant factors. All the problems of unobservables and omitted variables are pushed onto this channel, which is hopefully a lesser source of bias. The researcher will, of course, not estimate the above model without controls. The model estimated will include a set of time varying controls A_{it}, which is to be as complete as possible:

$$Y_{it} = \alpha + \beta X_{it} + A_{it}\gamma + f_i + y_t + \varepsilon_{it} \tag{4}$$

Finally, beyond observable controls, effort should be made to ensure that units are as similar as possible across different amounts of the 'treatment' factor X. This can be done, for example by restricting the sample to units that are have different amount of X but are spatially nearby, since nearby units will be unobservably similar.

This final model is comparable to a difference-in-difference. The fixed effects ensure that only time-variation in the treatment variable X_{it} is used to estimate the effect. This is comparable to the first ($POST_t - PRE_t$) difference. The year effects ensure that any general trends correlated with the treatment are taken out. This is comparable with the second ($TREAT_i - CONTROL_i$) difference. In fact, panel fixed effects is simply a more general model and collapses to a diff-in-diff when the treatment is a dummy variable equal to one for treatment group in the post-treatment period and zero in all other cases (i.e. $X_{it} = TREAT_i \times POST_t$).

In Table 1, I illustrate how many sources of bias exist in the panel fixed effects / diff-in-diff model, compared with cross-sectional or time series analysis, and (perfect) instrumentation or randomisation. I assume that a researcher will realistically only ever think of controls for half of the potentially observable determinants of Y. This means that even with controls a threat of bias remains for observables. I assume give this an arbitrary point score of 0.5. This means that compared with a cross-sectional regression, panel fixed effects eliminate all fixed unobservables and half of the fixed observables (those that were not thought of). Further, by ensuring that 'treated' observations are arguably similar to 'control' observations the threat from time-variant factors is also reduced. Here I cut them by half. Of course it is impossible in reality to put an accurate point score on the size of any of these threats but this table does give a simple indication of where threats come from and where they are reduced across different methods.

To conclude this discussion on methodology, the panel fixed effects model can eliminate or reduce many sources of bias. In cases where it is possible to collect a panel dataset, then this method should be considered the minimum standard. In many cases, it may also be the best option available if, for example, no plausible instrument is available, or randomisation is not feasible. As such it represents a workhorse in urban economic research and in this thesis.

Table 1: Threats to internal validity

Threats to internal validity	Cross-sectional + controls	Time-series + controls	Panel fixed effects (or diff-in-diff) + controls	Randomisation or instrumentation
Time				
Observable z_t	0	0.5	0	0
Unobservable u_t	0	1	0	0
Location				
Observable z_i	0.5	0	0	0
Unobservable u_i	1	0	0	0
Time-location				
Observable z_{it}	0.5	0.5	0.25	0
Unobservable u_{it}	1	1	0.5	0
Sum of threats	3	3	0.75	0

2. Overview of thesis

This thesis is comprised of four main chapters. Although the chapters are distinct works, they are related by their focus on housing and land markets. They aim to contribute to the understanding how of how these markets function in order to help work towards an improved implementation of urban policy. In particular this thesis tries to understand how housing prices are determined by demand- and supply-side factors. It also investigates some of the consequences of the ways prices are determined such displacement from gentrifying neighbourhoods and welfare losses as a result of planning restriction to development. The overall message that emerges from the body of work is that urban policy should pay close attention to the way that supply and demand interact to determine prices in markets for housing and land. Section 2.1 provides a summary of the individual chapters and Section 2.2 synthesises the findings to deliver some broader policy implications.

2.1 Summary of chapters

In Chapter II: 'Does the law of one price hold for hedonic prices?' I specifically examine the concept of spatial equilibrium. I argue that hedonic prices of locational attributes in urban land markets are determined by a process of spatial arbitrage that is similar to that which underpins the law of one price. If hedonic prices deviate from their spatial equilibrium values then individuals can benefit from changing locations. I show that, under commonly adopted assumptions regarding individuals' preferences, spatial equilibrium is a necessary and sufficient condition for the law of one price to hold for hedonic prices. I go on to test whether the law holds for the hedonic price of rail access using a unique historical dataset for Berlin over a historical period (1890-1914) characterised by massive investment in the transport infrastructure. I estimate the hedonic price of rail access across multiple urban neighbourhoods and time periods to generate a panel dataset of hedonic price differences that I test for stationarity using a panel unit root test. Across multiple specifications I consistently fail to reject the null hypothesis of no unit root and accept the alternative hypothesis that LOP holds. My estimates indicate a half-life for convergence to the law of one price that lies between 1.2 and 2 years. This result is consistent with spatial equilibrium.

Chapter III: 'Gentrification and displacement in English cities' uses the British Household Panel Survey (1991-2008) and the UK Census (waves 1991, 2001 and 2011) to examine whether gentrification of neighbourhoods in English cities leads to displacement of the original residents. Gentrification is the phenomenon of a large and relatively sudden in-migration of wealthy or middle class residents into a previously poor or working class neighbourhood. I use the change in the share of neighbourhood population that holds a degree certificate as a measure of the pace of gentrification. I relate this measure of gentrification to neighbourhood exits at the household level. My empirical strategy aims to control for differences in natural mobility rates due to the sorting of households across different neighbourhoods. I take several steps to deal with this issue such as estimating a neighbourhood fixed effects model and interacting of the gentrification treatment with household characteristics that indicate vulnerability to displacement. The findings indicate that gentrification is associated with significant displacement of low income (private) renters especially in the early stages of the process. These are the first estimates of displacement for English cities and the first to estimate the effect at different stages of gentrification and at different income levels. The evidence presented in this chapter is contradictory to the prevailing evidence on displacement and is more consistent with the theoretical understanding of gentrification as process of outbidding.

Chapter IV: 'Game of zones: The political economy of conservation areas' examines the process behind the designation of conservation areas by looking at the costs and benefits to local homeowners. The chapter asks whether local homeowners are somehow able to game system to their advantage. Provided there are positive external benefits attached to the historic character of buildings, owners of properties in designated conservation areas benefit from a reduction in uncertainty regarding the future of their area. At the same time, the restrictions put in place to ensure the preservation of the historic character limit the degree to which properties can be altered and thus impose a cost to their owners. Given the existence of local costs and benefits, this chapter tests a simple political-economic theory of the designation process which postulates that the level of designation is chosen to comply with interests of local homeowners. The implication of the model is that a) an increase in preferences for historic character should increase the likelihood of a designation, and b) new designations at the margin should not be associated with immediate house price capitalisation effects. The empirical results are in line with these predictions.

Finally Chapter V: 'The welfare economics of conservation areas' looks at the costs and benefits of conservation area at the wider level. These policies improve the quality of life in cities by preserving neighbourhoods of special architectural and historic character. But they do so by restricting the supply of housing and increasing its cost. A crucial policy consideration, therefore, is how large each of these effects are and what the net effect is. This chapter provides evidence on this question by looking at ten years of conservation area designations in England (1997-2007). I employ the two-step approach outlined by Albouy and Ehrlich (2012), which is underpinned by a general equilibrium model of a system of cities, and allows for the disentangling of demand and supply effects on the price of housing. The first step is to estimate the supply-side cost function across English cities (Housing Market Areas) using a unique panel dataset of house prices, land values and construction costs. This step reveals the impact of city characteristics on housing productivity, defined as the amount of physical housing that can be produced for given quantities of inputs. I find that a standard deviation increase in conservation area designation (equivalent to an increase of 0.013 in the designated land share) significantly decreases housing productivity by between 7% and 9%, implying a supply-driven increase in house prices of the same magnitude. The second step is to estimate the demand-side amenity effects by generating a quality of life index for cities based on house prices and wages and then to regress the quality of life index on housing productivity differences predicted by designation. I find the effect to be statistically insignificant. These findings suggest that the overall impact of conservation areas is welfare decreasing.

2.2 Synthesis of findings and policy implications

Taken together, the findings presented over the four chapters of this thesis suggests that an understanding of the functioning of the housing market is essential for urban policy if social welfare and distributional outcomes in cities are a consideration. The findings support two policy stances. The support for the first stance is more circumstantial in nature and relies on a threading together of the evidence to support the view that area-based policy may have harmful distributional effects. The second is based on direct evidence on the effects of conservation areas. This view suggests that restrictive planning policies set at the local level can reduce social welfare. Again this is likely to have distributional consequences.

2.2.1 Area-based policy

Here I use the findings of this thesis to argue that are-based improvements to neighborhoods in the role of regeneration or revitalization strategies should be carefully thought through if they wish to have helpful distributional impacts. I argue that polices that regenerate neighbourhoods, especially if they are based in a major part on physical improvements (e.g. to the dwelling stock), could have adverse consequences for low income residents, who rent in the private market.

Firstly, improvements to a neighbourhood will lead to price increases via capitalisation. I show in several parts of this thesis that price differences at the neighbourhood level are determined by differences in demand as a result of differences in amenities. This capitalisation effect is theoretically supported by the spatial equilibrium assumption that I test directly in Chapter II. Further evidence that local costs and benefits capitalise into prices are presented in Chapter II, for rail, and Chapters IV and V for the preservation of heritage.

Secondly, gentrification of neighbourhoods is likely to accompany improved amenities and be the driving force behind price increases. This is not something I provide significant evidence on here, but is an important step in the argument. The evidence I do provide is from the first stage of the instrumentation strategy in Chapter V that shows that amenities (rail access and museums) do predict gentrification. Furthermore, it is a theoretically and empirically established idea in the literature that amenity improved amenities are associated with gentrification (e.g. Brueckner & Rosenthal, 2009; Helms, 2003).

Thirdly, and finally, I show that gentrification of neighbourhoods leads to strong displacement of private renters (presumably due to outbidding in the housing market). This displacement effect is demonstrated empirically in Chapter III. Displacement is likely to be the main outcome of gentrification for low income renters, since evidence from recent field experiments suggests that no improved economic outcomes for the disadvantaged from living in more mixed communities (Ludwig et al., 2013; Oreopoulos, 2003).

Putting these three points together suggests that area-based improvements to amenities will likely result in the displacement of original residents who rent privately (and especially if they have a low income) due to the escalation of housing costs. Original residents who rent socially or own their homes will not be displaced.

This suggests that area-based policy concerned with distributional outcomes should be combined with support for private renters, either through rental assistance, the provision of social housing, or help towards gaining homeownership. It should also avoid purely physical improvements to neighbourhoods, since these are unlikely to directly improve economic outcomes of residents. Improvements combined with the provision of services such active labour market policy or better employment access (e.g. rail upgrades) may allow some residents to resist displacement by increasing employment and incomes. Furthermore, area-based schemes could be designed such that benefits are attached to original residency as is the case for many Employment Zones policies where local tax relief for firms is conditional on the employment of local residents. Finally, area-based schemes could be avoided all together in favour of people-based measures such as education and redistribution through the tax system that directly target the causes of inequality.

2.2.2 Planning systems

The above arguments are based mostly on evidence presented in the first two chapters. The last two chapters examine a particular form of planning policy that restricts development in historic zones in order to preserve built heritage i.e. conservation areas. I argue here that planning policy should not be implemented on a local level if the benefits are localised and the costs are incurred at the wider market level.

Chapters IV and V are based on an understanding of housing markets that suggests that price differences at the local level (i.e. between neighbourhoods) is determined principally by demand, but that price at the market level (i.e. between cities) is based on both demand and supply. Chapter IV demonstrates by means of a local level analysis that conservation area designation appears to adhere to the interest of local homeowners. These owners stand to gain from designation through house price growth related to the fact that the policy provides increased security over the future character of the neighbourhood i.e. it represents an amenity improvement. Before coming on to the supply side effects, the above arguments on the impact of area-based policies already suggests that this policy may have distributional consequences via potential the displacement of private renters in the conservation area. Indeed this chapter demonstrates that conservation area designation is tightly interlinked with the gentrification process.

Chapter V examines the Housing Market Area (HMA) level effects of conservation area designation. These HMAs roughly correspond with urban areas and allow for the analysis of the effects of supply restrictions. The findings suggest that through the restrictions placed on development the cost of housing is significantly increased by designation. However, the amenity, or quality of life, benefits of designation are found to be insignificant at the city level. Taken together with the findings from Chapter IV this suggests that the benefits of designation are highly localised and insignificant compared with the costs when examining the wider housing market. Conservation areas impact negatively on the economic welfare of wider society but are influenced locally by homeowners who stand to benefit from localised house price increases. These findings are easily generalised to suggest that planning systems should be designed so as there is limited influence at the very local level over development permission. Whilst local impact is an important consideration, this should be balanced with societal needs for housing. Such decisions would better be made at the city level or larger.

Taken together, the findings presented over the four chapters of this thesis suggest that an understanding of the functioning of the housing market is essential for urban policy if social welfare and distributional outcomes in cities are a consideration.

3. Literature

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CHAPTER II

DOES THE LAW OF ONE PRICE HOLD FOR HEDONIC PRICES?

1. Introduction*

Glaeser (2008) states that the spatial equilibrium condition is to urban economics what the no-arbitrage condition is to financial economics. Indeed, spatial equilibrium is one of the fundamental concepts around which urban economics is built. However, to date, the assumption has received no empirical scrutiny, perhaps because it not clear how it could be tested. In this paper, I demonstrate that under homogenous preferences that are log-linear in amenities, spatial equilibrium is a necessary and sufficient condition for the law of one price (LOP) to hold for hedonic prices of amenities in urban land markets. Therefore, a test of LOP for hedonic prices represents a joint-test of some of the most commonly adopted assumptions in urban economic theory. I develop a two-stage test for whether LOP holds for the hedonic prices and implement this test for the amenity of rail access in the case of Berlin over 1890-1914, a period characterised by large and frequent transport innovations.

The law of one price states that in an efficient market the price of an identical good or asset must be the same at all locations, otherwise there would be an opportunity for arbitrage. If a local supply (or demand) shock increases the price in one location, then rational agents will transport the good to the expensive location from the cheaper location to make a profit. This arbitrage will quickly eliminate the price difference. A similar argument unpins the assumption of spatial equilibrium in the determination of hedonic prices of the attributes of land (or housing): land prices must exactly compensate for differences in amenities across locations otherwise individuals would want to change location.

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A local shock to amenities (e.g. a new rail line) without a land price adjustment would imply the amenity (rail access) is 'too cheap' in the improved locations i.e. that the hedonic price is below its spatial equilibrium value. Utility maximising households would demand land at the improved locations where rail access is cheaper. This pushes up the price of land until it fully compensates for the amenity improvement i.e. until the spatial equilibrium hedonic prices of rail access are restored. This process is similar to LOP but where individuals move *themselves* to where non-tradable goods (attributes) are cheaper instead of transporting the goods.

This paper investigates the case of Berlin between 1890 and 1914, a period characterised by a series of massive infrastructure projects that represent a barrage of local shocks to the hedonic price of rail access across different neighbourhoods and time periods. Significant spatiotemporal variation in hedonic prices allow me to test if neighbourhood-specific shocks to hedonic prices are persistent or if price deviations from equilibrium are eliminated via spatial arbitrage. Put another way, this historical case provides an excellent scenario with which to examine if hedonic prices across urban locations are tied together in a long-run LOP relationship, and therefore (under certain assumptions) whether spatial equilibrium holds.

I provide evidence on this question by developing and implementing a two-stage approach. In the first stage I use a unique historical panel dataset of land values and transport infrastructure for Berlin (1890-1914) where I estimate the hedonic price of rail access in city-neighbourhoods over time. I use these estimates to produce a panel dataset of hedonic price differences between neighbourhoods. In the second stage, I adopt a standard test in the LOP literature which is to examine the price differences for stationarity using a panel unit root test. In particular I employ a test which exhibits good properties for short panels (Blander & Dhaene, 2012). Across multiple specifications I consistently fail to reject the null hypothesis of no unit root and accept the alternative hypothesis that LOP holds. My estimates indicate a half-life for convergence to the law of one price that lies between 1.2 and 2 years.

This evidence provides support for some of the most commonly adopted assumptions in urban economics. If the assumptions about preferences are appropriate then these results tell us that spatial equilibrium holds in the long run. Conversely, if spatial equilibrium is accurate then this result tells us that commonly adopted utility functions work well enough to empirically capture spatial adjustment in hedonic prices. Whilst this approach is not able to the test the assumptions individually, the findings are broadly consistent with the way urban economists think about spatial arbitrage and the determination of land prices in cities. This provides reassurance that the theoretical frameworks in urban economics are describing the actual processes at hand and lends strength to results founded on these frameworks.

These results contribute to the theoretical literature in urban economics that relies on the spatial equilibrium assumption such intra-urban models of the Alonso-Mills-Muth type (Alonso, 1964; Brueckner, 1987; Mills, 1969; Muth, 1969) and inter-urban models of the Rosen-Roback type (Albouy, 2009; Roback, 1982b). It also contributes to the literature on the determination of hedonic prices in equilibrium (Epple, 1987; Rosen, 1974) and the literature that estimates the value of urban amenities and policies using the hedonic method (e.g. Black, 1999; Chay & Greenstone, 2005; Linden & Rockoff, 2008), particularly that which values transport innovations (e.g. Gibbons & Machin, 2005). Finally it contributes to the literature on the law of one price (e.g. Frankel, 1986; Frenkel, 1980; Hakkio, 1984; Isard, 1977; Jenkins & Snaith, 2005; Krugman, 1978; Protopapadakis & Stoll, 1983; Richardson, 1978; Rogers & Jenkins, 1993), in particular to the more recent work that looks to test the absolute/relative versions of LOP with panel unit root tests (e.g. Blander & Dhaene, 2012; Funke & Koske, 2008; Goldberg & Verboven, 2004, 2005; Parsley & Wei, 1996) and that which looks to test if LOP applies for heterogeneous goods (e.g. Spreen et al., 2007).

The structure of the paper is as follows. Section 2 provides a brief overview of the literature on LOP, highlighting the different versions of LOP and the typical empirical tests. In section 3, I ask the question of whether I would theoretically expect hedonic prices to conform to the LOP. In section 4, I outline the data on historical Berlin. Section 5 develops the two-stage empirical approach. Section 6 gives the results of the hedonic price estimation and unit root tests. Section 7 concludes.

2. The Law of One Price

In this section I provide a brief outline the law of one price and its interpretations. In particular, I highlight that long-run LOP implies price differences across locations will exhibit convergence. In the *Absolute* version of LOP, the convergence will be to zero and under *Relative*-LOP the convergence is to a non-zero constant i.e. there exists a fixed price difference between locations. Both versions imply that price differences between locations will be stationary which lends itself conveniently to empirical testing via a unit root test. This section provides just sufficient detail for understanding the approach taken in this paper. However, an interested reader may see the more detailed overview of the LOP literature that is provided in the appendix.

2.1 Strong (short-run) LOP

The strong, or short-run, version LOP is the most literal translation of the law and requires instantaneous elimination of price differences between locations. This implies that prices must be equal across locations at all times. The early empirical literature focussed on testing strong LOP by examining price differences of homogenous goods across countries (e.g. Frenkel, 1980; Isard, 1977; Krugman, 1978; Protopapadakis & Stoll, 1983; Richardson, 1978). This literature used regressions of the log of prices in a home country against the log of prices in a foreign country and the exchange rate. Generally, though, the law performed badly and the null hypothesis that the coefficient on foreign prices is equal to one (i.e. that LOP holds) was usually rejected.

2.2 Weak (long-run) LOP

Confronted with this poor performance, the next wave of empirical literature examined whether LOP held in the long run (e.g. Frankel, 1986; Hakkio, 1984; Jenkins & Snaith, 2005; Rogers & Jenkins, 1993). This less strict interpretation (the weak version of LOP) allows for price differences to exist, but states that they cannot persist in the long-run. Price differences are not necessarily eliminated immediately since there are transportation, information and transaction costs that may inhibit arbitrage (Engel & Rogers, 1994; Parsley & Wei, 1996, 2001). But the larger the price differences the more likely the good will be the subject of arbitrage. This entails convergence of price differences to an 'attractor equilibrium'. Therefore, this wave of literature focuses on testing for the existence of convergence through the application of unit root tests[2]. Most recently, tests of LOP have found strong support for price convergence using panel unit root tests on the price differences for homogenous goods across numerous countries (e.g. Blander & Dhaene, 2012; Funke & Koske, 2008; Goldberg & Verboven, 2004, 2005; Parsley & Wei, 1996). The test provided by Blander and Dhaene (2012) is of particular relevance to this paper, since it is suitable for short panels. This is the test I will use in the empirical section.

2.3 Absolute and Relative LOP

As discussed above, weak LOP suggests that price differences between locations will not persist in the long run and will, therefore, exhibit stationarity. Stationary series, however, do not necessarily converge to a mean of zero. The literature on Relative-LOP provides some reasons why there may exist a persistent and constant price difference between locations. For example, Goldberg and Verboven (2005) suggest reasons such as differences in trade policies, local distribution costs, or elasticities of demand. For example with local distribution costs, the price differences should converge to a constant that is equal to the difference in distribution costs between the locations. Therefore, Absolute-LOP is defined as a stationary price series that converges to a mean of zero and Relative-LOP is convergence to a non-zero constant.

2.4 LOP in this paper

Before going on to the next section, it worth considering for a moment which of these versions of LOP is likely to be relevant to the context of hedonic prices in

[2] The methods of co-integration and error-correction have also been used in the LOP literature but are less common. See Froot and Rogoff (1996) for a detailed comparison of the different methods

an urban context. Whilst short-run LOP has not received great support in cross-country tests, it is possible that there are fewer frictional costs to arbitrage in an intra-city context. Information should flow fairly quickly over such short distances. Transportation, in terms of individuals moving between urban locations, on the other hand, represents an entirely different cost structure to the cross country transportation of goods and it is difficult to suppose which is more or less costly. Finally, there may be transaction costs in the form of rental contracts, zoning restrictions and regulation. Overall, it seems plausible that either the short-run or the long-run version may hold for hedonic prices. This paper concerns itself nevertheless with testing the long-run version. Notably, if LOP holds in the short run, then it would also hold in the long-run. In terms of Absolute- and Relative-LOP, whether hedonic prices are identical across locations or characterised by a fixed differences is partially addressed the next section where I examine theoretically whether I expect hedonic prices to adhere to any version of LOP.

3. Should hedonic prices adhere to the law of one price?

In this section I aim to demonstrate that equilibrium hedonic prices should adhere to the law of one price under some common assumptions regarding individuals' preferences. Firstly, I outline in a general model, the theory behind hedonic price determination, based on Rosen (1974). Further details on this are in the appendix. Secondly, I show how spatial equilibrium leads to hedonic prices adjustment. Thirdly, I impose assumptions on preferences and demonstrate the equivalence of spatial equilibrium with LOP. I show that for homogenous preferences that are linear in amenities then spatial equilibrium is a necessary and sufficient condition for Absolute-LOP to hold. I also briefly discuss the case of heterogeneous preferences, which relates tangentially to Relative-LOP. Finally, I discuss the case of log-linear homogenous preferences, which implies a LOP in percentage terms. This final model resembles that which is commonly observed in the literature and this is the model I will take to the data.

3.1 Hedonic price determination

This section provides a brief overview of hedonic price determination in spatial equilibrium that is based the theoretical framework provided by Rosen (1974). (The appendix provides further detail.) I depart from Rosen (1974) by assuming, for simplicity, that individuals consume land directly. This bypasses the housing supply side of the model and entails that locational amenities are given exogenously. Plots of land are characterised by a vector of N amenities $z = z_1, z_2, \dots, z_N$ and the price of land is function of its amenities $p(z) = p(z_1, z_2, \dots, z_N)$. This is the hedonic price function, where the partial derivatives with respect to each amenity, denoted $p_1, p_2, \dots, p_N$, are the hedonic prices of those amenities. Individuals' utility is a function of consumption good x, the price of which is the numeraire, and the amenities z. Individuals have a budget constraint $y = x + p(z)$, where y is their exogenous income and must choose a location that maximises their utility. Individuals' bid functions θ describe the maximum amount they are willing to pay for land with amenity levels z in order that they achieve given utility level u:

$$U(y - \theta, z_1, z_2, \dots, z_N) = u \tag{1}$$

The bid function is therefore given by $\theta(z; u, y)$. Maximisation of utility occurs when individuals choose a location where the hedonic price for each amenity is equal to their marginal rate of substitution for those amenities $p_n = \rho_n$. This means that individuals are located on their highest possible indifference curve when their own bid function is tangential to the hedonic price function for each amenity. Solving the maximising decision for each individual gives us the demand at each amenity level for any given set of hedonic prices. Spatial equilibrium is given by the set of hedonic prices that that equalise demand with the exogenously given supply at each amenity level[3]. If there is excess demand at a particular amenity level then that level is under-priced and the hedonic prices must rise until the demand matches the available supply.

3.2 Adjustment to spatial equilibrium hedonic prices

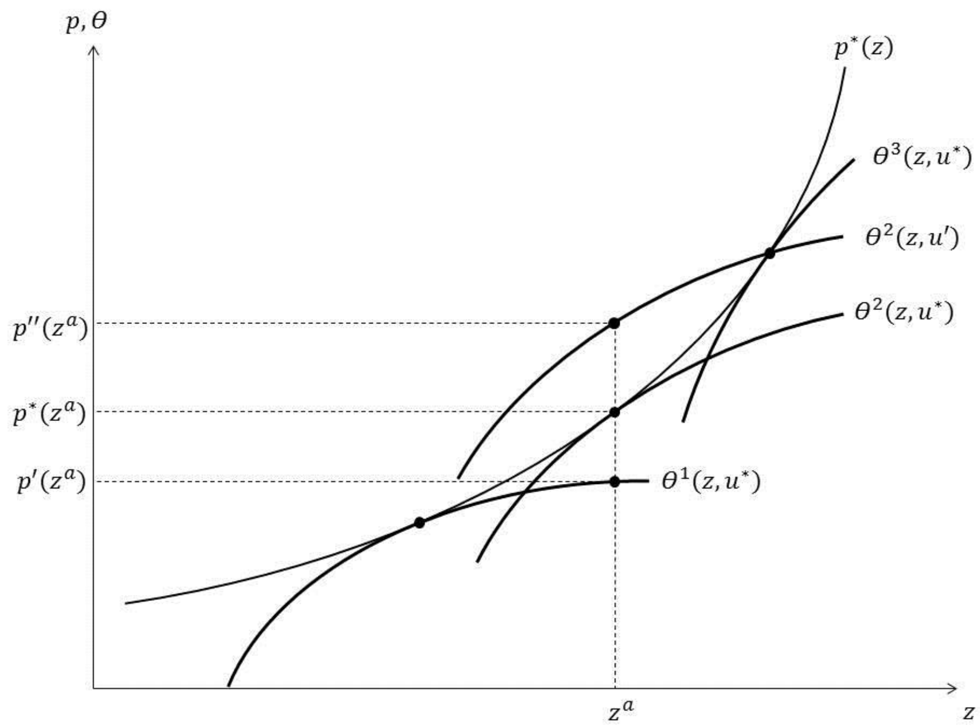
It is possible to describe the process by which spatial equilibrium is achieved by imagining a counterfactual where spatial equilibrium does not hold. In Figure 1 below, the line $p^*(z)$ describes the spatial equilibrium hedonic price function where this is only a single amenity z. (This switch to a single attribute is for simplicity, and is the only change from the model presented in the previous section.) I assume that each location is associated with a different amount of amenity z. Three bid functions are given for three different individuals who have chosen locations that maximise their utility with respect to these equilibrium hedonic prices. These individual choose different locations because they have different preferences for amenity z or different incomes. However, the parameters that deliver these equilibrium choices are not the focus of the

[3] As mentioned I have ignored the producer side from Rosen (1974) since I aim to deal only with land where the attributes are assumed to be given exogenously.

following analysis, rather, how individuals react to deviations in the hedonic prices from equilibrium.

Individual 2 has chosen the location a associated with the amenity amount z^a. This is because this person's equilibrium bid-rent $\theta^2(z; u^*)$ is tangential to the hedonic price function at this point a. The individual cannot gain by moving because other locations with different amounts of z would offer this individual a higher indifference curve (i.e. a lower utility) at the equilibrium hedonic prices.

Figure 1: Spatial equilibrium and spatial arbitrage



Note: this figure is based on Figure 1 from Rosen (1974: p.39) but has been adapted to demonstrate out of equilibrium situations.

What would happen if the price at location a was at $p'(z^a)$ below the equilibrium price? The dash, instead of an asterisk, represents simply an out of equilibrium price. In this situation, individual 1 is indifferent between his or her current location and location a. All individuals located between individual 1 and location a will find they can reach a higher indifference curve by moving to location a. This means there is a spatial disequilibrium and an adjustment is necessary. Since many individuals are demanding just one single location (a) the price of that location must rise until it is equal to the spatial equilibrium hedonic price.

Further, suppose that the price at location a was equal to $p''(z^a)$, above the equilibrium price. In this case, individual 2 would be indifferent between his or her current location (a) and the location where individual 3 resides since both would offer the level of utility u'. All of the locations between the two locations would therefore offer individual 2 a higher level of utility than u' and individual 2 would benefit from moving away from a to one of these locations. This is also spatial disequilibrium since there is not enough demand to match the supply at location a and therefore the price must fall.

It is clear from this thought experiment that spatial equilibrium implies that hedonic prices are related to one another across locations. If the hedonic price in one location violates spatial equilibrium then there exist other locations that offer a better deal to some individuals. These individuals will move themselves (or at least place their demands to move) until prices return to the equilibrium relationship. The described process is one of spatial arbitrage that is comparable to the law of one price but where rational agent move goods from cheaper locations to more expensive locations (instead of moving themselves). However, precisely how they are related remains unclear. In fact, in the following I show that it depends on the particular assumptions made regarding the preferences of individuals. In the next paragraph I examine different scenarios to see what different sets of assumptions imply for hedonic price relationships across locations.

3.3 Linear utility with homogenous preferences

I return to the case with N amenities but now individuals are now assumed to have homogenous preferences. To begin with I also assume individuals possess utility functions that are linear in amenities and the consumption good[4]:

$$U = x + \bar{\rho}_1 z_1 + \bar{\rho}_2 z_2 + \cdots + \bar{\rho}_N z_N \tag{2}$$

where $\bar{\rho}_1, \bar{\rho}_2, \ldots, \bar{\rho}_N$ represent the common marginal willingness to pays for each amenity (since individuals are identical). As discussed above, maximisation of utility occurs when individuals choose a location where the hedonic price for each amenity is equal to their marginal rates of substitution for each amenity, e.g. for the n-th amenity it is $p_n = \bar{\rho}_n$. The common bid functions $\bar{\theta}$ can then be obtained by asking what is the maximum willingness to pay for any given set of amenities to achieve a common attainable utility level $\bar{u}$. Slightly rearranged this gives:

$$\theta = (\bar{y} - \bar{u}) + \bar{\rho}_1 z_1 + \bar{\rho}_2 z_2 + \cdots + \bar{\rho}_N z_N \tag{3}$$

Given this bid-rent, spatial equilibrium then occurs when hedonic prices are such that the quantity demanded at each location associated with a particular amenity level is equal to the quantity supplied. Rosen (1974) demonstrates how this can be solved depending on the distributions of preferences in the city relative to the distributions of amenities. However, under the case of homogenous preferences, without a housing supply sector, the problem is trivial. If the equilibrium hedonic price function $p^*(z)$ is simply equal to the common bid rent then all individuals are indifferent between all locations and supply equals demand at all amenity levels. Therefore the spatial equilibrium hedonic prices are given by the partial derivatives of the common bid rent function $p_n^* = \theta_n = \bar{\rho}_n$. This makes the

[4] Here, units of x have been normalised such that its parameter is equal to one. This aspect means that ρ_n is the parameter for the$n$-th attribute and the marginal rate of substitution between z_n and x.

equilibrium hedonic price difference across locations i and j always equal to zero[5]:

$$p_{n,i}^{*} - p_{n,j}^{*} = \bar{\rho}_n - \bar{\rho}_n = 0 \tag{4}$$

Therefore under the case of linear homogenous preferences, spatial equilibrium is a necessary and sufficient condition for a law of one price for the hedonic price of all amenities. It is necessary since because if there is spatial equilibrium then hedonic prices must be equal. It is sufficient because if prices are equal then all locations offer the same utility and there must be spatial equilibrium. However, linear preferences are not intuitive and for this reason not commonly adopted in theoretical or empirical literature. In reality a degree of complementarity is expected which implies utility is determined by a complex product of utility-bearing attributes.

3.4 Homogenous preferences – log-linear

I present a homogenous preferences model with a log-linear utility function that depends on rail access, among other amenities. This model captures a more typical theoretical set-up in the urban economics literature (e.g. Glaeser, 2008). As such it provides the framework for my empirical analysis. Identical individuals maximise utility at each location i in a city by allocating their exogenously given budget W between a consumption good C, whose price is the numeraire, and land L_i, whose value is given by the bid-rent θ_i:

$$\max_{C,L} U_i = A_i C^{\gamma} {L_i}^{1-\gamma} \quad \text{s.t.} \quad W = C + \theta_i L_i \tag{5}$$

[5] In general, the equilibrium hedonic price at any urban location i is a function of the amenity level at that location $p_{n,i}^{*} = \frac{dp^{*}(z_{n,i})}{dz_{n,i}}$. However, given individuals are identical and preferences are linear the price at all location is simply $p_{n,i}^{*} = \bar{\rho}_n$ irrelevant of the amount of the amenity there.

where the amenities term A_i is defined as $A_i = e^{\mathrm{a}_i'\Omega} e^{\Psi ACC_i}$, where a_i represents a vector of m amenities, Ω is a vector of amenity preferences[6], ACC_i is the urban rail access at i and Ψ is its preference parameter.

In spatial equilibrium each location must offer the same level of utility $\overline{U}$ to maximising individuals such that no individual can gain by changing location. Since this has no given units, I set this equal to one for simplicity:

$$U_i = e^{\mathrm{a}_i'\Omega} e^{\Psi ACC_i} (\gamma W)^{\gamma} \left((1-\gamma) \frac{W}{\theta_i} \right)^{1-\gamma} = \overline{U} = 1 \tag{6}$$

and solve for land values:

$$\theta_i = (1-\gamma) \left(\gamma^{\gamma} W e^{\mathrm{a}_i'\Omega} e^{\Psi ACC_i} \right)^{\frac{1}{1-\gamma}} \tag{7}$$

The derivatives of the hedonic function with respect to amenities give the hedonic prices of those amenities for spatial equilibrium (Rosen 1974).

The equilibrium value (denoted with an asterisk) for rail access is therefore:

$$p_i^* = \frac{d\theta_i}{dACC_i} = \frac{\Psi}{1-\gamma} \theta_i \tag{8}$$

By calculating the hedonic price at another location j and rearranging I reach a relation of hedonic prices between city locations i and j:

$$\frac{p_i^*}{\theta_i} = \frac{p_j^*}{\theta_j} = \frac{\Psi}{1-\gamma} \tag{9}$$

The hedonic prices divided by total price (i.e. the hedonic price in percentage terms) in different city locations should be equal. This is therefore a form of the

[6] The m amenities and their parameters can defined:

$$\mathrm{a}_i'\Omega = [\mathrm{a}_{0,i} \quad \cdots \quad \mathrm{a}_{m,i}] \begin{bmatrix} \Omega_0 \\ \vdots \\ \Omega_m \end{bmatrix}$$

law of one price in percentage terms. Notably, in order to empirically estimate the hedonic price in a log-linear model it is necessary to first take logs of the bid rent. This means that the estimates of the marginal price of rail access will already be in percentage terms and the test for LOP will simply be to test for stationarity of their price differences across neighbourhoods i.e. it is not necessary to make a further calculation of the percentage terms since the log-linearisation removes this already.

This subsection has shown that when preferences are homogenous and log-linear in amenities then spatial equilibrium implies a LOP in percentage terms for hedonic prices. The basic intuition behind this empirical finding is that in the log-linear formulation, the individual contributions of each amenity to utility depend on the levels of the other arguments in the utility function. This is because the arguments are multiplicative rather than additive as in the linear form. Hence the marginal willingness to pay depends for any one amenity is a factor of the total price because this reflects all the utility-bearing attributes at that location.

This further highlights that complementarities between amenities are important in modifying the version of the law of one price that applies. The log-linear formulation captures a certain type of complementarity but others may exist. Therefore in the empirical strategy I will develop ensure to pay careful attention to potential additional (not capture by log-linear form) complementarities generating interaction terms between amenities (both rail access and amenity controls).

So far I have only analysed the case of homogenous preferences. This scenario most neatly fits the LOP interpretation of spatial equilibrium. This is the model I will test empirically and the findings will be subject to the reliability of this assumption. However, homogenous preferences are relatively unrealistic, given that individuals place different values on different amenities and have different incomes. Therefore at least an exploratory discussion of the case of heterogeneous preferences is desirable. I provide such a discussion in the next section; however, this is not necessary for understanding the remainder of the paper and can be safely skipped by the casual reader. Furthermore, since this case is complex and not the focus of this paper, I provide only the intuition here and leave the detailed discussion for the appendix.

3.5 Discussion of heterogeneous preferences

This section provides a brief discussion of the case of heterogeneous preferences in the linear case, demonstrating how it relates tangentially to Relative-LOP. The intuition begins with a linear model similar to that outlined in 3.3 but with only one amenity z_1. Preferences are heterogeneous and described by an exogenous distribution across the population. There is also an exogenous distribution of the amenity across locations. Spatial equilibrium is characterised by individuals of different preferences sorting across locations such that the location with the highest amenity amount goes to the individual with the highest preferences for the amenity, the location with the next highest amount goes to the individual with the next highest preferences, and so on. In spatial equilibrium each individual pays a hedonic price equal to their marginal willingness to pay for the amenity.

The amenity distribution therefore maps onto the preferences distribution via sorting across locations. Each location has an amenity level is associated with a particular preference level. The hedonic price is a function of the amenity amount in each location which gives the preferences of the individuals located there in equilibrium. Assuming the distributions of preferences and the amenity are fixed then the equilibrium hedonic price difference between locations is equal to a fixed constant. To take the example from the appendix, if the exogenous distribution of population across marginal rates of substitution (for the amenity z_1) is given according to the function $f(\rho) = b\,d\rho$ and the locations are distributed across amenity levels according to $g(z_1) = k\,dz_1$, where b and k are constants that describe the density of the distributions then the equilibrium hedonic price difference between locations i and j is given by equating supply and demand at each amenity level which gives[7]:

[7] Derivation of this hedonic price difference is given in the appendix.

$$p_{1i}^{*} - p_{1j}^{*} = \frac{k}{b}(z_{1i} - z_{1j}) \tag{10}$$

In contrast to the homogenous preferences case, this is not zero. But if the distributions are fixed then this is equal to a constant. Deviations from this fixed price difference should be met with adjustments since utility maximising consumers would stand to benefit by changing locations. Therefore, this is equivalent to Relative-LOP in the case of fixed distributions.

However, in reality the most likely source of shocks to hedonic prices away from their equilibrium values is changes in the distributions themselves. For example a transport shock to location i would alter both the overall distribution of the rail access amenity and the locations place in this distribution meaning convergence will be to an entirely different equilibrium hedonic price difference. For this reason, heterogeneous preferences, even with linear utility functions do not necessarily describe a case of LOP for hedonic prices. In the appendix I make the argument that the test for LOP may still be appropriate if the price deviations from equilibrium (due to shocks) are typically significant larger than the movements in the equilibrium they converge to (due to changes in the distributions). Furthermore, I argue that heterogeneous preferences may lead to committing a Type II error, where I fail to reject stationarity even though spatial equilibrium doesn't hold but that it cannot lead to committing a Type I error where I reject spatial equilibrium even though it does hold. This entails that a confirmation of LOP will still provide evidence for spatial equilibrium, even if individuals are heterogeneous.

3.6 Summary

To summarise this section, I have asked whether the LOP is expected to apply to hedonic prices of locational amenities in urban land markets. I have shown that whilst the process of spatial arbitrage is similar to traditional LOP, it is not identical. There are a number of reasons why hedonic prices might not be exactly equal even under spatial equilibrium. Heterogeneous preferences and complementarities between goods mean that the hedonic price for attributes in different location may be different even in equilibrium. I have demonstrated how this depends on the assumed utility function for individuals. Under homogenous preferences, if utility is linear then Absolute-LOP should hold for hedonic preferences. Further, I developed a model specifically for the case of rail access that uses the commonly adopted log-linear utility function with homogenous preferences. This demonstrates that under these assumptions spatial equilibrium implies a LOP in percentage terms i.e. rail access should increase land prices by the same *percentage* in all locations. This is the model I will take to the data, which I present in the next section. The findings will be subject to the reliability of the assumptions regarding preferences.

4. Data: historical Berlin

As I have mentioned, local shocks to amenities are a source of possible violations of spatial equilibrium. Therefore in order to test for the existence of potential adjustment processes it is helpful to examine a period in which I expect a lot of local shocks. I use a unique dataset that covers historical Berlin between 1890 and 1914. This is a period characterised by significant change, including a population growth (almost doubled between 1880 and 1912), large transport infrastructure projects and large changes in the structure of land use. These dynamic factors mean that the utility of land at different locations will be subject to an almost continual battery of 'shocks' requiring constant adjustment in land values in order to maintain spatial equilibrium. This makes it a very appropriate case study with which to examine the existence and speed of convergence.

4.1 Land values

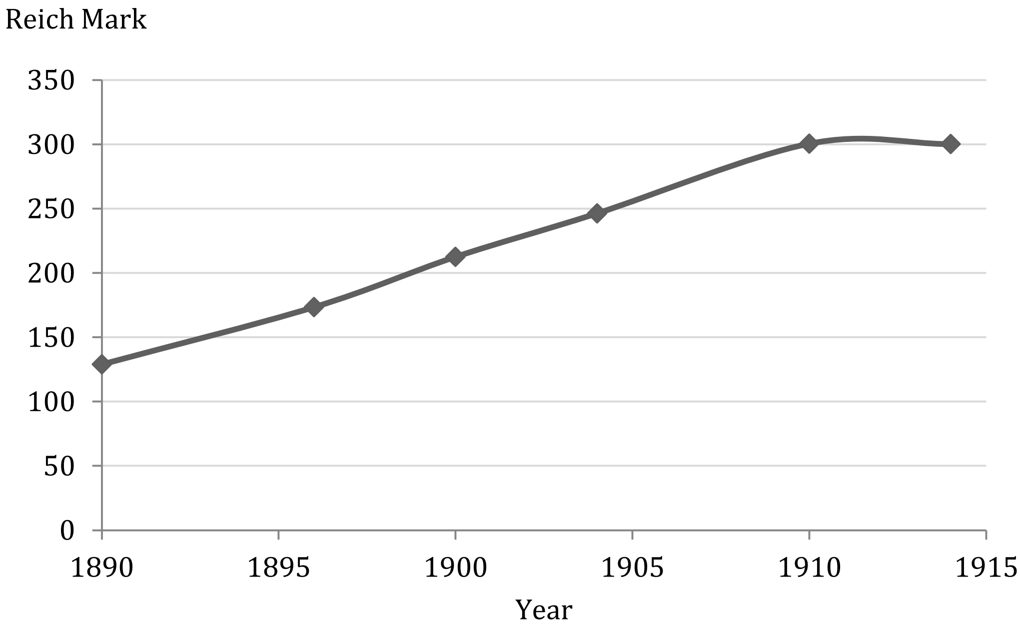
Land values are the dependent variable in the first stage of the analysis and allow for the estimation of the hedonic prices of rail access. Land values are given at the plot level for Berlin for 6 time periods (approximately every 5 years) between 1890 and 1914. This land value dataset was produced by the renowned technician Gustav Müller under the imperial valuation law or *Reichsbewertungsgesetz* of the German Reich. This law includes the strict direction to use capital values for assessing the pure value of land plots based on the fair market price. Müller's values adjust for all structural building and garden characteristics as well as plot specificities such as soil properties, courtyards and whether it is a corner lot. The data were produced in order to serve as official guides to private and public investors into Berlin's real estate market.

The Berlin land values dataset can be compared to the *Olcott's Blue Book of Land Values for Chicago* which is well known in the field of urban economics and has helped Chicago to become a unique laboratory for testing theories of urban economics (McDonald & McMillen, 1990; McMillen, 1996). The Berlin data, like the Olcott values, are available as highly detailed maps. They have also contributed to historical Berlin becoming somewhat of a laboratory of its own.

Previous research has used these data to estimate the changing land gradient (Ahlfeldt & Wendland, 2011), valuing transport innovations (Ahlfeldt et al., 2011; Ahlfeldt & Wendland, 2009) and exploring the role of agglomeration economies (Ahlfeldt & Wendland, 2013). Due to the rapid growth of the city over this period and restructuring of the patterns of land use, the land values are originally an unbalanced panel. From this I took the maximum possible balanced panel resulting in a dataset of 31,790 observations per time period that covers approx. 75 km^2 of land area and 1,758 city blocks. Figure 2 shows these land values for a small section of Berlin in 1914 and Figure 3 illustrates the evolution of mean land value over the sample period.

Figure 2: Section of land values (1914)

Figure 3: Land values (sample mean)



4.2 Quasi-Neighbourhoods

In order to estimate the hedonic price over time in each neighbourhood in the city I define a set of arbitrary grid-neighbourhoods called quasi-neighbourhoods. The reason I define arbitrary grids rather than using administrative unit is so that I can flexibly vary neighbourhood size (and therefore number) in order to vary the width of the resulting panel of hedonic price differences. A wider panel (more neighbourhoods) will increase the power of the panel unit root tests on these price differences. However, a wider panel requires reducing the size of neighbourhoods used to estimate the hedonic price of rail access leading to less precise estimates. In order to demonstrate robustness in the face of this trade-off, I define quasi-neighbourhoods of different sizes.

First I define an 8×16 grid to create 128 grids cells in abstract space. These grid cells are laid over the land value sample as illustrated in Figure 4. In the first neighbourhood definition, these grid cells are divided between two areas by a vertical line as illustrated in Figure 4 by the thick line labelled '2'. In this two-neighbourhood definition, the 64 grid cells to the west of the dividing line make up Neighbourhood 1 and the 64 to the east are Neighbourhood 2. In order to generate the four-neighbourhood definition, I draw an additional (horizontal) line, marked by '4' in Figure 4. The resulting definitions are shown in Figure 5(a) for two neighbourhoods (b) for four neighbourhoods. This procedure is repeated for 8, 16, 32 and 64 neighbourhoods. It is apparent however, that some of the neighbourhoods in some of these definitions will have very few observations or even none within their boundaries. This is problematic for the estimation of hedonic prices within these zones and the following solution is implemented. If the number of observations in one neighbourhood is less than a third of the mean number of observation across all neighbourhoods, then it is merged with an adjacent neighbourhood. An example of this is illustrated in Figure 6, where the first and second neighbourhoods have been merged into Neighbourhood 1. Therefore, what was initially Neighbourhood 3 now becomes Neighbourhood 2, and so on such that the original eight neighbourhoods collapse to seven. Due to this merging criterion the final neighbourhood definitions are characterised by 2, 4, 7, 13, 26 and 47 neighbourhoods instead of 2, 4, 8, 16, 32 and 64 respectively.

Figure 4: Quasi-neighbourhood dividing lines

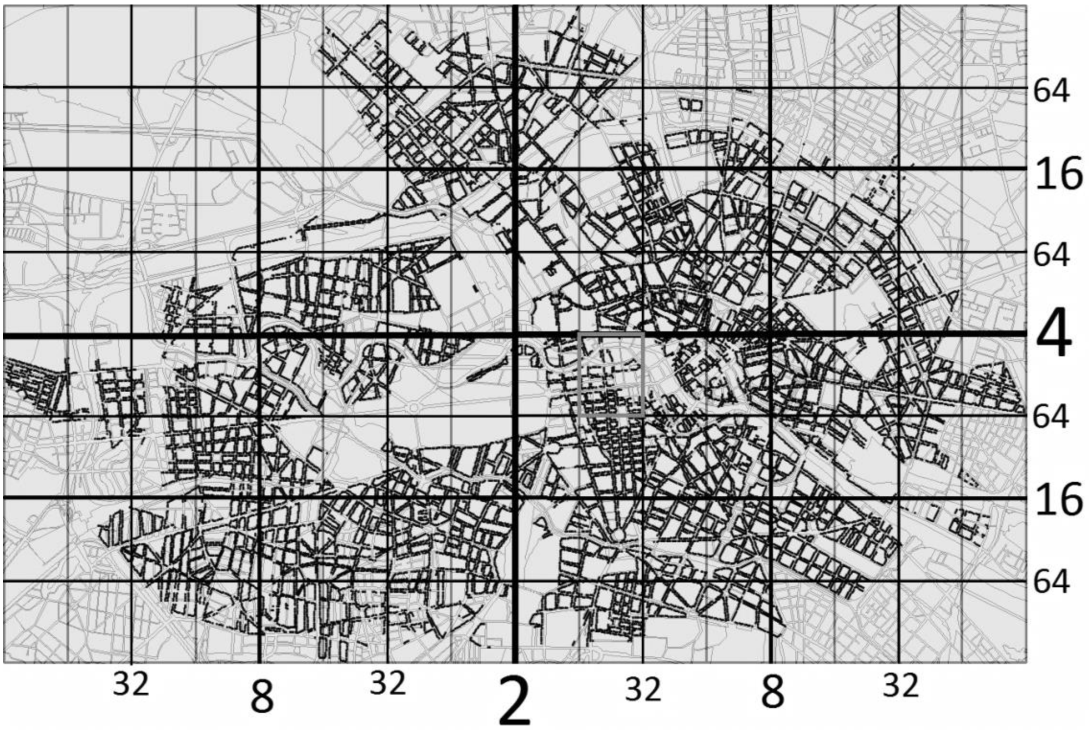


Figure 5: Quasi-neighbourhoods with N = 2 and N = 4

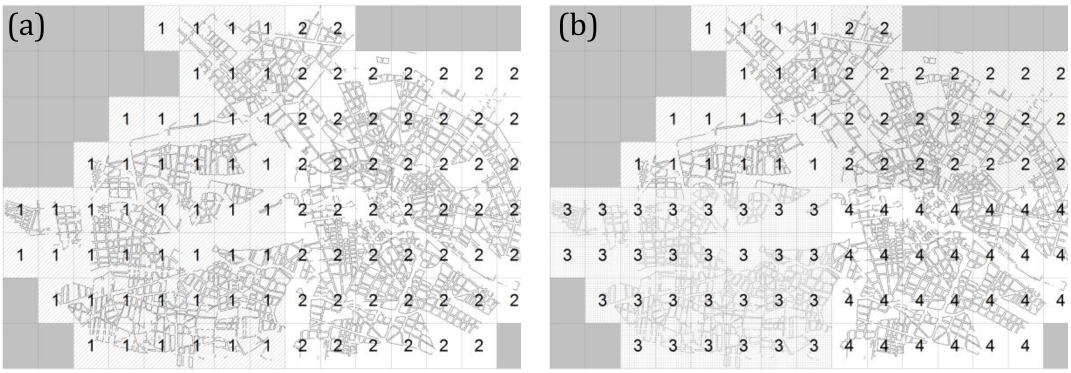
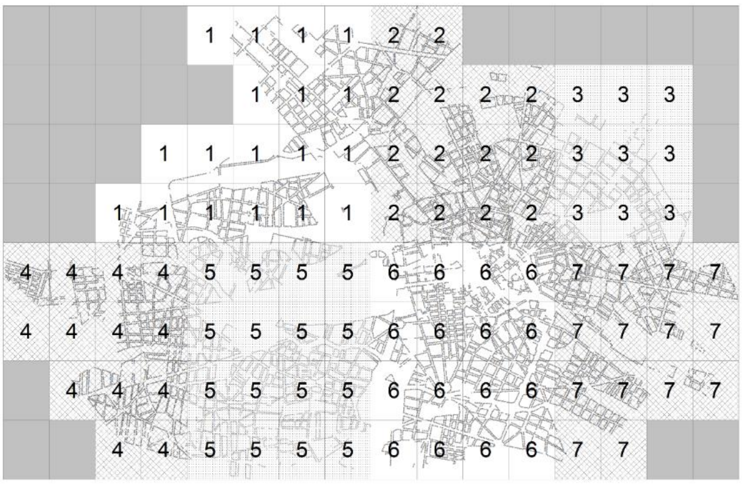


Figure 6: Quasi-neighbourhoods (merging example)



4.3 Rail access

Rail access is the variable of interest and the amenity for which I estimate the hedonic prices. I capture rail access by a measure of station density. The station locations are obtained from a combination of network plans and information on the historical development of the networks such as construction dates[8]. Thus, the urban rail network for Berlin was reconstructed historically for each of the 6 observation time periods in order to compute the time-variant station density variable.

The station density measure is a kernel density function generated in ArcGIS. The procedure involves fitting a smoothly curved surface a kernel around each point (station). The surface is at its highest where the station is located and moving away declines to height of zero at the specified search radius, which I define as the typically assumed maximum walking distance of 2km (Gibbons & Machin, 2005). The precise formulation of the kernel used by ArcGIS is given by the quadratic function described by Silverman (1986a), p. 76, equation 4.5. The volume under the kernel for each station is equal to one. The kernel density is calculated for each land value observation as the sum of the individual kernel surfaces where they overlay that plot. Figure 7 shows transport network and the kernel density measures in relation to the land value plots for 1890 and 1914. Figure 8 shows the development over the period of the mean of station density across the land value observations. There is clearly a large development of the network over the period I study, particularly in the inner-city neighbourhoods. In fact the total number of stations in Greater Berlin increased from 65 to 155 over this period. This point is also clear from the scale used to display station density in 1890 (from 0 to 0.68) compared with 1914 (from 0 to 2.45).

[8] This information can be found at the following websites: www.bahnstrecken.de, berlineruntergrundbahn.de, www.stadtschnellbahn-berlin.de, and www.berlinerverkehr.de.

Figure 7: Station kernel density in 1890 (top) and 1914 (bottom)

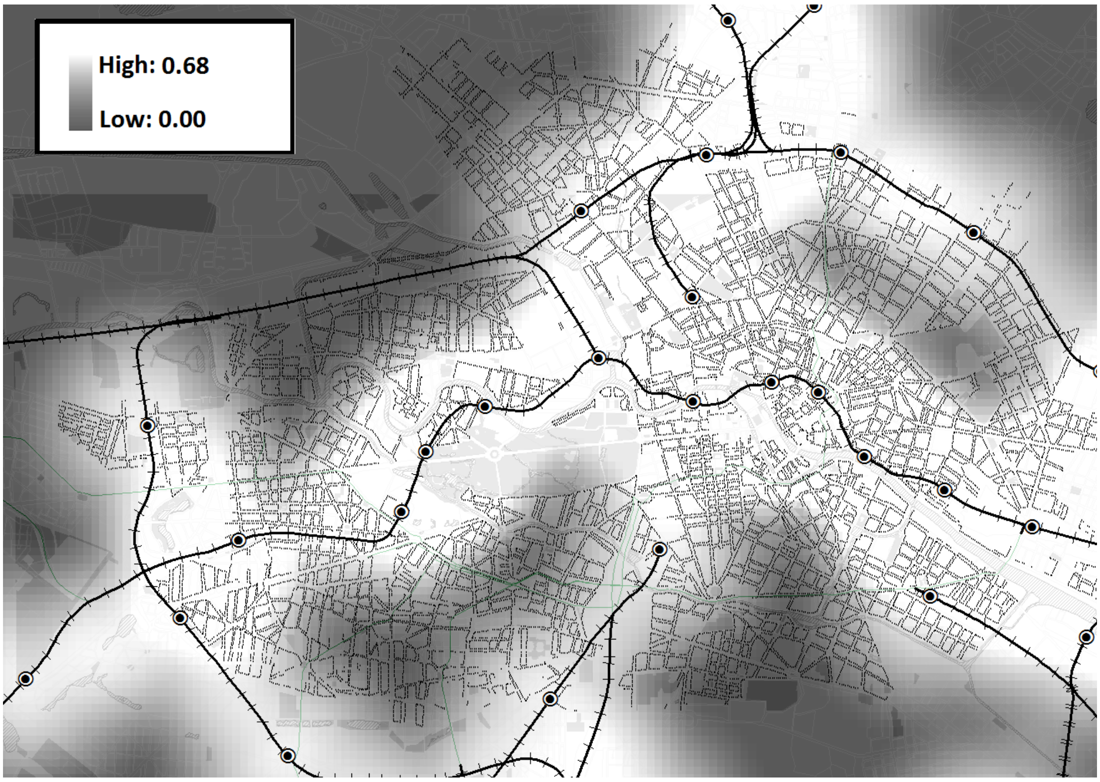
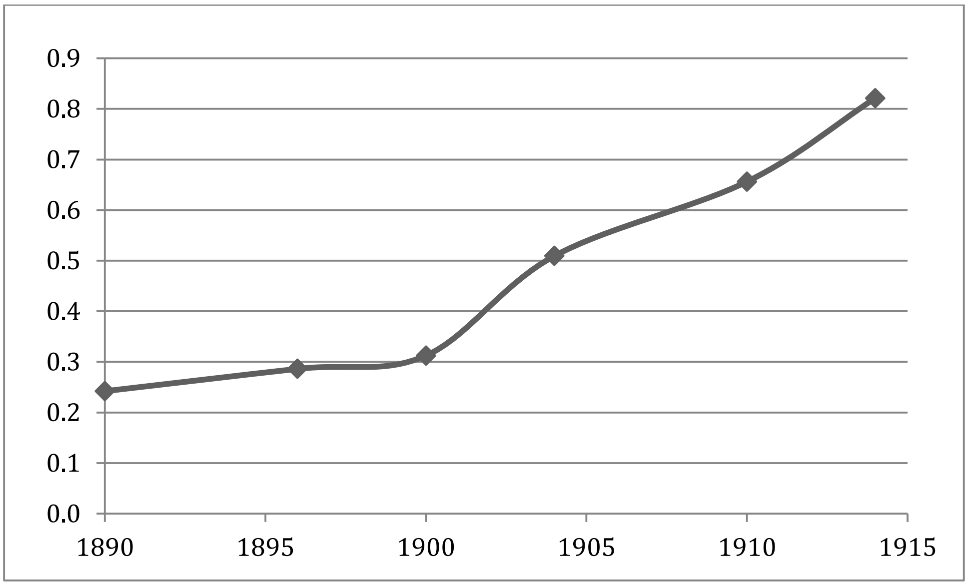


Figure 8: Station density (mean of observations)



4.4 Control variables

In order to gain estimates of the hedonic price of rail access that are as unbiased as possible I use control variables for other urban amenities. The control variables area as follows: distance to nearest green space, distance to nearest water body, distance to the central business district, distance to the secondary centre in west Berlin, Kurfürstendamm, and to capture the disamenity of noise, distance to overground track. These distance measures are calculated for each land value plot in ArcGIS. Distance to track is calculated for each observation period, whilst the other controls are time invariant measures. Table 1 provides summary statistics of all the variables discussed in this data section.

Table 1: Descriptive statistics

Variable	Observations	Mean	Std. Dev.	Min	Max
Land values (RM)					
Land value in 1890	31,790	128.9	177.4	3	2,000
Land value in 1896	31,790	173.4	216.8	5	2,100
Land value in 1900	31,790	212.5	250.0	5	2,120
Land value in 1904	31,790	246.3	276.1	3	2,150
Land value in 1910	31,790	300.5	333.9	3	2,250
Land value in 1914	31,790	300.1	332.5	21	2,750
Station density (kernel)					
Station density in 1890	31,790	0.24	0.16	0	0.66
Station density in 1896	31,790	0.29	0.15	0	0.66
Station density in 1900	31,790	0.31	0.15	0	0.66
Station density in 1904	31,790	0.51	0.29	0	1.47
Station density in 1910	31,790	0.66	0.37	0	1.65
Station density in 1914	31,790	0.82	0.43	0	1.77
Distance controls (km) – no time variation					
Distance to Green space	31,790	0.25	0.17	0	1.07
Distance to Water	31,790	0.81	0.62	0	3.01
Distance to CBD	31,790	3.60	1.63	0	8.34
Distance to Kurfürstendamm	31,790	4.30	2.14	0	9.32

Note: Max station density for land value plots differs from max station density for corresponding year in Figure 7 because the figure shows station density over space, where there may not be any plots.

5. Empirical Approach

In this section I introduce my two-stage empirical strategy for testing spatial equilibrium. The first stage involves the estimation of the hedonic price of urban rail access. Using a dummy variable interaction model I estimate the hedonic price separately in each quasi-neighbourhood in each time period of the panel. This results in a $N \times T$ panel dataset of implicit prices, where N is the number of neighbourhoods and T the number of panel years. In the second stage of the analysis I examine, in the spirit of the law of one price (LOP), whether differences in hedonic prices between neighbourhoods exhibit mean reversion. Since I have 6 time periods, I employ a panel unit test with good properties when testing short panels that also accounts for the possibility of AR(1) correlated error terms (Blander & Dhaene, 2012). A further discussion of panel unit root tests is also provided in the overview of the LOP literature in the appendix.

5.1 The first stage: estimating the hedonic price of rail access

Stage one of my empirical strategy is to use the dataset to estimate hedonic prices of rail access that vary across neighbourhoods and time. I start by taking logs of equation (7) from the theory section 3.4. The bid-rent is then empirically represented by land values, the accessibility measure by station density and the amenities vector by a set of controls, as described above in section 4 on data. In order to gain estimates that vary across locations I use the quasi-neighbourhoods also described in section 4. Each land value observation i belongs to a neighbourhood n $(1 \leq n \leq N)$ and time period $(1 \leq t \leq T)$, where there are N neighbourhoods and T time periods in total. I define for each observation i an $N \times 1$ vector Q_i in which the n-th element is a neighbourhood dummy variable that equals one if land value plot i is observed within neighbourhood n and equal to zero otherwise. I define a similar $T \times 1$ vector Y_i for year dummies, where $\mathrm{Y}_i = 1$ if land value plot i is observed within year t, and $\mathrm{Y}_i = 0$ otherwise.

I estimate the following:

$$\ln LV_i = \alpha + Q_i' \mathrm{B} \mathrm{Y}_i \, SDENS + \mathrm{X}_i' \Phi + \varepsilon_i \tag{11}$$

where B is a matrix of neighbourhood-year specific coefficients for the hedonic price of rail access to be estimated. To write this out for the case of four neighbourhoods ($N = 4$) and six time periods ($T = 6$) I estimate:

$$\ln LV_i = \alpha + [\mathrm{Q}_{i\,1}\ \mathrm{Q}_{i\,2}\ \mathrm{Q}_{i\,3}\ \mathrm{Q}_{i\,4}] \begin{bmatrix} \mathrm{B}_{11} & \mathrm{B}_{12} & \mathrm{B}_{13} & \mathrm{B}_{14} & \mathrm{B}_{15} & \mathrm{B}_{16} \\ \mathrm{B}_{21} & \mathrm{B}_{22} & \mathrm{B}_{23} & \mathrm{B}_{24} & \mathrm{B}_{25} & \mathrm{B}_{26} \\ \mathrm{B}_{31} & \mathrm{B}_{32} & \mathrm{B}_{33} & \mathrm{B}_{34} & \mathrm{B}_{35} & \mathrm{B}_{36} \\ \mathrm{B}_{41} & \mathrm{B}_{42} & \mathrm{B}_{43} & \mathrm{B}_{44} & \mathrm{B}_{45} & \mathrm{B}_{46} \end{bmatrix} \begin{bmatrix} \mathrm{Y}_{i\,1} \\ \mathrm{Y}_{i\,2} \\ \mathrm{Y}_{i\,3} \\ \mathrm{Y}_{i\,4} \\ \mathrm{Y}_{i\,5} \\ \mathrm{Y}_{i\,6} \end{bmatrix} SDENS_i + \mathrm{a}_i \Phi + \varepsilon_i \tag{12}$$

This matrix B with N rows and T columns gives us a panel dataset of hedonic prices of rail access that vary with neighbourhood and year where the matrix element B_{nt} gives us the hedonic price in neighbourhood n and time period t.

Following the convention in the LOP literature I generate price differences from a reference location i.e. $q_{nt} = \mathrm{B}_{nt} - \mathrm{B}_{ref,t}$. Normally I would take log differences but since the hedonic prices are already in logs due to the transformation applied to land values this step is not necessary. If I were to choose the first neighbourhood ($n = 1$) as the reference location I define the relative prices for the remaining three neighbourhoods as:

$$\mathrm{q} = \begin{bmatrix} \mathrm{B}_{21} - \mathrm{B}_{11} & \mathrm{B}_{22} - \mathrm{B}_{11} & \mathrm{B}_{23} - \mathrm{B}_{11} & \mathrm{B}_{24} - \mathrm{B}_{11} & \mathrm{B}_{25} - \mathrm{B}_{11} & \mathrm{B}_{26} - \mathrm{B}_{11} \\ \mathrm{B}_{31} - \mathrm{B}_{11} & \mathrm{B}_{32} - \mathrm{B}_{11} & \mathrm{B}_{33} - \mathrm{B}_{11} & \mathrm{B}_{34} - \mathrm{B}_{11} & \mathrm{B}_{35} - \mathrm{B}_{11} & \mathrm{B}_{36} - \mathrm{B}_{11} \\ \mathrm{B}_{41} - \mathrm{B}_{11} & \mathrm{B}_{42} - \mathrm{B}_{11} & \mathrm{B}_{43} - \mathrm{B}_{11} & \mathrm{B}_{44} - \mathrm{B}_{11} & \mathrm{B}_{45} - \mathrm{B}_{11} & \mathrm{B}_{46} - \mathrm{B}_{11} \end{bmatrix} \tag{13}$$

In order to demonstrate robustness with respect to choice of base neighbourhoods, I will conduct the multiple unit roots test, changing the reference neighbourhood each time until all neighbourhoods have served as the reference.

There may be some problems with this simple estimation approach. First, there may exist unobserved year and neighbourhood specific factors that affect land values. For year effects, these could be exogenous macroeconomic factors that affect the whole of Berlin. For neighbourhood effects, these could be socioeconomic characteristics that affect the valuation of an area. These could potentially vary over time as neighbourhoods change with the development of the city. Therefore introduce neighbourhood-year indicators to capture these time-place specific effects. These are simply the uninteracted version of the dummy variables that were interacted with the station density measure in the last specification.

Second, I have applied logs to the bid-rent function derived from the theoretical model in order to reach a linear regression model. Whilst this functional form captures a degree of complementarity between amenities, the actual complementarities may be structurally different. Therefore I attempt to generalise the model by adding interaction terms between rail access and the vector of all other amenities. I interact rail access with itself in the form of a squared interaction term. This captures potential further non-linearities not removed by the log-linearisation. The final model estimated is:

$$\ln LV_i = Q_i' A \mathrm{Y}_i + Q_i' \mathrm{B} \mathrm{Y}_i\, SDENS_i + \delta\, SDENS_i{}^2 + \mathrm{X}_i' SDENS_i \Pi + \mathrm{X}_i' Q_i' \Theta \mathrm{Y}_i + \varepsilon_i \quad (14)$$

where A is a matrix of neighbourhood-year specific constants to be estimated, δ is a parameter for the station density squared term, Π is a vector of parameters for the interaction terms of station density with each amenity and Θ is a matrix of neighbourhood-year specific parameters for amenities. In total I have $(m + 2)NT + 1 + m$ coefficients to estimate. In the specification with the smallest and most numerous neighbourhoods definition (N=47) and the total number of parameters is 1,980.

It is important to note that only the parameters for the uninteracted station density are the focus of the unit root testing in the next stage. The interacted versions including the squared term are conceptualised are introduced with the intention of removing non-linearities and complementarities from the hedonic price of rail access. What is left is only the linear component of the overall hedonic price. This may be conceptualised as the interacted and uninteracted amenities each capturing something of distinct amenity value. In this sense they represent individual amenities with their own hedonic prices. As discussed in the theory section it is the linear hedonic price that is expected to most closely adhere to the law of one price and this is what is taken forward to the next stage.

5.2 The second stage: panel unit root test

In the second stage of I proceed to test the estimated matrix of hedonic prices for compliance with LOP. To do this I test the matrix of estimated price differences $\hat{q}$ as in equation (13) for stationarity using the unit root test described by Blander and Dhaene (2012):

$$\hat{q}_{nt} = \alpha_n + \varphi\hat{q}_{nt-1} + \rho\Delta\hat{q}_{nt-1} + \varepsilon_{nt} \tag{15}$$

where the null hypothesis is $\varphi = 1$, that the price differences have a unit root and that LOP does not hold. A rejection of this null hypothesis implies that q_{nt} exhibits convergence and that LOP holds. If the constant terms α_n are zero then absolute LOP holds and if they are positive and significant then relative LOP holds. This test also incorporates a single lagged difference (with parameter ρ) and is hence the panel equivalent of an ADF(1) test. This allows for AR(1) error terms. The Blander-Dhaene test exhibits strong properties for short panels and is therefore suitable for a dataset with only 6 time periods. A general discussion of panel unit root tests is provided in the review of the LOP literature in the appendix. The authors also note that results using panel unit root tests are sensitive to the choice of reference location when calculating price differences. Therefore I will conduct the analysis using every location as a reference location once.

6. Results

In this section I indicate the results of the two-step empirical strategy and interpret them in the context of the LOP.

6.1 Stage one: hedonic price estimates

In column (1) of Table 2 I present the results of estimating equation (11) for a single neighbourhood (i.e. $N = 1$) and without controls. Station density is interacted with year effects and the corresponding coefficients indicate the hedonic price evolution for the whole of Berlin. It is apparent that there is a positive amenity value to station density, which in the initial period (1890) has a coefficient of 1.23 and is significant at the 1% level. The interactions with year effect indicate that the hedonic price is higher in every period than in the initial period. Since the dependent variable is the log of land values, the coefficient can be interpreted as a percentage effect[9]. A one unit increase in station density is therefore associated with a 242% increase in land value in 1890. The size of this coefficient is not entirely surprising considering that one unit increase in station density represents a very large increase. A one unit increase in station density can only be achieved when there are many new stations close to the location. It is natural therefore that it should be associated with a very large response.

Next, in column (2) I introduce the full set of control variables as in equation (14) and this only slightly changes to the coefficient for station density to 1.27 (or 256%) in 1890 but it is in lower in every year after compared with the model without controls. Figure 9(a) plots the hedonic price evolution over time as estimated using this model specification for one neighbourhood. The coefficient for distance to CBD is -0.53, which is interpreted as a 70% decrease in land values per km further from the CBD. Whilst this seems fairly steep it is roughly in line with other estimates of CBD gradients in historical contexts (Ahlfeldt &

[9] The formula used to calculate the percentage effect of a coefficient β is $(e^{\beta} - 1) \times 100$.

Wendland, 2011 provide a summary). The distance to Kurfürstendamm (*Ku'damm* for short) captures the amenity effect associated with proximity to the Berlin's most important sub-centre. The coefficient of 0.05 is equivalent to a 5% per km increase with distance from the secondary centre. This is the opposite sign to what is expected and probably due to significant non-linearities in the effect. Distance to green space (-0.65, or 92% per km) and distance to water bodies (-0.20, or 22% per km) are also found to be amenities that capitalise into land values. Finally, distance to track, which is intended to capture the disamenity of rail noise, is associated with a coefficient of 0.05 (5% per km). This suggests that distance to rail indeed captures some negative aspect of rail such as train noise.

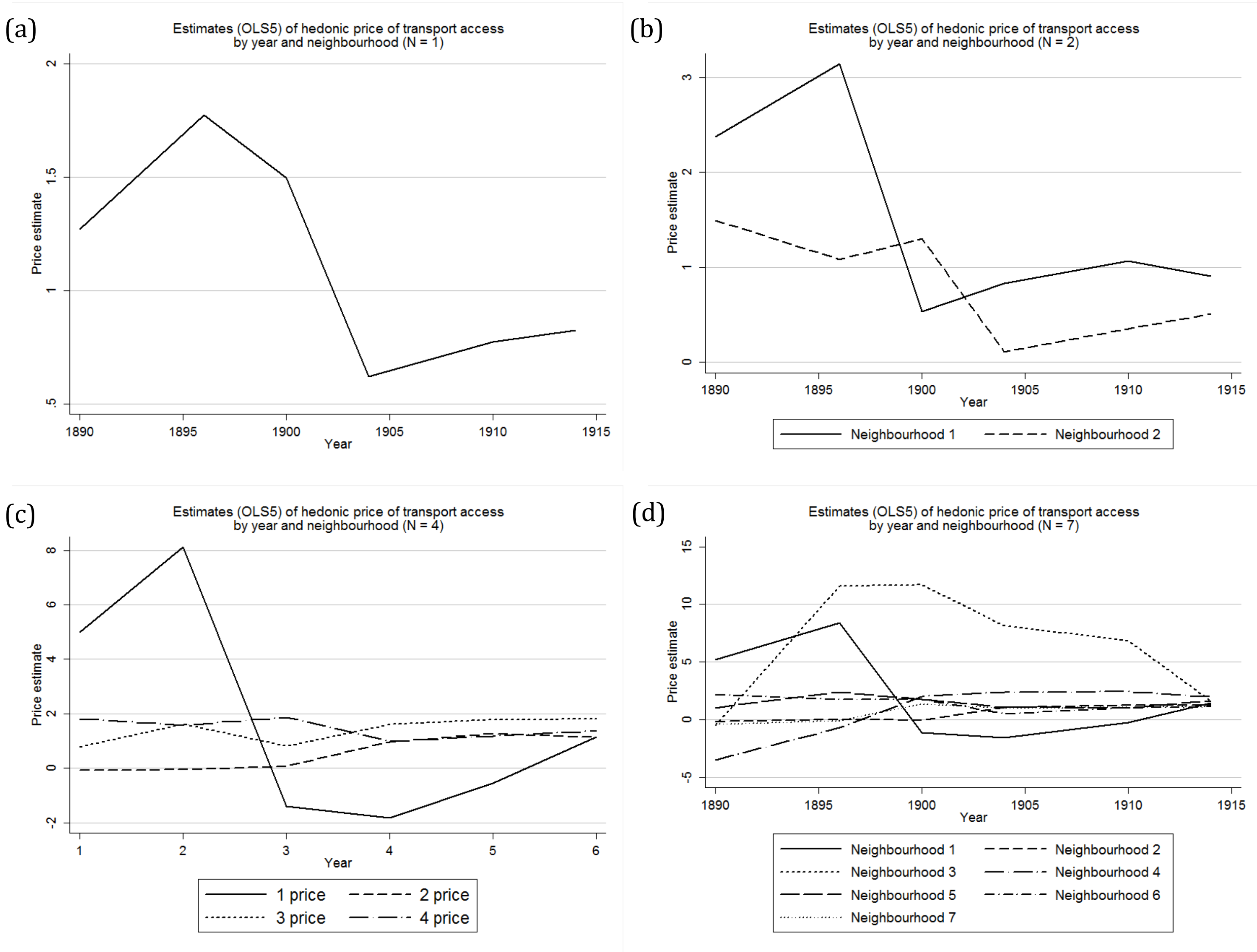
Table 2: Hedonic estimates of price of transport accessibility

	(1)	(2)	(3)	
			n=1	*n=2*
Station density	1.231***	1.271***	2.373***	-0.877***
	(0.021)	(0.040)	(0.059)	(0.069)
Station density ×1896	0.861***	0.503***	0.775***	-1.186***
	(0.023)	(0.044)	(0.084)	(0.103)
Station density ×1900	1.235***	0.224***	-1.839***	1.644***
	(0.023)	(0.044)	(0.076)	(0.096)
Station density ×1904	0.693***	-0.652***	-1.540***	0.154**
	(0.020)	(0.035)	(0.060)	(0.073)
Station density ×1910	0.518***	-0.497***	-1.305***	0.163**
	(0.020)	(0.035)	(0.059)	(0.070)
Station density ×1914	0.234***	-0.448***	-1.466***	0.481***
	(0.020)	(0.036)	(0.061)	(0.071)
Station dens. × Station dens.		-0.195***	-0.118***	
		(0.017)	(0.018)	
Distance to Track		0.045***	0.017	
		(0.008)	(0.012)	
Distance to CBD		-0.530***	-0.474***	
		(0.002)	(0.004)	
Distance to Ku'damm		0.047***	0.119***	
		(0.002)	(0.004)	
Distance to Green		-0.653***	-0.628***	
		(0.019)	(0.027)	
Distance to Water		-0.200***	-0.443***	
		(0.006)	(0.011)	
N	190,740	190,740	190,740	
Adjusted R^2	0.32	0.76	0.78	

Dependent variable is ln land value. Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Next I estimate hedonic prices of rail access that vary by neighbourhoods. I begin with the neighbourhood definition that comprises two neighbourhoods ($N = 2$). The results of this specification are presented in column (3). The station density estimates are divided into two columns where the coefficients in column (3: n=1) represent estimates for Neighbourhood 1 and (3: n=2) for Neighbourhood 2. The coefficients in (3: n=2) are all relative to the coefficients in the same row in (3: n=1) i.e. they are the coefficients on the variables in the left hand column interacted with the indicator variable Neighbourhood 2 (the baseline and omitted category in Neighbourhood1). For example, Neighbourhood 1 has a coefficient of 2.37 in 1890 and Neighbourhood 2 has a coefficient of -0.88 (relative to Neighbourhood 1). This means that Neighbourhood 2 has a hedonic price of 1.50 (calculated as $2.373 - 0.877$). The evolution of these two estimated hedonic price series over time is illustrated in Figure 9(b) below. I then estimate the model in a similar fashion for more numerous neighbourhoods. In order to save space the hedonic prices for versions with numerous neighbourhoods are not reported as tables. Instead, the estimates for 1, 2, 4 and 7 neighbourhoods are displayed in Figure 9. These plots illustrate the panel of hedonic prices. Similar panels were created for 13, 26 and 47 neighbourhoods but would be too crowded to display as line plots. These panel of the hedonic price of rail access across time in neighbourhoods of varying size are used in the next step to test for the law of one price.

Figure 9: Estimates of the hedonic price of rail access (N=1, 2, 4 and 7)



6.2 Stage two: unit root test of hedonic price differences

I estimate Blander and Dhaene's unit root test for price differences according to equation (15). The results of these tests for various neighbourhood sizes are illustrated in Figure 10 and Figure 11. Figure 10 illustrates the estimates for the unit root parameter φ and whether the null hypothesis ($\varphi = 1$) can be rejected at the 1% level. In each figure, the first unit root parameter is for the hedonic price series itself (not price differences) and this is always shown to be non-stationary[10]. The remaining estimates are based on the panel unit root test of price differences, but in each case changing the reference neighbourhood. This ensures that the results are not artefact of the choice of reference neighbourhood. For example, with $N = 4$, Figure 10(a) shows that in each case the null of non-stationarity is rejected in favour of convergence to LOP. This is indicated by the fact that the top of the bar (2% confidence band) around the point (phi estimate) falls underneath the dotted line at $\varphi = 1$. This represents a rejection of the null at the 1% level since it is a one-tailed test ($H_2: \varphi < 1$). The remaining charts of Figure 10 indicate that, on the whole, the unit root is rejected for all neighbourhood sizes. In some cases, there is dependence on the choice of base neighbourhood. In Figure 10(b) ($N = 7$) I fail to reject a unit root when Neighbourhood 3 is chosen as the base neighbourhood and in Figure 10(c) ($N = 13$) I fail when Neighbourhood 5 is the base neighbourhood. However these are the only two cases across all specifications where the null is accepted and therefore the majority of the evidence is in favour of convergence to LOP. A half-life can be computed from the phi estimate to give an idea of the speed of convergence[11]. If I average the phi estimates from models with different reference neighbourhoods then the half-life is calculated to be 1.36 years when there are four neighbourhoods, 1.39 years for $N = 7$, 1.20 years for N=13 and

[10] This result is not of particular relevance to the questions posed by this paper, however, it is interesting that hedonic prices share the property of non-stationarity that is typically the case with market prices. This result also rules out the possibility of testing LOP in the short run as explained in Section 2.

[11] This is calculated as $\frac{1}{2}\log(0.5)/\log(\varphi)$.

1.97 years for N=26. Overall, there appears to be no clear relationship between neighbourhood size and speed of convergence. However, notably, the longest convergence speed is measured for the smallest neighbourhood size definition. This could either reflect the fact that the neighbourhood in this definition rarely share a border and are can therefore be considered more spatially separated from one another than the larger definitions. It could also be simply that the smaller neighbourhood are more imprecise in a way that obscures the real convergence speed.

Finally I aim to distinguish between the absolute and relative versions by examining the individual fixed effects. Again I aim to obtain robust results by reporting results for every possible base location. Therefore there are $N-1$ fixed effects for each specification and a total of N specifications[12]. The fixed effects coefficients are displayed in Figure 11. The x-axis indicates which neighbourhood is used the reference neighbourhood for the price differences and the y-axis indicates the neighbourhood that the reported fixed effect is for. For example in Figure 11(a), the first column of coefficients reports the individual fixed effects estimated in the unit root test of price differences when Neighbourhood 1 is used as the reference. The coefficient for Neighbourhood 2 indicates that there is a constant -1.4 difference in the hedonic price between this neighbourhood and the reference neighbourhood (1). Significant coefficients are displayed with a black bar and insignificant with grey. So whilst there are reported differences between hedonic prices across neighbourhoods, they all statistically insignificant in the case of $N = 4$. This is evidence in support of the absolute version of LOP.

For the other neighbourhood sizes there are instances of significant fixed effects indicating the relative version holds in some cases. In total, however, these

[12] Note that the diagonal indicates the fixed effect for Neighbourhood n when Neighbourhood n is the reference and is therefore always zero since price differences from itself are always zero. All fixed effect above the diagonal mirror those below, in that they are equal and of opposite sign.

represent only 7.7% of the cases across all specifications[13]. As discussed in the theory, I do not necessarily expect price difference to converge to zero. There may be persistent differences in price as a result of differences in the marginal willingness to pay of individuals sorted across locations. Hence this result could merely reflect the fact that some locations have significantly different hedonic prices for rail access. On the other hand, the individual fixed effect are estimated using only a single series of price differences of only 6 time periods, hence, there is little power to reject the null of a zero coefficient. This means that in reality there may be far more instances of price differences between locations than I show statistically.

In summary, the results demonstrate that price differences are stationary in the vast majority of cases. The few instances when this is not true may be explained by poorly estimated hedonic prices, perhaps due to particular neighbourhood specific biases. It could also be that the neighbourhoods that do not exhibit convergence are somehow in reality different to the other locations. Perhaps they are subject to some regulations or rent control that means they are not adjusting flexibly to shocks to amenity levels. Overall, though, the majority of the evidence is in favour of convergence.

[13] In total there are 33 significant constants from a possible 430 estimated across all specifications. For N=4 there are no significant individual constants. For N=7, there is 1 significant from 21 parameters. For N=13, there are 8 from 78. For N=26, there are 24 from 325.

Figure 10: Unit root parameter estimates (Blander-Dhaene) for hedonic price of rail access

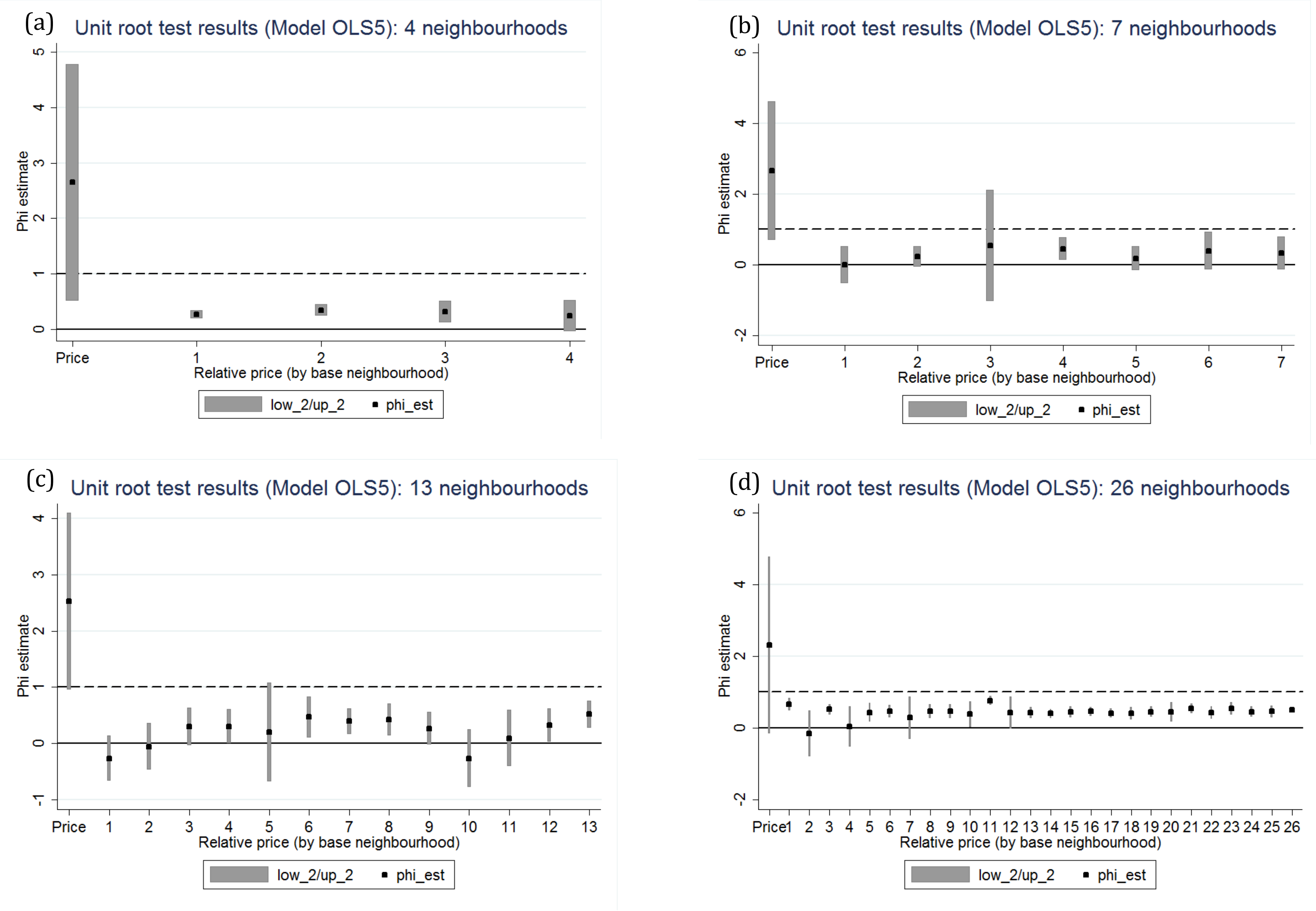
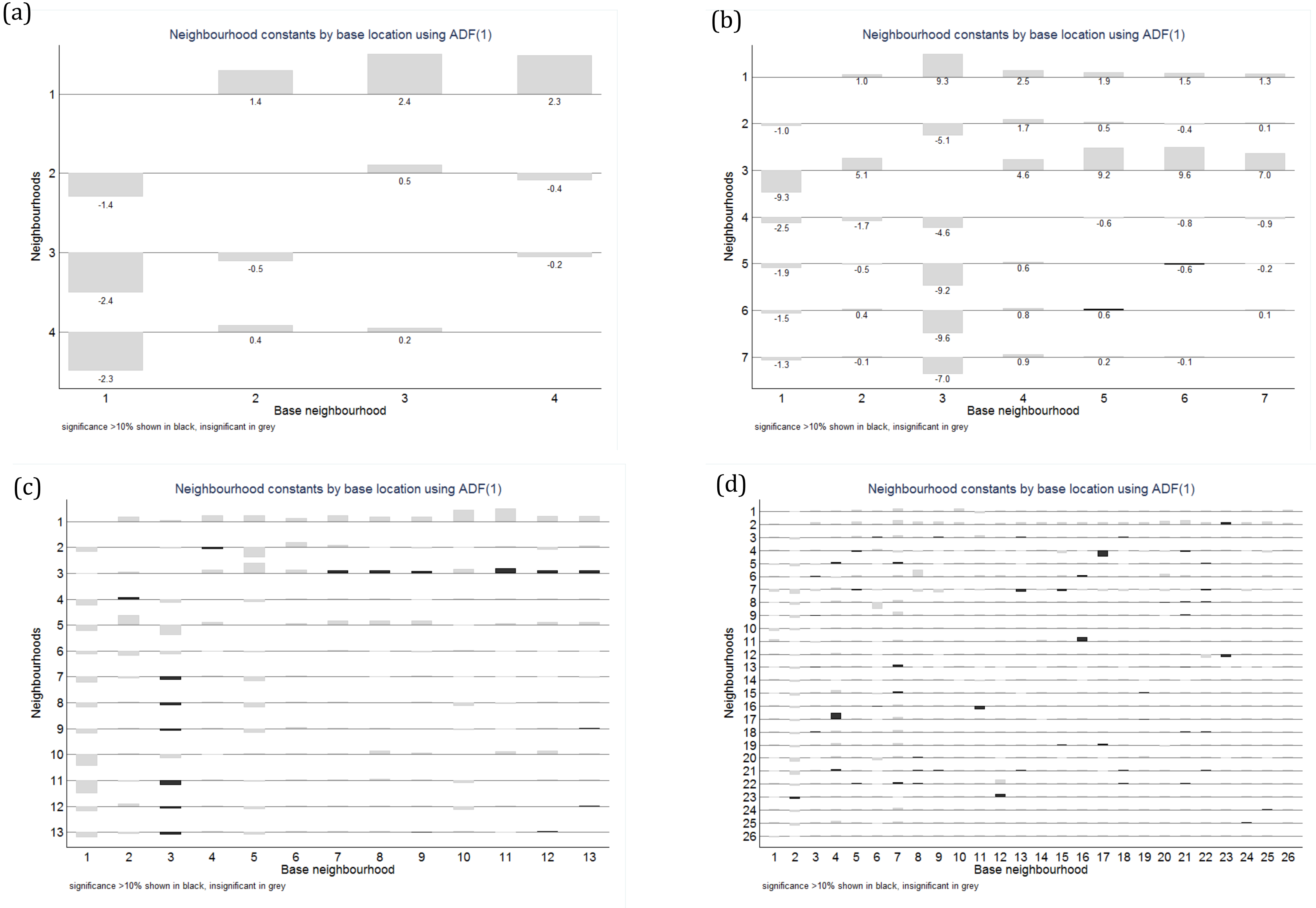


Figure 11: Individual constants from unit root test (Blander-Dhaene)



7. Summary and conclusions

This paper has asked whether the law of one price holds for hedonic prices. The literature on LOP has been reviewed for different interpretations of the law and appropriate methods and for testing whether it holds. I have highlighted that the LOP literature does not strictly require prices to be equal across location and identified the panel unit root test as the appropriate method for testing whether price differences converge across locations and for distinguishing between the relative and absolute versions of the law. I demonstrated Absolute-LOP should hold for hedonic prices only when utility functions are linear or log-linear and individuals are identical. In this specific case, spatial equilibrium is a necessary and sufficient condition for Absolute-LOP to hold. I have also explored the case of heterogeneous preferences arguing that LOP may still hold in this case but that it may more closely resemble Relative-LOP.

Using a panel dataset for Berlin (1890-1914) I found that differences in the hedonic price of rail access across different city locations converges to the law of one price (in percentage terms). This finding means that hedonic prices across locations are tied together in a long run equilibrium relationship. A secondary finding was that the individuals fixed effects from the panel unit root tests are insignificant in the majority of cases. This indicates that there is no persistent difference in hedonic prices of rail access across locations. This provides evidence for the absolute over the relative version of LOP. The key finding of this paper is that hedonic price differences across locations exhibit convergence. This is theoretically consistent with the existence of spatial equilibrium, providing some support to the assumption and results that rely on it.

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Appendix to Chapter II

1. Introduction

This appendix complements the main chapter and is not designed to stand alone or as a replacement. Each section provides additional material on the section from the main chapter with the same section number. Section 2 provides additional detail on the review of literature on the law of one price. Section 3 complements the theoretical discussion in the main chapter on whether hedonic prices should conform to the law of one price by looking at the case of heterogeneous preferences.

2. The Law of one price

This section complements the main chapter by providing a more detailed overview of the law of one price (LOP) and its alternative versions. This may be useful for readers interested in the broader literature or for clarification of anything not clear in the main text.

2.1 The international context

The law of one price in international markets implies that the price of goods sold in different countries must be equal when expressed in the same currency:

$$P = P^*E \tag{1}$$

where P is the price of a product in the domestic country, P^* is the price of the product in the foreign country and E is the (exogenous) exchange rate expressed as units of domestic currency per unit of foreign currency.

2.2 Transport costs

If I assume that the flow of goods is only in one direction (from the foreign to the domestic country) then LOP can be adapted to account for the costs of moving goods between locations:

$$P = P^{*}E + T \tag{2}$$

The price P (at the receiving location) is determined exactly by the price $P^{*}E$ (where the good is produced) plus the cost T of transportation between the locations. If the price difference $(P - P^{*}E)$ was above the transportation costs then more goods would be shipped, increasing the supply and bringing the price back down. In the opposite case where the price difference is below the transportation costs, rational agents would stop shipping goods until the P increased again. If I now allow for goods to flow both ways then the above relation becomes an inequality that applies in both locations:

$$P < P^{*}E + T \tag{3}$$

$$P^{*}E < P + T \tag{4}$$

The price differences in either direction must not be greater than the transportation cost. This means is a range of values within which price differences can fluctuate but if the price difference goes outside this band it triggers arbitrage bringing prices back together. Clearly then, the lower the transport/transaction costs, the more closely tied together the prices will be across locations.

2.3 Early empirical tests

This section cover the early empirical literature on LOP, which was focussed on testing the strong (short run) version. The short run version interprets the law literally, stating that prices across locations should be equal at all times. Price differences are not allowed to exist – since these would trigger arbitrage. (This is in contrast to the long-run version of LOP, covered in the next section, which allows price differences to exist but states they will be eliminated in the long run).

Taking logarithms of equation (1) gives $p_t = p_t^* + e_t$, where non-capitalised letters indicates logs of the capitalised letters. This can be empirically examined by estimating:

$$p_t = a + \beta_0 p_t^* + \beta_1 e_t + \varepsilon_t \tag{5}$$

with the null hypothesis that $a = 0$ and $\beta_0 = \beta_1 = 1$. Failure to reject this joint null means that LOP holds in the short run. This is a strict interpretation of LOP that implies that prices between countries are always equal such that deviations from this relationship are eliminated in the short run. This implies empirically that violations of the law are simply equal to a random error.

Using this form of examination, Richardson (1978) studies aggregated price indices for product groups (SIC classifications) traded between the US and Canada finding only weak evidence in support of commodity arbitrage. Isard (1977) examines price data for some highly traded export goods from the US, Japan, Canada and Germany finding violations of the LOP are large and persistent. Protopapadakis and Stoll (1983) find that LOP holds on average for a selection of countries over 1973-1980 but that there are some commodity-specific violations of the law. Frenkel (1980) employs a similar type of specification to test for LOP applied all goods – i.e. purchasing power parity (PPP) – finding that it performs badly in industrialised countries in the 1970s. Krugman (1978) also rejects the null hypothesis using an instrumental variables approach to account for endogeneity in price levels. Overall, the evidence from the early literature can be summarised as providing weak support for the law.

2.4 Long run LOP

The empirical failures of LOP and PPP raised concerns over their validity as a short run conditions. In particular, several explanations have been cited for why these conditions might not apply in the short run. Some authors have argued that LOP may not hold because in many cases the key requirements required for it to hold are not fulfilled. Spreen et al. (2007) argue that in many cases goods that appear homogenous actually exhibit systematic differences. For example agricultural goods sold in different countries may be different in terms of size, shape and taste even if belonging to the same product grade. Some things may differ that do not show up in the data such as package costs. Other authors argue that the assumption of an integrated market is often violated in studies of LOP. Both distance between markets and the existence of borders have been shown to have a significant impact on convergence rates (Engel & Rogers, 1994; Parsley & Wei, 1996, 2001). This can be explained transport costs, formal barriers to trade such as tariffs and quotas and informal barriers such as language or currency differences. In addition, there exist many real world factors that might make arbitrage more difficult than is hypothesised. If information is imperfect or if there are risks to arbitrage, rational actors may only begin to exploit violations of LOP where they are large and appear persistent. For these reasons it is thought that short-run LOP is unrealistic and that LOP is more likely to hold in the long run.

In addition to these theoretical concerns, the empirical approaches used in the early literature have been criticised for failing to take into account the time-series properties of the price data. Specifically, the prices series may be non-stationary. Engels and Granger (1987) demonstrate that linear regressions using non-stationary data can produce spurious regressions. Ardeni (1989) argues that even when the equations such as equation (5) are estimated in differences it does not fully deal with the problem of potential non-stationarity of the price series. Thus, the evidence from the early wave of empirical literature should be treated with caution. Confronted with these theoretical and empirical doubts, the literature began to use different empirical approaches that were based on a weaker (long run) interpretation of LOP. In these tests LOP is interpreted as an attractor equilibrium and price difference are tested for mean-reversion using a unit root test[14]. This version of the LOP is more realistic since it allows for possible short-run price deviations, plus it takes into account the non-stationarity of the data by taking differences across location.

Using this method of testing for mean-reversion, Rogers and Jenkins (1993) examine relative prices of 54 goods between the US and Canada. They look for the presence of a unit root using an ADF test. They cannot reject the null hypothesis of a unit for non-traded goods (e.g. haircuts) but there is more success for traded goods (e.g. potatoes). In PPP literature evidence of non-stationarity is fairly rare (Hakkio, 1984). Froot and Rogoff (1996) cite the main problem with these tests as being their low power to reject the null of a unit root against near-unit root alternatives. Frankel (1986) argues that failure to find evidence in support of PPP can be attributed to the to the fact that previously used datasets do not cover a long enough time periods to reject the null hypothesis of a random walk. He attempts to address this problem by using longer time series (1869-1984 dollar/pound exchange rate) and was able to reject the random walk hypothesis.

2.5 Panel unit root tests

In the most recent empirical literature, authors use panel data and associated methods in order to increase the power of the tests. Panel data tests are based on generating a panel of relative prices q_{it} for each location i:

$$q_{it} = p_{it} - p_{0t} - e_{it} \tag{6}$$

where e_{it} is the log of the exchange rate between location i and the reference location at time t, p_{0t} is the log price a product in the reference location at time t, and p_{it} is the log price in location i at time t. This results in a panel dataset of relative prices over $N-1$ locations and T time periods. This panel dataset of

[14] The methods of co-integration and error-correction have also been used in the LOP literature but are less common. See Froot and Rogoff (1996) for a detailed comparison of the different methods

relative prices is then tested together for the presence of a unit root via a panel unit root test, of which there is a great variety available. Most of these tests start by estimating an ADF-type test on the panel of price differences.

Further auxiliary regressions and transformations are implemented in order to make full-use of the information contained within the panel data. Depending on the steps implemented the different tests have different asymptotic behaviour of the time-series and the cross-sectional dimensions. For example the Levin-Lin-Chu (LLC) test performs well when N is between 10 and 250 and T is between 5 and 250. As well as asymptotic properties the tests also differ in other ways such as the assumptions used regarding the existence of cross-sectional dependence and the heterogeneity of the coefficients. These points can be illustrated by comparing two of the most popular panel unit root tests, the LLC and the Im-Pesaran-Shin (IPS)[15]. The LLC test imposes a homogenous autoregressive parameter such that the null hypothesis of $\varphi = 1$ implies that a unit root exists in all the series in the panel. This null hypothesis is very restrictive. Furthermore it relies heavily on the assumption of cross-sectional independence. The IPS test, on the other hand, allows for individual autoregressive parameters and tests the null hypothesis $\varphi_i = 1$ for every series such that a rejection of the null can come from the rejection of a unit root in any of the series.

In addition, the estimation of ADF-like models (with individual constants) are subject to a bias first identified by Nickell (1981). The Nickell bias is a well-known effect that occurs when using small-T time-series data that means the lagged dependent variable is correlated with the error term. The Arellano and Bond (1991) estimator and others have been proposed to deal with this bias when panel data is stationary. However, this requirement of stationarity clearly rules out the Arellano-Bond estimator as a solution that can be applied to unit root tests since the aim of a unit root test whether a series is stationary or not.

[15] These tests are suggested by Levin et al. (2002) and Im et al. (2003).

Therefore panel unit root tests must apply an adjustment factor to correct for this bias.

It is noted that whilst the panel cointegration methods are sometimes used in the literature (Jenkins & Snaith, 2005) they are of less popularity. Funke and Koske (2008) consider unit root tests to be stronger than cointegration tests since homogeneity and proportionality are directly imposed in the former. Further, when using panel data the cointegration approach is complicated where the existence of potential cross-sectional cointegration is considered (Banerjee et al., 2004; Breitung & Pesaran, 2005).

Parsley and Wei (1996) estimate ADF-type panel unit root tests for 51 goods and services across 48 cities. They do not include constant or trend terms and are able to reject the random walk null for the majority of products. They generate relative prices using New Orleans as the reference location but do not find the results differ much when using New York instead. (Goldberg and Verboven (2004), 2005)) find strong evidence for price convergence in the EU car market using the LLC and the IPS panel unit root tests. They find a half-life of convergence to be around one and a half years. In their 2004 and 2005 papers they choose the Netherlands and Belgium, respectively, as reference countries for the relative prices. In both papers, they state that conclusions are robust with respect to choice of base country. Funke and Koske (2008) employ panel unit root tests to examine the validity of LOP in EU countries. They employ the LLC, the IPS and the Maddala and Wu (1999) panel unit root tests testing 90 different product groups from 25 countries. They find the law holds better for the first 15 member countries than when the sample in increased to include the 10 members that joined in 2004. Blander and Dhaene (2012) also examine the EU[16] car market as

[16] Factors such as trade barriers, exchange rate volatility and transportation costs and are often cited as causes for the failure of LOP. Therefore it is common to test for LOP across European Union (EU) countries where these problems should be mostly mitigated due to the integration of the market, the common currency and the relatively limited geographical extent. All these factors should make arbitrage easier and hence more likely that LOP holds.

a case study for their new unit root test. Like the Harris and Tzavalis (1998) test, their test has particularly good properties for short panels. They adapt the Harris-Tzavalis test to allow for an AR(1) structure. Blander and Dhaene (2012) find evidence in support of convergence to the LOP in EU car markets with a reported half-life for convergence of 0.898 years.

This section has examined the literature on LOP and its equivalents such as PPP. Three interpretations of LOP have been identified in the empirical literature: strong, weak-absolute and weak-relative. It is important to note that the empirical literature is not directed towards demonstrating categorically that prices are equal across locations. As such the empirical approach has developed to test whether prices exhibit convergence that is consistent with the operation of arbitrage processes. Therefore, the empirical tests outlined here may remain relevant to the case of hedonic prices even though they are not necessarily equal across urban locations. I will make use of the last panel unit root test mentioned above (the Blander-Dhaene test) since it is suitable for short panels.

3. Should hedonic prices adhere to the law of one price?

This section complements the main chapter by giving a more detailed overview of Rosen (1974) theoretical framework of equilibrium hedonic prices and by giving a more detailed discussion of the case of heterogeneous preferences.

3.1 Hedonic price determination

Hedonic theory is based on the idea that the market price of a composite good such as housing or a car, reflect the value of the attributes they embody. For example the value of a house might reflect attributes such as the number of bathrooms, the architectural design or its proximity to shops. And the value of a car might reflect the number of doors, its top speed and its overall design. Hedonic prices of attributes are therefore interpreted as their contributory value to the overall price of the composite good. Rosen (1974) provides the theoretical framework for the determination of hedonic prices in spatial equilibrium.

Consider a vector z that describes a bundle of N utility-bearing amenities of land at a location:

$$z = (z_1, z_2, \ldots, z_N) \tag{7}$$

I depart from Rosen (1974) by assuming, for simplicity, that individuals consume land directly. This bypasses the housing supply side of the model and entails that locational amenities are given exogenously. Plots of land are characterised by a vector of amenities $z = z_1, z_2, \ldots, z_n$. The hedonic price function gives the price of land as a function of these amenities:

$$p(z) = p(z_1, z_2, \ldots, z_N) \tag{8}$$

The partial derivatives with respect to each amenity, denoted $p_1, p_2, \ldots, p_N$, are the hedonic prices of those amenities. The hedonic price function may be linear or non-linear and include complementarities between characteristics. Econometrically, hedonic prices can be estimated via hedonic regression which involves the regression of the price on characteristics. Individuals may discover this hedonic price function by comparing the prices and amenity levels of land observed in the market place.

Individual utility is a function of consumption good x, the price of which is the numeraire, and amenities z. Individuals have a budget constraint $y = x + p(z)$, where y is their exogenous income. Individuals' utility is a function of consumption good x and the amenities z:

$$U = U(x, z) = U(x, z_1, z_2, \ldots, z_N) \tag{9}$$

They maximise utility by choosing a location (i.e. a bundle z). Individual bid functions θ describe the maximum amount they are willing to pay for land with given attribute levels in order that they achieve given utility level u:

$$U(y - \theta, z_1, z_2, \ldots, z_N) = u \tag{10}$$

The bid function is therefore given by $\theta(z;u,y)$. Individuals take the hedonic prices $p(z)$ to be exogenous to their consumption decision. The optimal choice is determined by the first order conditions, which for the n-th amenity is:

$$\frac{dp(z_n)}{dz_n} = p_n = \rho_n = \frac{U_{z_n}}{U_x}, \qquad n = 1, 2, \dots, N \tag{11}$$

This means that individuals choose a location where the hedonic prices of each attribute n are equal to their marginal rates of substitution ρ_n for those attributes. This means that individuals are located on their highest possible indifference curve when their own bid function is tangential to the hedonic price function for each attribute. Solving the maximising decision for each individual gives us the demand at each attribute level for any given set of hedonic prices. If there is a build-up of demand for a particular attribute level then that level is under-priced and the hedonic prices must rise until the demand matches the available supply. Spatial equilibrium, therefore, is given by the set of hedonic prices that that equalise demand with the exogenously given supply at each attribute level.

3.2 Heterogeneous preferences

This section quite closely follows the analysis laid out by Rosen (1974). I depart from Rosen (1974) only by assuming land is consumed directly i.e. that there is no housing supply sector and that the supply of amenities is therefore given exogenously. Utility functions are linear in amenities and the consumption good and there is only a single amenity:

$$U(x, z_1) = x + \rho_1 z_1 \tag{12}$$

Maximisation occurs when individuals choose a location (i.e. an amenity level z_1) where the hedonic price for the amenity is equal to the marginal rate of substitution $p_1 = \rho_1$.

Following Rosen (1974), I make some assumptions about the distribution of preferences and amenity amounts available in the market. The following function

f gives the exogenous distribution of population across marginal rates of substitution (for the amenity z_1):

$$f(\rho) = b\, d\rho \qquad \rho_s \leq \rho \leq \rho_l \tag{13}$$

where ρ_s and ρ_l give the smallest and largest marginal rates of substitution in the population and b is a constant. This implies that there is an equal distribution of the preferences between the smallest and the largest marginal rates of substitution. Combining the distribution of preferences and the maximisation condition gives the quantity demanded for each amount of the amenity: $Q^d(z)\, dz_1 = b \frac{d^2 p(z_1)}{d{z_1}^2}\, dz_1$. Next I define an exogenous distribution g of locations at each levels of amenity z_1 to be:

$$g(z_1) = k\, dz_1 \qquad z_{1s} \leq z_1 \leq z_{1l} \tag{14}$$

where z_{1s} and z_{1l} give the smallest and largest levels of amenity z_1 in the urban area and b is a constant. This implies that there is an equal distribution of the amenity between the smallest and the largest amounts. The quantity supplied across amenity amounts is therefore: $Q^s(z_1)\, dz_1 = k\, dz_1$. Spatial equilibrium requires demand equals supply at every amenity level. Putting the quantity supplied equal to the quantity demand gives:

$$\frac{d^2 p(z_1)}{d{z_1}^2}\; dz_1 = \frac{k}{b}\, dz_1 \tag{15}$$

I distinguish between two scenarios: firstly, homogenous preferences where $\rho_{1s} = \rho_1 = \rho_{1l} = \bar{\rho}_1$ and, secondly, heterogeneous preferences where $\rho_{1s} < \bar{\rho}_1 < \rho_{1l}$. The first scenario is outlined in the main chapter and the spatial equilibrium hedonic price is simply equal to $p_1^* = \bar{\rho}_1$. In this case the hedonic price is equal to the common marginal rate of substitution at all locations since this price ensures individuals are indifferent between all locations. The hedonic price difference between locations i and j is of course zero:

$$p_{1i}^{*} - p_{1j}^{*} = \bar{\rho}_1 - \bar{\rho}_1 = 0 \tag{16}$$

This suggests that in the case of identical individuals and linear utility functions, spatial equilibrium is a necessary and sufficient condition for a law of one price for hedonic prices. In the second scenarios the spatial equilibrium hedonic price of amenity z is a more complicated due to sorting of individuals[17]:

$$p_1^{*} = \rho_{1l} - \frac{k}{b}(z_{1l} - z_1) \tag{17}$$

Here the hedonic price depends on the amount of the amenity and hence location. The hedonic price at the location with the largest amenity amount (where $z_1 = z_{1l}$) is equal to the largest marginal rate of substitution of the population $p_1 = \rho_{1l}$. The hedonic price declines with decreases in the amount of the amenity ($z_1 < z_{1l}$) at a speed determined by the relative distributions of preferences and the amenity ($\frac{k}{b}$). This is because the relative distributions determine the particular pattern of sorting and therefore the particular preferences of the individuals associated with each amenity amount. The hedonic price difference between locations i and j is:

$$p_{1i}^{*} - p_{1j}^{*} = \frac{k}{b}(z_{1i} - z_{1j}) \tag{18}$$

Therefore the difference in prices is equal is determined by the difference in amenity amounts and the entire distributions of preferences and of the amenity. Holding these distributions fixed the hedonic price differences across location

[17] These results require use of the boundary conditions from Rosen (1974). In particular, the upper boundary condition states that the highest amenity level is consumed by individuals with the highest marginal rate of substitution, or $p_1^{*}(z_{1l}) = \rho_{1l}$. The lower boundary can be described by three alternative conditions, of which I use the third since there always exists some positive amount of the amenity. This condition states that the hedonic price at the minimum level of the amenity individuals must ensure individuals are indifferent between consuming that level and not consuming at all, or $[y - p(z_{1s})]/\, z_{1s} = p_1^{*}(z_{1s})$.

can be compared to the law of one price in relative terms. Deviations from this fixed difference in price should be met with adjustments. But, as discussed, an important source of deviations is local shocks to amenity levels. These may lead to temporary disequilibrium but would also impact on the condition stated above, altering the new equilibrium relationship. This means that it is not clear whether observed movements in price differences are movements around the equilibrium or simply movements in the equilibrium itself. The degree to which this will obscure convergence behaviour depend on the relative volatility equilibrium relationship itself compared with movements towards and away from the relationship. This is discussed in detail in the next section.

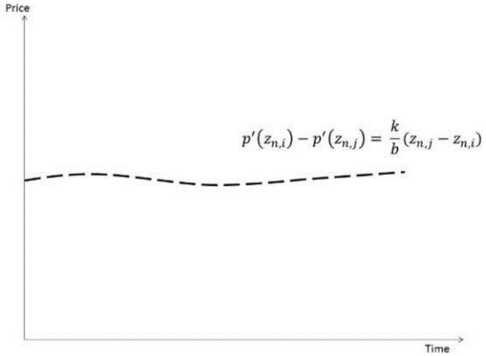
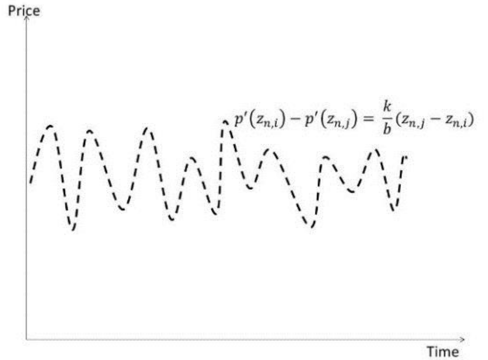
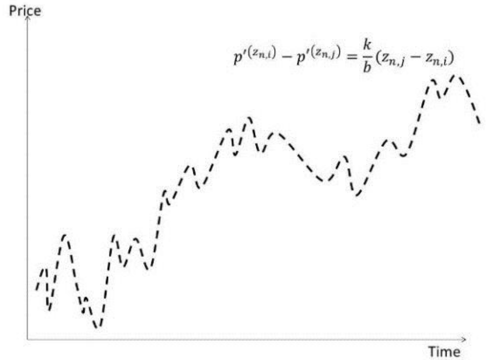
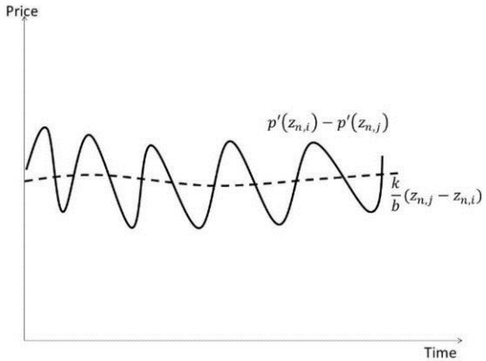
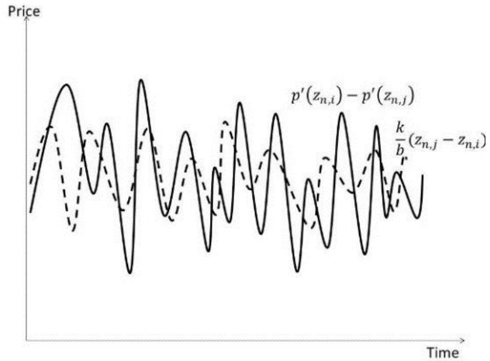
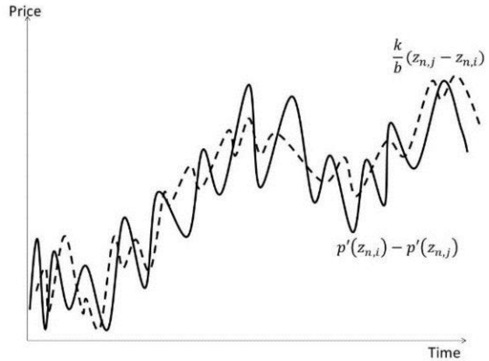
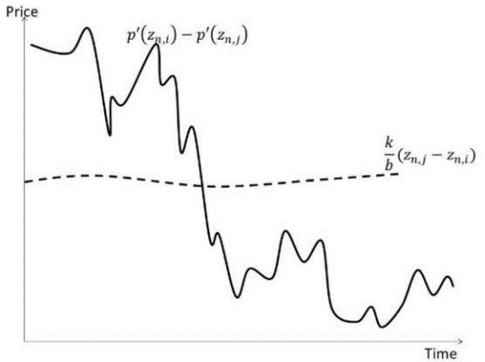
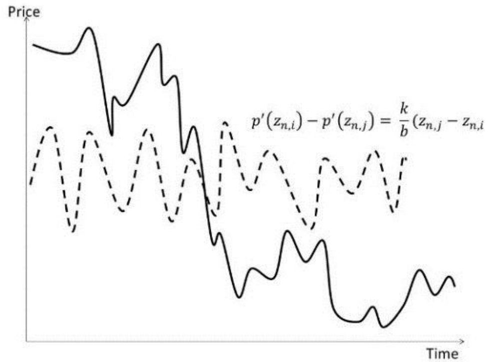
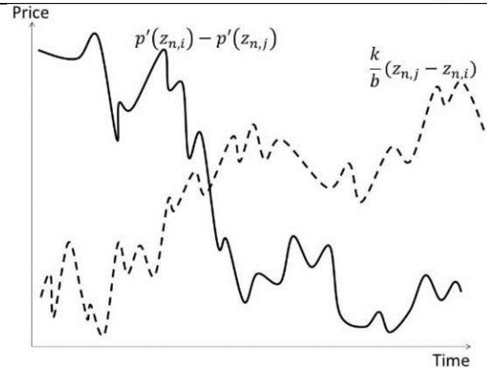
3.3 Relevance of LOP tests under heterogeneous preferences

Given the model of linear utility functions and heterogeneous individuals presented above, it may be difficult to distinguish movements in prices around the equilibrium from movements in the equilibrium itself. The set of diagrams in Figure 1 below illustrates some different possibilities. The three columns indicate scenarios for movements in the equilibrium relationship; either the equilibrium is (1) relatively steady over time (non-volatile), or it is volatile and either (2) stationary or (3) non-stationary. The equilibrium is shown by the dotted line on all charts. The rows correspond with different degree of spatial equilibrium in reality. In row (a) spatial equilibrium hold in the short run, therefore, price differences are equal to equilibrium relationship (the dotted line). In row (b) equilibrium holds in the long and the observed price differences (the solid black line) converge to the equilibrium over time. In row (c) there is no spatial equilibrium and the price difference are a random walk that bear no relation to the equilibrium.

The can be used to illustrate scenarios when the stationarity of the price differences is misleading with respect to the type of spatial equilibrium that holds in reality. When spatial equilibrium hold in the short run, I will find price differences to be stationary in 2 cases but not when the equilibrium is non-stationary. This will lead to a Type I error. When SE holds in the long run prices may be found to be stationary, if the equilibrium is very volatile and/or non-stationary. This is also a potential Type II error. Where spatial equilibrium does not hold, the type of movements in the equilibrium do not affect the price differences, therefore it cannot drive stationarity. The instability of the equilibrium cannot drive a Type II error since if there is no spatial competition then price differences are unrelated to their equilibrium level.

If the equilibrium relationship itself is very unstable relative to the process of shocks and convergence then there is a high likelihood of committing a Type I error and rejecting SE even though it does hold. However, there does not appear to be a scenario in which changes to the equilibrium level could lead to a Type II error, where I fail to reject stationarity even though SE doesn't hold.

Figure 1: Illustration of adjustments to equilibrium and shifts in the equilibrium

	1. Equilibrium not volatile	2. Equilibrium volatile: stationary case	3. Equilibrium volatile: non-stationary case
(a) Short -run			
(b) Long -run			
(c) None			

8. Literature

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CHAPTER III

GENTRIFICATION AND DISPLACEMENT IN ENGLISH CITIES

1. Introduction*

Gentrification is the phenomenon of a large and relatively sudden in-migration of wealthy or middle class residents into a previously poor or working class neighbourhood. Following the suburbanisation of previous decades, gentrification marks the most recent major trend in the history of cities in industrialised countries. Gentrification is also a very controversial topic. For example, Lees et al. (2013) refer to the phenomenon as a “battleground in urban geography”. The proponents of gentrification refer to it as urban revitalisation and consider it the reversal of ‘white flight’ and urban decay. In support of this view is a literature on neighbourhood effects that suggests economic outcomes of households may be improved by living in neighbourhoods with overall better outcomes (e.g. Buck, 2001). As such, certain types of policy attempt to actively gentrify neighbourhoods, sometimes by making physical improvements to the dwelling stock (e.g. Housing Market Renewal in the UK). The opponents of gentrification, however, suggest that if the original residents are displaced then they are not able to benefit from any neighbourhood effects that do exist[18]. Displacement itself represents a huge cost to the displaced households (Slater, 2009) and original residents that remain in the neighbourhood may feel alienated by the changes to neighbourhood’s character (Lees et al., 2010).

The issue of displacement is central to the debate, and whether gentrification displaces original residents drastically changes how the phenomenon should be viewed and dealt with by policy. If residents are displaced then it becomes

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[18] In fact, the emerging evidence from field experiments that shows households do not benefit from living in wealthier neighbourhoods (Ludwig et al., 2013; Oreopoulos, 2003).

difficult to motivate policies that actively gentrify neighbourhoods with the suggestion that they are beneficial for original residents. Furthermore if the negative consequences of gentrification are to be avoided then policies should aim to provide more social protection for incumbent residents.

Whilst gentrification is an important recent trend, whether it actually causes displacement remains an open question. In fact, research on the effects of gentrification is characterised by a theoretical-empirical divide. The theoretical literature in urban economics describes gentrification as a process of outbidding (Brueckner & Rosenthal, 2009; Brueckner et al., 1999). An increase in amenities or preferences of a rich group for an urban neighbourhood leads to a shift in the bid rent to above what the poor group are willing and able to pay. The poor group are outbid and must relocate to the periphery in a large scale displacement. However, the (small) empirical evidence to date finds no evidence of displacement (Freeman, 2005; Freeman & Braconi, 2004; McKinnish et al., 2010; Vigdor et al., 2002). By examining exit rates in gentrifying neighbourhoods compared with non-gentrifying neighbourhoods this literature finds that low-status households are not more likely to exit under gentrification.

This paper provides new evidence on the question of whether gentrification leads to displacement. I use the British Household Panel Survey (1991-2008) and the UK Census (1991, 2001 and 2011) to examine the association between gentrification and displacement in English cities. I use the change in degree share to capture different speeds of gentrification and interact this with the initial degree share to capture effects at different stages of gentrification. I relate this measure of gentrification to neighbourhood exits at the household level. I argue that previous estimates of displacement deal unsatisfactorily with differences natural mobility rates due to the sorting of households across different neighbourhoods. My empirical strategy takes several steps to deal with these unobserved differences, including the estimation of a neighbourhood fixed effects model and the interaction of the gentrification treatment with household characteristics that indicate vulnerability to displacement. In particular I compare renters with homeowners and private renters with social renters. These interactions help compare exit rates of affected with unaffected groups to isolate the displacement effect. The findings indicate that gentrification is associated with significant displacement of low income renters especially in the early stages of the process. The displacement effect is shown to be greatest for private renters.

This paper makes several contributions to the literature. The estimates presented here are the first robust evidence on displacement for English cities and the first to estimate displacement effects at different stages of gentrification and at different income levels. The empirical strategy makes several improvements over the existing literature. Finally, the evidence presented here is contradictory to the prevailing evidence on displacement and is more consistent with the theoretical understanding of gentrification as process of outbidding (e.g. Brueckner & Rosenthal 2009).

This research is relevant to a number of areas of the literature. It contributes directly to the literature that estimates the displacement effect of gentrification (Freeman, 2005; Freeman & Braconi, 2004; McKinnish et al., 2010; O'Sullivan, 2005; Vigdor et al., 2002) and that on broader empirical issues related to gentrification (e.g. Ahlfeldt, 2011a; Bostic & Martin, 2003; Ellen & O'Regan, 2008; Helms, 2003; Vigdor, 2010). It also relates to the theoretical literature that describes gentrification (e.g. Brueckner & Rosenthal, 2009; Brueckner et al., 1999; Guerrieri et al., 2013; O'Sullivan, 2005; Rosenthal, 2008) and patterns of residential income segregation (e.g. Glaeser et al., 2008; LeRoy & Sonstelie, 1983; Tivadar, 2010; Wheaton, 1977). Finally it contributes, particularly from a policy standpoint, to the literature that evaluates physical regeneration efforts (e.g. Collins & Shester, 2013; Richter et al., 2013). The paper structure is as follows. Section 2 reviews the existing empirical literature on the displacement question, highlighting key empirical challenges and results. Section 3 outlines the BHPS and UK census data used in this analysis. In section 4 I construct the empirical strategy with a focus on addressing mobility differences. Section 5 presents the results and section 6 concludes with some policy recommendations.

2. Empirical Literature

Empirical studies on whether gentrification is associated with displacement typically define gentrification based on increases in neighbourhood income or educational attainment. It is then examined whether there is a statistical relationship between this measure and the mobility rates of existing residents. Freeman (2005) points out that earlier studies tended to suffer from methodological flaws such as failing to include in the analysis a counterfactual group of neighbourhoods that did not gentrify i.e. examining only time variation for neighbourhoods that did gentrify. This means that it is impossible to tell whether the observed displacement rates in gentrifying neighbourhoods is in fact any higher than the rate in non-gentrifying neighbourhoods. Freeman (2005) and Vigdor et al. (2002) both provide good reviews of these early empirical studies. In this literature review I focus on four of the most recent studies that are the most methodologically robust. These are Vigdor et al. (2002), Freeman and Braconi (2004), Freeman (2005) and McKinnish et al. (2010). From this review I will identify both key results, particularly regarding whether gentrification leads to displacement, and key empirical issues such as important control variables and identification strategies.

Typically, studies into gentrification and displacement make use of two data sources. One for households that gives a dependent variable relating to exit or mobility rates, and one for neighbourhoods that allow for characterising gentrification, usually in terms of income growth or educational attainment growth. Vigdor et al. (2002), for example, make use of the American Housing Survey (AHS) for 1985-89 and 1989-93 and the Public Use Microdata Sample (PUMS) for the Census (1980-1990). The dependent variable is whether a housing unit from the AHS still holds the same household at the end of the period that is did in the beginning. Gentrification is then defined as neighbourhoods that experience an increase in the share of population that hold a degree of more than 50% above the average for the Metropolitan Statistical Area (MSA).

The mobility variable is then regressed on the gentrification variable usually in either a logit or a probit model. This empirical strategy[19] is intended to address the difficulty involved with showing actual displacement rather than simply mobility. If it can be shown that the mobility rates are higher in the gentrifying neighbourhoods than in the other neighbourhoods, and that the higher rates can be attributed directly to the gentrification, then this can be taken as evidence for displacement. The important caveat is that it must be shown that the higher rates are due to the gentrification and not to other factors that may be different between neighbourhoods. Neighbourhoods that gentrify are likely to be different from neighbourhoods that do not gentrify, for example, they may have fewer social housing units. Social housing units have different characteristics to other units that will directly affect the exit rates of the households that live in them. Also, different housing characteristics will attract different types of individuals who have different baseline mobility rates. Therefore a lower exit rate in a non-gentrifying tract may not be directly related to the fact that the neighbourhood in not gentrifying but to something else entirely, such as the proportion of social housing in that neighbourhood.

Important controls for differences in householder characteristics are things such as age, education, income, tenure, number of years at current residence, ethnicity, nationality, marital status and employment status. Household and neighbourhood characteristics that have been shown to be important factors related to mobility are household size (particularly in relation to number of rooms), maintenance deficiencies, rent subsidies, rent control, public housing complexes and vacancy rates (Vigdor et al. 2002, Freeman and Braconi 2004, Freeman 2005). Vigdor et al. (2002) also controlled for the householder's own rating of the neighbourhood and particular housing unit they live in.

[19] Vigdor et al. (2002) was one of the first empirical studies on gentrification and displacement to compare mobility rates in gentrifying neighbourhoods to a counterfactual group of non-gentrifying neighbourhoods.

Another important empirical issue is to compare gentrifying neighbourhoods to similar neighbourhoods that did not gentrify. Vigdor et al. (2002) and Freeman and Braconi (2004) control for various household characteristics, but they compare mobility in gentrifying neighbourhoods to mobility in all other neighbourhoods. On the other hand, Freeman (2005) and McKinnish et al (2010) provide more plausible counterfactuals by first selecting a sample of neighbourhoods that might have potentially undergone gentrification and then comparing the ones that did to the ones that didn't. For Freeman (2005) the neighbourhoods must be central city areas, with a comparatively (compared to MSA median) low median income and a comparatively low share of housing built in the last 20 years. The gentrifying neighbourhoods are then the ones that experienced a comparatively large increase in educational attainment and an increase in real housing prices. For McKinnish at al. (2010) the potential neighbourhoods must be both urban and in the bottom quintile in terms of median household income. The gentrifying ones are defined as those that experience at least a $10,000 dollar increase in mean household income.

A further issue is that unit of analysis. If *households* are observed, as in Freeman (2005), it is possible to examine whether they exit the neighbourhood that is gentrifying. However, if *housing units* are observed (Vigdor et al. 2002, Freeman and Braconi, 2004) then it is only possible to say if the household left the unit and nothing about how far it went. This makes it impossible to tell whether the household actually exited the area that is gentrifying. Hence, the claim that empirical analysis is testing the relationship between gentrification and displacement is made weaker by this fact. Finally, McKinnish at al. (2010) only use one data source (confidential US Census data) to characterise both exits and gentrification. As a result they are neither able to say where households move to, or in fact, whether any specific household has moved at all. Instead they use a less reliable cohort analysis that looks at the populations and characteristics of individuals who report to have stayed in the neighbourhood for at least ten year compared to groups from the previous census with similar characteristics who are ten years younger, with the intention that they are the sample people. Thus when they find that the income of a particular group tends to increase more in gentrifying neighbourhoods than in non-gentrifying neighbourhoods they are not able to say whether this is because the households toward the lower income distribution in this group left the area or because there has been a general increase in income across all the households of this group.

Also important is the size of the areas used to classify gentrification. In the two earlier papers by Vigdor et al. (2002) and Freeman and Braconi (2004), the areas used are too large to be referred to as neighbourhoods. Vigdor et al. (2002) look at AHS Zones in Boston, which are of around 100,000-200,000 people in size. The city of Boston itself is made up of only 5 zones. Freeman and Braconi (2004), in their study of New York also use areas of around 100,000 in population. These large areas are problematic for several reasons. Gentrification is an urban phenomenon but since there were only 5 areas for the city of Boston, Vigdor et al. (2002) are forced to expand his analysis to the surrounding suburbs and county in order to make sufficient sample size. Even including these, the spatial variation in the gentrification variable is rather coarse. In Vigdor et al.'s (2002) 'exclusive' definition, only one area is defined as gentrifying and in his 'inclusive' definition there are only a few more. Freeman and Braconi (2004) have only seven gentrifying areas (selected using anecdotal evidence) from a total of 55 areas. Since gentrification is a highly localised phenomenon, using large areas means that for any household the gentrification indicator for their area may not be a very reliable reflection of whether they are in a gentrifying neighbourhood or not. Also, these aggregate areas has important implications for the standard errors of the estimates that should be clustered at the area level (this was correctly implemented only by McKinnish et al. 2010). Finally, using smaller areas allows for a more precise indication of whether a household move actually exits the area that is gentrifying (if one is using a household survey). Freeman (2005) and McKinnish et al (2010) both use much smaller Census tracts of around 1,000-8,000 people and their samples also cover the whole of the US.

Before turning to review the results of these papers, I examine one last empirical issue that is the conditioning of the gentrification effect on other factors. It is not sufficient to add as controls the factors that are thought to have a significant effect on the relationship between gentrification and displacement. For example in gentrifying areas, particular groups such as renters or the low income may be more susceptible to being displaced than other groups. Homeowners are protected from the escalation of rent prices that accompanies gentrification and an increase in the price of their home brings no extra costs until the point of sale[20], where the costs will only represent a fraction of the overall benefits associated with selling at a higher price. Hence the gentrification variable can be interacted with various household characteristics to reveal conditional effects. Vigdor et al. (2002) look at the effect conditional on educational attainment (high school diploma) of the head of household. Freeman and Braconi (2004) do not estimate an interacting variable but restrict their sample to either low income or low education householders. Freeman (2005) looks at the effect for a group defined as 'poor renters', who have both a low income and are renters.

The results of the papers I examine here generally find no evidence of displacement as a result of gentrification. Vigdor et al. (2002), in fact, find after introducing controls that households are *more likely* to stay say in their housing unit if they live in gentrifying areas in Boston. In another specification they finds this to be true only for low educated householders. There is no evidence found for displacement for any group. Freeman and Braconi (2004) for New York in the 1990s also find slower residential turnover for poor and less educated households in areas that are undergoing a process of gentrification compared to other areas. Freeman (2005) is the only paper in this review that does find evidence for displacement, but not a significantly higher effect for the poor renter group. Finally, McKinnish et al. (2010) do not claim to find evidence for displacement although admit that there is some ambiguity in the interpretation of their results due to the methods used. The overall empirical evidence is not in favour of gentrification being associated with displacement. However, the analyses do have a lot of empirical problems and therefore something new may

[20] In the UK, a tax called stamp duty is applied at the point of sale and represents a percentage of the transaction price. There are no increased costs in terms of tax assessment associated with owning a property of a higher value; therefore, there is no displacement pressure on low income homeowners in gentrifying areas.

be learned from an analysis that improves in the highlighted areas. Also, the paper that appear to suffer the least from methodological issues, Freeman (2005), does find some evidence for displacement suggesting that there may indeed be an effect.

Table 1 below also provides a summary of the literature I have reviewed here in terms of all the important aspect identified. I will now recap the important issues learnt from the review of the literature and therein lay out the criteria that this paper should meet. Firstly, the analysis must include appropriate household and neighbourhood control variables. This paper therefore matches the controls used in all the previous paper and adds some further controls. The identification strategy in general will be improved by implementing ward fixed effects. This specification will eliminate the unobservable bias due to fixed difference in wards by estimating the displacement using only time variation in gentrification in each ward. Secondly, the areal unit must be sufficiently small. Hence I will work with Census wards, which have a population of around 6,000-7,000 and are roughly comparable to smallest neighbourhoods used in previous analyses. Thirdly, it is best to work with household data. Hence I use the BHPS to identify household exits from neighbourhoods over the period 1991-2008. Fourthly, it is important to identify an appropriate control group of potentially gentrifying neighbourhoods. I intend to build on this further by introducing a flexible definition of gentrification that compares the effect of increases in degree share conditional on the initial level of degree share. This is advantageous in that it measures the effect with respect to the magnitude of the gentrification (rather than a binary variable) and also conditions the effect on how gentrified the neighbourhood is already. Fifthly, further interacting relationship may yield interesting insights and help control for unrelated differences in mobility rates. I therefore intend to interact the main relationship with both tenure and income as well and tenure interacted with income. In the next section I examine the data that will be used in the analysis.

Table 1: Review of literature

	Vigdor (2002)	Freeman and Braconi (2004)	Freeman (2005)	McKinnish et al. (2010)
Case	Boston (1985-93)	New York City (1991-1999)	U.S. (1986-1998)	U.S. (1990-2000)
Regression type	Probit	Logit	Logit	Logit
Households data	1. American Housing Survey (AHS)	1. NYC Housing and Vacancy Survey (NYCHVS)	1. Panel Study of Income Dynamics (PSID)	(Cohort analysis)
Neighbourhoods data	2. Public Use Microdata Sample (PUMS)	(Gentrifying areas selected anecdotally)	2. U.S. Census	2. U.S. Census
Unit of observation	Housing unit	Housing unit	Heads of households	Synthetic cohorts
Dependent variable	Binary variable: Same household in unit at end of period	Binary variable: Same household in unit at end of period	Two alternatives (binary): Displaced Exits	Two alternatives: Population change Income change
Neighbourhoods	AHS Zone (100,000-200,000 people)	Community Board Districts (46,000 people)	Census Tracts (1,000-8,000 people)	Census Tracts (1,000-8,000 people)
Neighbourhood sample	All areas	All areas	Must be all of: 1. Central City 2. Low income 3. Old housing	Must be all of: 1. Urban 2. Low income
Gentrification variable	Two alternatives (binary): 1. 'Exclusive' - from Wyly and Hammel (1990) 2. 'Inclusive' - increase in education	Binary variable chosen based on familiarity with areas	Two alternatives (binary): 1. increase in education 2. increase in real house prices	Binary variable: $10,000 increase in household income

Table 1: Review of literature (continued)

Conditional relationships	High school diploma	Poor and Non-college grad (restricted samples)	Poor renters	Ethnicity x Education x Age
Controls	Age	Age	Age	CSMA fixed effects
	High school diploma	Education	Assisted Housing	Lag of tract income
	Income	Employment	Education	Marital status
	Own house rating	Ethnicity	Employment	Marital status x race
	Own neigh rating	Income	Ethnicity	Immigrant
	Owner	Maintenance deficiencies	Household composition	Immigrant x race
	Public complex	Marital	Immigrant	race
	Rent control	Native	Income	
	Subsidized	Neighbourhood rating	Marital status	
		Other regulation	Region /Year	
		Overcrowded	Renter	
		Rent-controlled	Sex	
		Rent-stabilised	Unit crowded	
		Sex	Vacancy rate	
		Year	Years in residence	
		Years in residence		
Main findings	Less educated householders *more likely* to stay in unit if in gentrifying areas.	Low income and less educated are less likely to exit gentrifying areas than other areas.	Displacement occurs but not at a higher rate for poor renters.	No evidence for displacement.

3. Data

The data used in this study come from two sources; the British Household Panel Survey (BHPS) and the UK Census (1991, 2001 and 2011). The UK Census is conducted decennially for the entire UK population. The Census is used in this study to characterise the extent of gentrification in neighbourhoods using 10-year changes in the share of individuals in a census ward that hold degree certificate or higher. Further, the UK Census will provide some of the ward level control variables for the analysis. The BHPS is a longitudinal survey of households that was conducted annually for 18 waves (1991-2008). It provides home location identifiers as well as a very rich set of household characteristics. Head of household-years are the unit of analysis for this paper and whether or not they exit their neighbourhood is the outcome variable. Household heads must be observed one period ahead in order to know if they exited their neighbourhood or not. Because of this, exits cannot be observed in the last wave of the BHPS (2008). The BHPS household-years will be merged with the census data at the neighbourhood (CAS Ward) level, with BHPS observations from 1991-2000 being merged with changes over the intercensal period 1991-2001 and BHPS observations from 2001-2007 with the intercensal period 2001-2011.

3.1 Gentrifying neighbourhoods (UK Census)

In order to characterise neighbourhoods in terms of their gentrification status I use the share of population that holds a degree, provided by the UK Census. Educational status has been used in previous literature to measure gentrification, along with measures of income. However, educational attainment is a more stable personal characteristic than income and therefore serves as a more reliable measure of inflow of different demographic group rather than simply changes in the characteristics of existing groups. The degree share variable was obtained from the 1991, 2001 and 2011 Censuses at the ward level. The exact ward definitions differ from census to census and so the figures were converted to comparable geographical units using conversion tables. The resulting data are defined according to the 2001 Census Area Statistics (CAS) Wards for which there are 7,969 covering England. These wards have an average population of 6,669 individuals and an average size of 16.7km^2. These are more suitable for an analysis at the neighbourhood level than the more aggregated areas (over 100,000 people) used in similar studies (Vigdor 2002, Freeman and Braconi 2004). They are comparable to more recent studies that make uses of non-public census data for the US (McKinnish et al. 2010 and Freeman 2005).

Table 2: Wards by initial degree share quintiles (1991-2001)

Quintile	N	Mean	Min	Max
1st	1,593	0.256	0.203	0.599
2nd	1,594	0.178	0.155	0.203
3rd	1,594	0.134	0.114	0.155
4th	1,594	0.096	0.076	0.114
5th	1,594	0.052	0.005	0.076
Total	7,969	0.143	0.005	0.599

Table 3: Wards by initial degree share quintiles (2001-2011)

Quintile	N	Mean	Min	Max
1st	1,594	0.344	0.271	0.725
2nd	1,594	0.239	0.211	0.271
3rd	1,594	0.186	0.163	0.211
4th	1,593	0.139	0.114	0.163
5th	1,594	0.088	0.031	0.114
Total	7,969	0.199	0.031	0.725

Table 4: Wards by change in degree share quintiles (1991-2001)

Quintile	N	Mean	Min	Max
1st	1,593	0.127	0.084	0.590
2nd	1,594	0.069	0.056	0.084
3rd	1,594	0.047	0.039	0.056
4th	1,594	0.031	0.023	0.039
5th	1,594	0.005	-0.358	0.023
Total	7968	0.056	-0.358	0.590

Table 5: Wards by change in degree share quintiles (2001-2011)

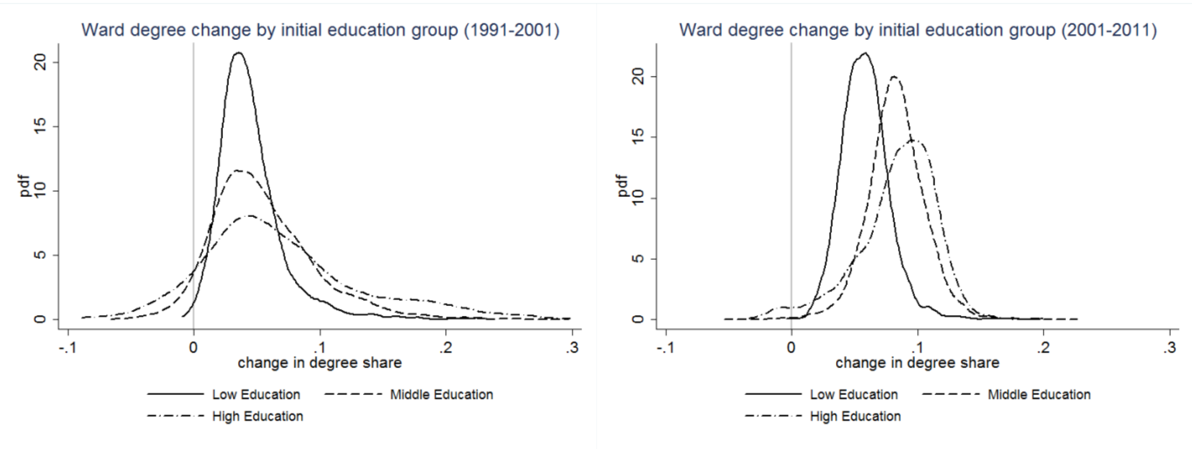
Quintile	N	Mean	Min	Max
1st	1,594	0.114	0.100	0.227
2nd	1,594	0.092	0.085	0.100
3rd	1,594	0.079	0.072	0.085
4th	1,593	0.065	0.057	0.072
5th	1,594	0.041	-0.118	0.057
Total	7969	0.078	-0.118	0.227

Gentrifying wards are those that have a low initial degree share followed by a large over the intercensal period (1991-2001 or 2001-2011). Table 2 and Table 3 illustrate the initial degree share variable by quintiles for the two intercensal periods. The initial degree share for 2001-2011 has a higher mean, min and max in every quintile and in total than for 1991-2001. This is consistent with a general 'upgrading' of human capital in England over the period 1991-2011. Further the tables illustrate that degree shares in 1991 range from 0.5% to 59.9% with a mean of 14.3% and in 2001 range from 3.1% to 72.5% with a mean of 19.9%. The intercensal change in degree share is given in Table 4 and Table 5. Again, 2001-2011 has the highest mean (a 7.8% increase compared with a 5.6% increase over 1991-2001) but it does not have the highest max (only 22.7% compared with 59.0%). There are some wards in both periods that experienced large decreases in degree shares. An urban/rural indicator, introduced in 2004 by the Rural Evidence Research Centre at Birkbeck College (RERC), was obtained at the ward level for England. Urban wards were then selected as those that belong to a settlement with a population of over 10,000.

Next I examine how the change in degree share varies across wards with different initial degree shares. Figure 1 shows more highly educated wards tend to have larger increases in degree share than less educated wards. This correlation is stronger in the second decade than in the first. The variance in change in degree share is also much larger for the more educated wards.

Finally, Figure 2 below illustrates the wards for England and whether they are urban, low education or gentrifying. The categories used in this map are based on the quintiles and are therefore fairly arbitrary. The map is merely intended to give a general overview of the spatial pattern of gentrification in England and a more flexible definition will be used in the empirical analysis. The map shows a few things. Firstly, the low income neighbourhoods are more concentrated in the centre of each urban area with London being a significant exception. Secondly, gentrification begins (in the 90s) in the most central of these low income neighbourhoods and then (in the 00s) spreads out to the next most central low income neighbourhoods. Thirdly, there are very few wards that gentrify in both periods. This is because, due to the way gentrification has been defined here, if a ward gentrifies in the first period it is highly likely to be a non-low education ward and so cannot gentrify again in the second period. The observed pattern of concentric waves of gentrification spreading out from the urban centres is consistent with the model proposed by Brueckner and Rosenthal (2009). This pattern can also explain why there are no low education wards in the centre of London, if it is that gentrification started long before the 90s in central London.

Figure 1: Kernel density plots of degree change by initial degree group



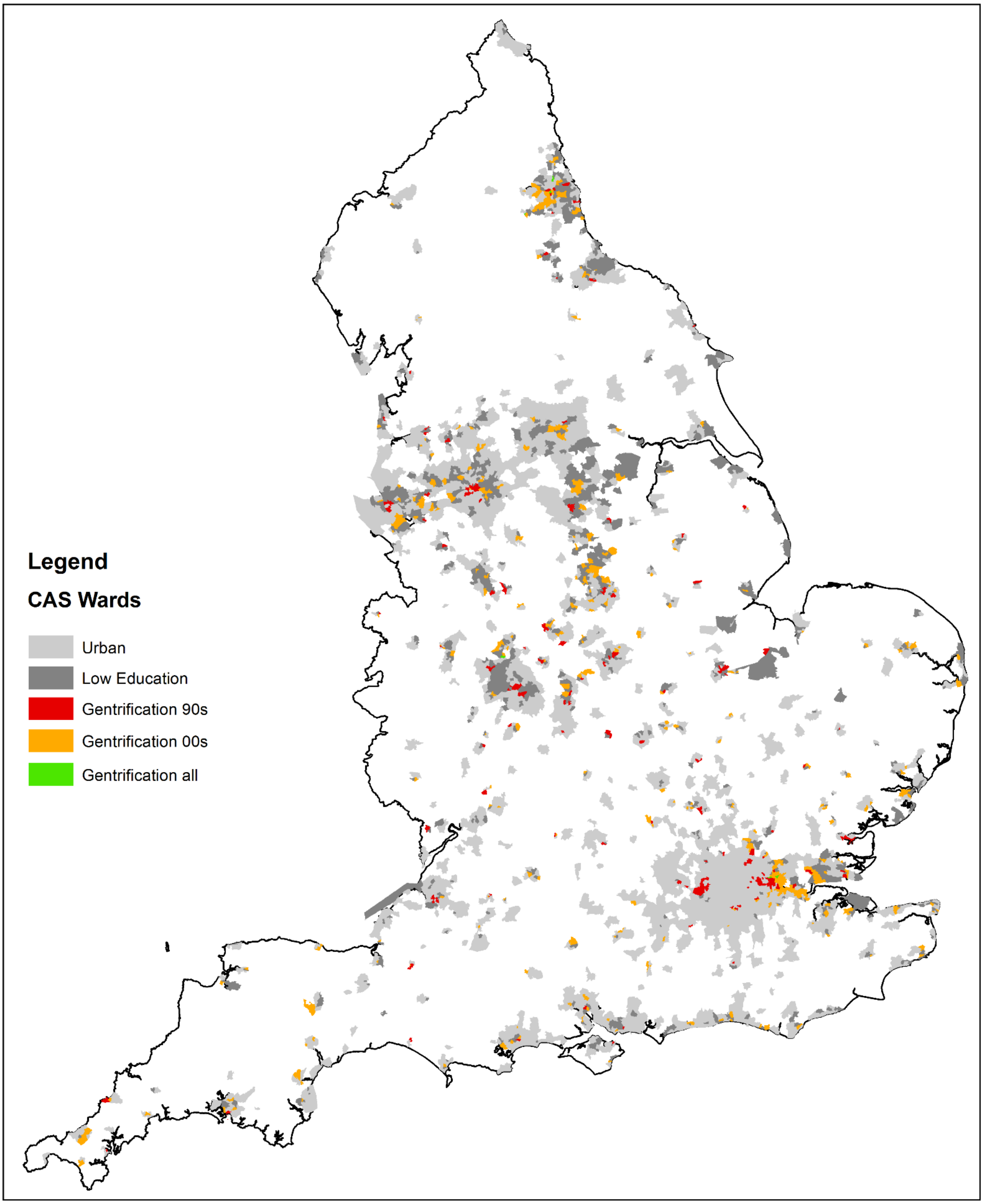
3.2 The households (BHPS)

The BHPS is an annual survey of a representative sample of more than 5,000 British households. Interviews are conducted with heads of households and with all other household members over the age of 16. Heads of household are re-interviewed in subsequent waves. If the heads split from their previous household then all the members of their new households are also interviewed. In this study, each survey entry for a head of household in any wave represents a single observation. By merging together the heads of households across waves it is possible to see if a head of household observed in a particular year lives in a different ward in the next year. This feature will help construct the dependent variable of household exits that will be used to identify displacement.

Previous literature has highlighted the importance that measures of displacement look at forced moves rather than due to normal reasons such as employment changes. A different variable in the BHPS asks individuals whether they lived at the same address last year and if they report "no" then it asks a follow question relating to the reason for the move. The reasons given in the BHPS for moves are wide ranging and often unspecific such as “felt like a change”. Unfortunately, there exists no category for movements due to rising housing costs. Responses that cite “move to larger” or “move to smaller” accommodation do not help too much because it may be that displaced households move from a small property with escalating rents to a larger home somewhere far cheaper. The only category that appeals to displacement are directly is “evicted, or repossessed”. However this represents too few observations to be of much use (80 evictions across all observations). The categories “moved for employment reasons” and “split from partner”, however, cannot plausibly be linked to displacement. Therefore the dependent variable for a head of household-year i is coded as 1 if the head resides in a different ward in the next year and if the move was not for employment reasons or a split from partner. The variable is coded as 0 if the head lives in the same ward or if the exit was for employment reasons or a split from partner. If the head is not observed again in any later waves the variable is coded as missing.

After coding exits I then dropped all observations where exits were unknown because the head of household is not observed again in the sample (6.6% of observations). This means dropping all observations for heads in the last year that they are observed and all observations from the last wave (2008) of the BHPS.

Figure 2: Gentrification map of England (1991-2011)



Notes: Urban is defined as a ward that belongs to a settlement of over 10,000 in population. Low education is a ward that is in the 5th quintile for initial degree share in either period. Gentrification is if the change in degree share in in the 1st quintile for that period.

Since gentrification is an urban phenomenon all observations were dropped where the ward is not categorised as urban (21.7% of the observations). I also dropped all observation not in England. The resulting dataset is 39,170 observations, which is around 53.9% of the original sample of 72,739 observations. I obtained Lower Layer Super Output Area (LSOA) identifiers for household location under a Special Licence Access from the Economic and Social Data Service (ESDS). I aggregated these to CAS Wards, which are described in the previous section, and merged the households data with neighbourhood characteristics from the UK Census, in particular degree share variable described above.

The BHPS also provides a very rich set of household characteristics. Household income is important since this study aims to examine the effect in particular for low income households. I reflated household income to 2011 prices and then calculated the median household income for each Travel To Work Area (TTWA). These TTWAs resemble economic zones in which most people live and work within their boundaries. They are designed such that as few commutes as possible cross their boundaries. Since poverty is a relative measure, these economic zones represent a good benchmark for regional variations in household income. Other control variables used are the age of head of household, the tenure status, whether renters receive housing benefit and whether landlords are private. Renters with private landlords are those that do not live in social housing or let housing from friends, employers or any type of housing association.

Table 6 provides summary statistics of the variables and control variable that will be used in the regression analysis. The table includes mean values or percentage shares for the categorical variables. It also provides exit rates for the categorical variables. These can be compared with the baseline exit rate of 7.3%. Household head types with striking differences from the baseline include Pensioners (at 2.6%), homeowners (at 4.8%) and renters with private landlords (at 28.4%). The lower rates for pensioners, higher rates for renters, and higher still for private rents are consistent with previous literature (e.g. Freeman 2005).

Table 6: Descriptive statistics for variables used in regressions

Variable	Mean	Std. Dev	Min	Max	Exit rate
Change in degree share	0.064	0.041	-0.115	0.308	
Lag degree share	0.146	0.088	0.005	0.655	
Homeowner dummy	0.625	0.484	0	1	0.048
Household income (TTWA-adj.)	0.773	0.426	0	103	
Decade dummy: 90s	0.584	0.493	0	1	0.071
Holds a degree	0.082	0.274	0	1	0.127
Private landlord	0.081	0.274	0	1	0.284
Housing benefit	0.094	0.291	0	1	0.057
Number of children	0.503	0.945	0	8	
People per room	0.516	0.266	0	5	
- Unknown/missing	0.027	0.161	0	1	0.168
Male	0.525	0.499	0	1	0.070
Age of household head	50.9	20.01	0	98	
Head > 65 years age	0.313	0.464	0	1	0.026
Self-employed	0.064	0.245	0	1	0.063
Employed	0.414	0.493	0	1	0.086
Unemployed	0.041	0.199	0	1	0.104
Born outside UK	0.033	0.178	0	1	0.070
Married	0.422	0.494	0	1	0.045
Divorced	0.165	0.371	0	1	0.070
Widowed	0.163	0.370	0	1	0.029
Health score: 1 Excellent - 5 Very Poor	1.98	1.195	1	5	
- Unknown/missing	0.137	0.344	0	1	0.068
Likes neighbourhood	0.865	0.342	0	1	0.066
Years living at address	11.8	13.6	0	86	
- Unknown/missing	0.062	0.241	0	1	0.038
Satisfied with house: 1 -7	3.22	2.86	1	7	
- Unknown/missing	0.396	0.489	0	1	0.069
House needs maintenance	0.170	0.376	0	1	0.094
Lag vacancy rate	0.040	0.024	0	0.331	
Lag population density	3,523	2,609	48	25,013	
Employment potentiality	1.10E+06	1.60E+06	3021	1.20E+07	
Ward size (km^2)	5.45	8.65	0.41	153.7	
Distance (km) to TTWA centroid	9.50	4.93	0.27	26.9	

Notes: The Mean column gives shares for categorical variables and means for non-categorical variables. Categorical variables are also given an exit rate in the final column. Exit rate refers to exit from neighbourhood not for employment reasons. The baseline exit rate is 0.073.

4. Empirical strategy

The strategy outlined here aims to deal with the empirical problems posed by the fact that gentrification may be associated higher exit rates for reasons other than displacement. This is fundamentally a problem caused by sorting. I first highlight the issues, and then implement empirical steps to address them.

4.1 The sorting problem

Households of different types sort into neighbourhoods with different characteristics. As shown in the BHPS data, households with different observed characteristics have different mobility rates. Therefore, some neighbourhood types may have higher 'natural' mobility rates than others. If these neighbourhoods are also gentrifying neighbourhoods then it may appear to be displacement when it is simply higher natural mobility. This can only partly dealt with by controlling for observable households characteristics because households also differ unobservably. There are two main reasons why neighbourhoods with high natural mobility rates may be identified as gentrifying neighbourhoods.

Firstly, it may be that high mobility neighbourhoods are the same type of neighbourhoods that typically gentrify. For instance, if neighbourhoods with good rail access are (a) more likely to gentrify (as shown by Helms, 2003) and (b) traditionally home to residents with high mobility rates. This could be the case if double-job households have higher mobility rates and are attracted to neighbourhoods with good rail access[21]. Secondly, it may be that high mobility in neighbourhoods leads mechanically to increases in degree share. There is a general increasing trend in degree share over the sample period so

[21] Conversely it may be that double-job households have lower mobility rates since they do not typically wish to move once they have found a neighbourhood with good accessibility to both jobs. In this case actual displaced may be concealed by their lower initial mobility rates.

neighbourhoods with higher turnover, may experience faster degree share changes where new highly educated generations simply move in to any free housing unit. To sum up these empirical concerns, both are caused by differences in mobility across different types of neighbourhood due to sorting. The first occurs when high mobility neighbourhood attract gentrification due to the same characteristics. The second occurs when high mobility neighbourhood mechanically gentrify.

4.2 Approach

The follow empirical steps aim to address the problems caused by sorting. Firstly, in addition to household controls, I add neighbourhood controls like population density and employment accessibility. These controls aim to capture neighbourhood characteristics that may be associated with higher mobility rates (and gentrification). However, this does not help with unobserved neighbourhood differences. Therefore, secondly, I implement a fixed effects model that controls for any differences in mobility rates associated with fixed unobservable differences in neighbourhoods. This is helpful to the extent that the neighbourhood factors associated with different mobility rates are fixed over time. The factors that are typically thought to lead to gentrification such as centrality, rail access and housing stock are relatively fixed.

However, time-variant unobserved neighbourhood characteristics remain a problem. To help with this, thirdly, I eliminate from the sample all residents who have been in the neighbourhood for 5 year or less. This helps ensure I do not identify from new residents arriving with different mobility rates in gentrifying periods[22]. It does not help, though, if the old residents were already different in a way correlated with future changes in neighbourhood unobservables. Further, it does not help if neighbourhood changes directly lead to exits. This may be the case if, for example, a factory employing low income workers closes. Fourthly

[22] It also ensures I am really looking at 'original residents' which stays closer to the idea of displacement.

then, I interact the gentrification variable with household income, a renter dummy and the interaction of the renter dummy with household income[23]. The specification ensures that any general differences in mobility rates in gentrifying periods that are not to do with displacement are absorbed by the uninteracted gentrification variable. It also ensures that differences in mobility across income or homeownership (but not related to displacement) are absorbed by the income and renter interactions. Finally, the renter-income interaction captures displacement by estimating how exit rates of renters under gentrification changes with income compared with homeowners. Renters become less vulnerable as their incomes increase, whereas, homeowners do not since they are not susceptible to displacement at any income level. Such an empirical strategy would not have been possible in the U.S. literature that has preceded this, since homeowners in the U.S. may be displaced as a result of home price increases since their tax liabilities increase.

4.3 Renter displacement

Following the above steps I estimate this OLS[24] model for households living in the neighbourhood for more than 5 years:

$$\begin{aligned} E_{iwt} = \alpha &+ \beta_{MobRent}(Gent_{wt} \times Renter_i) + \beta_{Disp}(Gent_{wt} \times Renter_i \times Hinc_i) \\ &+ \beta_{Mob}(Gent_t) + \beta_{MobInc}(Gent_t \times Hinc_i) + X_i\Psi + W_{wt}\Omega + Y_y \\ &+ f_w + \varepsilon_{it} \end{aligned} \tag{1}$$

where E_{iwt} is the neighbourhood exit indicator for household i living in ward w observed in intercensal period t, $Gent_{wt}$ is a gentrification variable described

[23] Interactions are not new to the displacement literature. However, typically only one indication of low status is used, such as low education. The use of both renter and income as separate and combined interactions is novel. So is the use of continuous income rather than a 'low income' dummy variable. And is the use of private renters, as defined further down.

[24] Binary outcome variable is usually estimated using logit or probit models, but an OLS estimation is also feasible. A logit specification is presented in the appendix and the results remain qualitatively similar.

further down, $Renter_i$ is a dummy variable for renter households, $Hinc_i$ is household income normalised to 1 = Travel to Work Area (TTWA) median, X_i is a vector of household control variables, W_{wt} are ward controls, Y_y is a set of year effects, and f_w are the ward fixed effects. The parameters to be estimated are the constant term α, the gentrification parameters $\beta_{MobRent}$, $\beta_{DispHinc}$, β_{Mob} and β_{MobInc}, the vector of household control parameters Ψ, the ward control parameters Ω and the year and ward effects.

This empirical model implements all four steps discussed so far and should eliminate a large proportion of non-displacement mobility differences associated with gentrification. The fixed effects and ward controls eliminate all but unobservable time-variant differences neighbourhoods. Time-variant neighbourhood unobservables may attract higher mobility residents but these households are dropped from the analysis. Finally, the interaction terms capture remaining differences in natural mobility of original residents that are general (β_{Mob}) or related to income (β_{MobInc}) or homeownership ($\beta_{MobRent}$). The mechanical relationship between mobility and gentrification discussed above will also be captured by these parameters. Therefore, the parameter β_{Disp} should capture just displacement. It tells us how household income changes the relationship between under gentrification and household exits for renters compared with homeowners. If displacement occurs then β_{Disp} is expected to be negative. This tells us that as income goes up the exit rates of renters under gentrification goes down compared with homeowners.

Finally, going back to the $\beta_{MobRent}$ parameter, as well as absorbing the non-displacement differences between renters and homeowners under gentrification, this will include the displacement effect for renter households evaluated for an income of zero. Therefore, to the extent that homeowners act as decent controls for renters, this is expected to positive.

4.4 Private renter displacement

The above specification essentially uses homeowners of different incomes as a control for renters of different incomes. This may not be appropriate where e.g. low income homeowners react in significantly differently to changes in neighbourhood characteristics to low income renters. Therefore, I propose an alternative model private renters become the vulnerable group with social renters as the control. I estimate the following model dropping homeowners from the sample:

$$\begin{aligned} E_{iwt} = \alpha &+ \beta_{MobPriv}(Gent_{wt} \times Private_i) + \beta_{Disp}(Gent_{wt} \times Private_i \times Hinc_i) \\ &+ \beta_{Mob}(Gent_t) + \beta_{MobInc}(Gent_t \times Hinc_i) + X_i\Psi + W_{wt}\Omega + Y_y \\ &+ f_w + \varepsilon_{it} \end{aligned} \tag{2}$$

where $Private_i$ indicates if the renter rents from a private individual or corporation. The omitted category is social renter, where the household live in social housing, rents from a housing association, or rents from family, friends, or employer. Since renters of different types should be more similar to each other than renters and homeowners, this represents a stronger counterfactual strengthening the likelihood that β_{Disp} captures displacement. It also makes it more likely that $\beta_{MobPriv}$ (rather than $\beta_{MobRent}$ above) captures the displacement effect evaluated at an income of zero. However, the drawback is that there is a smaller sample of households and the estimates may therefore be less precise.

4.4.1 Income bands

In a final specification the gentrification effect is estimated across five income bands: 0-0.4, 0.4-0.8, 0.8-1.2, 1.2-1.6 and 1.6-2 times the TTWA median household income. I estimate the following model for both the renter and private renter models[25]:

$$\begin{aligned} E_{iwt} = \alpha &+ \sum_b \beta_{b,Dis}\left(Gent_{wt} \times Renter_i \times Hinc_{b,i}\right) \\ &+ \sum_b \beta_{b,Mob}\left(Gent_t \times Hinc_{b,i}\right) + X_i\Psi + W_{wt}\Omega + Y_y + f_w + \varepsilon_{it} \end{aligned} \tag{3}$$

[25] Only the renter model is indicated in equation (3). The private renter model replaces the renter variable with the private variable and drops all homeowners from the sample.

where $Hinc_{b,i}$ is coded to one if a household i falls into income band b. For this model I drop all households with an income above 2.4 times the TTWA median which leaves a residual income band of 2-2.4 TTWA-medians. This is upper band is close enough to the other bands such that households should be (unobservably) similar. Nevertheless the income level is high enough such that renters should be particularly vulnerable to displacement. Thus the differential effect at this income level should capture purely the difference in mobility level between the two groups associated with gentrification but not due to displacement. The parameters $\beta_{d,Dis}$ in this model are interpreted as the displacement effect at income band b since they are net of the constant difference in mobility rates between renters and homeowners under gentrification. Thus wealthy renters serve as a control for low income renters. This specification is also estimated using the private renter model.

4.4.2 The gentrification variable

I acknowledge that gentrification occurs at different speeds and is at different stages of development in different neighbourhoods. Therefore, I use the actual change in degree share to capture the pace of gentrification and interaction of change in degree share with initial degree to capture the stage of development[26]. The $Gent$ variable in the above estimation equations is replaced with two separate variables each with their own parameter to be estimated:

$$\beta Gent = \beta_1 \Delta D_{wt} + \beta_2 (\Delta D_{wt} \times D_{wt-1})$$

where ΔD_{wt} is the change in degree share in ward w over intercensal period ending in t and D_{wt-1} is the initial degree share. The parameter β represents the original parameter for gentrification (interacted or uninteracted versions) which is replaced by two new parameters in each case. The β_1 parameter is interpreted as the impact on exit rates of changes in degree share where the initial degree share is zero. Hence this is the constant term for the gentrification effect by stage

[26] This has the drawback of identifying from negative changes, but since only 2% of the degree share changes are negative, this is not considered a significant issue.

of development. Then β_2 gives how this gentrification effect varies with respect to the initial degree share or, put another way, how gentrified the neighbourhood is to begin with. Finally, I also add the un-interacted lagged degree share as a ward control[27]. This could be interpreted partly as the lagged effect of earlier waves of gentrification but here I simply interpret it as a control.

4.5 Consideration of an IV approach

The empirical strategy presented above attempts to deal with differences in mobility due to the sorting of different households across neighbourhoods of different types. However, it remains a possibility that the groups highlighted as potentially vulnerable to displacement (low income renter, particularly private renters) have exit higher exit rates under gentrification for reasons unrelated to displacement. The only full solution to this problem would be to instrument for gentrification. However, good instruments are notoriously difficult to find in most scenarios. Given that gentrification and displacement are so tightly interlinked it seems implausible that an exogenous instrument may be found. Specifically, most factors that predict gentrification (e.g. rail access) are likely to also determine the mobility rates of original residents. Further, if the neighbourhood were subject to some sort of random amenity shock that lead to gentrification, there is no guarantee that the same amenity shock does not lead directly to elevated exit rates of original residents (violating the exclusionary restriction). Overall, since both the explanatory variable (gentrification) and the dependent variable (neighbourhood exits) represent locations decisions of households, I am unable to think of an instrument that affect one but not the other. Therefore, the empirical approach taken in this paper is to remove as much unobserved heterogeneity as possible through the use of fixed effects and

[27] The initial share controls for the precise level of education in the neighbourhood rather than a fairly wide band. Initial degree share is likely to be correlated with various unobserved neighbourhood and household characteristics that can also effect exit rates. Simply restricting to the lowest quintile is problematic if, for example, within the low education band, the neighbourhoods that gentrify are typically toward the top end and therefore different types of neighbourhood.

interaction terms that capture treatment intensity. The aim being to demonstrate an association between gentrification and elevated exit rates in a way that is consistent with displacement activity across a variety of alternative specifications. The results are presented in the next section.

5. Results

Table 7 reports the results for the OLS estimation of equation (1) which compares renters with homeowners. This table only reports coefficients of the variables interacted with the change in degree share. The full table of coefficients is reported in the appendix. Column (1) includes just a basic set of control variables[28] and year effects Y_y. Column (2) introduces ward level controls, column (3) household controls and column (4) is the fixed effect specification. The first four rows' coefficients report mobility differences under gentrification in the early stages (i.e. at a zero initial share). The next four rows describe how these mobility differences change with the advancement of gentrification.

The second row coefficient $\beta_{1,Dis}$ gives the displacement parameter for the early stages of gentrification (zero initial degree share). The coefficient is negative (but not always significant) across all models and is significant at the 5% level in the strongest, fixed effects, specification. A negative finding indicates the existence of displacement since relationship between gentrification and neighbourhood exits decreases with income for renters (compared with homeowners). In column (4), an increase in household income by the Travel To Work Area median reduces the effect of a one point increase degree share on probability of exit by 0.637 points. To put this into context, the displacement effect can be computed for a neighbourhood (with a zero initial degree share) that experiences a top quintile increase in degree share of about 0.12. A household with 0.5 compared with 1.5 times the TTWA median would have an increased exit rate of $(0.12 \times 0.637 =)$ 0.076 for renters over homeowners. Given the baseline exit rate is around 0.073 this represents a large effect.

The positive and significant coefficient for the interaction with initial degree share ($\beta_{2,Dis} = 2.794$) suggests that the displacement effect disappears as gentrification progresses. In fact it reaches zero at a degree share of 0.14

[28] Basic controls are included to maintain the hierarchy of interaction terms.

(approximately the mean in 1991). Together these findings indicate that displacement is significant in the early stages of gentrification but disappears once the neighbourhood becomes significantly gentrified. This could be explained by considering that the households most unobservably vulnerable to displacement are displaced early on the gentrification process. But by the time that the ward has a high degree share, those households that remain are probably more capable of resisting displacement in ways not captured by observed income i.e. if they have savings or financial help from family.

Table 7: Renter displacement regression (OLS)

		(1)	(2)	(3)	(4)
		Dep. Var.: Household exits			
Change in degree share × Renter	$\beta_{1,MobRent}$	0.371	0.389	0.431*	0.666**
		(0.235)	(0.236)	(0.233)	(0.262)
Change in degree share × Renter × Household income (TTWA-adj.)	$\beta_{1,Dis}$	-0.321	-0.356	-0.415*	-0.637**
		(0.221)	(0.223)	(0.214)	(0.262)
Change in degree share	$\beta_{1,Mob}$	0.076	0.050	0.035	0.390*
		(0.117)	(0.118)	(0.125)	(0.226)
Change in degree share × Household income (TTWA-adj.)	$\beta_{1,MobInc}$	0.059	0.068	0.098	0.170*
		(0.074)	(0.074)	(0.076)	(0.097)
Change in degree share × Lag degree share × Renter	$\beta_{2,MobRent}$	-0.774	-0.878	-0.859	-1.979
		(1.235)	(1.247)	(1.249)	(1.433)
Change in degree share × Lag degree share × Renter × Household income (TTWA-adj.)	$\beta_{2,Dis}$	1.018	1.245	1.377	2.794**
		(1.145)	(1.156)	(1.106)	(1.388)
Change in degree share × Lag degree share	$\beta_{2,Mob}$	-0.514	-0.461	-0.381	-1.376
		(0.577)	(0.581)	(0.610)	(0.911)
Change in degree share × Lag degree share × Household income (TTWA-adj.)	$\beta_{2,MobInc}$	-0.227	-0.256	-0.395	-0.809
		(0.321)	(0.324)	(0.334)	(0.495)
Basic controls (Incl. Year effects)		YES	YES	YES	YES
Ward controls			YES	YES	YES
Household controls				YES	YES
Ward fixed effects					YES
R^2		0.002	0.003	0.023	0.017
AIC		-17738.5	-17756.4	-18290.0	-21201.4
Observations		28,460	28,460	28,460	28,460

Notes: Basic controls are all remaining possible combinations of interaction terms for the reported interacted variables plus year effects. The full table in the appendix reports all coefficients. Standard errors in parentheses clustered on wards in all models. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The mobility coefficients also allow for interesting interpretations. The coefficient on the change in degree share interacted with renters in the first row ($\beta_{1,MobRent}$) tells us that a one point increase in degree share is associated with a 0.666 point increase is the exit rates evaluated for a renter household with an income of zero (in a neighbourhood with a zero initial degree share). This is consistent with displacement. However, as discussed in the empirical strategy, the coefficient also captured any difference natural mobility levels between renters and homeowners under gentrification. The other mobility terms, tell us that exit rates for homeowners (the comparison group) are higher under gentrification ($\beta_{1,Mob} = 0.390$) and that higher income increases probability of exit under gentrification ($\beta_{1,MobInc} = 0.170$). The income interaction is small, positive and barely significant suggesting income does not impact too greatly on mobility rates under gentrification in general. This provides reassurance that the strong negative coefficient for the income-renter interaction is due to displacement, not natural mobility differences.

Table 8 presents the results for the private renter model of equation (2). Here, the counterfactual is improved since renters of different types are likely to be more similar in characteristics than homeowners of different types. However, the sample size is significantly reduced and the estimates may be less precise. In this model the displacement parameter ($\beta_{1,Dis}$) is much larger and significant across all specifications. It remains 5% significant in the fixed effects model. Whilst the variation change across income for private renters is very large ($\beta_{1,Dis} = -4.222$) the change across income in general (i.e. for social renters) is insignificant. This suggests that all the effect observed in the above renter vs. homeowner model comes from private renters that make up only 9% of head of household-years in the sample of 5.990. Furthermore, the intercept mobility differences for private renters ($\beta_{1,MobPriv}$) is positive and significant in all models (although at only 10% level in the fixed effect specification). As discussed in the empirical strategy is stronger evidence for displacement (at zero income) than the equivalent parameter in the renter vs. homeowner model.

The interactions with lagged degree share show again that these effects decrease with the stage of gentrification. The $\beta_{2,Dis}$ parameter shows the displacement effect becomes zero at a degree share of around 0.20, which is in the 2nd quintile across both decades. Therefore, the private renter model highlights a much larger displacement effect which also persists longer through the stages of gentrification.

Table 8: Private renter displacement regression (OLS)

		(1)	(2)	(3)	(4)
		Dep. Var.: Household exits			
Change in degree share × Private	$\beta_{1,MobPriv}$	6.155***	6.218***	5.879***	3.791*
		(1.787)	(1.789)	(1.664)	(1.930)
Change in degree share × Private × Household income (TTWA-adj.)	$\beta_{1,Dis}$	-4.930***	-5.007***	-4.883***	-4.222**
		(1.426)	(1.464)	(1.369)	(2.103)
Change in degree share	$\beta_{1,Mob}$	0.125	0.107	0.144	0.698
		(0.210)	(0.212)	(0.207)	(0.439)
Change in degree share × Household income (TTWA-adj.)	$\beta_{1,MobInc}$	-0.066	-0.075	-0.069	-0.011
		(0.235)	(0.237)	(0.233)	(0.256)
Change in degree share × Lag degree share × Private	$\beta_{2,MobPriv}$	-27.391***	-27.921***	-26.981***	-15.633*
		(8.260)	(8.318)	(7.818)	(9.008)
Change in degree share × Lag degree share × Private × Household income (TTWA-adj.)	$\beta_{2,Dis}$	24.428***	25.002***	25.250***	21.150*
		(7.421)	(7.621)	(7.259)	(10.968)
Change in degree share × Lag degree share	$\beta_{2,Mob}$	0.120	0.138	0.214	-1.468
		(1.155)	(1.164)	(1.191)	(1.732)
Change in degree share × Lag degree share × Household income (TTWA-adj.)	$\beta_{2,MobInc}$	0.032	0.126	-0.156	-0.200
		(1.182)	(1.199)	(1.192)	(1.261)
Basic controls		YES	YES	YES	YES
Ward controls			YES	YES	YES
Household controls				YES	YES
Ward fixed effects					YES
R^2		0.019	0.021	0.050	0.038
AIC		-3282.6	-3283.9	-3415.5	-4785.9
Observations		5,990	5,990	5,990	5,990

Notes: Basic controls are all remaining possible combinations of interaction terms for the reported interacted variables plus year effects. Standard errors in parentheses clustered on wards in all models.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The next specification breaks the effect down by income bands. Table 9 and Table 10 show the results of the estimation of equation (3) for renters and private renters, respectively. Concentrating on strongest results in column (4), the first five rows of Table 9 show evidence of displacement of renters in low income bands in the early stages of gentrification. These coefficients reveal that the difference in exit rates under gentrification between renters and homeowners is far higher in lower income bands than in the wealthy omitted group (2.0 ≤ Income < 2.4). The general trend is downwards as income increase, with the only exception being a spike at an income of 1.2-1.6 TTWA medians. This fourth band and the first two bands (0-0.4 and 0.4-0.8 TTWA medians) are statistically significant at least at the 5% level. The next five rows describe how these displacement effects decline with the stage of gentrification. They suggest the effect becomes zero across all income bands at an initial share again of around 0.2, i.e. the 2nd quintile.

Table 9: Renter displacement effect by income bins (OLS)

	(1)	(2)	(3)	(4)
	Dep. Var.: Household exits			
Degree change × Renter				
× (0.0 ≤ Income < 0.4)	1.784**	1.751**	2.033**	2.088***
	(0.734)	(0.745)	(0.790)	(0.725)
× (0.4 ≤ Income < 0.8)	1.248*	1.238*	1.426**	1.579**
	(0.670)	(0.675)	(0.713)	(0.666)
× (0.8 ≤ Income < 1.2)	0.551	0.532	0.877	0.834
	(0.712)	(0.709)	(0.744)	(0.691)
× (1.2 ≤ Income < 1.6)	1.869*	1.809*	2.154**	1.750**
	(0.961)	(0.973)	(0.994)	(0.886)
× (1.6 ≤ Income < 2.0)	0.385	0.329	0.366	0.116
	(0.925)	(0.922)	(0.918)	(0.736)
Degree Change × Lag degree × Renter				
× (0.0 ≤ Income < 0.4)	-6.650*	-6.872*	-8.585**	-10.591**
	(3.818)	(3.951)	(4.342)	(4.116)
× (0.4 ≤ Income < 0.8)	-4.848	-5.150	-6.253	-8.802**
	(3.515)	(3.581)	(3.887)	(3.715)
× (0.8 ≤ Income < 1.2)	-1.633	-1.972	-3.758	-5.336
	(3.679)	(3.716)	(4.010)	(3.793)
× (1.2 ≤ Income < 1.6)	-7.356	-7.424	-9.356*	-9.087*
	(5.015)	(5.131)	(5.408)	(4.790)
× (1.6 ≤ Income < 2.0)	-3.206	-3.300	-4.039	-3.569
	(5.568)	(5.569)	(5.543)	(4.212)
Basic controls (Incl. Year effects)	YES	YES	YES	YES
Ward controls		YES	YES	YES
Household controls			YES	YES
Ward fixed effects				YES
R^2	0.004	0.005	0.025	0.020
AIC	-16526.2	-16540.8	-17014.1	-19695.8
Observations	25,759	25,759	25,759	25,759

Notes: Basic controls are all remaining possible combinations of interaction terms for the reported interacted variables plus year effects. Income is household income normalized to 1=TTWA median. Omitted income category is 2.0 ≤ Income < 2.4. Standard errors in parentheses clustered on wards in all models. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10 gives the result for the comparison between private renters and social renters. Again, for private renters the effect sizes are very much larger. A 1 point increase in degree share is associated with a 21.6 point increase in the probability of exit for household with lowest income compared with their TTWA median. To put this into perspective, for a ward with an initial degree share of zero, even a very small 5th quintile increase in degree share (0.016) would be associated with an increase in exit probability by 0.342. Private renters already have some of the highest exit rates, with a mean of 0.284, but this would still represent a more than doubling of the exit rate. Since an exit probability of 1 indicates guaranteed exit within the year, it is clear that large increases in degree share, as are typical for gentrification, will be associated with almost guaranteed exit of low income private renters. The first three income bands are positive and significant suggesting displacement occurs up to an income of 1.2 times the TTWA median. Since these coefficients are approximately ten times the size of their Table 9 equivalents and private renters make up 10%, of total renters, this is again suggestive that the effects seen for all renters are essentially just the diluted effects of private renters.

Table 10: Private displacement effect by income bins (OLS)

	(1)	(2)	(3)	(4)
	Dep. Var.: Household exits			
Degree change × Private				
× (0.0 ≤ Income < 0.4)	22.076**	21.583**	19.347*	21.576**
	(10.408)	(10.302)	(10.420)	(9.734)
× (0.4 ≤ Income < 0.8)	15.537	14.977	13.461	17.440*
	(10.229)	(10.133)	(10.305)	(9.682)
× (0.8 ≤ Income < 1.2)	21.523**	21.204**	18.934*	23.896**
	(10.333)	(10.250)	(10.436)	(9.878)
× (1.2 ≤ Income < 1.6)	14.212	13.576	11.500	11.047
	(10.507)	(10.430)	(10.603)	(10.285)
× (1.6 ≤ Income < 2.0)	7.620	6.934	5.713	10.781
	(10.847)	(10.753)	(10.775)	(10.234)
Degree Change × Lag degree × Private				
× (0.0 ≤ Income < 0.4)	-53.158	-52.055	-46.052	-55.534
	(46.146)	(45.583)	(45.945)	(42.677)
× (0.4 ≤ Income < 0.8)	-32.079	-30.292	-27.646	-45.146
	(45.070)	(44.528)	(45.128)	(42.416)
× (0.8 ≤ Income < 1.2)	-49.631	-49.420	-43.089	-65.634
	(44.737)	(44.311)	(45.041)	(42.358)
× (1.2 ≤ Income < 1.6)	-23.880	-21.523	-14.257	10.297
	(49.583)	(49.172)	(49.910)	(49.620)
× (1.6 ≤ Income < 2.0)	21.579	24.282	25.237	-3.075
	(47.921)	(47.343)	(47.069)	(48.894)
Basic controls (Incl. Year effects)	YES	YES	YES	YES
Ward controls		YES	YES	YES
Household controls			YES	YES
Ward fixed effects				YES
R^2	0.035	0.037	0.064	0.054
AIC	-3197.9	-3198.5	-3320.4	-4710.3
Observations	5,912	5,912	5,912	5,912

Notes: Basic controls are all remaining possible combinations of interaction terms for the reported interacted variables plus year effects. Income is household income normalized to 1 = TTWA median. Omitted income category is 2.0 ≤ H. Income < 2.4. Standard errors in parentheses clustered on wards in all models. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Finally in Figure 3, I plot the displacement effects by income band at different initial degree shares. The left column of charts shows the effects for renters over homeowners, and the right column shows the effects of private renters over social renters. Histograms of the sample distributions across income for renters and private renters are also shown light grey in the chart backgrounds. Since graphical illustration allows for the display of more coefficients, I have used finer income bands of 0.2 TTWA medians in width. The omitted band remains 2-2.4 TTWA median incomes.

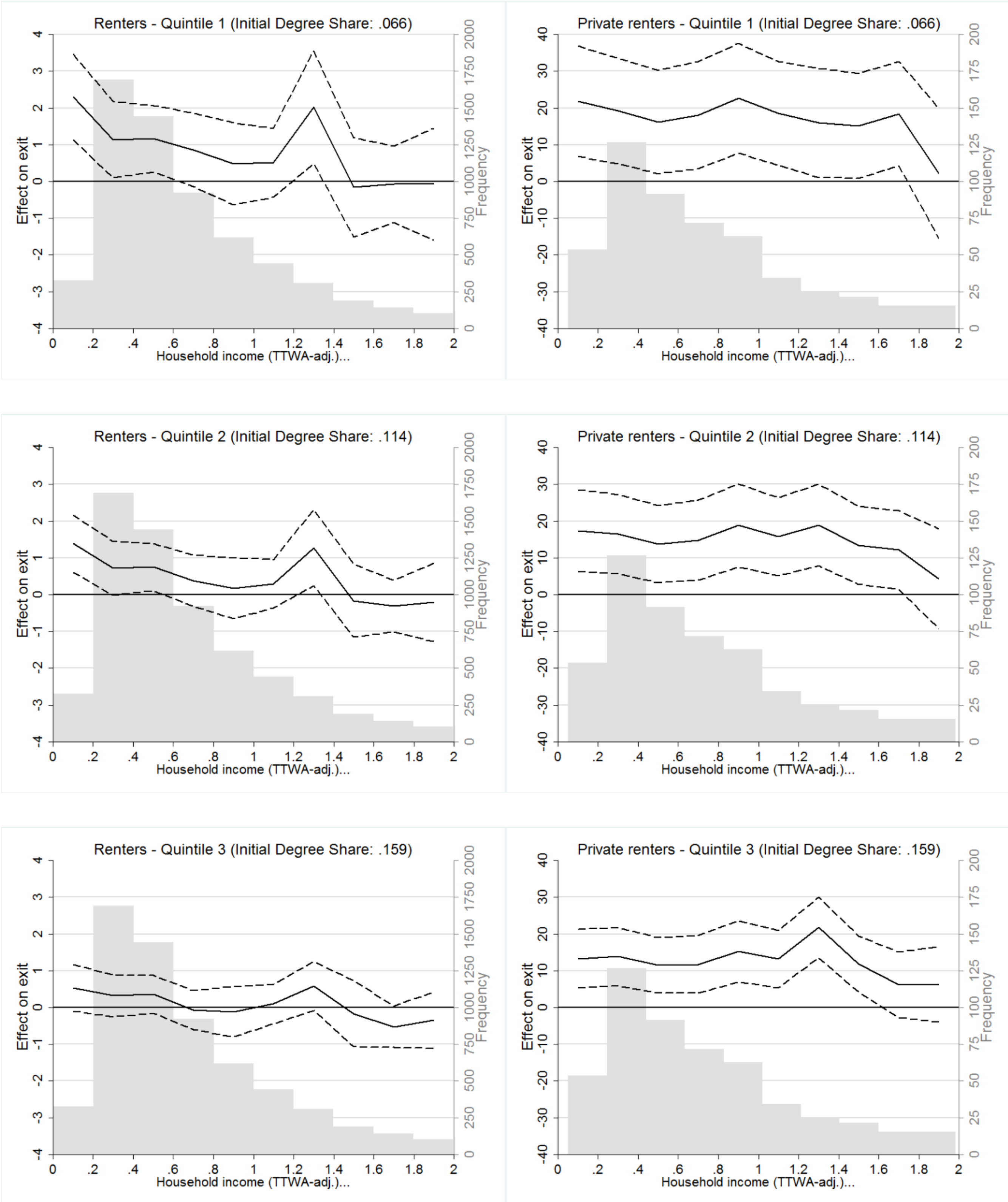
The first row of charts shows the effect in the early stages of gentrification (5th quintile of initial degree share). These show displacement is statistically significant up to around 0.6 times the median income for renters, but all the way up to 1.8 for private renters. There is a clear downwards slope across income in both types of comparisons. In the next row (4th quintile) the effects are illustrated for the not-so-early stages of gentrification. Across both models, the size of displacement decrease and the change with respect to income flattens slightly. Once we look at the middle stages of gentrification in the third row (3rd quintile) the effects are insignificant at all incomes for the renter model. The private renter model, however, continue to illustrate displacement activity significant up to 1.6 times the TTWA median income. The fourth row charts illustrate much the same as the third. Only in the fifth row, in the most gentrified of neighbourhoods, does the displacement effect disappear even for private renters (except for the spike at 1.2-1.4 times the TTWA income)[29]. Together the right column of charts suggests very pronounced displacement of private renter households, even where they have an income above the TTWA-median. This makes sense if household up to 1.6 times the median income are not so wealthy as to be invulnerable to rising housing costs. Further, if these households spend approximately the same share of their income on housing as lower income

[29] The sample distribution for the private renter models indicates only around 25 observations or fewer in each income band beyond 1.2 times the TTWA median. Therefore the spike at 1.2-1.4 is most likely due to imprecision in the estimates at high incomes.

families, or that they are generally households of larger sizes then proportional increases in rents could easily lead to financial difficulties.

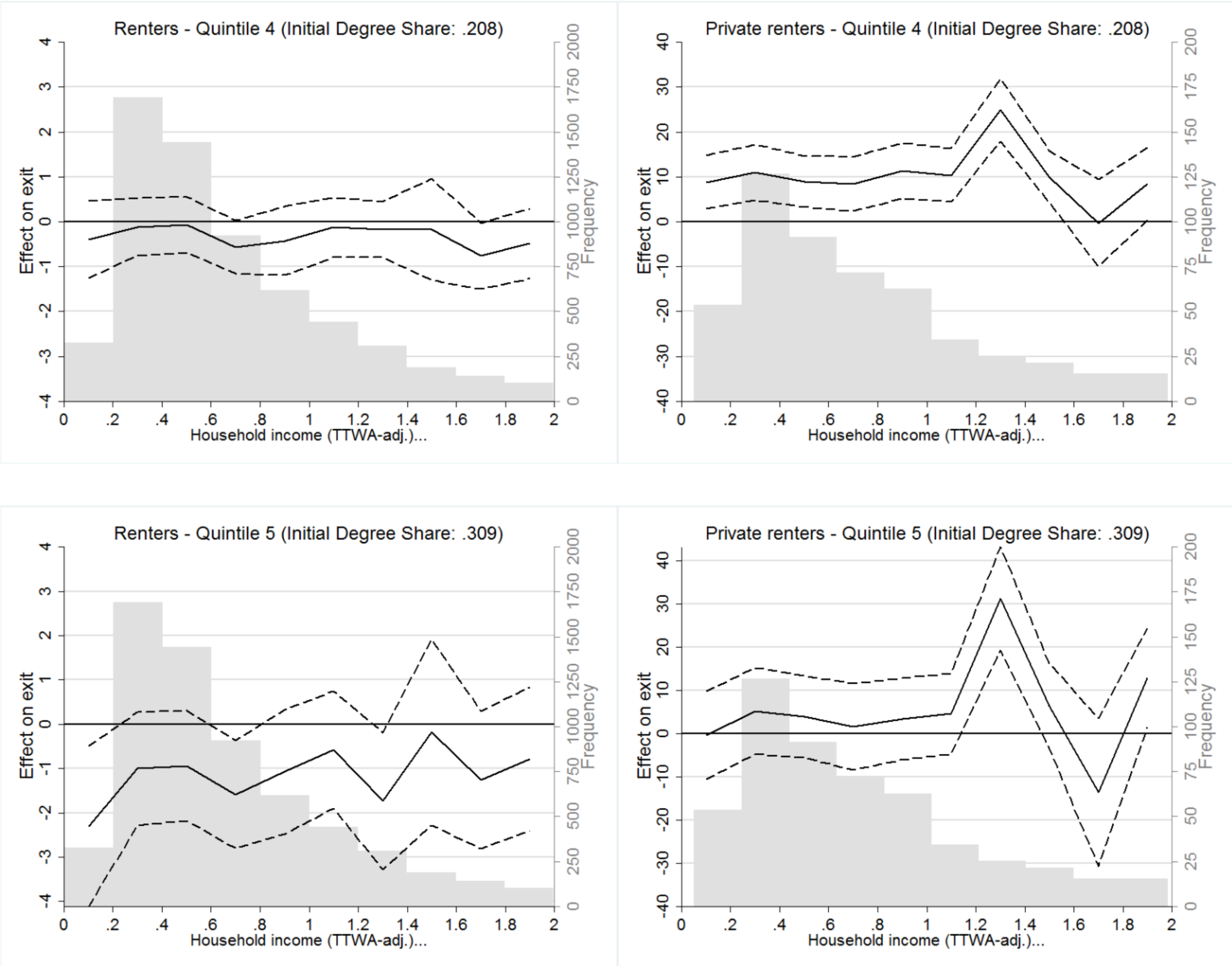
To summarise the results, both the renter and private renter models have indicated significant displacement of low income households in the early stages of gentrification. However, the private renter displacement is a much larger effect that persists longer through the latter stages of stages of gentrification. The effect size indicates that private renters are very quickly displaced from gentrifying neighbourhoods. The results suggest that the effect observed for all renters may be simply the diluted effect for private renters. This makes sense if social housing and housing association rents are not at all linked to market rates.

Figure 3: Marginal effect of change in degree share on exit



Note: Charts of coefficients based on OLS estimation of equation (3) using ten income bands of 0.2 times TTWA median. Depicted are (a) the marginal effects (solid black lines, left axis) with 5% confidence intervals (dashed lines) and (b) the sample distribution (grey bars, left axis) for renters (left charts) and private renters (right charts).

Figure 3 (continued): Marginal effect of change in degree share on exit



Note: Charts of coefficients based on OLS estimation of equation (3) using ten income bands of 0.2 times TTWA median. Depicted are (a) the marginal effects (solid black lines, left axis) with 5% confidence intervals (dashed lines) and (b) the sample distribution (grey bars, left axis) for renters (left charts) and private renters (right charts).

6. Conclusions and policy implications

This paper has investigated whether gentrification is associated with displacement of pre-existing residents and in particular of low-income (private) renters. It has made methodological advancements over previous literature in terms of controlling for unobservables and investigating interactive effects. In contrast to much of the earlier literature it finds strong evidence for a displacement effect associated with gentrification, measured by change in degree share. Gentrification has been found to be positively associated with higher exit rates of renter households and private renters in particular. A further result is that the effect decreases substantially at more advanced stages of gentrification. This is reassuring because it adds meaning to the definition of gentrification as an inflow of middle class households into a previously working class or poor neighbourhood, setting it apart in consequences from a simple increase in degree share. A potential explanation for finding is that the most vulnerable households have already been displaced from neighbourhoods in the later stages of the gentrification process.

This finding has two important implications for policy. Firstly, policymakers wishing to improve the outcomes for low income households should implement measures to reduce the impact of gentrification on displacement. This may be achieved, for example, by following policies from Germany that prevent the rent eviction of tenants for up to 7 years after newly purchasing a property. The second implication is that more general policies that aim to improve outcomes for the poor may be mistargeted as a result of displacement process. For example, spatially-targeted policies to help the poor miss their target if improvements in local amenities are followed by an in-migration of wealthier households and displacement of pre-existing residents. Furthermore policies aimed at mixing neighbourhoods may be misguided if they too lead to displacement. Policy-makers wishing to improve outcomes for low-income households they may be better off directly targeting incomes and sources of poverty or by combining neighbourhood improvement policies with incentive for low-income renters to become homeowners.

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APPENDIX TO CHAPTER III

1. Introduction

This appendix complements the main paper and is not designed to stand alone or as a replacement. Section 5, the only section to this appendix, complements the results section of the main paper by providing alternative tables.

5. Results

This section presents results not shown in the main paper. The logit version of the main renter displacement regression is discussed, as well as the full table of coefficients for the OLS regression.

5.1 Logit regression

Table 1 presents the results of the logit estimation of equation (1). The results are qualitatively similar to the equivalent results presented in Table 7 in the main paper. Focusing on the fixed effect model in column (4), the displacement effect (2^{nd} row) is negative and significant, indicating the occurrence of displacement. The positive final row coefficient shows that this displacement effect reduces with initial degree share. However, the effect remains until an initial degree share of 0.285, which is a more persistent displacement effect than shown in the OLS model. Nevertheless, the broad pattern of effect is similar.

Table 1: Renter displacement regression (Logit)

	(1)	(2)	(3)	(4)
Dependent Variable:	Household exits neighbourhood			
Change in degree share × Renter	8.362	9.068	11.404*	24.049***
	(5.999)	(5.808)	(6.272)	(7.327)
Change in degree share × Renter × Household income (TTWA-adj.)	-8.080	-9.122	-10.798*	-21.620**
	(6.005)	(5.924)	(5.794)	(9.626)
Change in degree share × Lag degree share × Renter	-14.984	-19.440	-19.713	-75.909**
	(32.541)	(30.688)	(33.606)	(37.466)
Change in degree share × Lag degree × Renter × Household income (TTWA-adj.	26.416	33.060	36.568	87.152**
	(30.215)	(28.530)	(28.856)	(41.938)
Basic controls (incl. Year effects)	YES	YES	YES	YES
Ward controls		YES	YES	YES
Household controls			YES	YES
Ward fixed effects				YES
Pseudo R^2	0.003	0.006	0.065	0.065
AIC	8154.4	8137.4	7707.9	5470.1
Observations	28,460	28,460	28,460	22,226

Notes: Basic controls are all remaining possible combinations of interaction terms for the reported interacted variables plus year effects. Standard errors in parentheses clustered on wards in all models. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2 Full results of OLS model

In Table 2 below I report and discuss the full set of coefficient for the OLS estimation of equation (1) from the main paper. Column (1) includes just the basic set of controls, which are remaining possible combinations of interaction terms for the reported interacted variables plus year effects. In this column, the gentrification effects are of the same signs as in the other models but insignificant. The only significant coefficients are for the year effects for 1995 and 1996, which are positive, and the constant.

In column (2) I introduce ward controls. The main gentrification variables are approximately unchanged in magnitude and significance. The ward controls are typically significant, though. The two measures of centrality, population density and distance to TTWA centroid are both associated with higher exit rates[30], whereas access to employment is associated with lower exit rates. Together this

[30] Although distance to TTWA centroid is insignificant, perhaps because population density better captures centrality.

implies that central wards may be associated with a generally higher pace of life that includes more frequent moves but that accessibility to employment is a valued amenity that households do not want to move away from. Ward size is negative and significant suggesting as expected that moves are more likely to exit a ward if the ward is smaller is size. An alternative interpretation that smaller wards are denser is made less likely due to the inclusion of population density as a control.

In column (3) I introduce household controls. The gentrification effects increase in size and become marginally significant. In particular the displacement parameter (2nd row) is significant at the 10% level. The household characteristics are also shown to be important. The exit rates are 0.008 higher for households where the head holds a degree certificate or higher (10% significant). Exits are 0.051 higher for renters with a private landlord. The effect of whether the household receives housing benefit is negative but insignificant. Exits are 0.004 lower for each dependent child, but the effect is insignificant for each person per room. The gender of the head of household is insignificant. The age, age^2 and pension coefficients suggest a non-linear relationship between exits and age of the head of household. Exit rates decrease age but at a decreasing rate. They experience a significant downwards discontinuity above the pensionable age of 65. The employed, self-employed and unemployed have significantly lower exit rates (-0.014, -0.009, and -0.016 respectively) than economic non-participants. Those born outside of the UK also have lower exit rates by 0.041. Marital status of heads of households has no effect on exit rates apart from widowed status which has higher exit rates (10% significant). The coefficient on self-reported health status suggests no effect on exit rates. Only where this variable is missing are there significant differences in exits of -0.053. It is unclear whether this parameter measures an effect to do with their health status or to do with the characteristics of non-responders. Heads who like their neighbourhood are less likely to move away from it (-0.041). Households who have been a long time in the neighbourhood are also less likely to move away, by 0.001 per year at current address. Those satisfied with their house are less likely to move away (-0.010 per point). The households with this variable missing are less likely to move away (-0.060). This is reasonably consistent with the mean score where known of 5.33. Finally, whether the house needs some maintenance work has no effect on exits.

Table 2: Renter displacement regression full (OLS)

	(1)	(2)	(3)	(4)
Dependent Variable:	Household exits neighbourhood			
Displacement Effects				
Change in degree share × Renter	0.371	0.389	0.431*	0.666**
	(0.235)	(0.236)	(0.233)	(0.262)
Change in degree share × Renter	-0.321	-0.356	-0.415*	-0.637**
× Household income (TTWA-adj.)	(0.221)	(0.223)	(0.214)	(0.262)
Change in degree share × Lag degree share	-0.774	-0.878	-0.859	-1.979
× Renter	(1.235)	(1.247)	(1.249)	(1.433)
Change in degree share × Lag degree share	1.018	1.245	1.377	2.794**
× Renter × Household income (TTWA-adj.)	(1.145)	(1.156)	(1.106)	(1.388)
Basic Controls				
Change in degree share	0.076	0.050	0.035	0.390*
	(0.117)	(0.118)	(0.125)	(0.226)
Change in degree share × Household	0.059	0.068	0.098	0.170*
income (TTWA-adj.)	(0.074)	(0.074)	(0.076)	(0.097)
Change in degree share × Lag degree share	-0.514	-0.461	-0.381	-1.376
	(0.577)	(0.581)	(0.610)	(0.911)
Change in degree share × Lag degree share	-0.227	-0.256	-0.395	-0.809
× Household income (TTWA-adj.)	(0.321)	(0.324)	(0.334)	(0.495)
Lag degree share	0.060	0.069	0.070	0.320*
	(0.049)	(0.050)	(0.052)	(0.168)
Renter	-0.006	-0.006	-0.014	-0.022
	(0.015)	(0.015)	(0.016)	(0.017)
Household income (TTWA-adj.)	0.002	0.002	-0.001	-0.006
	(0.006)	(0.006)	(0.006)	(0.008)
Lag degree share × Renter	-0.005	-0.000	-0.028	0.042
	(0.097)	(0.097)	(0.099)	(0.115)
Household income (TTWA-adj.) × Renter	0.008	0.010	0.007	0.016
	(0.015)	(0.015)	(0.015)	(0.018)
Lag degree share × Household income	0.003	0.004	0.016	0.046
(TTWA-adj.)	(0.029)	(0.029)	(0.031)	(0.046)
Lag degree share × Renter × Household	-0.015	-0.031	-0.023	-0.110
income (TTWA-adj.)	(0.084)	(0.084)	(0.084)	(0.111)
Year: 1992	0.002	0.002	-0.052***	-0.040***
	(0.006)	(0.006)	(0.011)	(0.012)
Year: 1993	0.008	0.008	-0.047***	-0.035***
	(0.006)	(0.006)	(0.011)	(0.012)
Year: 1994	0.005	0.005	-0.051***	-0.037***
	(0.006)	(0.006)	(0.011)	(0.012)
Year: 1995	0.011*	0.011*	-0.044***	-0.031**
	(0.006)	(0.006)	(0.011)	(0.013)
Year: 1996	0.014**	0.014**	-0.044***	-0.028**
	(0.006)	(0.006)	(0.013)	(0.014)

Table 2 (continued)				
Year: 1997	0.002	0.003	-0.053***	-0.036**
	(0.005)	(0.005)	(0.013)	(0.014)
Year: 1998	0.003	0.003	-0.052***	-0.039***
	(0.005)	(0.005)	(0.013)	(0.014)
Year: 1999	0.009	0.009	0.005	0.006
	(0.006)	(0.006)	(0.008)	(0.009)
Year: 2000	0.008	0.010	-0.048***	-0.048***
	(0.006)	(0.006)	(0.013)	(0.017)
Year: 2001	0.001	0.002	-0.051***	-0.054***
	(0.006)	(0.006)	(0.012)	(0.016)
Year: 2002	0.003	0.004	-0.053***	-0.053***
	(0.006)	(0.006)	(0.013)	(0.017)
Year: 2003	0.000	0.001	-0.054***	-0.055***
	(0.006)	(0.006)	(0.013)	(0.018)
Year: 2004	-0.003	-0.001	-0.058***	-0.057***
	(0.006)	(0.006)	(0.013)	(0.017)
Year: 2005	-0.003	-0.001	-0.060***	-0.057***
	(0.006)	(0.006)	(0.013)	(0.018)
Year: 2006	0.010	0.011*	-0.051***	-0.045**
	(0.006)	(0.006)	(0.013)	(0.017)
Year: 2007	-0.008	-0.007	-0.067***	-0.056***
	(0.005)	(0.005)	(0.013)	(0.018)
Ward Controls				
Lag vacancy rate		0.117*	0.047	0.009
		(0.060)	(0.059)	(0.124)
Lag population density		0.000***	0.000*	-0.000
		(0.000)	(0.000)	(0.000)
Employment potentiality		-0.000**	-0.000**	
		(0.000)	(0.000)	
Ward size (km^2)		-0.000**	-0.000***	
		(0.000)	(0.000)	
Distance (km) to TTWA centroid		-0.000	-0.000	
		(0.000)	(0.000)	
Household Controls				
Holds a degree			0.008*	0.003
			(0.005)	(0.006)
Private landlord			0.051***	0.037***
			(0.013)	(0.014)
Housing benefit			-0.006	-0.008
			(0.005)	(0.006)
Number of children			-0.004**	0.000
			(0.002)	(0.002)
People per room			0.008	0.009
			(0.006)	(0.007)
- Unknown/missing			0.013	0.016
			(0.012)	(0.013)
Male			0.004	0.001
			(0.003)	(0.003)

Table 2 (continued)

Age of household head			-0.003***	-0.002***
			(0.000)	(0.000)
Age^2 of household head			0.000***	0.000***
			(0.000)	(0.000)
Head > 65 years age			-0.008*	-0.011**
			(0.004)	(0.004)
Self-employed			-0.009**	-0.007
			(0.005)	(0.005)
Employed			-0.014***	-0.013***
			(0.003)	(0.004)
Unemployed			-0.016**	-0.014*
			(0.007)	(0.007)
Born outside UK			-0.041***	-0.043***
			(0.007)	(0.008)
Married			-0.003	-0.003
			(0.005)	(0.005)
Divorced			0.007	0.006
			(0.005)	(0.006)
Widowed			0.008*	0.007
			(0.005)	(0.006)
Health score (1-5)			-0.001	-0.000
			(0.001)	(0.001)
- Unknown/missing			-0.053***	-0.040***
			(0.010)	(0.011)
Likes neighbourhood			-0.041***	-0.042***
			(0.006)	(0.006)
Years living at address			-0.001***	-0.001***
			(0.000)	(0.000)
If satisfied with house			-0.010***	-0.010***
			(0.001)	(0.001)
- Unknown/missing			-0.060***	-0.058***
			(0.011)	(0.011)
House needs maintenance			-0.002	-0.001
			(0.003)	(0.004)
Constant	0.015*	0.008	0.290***	0.207***
	(0.009)	(0.010)	(0.026)	(0.040)
Observations	28460	28460	28460	28460
R^2	0.002	0.003	0.023	0.017
AIC	-17738.5	-17756.4	-18290.0	-21201.4

Standard errors in parentheses clustered on wards in all models. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

CHAPTER IV

GAME OF ZONES: THE POLITICAL ECONOMY OF CONSERVATION AREAS

1. Introduction*

One of the key motivations for a variety of spatial planning policies is how to solve coordination problems inherent to free markets. Among such policies historic preservation occupies a leading position in terms of the rigidity of the related regulations as well as the complexity of related social and private costs and benefits. These policies restrict individual property rights in order to protect buildings with a particular aesthetic, cultural or historic value. In doing so the policy may overcome a coordination problem by ensuring that owners can no longer "freeride" on the character of nearby buildings while making inappropriate changes to their own properties. In other words it may help to solve a so-called prisoner's dilemma (Holman & Ahlfeldt, 2012). A welfare maximizing preservation policy must take into account social costs and benefits of preservation incurred by the wider society and even future generations. It is therefore unlikely that designation decisions that are considered socially optimal are also in the interest of local homeowners. In this paper we ask the question whether owners are able to 'game the system' to their advantage i.e. whether the designation status of each zone in a neighbourhood is determined by the preferences of the homeowners residing there. We answer this question by deriving a model of the designation process in which a planner acts as an agent of local homeowners and then empirically testing its predictions.

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Our theory distinguishes between a *heritage effect*, which can be internal or external, i.e., the effect of the appearance of a historic building on the perceived value of the house itself (internal) or nearby houses (external), and a *policy effect*, which results from the legal treatment of the designation policy. We argue that with positive heritage effects, the policy benefits the owners by removing uncertainty regarding the future of the neighbourhood, i.e., the presence of the heritage effect. These benefits are opposed by the costs of regulation (in the form of development restrictions and maintenance obligations) so that the net effect of the policy effect is ambiguous. Our theoretical framework predicts positive, but diminishing returns to designation. Taking on the assumption that the planner acts in the interests of local homeowners we can derive a condition for the (political) equilibrium level of designation. This condition generates two empirically testable hypotheses. Firstly, new designations will result from increases in the local preferences for heritage. Secondly, in equilibrium, the marginal costs and benefits of designation will offset each other, resulting in a zero impact of new designations on house prices. At all other locations in a neighbourhood the effect will be positive.

We test these implications using two different empirical approaches. Firstly, we estimate the effect of changes in neighbourhood composition, what we define as gentrification, on the likelihood of designations using a tobit IV approach. Secondly, we use a hybrid difference-in-differences (DD) and regression discontinuity design (RDD) identification strategy to estimate the effect of new designations on the market value of properties. Our analysis is based on the whole of England, making use of 1 million property transactions from 1995 to 2010 and of about 8,000 designated conservation areas, of which 915 have been designated in the same observation period. We also make use of ward level education data from the UK census for 1991, 2001, and 2011 in order to analyse the effect of changing neighbourhood characteristics on the designation status. Previewing our results we find that an increase in the local share of residents holding a university or college degree leads to an expansion of the designated area. The property price effect inside newly designated conservation areas turns out not to be statistically distinguishable from zero. We find evidence that the effect just outside the conservation area boundary is positive and significant.

These results are in line with the political equilibrium policy level suggesting that the planner adheres to local homeowner interests.

Our analysis of the conservation area designation process adds to a growing body of literature on the political economy of housing markets, which implicitly or explicitly assumes that property owners are able to influence political outcomes in their own interest (e.g. Boes & Nüesch, 2011; Brunner & Sonstelie, 2003; Brunner et al., 2001; Cellini et al., 2010; Dehring et al., 2008; Fischel, 2001a, 2001b; Hilber & Mayer, 2009; Oates, 1969). We also contribute to a literature that investigates policies related to spatial externalities (Hansen & Libecap, 2004; Libecap & Lueck, 2011; Rossi-Hansberg et al., 2010), and a literature that investigates the costs and benefits of restrictive planning regimes (e.g. Cheshire & Hilber, 2008; Cheshire et al., 2011; Hilber & Vermeulen, 2010). Our results are also relevant to research that has looked into the value amenities add to neighbourhoods and cities more generally (e.g. Ahlfeldt et al., 2012; Bayer et al., 2007; Brueckner et al., 1999; Chay & Greenstone, 2005; Cheshire & Sheppard, 1995; Glaeser et al., 2001). Notably, there is also a growing body of literature that is investigating the property price effects of designation policies, mostly focused on the U.S. (e.g. Asabere et al., 1989; Asabere & Huffman, 1994; Asabere et al., 1994; Coulson & Lahr, 2005; Coulson & Leichenko, 2001; Glaeser, 2011; Leichenko et al., 2001; Noonan, 2007; Noonan & Krupka, 2011; Schaeffer & Millerick, 1991).

The key contribution of this study is to provide insights into the political economy of conservation area designation and to examine whether the outcome follows local homeowners interests. We also make a number of more specific, though still important contributions. Firstly, the theoretical framework we develop lends a structure to the designation process that helps to interpret the existing evidence that has typically been derived from ad-hoc empirical models. Secondly, our analysis of conservation area effects on property prices is one of the few rigorous analysis of this kind available for Europe (e.g. Ahlfeldt & Maennig, 2010; Koster et al., 2012; Lazrak et al., 2013) and the first to analyse England. It is unique in terms of the size and spatial detail of the data set and special in its focus on the spatial modelling of heritage externalities. Thirdly, our difference-in-differences analysis of the designation effects on property prices is one of the few studies that uses a quasi-experimental research design to separate the policy effect of designation from correlated location effects (Koster et al., 2012; Noonan & Krupka, 2011). Fourthly, we make use of a novel combination of RDD and DD approaches to identify the policy effects on outcome trends and discontinuities from quasi-experimental variation, which could be applied more generally to program evaluations. Fifthly, we provide one of the few empirical analysis of the determinants of heritage designation (Maskey et al., 2009; Noonan & Krupka, 2010, 2011). More generally, we establish a novel connection between the spatial outcome of a political bargaining process and one of the most striking contemporary urban phenomena: gentrification.

The structure of the paper is as follows. The next section introduces our theoretical model of heritage designations and the institutional setting. Section three presents our empirical strategy. A presentation and discussion of our empirical results is in section 4. The last section concludes.

2. Theory and context

2.1 Theoretical Framework

We assume that a linear neighbourhood exists along a spatial dimension x on the interval [0,1]. At each point along x there exists a small zone of housing which may be designated as a conservation area as a whole or not.[31] Housing in each zone is endowed with units of internal heritage according to the function $h(x)$, described below. The aggregate of the distribution of internal heritage across all

[31] The planner can either designate the whole zone or none of the zone, consistent with the idea of conservation areas as ensembles of buildings that work together to produce a desirable local character. Protection of single buildings is covered by listed building status. Designating a zone is assumed to approximate a marginal increase in the level of designation for the whole neighbourhood. Essentially the zone represents an infinitely small part of the whole neighbourhood.

zones gives the heritage character (external heritage) H of the whole neighbourhood at any point in time.

Owners in each zone care about their *initial endowment* of internal heritage $h(x)$, which is under their full control, and the *long run* external heritage, which may be damaged by their neighbours' (in all zones) property (re)developments. Such redevelopments occur in the long run with a probability of $(1 - \pi)$ where $0 \leq \pi < 1$ is the 'preservation probability' in the absence of conservation policies. The effect of designating a particular zone is to increase the preservation probability to 1 within that zone.[32] Therefore, the long-run external heritage depends on both the internal heritage distribution and the level of designation.

Within the neighbourhood, the initial internal heritage monotonically decreases in x. The theoretical argument does not depend on the functional form. For simplicity we assume *h(x)* to be a linear function of the heritage endowment of the zone at the neighbourhood's centre (h_0):

$$h(x) = h_0(1 - x) \tag{1}$$

One way to rationalize this distribution is to assume a neighbourhood that grew outwards from its historical centre (at $x = 0$) until the neighbourhood limit (at $x = 1$) and an internal heritage that strictly increases in the age of the housing unit.[33]

To protect the neighbourhood heritage, a planner can choose to designate all zones from the historical centre up to where $x = D$ and hence, a share $0 \leq D \leq 1$ of the neighbourhood. Since heritage is monotonically decreasing in x it is always rational to start designating at $x = 0$. By affecting the preservation probability, the designation share D determines the external heritage amount to be expected

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[32] Our argument does not depend on the assumption of full preservation probability, only that preservation is *more likely* inside conservation areas.

[33] Alternatively, x can simply be interpreted as the rank of a zone in the heritage distribution.

in the long run. The expected long-run external heritage derived from undesignated zones ($x > D$) corresponds to the integral of the distribution of internal heritage multiplied by the preservation probability, $\int_D^1 \pi h(x)\, dx$. This is added to the amount derived from designated zones ($x \leq D$), which is simply the integral of the internal heritage as the preservation probability is equal to one, $\int_0^D h(x)\, dx$.

$$E[H|D] = \int_0^D h(x)\, dx + \int_D^1 \pi h(x)\, dx \tag{2}$$

$$E[H|D] = h_0 \left(1 - \frac{D}{2}\right) D + \frac{\pi}{2} h_0 (1 - D)^2 \tag{3}$$

The expected external heritage integral $E[H|D]$ is indicated by the whole grey-shaded area in Figure 1 below. The expected amount of external heritage saved by the preservation policy is illustrated as the black-dotted area $\breve{H}$ which denotes the difference in (expected) external heritage between a scenario with no designation and a scenario with a designation share D. This amount is:

$$\breve{H} = h_0 (1 - \pi) \left(1 - \frac{D}{2}\right) D \tag{4}$$

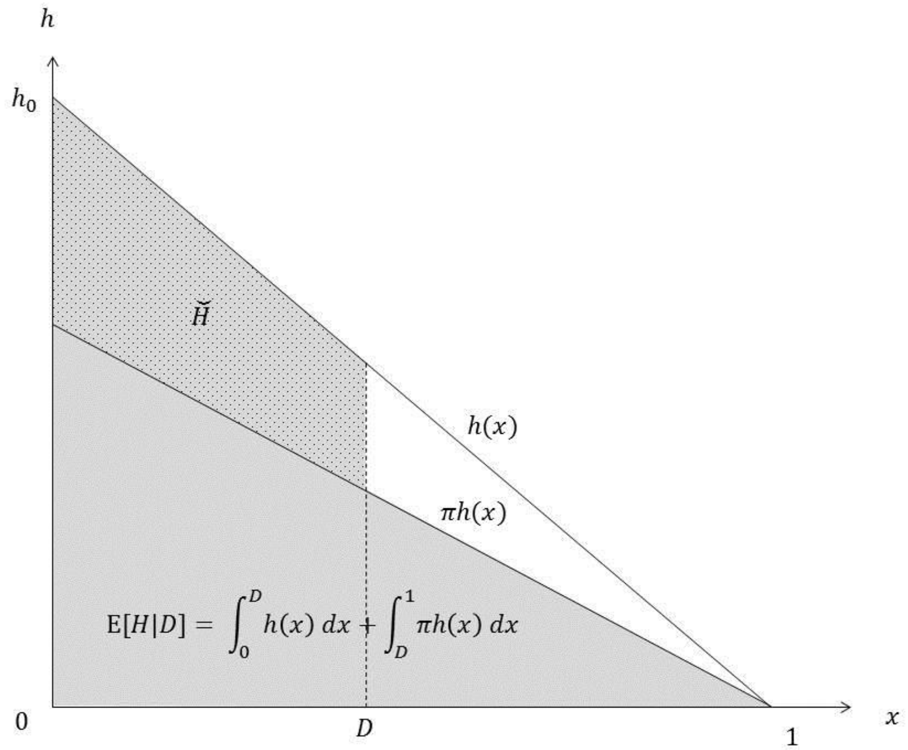
As evident from the partial derivatives, the amount of external heritage saved by the policy increases with the designation share but at a decreasing rate:

$$\frac{\partial \breve{H}}{\partial D} = \frac{\partial E[H|D]}{\partial D} = h_0 (1 - D)(1 - \pi) > 0 \tag{5}$$

$$\frac{\partial^2 \breve{H}}{\partial D^2} = \frac{\partial^2 E[H|D]}{\partial D^2} = -h_0 (1 - \pi) < 1 \tag{6}$$

The partial derivatives of $\breve{H}$ (which are the same as of H) with respect to D establish a central stylized fact of our theory: There are diminishing returns to designation.

Figure 1: Expected heritage distribution with partial designation



Notes: The function $h(x)$ gives the internal heritage at each zone in the neighbourhood. The expected external heritage is equal to the grey-shaded area and is the integral of $h(x)$ up to the designation share plus the integral of π times this $h(x)$ from the designation share until the neighbourhood limit at $x = 1$. The stippled area marked $\breve{H}$ is the amount of expected external heritage preserved by the policy.

To link the distribution of heritage in the neighbourhood to the utility U of a representative individual residing in a zone at x we define a utility function:

$$U(x) = A(x)X^{\delta}L^{1-\delta} \tag{7}$$

where X is a composite consumption good and L is housing space. The Cobb-Douglas form is motivated by the empirical observation that housing expenditure shares tend to be relatively constant across geographies and population groups (Davis & Ortalo-Magné, 2011). $A(x)$ is a composite amenities term:

$$A(x) = a(x)e^{\varphi h(x)}e^{\gamma E[H|D]}e^{-c\widetilde{D}(x)} \tag{8}$$

where a is a further composite indicator of m non-heritage amenities,[34] $h(x)$ is the internal heritage endowment (i.e., heritage character of the specific housing unit), φ is the internal heritage preference parameter, $E[H|D]$ is the external heritage (i.e., expected heritage of surrounding units, which depends on the designation policy) and is conditional on the designation share as defined above, γ is the external heritage preference parameter, and c represents the costs of designation policies, which arise from the development restrictions imposed inside conservation areas. The cost to a representative individual is $e^{-c\widetilde{D}(x)}$ and depends on their zone's designation status $\widetilde{D}(x)$, a binary function of x, which takes the value of one if $x \leq D$ and zero otherwise.

We assume that the designation of a single zone approximates a marginal change to the designation share of the neighbourhood as a whole. The positive utility effect of designating a single zone is therefore given by:

$$\frac{dU(x)}{dD} = \frac{\partial U}{\partial E[H|D]} \frac{\partial E[H|D]}{\partial D} = \gamma U(x) h_0 (1 - D)(1 - \pi) \tag{9}$$

The negative utility effect is incurred only by owners within the zone that changes designation status and is given by:

$$\frac{dU(x)}{d\widetilde{D}(x)} = \frac{\partial U}{\partial \widetilde{D}(x)} = -cU(x) \tag{10}$$

By setting these two equal we find D^*, which is the particular zone for which the net effect of designation will be zero:

$$D^* = 1 - \frac{c}{(1 - \pi)\gamma h_0} \tag{11}$$

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[34] Non-heritage amenities are given by: $a = b \prod_m \mathrm{a}_m{}^{\rho_m}$ where the different amenity levels are denoted a_m and are given a collective scaling factor b and individual parameters ρ_m.

The relevance of $D^*$to the planner can be explained in a context where the planner wishes to satisfy as many homeowners in the neighbourhood as possible. Simply put, the representative individual in all zones at x<D^* will want their zone to be designated because the benefits to them will outweigh the costs to them. However, in zones at x>D^* the cost of being designated outweighs the benefit for the representative individual. This makes D^* the equilibrium designation share for a planner that wishes to ensure that the representative homeowner in each zone is happy with their zones designation status.[35]

Based on this condition we can derive some useful comparative statics (see also Figure 1 in the Appendix). The equilibrium designation share is greater when people have a greater taste for external heritage γ or where there is altogether more heritage (determined by the heritage endowment at the neighbourhood centre h_0, and implicitly the age of the neighbourhood):

$$\frac{\partial D^*}{\partial \gamma} > 0 \tag{12}$$

$$\frac{\partial D^*}{\partial h_0} > 0 \tag{13}$$

The equilibrium level of designation decreases with the preservation probability π and the cost of designation c:

$$\frac{\partial D^*}{\partial \pi} < 0 \tag{14}$$

$$\frac{\partial D^*}{\partial c} < 0 \tag{15}$$

[35] It should be noted again here that this is not the optimal designation share in the sense of social welfare. The level D^* may be below the optimal level because the externality benefit is incident on all other zones in the neighbourhood. Further, it may be above the optimal level because designation reduces housing supply.

These theoretical implications are in line with intuition and can in principle be transformed into empirically testable hypotheses. However, the heritage at the neighbourhood centre h_0, the preservation probability π and the costs to owners of conservation policies c are all difficult to observe in reality. For that reason we will concentrate on testing the first comparative statics implication about taste for heritage (proxied by the education level of the local population) in the empirical section.

To develop a testable hypothesis on whether the equilibrium condition is fulfilled, i.e., the planner sets $D = D^*$, we incorporate capitalization effects in the next step. We first assume that individuals maximize their utility defined above subject to a budget constraint: $W = X + \theta(x)L$, where $\theta(x)$ is a housing bid rent. Furthermore we assume spatial equilibrium such that all zones offer the same level of utility $\bar{U}$ which we set equal to one:

$$U(x) = A(x)[\delta W]^{\delta}[(1-\delta)\frac{W}{\theta}]^{1-\delta} = \bar{U} = 1 \tag{16}$$

This can be rearranged to give the spatial equilibrium bid rents for a representative homeowner:

$$\theta(x) = (1-\delta)\left[\delta^{\delta} W a(x) e^{\varphi h(x)} e^{\gamma E[H|D]} e^{-c\widetilde{D}(x)}\right]^{\frac{1}{1-\delta}} \tag{17}$$

In keeping with intuition, the bid rent increases in the expected external heritage, which depends on the designation share D and the internal heritage endowment $h(x)$ and decreases in the designation cost, which is locally constrained to $x \leq D$ as defined above.

The spatial equilibrium condition can be used to derive the marginal effect of an increase in designation share on prices in the neighbourhood. In all zones in the neighbourhood a marginal increase in designation share D triggers a positive effect on prices through an increase in the expected external heritage. In the marginal zone, in addition, the change in designation status $\widetilde{D}$ also creates a cost.

$$\frac{d\theta(x)}{dD} = \begin{cases} \dfrac{\partial\theta(x)}{\partial E[H|D]}\dfrac{\partial E[H|D]}{\partial D} + \dfrac{\partial\theta(x)}{\partial\widetilde{D}(x)}d\widetilde{D}(x)\ if\ x = D \\ \dfrac{\partial\theta(x)}{\partial E[H|D]}\dfrac{\partial E[H|D]}{\partial D}\ if\ x \neq D \end{cases} \tag{18}$$

Substituting in the equilibrium designation share $D = D^*$ derived above we get:

$$\frac{d\theta(x)}{dD} = \begin{cases} \dfrac{\theta(x)}{1-\delta}\left[\gamma h_0\left(1 - 1 + \dfrac{c}{(1-\pi)\gamma h_0}\right)(1-\pi) - c\right] = 0\ if\ x = D \\ \dfrac{\theta(x)}{1-\delta}\left[\gamma h_0\left(1 - 1 + \dfrac{c}{(1-\pi)\gamma h_0}\right)(1-\pi) - c\right] = \dfrac{\theta(x)}{1-\delta}\ if\ x \neq D \end{cases} \tag{19}$$

The two conditions directly translate into two testable hypotheses. If the designation process is in equilibrium, we expect the marginal effect of designation on house prices to be zero in newly designated zones and to be positive at all other zones in the neighbourhood. Likewise, an excessive or restrictive designation policy will be associated with negative or positive marginal designation effects.

Assuming that the preservation probability (if undesignated) and the preservation costs are held constant our theory predicts that, in equilibrium designations occur as a result of an increase in the benefits associated with (external) heritage. Such increases in benefits will occur mechanically over time if the internal (and thus the external) heritage depends on housing age. The effective benefits will also increase as a result of neighbourhood turnover, if the in-migrating residents have larger heritage preferences than the incumbents. Designation then becomes a collateral effect of 'gentrification'. The older the conservation area, the greater the accrued benefits of designation may be.

Contrary to the assumption in our theory there is evidence suggesting that heritage externalities (Ahlfeldt & Maennig, 2010; Holman & Ahlfeldt, 2012) or housing externalities more generally (Rossi-Hansberg et al., 2010) decline quite steeply in distance. This means that there may not be a strong positive policy effect outside a newly designated conservation or it may at least be very spatially confined. Further, since our 'zones' are supposed to be infinitely small it may be that some new conservation areas represent the designation of several zones all in one go. For example in the case of a less than marginal change in the taste for heritage. In this case it would be the last zone, or the outer edge of the newly designated conservation area where we would expect a zero effect. There may be positive effects towards the centre of a conservation area (under the existence of spatial decay) where the internal heritage density is greater. Whilst we justify our simplified theory on the grounds that most conservation areas are small in reality even compared to the narrow scope of housing externalities, in the empirical section we allow for more flexibility to test these caveats.

2.2 Institutional context

In England, the designation of conservation areas started in 1967 and continues today under the provisions 69 and 70 of the Planning Act 1990 (Listed Buildings and Conservation Areas).[36] Conservation areas are those that have been identified as having "special architectural or historic interest, the character or appearance of which is desirable to preserve or to enhance" (Section 69). The Planning Policy Guidance Note 15 (PPG15) states that a conservation area "may form groups of buildings, open spaces, trees, historic street patterns, village greens or features of historic or archaeological interest. It is the character of the areas rather than individual buildings that conservation areas seek to enhance." Conservation areas are designated on the grounds of local and regional criteria. After the designation, the Local Authority has more control over minor developments and the demolition of buildings (Botrill, 2005). However, the

[36] However, the first legislation to protect the historic environment was enacted in 1882 when the Ancient Monuments Protection Act was passed to protect a small number of designated ancient monuments. More statutory measures came into force in the ensuing years, but it was the passage of the Ancient Monuments Consolidation and Amendment Act in 1913 that set out a more comprehensive legislative framework for the protection of ancient monuments.

protection an area receives when it is designated a conservation area is determined at the national level to reflect the wider interests of society.

In 2011 there were around 9,800 conservation areas in England. Conservation areas vary in character and size. Many have strong historical links, for example an architectural style associated with a certain period. Besides these characteristics, designation is made based on softer benefits said to have emanated from conservation area designation including: the creation of a unique sense of place-based identity, encouraging community cohesion, and promoting regeneration (HM Government, 2010).[37] This 'instrumentalisation' of conservation policy, which seeks to encompass heritage values, economic values, and public policy outcomes, has been identified as a key shift in the English policy context (Pendlebury, 2009; Strange, 2003). This is reflective of the notion of heritage not as a single definable entity, but as a political, social, cultural, and economic "bundle of processes" (Avrami, 2000cited in Pendlebury, 2009: 7).

In combination with bottom-up schemes leading to designation (e.g., community-led designation), the complex heritage preservation agenda which pursues a multitude of objectives and the institutional setting with responsibilities shared across several institutional layers creates significant scope for organized interest groups like property owners to influence the outcome of a political bargaining process.

3. Empirical Strategy

3.1 Designation process

The first potentially testable implications of our theoretical model are the partial derivatives (12) to (15). As mentioned in the theory section it is difficult to find feasible proxies for the variables π, c and h_0. We therefore concentrate on testing

[37] See for details HM Government (2010): *The Government's Statement on the Historic Environment for England*. London: DCMS.

the first of these conditions, i.e., the 'taste' for heritage γ has a positive effect on optimal designation share D^* in a neighbourhood. We adopt the common assertion that the demand for urban consumption amenities increases in education and income (Brueckner et al., 1999; Carlino & Saiz, 2008; Glaeser & Gottlieb, 2006; Shapiro, 2006; van Duijn & Rouwendal, 2013). In particular, we assume that the preference for heritage γ_n in a neighbourhood n is related to the share of people in the neighbourhood who hold a higher education certificate (DEG_i)[38] with the following functional form:

$$\gamma_{nt} = {DEG_{nt}}^{\vartheta} e^{-\varepsilon_{nt}} \tag{20}$$

where $\vartheta > 0$ such that the relationship is positive. The selection of DEG as educational proxy is driven by data availability. It is perhaps notable that assuming 12 [16] years of education for non-degree [degree] holders a 100% increase in degree share is synonymous to an additional average year of education in a ward with an initial degree share of 25%. Since the purpose of our empirical exercise is to evaluate the causal impact of changes in heritage preferences on designation status – and not the causal impact of education on heritage preference – it is sufficient to assume that ϑ captures a correlation between education and heritage preferences. ε_{nt} is a random disturbance term capturing determinants of heritage preferences that are not correlated with education. Rearranging the equilibrium designation share equation (11), substituting the education degree proxy relationship and taking logs we arrive at the following empirical specification:

$$\log(1 - D_{nt}) = \alpha - \vartheta \log(DEG_{nt}) - \omega_n + \varepsilon_{nt} \tag{21}$$

$$\text{where } \alpha = \log(1-\pi) - \log(c) \text{ and } \omega_n = \log(h_{0n}) + l_n. \tag{22}$$

[38] We also use income as a proxy for a subsample of our data set – results are reported in the appendix.

The n subscripts correspond to the individual 'neighbourhoods' of our theoretical model and we choose to represent these empirically as UK Census wards. Wards are the smallest geographical areas that are comparable between 1991 and 2011 censuses. Subscript t stands for time periods for which we use the Census years of 1991 and 2011. All idiosyncratic time-invariant location components l_n (location-specific determinants of designation not modelled in our theory) and the unobserved heritage endowment h_{0n} of a neighbourhood n as captured by ω_n as well as the preservation probability π and the costs to owners of conservation policies are removed by taking first-differences:

$$\Delta\log(1 - D_n) = \Delta\alpha - \vartheta\,\Delta\log(DEG_n) + \Delta\varepsilon_n \tag{23}$$

Our estimation equation now depicts that a neighbourhood change reflected in a positive change in (log) educational degree share causes the (logged) share of non-designated land on the left-hand side to decrease. This is just another way of saying that a positive change in educational degree leads to a higher designation share, although the transformation is non-linear. Note that we implicitly assume that we are in equilibrium in the sense that all areas that should be designated at t are in fact designated. To support the case, we estimate our model using a long difference between 1991 and 2011, which is more than two decades after the start of the policy and the initial wave of designations. Results for the smaller differences between 1991–2001, and 2001–2011 respectively, are reported in the appendix.

Equation (23) evidently follows from a stylized model world. In the empirical implementation we add a number of covariates to control for alternative determinants of designation. The on-going designation is then only determined by the local changes in preferences and the steady aging of buildings and the effects on heritage, which are differentiated out. To control for the contagion effects in designation we add the initial (1991) designation share which we instrument with the share in 1981 to avoid a mechanical relationship with the dependent variable. A number of variables are added to account for heterogeneity in the net benefits of designation and abilities to express (collective) opinions in a political bargaining that may influence the designation decision. These include the initial (1991) degree share, the homeownership rate, the household size, the average population age, and the share of foreigners (both in initial shares and changes). We alter the baseline model in a number of robustness checks to account for institutional heterogeneity at the TTWA level, neighbourhood appreciation trends and, to the extent possible, the historic and physical quality of the housing stock.

In practice, however, it is difficult to control for all determinants of designation that are external to our model. One particular concern is that areas can be designated if the heritage is threatened by poor maintenance in a declining neighbourhood. Such derelict is likely to be negatively correlated with our explanatory variable and is unlikely to be fully captured by the control variables we have at hand. At the same time, the policy itself could make it more likely that educated people are attracted to designated areas due to a different valuation of uncertainty (reverse causality). Since an OLS estimation of equation (23) can result in a significant bias in either direction we make use of instrumental variables z_n, which predict changes in education, $\rho(z_n, \Delta\log DEG_n) \neq 0$, but must be conditionally uncorrelated with the differenced error term, $\rho(z_n, \Delta\varepsilon_{nt}) = 0$. We argue that rail station (in London additionally Tube station) density as well as effective employment accessibility (both time-invariant in levels) are good predictors of neighbourhood gentrification (Florida, 2002a; Glaeser et al., 2001).[39] We also argue that it is unlikely that these level variables directly impact on the likelihood of designation conditional on the unobserved heritage endowment in the fixed effects ω_n.

[39] Our measure of effective employment accessibility aggregates employment in surrounding regions weighted by distance. We use exponential distance weights that are popular in the theoretical (Fujita & Ogawa, 1982; Rossi-Hansberg et al., 2010) and the decay parameter estimate provided by Ahlfeldt (2013). Transport infrastructure is captured by a kernel density measure (Silverman, 1986a) with a radius of 2 km which is considered to be the maximum distance people are willing to walk (Gibbons & Machin, 2005).

Another empirical concern is that, theoretically, a decrease in preferences for heritage must provoke a reduction of the designated area. The abolishment of conservation areas, however, is extremely rare in England so our data is left-censored (we do not observe increases in the share of non-designated land). Since we are interested in testing whether the mechanisms emphasized by the model are at work, and not simply the causal effect of changes in degree share on designation share, we take the model to the data using a tobit approach:

$$Y_n^* = \Delta\alpha - \vartheta\,\Delta\log(DEG_n) + \Delta\varepsilon_n, \qquad \Delta\varepsilon_n \sim N(0,\sigma^2) \tag{24}$$

where $Y_n^* = \Delta\log(1 - D_n)$ is a latent variable and the observed variable is defined as follows:

$$Y_n = \begin{cases} Y_n^*, & if\ Y_n^* = \Delta\log(1-D_n) \ < 0 \\ 0, & if\ Y_n^* \qquad\qquad\qquad \geq 0 \end{cases} \tag{25}$$

3.2 Equilibrium designation

To test whether the designation share in practice is set at the equilibrium level (D^*) we employ hedonic regression (Rosen, 1974) to estimate the effect of the event of designation on property prices within and surrounding conservation areas. In its essence our quasi-experimental methods are a derivative of the established difference-in-differences (DD) methodology (e.g. Bertrand et al., 2004). We draw elements of the increasingly popular regression discontinuity designs (RDD) (Imbens & Lemieux, 2008), however, to relax the DD assumptions of homogeneous trends and a singular treatment date to separate smooth variation (e.g., externalities) and discontinuities (e.g., conservation area boundaries) in treatment effects from correlated unobservables.

Difference-in-differences

We define a group of 912 'treated' conservation areas as those that were designated between the years 1996 and 2010 to ensure we observe property transactions both before and after the designation date. Our counterfactuals are established via various control groups of housing units that are similar to the treated units but are themselves not treated. These control groups are discussed in more detail in the results section and in the appendix (Section A2.2).

Our baseline DD model takes the following form:

$$p_{it} = \beta^I I_i + \beta^E E_i + \beta^{IPost}(I_i \times Post_{it}) + \beta^{EPost}(E_i \times Post_{it}) + \mathrm{X}_i'\mu + f_n + Y_t + \epsilon_{it} \tag{26}$$

where p_{it} is the natural logarithm of the transaction price for property i in time period t, I_i is a dummy variable equal to one if the observation is internal to a treated conservation area, E_i indicates observations external to the treated CA. While our standard models use a buffer area of 500m we also experiment with various alternative spatial specifications. $Post_{it}$ is a dummy variable indicating whether the transaction year t is equal to or greater than the designation year, X_i is a vector of controls for property, neighbourhood and environmental characteristics, f_n is a set of n location fixed effects and Y_t are year effects. The β^{IPost} and β^{EPost} parameters give the difference-in-differences estimates of the designation effect on the properties within and just outside a conservation area. We show in Appendix 2.2 that β^{IPost} is equal to the net marginal policy (designation costs and benefits) effect while β^{EPost} reflects the pure (albeit spatially discounted) policy benefit.

Temporal regression discontinuity design of differences (RDD-DD)

The standard DD specification (26) identifies the policy treatment effect under some arguably restrictive assumptions. Firstly, the treatment and control groups follow the same trend before and after the treatment. Secondly, the treatment occurs at a singular and *a priori* known date and affects the level (and not the trend) of the outcome variable. These assumptions are evidently violated if the outcome variable does not respond immediately to the treatment, e.g., because of costly arbitrage, or in anticipation of the treatment, for example because of an investment motive by buyers (Ahlfeldt & Kavetsos, 2013). In our case, a positive pre-trend can also be associated with the gentrification that causes designation according to our theoretical model, a reverse causality problem.

To address these limitations of the standard DD we refine the model to accommodate differences in trends across the treatment and the control group. We borrow the functional form from the RDD literature where a (temporal) treatment effect is identified as an instant adjustment – a discontinuity – conditional on higher order polynomial (pre- and post-) trends, which are assumed to be unrelated to the treatment (Bento et al., 2010). In our regression discontinuity design of differences (RDD-DD) we combine an RDD-type polynomial specification of trends with the control group-based counterfactual from the DD. It is therefore possible to attribute pre- and post-trends to the treatment as long as it is credible to assume that treatment and control groups would have followed the same trend in the absence of the treatment. It is notable that even if this assumption is violated the RDD-DD (unlike the standard RDD) will at least remove macro-economic shocks from the treatment effect by taking differences from the control group. This improves identification so long as the control group remains unaffected by the treatment. Our RDD-DD with linear trends takes the following form:

$$\begin{aligned} p_{it} = {} & \beta^I I_i + \beta^{IYD}(I_i \times YD_{it}) + \beta^E E_i + \beta^{EYD}(E_i \times YD_{it}) \\ & + \beta^{IPost}(I_i \times Post_{it}) + \beta^{IPostYD}(I_i \times Post_{it} \times YD_{it}) \\ & + \beta^{EPost}(E_i \times Post_{it}) + \beta^{EPostYD}(E_i \times Post_{it} \times YD_{it}) + \mathrm{X}_i'\mu \\ & + f_n + Y_t + \epsilon_{it} \end{aligned} \tag{27}$$

where YD_{it} is the number of years since the designation date, with the pre-designation years having negative values. As in the RDD, the polynomial degree of the trend can be increased subject to sufficient degrees of freedom. We make use of a quadratic trend specification and evaluate the fit of the parametric polynomial function using a semi-parametric version of (27) that replaces the YD_{it} variables with full sets of years-since-designation effects (details in the appendix).

A significant 'dis-in-diff' parameter (β^{IPost} or β^{EPost}) can be entirely attributed to the treatment even under the existence of complex relative trends that are unrelated to the treatment or may even have caused the treatment as the comparison is made just before and just after the treatment date. Under the assumption of homogeneous counterfactual trends the significant pre-trend parameters (β^{IYD} or β^{EYD}) describe the anticipation effects. Significant post-trend parameters ($\beta^{IPostYD}$ or $\beta^{EPostYD}$) then indicate changes in relative trends after the treatment. In conjunction, the 'dis-in-diff' and the pre- and post-trend parameters describe the full temporal structure of the treatment effect. As a program evaluation tool that is applicable to a variety of event studies, the RDD-DD thus naturally comes with a stronger test (dis-in-diff) and a weaker test (trends) of whether there exists an effect of the treatment.

Spatial regression discontinuity design of difference-in-differences (RDD-DD)

In contrast to our theory, in reality there most likely exists a spatial decay to the heritage externalities. This decay implies that the external heritage effect should be stronger at the centre of the conservation area than at the boundaries. The policy benefit, which is a transformation of the external heritage effect, should also be greater at the centre of the newly designated conservation area. Likewise, the predicted positive policy effects just outside the boundary should be decaying in distance to the conservation area (CA) boundary. At the CA boundary there may be a discontinuity as the cost of the policy ends abruptly at the boundary, whereas potential externalities decay smoothly across it. The combination of trends and discontinuities potentially caused by the treatment resembles the temporal identification problem just described and will be addressed by a similar combination of RDD and DD tools. Essentially, we use the RDD tools to capture how the difference (before and after) in the differences (treatment vs. control) of property prices varies along the (internal and external) distances from the CA boundary. Unlike in the standard (spatial) RDD, unobserved time-invariant spatial effects can be held constant due to the availability of spatiotemporal variation.[40] In our spatial RDD-DD model it is therefore possible to attribute spatial trends (with respect to distance to the CA boundary) as well as a

[40] Dachis, Duranton, & Turner (2012) also make use of spatiotemporal variation in their RDD. Our specification additionally takes differences from a control group.

discontinuity (at the CA boundary) to the treatment provided that the spatial trends are uncorrelated with unobserved temporal trends.

The spatial RDD-DD we estimate takes the following form:[41]

$$p_{it} = \beta^{I}T_i + \beta^{ID}(T_i \times D_i) + \beta^{IPost}(T_i \times Post_{it}) + \beta^{IDPost}(T_i \times D_i \times Post_{it}) + \beta^{E}O_i + \beta^{OD}(O_i \times D_i) + \beta^{OPost}(O_i \times Post_{it}) + \beta^{ODPost}(O_i \times D_i \times Post_{it}) + \mathrm{X}_i'\mu + f_n + Y_t + \epsilon_{it} \tag{28}$$

where D_i is the distance from the property to the conservation area boundary (internal distances are negative values), O_i indicates properties outside a treated conservation area and T_i indicates the conservation area that is nearest to a property that is treated at any point of the study period. In order to fully explore the extent of spatial externalities O_i indicates a larger area outside CAs[42] rather than just within 500m as indicated by E_i in previous models. As with the temporal RDD-DD specification we also estimate an expanded model specification in which we allow for quadratic distance trends and semi-nonparametric specifications replacing the distance variable with some distance bin effects.

The coefficient β^{IPost} gives the intercept of the internal effect (i.e., the internal effect at the boundary) and β^{IDPost} estimates how this changes with respect to internal distance. Jointly, these terms capture the net policy costs and benefits of designation for internal treated areas. A zero β^{IPost} coefficient would be reflective of a zero effect at the boundary and would be in line with the optimality condition derived in the theory section. A negative β^{IDPost} would be in line with the existence of policy benefits (due to increased preservation probability) that

[41] In models with historical CAs as control groups the following terms are also included $\beta^{CD}(C_i \times D_i) + \beta^{EC}EC_i + \beta^{ECD}(EC_i \times D_i)$, where C_i indicates internal to control CA and EC_i external to control CA. This ensures that spatial effects are estimated conditional on the spatial trends in control CA.

[42] Specifically, the empirical analysis uses properties within 1,400m of the treated conservation area.

spillover with decay. The parameters β^{EPost} and β^{EDPost} allow for a spatial discontinuity treatment effect at the boundary and heterogeneity in spatial trends inside and outside the treated areas. As with β^{IDPost}, a jointly negative $\beta^{IDPost} + \beta^{EDPost}$ would be in line with the decaying policy benefits external to the conservation area. The discontinuity at the border is measured by the external intercept term β^{EPost}. A statistically positive estimate would indicate a cost to the policy. A jointly positive effect of $\beta^{IPost} + \beta^{EPost}$ would in turn indicate the existence of policy benefits.

4. Data

We have compiled two distinct data sets for the two stages of the empirical analysis. Both data sets make use of data provided by English Heritage. These include a precise GIS map of 8,167 conservation areas in England, the Conservation Areas Survey containing information on community support and risk status (average condition, vulnerability and trajectory of a conservation) and a complete register of listed buildings.

For the analysis of the determinants of designation we use UK census wards as a unit of analysis. Shares of designated land within each Census ward are computed in a Geographical Information Systems (GIS) environment. Various ward level data on educational level, age, ethnical background, average household size and homeownership status and vacancy rate were obtained from the UK Census. Any changes in ward boundaries between the years were corrected for using the online conversion tool GeoConvert.[43] For robustness tests we also collected a measure of the ward's average income (Experian). The instrumental variables station density and employment potential are regenerated data that stem from Nomis (workplace employment) and the Ordinance Survey (rail stations). The average turnover in housing is approximated as the number of properties

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[43] http://geoconvert.mimas.ac.uk/

transacted per year in a ward as recorded in the Nationwide Building Society data set (see below).

For the analysis of the capitalization effects of designation we use transactions data related to mortgages granted by the Nationwide Building Society (NBS) between 1995 and 2010. The data for England comprise 1,088,446 observations and include the price paid for individual housing units along with detailed property characteristics. These characteristics include floor space (m^2), the type of property (detached, semi-detached, flat, bungalow or terraced), the date of construction, the number of bedrooms and bathrooms, garage or parking facilities and the type of heating. There is also some buyer information including the type of mortgage (freehold or leasehold) and whether they are a first-time buyer. Importantly, the transaction data includes the full UK postcode of the property sold allowing it to be assigned to grid-reference coordinates.

With this information it is possible within GIS to calculate distances to conservation area borders and to determine whether the property lies inside or outside of these borders. Furthermore, it is possible to calculate distances and other spatial measures (e.g., densities) for the amenities and environmental characteristics such as National Parks, as well as natural features like lakes, rivers and coastline. The postcode reference also allows a merger of transactions and various household characteristics (median income and ethnic composition) from the UK census, natural land cover and land use, various amenities such as access to employment opportunities, cultural and entertainment establishments and school quality. A more detailed description of all the data used is in the appendix.

5. Results

5.1 Designation process

Table 1 reports the results of our tobit model of the designation process defined in equation (24). The non-instrumented baseline model is in column (1). As predicted by our theory, increases in educational levels that are presumably correlated with heritage preferences are associated with reductions in the share of non-designated land. More precisely, an increase in the degree share by 1% is associated with a 0.12% reduction in the share of non-designated land. This decrease corresponds to an $0.12\% \times (1 - \overline{D}_{t-1})/\overline{D}_{t-1} = 2.61\%$ increase in the share of designated land for a ward with the mean of the positive initial designation shares $\overline{D}_{t-1} = 4.4\%$. The effect substantially increases once we instrument the change in degree share using rail station density and employment potential (column 2). This increase is in line with unobserved (positive) deterioration trends that a) increase the likelihood of designation and b) are negatively correlated with changes in degree share. Introducing the instruments, the effect of a 1% increase in degree share on the share of non-designated land increases to 0.88%, which for a ward with the mean initial designation share $\overline{D}_{t-1}$ corresponds to an increase in the designated land share of about 19%. While we have argued that our estimates are supposed to reflect a causal estimate of gentrification (proxied by degree shares) on designation probabilities and not necessarily a causal effect of degree share on designation share, a parameter estimate of $\hat{\vartheta} = 0.88$ is at least indicative of heritage preferences increasing relatively steeply in education. It is notable that increases in the share of designated land are also positively correlated with high initial levels of degree shares.

The remaining columns in Table 1 provide variations of the benchmark model (2). We add TTWA effects to control for unobserved institutional heterogeneity in column (3). Column (4) adds several conservation area characteristics that capture historic quality (e.g. number of listed buildings), risk (e.g. various measures capturing vulnerability and trajectory) and development pressure (e.g. vacancy rate). The latter includes a measure of property price appreciation, which we obtain from ward-level regressions of log property prices on a time trend (and property controls, see the appendix for details). With this variable we control for a potentially positive correlation between owners' risk aversion and the value of their properties – typically their largest assets. This is a potentially important control since a larger risk aversion increases the benefit from a policy that increases certainty regarding the future of the neighbourhood and, thus, potentially increases the optimal designation share. It is a demanding control since positive price trends are potentially endogenous to changes in neighbourhood composition and may thus absorb some of the gentrification effect on designation. Specification (5) replicates the benchmark model on a reduced sample of predominantly residential to ensure that the results are not driven by commercial agents, which we don't model in our theory.[44] None of these model alterations changes the education effect substantially. Model (6) tests for an interaction effect between homeownership rate and degree share. We find that the (positive) impact of neighbourhood change on designations shares (interaction term) is particularly large in high homeownership areas (see column 6). This is in line with a political economy literature that suggests that homeowners tend to form well-organized interest groups (e.g. Brunner & Sonstelie, 2003; Dehring et al., 2008; Fischel, 2001a).

The results in Table 1 offer some further interesting insights on potential determinants of designation. We do not find evidence in support of contagion effects in designation, i.e., designated land shares do not tend to increase where shares were initially high. The likelihood of designation rises with ward population age, which could be related to a higher appreciation of heritage by the elderly. The likelihood declines in the share of foreigners, which, likewise, could reflect a lower appreciation among people with different cultural backgrounds. An alternative and potentially complementary explanation may be a lack of familiarity with the institutional context and, thus, a difficulty to `game the system'.

[44] In the results reported we drop wards with more workers than inhabitants, which amount to about 7.4% of the total sample. The results do not change qualitatively even if we drop the top quintile according to the same metric.

Table 1: Designation process

	(1) Tobit	(2) IV Tobit	(3) IV Tobit	(4) IV Tobit	(5) IV Tobit	(6) IV Tobit
			Δ log share non designated land (t)			
Δ log degree share (t) (ϑ)	-0.112***	-0.875***	-0.754***	-0.794***	-0.874***	-0.871***
	(0.022)	(0.105)	(0.136)	(0.100)	(0.100)	(0.103)
log degree share (t-1)	-0.116***	-0.426***	-0.401***	-0.394***	-0.438***	-0.403***
	(0.012)	(0.043)	(0.060)	(0.042)	(0.042)	(0.041)
log designation share (t-1)	-0.005***	0.003*	0.005**	0.004**	0.004**	0.003*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Δ log homeownership (t)	0.207***	0.618***	0.563***	0.582***	0.658***	0.530***
	(0.034)	(0.067)	(0.082)	(0.073)	(0.070)	(0.061)
log homeownership (t-1)	0.134***	0.195***	0.208***	0.220***	0.238***	0.588***
	(0.020)	(0.023)	(0.026)	(0.029)	(0.027)	(0.065)
Δ log average household size (t)	0.037	-0.336***	-0.205**	-0.346***	-0.454***	-0.121
	(0.050)	(0.074)	(0.082)	(0.076)	(0.086)	(0.074)
log average household size (t-1)	-0.027	-0.304***	-0.289***	-0.376***	-0.229***	-0.353***
	(0.058)	(0.074)	(0.082)	(0.077)	(0.078)	(0.076)
Δ log pop age (t)	-0.014	-0.277***	-0.214**	-0.332***	-0.477***	-0.078
	(0.068)	(0.081)	(0.084)	(0.091)	(0.100)	(0.084)
log pop age (t-1)	-0.109***	-0.252***	-0.275***	-0.288***	-0.232***	-0.263***
	(0.055)	(0.062)	(0.068)	(0.074)	(0.066)	(0.063)
Δ log share of foreigners (t)	0.004	0.075***	0.066***	0.074***	0.071***	0.051***
	(0.011)	(0.015)	(0.017)	(0.015)	(0.015)	(0.014)
log of share of foreigners (t-1)	-0.003	0.079***	0.051***	0.079***	0.083***	0.071***
	(0.007)	(0.013)	(0.016)	(0.013)	(0.013)	(0.012)
log price trend				0.017		
				(0.022)		
Δ log vacancy rate (t)				-0.003		
				(0.010)		
log vacancy rate (t-1)				-0.009		
				(0.015)		
log turnover in housing				-0.007		
				(0.006)		
log listed buildings density				-0.003		
				(0.004)		
log of share of building from				-0.021***		
pre1945				(0.006)		
average condition score				-0.069***		
(1 best, 4 worst)				(0.020)		
average vulnerability score				-0.052***		
(1 low, 8 high)				(0.019)		
average trajectory score				0.037		
(-2 improving, +2 deteriorating)				(0.038)		
Δ log degree share (t)						-0.953***
x homeownership (t-1)						(0.138)
Constant	0.490**	1.470***	1.565***	1.801***	1.351***	1.724***
	(0.235)	(0.286)	(0.323)	(0.360)	(0.300)	(0.299)
TTWA Effects	NO	NO	YES	NO	NO	NO
Residential wards only	NO	NO	NO	NO	YES	NO
CHI2		328.334	617.186	491.909	312.116	332.841
EXOG_P		0.000	0.000	0.000	0.000	0.000
OVERID		0.001	.	0.435	5.805	0.242
OVERIDP		0.981	.	0.509	0.016	0.623
Observations	7965	7965	7965	7965	7379	7965

Notes: See the data section for a description of control variables. IVs are station density, employment potential and degree share in 1981 in all models except model (1). Standard errors in parentheses. $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

Further robustness

While our IVs comfortably pass the typical statistical tests, we have experimented with four alternative sets of IVs. We have also split up the 1991–2011 long difference into two shorter differences (1991–2001 and 2001–2011), used the change in income as a proxy for heritage preferences (for 2001–2011) and run the baseline model in OLS keeping only observations with positive changes in shares of designated land. The results are presented in the technical appendix and support those discussed here.

5.2 Equilibrium designation

Difference-in-differences

Table 1 shows the results from an estimation of the standard DD equation (26) for different selections of control groups and fixed effects. Each model includes controls for property, location, and neighbourhood characteristics, year effects and location fixed effects to hold unobserved time-invariant effects constant. Column (1) is a naive DD using the mean price trend of all properties located beyond 500m of a treated conservation area as a counterfactual. Columns (2) to (7) provide more credible counterfactuals by restricting the control group to properties that are presumably similar to the treated properties. Column (2), with ward fixed effects, and (3), with nearest CA fixed effects, provide a spatial matching by restricting the sample to properties within 2km of a treated CA, where many unobserved *location* characteristics are likely to be similar. In column (4) we impose the additional restriction that properties in the control group must fall within 500m of the boundaries of a historically designated conservation area (before 1996), which increases the likelihood of unobserved *property* characteristics being similar. While areas that are designated at any point in time are likely to share many similarities, the diminishing returns to designation in our theoretical framework also imply that heritage-richer areas should generally be designated first. To evaluate whether the designation date of the treated conservation areas, relative to those on the control group, influences the DD estimate, we define CA designated 1996–2002 as a treatment group and form control groups based on CAs designated just before (1987–1994) or right after (2003–2010) in columns (5) and (6). In column (7), finally, we use environmental, property and neighbourhood characteristics to estimate the propensity of being in a treated (1996–2010) CA over a historical (<1996) CA. Then the treated CAs are matched to their 'nearest-neighbour', i.e., the most similar non-treated CA, based on the estimated propensity score (Rosenbaum & Rubin, 1983). A fixed effect is defined for each treated CA and its nearest-neighbour control CA such that the treatment effect is estimated by the direct comparison between the treated CA and its nearest-neighbour.

We anticipate that the strength of the counterfactual increases as we match the treatment and control group based on proximity (2 & 3), proximity and qualifying for designation (4, 5, & 6) and qualifying for designation and a combination of various observable characteristics (7). As the credibility of the counterfactual increases, the statistical significance of the treatment effect tends to decrease. Benchmarked against the nationwide property price trend both the internal effect (Inside × Post) and the external effect (Within 500m × Post) are significant at the 5% level. The magnitudes of these effects are of similar size, implying a 2.8% premium for houses inside newly designated conservation areas and a 2.3% premium outside. The spatial matching (2 & 3) renders the internal treatment effect insignificant (2 & 3). With further refinements in the matching procedure the external effect also becomes insignificant. Table 2 results, thus, suggest that designation does not lead to significant property price adjustments. Evidence is weak for positive (policy) spillovers to nearby areas.

Table 2: Conservation area premium – designation effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	log property transaction price						
Inside treated CA × Post designation	0.028*** (0.009)	0.014 (0.009)	0.014 (0.010)	0.003 (0.012)	-0.024 (0.070)	-0.077 (0.111)	-0.003 (0.013)
Within 500m buffer of treated CA × Post des.	0.023*** (0.004)	0.013*** (0.004)	0.012*** (0.005)	0.004 (0.006)	0.012 (0.027)	-0.005 (0.022)	-0.005 (0.010)
Inside treated CA	-0.043*** (0.009)	-0.038*** (0.009)	-0.048*** (0.010)	-0.037*** (0.012)	-0.062 (0.057)	0.029 (0.108)	-0.024 (0.021)
Within 500m buffer of treated CA	-0.010** (0.004)	-0.004 (0.004)	-0.011** (0.005)	0.005 (0.005)	0.003 (0.030)	0.006 (0.023)	-0.002 (0.013)
Hedonic controls	YES	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES	YES
Neighbourhood cont.	YES	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES	YES
Ward effects	YES	YES	NO	NO	NO	NO	NO
Nearest treat. CA effects	NO	NO	YES	YES	YES	YES	NO
Matched CA effects	NO	NO	NO	NO	NO	NO	YES
Treatment group: CAs designated	1996-2010	1996-2010	1996-2010	1996-2010	1996-2002	1996-2002	1996-2010
Control group	Full England sample	Within 2km of treated CA	Within 2km of treated CA	Within 500m of CA designated before 1996 & within 2km of treated CA	Within 500m of CA designated 1987-1995 & within 4km of treated CA	Within 500m of CA designated 2003-2010 & within 4km of treated CA	Within 500m of pre-1996 CA matched on propensity score
R^2	0.921	0.922	0.915	0.915	0.861	0.864	0.909
AIC	-587,375	-156,426	-130,469	-67,046	-5,408	-8,475	-41,184
Observation	1,088k	302k	302k	178k	21k	32k	133k

Notes: Standard errors in parentheses are clustered on location fixed effects. Conservation area control groups in columns (4)-(7) have separate fixed effects for the areas inside and outside a conservation area. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Temporal RDD-DD

Table 3 illustrates the results of the estimation of the (temporal) RDD-DD outlined in equation (27). We present the results of a variety of models that feature linear (1–5) and quadratic (6–10) trends and several of the control groups utilized in Table 2 One important finding across these specifications is that the external (Within 500m × Post) 'dis-in-diff' parameter estimate is significant in four of 10 specifications at the 5% level and in one half of the specifications at the 10% level, whereas, the internal (Inside × Post) parameter is only significant in one specification at the 10% level (column 8). This suggests primarily that there exists a significant treatment effect exactly at the treatment date only for the external area. This interpretation is in line with the predictions of our theoretical model. Another finding illustrated by Table 3 is the positive change in the internal price trend after a CA has been designated (Inside treated CA × Post designation × Years designated). The change in trend, which is significant at the 5% level in seven of the 10 models, may be regarded as evidence for a cumulative internal effect of the designation policy. There is also a faster appreciation in the external area post-designation that is significant in four of the 10 models. In short, the temporal RDD-DD has confirmed that designation policy causes no immediate effect inside the conservation area but shows instead that it increases the speed of price appreciation over time. The RDD-DD has also uncovered that areas external to the conservation area receive an immediate shift in prices at the designation date in line with our theoretical hypothesis.

Figure 2 provides a graphical illustration of the predicted effect of being in the treatment group over the control group against years-since-designation. A horizontal red line is drawn at the mean of the pre-treatment effects in order to illustrate the differences between the RDD-DD results and those of the standard DD. The positive impact of designation on (relative) price trends suggested by the RDD-DD (black lines) is supported by the functionally more flexible semi-parametric estimates for the 'years-since-designation bins' (grey dots).[45] However, the post-treatment effects are never statistically distinguished from the pre-period mean, which is in line with the DD estimates.

Figure 3 provides an analogical illustration for the external treatment effect, i.e., the spillovers onto areas adjacent to the designated CAs. Again, the post-period estimates do not deviate significantly from the pre-period mean. However, the top-left panel illustrates a large discontinuity at the treatment date that is statistically significant in Table 3. As with the internal effects, there is a positive trend shift post-designation.

[45] Confidence bands for the semi-parametric 'bins' model are presented in the appendix.

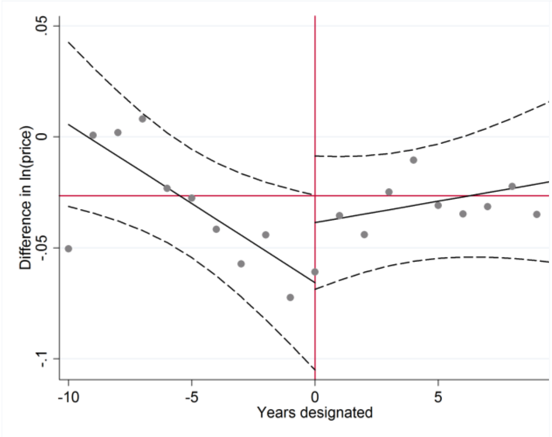
Table 3: Regression discontinuity design of differences between treatment and control (RDD-DD)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	log property transaction price									
Inside treated CA × Post designation	0.015	0.022	0.024	0.027	-0.006	0.023	0.033	0.038*	0.036	0.020
	(0.015)	(0.015)	(0.015)	(0.017)	(0.018)	(0.023)	(0.021)	(0.023)	(0.024)	(0.024)
Within 500m buffer of treated CA	0.006	0.013*	0.015**	0.020**	-0.007	0.013	0.017**	0.022**	0.017	0.009
× Post designation	(0.007)	(0.007)	(0.007)	(0.008)	(0.012)	(0.008)	(0.008)	(0.009)	(0.010)	(0.014)
Inside treated CA × Years designated	0.000	-0.004	-0.004	-0.007**	-0.002	-0.010	-0.016*	-0.019*	-0.019*	-0.020*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.010)	(0.009)	(0.010)	(0.010)	(0.011)
Inside treated CA × Years designated^2						-0.001	-0.001	-0.001	-0.001	-0.002*
						(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Inside treated CA × Post designation	0.003	0.007**	0.008**	0.009**	0.008*	0.020	0.026**	0.032**	0.031**	0.031**
× Years designated	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.014)	(0.012)	(0.013)	(0.013)	(0.014)
Inside treated CA × Post Designation						0.000	0.000	0.000	0.000	0.001
× Years designated^2						(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Within 500m of treated CA	0.002	-0.002*	-0.002*	-0.004***	-0.001	-0.001	-0.004	-0.007*	-0.004	-0.009
× Years designated	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)	(0.004)	(0.004)	(0.005)	(0.007)
Within 500m of treated CA						-0.000	-0.000	-0.000	0.000	-0.001
× Years designated^2						(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Within 500m of treated CA	0.001	0.004***	0.004***	0.005***	0.003	0.003	0.007	0.011**	0.008	0.009
× Post designation ×Years des.	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)	(0.005)	(0.005)	(0.005)	(0.006)	(0.010)
Within 500m of treated CA						0.000	0.000	0.000	-0.000	0.001
× Post designation × Years des.^{2}						(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Hedonic controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Neighbourhood controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Ward effects	YES	YES	NO	NO	NO	YES	YES	NO	NO	NO
Nearest treated CA effects	NO	NO	YES	YES	NO	NO	NO	YES	YES	NO
Matched CA effects	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES
Control group as in Tab. 2, column	(1)	(2)	(3)	(4)	(7)	(1)	(2)	(3)	(4)	(7)
R^2	0.920	0.921	0.912	0.914	0.907	0.920	0.921	0.912	0.914	0.907
AIC	-547,688	-147,818	-120,160	-64,425	-39,321	-548,078	-147,839	-120,191	-64,467	-39,329
Observations	995k	277k	277k	164k	123k	995k	277k	277k	164k	123k

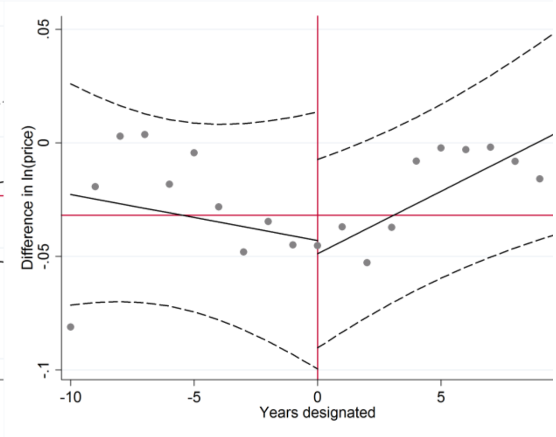
Notes: Standard errors in parentheses are clustered on the location fixed effects. Conservation area control groups in columns (4)-(7) have separate fixed effects for the areas inside and outside a conservation area. Observations dropped if years designated falls outside of range -10 years:+10 years. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2: RDD-DD internal estimates

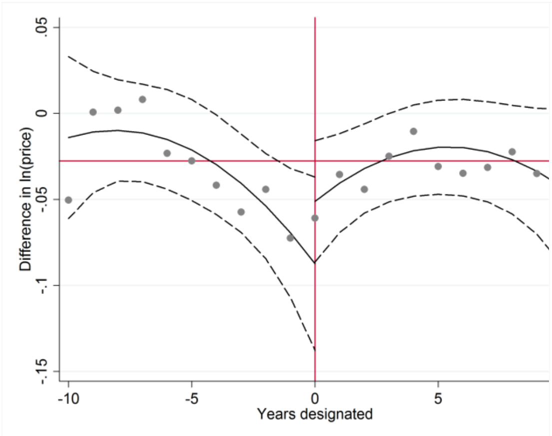
Nearest treated CA (linear trends)
Tab.3, column (4)



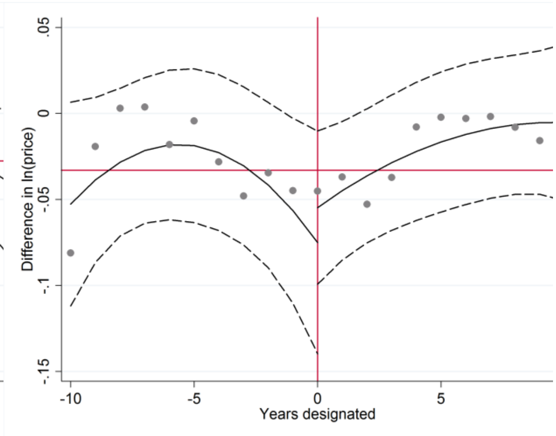
Matched CA (linear trends)
Tab. 3, column (5)



Nearest treated CA (quadratic trends)
Tab. 3, column (9)



Matched CA (quadratic trends)
Tab. 3, column (10)

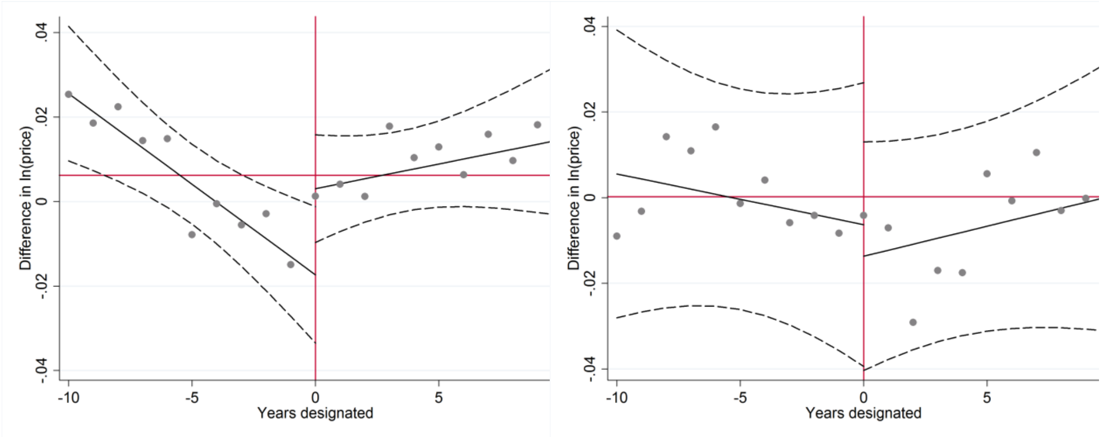


Note: The solid lines are graphical illustrations of the parametric estimates presented in appendix Table 3 and estimated using equation (27). The dashed lines indicate the 95% CI which are calculated using standard errors of multiplicative interaction terms presented by Aiken and West (1991). The grey dots plot the point estimates of 'years-since-designation bins' effects obtained from separate regression described and presented in more detail in the appendix. The horizontal red line illustrates the mean of the pre-treatment estimates.

Figure 3: RDD-DD external estimates

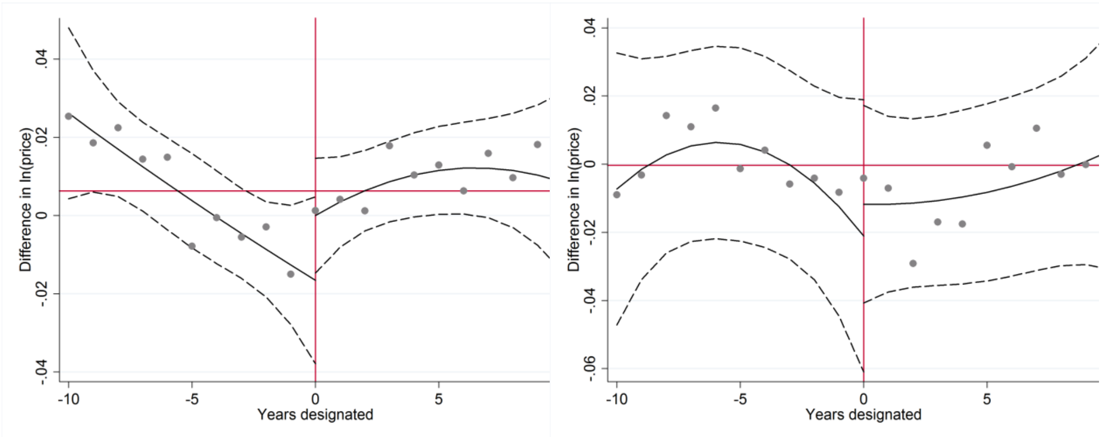
Pre-1996 CA within 2km (linear trends)
Tab. 3, column (4)

Matched pre-1996 CA (linear trends)
Tab. 3, column (5)



Pre-1996 CA within 2km (quadratic trends)
Tab. 3, column (9)

Matched pre-1996 CA (quadratic trends)
Tab. 3, column (10)



Notes: The solid lines are graphical illustrations of the parametric estimates presented in Table 3 and estimated using equation (27). The dashed lines indicate the 95% CI which are calculated using standard errors of multiplicative interaction terms presented Aiken and West (1991). The grey dots plot the point estimates of 'years-since-designation bins' effects obtained from separate regression described and presented in more detail in the appendix. The horizontal red line illustrates the mean of the pre-treatment estimates.

Spatial RDD-DD

Table 4 shows the results of the estimation of the (spatial) RDD-DD model outlined in equation (28). As with the temporal RDD-DD, we present the results of a variety of models that feature linear (1–5) and quadratic (6–10) trends and several of the control groups utilized in Table 2. One interesting and consistent feature of Table 4 is that the positive discontinuity coefficient (Outside × Post) matches the expected (positive) sign under the existence of a policy cost inside. However, the parameter is statistically insignificant in all models.

We have argued that the model predictions for capitalization effects under equilibrium designation policy and a spatial decay in heritage externalities hold at the conservation area boundary, i.e., we expect a zero effect just inside and a positive effect just outside the boundary. Figure 4 illustrates the joint effect of the parametric estimates reported in Table 4 at varying (internal and external) distances from the CA boundary. With the control group of historical CAs within 2km of the treatment CA (left panels) we find a positive capitalization effect just inside and outside the boundary, which is in line with the baseline DD result in Table 2, column (4). Moreover, the treatment effect increases toward the centre for the CA and decreases in external distance to the boundary until it becomes zero at around 700m. This distance is in line with existing evidence on a relatively steep decay in heritage and housing externalities (Ahlfeldt & Maennig, 2010; Lazrak et al., 2013; Rossi-Hansberg et al., 2010). However, the effect is statistically indistinguishable from zero at almost all distances. The single exception is a significant (at 5% level) 1.6% effect just outside the CA in the quadratic model. While the effect is only significant within 100m of the CA, this is precisely where we expect a positive effect in a world with spatial decay in heritage (housing) externalities. In the context of the model the lower and not statistically significant effect just inside the CA indicates the presence of a cost that compensates for some of the benefits associated with designation.

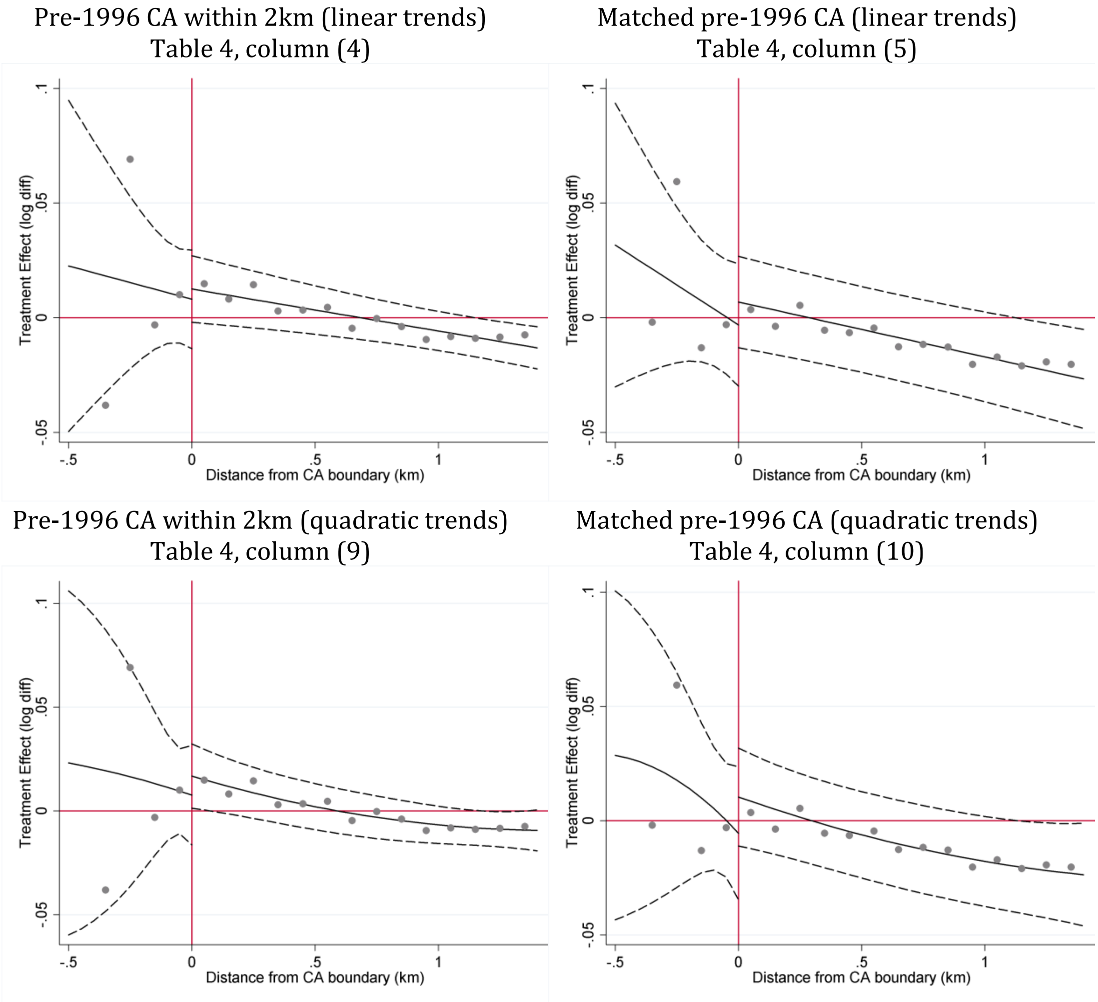
With the control group of matched CAs (right panels) the treatment effect just inside the CA boundary is remarkably close to zero. The joint effect just outside the boundary is positive, although not statistically significant. Briefly summarized, the spatial RDD-DD model suggests that across the treated CAs owners – at least on average – are not harmed by designation. There is some evidence that owners just outside a conservation area receive some benefit.

Table 4: Spatial regression discontinuity design of difference-in-differences (RDD-DD)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	log property transaction price									
Within 1400m of treated CA × Post designation	0.027*** (0.010)	0.014 (0.010)	0.012 (0.011)	0.008 (0.011)	-0.003 (0.014)	0.026** (0.011)	0.014 (0.012)	0.012 (0.012)	0.008 (0.012)	-0.005 (0.015)
Within 1400m of treated CA × Distance to boundary x Post des.	-0.057 (0.081)	-0.032 (0.075)	-0.030 (0.080)	-0.029 (0.077)	-0.070 (0.068)	-0.096 (0.156)	-0.046 (0.154)	-0.040 (0.162)	-0.040 (0.157)	-0.118 (0.143)
Within 1400m of treated CA × Distance to boundary^2 × Post des.						-0.059 (0.132)	-0.017 (0.131)	-0.018 (0.140)	-0.017 (0.136)	-0.099 (0.130)
Outside treated CA × Post designation	0.004 (0.010)	0.005 (0.010)	0.005 (0.010)	0.004 (0.009)	0.010 (0.011)	0.009 (0.012)	0.009 (0.012)	0.008 (0.011)	0.009 (0.011)	0.016 (0.012)
Outside treated CA × Distance to boundary × Post des.	0.039 (0.081)	0.016 (0.075)	0.013 (0.080)	0.011 (0.078)	0.046 (0.069)	0.064 (0.157)	0.014 (0.155)	0.013 (0.163)	0.004 (0.159)	0.080 (0.145)
Outside treated CA × Distance to boundary^2 × Post des.						0.070 (0.133)	0.028 (0.132)	0.025 (0.140)	0.029 (0.136)	0.109 (0.130)
Hedonic controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Neighbourhood controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Ward effects	YES	YES	NO	NO	NO	YES	YES	NO	NO	NO
Nearest treated CA effects	NO	NO	YES	YES	NO	NO	NO	YES	YES	NO
Matched CA effects	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES
Control group	Full England sample	Within 2km of treated CA	Within 2km of treated CA	Within 1.4km of CA designated before 1996 & within 2km of treated CA	Within 1.4km of pre-1996 CA matched on propensity score	Full England sample	Within 2km of treated CA	Within 2km of treated CA	Within 1.4km of CA designated before 1996 & within 2km of treated CA	Within 1.4km of pre-1996 CA matched on propensity score
R^2	0.921	0.922	0.915	0.914	0.905	0.921	0.922	0.915	0.914	0.921
AIC	-587,538	-156,448	-130,478	-118,076	-101,076	-587,533	-156,444	-130,478	-118,074	-587,538
Observation	1088k	302k	302k	281k	327k	1088k	302k	302k	281k	327k

Notes: Standard errors in parentheses are clustered on the location fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 4: RDD-DD spatial treatment effects



Notes: The solid lines are graphical illustrations of the parametric estimates presented in Table 4 and estimated using equation (28). The dashed lines indicate the 95% CI which are calculated using standard errors of multiplicative interaction terms presented by Aiken and West (1991).

6. Conclusion

Historic preservation policies are among the most restrictive planning policies used to overcome coordination problems in the housing market internationally. These policies aim at increasing social welfare at the cost of constraining individual property rights. From the perspective of owners of properties in conservation areas, the policy may help to solve a collective action problem, preventing owners from freeriding on the heritage character of nearby buildings while inappropriately altering their own property. If property owners value the heritage character of nearby buildings and can influence the designation process they will seek out a (local) level of designation where the marginal costs of designation equate the marginal benefits. An increase in the marginal benefit of designation will lead to an increase in designation activity. If the planner acts on behalf of the local owners, additional designations in a neighbourhood will not lead to an adverse impact on those being designated.

We provide evidence that is supportive of this scenario using two empirical approaches that follow from a simple model of equilibrium conservation area designation. First, we present a neighbourhood level IV tobit analysis that reveals a positive impact of an increase in degree share, which is presumably (positively) correlated with heritage preferences, on the share of designated land. Gentrification, by increasing the value of neighbourhood stability to local owners, can cause designation. Second, we combine the strengths of difference-in-differences (DD) and regression discontinuity designs (RDD) to estimate the capitalization effect of designation on newly designated areas as well as spillovers to adjacent areas. This RDD-DD methodology qualifies more generally as a useful tool for program evaluations where a treatment is suspected to lead to an impact on (spatial or temporal) trends and discontinuities. Within newly designated conservation areas we find no significant short-run effects of designation and some evidence for positive capitalization effects in the long run. There is some evidence for positive spillovers onto properties just outside.

These results suggest that the policy is either deliberately adhering to the interests of local owners or, as suggested in the literature on the political economy of housing markets, homeowners are able to successfully influence the outcome of local policies in their interest. It is therefore unlikely that the policy is welfare enhancing on a wider geographic scale. Depending on the general restrictiveness of the planning system, historic preservation may constrain housing supply and generate welfare losses. The net-welfare effect to a wider housing market area is an interesting and important question that we leave to future research.

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APPENDIX TO CHAPTER IV

1. Introduction

This appendix complements the main paper and is not designed to stand alone or as a replacement. Section 2 provides an illustration of how a planner determines the designation share and adds to the theory section of the main paper. Section 3 complements the empirical strategy section of the main paper by providing a more detailed discussion of the control variables in tobit designation process models. The section also links the reduced form difference-in-differences parameters to the marginal policy effect in the theoretical model. Section 4 provides a detailed overview of the data we use, its sources, and how they are processed. Finally, section 5 complements the empirical results section of the main paper by showing the results of a variety of robustness tests and model alterations not reported in the main paper for brevity.

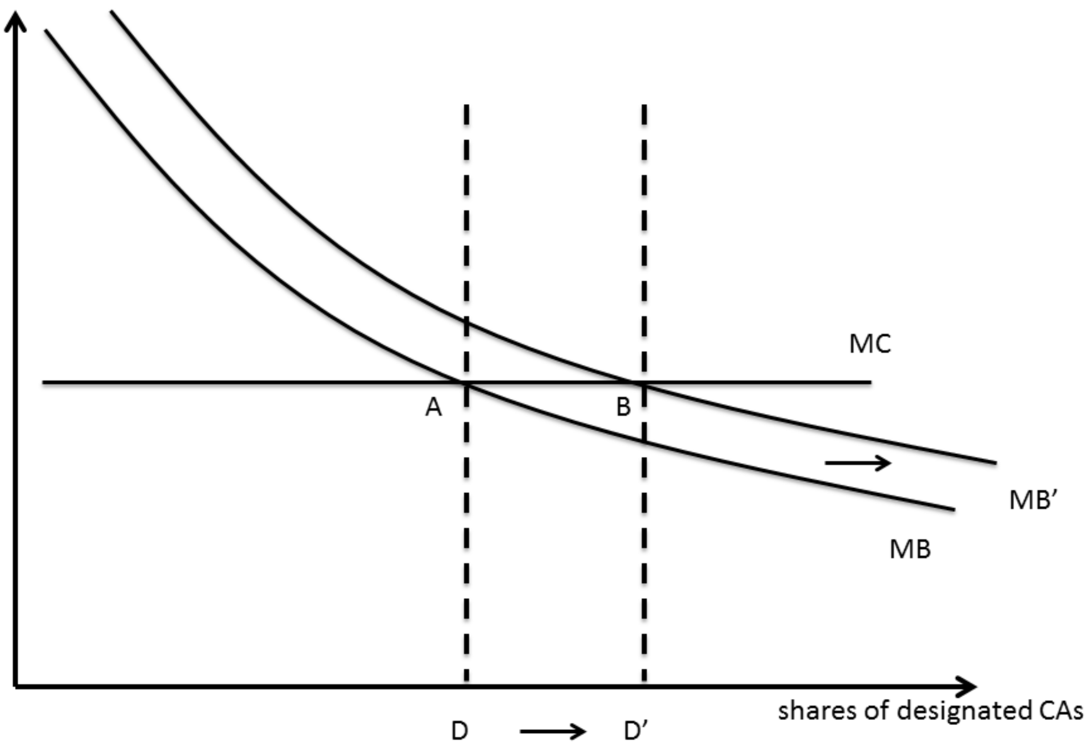
2. Theory and context

2.1 Theoretical Framework

This section briefly illustrates how a planner determines the designation share. The political equilibrium between the social marginal benefits (MB) of designation (equation 9 in the main paper) and the marginal costs (MC) (equation 10) is depicted by Figure 1. At point A the designation share D adheres to local homeowner interests. The representative homeowner in each zone along x is happy with the designation status of their zone. However this is not a welfare maximizing equilibrium since a further extension would benefit all owners in zones to the left of A and to the right of B as they would profit from increasing the expected heritage in the neighborhood without experiencing a change in marginal cost. In zones between A and B, however, the social marginal benefit would also increase, but the increase would not compensate for the private marginal costs associated with a change in the designation status from undesignated to designated.

If there is, for instance, a change in preferences and residents develop a greater taste for external heritage γ their marginal benefits curve shifts to the right. The planner adapts to this situation and raises the designation share to set marginal benefits equal to marginal costs again. This new equilibrium is illustrated by point B where the designation share increases to D'.

Figure 1: Designation equilibrium



3. Empirical strategy

3.1 Designation process – control variables

This section provides a detailed description and motivation of the control variables we use to account for the determinants of conservation area designation that are unrelated to the mechanisms modeled in our theory. In particular we try to control for composition effects, neighborhood sorting, heterogeneity in terms of homeownership, and whether the heritage in a neighborhood is at particular risk.

We add the initial period (1991) degree share for two reasons. First, we assume that the highly educated derive higher (net-)benefits from neighborhood heritage. To the extent that this group is capable of more efficiently articulating their will in a political bargaining a higher degree share will make the designation more likely. It is important to control for the initial degree share since levels and changes may be correlated in either direction. On the one hand there may be catch-up growth in the degree share of less educated regions, i.e., mean reversion. On the other hand, people with degrees may be more likely to move to areas with an already high share of people with degrees, which would imply a self-reinforcing process leading to spatial segregation.

We also include a control for the extent of designation in the initial period (1991). The share of designated land area in the total ward area would be (positively) correlated with the change in the designation share if designations spark further designations as in a contagion model. Initial designation also helps to control for the possibility that the skilled may be attracted to areas with a lot of designated land. To avoid a mechanic relationship between the dependent variable and the lagged designation share we instrument designation in the initial period (1991) by its lagged value, i.e. the designation share in 1981.

Another set of controls is driven by the interest in homeowners within the designation process. Homeowners experience extra benefits/costs from designation since, unlike renters, they are not compensated for changes in neighborhood quality by increases in degrees or rents. Homeowners, thus have additional incentives to engage in political bargaining. Similar to the other controls, homeownership status enters in lagged levels and differences. In a final specification we also add an interaction of the logged change in degree with homeownership (rescaled to a zero mean to make coefficients comparable). We use average household size (both in differences and lagged levels) to control for the presumption that larger households are more likely to lobby against designation and the resulting constraint on available floor space. We control for further neighborhood characteristics by including average population age and the share of foreigners inside a ward (also both in differences and lagged levels). We expect older residents to appreciate heritage stronger making it more likely that they lobby for designation. Conversely, a high share of foreigners is expected to be negatively correlated with designation. Foreigners, on average, might not know the planning system that well and perhaps find it more difficult to form interest groups. Moreover, they might value English heritage differently due to their cultural background.

A larger risk aversion increases the benefit from a policy that increases certainty regarding the future of the neighborhood and, thus, potentially increases the optimal designation share. To control for a potentially positive correlation between owners' risk aversion and the value of their properties – typically their largest assets – we add a measure of neighborhood appreciation. We generate ward-level property price trends in n separate auxiliary regressions of the following type:

$$\log(P_{itn}) = \alpha_n + X_{ni}b_n + \beta_n T_t + \varepsilon_{itn} \tag{1}$$

where X is a vector of property and neighborhood characteristics and T is a linear time trend. To avoid a reverse effect of designation on the property price trend we only consider transactions that occur outside conservation areas.

A second set of controls deals with potential development risk. Areas that experience development pressure or are in poor and/or declining condition may be more likely to be designated in order to protect against the threats to the heritage character of the neighborhood. We use the vacancy rate, a density measure of listed buildings, housing turnover, the share of pre-1945 buildings as well as score measures for a conservation area's condition, vulnerability and trajectory provided by English Heritage to capture development pressure. We expect that neighborhoods with few vacancies will be put under higher development pressure. Vacancies enter the specification both in differences and lagged levels. The reason for the differenced term is that a change in development pressure is likely to lead to a change in designation status as a result. We argue that the lagged level may also capture changes (not just levels) in development pressure. This is because of external factors and conditions (i.e., population growth) that effect areas unevenly depending on their level in certain attributes (e.g., vacant housing). It seems likely that general population growth would put greater development pressure on neighborhoods with lower vacancy rates. By using the total number of houses sold between 1995 and 2010 we introduce an alternative measure of development pressure. The share of houses built before 1945 serves as an indicator of potential heritage. If we are not in a steady state, building age could affect the change in designation share. The score measures reflect the development risk inside a conservation area and come from a survey provided by English Heritage. The higher the condition score, the worse the heritage conditions. A higher vulnerability as well as a higher trajectory are also indicated by higher scores. Except for the score variables, all control variables enter our empirical specification in logs.

While taking first-differences of the empirical specification will remove all time-invariant ward-specific effects that might impact on the level of designation (including the heritage itself), it will not help if there are location-specific effects that impact on the *changes* in designation status. For example, if there is heterogeneity across Local Authorities (LAs) about how difficult or easy it is to designate arising from different bureaucratic practices then this would affect changes in designation for all wards within a particular LA. We therefore estimate a fixed effects specification for the 166 English Travel To Work Areas (TTWAs). The TTWAs are designed to approximate city regions which can be described as somehow self-contained economic areas from a job market perspective. By applying a TTWA fixed effect model we are therefore able to control for socio-economic heterogeneity across TTWAs.

3.2 Difference-in-differences

This section motivates the difference-in-differences approach for the estimation of the marginal policy effect. Firstly, we illustrate how the policy and heritage effects are difficult to disentangle in a simple cross-sectional hedonic estimation. Secondly, we lay out how the difference-in-differences treatment effect is used to estimate the marginal policy effect laid out in terms of the structural parameters of our model.

Cross-sectional hedonics

Taking logs of the spatial equilibrium price equation (17) from the main paper gives:[46]

$$\ln\theta(x) = \tau + \frac{1}{1-\delta}\ln a(x) + \frac{\varphi h(x)}{1-\delta} + \frac{\gamma E[H|D]}{1-\delta} - \frac{c\widetilde{D}(x)}{1-\delta} \quad (2)$$

The following heritage and policy effects determine the bid rent:

$$Policy\ cost = \frac{c\widetilde{D}(x)}{1-\delta} \quad (3)$$

$$External\ heritage\ effect\ (conditional\ on\ designation) = \frac{\gamma E[H|D]}{1-\delta} \quad (4)$$

$$Internal\ heritage\ effect = \frac{\varphi h(x)}{1-\delta} \quad (5)$$

Consider the cross-sectional reduced form equation:

$$p_{it} = \aleph I_i + \mathrm{X}_i'\mu + f_n + Y_t + \epsilon_{it} \quad (6)$$

where p_{it} is the natural logarithm of the transaction price for property i in time period t, I_i is a dummy variable equal to one if the observation is internal to a treated conservation area, X_i is a vector of controls for property, neighborhood, and environmental characteristics, f_n is a set of n location fixed effects and Y_t are year effects. The coefficient $\aleph$ on the CA_i dummy identifies the policy cost associated with the location of a property inside a conservation area $\widetilde{D}(x) = 1$. The policy cost should have a negative effect on logged house prices. The coefficient also partly identifies the internal heritage effect. Specifically, it identifies the value of the difference between the mean internal heritage inside conservation areas and the mean internal heritage outside conservation areas (i.e. $\varphi/(1-\delta)(\overline{h_{CA_\iota=1}} - \overline{h_{CA_\iota=0}})$). This should be positive because the

[46] Where τ is a constant and equal to: $\ln(1-\delta) + \frac{\delta}{1-\delta}\ln\delta + \frac{1}{1-\delta}\ln W$.

policymaker would normally designate areas that have the most heritage. Finally, under the existence of some spatial decay in externalities, it will also identify the value of the difference inside and outside conservation areas in the external heritage effect (i.e., $\gamma(1-\delta)\left(\overline{E[H|D]_{CA_\iota=1}} - \overline{E[H|D]_{CA_\iota=0}}\right)$). This is a function of internal heritage and will therefore also be positive.

The coefficient ℵ thus reflects a composite effect of policy costs, policy benefits, and correlated internal heritage effect. Furthermore, in reality the actual distribution of internal heritage is unknown and there is likely a spatial decay to externalities, further complicating the estimate.[47] In practice, ℵ will also be affected by unobserved neighborhood characteristics that are correlated with the distance to the conservation area. A positive ℵ parameter, at best, tells us only that the overall higher levels of heritage (internal and external) combined with the policy benefits of conservation outweigh the policy costs. This does not provide a comprehensive evaluation of the policy effect itself. To try and disentangle these effects we implement a different empirical approach.

Difference-in-differences

Using the difference-in-differences (DD) approach to estimate the marginal effect of a change in designation status offers an improved identification.

Our empirical difference-in-differences specification is equation (26) from the main paper:

$$p_{it} = \beta^I I_i + \beta^E E_i + \beta^{IPost}(I_i \times Post_{it}) + \beta^{EPost}(E_i \times Post_{it}) + \mathrm{X}_i'\mu + f_n + Y_t + \epsilon_{it} \qquad (7)$$

Table 1 illustrates the conditional mean prices (after controlling for time effects) for the treatment and control group in the pre- and post-treatment periods. It is

[47] In a general case the estimate would be equal to:

$$\aleph = \frac{\varphi}{1-\delta}\left(\overline{h_{CA_\iota=1}} - \overline{h_{CA_\iota=0}}\right) + \frac{\gamma}{1-\delta}\left(\overline{E[H|D]_{CA_\iota=1}} - \overline{E[H|D]_{CA_\iota=0}}\right) + \frac{c}{1-\delta}$$

important to note that the year fixed effects Y_t capture the general development of price over time. Without this feature it would be necessary to control for the overall growth in price between the pre- and post-treatment periods via the inclusion of a non-interacted version of $Post_{it}$.

Table 1: Treatment effect

Conditional mean of prices	Pre	Post
Treated (Internal)	$\bar{p}_{Pre}^{Treat} = \beta^I$	$\bar{p}_{Post}^{Treat} = \beta^I + \beta^{IPost}$
Control	$\bar{p}_{Pre}^{Con} = 0$	$\bar{p}_{Post}^{Con} = 0$

$Treatment\ Effect = (\bar{p}_{Post}^{Treat} - \bar{p}_{Pre}^{Treat}) - (\bar{p}_{Pre}^{Con} - \bar{p}_{Pre}^{Con})$

$Treatment\ Effect = ([\beta^I + \beta^{IPost}] - [\beta^I]) - ([0] - [0])$

$Treatment\ Effect = \beta^{Post}$

Notes: The conditional mean of prices in the treatment group in the pre-period is denoted $\bar{p}_{Pre}^{Treat}$. This represents the log of prices conditional on fixed and year effects $(f_n + Y_t)$ and controls X_i. The same notation is used for the other groups.

Our treatment coefficient β^{IPost} essentially differentiates across the treatment and control groups before and after designation and is, thus defined as follows:

$$\beta^{IPost} = (\bar{p}_{Post}^{Treat} - \bar{p}_{Pre}^{Treat}) - (\bar{p}_{Post}^{Con} - \bar{p}_{Pre}^{Con}) \tag{8}$$

Let's assume that the relationship between the observed conditional mean and the theoretical bid rent is given by:

$$\bar{p}_{Post}^{Treat} = \theta_{Post}^{Treat} + u_{Post}^{Treat} \tag{9}$$

where u_{Pre}^{Treat} are partially unobservable factors specific to properties in the Treated-Post cell. The same relationship applies for the other cells (Treated-Pre, Control-Post and Control-Pre). At the heart of our identification strategy we assume that the price trends unrelated to the policy are the same within the treatment and the control group. The typical identifying assumption on which the difference-in-differences identification strategy relies can be expressed as follows:

$$(u_{Post}^{Treat} - u_{Pre}^{Treat}) = (u_{Post}^{Con} - u_{Pre}^{Con}) \tag{10}$$

The credibility of the counterfactual rests on the likelihood that the treatment group, in the absence of the intervention, would have followed a trend that is similar to that of the control group. An appropriate definition of the control group is therefore a critical element of the identification strategy. We therefore consider a number of different control groups in which we try to reduce the potential heterogeneity between properties in the treatment and control group.

The first treatment group is a spatial match where we choose the observations that fall within a 2km buffer surrounding conservation areas that changed designation status during the observation period (1995–2010). As an alternative, we consider a number of matching procedures that rest on the idea that properties inside conservation areas generally share similarities. Properties in conservation areas that did not change designation status therefore potentially qualify as a control group. To make the areas in the treatment and control group more similar, we select conservation areas based on similarities with those in our treatment group (Rosenbaum & Ruben, 1983). For the matching procedure we only make use of variables that turn out to have significant impact in the auxiliary propensity score matching regression.[48] We use a nearest neighbor matching procedure, which produces a broader and a narrower group.

Under the assumptions made it is straightforward to demonstrate that the DD treatment coefficient gives the pure policy effect we are interested in. Combining the theoretical bid rent of equation (17) from the main paper with the definition of $\bar{p}_{Post}^{Treat}$ in appendix equation (9) gives the conditional mean price of (treated) properties inside newly designated conservation areas before (pre) and after (post) designation can be expressed as follows[49]:

$$\bar{p}_{Pre}^{Treat} = \tau + \frac{1}{1-\delta}\ln a_i + \frac{\varphi h_i}{1-\delta} + \frac{\gamma E[H|D]}{1-\delta} + u_{Pre}^{Treat} \qquad (11)$$

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[48] A list of significant controls in propensity score matching regressions is included in the next subsection.

[49] Where the theoretical locations x have been replaced by observed housing transactions i.

$$\bar{p}_{Post}^{Treat} = \tau + \frac{1}{1-\delta}\ln a_i + \frac{\varphi h_i}{1-\delta} + \frac{\gamma}{1-\delta}\left(E[H|D] + \frac{dE[H|D]}{dD}\right) - \frac{c\widetilde{D}_i}{1-\delta} + u_{Post}^{Treat} \quad (12)$$

where a new designation is represented as an increase in designation share D. For a control group sufficiently far away to not be exposed to the heritage externality we similarly get:

$$\bar{p}_{Pre}^{Con} = \tau + \frac{1}{1-\delta}\ln a_i + \frac{\gamma E[H|D]}{1-\delta} + u_{Pre}^{Con} \quad (13)$$

$$\bar{p}_{Post}^{Con} = \tau + \frac{1}{1-\delta}\ln a_i + \frac{\gamma E[H|D]}{1-\delta} + u_{Post}^{Con} \quad (14)$$

where there is (by definition) no new designation. Given the common trend assumption of equation (10), β^{IPost} identifies the pure net policy effect of designation:

$$\beta^{IPost} = \frac{\gamma}{1-\delta}\frac{dE[H|D]}{dD} - \frac{c\widetilde{D}(x)}{1-\delta} \quad (15)$$

In the empirical implementation of the DD strategy we also consider alternative treatment groups that consist of properties just outside conservation areas, which are potentially exposed to spillovers, but not to the cost of designation. The interpretation of the external treatment coefficient can be derived analogically where designation leads to benefits but without the associated costs:

$$\bar{p}_{Pre}^{Treat} = \tau + \frac{1}{1-\delta}\ln a_i + \frac{\gamma E[H|D]}{1-\delta} + u_{Pre}^{Treat} \quad (16)$$

$$\bar{p}_{Post}^{Treat} = \tau + \frac{1}{1-\delta}\ln a_i + \frac{\gamma}{1-\delta}\left(E[H|D] + \frac{dE[H|D]}{dD}\right) + u_{Post}^{Treat} \quad (17)$$

Under the common trends assumption the treatment coefficient reflects the pure policy benefit associated with the reduction in uncertainty as predicted by the stylized theory:

$$\beta^{EPost} = \frac{\gamma}{1-\delta}\frac{dE[H|D]}{dD} \tag{18}$$

Propensity score matching regression

In order to determine the control group for the difference-in-differences specification a propensity score matching approach was employed. We used a stepwise elimination approach in order to determine which variables have a significant impact on propensity score. With a significance level criterion of 10% the following variables remained in the final CA propensity score estimation:

CA characteristics: Urban, Commercial, Residential, Industrial, World Heritage Site, At Risk and Article 4 Status.

Environmental characteristics: Land Cover Type 9 (Inland bare ground), Land Cover Type 3 (Mountains, moors and heathland), distance to nearest National Nature Reserve, distance to nearest National Park, National Park (kernel density) and Area of Outstanding Natural Beauty (kernel density).

Neighbourhood characteristics: Median Income and Ethnicity Herfindahl index

Amenities: Distance to nearest Bar, distance to nearest Underground Station, distance to nearest Hospital, distance to nearest Motorway and distance to nearest TTWA centroid.

Semi-parametric temporal and spatial estimations of treatment effects

We estimate a semi-parametric version of (27) that replaces the YD_{it} variables with a full set of years-since-designation bins. We group transactions into bins depending on the number of years that have passed since the conservation area they fall into or are near to had been designated. Negative values indicate years prior to designation. These bins (b) are captured by a set of dummy variables PT_b:

$$p_{it} = \sum_b \beta_b^I\left(PT_i^b \times I_i\right) + \sum_b \beta_b^E\left(PT_i^b \times E_i\right) + \sum_b \beta_b PT_i^b + \mathrm{X}_i'\mu + f_n + Y_t + \epsilon_{it} \tag{19}$$

The parameters β_b^I and β_b^E give the difference in prices between treatment and control groups in each years-since-designation bin b. The results of this semi-parametric estimation are plotted in Figure 2 in Appendix 5.2. In order to allow for a casual inspection of the fit of the parametric models the semi-parametric point-estimates are also plotted in Figure 2 (internal) and Figure 3 (external) of the main paper.

As with the temporal models, we relax the parametric constraints of the spatial estimations by replacing the distance variable in equation (28) with distance bins:

$$p_{it} = \sum_d \beta_d \left(DB_i^d \times T_i\right) + \sum_d \beta_d^{Post}\left(DB_i^d \times T_i \times Post_{it}\right) + X_i'\mu + f_n + Y_t + \epsilon_{it} \tag{20}$$

where DB_i^d are positive (external) and negative (internal) distance bins from the designation area boundary and β_d^{Post} are d treatment effect parameters at different distances inside and outside the conservation area. If the planner designates according to local homeowner interests then the bin that corresponds to the locations just inside the treated conservation area should indicate a zero treatment effect. This may or may not be associated with a positive effect for the bins deepest inside the conservation area. Furthermore, if there are significant externalities associated with the designation (and heritage in general) then the bins just outside the boundary should indicate a positive effect. A lower effect for further out bins would indicate a spatial decay to this externality. The results from this specification are presented Figure 3 0in Appendix 5.2 and in Figure 4 of the main paper.

4. Data

4.1 Data sources

Housing transactions

The transactions data relates to mortgages for properties granted by the Nationwide Building Society (NBS) between 1995 and 2010. The data for England comprise 1,088,446 observations and include the price paid for individual housing units along with detailed property characteristics. These characteristics include floor space (m^2), the type of property (detached, semi-detached, flat, bungalow or terraced), the date of construction, the number of bedrooms and bathrooms, garage or parking facilities and the type of heating. There is also some buyer information including the type of mortgage (freehold or leasehold) and whether they are a first-time buyer.

Importantly, the transaction data includes the full UK postcode of the property sold allowing it to be assigned to grid-reference coordinates. With this information it is possible within a Geographical Information Systems (GIS) environment to calculate distances to conservation area borders and to determine whether the property lies inside or outside these borders. Furthermore it is possible to calculate distances and other spatial measures (e.g., densities) for the amenities and environmental characteristics that will be used as control variables. Since the data set refers to postcodes rather than individual properties, it is not possible, however, to analyze repeated sales of the same property. This is a limitation shared with most property transaction data sets available in England, including the land registry data.

Neighborhood characteristics

The main variables used for estimating capitalization effects of neighborhood characteristics are median income and ethnic composition. The income data is a model-based estimate of median household income produced by Experian for Super Output Areas of the lower level (LSOA). This is assigned to the transaction data based on postcode. The data on ethnicity was made available by the 2001 UK Census at the level of Output Area (OA). Shares of each of the 16 ethnic groups and a Herfindahl index[50] were computed to capture the ethnic composition of neighborhoods.

Environmental variables

The environmental variables capture the amenity value of environmental designations, features of the natural environment, different types of land cover and different types of land use.

Geographical data (in the form of ESRI shapefiles) for UK National Parks, Areas of Outstanding Natural Beauty, and National Nature Reserves are available from Natural England. National Parks and Areas of Outstanding Natural Beauty are protected areas of countryside designated because of their significant landscape value. National Nature Reserves are "established to protect sensitive features and to provide 'outdoor laboratories' for research" (National England website). Straight line distances to these designations were computed for the housing units as geographically located by their postcodes. Furthermore, density measures that take into account both the distance to and the size of the features were created. We apply a kernel density measure (Silverman, 1986b) with a radius of 2km which is considered to be the maximum distance people are willing to walk (Gibbons & Machin, 2005).

The location of lakes, rivers and coastline are available from the GB Ordinance Survey. The distance to these features is also computed for the housing units from the transaction data. The UK Land Cover Map produced by the Centre for Ecology and Hydrology describes land coverage by 26 categories as identified by satellite images. We follow Mourato et al. (2010) who construct nine broad land cover types from the 26 categories. Shares of each of these nine categories in 1km grid squares are calculated and the housing units take on the value of the grid square in which they reside.

[50] The Herfindahl index (HI) is calculated according to the following relation: $HI = \sum_{i=1}^{N} s_i^2$, where s_i is the share of ethnicity i in the LSOA, and N *is* the total number of ethnicities.

The generalized Land Use Database (GLUD) available from the Department for Communities and Local Government gives area shares of nine different types of land use within Super Output Areas, lower level (LSOA). These nine land use types are domestic buildings, non-domestic buildings, roads, paths, rail, domestic gardens, green space, water, and other land use. These shares are assigned to the housing units based on the LSOA in which they are located.

Amenities

The locational amenities variables capture the benefits a location offers in terms of accessibility, employment opportunities, schools quality, and the proximity of cultural and entertainment establishments.

Employment accessibility is captured both by the distance to Travel to Work Area (TTWA) centroid and a measure of employment potentiality. TTWAs are defined such that 75 per cent of employees who work in the area also live within that area. Thus they represent independent employment zones and the distance to the center of these zones is a proxy for accessibility to employment locations. A more complex measure of accessibility is the employment potentiality index (Ahlfeldt, 2011b).[51] This is computed at the Super Output Area, lower level (LSOA) and represents an average of employment in neighboring LSOAs weighted by their distance.

Key Stage 2 (ages 7–11) assessment scores are available from the Department for Education at the Super Output Area, middle layer (MSOA). School quality is thus captured at the housing unit level by computing a distance-weighted average of the KS2 scores of nearby MSOA centroids.[52]

[51] Further detail on the construction of the employment potentiality measure is provided in section 4.2.

[52] This is calculated as an Inverse Distance Weighting (IDW) with a threshold distance of 5km and a power of 2.

Geographical data on the locations of motorways, roads, airports, rail stations and rail tracks are available from the GB Ordinance Survey. Distances were computed from housing units to motorways, A-roads, B-roads and rail stations to capture accessibility. Buffer zones were created around the motorways and roads along with distance calculations to rail tracks and airports in order to capture the disamenity noise effects of transport infrastructure.

Further data on local amenities were taken from the Ordinance Survey (police stations, places of worship, hospitals, leisure/sports centers) and OpenStreetMap (cafés, restaurants/fast food outlets, museums, nightclubs, bars/pubs, theaters/cinemas, kindergartens and monuments, memorials, monuments, castles, attractions, artwork). The number of listed buildings was provided by English Heritage. Kernel densities for these amenities were computed for housing units using a kernel radius of 2km and a quadratic kernel function (Silverman, 1986b). The radius of 2km is consistent with amenities having a significant effect on property prices only when they are within walking distance.

Table 2: Variable description

Dependent Variable	
Price	Per square meter transaction price in British pounds of the corresponding floor space (expressed as natural logarithm). Transaction data from the Nationwide Building Society (NBS).
Independent Variables	
CA Effects	Dummy variables denoting property transactions taking place within the boundaries of an currently existing conservation area, in a conservation area at the time when designated or where the designation date is unknown as well as various buffer areas surrounding current or treated conservation areas.
Fixed Effect Control	Travel to Work Areas, nearest conservation area catchment areas and interactives with year effects.
Housing information	Set of property variables from the NBS including: Number of bedrooms, number of bathrooms, floor size (in square meter), new property (dummy), building age (years), tenure (leasehold/freehold), central heating (full: gas, electric, oil, solid fuel), central heating (partial: gas, electric, oil, solid fuel), garage (single or double), parking space, property type (detached, semi-detached, terraced, bungalow, flat-maisonette).
Neighborhood information	Set of neighborhood variables including: media income (2005, LSOA level), share of white population at total population (2001 census, output area level), share of mixed population at total population (2001 census, output area level), share of black population at total population (2001 census, output area level), share of Asian population at total population (2001 census, output area level), share of Chinese population at total population (2001 census, output area level), Herfindahl of ethnic segregation (including population shares of White British, White Irish, White others, Mixed Caribbean, Mixed Asian, Mixed Black, Mixed other, Asian Indian, Asian Pakistani, Asian others, Black Caribbean, Black African, Black other, Chinese, Chinese other population, 2001 census output area).
Conservation area Characteristics	Set of characteristic variables for conservation areas from English Heritage including: Conservation area land use (dummy variables for residential, commercial, industrial or mixed land use), conservation area type (dummy variable for urban, suburban or rural type), conservation area size (dummy for areas larger than mean of 128,432.04 square meters), conservation area (square meter), conservation area has an Article 4 Direction implemented (dummy), oldness of conservation area (dummy for areas older than mean of 1981), conservation area at risk (dummy), conservation area with community support (dummy), conservation area is World Heritage Site (dummy).
Environment Characteristics and Amenities	Set of locational variables processed in GIS including: National Parks (distance to, density), Areas of Outstanding Beauty (distance to, density), Natural Nature Reserves (distance to, density), distance to nearest lake, distance to nearest river, distance to nearest coastline, land in 1km square: Marine and coastal margins; freshwater, wetland and flood plains; mountains, moors and heathland; semi-natural grassland; enclosed farmland; coniferous

	woodland; broad-leaved/mixed woodland; urban; inland bare ground.
Other amenities	Set of locational variables created in GIS including: Average key stage 2 test score (MSOA averages as well as interpolated in GIS), distance to electricity transmission lines, A-Roads (distance to, buffer dummy variables within 170m), B-Roads (distance to, buffer dummy variable within 85m), motorway (distance to, buffer dummy variable within 315m; buffer distances refer to the distance were noise of maximum speed drops drown to 50 decibel), distance to all railway stations, distance to London Underground stations, distance to railway tracks, distance to bus stations, distance to airports, densities of cafés, restaurants/fast food places, museums, nightclubs, bars/pubs, theaters/cinemas, kindergartens, monuments (memorial, monument, castles, attraction, artwork), hospitals, sports/leisure centers, police stations and worship locations, distance to Travel to Work Areas, employment potentiality (based on Travel to Work Areas with an time decay parameter of 0.073).
Neighborhood Distance Controls	Set of neighborhood distance dummy variables created in GIS including: Distances outside conservation area border (up to 50m, 100m, 150m, 200m, 250m, 300m, 350m, 400m, 1km, 2km and 3km), distances inside conservation area border (up to 50m, 100m, 150m, 200m).

4.2 Further notes on data methods

Employment potentiality

The employment potentiality index is computed at the Super Output Area, lower level (LSOA) and represents an average of employment in neighboring LSOAs weighted by their distances. Employment potentiality is calculated for each Lower Layer Super Output Area i (LSOA) based on employment in all other LSOAs j using the following equation:

$$\mathrm{EP_i} = \sum_{\mathrm{j}} \mathrm{E_j} \mathrm{e}^{-\mathrm{a}\,\mathrm{d_{ij}}}, with\ i \neq j \tag{21}$$

where d measures the straight line distance converted into travel time assuming an overall average speed of 25km/h (Department for Transport, 2009) and Employment the absolute number of workers in the respective LSOA. The indicator is weighted by a decay parameter of a = -0.073 estimated by Ahlfeldt (in press). Internal distances are calculated as:

$$d_{ii} = \frac{1}{3}\sqrt{\frac{\mathrm{Area}_i}{\pi}} \tag{22}$$

Kernel densities for National Parks, Areas of Outstanding Natural Beauty and National Nature Reserves

The kernel density is a measure that takes into account both the proximity and the size of NPs, AONBs and NNRs. Every 100x100m piece of designated area is assigned a point and the density of these resulting points calculated for 10km kernels and a quadratic kernel function (Silverman, 1986, p. 76, equation 4.5) around each housing unit using a kernel density method. The result is similar to calculating a share of NP area within a circle, the one difference being that the points are additionally weighted by distance to the housing units according to a normal distribution.

Buffers for motorways and roads

The buffer sizes for the different roads are as follows: B-Road (85m), A-Road (170m) and Motorway (315m). These distances are calculated based on how far it is expected that the noise from traffic travelling at the speed limit of the respective roads (Steven, 2005) would decline to an assumed disamenity threshold level of noise of 50db (Nelson, 2008).

Land cover map Broad Categories

Table 3: Land Cover Broad categories as defined by Mourato et al. (2010)

1	Marine and coastal margins
2	Freshwater, wetlands, and flood plains
3	Mountains, moors, and heathland
4	Semi-natural grasslands
5	Enclosed farmland
6	Coniferous woodland
7	Broad-leaved/mixed woodland
8	Urban
9	Inland bare ground

5. Results

5.1 Designation process

In order to test our theoretical implication that changes in heritage preferences lead to changes in designation we estimate the regression model as outlined in section 3.1. The prediction of the model is that positive changes in heritage preferences should lead to negative changes in the share of non-designated land in a neighborhood. OLS regression results are reported in Table 4. We drop all zeros and identify the effect based on the sample of observations with observable changes in conservation area shares. The standard OLS estimates without (1) and with a basic set of composition controls (2) are insignificant. Due to the potential sources of bias in OLS discussed in the main paper (section 3.1) we re-estimate the two models using our instrumental variables. The 2SLS estimates (3) and (4) are in line with the tobit results reported in the main paper and support the theory that a positive change in degree share leads to higher designation.

Table 4: Designation regressions: OLS/2SLS models

	(1) OLS Δ log designation share (t)	(2) OLS Δ log designation share (t)	(3) 2SLS Δ log designation share (t)	(4) 2SLS Δ log designation share (t)
Δ log degree share (t)	-0.016 (0.013)	0.002 (0.014)	-0.602*** (0.096)	-0.871*** (0.247)
log degree share (t-1)		-0.015 (0.013)		-0.379*** (0.105)
log designation share (t-1)		0.001 (0.001)		0.006* (0.004)
Δ log homeownership (t)		0.041 (0.032)		0.492*** (0.140)
log homeownership (t-1)		0.011 (0.023)		0.056 (0.036)
Δ log average household size (t)		0.140 (0.107)		-0.483** (0.193)
log average household size (t-1)		0.209*** (0.032)		-0.107 (0.125)
log pop age (t-1)		0.126*** (0.041)		-0.025 (0.103)
Δ pop age (t)		0.183*** (0.047)		-0.222 (0.164)
log foreigner share (t-1)		-0.019*** (0.007)		0.083*** (0.031)
Δ foreigner share (t)		0.004 (0.007)		0.068*** (0.026)
Constant	-0.040*** (0.011)	-0.782*** (0.169)	0.361*** (0.066)	0.299 (0.497)
IV	NO	NO	YES	YES
Controls	NO	YES	NO	YES
R^2	0.001	0.047	-0.733	-0.445
F	1.516	15.628	38.934	5.724
AIC	-871.268	-925.893	-1.359	-268.685
OVERID	.	.	2.936	2.103
OVERIDP	.	.	0.087	0.147
Observations	1580	1580	1580	1580

Notes: See the data section for a description of control variables. IVs are station density, employment potential and the degree share in 1981. Standard errors in parentheses and clustered on fixed effects. $^{*}p< 0.05$, $^{**}p< 0.01$, $^{***}p< 0.001$.

Table 5 reports the first stage results to the second-stage results reported in Table 1 in the main paper. IVs are (conditionally) positively correlated with the change in degree share, and initial designation share respectively.

Table 5: Standard IV models – First stage regressions

	(1) Δ log degree share (t)	(2) Δ log degree share (t)	(3) Δ log degree share (t)	(4) Δ log degree share (t)	(5) Δ log degree share(t) x homeowner (t-1)	(6) log designation share (t)
rail station density	0.098***	0.100***	0.070***	0.102***	0.021***	-0.033
	(0.026)	(0.024)	(0.019)	(0.020)	(0.006)	(0.208)
employment potentiality	2.14E-8***	2.08E-8***	2.85E-8***	2.97E-8***	1.46E-9	7.54E-8
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
predicted Δ log degree share (t) x homeownership (t-1)					0.481***	
					(0.024)	
log degree share (t-1)	0.005***	0.006***	0.003**	-0.415***		0.828***
	(0.001)	(0.001)	(0.001)	(0.011)		(0.019)
log designation share (t-2)	-0.021	-0.020	-0.021***	0.005***	-0.025***	-0.005***
	(0.017)	(0.015)	(0.008)	(0.001)	(0.008)	(0.001)
Δ log homeownership (t)	0.527***	0.540***	0.636***	0.596***	-0.007	-0.707***
	(0.063)	(0.062)	(0.074)	(0.078)	(0.030)	(0.181)
log homeownership (t-1)	0.145***	0.174***	0.228***	0.183***	0.213***	-0.536***
	(0.030)	(0.033)	(0.045)	(0.041)	(0.019)	(0.131)
Δ log average hh. size (t)	-0.445***	-0.400***	-0.495***	-0.529***	0.162*	-0.153
	(0.076)	(0.067)	(0.079)	(0.089)	(0.068)	(0.286)
log average hh. size (t-1)	-0.235***	-0.277***	-0.250**	-0.091	-0.006	-1.318**
	(0.070)	(0.069)	(0.086)	(0.095)	(0.045)	(0.442)
log pop age (t-1)	-0.087	-0.040	-0.289***	0.001	0.008	0.584
	(0.052)	(0.055)	(0.072)	(0.059)	(0.033)	(0.335)
Δ pop age (t)	-0.321***	-0.256***	-0.490***	-0.552***	0.155***	0.216
	(0.086)	(0.068)	(0.095)	(0.079)	(0.042)	(0.356)
log foreigner share (t-1)	0.080***	0.083***	0.079***	0.076***	-0.005	0.053
	(0.008)	(0.009)	(0.009)	(0.007)	(0.003)	(0.045)
Δ foreigner share (t)	0.091***	0.087***	0.093***	0.077***	-0.003	0.009
	(0.019)	(0.016)	(0.020)	(0.016)	(0.003)	(0.068)
Log price trend			0.001			
			(0.028)			
Δ log vacancy rate (t)			0.037**			
			(0.012)			
log vacancy rate (t-1)			0.070***			
			(0.013)			
Log listed buildings			0.008			
			(0.004)			
log turnover in housing transactions (t)			-0.016**			
			(0.006)			
log of share of building from pre1945			0.016***			
			(0.004)			
average condition score (1 best, 4 worst)						
average vulnerability score (1 low, 8 high)						
average trajectory score (-2 improving, +2 deteriorating)						
Constant	0.687**	0.537*	1.457***	0.242	0.052	-0.739
	(0.233)	(0.219)	(0.342)	(0.309)	(0.171)	(1.446)

Table 5 (continued)						
Controls	YES	YES	YES	YES	YES	YES
FE	NO	YES	NO	NO	NO	NO
Price Trend	NO	NO	YES	NO	NO	NO
Housing Cond.	NO	NO	YES	NO	NO	NO
Residential wards	NO	NO	NO	YES	NO	NO
Observations	7965	7965	7965	7379	7965	7965
F	592.006	.	339.162	508.799	.	1852.756
R^2	0.708	0.742	0.719	0.709	0.960	0.717

Notes: See the data section for a description of control variables. IVs are station density, employment potential and the degree share in t-2 all models. Model (3) includes a dummy variable indicating 60 wards for which no price trend could be computed due to insufficient transactions. We derive the instrument (predicted *Δ* log degree share (t) x homeownership (t-1)) for the interaction term in model (5) by interacting homeownership (t-1) with the predicted values of an auxiliary regression where we regress *Δ* log degree share on the exogenous variables, i.e. on the standard IVs and controls. Standard errors in parentheses and clustered on fixed effects. $^{*}p< 0.05$, $^{**}p< 0.01$, $^{***}p< 0.001$.

We have tried four alternative IV models which are based on the benchmark model, i.e., including the set of controls (Table 1, column 2 in the main paper). The coefficient estimates reported in Table 6 remain qualitatively similar and quantitatively close to the main model. First stage results are reported in appendix Table 7. The alternative instruments, again, pass the validity tests. Only the overidentification test is failed by specification (1) using employment potentiality and museum density as instruments.

Table 6: Alternative IV models

	(1) Δ log designation share (t)	(2) Δ log designation share (t)	(3) Δ log designation share (t)	(4) Δ log designation share (t)
Δ log degree share (t)	-0.828***	-0.860***	-0.845***	-0.875***
	(0.113)	(0.115)	(0.111)	(0.117)
log degree share (t-1)	-0.408***	-0.421***	-0.415***	-0.427***
	(0.047)	(0.047)	(0.046)	(0.048)
log designation share (t-1)	0.003	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Δ log homeownership (t)	0.594***	0.612***	0.604***	0.610***
	(0.070)	(0.071)	(0.070)	(0.071)
log homeownership (t-1)	0.194***	0.196***	0.194***	0.197***
	(0.023)	(0.023)	(0.023)	(0.023)
Δ log average household size (t)	-0.313***	-0.329***	-0.324***	-0.334***
	(0.077)	(0.078)	(0.077)	(0.078)
log average household size (t-1)	-0.281***	-0.295***	-0.289***	-0.299***
	(0.075)	(0.076)	(0.075)	(0.076)
log pop age (t-1)	-0.240***	-0.246***	-0.243***	-0.246***
	(0.062)	(0.062)	(0.062)	(0.062)
Δ pop age (t)	-0.270***	-0.280***	-0.277***	-0.273***
	(0.083)	(0.083)	(0.082)	(0.082)
log foreigner share (t-1)	0.074***	0.077***	0.075***	0.078***
	(0.014)	(0.014)	(0.014)	(0.014)
Δ foreigner share (t)	0.070***	0.073***	0.072***	0.075***
	(0.016)	(0.016)	(0.016)	(0.016)
Constant	1.394***	1.436***	1.419***	1.438***
	(0.289)	(0.291)	(0.289)	(0.291)
Controls	YES	YES	YES	YES
IV	YES	YES	YES	YES
Observations	7965	7965	7965	7968
CHI2	319.851	318.289	321.092	316.186
EXOG_P	0.000	0.000	0.000	0.000
OVERID	2.289	0.084	0.500	0.233
OVERIDP	0.130	0.772	0.479	0.629
Instruments (as densities except employment pot.)	Employment potentiality Museum	Employment potentiality Coffee place	Employment potentiality Bar	Rail station Coffee place

Notes: See the data section for a description of control variables. Standard errors in parentheses and clustered on fixed effects. $^{*}p< 0.05$, $^{**}p< 0.01$, $^{***}p< 0.001$.

Table 7: Alternative IV models – first stage regressions

	(1) Δ log degree share (t)	(2) Δ log degree share (t)	(3) Δ log degree share (t)	(4) Δ log degree share (t)
employment potentiality	3.07E-8*** (0.000)	2.95E-8*** (0.000)	2.85E-8*** (0.000)	
museum density	0.086 (0.053)			
coffee place density		0.004 (0.004)		-0.007 (0.005)
bar density			0.004 (0.003)	
rail station density				0.196*** (0.018)
log degree share (t-1)	-0.409*** (0.010)	-0.410*** (0.010)	-0.411*** (0.010)	-0.409*** (0.009)
log designation share (t-2)	0.005*** (0.001)	0.005*** (0.001)	0.005*** (0.001)	0.005*** (0.001)
Δ log homeownership (t)	0.521*** (0.064)	0.516*** (0.063)	0.521*** (0.067)	0.534*** (0.061)
log homeownership (t-1)	0.137*** (0.032)	0.135*** (0.034)	0.141*** (0.034)	0.128** (0.039)
Δ log average household size (t)	-0.465*** (0.070)	-0.463*** (0.070)	-0.455*** (0.070)	-0.441*** (0.077)
log average household size (t-1)	-0.272*** (0.067)	-0.276*** (0.066)	-0.257*** (0.061)	-0.240*** (0.064)
log pop age (t-1)	-0.099 (0.051)	-0.099 (0.052)	-0.088 (0.053)	-0.101 (0.052)
Δ pop age (t)	-0.314*** (0.086)	-0.316*** (0.090)	-0.312*** (0.085)	-0.345*** (0.086)
log foreigner share (t-1)	0.081*** (0.009)	0.082*** (0.009)	0.081*** (0.009)	0.087*** (0.010)
Δ foreigner share (t)	0.090*** (0.019)	0.091*** (0.019)	0.091*** (0.019)	0.091*** (0.018)
Constant	0.039 (0.092)	0.051 (0.094)	0.035 (0.091)	-0.015 (0.091)
Controls	YES	YES	YES	YES
Observations	7965	7965	7965	7968
F	568.539	566.433	573.506	525.781
R^2	0.706	0.706	0.707	0.705

Notes: See the data section for a description of control variables. Standard errors in parentheses and clustered on fixed effects. $^{*}p< 0.05$, $^{**}p< 0.01$, $^{***}p< 0.001$.

Furthermore, we have split the long difference between 1991 and 2011 into two shorter differences of 1991 to 2001 and 2001 to 2011. For the latter short difference we moreover used the change in income instead of change in degree as a proxy for heritage preferences. The coefficient estimates remain qualitatively similar to the main model and are reported with their first stages in tables 8 and 9. The coefficient of the key variable is slightly smaller in the benchmark specification of the short different between 1991 and 2001 (column 4) and considerably larger for the period between 2001 and 2011 (column 8). In columns (9)–(12) we use income as a proxy of heritage preference. Focusing on the benchmark specification in the final column, doubling income more than quadruples the designation share. The respective instruments are valid and sufficiently strong. Overall, the results are in line with our theory; increases in heritage preferences, proxied by change in degree or change in income, lead to increases in designation shares.

Table 8: Short differences and income model

	(1) 1991-2001 Δ log designation share (t)	(2) 1991-2001 Δ log designation share (t)	(3) 1991-2001 Δ log designation share (t)	(4) 1991-2001 Δ log designation share (t)	(5) 2001-2011 Δ log designation share (t)	(6) 2001-2011 Δ log designation share (t)	(7) 2001-2011 Δ log designation share (t)	(8) 2001-2011 Δ log designation share (t)	(9) 2001-2011 Δ log designation share (t)	(10) 2001-2011 Δ log designation share (t)	(11) 2001-2011 Δ log designation share (t)	(12) 2001-2011 Δ log designation share (t)
Δ log degree share (t)	-0.017**	-0.216***	-0.066***	-0.483***	0.477***	1.653***	-0.010	-2.129**				
	(0.009)	(0.021)	(0.014)	(0.079)	(0.052)	(0.126)	(0.080)	(0.919)				
log degree share (t-1)			-0.056***	-0.185***			-0.117***	-0.535***				
			(0.007)	(0.024)			(0.027)	(0.182)				
log designation share (t-1)			-0.003***	0.004***			-0.012***	-0.009***			-0.014***	0.004
			(0.001)	(0.001)			(0.003)	(0.003)			(0.002)	(0.007)
Δ log homeownership (t)			-0.056***	0.386***			-0.117***	0.732*			-0.027	1.194***
			(0.007)	(0.056)			(0.027)	(0.385)			(0.116)	(0.434)
log homeownership (t-1)			0.129***	0.077***			-0.122	0.340***			0.098**	0.777***
			(0.028)	(0.014)			(0.115)	(0.127)			(0.042)	(0.237)
Δ log average household size (t)			0.068***	-0.245***			0.057	-0.727			0.190	0.074
			(0.013)	(0.062)			(0.037)	(0.450)			(0.181)	(0.272)
log average household size (t-1)			0.004	-0.162***			0.219	-0.099			0.278***	0.129
			(0.037)	(0.049)			(0.185)	(0.177)			(0.095)	(0.149)
log pop age (t-1)			-0.027	-0.158***			0.241**	0.041			0.285**	-1.364**
			(0.037)	(0.036)			(0.095)	(0.185)			(0.112)	(0.559)
Δ pop age (t)			-0.109***	-0.188***			0.389***	-0.107			0.519**	-2.009**
			(0.033)	(0.056)			(0.112)	(0.362)			(0.217)	(0.899)
log foreigner share (t-1)			-0.044	0.057***			0.557***	-0.004			-0.025*	0.101**
			(0.048)	(0.011)			(0.211)	(0.016)			(0.015)	(0.046)
Δ foreigner share (t)			0.001	0.121***			-0.017	-0.001			-0.026	-0.104**
			(0.004)	(0.025)			(0.014)	(0.038)			(0.028)	(0.048)
Δ log income									-0.218***	-9.330***	-0.142**	-7.305***
									(0.069)	(2.024)	(0.070)	(2.364)
log income (t-1)											-0.144***	-0.909***
											(0.037)	(0.261)
Constant	0.159***	0.224***	0.489***	0.864***	0.317***	-0.126***	-1.436***	0.367	0.549***	2.881***	0.007	13.647***
	(0.005)	(0.009)	(0.143)	(0.167)	(0.022)	(0.043)	(0.472)	(0.900)	(0.027)	(0.524)	(0.556)	(4.552)

Table 8 (continued)

IV	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Observations	7965	7965	7965	7965	7966	7966	7966	7966	7966	7966	7966	7966
CHI2		103.847		202.519		170.741		203.917		21.242		88.061
EXOG_P		0.000		0.000		0.000		0.012		0.000		0.000
OVERID		7.555		1.413		1.385		19.198		13.526		0.741
OVERIDP		0.006		0.235		0.239		0.000		0.000		0.389

Notes: See the data section for a description of control variables. Standard errors in parentheses. $^{*}p< 0.05$, $^{**}p< 0.01$, $^{***}p< 0.001$.

Table 9: Short differences and income model – First stage regressions

	(1) 1991-2001 Δ log degree share (t)	(2) 1991-2001 Δ log degree share (t)	(3) 1991-2001 log designation share (t-1)	(4) 2001-2011 Δ log degree share (t)	(5) 2001-2011 Δ log degree share (t)	(6) 2001-2011 log designation share (t-1)	(7) 2001-2011 Δ log income (t)	(8) 2001-2011 Δ log income (t)	(9) 2001-2011 log designation share (t-1)
rail station density	0.055	0.053*	-0.003	-0.062***	0.038***	0.059	-0.012	0.018	0.066
	(0.049)	(0.021)	(0.208)	(0.010)	(0.008)	(0.151)	(0.037)	(0.029)	(0.159)
employment potentiality	0.000***	0.000***	0.000	-0.000***	-0.000	0.000	0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log degree share (t-1)		0.055	0.053*	-0.003	-0.062***	0.038***			
		(0.049)	(0.021)	(0.208)	(0.010)	(0.008)			
log designation share (t-2)		0.007***	0.828***		0.000	0.922***		0.002**	0.927***
		(0.001)	(0.019)		(0.000)	(0.010)		(0.001)	(0.009)
Δ log homeownership (t)		0.586***	-0.613**		0.408***	-1.137***		0.172	-1.232***
		(0.067)	(0.232)		(0.048)	(0.328)		(0.101)	(0.343)
log homeownership (t-1)		0.061**	-0.431***		0.143***	-0.114		0.110***	-0.141
		(0.022)	(0.118)		(0.018)	(0.102)		(0.028)	(0.126)
Δ log average household size (t)		-0.534***	-0.161		-0.424***	0.664		-0.009	0.733
		(0.044)	(0.325)		(0.093)	(0.725)		(0.089)	(0.733)
log average household size (t-1)		-0.253***	-1.519***		-0.139*	0.273		-0.004	0.227
		(0.041)	(0.436)		(0.059)	(0.258)		(0.067)	(0.249)
log pop age (t-1)		0.004	0.555		-0.154***	0.744*		-0.217***	0.896*
		(0.048)	(0.337)		(0.045)	(0.350)		(0.058)	(0.345)
Δ pop age (t)		-0.231***	0.311		-0.325***	0.152		-0.362**	0.245
		(0.051)	(0.370)		(0.077)	(0.455)		(0.118)	(0.443)
log foreigner share (t-1)		0.110***	0.085*		0.004	-0.064		0.015**	-0.035
		(0.009)	(0.042)		(0.005)	(0.045)		(0.005)	(0.046)
Δ foreigner share (t)		0.267***	0.061		0.026*	0.023		-0.010	-0.021
		(0.017)	(0.084)		(0.012)	(0.065)		(0.007)	(0.066)
Log income (t-1)								-0.114***	0.191
								(0.020)	(0.101)
Constant	0.297***	0.278	-0.336	0.389***	0.790***	-3.160*	0.255***	1.880***	-5.076**
	(0.008)	(0.209)	(1.438)	(0.005)	(0.221)	(1.479)	(0.004)	(0.239)	(1.602)
Controls	NO	YES	YES	NO	YES	YES	NO	YES	YES
Observations	7965	7965	7965	7966	7966	7966	7966	7966	7966
F	134.968	557.956	1891.124	73.689	464.362	3091.590	8.301	17.028	2640.502
R^2	0.124	0.590	0.717	0.095	0.614	0.856	0.004	0.103	0.856

Notes: See the data section for a description of control variables. Standard errors in parentheses and clustered on fixed effects. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

5.2 Equilibrium designation

Table 10 below reports the conservation area effects as well as the full set of hedonic controls, housing characteristics in particular, for the difference-in-differences estimation given by equation (26) in the main paper. Column (7) shows that housing units with more bathrooms and bedrooms fetch higher prices, as do detached, semi-detached, and bungalows (over the omitted category flats/maisonettes). The sales price of terraced housing is insignificantly different from flats/maisonettes. Larger floor spaces are associated with higher price but with significant diminishing effects. There is a premium for new properties. Leased properties are of less value than those owned. Properties with parking spaces, single garages and double garages sell for higher prices than those without any parking facilities. There is a house price premium for properties with central heating over other types of heating. In order to control for a potentially non-linear relationship between housing age and house prices we included a series of house age bins. In order to separate the effects of pure building age (which may be associated with deterioration) from the build date (which may strongly determine the architectural style) we allow for age cohort and building data cohort effects. Since the 'New property' variable identifies all properties where the build age is zero years, the omitted category from the age variables is 1–9 years. All of the bins for properties older than this indicate significant negative premiums. The negative premium increases with age, mostly quickly over the first few categories and then more slowly until the penultimate category and finally decreases for buildings over 100 years. The effect of the build date is also non-linear. The general tendency is for buildings built in earlier periods to have higher prices than buildings built in the omitted period 2000–2010. However, this effect becomes insignificant in the 60s and 70s; periods associated with the architectural styles of the post-ward reconstruction phase that are today less appreciated than other styles. The greatest premium is attached to houses built pre-1900, the earliest category.

Table 10: Conservation area premium – designation effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Inside treated CA	0.028***	0.014	0.014	0.003	-0.024	-0.077	-0.003
× Post designation	(0.009)	(0.009)	(0.010)	(0.012)	(0.070)	(0.111)	(0.013)
Within 500m buffer of	0.023***	0.013***	0.012***	0.004	0.012	-0.005	-0.005
treated CA × Post des.	(0.004)	(0.004)	(0.005)	(0.006)	(0.027)	(0.022)	(0.010)
Inside treated CA	-0.043***	-0.038***	-0.048***	-0.037***	-0.062	0.029	-0.024
	(0.009)	(0.009)	(0.010)	(0.012)	(0.057)	(0.108)	(0.021)
Within 500m buffer of	-0.010**	-0.004	-0.011**	0.005	0.003	0.006	-0.002
treated CA	(0.004)	(0.004)	(0.005)	(0.005)	(0.030)	(0.023)	(0.013)
Number of bathrooms	0.007***	0.007***	0.006***	0.013***	0.057***	0.059***	0.014***
	(0.000)	(0.001)	(0.001)	(0.002)	(0.008)	(0.006)	(0.002)
Number of bedrooms	0.166***	0.172***	0.169***	0.165***	0.170***	0.179***	0.158***
	(0.002)	(0.004)	(0.005)	(0.005)	(0.014)	(0.011)	(0.006)
Number of bedrooms	-0.019***	-0.020***	-0.020***	-0.019***	-0.019***	-0.019***	-0.018***
squared	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
Detached house	0.254***	0.222***	0.211***	0.194***	0.235***	0.216***	0.193***
	(0.003)	(0.005)	(0.008)	(0.007)	(0.015)	(0.014)	(0.007)
Semi-detached house	0.119***	0.097***	0.088***	0.070***	0.082***	0.066***	0.073***
	(0.003)	(0.004)	(0.007)	(0.006)	(0.014)	(0.012)	(0.006)
Terraced	0.040***	0.026***	0.015**	0.001	0.002	-0.013	-0.000
house/Country cottage	(0.003)	(0.004)	(0.006)	(0.006)	(0.013)	(0.012)	(0.006)
Bungalow	0.311***	0.285***	0.281***	0.257***	0.292***	0.269***	0.257***
	(0.003)	(0.006)	(0.008)	(0.009)	(0.019)	(0.016)	(0.009)
Floorsize (m^2)	0.006***	0.006***	0.007***	0.007***	0.008***	0.007***	0.007***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Floorsize (m^2)	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
× Floorsize (m^2)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
New property	0.084***	0.087***	0.088***	0.088***	0.047**	0.076***	0.077***
	(0.002)	(0.004)	(0.005)	(0.006)	(0.024)	(0.017)	(0.006)
Leasehold	-0.054***	-0.067***	-0.065***	-0.073***	-0.100***	-0.104***	-0.070***
	(0.003)	(0.004)	(0.006)	(0.006)	(0.014)	(0.012)	(0.006)
Single garage	0.112***	0.097***	0.100***	0.097***	0.096***	0.097***	0.098***
	(0.001)	(0.002)	(0.003)	(0.003)	(0.007)	(0.005)	(0.003)
Double garage	0.190***	0.162***	0.161***	0.159***	0.160***	0.156***	0.158***
	(0.002)	(0.003)	(0.005)	(0.005)	(0.015)	(0.010)	(0.005)
Parking space	0.076***	0.063***	0.065***	0.061***	0.052***	0.049***	0.063***
	(0.001)	(0.002)	(0.003)	(0.003)	(0.007)	(0.005)	(0.003)
Central heating	0.089***	0.094***	0.098***	0.100***	0.085***	0.094***	0.095***
	(0.001)	(0.002)	(0.003)	(0.003)	(0.007)	(0.007)	(0.003)
Building age: 10–19	-0.047***	-0.063***	-0.062***	-0.075***	-0.071***	-0.068***	-0.069***
years	(0.002)	(0.003)	(0.004)	(0.005)	(0.016)	(0.015)	(0.005)
Building age: 20–29	-0.079***	-0.106***	-0.104***	-0.125***	-0.133***	-0.126***	-0.113***
years	(0.002)	(0.005)	(0.007)	(0.008)	(0.026)	(0.021)	(0.007)
Building age: 30–39	-0.092***	-0.127***	-0.123***	-0.150***	-0.169***	-0.141***	-0.133***
years	(0.003)	(0.006)	(0.010)	(0.011)	(0.032)	(0.027)	(0.009)
Building age: 40–49	-0.104***	-0.148***	-0.142***	-0.180***	-0.199***	-0.165***	-0.158***
years	(0.004)	(0.008)	(0.012)	(0.013)	(0.036)	(0.031)	(0.011)
Building age: 50–59	-0.121***	-0.171***	-0.167***	-0.207***	-0.232***	-0.204***	-0.175***
years	(0.004)	(0.009)	(0.015)	(0.016)	(0.044)	(0.038)	(0.014)
Building age: 60–69	-0.135***	-0.198***	-0.194***	-0.238***	-0.320***	-0.265***	-0.215***
years	(0.005)	(0.011)	(0.019)	(0.020)	(0.051)	(0.042)	(0.018)
Building age: 70–79	-0.136***	-0.213***	-0.207***	-0.263***	-0.326***	-0.273***	-0.234***
years	(0.006)	(0.013)	(0.021)	(0.022)	(0.053)	(0.046)	(0.019)
Building age: 80–89	-0.132***	-0.218***	-0.213***	-0.277***	-0.339***	-0.313***	-0.243***
years	(0.007)	(0.014)	(0.023)	(0.024)	(0.062)	(0.054)	(0.021)
Building age: 90–99	-0.111***	-0.208***	-0.204***	-0.280***	-0.360***	-0.304***	-0.248***
years	(0.008)	(0.016)	(0.025)	(0.027)	(0.068)	(0.063)	(0.023)

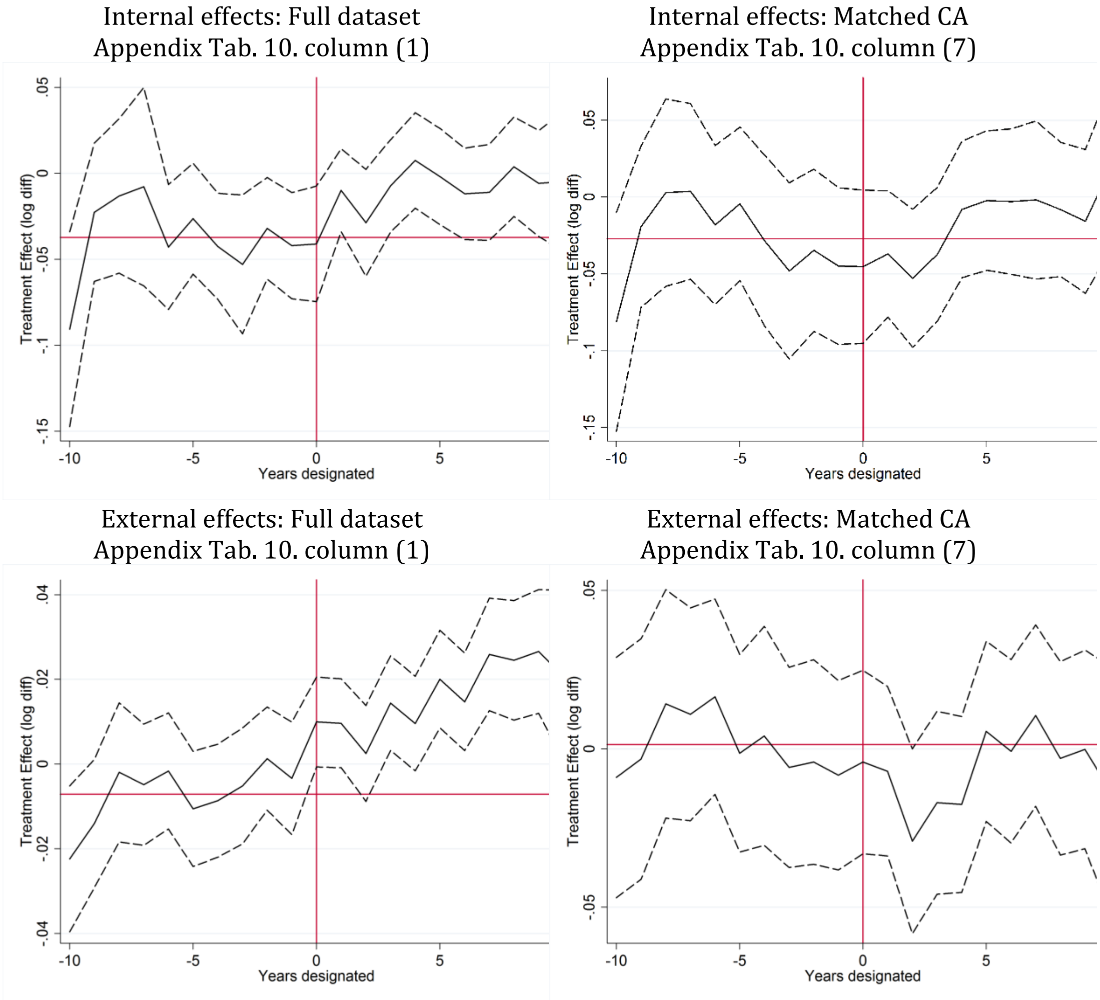
Building age: Over 100 years	-0.083***	-0.176***	-0.176***	-0.261***	-0.348***	-0.284***	-0.227***
	(0.009)	(0.017)	(0.027)	(0.030)	(0.074)	(0.065)	(0.025)
Build date: 1900–1909	0.040***	0.121***	0.128***	0.208***	0.256***	0.222***	0.173***
	(0.009)	(0.018)	(0.028)	(0.031)	(0.077)	(0.067)	(0.025)
Build date: 1910–1919	0.074***	0.153***	0.158***	0.226***	0.262***	0.256***	0.196***
	(0.008)	(0.016)	(0.027)	(0.028)	(0.071)	(0.059)	(0.024)
Build date: 1920–1929	0.093***	0.157***	0.162***	0.215***	0.225***	0.189***	0.190***
	(0.007)	(0.014)	(0.024)	(0.025)	(0.062)	(0.050)	(0.021)
Build date: 1930–1939	0.082***	0.128***	0.130***	0.168***	0.187***	0.163***	0.151***
	(0.006)	(0.013)	(0.021)	(0.023)	(0.058)	(0.045)	(0.020)
Build date: 1940–1949	0.040***	0.078***	0.078***	0.111***	0.063	0.053	0.096***
	(0.005)	(0.012)	(0.018)	(0.021)	(0.058)	(0.048)	(0.018)
Build date: 1950–1959	0.017***	0.033***	0.041***	0.057***	0.017	-0.004	0.046***
	(0.004)	(0.010)	(0.016)	(0.018)	(0.047)	(0.039)	(0.015)
Build date: 1960–1969	0.001	0.007	0.018	0.023	-0.017	-0.012	0.011
	(0.004)	(0.009)	(0.013)	(0.015)	(0.044)	(0.037)	(0.013)
Build date: 1970–1979	-0.015***	-0.016**	-0.008	-0.004	-0.059	-0.046	-0.011
	(0.003)	(0.007)	(0.011)	(0.012)	(0.042)	(0.033)	(0.011)
Build date: 1980–1989	0.013***	0.017***	0.025***	0.029***	-0.023	-0.010	0.024***
	(0.003)	(0.006)	(0.008)	(0.010)	(0.038)	(0.029)	(0.008)
Build date: 1990–1999	0.022***	0.020***	0.022***	0.029***	-0.020	-0.008	0.017**
	(0.002)	(0.005)	(0.006)	(0.008)	(0.034)	(0.025)	(0.008)
Build date: pre 1900	0.098***	0.149***	0.162***	0.244***	0.312***	0.259***	0.216***
	(0.009)	(0.018)	(0.029)	(0.031)	(0.081)	(0.070)	(0.026)
Location cont.	YES	YES	YES	YES	YES	YES	YES
Neighborhood cont.	YES	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES	YES
Ward effects	YES	YES					
Nearest treated CA effects			YES	YES	YES	YES	
Matched CA effects							YES
Treatment group: CAs designated	1996-2010	1996-2010	1996-2010	1996-2010	1996-2002	1996-2002	1996-2010
Control group	Full England sample	Within 2km of treated CA	Within 2km of treated CA	Within 500m of pre-1996 CA & within 2km of treated CA	Within 500m of CA designated 1987-1995 & within 4km of treated CA	Within 500m of CA designated 2003-2010 & within 4km of treated CA	Within 500m of pre-1996 CA matched on propensity score
R^2	0.921	0.922	0.915	0.915	0.861	0.864	0.909
AIC	-587375	-156426	-130469	-67044	-5410	-8475	-41206
Observation	1088k	302k	302k	178k	214k	323k	133k

Notes: Standard errors in parentheses are clustered on location fixed effects. Conservation area control groups in Columns (4)-(7) have separate fixed effects for the areas inside and outside a conservation area. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Semi-parametric temporal and spatial treatment effects

Figure 2 reports the results for the semi-parametric estimation of the temporal effects of designation using appendix equation (19). Instead of simply presenting our two strongest specifications, as we do in the main paper, here we present a different dimension to the results bin by comparing the bin estimates for the naïve DD in the left panels to the matched CA control group in the right panels. The left charts show that the post-period internal and external estimates deviate significantly from the pre-period mean (hence the significant DD estimates) but that this is driven by a general upward trends. This corroborates the results in Table 2, column (1) of the main paper where no significant discontinuity nor shift in trend for the naïve control group exists and hence the advantages of the RDD-DD over the standard DD method is highlighted. The charts in the right panels also corroborate the evidence presented using the parametric trends equations in the main paper. Specifically, they show that for the internal effects the post-treatment estimates tend not to deviate significantly from the pre-treatment effects but that there are upward shifts in the trend when compared to the pre-treatment trend. For the external effects there is a general upward trend in the less carefully matched control groups and a downward trend in the stronger control groups but no shift in the trend at the designation date.

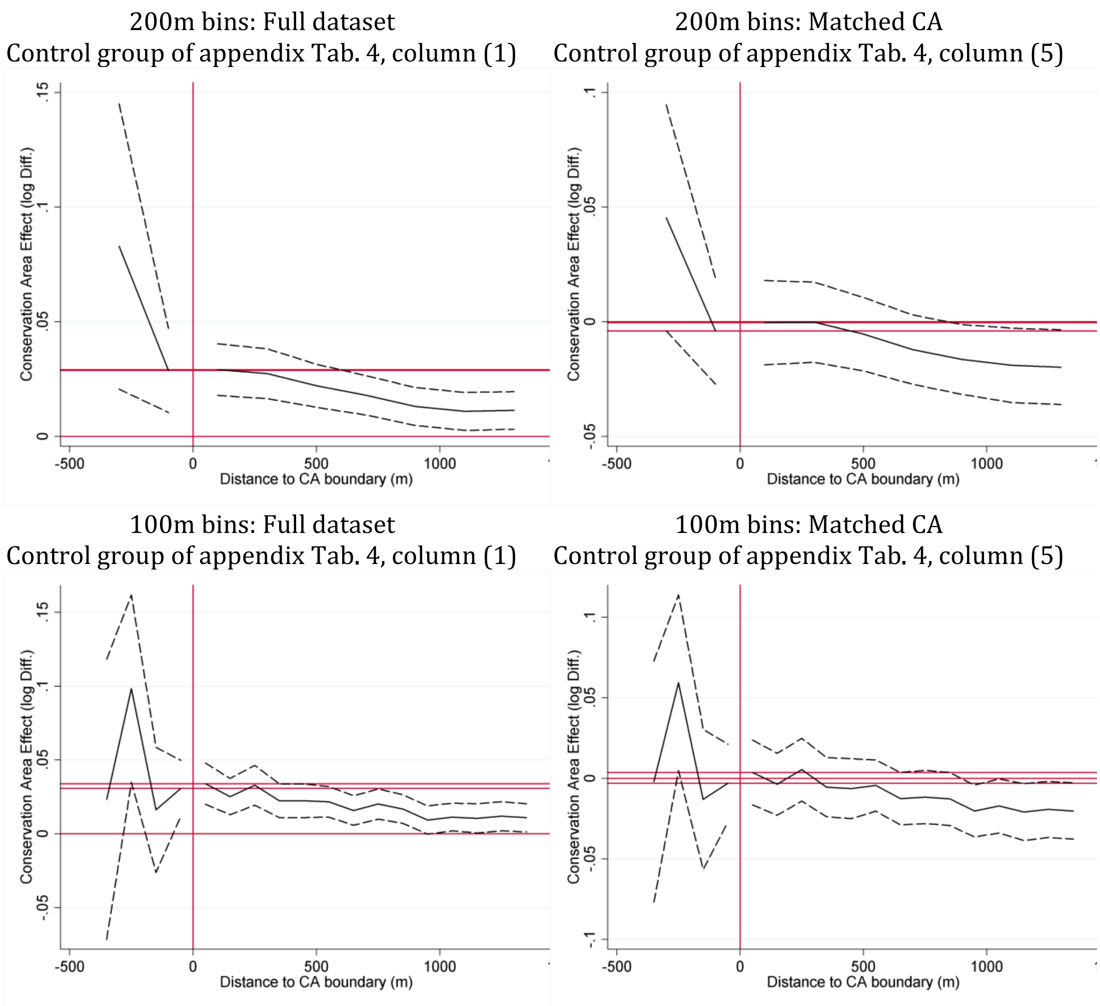
Figure 2: Semi-parametric temporal bins estimates



Notes: The solid black line plots the estimated differences between treatment group and control group against year since designation date using equation (19). The dashed lines indicate the 5% confidence intervals. The left charts show results for the control group used in column (1) of appendix Table 10. The right charts show results for the control group used in column (7) of appendix Table 10. The horizontal red line illustrates the mean of the pre-treatment estimates.

Figure 3 demonstrates the semi-parametric spatial effects using different bin sizes of 100m and 200m using appendix equation (20). These semi-parametric charts closely resemble their parametric counterparts. Notably, there is no significant and positive effect in the first bin outside the conservation area when using the preferred specification of column (7) from Table 10 This is consistent with the parametric findings and baseline DD findings that there is no significant external policy effect and that our second hypothesis cannot be accepted. There is, however, one significant bin inside the conservation area at 200–300m. This provides some support for the idea that heritage externalities are stronger deeper within the conservation areas such that there may be a positive policy effect. This effect then declines to zero for the deepest bin of greater than 300m.

Figure 3: Semi-parametric spatial bins estimates



Notes: The solid black line plots estimate the difference-in-differences treatment effect at different distances from the conservation area boundary using appendix equation (20). The dashed lines indicate the 5% confidence intervals. The left charts show results for the control group used appendix Table 4, column (1). The right charts show results for the control group used in appendix Table 4, column (5). The horizontal red lines illustrate the mean of the pre-treatment estimates, the final pre-period bin and the first post-period bin.

6. Literature

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CHAPTER V

THE WELFARE ECONOMICS OF HERITAGE CONSERVATION AREAS: QUALITY OF LIFE VS. HOUSING PRODUCTIVITY

1. Introduction*

The key to success for any city is to offer a high quality of life whilst remaining affordable to live in. Therefore, whether or not to regulate development to preserve historic districts is an important policy decision for any urban area. Such policies improve the quality of life in cities by preserving districts of special architectural and historic character. But they do so by restricting the supply of new housing space therefore increasing housing costs. A crucial policy consideration is how large each of these effects are and what the net effect is. Put simply, are conservation areas welfare improving or are they welfare decreasing?

Evidence suggests the costs of housing regulation are very significant. Hilber and Vermeulen (In Press) examine planning constraints in England finding that they lead to significantly higher housing costs. Such policies are usually intended to provide benefits by avoiding the negative externalities of density[53]. However the literature suggests that the *regulatory tax* outweighs any benefits of *externality zoning*. For example, Glaeser et al. (2003) examine building height restrictions in Manhattan, a policy that is intended to prevent towering developments that block the light and view available to existing structures. They find that the restrictions lead to such large increases in house prices that residents are left worse off even after accounting for the policy benefits. This finding is repeated in other studies such as that by Albouy and Ehrlich (2012) who look at the regulatory constraints

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[53] Or in the case of conservation areas also to preserve positive externalities.

across U.S. cities and Cheshire and Sheppard (2002) who examine land use planning in the city of Reading, England.

The literature on conservation areas, however, has tended to focus only on the determinants of designation and the local impacts on quality of life. The externality zoning effect has been found to be significant. Ahlfeldt et al. (2014a) find a positive impact of designation on house price growth that is related to the security designation gives residents about the future character of their neighbourhood (Holman & Ahlfeldt, 2014). Furthermore, in a model of the political economy of conservation areas (Ahlfeldt et al., 2014a) demonstrate that local homeowners may have a strong influence over the designation process. Hence designations that impose wider costs will still occur where there exist local benefits to those who have influence over the political process. So whilst the literature on conservation areas does not include any estimates of the size of the regulatory tax they impose on housing, the evidence does not preclude the possibility that such an effect exist.

This paper estimates the net effect of conservation areas on economic welfare, a question that is of clear policy importance but as yet unanswered in the literature. It does so by looking at ten years of conservation area designations in England (1997-2007). The two-step approach is based on the theoretical model and empirical strategy outlined by Albouy and Ehrlich (2012). Firstly, I estimate housing productivity across English Housing Market Areas (HMAs) using a unique panel dataset of house prices, land values and construction costs. HMAs, unlike other urban area definitions are endogenously defined to capture individual housing markets, based on evidence from patterns of commuting, migration and house prices. As such they typically approximate recognisable city regions. Housing productivity is defined as the amount of physical housing that can be produced for given quantities of inputs. I estimate the effect of various city-specific characteristics on housing productivity finding that conservation area designation significantly increases housing costs. Secondly, I generate a quality of life index for cities based on house prices and wages. Differences in housing productivity predicted by designation are not found to be significantly correlated with quality of life. My results therefore suggest that the overall impact of conservation areas is to reduce welfare by increasing housing costs without sufficiently compensating for this with quality of life improvements.

In addition to filling a gap in the literature by estimating both the supply-side costs and demand-side benefits of conservation I make a number of further contributions to the literature. To my knowledge, I estimate the first housing production function for England producing the first estimates of a land cost share and elasticity of substitution, which are of wider significance. I provide descriptive (cross-sectional) indications of the net welfare effect of protected land statuses in England such as Green Belts, National Parks and Areas of Outstanding Natural Beauty (albeit aggregated together). I also note the empirical problems specific to this methodological approach and demonstrate how fixed effects estimation serves as an improvement to both stages. Finally, I construct a unique dataset making use of some previously unused data for land values and constructions costs for England.

This analysis of the conservation areas adds to a growing body of literature on the effects of designation policies (e.g. Ahlfeldt et al., 2014a; Asabere et al., 1989; Asabere & Huffman, 1994; Asabere et al., 1994; Coulson & Lahr, 2005; Coulson & Leichenko, 2001; Glaeser, 2011; Leichenko et al., 2001; Noonan, 2007; Noonan & Krupka, 2011; Schaeffer & Millerick, 1991) and a literature that looks into the value amenities add to neighbourhoods and cities more generally (e.g. Ahlfeldt et al., 2012; Albouy, 2009; Bayer et al., 2007; Brueckner et al., 1999; Chay & Greenstone, 2005; Cheshire & Sheppard, 1995; Gibbons et al., 2011; Glaeser et al., 2001).

The results are also relevant to research that investigates the costs and benefits of restrictive planning regimes (e.g. Albouy & Ehrlich, 2012; Cheshire & Hilber, 2008; Cheshire et al., 2011; Glaeser et al., 2003; Glaeser et al., 2005; Hilber & Vermeulen, In Press) and a literature that estimates production functions for housing (e.g. Albouy & Ehrlich, 2012; Epple et al., 2010; McDonald, 1981; Thorsnes, 1997). The outline of the rest of the paper is as follows. In the next section I lay out the theoretical model which demonstrates the potential effects of conservation areas on quality of life and housing productivity. In section 3, I develop the two-stage empirical approach explaining the need to estimate a fixed effect model. In section 4, I go over the data used in empirical analysis and in section 5, I present the results. Section 6 concludes.

2. Model

The theoretical model presented here is a general equilibrium model of a system of cities from Albouy and Ehrlich (2012), which was developed from the earlier models of Roback (1982a) and Albouy (2009). Each city j is small relative to the national economy and produces a traded good X and a non-traded good Y (housing). The city-specific price of a standard housing unit is p_j and the uniform price of the traded good is equal to the numeraire. Households with homogenous preferences work in either the Y-sector or the X-sector and consume both housing and the traded good. The model involves two important assumptions; that of perfect competition which gives the zero profit conditions and that of labour mobility which gives the spatial equilibrium conditions.

2.1 Housing production under zero profits

Since the focus of this paper is on the housing sector the derivations for the traded good are relegated to footnotes. The housing good Y represents physical housing services. By physical, it is meant that the services are derived solely from the unit itself. This does not include any benefits derived from locational amenities, which come in to the individual utility function via a quality of life measure defined separately later on. Firms produce housing in each city according to[54]:

$$Y_j = A_j^Y F^Y(L, M) \tag{1}$$

where A_j^Y is a city-specific housing productivity shifter, F^Y is a constant returns to scale (CRS) production function, L is land (price r_j in each city) and M is the

[54] The traded good is produced from land, labour and capital according to $X_j = A_j^X F^X(L, N^X, K)$ where A_j^X is traded good productivity which is a function of city characteristics, N^X is traded good labour (paid wages w_j^X) and K is mobile capital paid a price i everywhere.

materials (non-land) input to housing (paid price v_j). Materials is conceptualised to include all non-land factors to housing production including labour and machinery. The housing productivity shifter represents the efficiency with which developers can convert land and non-land inputs into physical housing and is a function of city specific attributes which may include the level of conservation area designation. Conservation areas decrease housing productivity because it increases to planning restrictiveness making it more difficult and costly for developers to build on a given plot of land.

Firms choose among inputs to minimise the unit cost for given factor prices $c_j(r_j, v_j; A_j) = \min_{L,M} \{r_j L + v_j M : f(L, M; A_j) = 1\}$. Perfect competition means zero profits are given when the price of a unit of housing is equal to this unit cost i.e. $p_j = c_j(r_j, v_j; A_j)$. Log-linearisation plus taking deviations around the national average gives[55]:

$$\tilde{p}_j = \phi_L \tilde{r}_j + \phi_M \tilde{v}_j - \tilde{A}_j^Y \qquad (2)$$

where for any variable z the tilde notation represents log differences around the national average i.e. $\tilde{z}_j = \ln(z_j) - \ln(\bar{z})$, where $\bar{z}$ is the national average[56] (so $\tilde{p}_j$ is the log price differential for housing units), ϕ_L is the land cost share for housing and ϕ_M is the non-land cost share,. This condition tells us that the equilibrium price differential for housing is given by the sum of the input price differentials weighted by their cost shares, subtracting the city-specific productivity shifter.

[55] Zero profits in the traded good sector is given by $\tilde{A}_j^X = \theta_L \tilde{r}_j + \theta_N \tilde{w}^X$ where θ_L and θ_N are the land and labour cost shares, respectively, for the traded good.

[56] Taking deviations from the national average is not theoretically necessary to solve the cost function or empirically necessary since the same effect can be achieved by using a constant (or year effects in a panel). However, the differentials are necessary is other parts of the model, such as for the traded good side, to eliminate the interest rate i, and for the spatial equilibrium equation, to eliminate the unobserved reservation utility u. Therefore for simplicity and consistency it is adopted throughout the paper.

This means that lower levels of housing productivity (perhaps due to designation) must be accounted for by higher house prices and/or lower land and materials prices in order to maintain zero profits. Next we examine the household side of the model[57].

2.2 Consumption and spatial equilibrium[58]

Households with homogenous preferences have a utility function $U_j\left(x, y; Q_j\right)$ that is quasi-concave in the traded good x and housing y and increases in city-specific quality of life Q_j. Quality of life is determined by non-market amenities that are available at each city ranging from air quality and green space to rail access and consumption amenities. These may also include conservation area designation. Households supply one unit of labour to receive a wage w_j, to which a non-wage income I is added to make total household income m_j. Households optimally allocate their budget according to the expenditure function $e^k\left(p_j, u; Q_j\right) = \min_{x,y} x + p_j y : \; U_j\left(x, y; Q_j\right) \geq u$. Households are assumed to be perfectly mobile, therefore, spatial equilibrium occurs when all location offer the same utility level $\bar{u}$. Locations with higher house prices or lower levels of quality of life amenities must be compensated with higher income after local taxation τ, i.e. $e\left(p_j, \bar{u}; Q_j\right) = (1-\tau)(w_j + I)$. Log-linearised around national average this is:

$$\tilde{Q}_j = s_y \tilde{p}_j - (1-\tau) s_w \widetilde{w}_j \tag{3}$$

where s_y is the average share of expenditure on housing, τ is the average marginal income tax rate and s_w is the average share of income that comes from

[57] To complete the firm-side of the model, the non-land input is produced using labour and capital $M_j = F^M(N^Y, K)$ and the equivalent zero profit condition gives $\tilde{v}_j = \alpha \widetilde{w}^Y$, where α is the labour cost share of the non-land input.

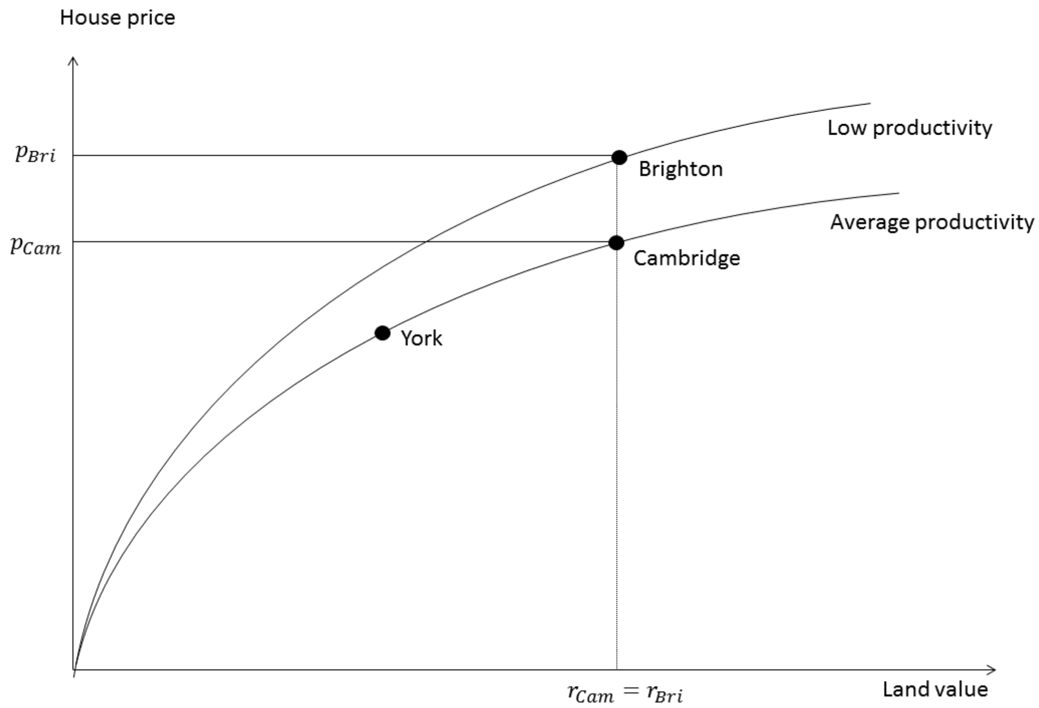
[58] There are two types of worker, those who work in housing and those who work in the traded good sector. They may each receive a different wage and may be attracted to different amenities. The condition for only one type of worker is presented here for simplicity.

wages. The spatial equilibrium condition tells us that the (expenditure-equivalent) quality of life differential in each location must be equal to the unit house price differential minus the wage differential, weighted by their shares in total expenditure. Essentially, if prices are high or wages are low then there must be plenty of quality of life amenities making a city attractive. This means that if designation impacts on quality of life, there must be a corresponding increase in house prices and/or decrease in wages to compensate. The two conditions, zero profit and spatial equilibrium, both suggest that conservation areas increase house prices but the two channels are entirely separate. Next, I examine each mechanism in turn to provide an intuition behind the different effects.

2.3 The effects of designation

Firstly, the zero profit implies that if two cities have similar equilibrium land values and material costs, then the one with lower housing productivity must have higher house prices. Figure 1 is adapted from Albouy and Ehrlich (2012) and illustrates this point for Cambridge, York and Brighton. The average productivity curve shows what house prices should be given different input prices (here just land values) for cities of average productivity if zero profits are maintained. Note that the curve is concave since developers substitute away from land as it becomes more expensive. For equally productive cities, if house prices are higher, then it must be that land values are higher, as in the case of Cambridge over York. Brighton, however, is less productive than Cambridge and this can be inferred from the fact that it has more expensive housing than Cambridge but has the same land values. This means that Brighton is less effective at converting housing inputs into housing outputs i.e. it is less productive. Therefore if we observe higher levels of designation in Brighton than in Cambridge and York, this may be because designation is reducing housing productivity in Brighton. Obviously, a sample size of three without any controls for other factors is not a very robust analysis but this should highlight the idea that underpins the empirical approach.

Figure 1: Cost function for housing



Note: this figure is an adaptation of Figure 1A from Albouy and Ehrlich (2012).

Secondly, the spatial equilibrium condition implies that if two cities have similar equilibrium wage levels, then the one with higher house prices must offer a higher quality of life. If cities that have higher level of quality of life also have high levels of designation then this may be because there exists a relationship between the two. It is important to note that this quality of life effect will look different to the relationship created by the housing productivity effect. If designation increases quality of life and housing productivity remains unchanged then house prices will increase to maintain spatial equilibrium but land values will also need to increase in order to maintain zero profits for developers. Hence, the city will have both higher house price and higher land values, moving upwards along the same productivity curve, e.g. from York to Cambridge in Figure 1. Thus the quality of life effect cannot be confused with the housing productivity effect. And vice versa, the housing productivity effect cannot be confused with the quality of life effect. If house prices are higher due to productivity difference (as in Brighton over Cambridge) but quality of life is the same then it must be that equilibrium wages are higher to maintain spatial equilibrium. Now that the intuition behind measuring the separate effects is clear, I move on to the empirical approach.

3. Empirical approach

The empirical approach takes two stages. First I estimate house prices as a function of input prices and factors that may affect housing productivity. Then I construct a quality of life index for each city using house prices and wages and relate this to productivity differences resultant from the level of designation. I conclude the section with a discussion of identification issues.

3.1 Estimation of housing productivity

Following Albouy and Ehrlich (2012) and Christensen et al. (1973) I first estimate an unrestricted translog cost function:

$$\begin{aligned}\tilde{p}_{jt} = {} & \beta_1\tilde{r}_{jt} + \beta_2\tilde{v}_{jt} + \beta_3\left(\tilde{r}_{jt}\right)^2 + \beta_4\left(\tilde{v}_{jt}\right)^2 + \beta_5\left(\tilde{r}_{jt}\tilde{v}_{jt}\right) + \pi\tilde{R}_{jt} + \sigma\tilde{N}_{jt} + \omega\tilde{P}_{jt} \\ & + \delta\tilde{D}_{jt} + u_{jt}\end{aligned} \tag{4}$$

where $\tilde{R}_{jt}$ (regulatory environment), $\tilde{N}_{jt}$ (natural constraints), $\tilde{P}_{jt}$ (population) are factors that are thought in the literature to affect housing supply (e.g. by Saiz 2010), $\tilde{D}_{jt}$ is conservation area designation, and π, σ, ω and δ are the parameters to be estimated. In this panel format, the log-differentials are taken around the national average in each year t. This is equivalent to using year effects in the regression, however, I continue to use the differentials that are suggested by the theoretical model. Imposing the restriction of CRS: $\beta_1 = 1 - \beta_2$; $\beta_3 = \beta_4 = -\beta_5/2$ makes this equivalent to a second order approximation of equation (2) and imposing the further restrictions of $\beta_3 = \beta_4 = \beta_5 = 0$ makes this a first order estimation i.e. a Cobb-Douglas cost function (Fuss & McFadden, 1978). Comparing equation (4) with equation (2) reveals that housing productivity is given by:

$$\tilde{A}_J^Y = -\tilde{R}_{jt}\pi - \tilde{N}_{jt}\sigma - \tilde{P}_{jt}\omega - \delta\tilde{D}_{jt} - u_{jt} \tag{5}$$

Housing productivity is the (negative of) observed and unobserved city attributes that impact on unit house prices after taking into account input prices. If designation (or any other factor) impacts negatively on housing productivity then its coefficient δ (π, σ, or ω) is expected to be positive i.e. it will raise house prices above what is predicted by factor prices alone.

3.2 Quality of life index

Increasing the cost of housing is not the intended effect of conservation areas. Rather they reduce housing productivity in order to preserve or improve the attractiveness of neighbourhoods. The second stage investigates the demand side effect of conservation areas by relating the housing productivity predicted by designation to a measure of quality of life. I compute a city quality of life index according to the spatial equilibrium condition of equation (3)[59]. I then regress the index on the components of housing productivity predicted in the regression of equation (4). The regression takes the form:

$$\tilde{Q}_{jt} = \mu_1\left(-\tilde{R}_{jt}\hat{\pi}\right) + \mu_2\left(-\tilde{N}_{jt}\hat{\sigma}\right) + \mu_3\left(-\tilde{P}_{jt}\hat{\omega}\right) + \mu_4\left(-\hat{\delta}\tilde{D}_{jt}\right) + \mu_5\left(-\varepsilon_{jt}\right) + \varepsilon_{jt} \quad (6)$$

where μ_1-μ_5 are the parameters to be estimated. Specifically μ_4 tells us how differences in housing productivity predicted by different levels of designation are associated with differences in quality of life. If conservation areas make areas more attractive then we expect this parameter to be negative. It is important to estimate this equation controlling for other amenities that impact on quality of life and may be correlated with housing productivity. The overall welfare effect can then be interpreted from the parameter estimates in the two stages of this approach in a way that is explained in the results section.

It is important to acknowledge the potential mechanical link between the quality of life index which includes the price index (minus wages) and the determinants of housing productivity that are also components of the price index (minus factor

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[59] For robustness, I create two separate quality of life indices, one using equation (3) and the other using an alternative concept of housing costs. These two measures are constructed in the data section below.

costs). If the theoretical model holds, however, this mechanical link should not exist. Higher prices due to unobserved demand factors will show up in the quality of life index but not the productivity residual since land prices will be higher to maintain zero profits. Higher prices (for given input prices) due to unobserved supply factors will show up in the productivity residual but not the quality of life index since wages will compensate for price differences spatial equilibrium.

3.3 Identification issues

There are three important problems with this strategy as it stands.

Unobservable productivity factors

Firstly, unobservable productivity factors in ε_{jt} may bias the estimates in equation (4). If, for example, soil quality is an important determinant of housing productivity then good quality soil for building will be associated with lower house prices. If this soil quality is unobserved and correlated with designation (a correlation is plausible if historical cities were built on good soil) then it will bias the estimate of δ downwards. An upwards bias could be the result of, for example, congestion in historical centres than lower housing productivity. Further, according to the model, omitted productivity factors are capitalised into land values leading to a *necessary* bias for the land cost share. Going back to the example of soil quality increasing housing productivity, this will both lower house prices and increase land values to maintain zero profits leading to a bias. This is problematic since the land cost share, and the elasticity of substitution are interesting parameters in their own right and are ideally estimated without bias. Furthermore, this implies that only the observed components of $\tilde{A}_J^Y$ in equation (5) are reliable since the residual part will be contained in the estimate of the land cost share.

An IV strategy is employed by Albouy and Ehrlich (2012) to address this first concern. They find plausible instruments for variation in land values (inverse distance to saltwater coast and mean winter temperature) that are exogenous to housing productivity. Such an instrument for land values is particularly important in their paper since they wish to estimate the total housing productivity $\tilde{A}_J^Y$ including the unobserved factors, which would otherwise be captured in the endogenously determined land values. Given that I wish to investigate specifically the housing productivity effect of observed designation this is a lesser concern. Furthermore, the IV approach has a number of problems. Firstly, the exogeneity is in doubt if the instruments are correlated with unobserved geographic factors that affect housing productivity such as if distance to coast were correlated with soil quality. Secondly, the exclusionary restriction is violated if the instruments directly affect housing productivity such as if it were harder to build in cold temperatures[60]. Thirdly, it is very difficult to find plausible instruments for all the endogenous variables. No instruments could be found by Albouy and Ehrlich (2012) for the regulatory restrictiveness of cities. Finally, the IV approach does not help with the next two problems.

Unobservable housing characteristics

Secondly, unobservable housing characteristics contained in the 'standardised' unit price of housing may bias the estimate. The standardised house prices are created using hedonic regression on housing characteristics and city-level indicator variables (see data section below). This entails that if there are unobserved housing characteristics that are typical to a certain city, they will not be removed from the hedonic regression and will be contained in the city price. If, for example, the quality of architecture is unobserved and varies across cities then this will be captured in the city price of housing. If this is correlated with designation (highly plausible) then designation may appear to increase house prices when in fact it does not.

Unobservable quality of life factors

In the quality of life regression there may be factors correlated with designation that are not captured in the control variables and hence bias the effect. Since a very wide range of amenities has been demonstrated to impact on quality of life

[60] This particular problem is noted by Albouy and Ehrlich (2012).

indicators, there are many potential sources of bias. Therefore the use of control variables is limited in the extent to which it can eliminate bias.

Fixed effects estimation as a solution

Since the IV approach is problematic, I propose the implementation of a fixed effects model to address the three empirical issues outlined above. By adding city fixed effects to equation (4) the parameters are estimated using only time-variation for each city.

$$\begin{aligned}\Delta\tilde{p}_{jt} = \beta_1\Delta\tilde{r}_{jt} + \beta_2\Delta\tilde{v}_{jt} + \beta_3\Delta\left(\tilde{r}_{jt}\right)^2 + \beta_4\Delta\left(\tilde{v}_{jt}\right)^2 + \beta_5\Delta\left(\tilde{r}_{jt}\tilde{v}_{jt}\right) + \pi\Delta\tilde{R}_{jt} \\ + \sigma\Delta\widetilde{N}_{jt} + \omega\Delta\tilde{P}_{jt} + \delta\Delta\widetilde{D}_{jt} + \Delta f_j + \Delta u_{jt}\end{aligned} \tag{7}$$

where the difference operator Δ signifies the difference from the within-city mean i.e. the average over the time observations and f_j are fixed unobserved factor that affect housing productivity in city-j. Since $\Delta f_j = 0$ this helps deal with the first problem if unobserved factors that impact housing productivity such as soil quality are fixed. Time variant unobservable factors that impact on housing productivity remain a problem however. This means it will be important to control for the underlying regulatory restrictiveness in $\tilde{R}_{jt}$ for each city so that the effect of this is not confused with an effect of designation if the two are correlated over time. The second problem is also dealt with if unobservable housing characteristics are fixed. Given that I look at a time period of only one decade I expect that the average characteristics of the housing stock at the city level to be approximately fixed. This should ensure that time variation in standardised house prices is predominantly due to change in the actual unit value of housing rather than changes to unobserved structural characteristics.

Further by adding fixed effects to equation (6) the quality of life effects are estimated from only time variation in quality of life and designation:

$$\begin{aligned}\Delta\tilde{Q}_{jt} = \mu_1\Delta\left(-\tilde{R}_{jt}\hat{\pi}\right) + \mu_2\Delta\left(-\widetilde{N}_{jt}\hat{\sigma}\right) + \mu_3\Delta\left(-\tilde{P}_{jt}\widehat{\omega}\right) + \mu_4\Delta\left(-\hat{\delta}\widetilde{D}_{jt}\right) \\ + \mu_5\Delta\left(-\varepsilon_{jt}\right) + \Delta q_j + \Delta\varepsilon_{jt}\end{aligned} \tag{8}$$

where the difference operator Δ again signifies the difference from the average over all time periods for each city and q_j are fixed unobserved factors that affect quality of life in city-j. Demeaning eliminates all fixed factors that affect quality of life. This represents an important empirical step since for a lot of these factors there will be far more variation over locations then there will be over a decade of time within a location. However, time variant unobservables remain a problem. Whilst FE estimation of both stages represents a probable improvement over the IV approach, a major drawback is the requirement of panel dataset with sufficient time variation in each city. This may be difficult to obtain for most variables due to data availability. Therefore, critical to this research is the construction of a panel dataset of land values, house prices, construction costs and designation presented in the next section.

4. Data

The empirical analysis is conducted at the housing market area (HMA) level[61]. A map of these areas is presented in the appendix. Unlike other urban area definitions, HMAs are rigorously defined to separate individual housing markets and are therefore considered a suitable empirical counterpart to the theoretical j-locations. The HMA boundaries are defined based on evidence from patterns of commuting, migration and house prices. As such they typically approximate recognisable city regions. The study period is 1997-2007 since this represents the greatest period of overlap of the different data. The final panel dataset, therefore, has T = 11 and N = 74. This dataset is a longer and narrower panel than that used by Albouy and Ehrlich (2012) and hence is more fitted to the implementation of a fixed effects model.

[61] In particular, I make use of 'strategic' rather than 'singular' HMAs since the former defines whole housing market areas whereas the latter defines housing markets sub-areas. I also make use of the 'silver standard' definition which sacrifices some detail in order to be more easily aggregated from smaller geographical units, such as the local authority district (LAD), which much of the data in the analysis are available on. The map in the appendix shows how these areas relate to LADs.

4.1 House prices and factor prices

House prices ($\tilde{p}_{jt}$)

House prices for 1,087,896 transactions in England over the period 1995-2010 come from Nationwide, the largest building society in the UK. In addition to the price paid, the data has property characteristics including postcode location, which is used to identify which HMA the transacted unit belongs to. All transactions that are 'leasehold' are dropped from the data since the price of these properties should not come into the quality of life index[62]. Following the empirical approach of Albouy and Ehrlich (2012) the house price index is computed by regressing the log of the transaction price p for unit i in HMA j and year t on a vector of property characteristics X_{ijt} and a set of HMA-year indicator variables:

$$p_{ijt} \ = \ X_{ijt}\beta \ + \varphi_{jt}\left(HMA_j \times YEAR_t\right) + \epsilon_{\ ijt} \tag{9}$$

The house price index is then constructed taking the predicted HMA-year effects $\hat{\varphi}_{jt}$ and subtracting the national average in each year, i.e. $\tilde{p}_{jt} = \hat{\varphi}_{jt} - \bar{\hat{\varphi}}_t$. As discussed earlier, one particular worry is if there are unobserved property characteristics in the error term that are correlated with the HMA-year effects. In this case the price differential may mistakenly be attributed to housing productivity differences (or quality of life differences) when it simply reflects differences in for example, architectural quality. This is of special significance when identifying the effects of conservation areas which will very likely correlate with certain unobservable housing characteristics. This is a particularly important motivation for employing a fixed effects strategy. A further potential problem highlighted by Albouy and Ehrlich (2012) is that the distribution of observed transactions within each HMA-year may differ from the actual

[62] The rationale here is that the spatial equilibrium in the housing market is the result of free movement of homeowners. The free movement of renters may deliver spatial equilibrium as well but it would be more direct to examine rents in this case rather than house prices of leasehold properties.

distribution of housing stock in the HMA. Therefore, I apply a population weight to the above regression. Each observation is weighted by the LAD dwellings count in 2003 divided by the LAD-year transaction count[63]. The results of this hedonic regression and a brief discussion of the coefficients are presented in the appendix.

Land values ($\tilde{r}_{jt}$)

Residential land values are obtained from the Valuation Office Agency (VOA). The residential land values are produced for the Property Market Report which has been released biannually since 1982. Land values for the full set of local authority districts (LADs) were, however, not made available until 2014 when they were placed online following my requests for the data. As such the full dataset has never previously been used in empirical analysis. The values are assessed for small sites (<2ha), bulk land (>2ha) and flat sites (for building flats) for vacant land with outline planning permission. The three different site categories have approximately the same value in each LAD therefore I use only small sites since this category has no missing values in any year for any LAD. Due to a reorganisation of local government in England some districts were merged together between 1995 and 1998 (but most were unaffected)[64]. Reflecting these adjustments I converted the data from the earlier definition to the current definition. I then took the mean of the biannually reported land values and

[63] The dwelling stock numbers are available from 2001-2011 for Output Areas from the Department for Communities and Local Government. Since this covers only part of the study period of this analysis I simply use the dwelling stock from 2003, in the middle of the sample period. Furthermore, I reweight the distribution at the LA level, rather than the much finer OA because other data (e.g. land values) are only available on the LA level and all the data should be weighted in the same way. Since there are only around 5 LAs per HMA in England this represents a fairly crude reweighting of the distribution but is the finest level possible. The main estimations reported are also conducted with no weights applied to any of the variables and the results are not changed significantly (see appendix).

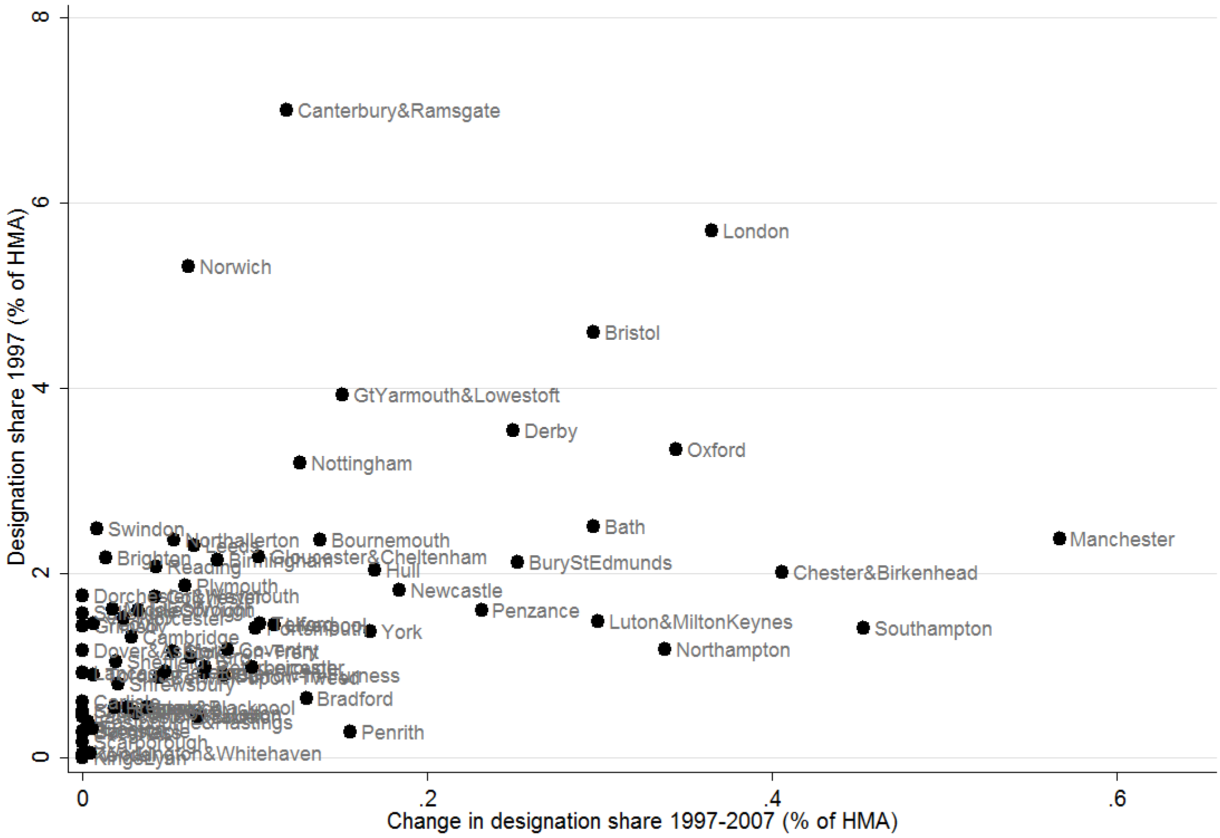
[64] Of the original 366 original districts, 21 were merged into 9 new districts, making the new total 354 districts.

aggregated to the HMA level, again using the distribution of housing stock in 2003 as weights. Finally I normalised by subtracting the national average in each year.

Construction costs ($\tilde{v}_{jt}$)

In order to capture the costs of non-land inputs to construction an index of rebuilding costs was obtained from the Regional Supplement to the Guide to House Rebuilding Cost published by the Royal Institute of Chartered Surveyors (RICS). Rebuilding cost is an approximation of how much it would cost to completely rebuild a standard unit of residential housing if it has been entirely destroyed. This takes into account the cost of construction labour (wages), materials costs, machine hire etc. and is considered to be an appropriate measure of the price of non-land inputs to housing. The data is based on observed tender prices for construction projects and the sample size of tenders is given with each factor. I make use of location adjustment factors that are available in annually from 1997-2008 at the LAD level and take into account the local variations in costs. To my knowledge this data has not been used before in empirical analysis at this level of detail. The location factors were scanned from hard copies and digitised using Optical Character Recognition (OCR) software. The separate years were then matched to form a panel dataset. Some districts were missing from the data, especially in the earlier years. However, a higher tier geography (corresponding in most cases with counties) was recorded completely enabling a simple filling procedure described in the appendix. In short though, the county factor and sample size is compared with factors and sample size for the available districts in that county in order to impute the values for the missing districts. These data were subject to the same district boundary changes as with the land value data and were corrected in the same way. Finally, the filled district level data was aggregated to HMA weighted by dwelling stock and then normalised as before.

Figure 2: Initial designation share against change for housing market areas (HMAs)



4.2 Conservation area designation ($\widetilde{D}_{jt}$)

In order to identify the impact of conservation area designation on housing productivity and quality of life a Geographic Information Systems (GIS) map of conservation areas (CAs) was obtained from English Heritage. This is a polygon dataset that precisely maps the borders of all CAs in England and has only been used once before in empirical analysis by Ahlfeldt et al. (2014a). The data include the date of designation, which lies between 1966 and 2011. Using this information I calculated in each year the share of land in each HMA that was covered by CAs. Figure 2 plots the initial designation share in 1997 against the change in share over 1997-2007. The chart clearly shows significant variation in both the initial share and change over the period. Blackburn & Burnley HMA is not depicted since the change in designation share over the period is 'off the chart' at 2.6% of the land area. The CA designation share is first computed at the LAD level in order to be aggregated to HMAs weighted by dwelling stock, ensuring all the data are produced comparably. The logged land shares are then normalised to have a mean of zero and a standard deviation of one. This is achieved by taking log-differences around the national average and then dividing by the standard deviation in each year. Such 'z-values' are created for each of the housing productivity factors to ensure the effects on log costs are comparable across each component. The estimated parameters after normalisation give the effect on log costs of a one standard deviation increase in that factor.

4.3 Regulatory restrictiveness ($\tilde{R}_{jt}$)

Planning refusal rates

In order to control for the underlying regularity restrictiveness in each city, the share of planning applications that are refused in each year from 1997-2007 was obtained. A more geographically detailed version of this data was first used by Hilber and Vermeulen (In Press) to analyse the effect of planning restrictiveness on housing costs in England. The authors kindly agreed to share their data for use in the current paper. The HMA level data were aggregated from Local Authority level (weighted by dwelling stock). The variation in refusal rates is volatile over time and only a small part of year-to-year variation is thought to represents actual changes in planning restrictiveness. The data were therefore smoothed in order to eliminate the short-term noise whilst keeping the long run trends in planning restrictiveness. This was done by estimating a quasi-probit regression of refusal share on a time trend (see appendix). The predicted refusal rates from this trend regression are used in the empirical analysis after normalising to z-scores.

Protected land

In order to control for other protected statuses that impact on housing productivity, GIS polygons were obtained for the following protection statuses: Greenbelt, National Parks, Areas of Outstanding Natural Beauty, Sites of Specific Scientific Interest, National Nature Reserves and Registered Common Land. These spatial data were obtained from the University of Edinburgh (Greenbelt) and Natural England (everything else). The share of land in each HMA that falls under any one of these protected statuses was computed using GIS. The resulting protected land shares were weighted with dwelling stock and z-values were computed. Notably, there is no time variation in these designations, therefore, they are used only in the preliminary cross-sectional regressions.

4.4 Natural constraints ($\widetilde{N}_{jt}$)

Undevelopable land

In order to control for geographic factors that may influence housing productivity I follow Saiz (2010) in constructing a measure of geographical constraints based on entirely natural factors. I compute the developable share of land within 25-km of each HMA centroid[65]. Developable land is defined as land that is flat (< 15 degree slope) and dry (solid land covers). To calculate the slopes I use the OS Terrain 50 topography dataset which is a 50m grid of the UK with land surface altitudes recorded for the centroid of each grid square. I calculate the slope in the steepest direction for each grid square and if this is greater than 15 degrees then the 50m grid square is also defined as undevelopable. To identify dry land I use The Land Cover Map 2000, which is a 25m grid for the whole of Great Britain where each square is assigned to one of 26 broad categories of land cover. The grid square is defined as undevelopable if it is water, bog, marsh etc. The final developable land share is computed for each HMA as the total land area that not undevelopable divided by the total area in the 25-km circle. Finally, z-scores are computed but the shares are not weighted since they are intended to be entirely exogenous.

[65] Saiz (2010) uses 50-km circles around U.S. MSA centroids – whereas I define 25-km circles to adjust for the smaller size of English HMAs. The average area of a U.S. MSA is about 7,000 km^2, the area of circle of a radius of around 50-km. This may be the reasoning behind Saiz's choice of radius. Since the average HMA in England is about 1,800 km^2, an appropriately sized circle would have a radius of about 25-km.

4.5 City population ($\tilde{P}_{jt}$)

Population density (z-scores)

To account for agglomeration economies or congestion that may impact on housing productivity, either positively or negatively, I obtained population data for 2004 at the local authority level from NOMISWEB. These were aggregated to HMA (without weights) and divided by the land area to reach population densities. Finally z-scores were computed.

4.6 Quality of life ($\tilde{Q}_{jt}$)

Quality of life index

I construct two alternative quality of life indices. The first is most closely related to equation (3) and computed as follows:

$$QoL_{jt}^{1} = 0.31 \times \tilde{p}_{jt} - (1 - 0.225) \times 0.64 \times \tilde{w}_{jt} \qquad (10)$$

where 0.31 is the share of expenditure on housing, which comes from the Expenditure and Food Surveys (EFS) 2001-2007. The same price differential $\tilde{p}_{jt}$ is used as in the first stage, computed via hedonic regression. The annual wages $\tilde{w}_{jt}$ comes from the Annual Survey of Hours and Earnings at the local authority level and are aggregated (weighted by the number of jobs) to HMAs before taking log differences. Unlike the price data, the wage data has not been adjusted for characteristics. Hence city differences in wages may be due to different personal characteristics or a differential occupational or industrial composition rather than any effect of place. Controlling for these factors, therefore, represents an area for improvement[66]. The marginal income tax rate of 0.225 was computed using data from the HM Revenue and Customs for 2005/05 and the share of

[66] Gibbons et al. (2011) overcome this problem by identifying individual and city effects from movers. However, I require time variation in the wages variable so this is probably not a viable approach. Rather I would simply control for observable characteristics using the ASHE dataset on wages.

income from wages of 0.64 is from the Department for Work and Pensions for 2005/06[67]. Gibbons et al. (2011) note that the above measure assumes a constant expenditure share on housing which may not be the case across different locations in reality. Therefore they propose a number of other measures that aim to compute actual housing costs from house prices. One of these is the interest-rate method which I compute according to:

$$h_{jt} = \left[lv_t p_{jt} i_t + (1 - lv_t) p_{jt} s_t\right](1 + 0.19) + ctax_{jt} + stamp_{jt} \qquad (11)$$

where lv_t is the loan-to-value ratio in year t, p_{jt} is the standardised house price[68] in HMA j and year t, i_t is the standard variable rate of interest on mortgages in year t and s_t is the interest rate on savings in year t (interest data are available from the Bank of England), 0.19 is the maintenance and transaction costs reported in the EFS as a fraction of mortgage costs (in square brackets), $ctax_{jt}$ is council tax and $stamp_{jt}$ is stamp duty as in Gibbons et al. (2011). The second quality of life index is computed as:

$$QoL^2_{jt} = \frac{\Delta h_{jt} - \Delta d_{jt}}{1.7} \qquad (12)$$

where the difference operators Δ represent the difference (not logged) from the national average in each year and 1.7 is the average number of workers per household from the EFS. The city ranking for both of these quality of life indices is presented in the appendix.

Amenities

The above indices will be used to relate housing productivity (from designation) to quality of life. However, it is important to control for other factors. Therefore I obtain an array of environmental amenities and locational factors that may

[67] Notably these shares are UK averages but breakdowns for only homeowners (which would likely be higher) were not available from these sources.

[68] The house price is not a differential as before but the predicted price a property with average national characteristics located in each HMA-year.

influence quality of life. These are listed in Table 1 below and come from a variety of sources including OpenSteetMap, the Land Cover Map and the UK Census. These data were initially merged with the housing unit (Nationwide) dataset and then are collapsed to the HMA mean across all years. This means they capture the incidence of amenities on the actual distribution of the dwelling stock. For example, it is more meaningful to know the average distance to a lake for housing units in an HMA than the average distance to a lake from all points in an HMA.

Table 1: Summary statistics for amenities

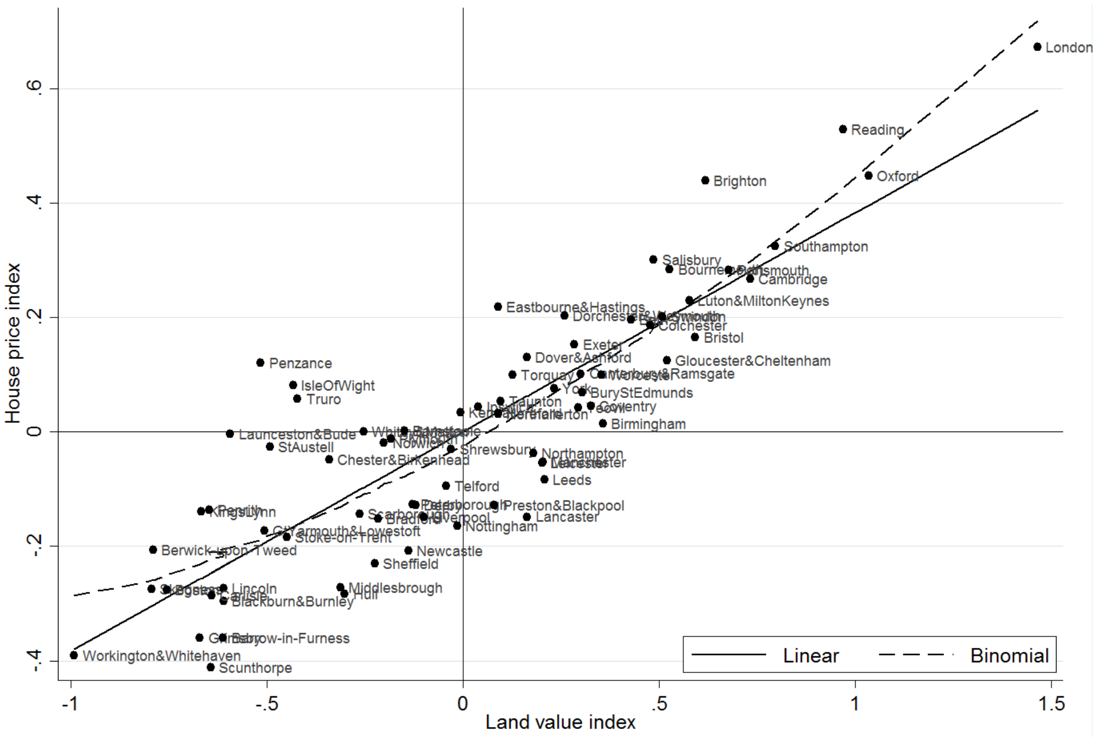
Variable	N	Mean	SD	Min	Max
Employment potentiality	74	0.05	0.05	0.00	0.40
Distance to rail station	74	3.53	1.79	1.04	9.68
Distance to airport	74	28.21	15.90	8.48	73.09
Cafes (kernel density)	74	0.19	0.19	0.01	1.06
Food establishment (kernel density)	74	0.55	0.42	0.02	2.37
Bar (kernel density)	74	0.96	0.54	0.16	2.69
Museum (kernel density)	74	0.03	0.03	0.00	0.21
Theatre (kernel density)	74	0.05	0.03	0.00	0.13
National Park (kernel density)	74	1.88	5.05	0.00	29.38
Distance to Lake	74	6.05	3.11	0.98	16.89
Distance to River	74	1.02	0.54	0.52	3.37
Distance to Coastline	74	18.59	19.95	0.95	77.37
Mountains, moors, and heathland (land share)	74	0.00	0.01	0.00	0.03
Semi-natural grasslands (land share)	74	0.08	0.03	0.02	0.16
Broad-leaved/mixed woodland (land share)	74	0.05	0.02	0.01	0.13
Urban (land share)	74	0.49	0.11	0.23	0.75
Gardens (land share)	74	0.21	0.06	0.04	0.33
Greenspace (land share)	74	0.53	0.12	0.28	0.84
Water (land share)	74	0.02	0.02	0.01	0.12
Key Stage 2 score (IDW)	74	27.27	0.45	25.24	28.16
Income 2005	74	24.26	2.87	19.88	33.96
Ethnicity Herfindahl index	74	0.90	0.06	0.61	0.97

5. Results

5.1 Housing cost function

Figure 3 illustrates a cross-sectional (linear and binomial) regression of mean house prices $\bar{\tilde{p}}_J$ on mean land values $\bar{\tilde{r}}_J$ i.e. the average differential for across all years for each HMA. The binomial regression is a simplified version of equation (4), which using the corresponding parameters is: $\bar{\tilde{p}}_J = \beta_1 \bar{\tilde{r}}_J + \beta_3 \left(\bar{\tilde{r}}_J\right)^2$. The slope of the linear trend would suggest $\phi_L = \beta_1 = 0.380$. The binomial slope is convex ($\beta_3 = 0.093$) suggesting an elasticity of substitution less than one. Specifically, it is $\sigma^Y = 0.220$.[69] However, since land values are likely correlated with construction costs and other factors these estimates are biased.

Figure 3: house price index vs. land value index for English HMAs



[69] This is computed from the biased estimates as $\sigma^Y = 1 - \frac{2\beta_3}{[\beta_1(1-\beta_1)]} = 1 - \frac{(2\times0.093)}{[0.372(1-0.372)]}$

The estimates in Table 2 are for the housing production function of the cross-sectional model in equation (4) using the weighted versions of the variables (the unweighted models presented in the appendix illustrate robustness with respect to weightings). The first two columns present the Cobb-Douglas results (restricted and unrestricted models) and the last two columns present the translog results. Across the four different models, the land cost share varies between 0.25 and 0.29, which is smaller than the 0.35-0.37 estimated for U.S. cities (Albouy & Ehrlich, 2012). The elasticity of substitution is estimated to be 0.373 in the restricted translog log model which is very similar to the 0.367 for the same model for the U.S. (Albouy & Ehrlich, 2012). This result suggests that developers in England face a similar degree of substitutability of inputs as developers in the United States. Since the elasticity of substitution is less than one, an increase in the relative price of either factor is accompanied by an increased expenditure on that factor i.e. the factors are gross complements. Across all these cross-sectional models, the relationship between designation and house prices is positive but insignificant suggesting that heritage conservation does not lower housing productivity significantly. For other protection statuses (such as National Parks, AONB, etc.) and for planning restrictiveness (as proxied by predicted refusal rates) the effect is positive (between 0.03 and 0.04) and significant. The effect for planning is slightly larger at around 0.04-0.05, meaning a standard deviation increase in planning refusals is associated with a 4-5% increase in house price. The Saiz undevelopable land share is small, positive and insignificant, suggesting that, in contrast to the U.S., natural factors may not play an important role in determining housing productivity in England. Finally, population density has a negative coefficient that is insignificant. This insignificance could be because population density is expected to have both positive and negative effects due to agglomeration economies or congestion. In terms of model selection, the Cobb-Douglas restriction is rejected in both columns (1) and (2). I choose to proceed with the restricted translog model since this is the functional form assumed in the theory, even though the CRS restriction is rejected in the translog model in column (4). This is also justifiable given the results of interest do not differ greatly across models.

Table 2: Cross-sectional cost function

	(1)	(2)	(3)	(4)
	Dependent variable: house price differential			
Land value differential	0.271***	0.287***	0.251***	0.285***
	(0.024)	(0.024)	(0.025)	(0.023)
Construction price differential	1.128***	0.713***	1.124***	0.715***
	(0.193)	(0.024)	(0.167)	(0.023)
Conservation area land share (z-score)	0.010	0.009	0.008	0.011
	(0.014)	(0.014)	(0.013)	(0.014)
Protected land share (z-score)	0.033**	0.034**	0.038***	0.034**
	(0.014)	(0.014)	(0.014)	(0.014)
Predicted refusal rate (z-score)	0.042***	0.046***	0.044***	0.045***
	(0.011)	(0.012)	(0.011)	(0.011)
Undevelopable land share <25km	0.004	0.009	0.005	0.009
(z-score)	(0.011)	(0.011)	(0.010)	(0.011)
Population density (z-score)	-0.016	-0.014	-0.013	-0.015
	(0.016)	(0.016)	(0.015)	(0.015)
Land value differential squared			0.019	0.064***
			(0.020)	(0.021)
Construction price differential squared			-6.763***	0.064***
			(2.173)	(0.021)
Land value differential			1.119***	-0.128***
x Construction price differential			(0.342)	(0.042)
Constant	-0.000	-0.000	-0.001	-0.016*
	(0.009)	(0.009)	(0.010)	(0.009)
R^2	0.812	0.806	0.831	0.817
AIC	-1454.8	-1429.5	-1533.1	-1474.2
Numbers of HMAs	74	74	74	74
Observations	814	814	814	814
p-value for CRS		0.033		0.001
p-value for CD	0.000	0.000		
p-value for all restrictions		0.000		
Elasticity of substitution	1.000	1.000		0.373

The estimates in this table are for the regression of equation (4). The EoS is $\sigma^Y = 1 - 2\beta_3/[\beta_1(1-\beta_1)]$. Standard errors in parentheses are clustered on HMAs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As described in the empirical strategy the estimates from this cross sectional approach are likely to be biased either by unobserved housing characteristics captured in the price differential or unobserved factors that influence productivity. Table 3 presents the results from the fixed effects model in equation (7) where bias from fixed unobservables is removed. Across all four specifications the land cost share varies between 0.14 and 0.15 and the elasticity of substitution is 0.11 in the restricted translog model. Again, CRS is rejected at the 1% level. The relationship between designation and house prices is larger in the fixed effects model at around 0.07-0.09 and is now significant. A standard deviation increase in the designated land share, an increase of 0.013, increases house prices by around 7-9%. This represents the main result of the cost function. The coefficient for regulation is much lower at just over 0.01 and statistically insignificant. The other protected statuses as well as population density have dropped out since they are fixed over time.

Table 3: Fixed effects cost function

	(1)	(2)	(3)	(4)
	Dependent variable: house price differential			
Land value differential	0.141***	0.140***	0.150***	0.151***
	(0.020)	(0.020)	(0.017)	(0.017)
Construction price differential	0.541***	0.860***	0.531***	0.849***
	(0.115)	(0.020)	(0.103)	(0.017)
Conservation area land share (z-score)	0.068***	0.083***	0.071***	0.085***
	(0.021)	(0.020)	(0.023)	(0.021)
Predicted refusal rate (z-score)	0.011	0.011	0.013	0.012
	(0.008)	(0.008)	(0.008)	(0.008)
Land value differential squared			0.057***	0.057***
			(0.012)	(0.013)
Construction price differential squared			-1.483	0.057***
			(1.139)	(0.013)
Land value differential			-0.178	-0.114***
x Construction price differential			(0.208)	(0.026)
R^2	0.953	0.952	0.956	0.955
AIC	-2587.0	-2574.0	-2632.7	-2619.6
Numbers of HMAs	74	74	74	74
Observations	814	814	814	814
p-value for CRS		0.006		0.006
p-value for CD	0.000	0.000		
p-value for all restrictions		0.000		
Elasticity of substitution	1.000	1.000		0.110

The estimates in this table are for the regression of equation (7). The EoS is $\sigma^Y = 1 - 2\beta_3/[\beta_1(1-\beta_1)]$. Standard errors in parentheses are clustered on HMAs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The fact that the estimated land cost share has dropped is most likely due to unobservable housing characteristics since factors such as high architectural quality are expected to lead to higher land values due to externalities. If they are unobserved and captured in the standardised house price then this would lead to an upwards bias. If the fact that designation now has a *larger* effect is to do with housing characteristics then it is because negative factors are correlated with designation, perhaps disrepair. Since we would typically assume designated areas to be associated with desirable property characteristics it is more likely that fixed unobserved housing productivity factors were the source of bias in the cross-sectional regression. That is, cities with lots of conservation areas have fixed factors that increase housing productivity. This is very plausible if there are unobserved environmental conditions that are amenable to development and drove the location of historic settlements.

Interpreted one way, the fact that refusal has becomes insignificant could suggest that the positive effect before was due to unobserved housing characteristics. However, if this were true it is unlikely that we would see such a drastically different story for designation. It seems more likely that the noisiness of the refusals data means it is not possible to identify an effect from time variation alone. It is also possible that the actual restrictiveness of LADs varies more systematically over areas than over time. To this extent, no great attention should be paid to the estimates for refusals, and it should be rather considered as simply an important control. Finally, the elasticity of substitution is much lower in the fixed effects model suggesting that there is very low substitutability of inputs witnessed in the time series variation over the 11-year period. This is what we would expect to see if it takes developers time to adjust their construction methods (i.e. to substitute) in response to significant changes to the relative prices of inputs. For example developers may be specialised in constructing taller buildings in London where they are used to high land prices relative to non-land prices. So compared with a cheaper-land city the price differential may not be so great. However, if a single town changes from low land prices to high land prices the price change is likely to be much larger since developers may continue for some time building low rise units. This could be because of either the lag between buying land and selling the house, the time it takes to shift methods (different skills, materials, etc.) or time to adjust for the planning regime.

To recap, the estimated effect for a standard deviation increase in designation is a 7-9% increase in house prices. In the next section I investigate whether the quality of life effect of designation outweighs this increase in housing costs.

5.2 Quality of life and conservation areas

In the next step I regress housing productivity as predicted from the above cost function on the two indices for quality of life. Table 4 presents the cross sectional estimates i.e. equation (5). All specifications include the controls for environmental amenities discussed in the data section and the full estimates are reported in the appendix. This model takes the predicted housing productivity from the cross-sectional version of the cost function estimated above. Columns (1) and (2) present the estimates for the constant share quality of life index and column (2) for the interest-based measure. In column (1) a one point increase in total housing productivity is associated with a 0.349 point decrease in the quality of life index, which is in expenditure equivalent units. Since a one point reduction in housing productivity corresponds to a 0.32 point increase in expenditure (i.e. the expenditure share on housing) and a 0.349 increase in expenditure equivalent quality of life, this implies that policies that reduce productivity are welfare improving. A policy that decreases housing productivity by one standard deviation will increase welfare by an amount equivalent to 4% of expenditure ($0.349 - 0.31 = 0.039$). It is reasonable to imagine that the each housing productivity factor impacts have different effects on quality of life. Therefore, in column (2) I use as regressors the constituent elements of housing productivity as predicted by city characteristics. Broken down this way, housing productivity predicted from designation has a much larger impact on quality of life than the aggregate indicator. This may not be necessarily surprising considering in light of the fact that conservation areas preserve positive externalities in addition to preventing negative externalities. A standard deviation increase in designation is equivalent to an 83% increase in expenditure. Given that designation was insignificant in the cost function estimation this would suggest that designation *only* increase quality of life[70]. Planning restrictiveness is associated with a lower quality of life impact. In fact the net effect is negative suggesting a standard deviation increase in refusal rates is equivalent to a 5.5% drop in expenditure.

[70] Given that designation has no significant effect on housing productivity in the first stage, it may seem counterintuitive to then use housing productivity predicted by designation in the next stage. In this case, $-\hat{\delta}\widetilde{D}_{jt}$ should simply be considered as a measure of designation that is simply *scaled* by the coefficient on housing costs in order that welfare comparisons are possible. Notably, the magnitude or significance of the coefficient on housing costs makes no difference to the significance of the coefficient in the second stage. It only affects the magnitude of the coefficient in the second stage in a way that makes for neat comparison with overall expenditure.

Protection statuses add to quality of life overall (equivalent to a 12.5% increase in expenditure) but since this is a mix of different types of designation it is not possible to attribute this effect to any one of them. Geographic constraints are associated with a very large increase in quality of life, quite probably because these constraints (e.g. mountains, lakes, sea) represent environmental amenities not perfectly captured by the controls. The quality of life effect from unobserved factors does not deviate to far from the aggregate effect and is overall roughly welfare-neutral.

Table 4: Cross-sectional quality of life regression

	(1)	(2)	(3)	(4)
Dependent variable:	Constant share (QoL^1_{jt})		Interest-based (QoL^2_{jt})	
Predicted housing productivity				
Total ($\tilde{A}^Y_j$)	-0.349***		-6.002***	
	(0.045)		(0.989)	
From designation ($-\hat{\delta}\widetilde{D}_{jt}$)		-1.140**		-13.827
		(0.488)		(11.630)
From planning refusals (part of $-\tilde{R}_{jt}\hat{\pi}$)		-0.255**		-2.561
		(0.100)		(2.637)
From protected (part of $-\tilde{R}_{jt}\hat{\pi}$)		-0.435***		-3.589
		(0.140)		(3.313)
From geo. constraints ($-\hat{\sigma}\widetilde{N}_{jt}$)		-2.011***		-41.281**
		(0.752)		(17.532)
From unobserved factors (-ε_{jt})		-0.320***		-5.765***
		(0.043)		(0.972)
Constant	0.101	0.023	1.070	-0.765
	(0.315)	(0.320)	(8.235)	(8.455)
Environmental Amenity controls	YES	YES	YES	YES
R^2	0.670	0.685	0.615	0.625
AIC	-2892.4	-2920.3	-2315.0	-2302.0
Observations	814	814	814	814

Cross sectional estimation – eq. (5) – with controls for environmental amenities. Full table presented in the appendix. Predicted housing productivity is taken from cross-sectional cost model – eq. (4), Table 2. Standard errors in parentheses are clustered on HMAs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The columns (3) and (4) represent a robustness check of the results in columns (1) and (2), since they do not easily lead to overall welfare estimates. Column (3) confirms the negative relationship between housing productivity and quality of life. The estimate suggests a one point increase in productivity is associated with a £6,000 decrease in expenditure. In column (4) the estimates have the same sign as in column (2), however, they are insignificantly different from zero apart from for geographic constraints. This weakens the overall result from the cross sectional regressions that designations increases quality of life (whilst having no effect on housing productivity). Furthermore, as noted in the empirical section, the cross-sectional estimates are subject to several sources of bias and are therefore unreliable.

Next in Table 5 I estimate the fixed effects model of equation (8) taking the predicted housing productivities from the fixed effects cost function estimation above. Column (1) continues to support that housing productivity has a negative relationship with quality of life. The coefficient is slightly smaller than the cross-sectional version and represents a small welfare gain. However, when broken down into constituent parts in column (2) designation and planning are both insignificant. Notably designation is only marginally insignificant with a t-statistic of -1.51 $(p > 0.136)$. Accepting this coefficient would imply that designation does increase quality of life but that the overall effect is welfare reducing, with a standard deviation of designation being equivalent to a 4% reduction in expenditure. The interest-based quality of life measure in columns (3) and (4) confirm the overall pattern that only aggregate housing productivity significantly impacts on quality of life (here a lower estimate equivalent to £4,100). The designation and planning effects in these models are both hugely insignificant.

Table 5: Fixed effects quality of life regression

	(1)	(2)	(3)	(4)
Dependent variable:	Constant share (QoL^1_{jt})		Interest-based (QoL^2_{jt})	
Predicted housing productivity				
Total ($\tilde{A}^Y_J$)	-0.320***		-4.166***	
	(0.032)		(0.938)	
From designation ($-\hat{\delta}\tilde{D}_{jt}$)		-0.268		1.379
		(0.178)		(5.105)
From planning refusals (part of $-\tilde{R}_{jt}\hat{\pi}$)		-0.392		-1.626
		(0.438)		(12.683)
From unobserved factors ($-\varepsilon_{jt}$)		-0.320***		-4.247***
		(0.036)		(1.003)
Environmental amenity controls	NO	NO	NO	NO
R^2	0.906	0.906	0.856	0.857
AIC	-3959.0	-3955.6	-1467.0	-1468.7
Observations	814	814	814	814

Fixed effects estimation – eq. (8). Environmental amenity controls are time invariant and drop out. Predicted housing productivity from fixed effects cost model – eq.(7), Table 3. Standard errors in parentheses are clustered on HMAs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To summarise the results, the cross-sectional models suggest that conservation areas do not increase housing costs significantly and are associated with large welfare gains. However, these estimates are likely to be subject to a significant bias. The fixed effects model eliminates the bias from time invariant unobservables and suggests the opposite result. Here designation is associated with large increases in housing costs and the quality of life benefits are neither not statistically significant nor large enough in magnitude to outweigh the costs due to lower housing productivity. An alternative explanation for the difference between the cross-sectional and fixed effects results is that the fixed effect model only examines recent designations that occurred between 1997 and 2007. The cross sectional model examines the effect of all designations since the policy was introduced in 1967. It may be that the earlier designations provided the most value in terms of quality of life improvements. This is, in fact, similar to the model presented in Ahlfeldt et al. (2014a) where the planner designates the areas with the most heritage first.

6. Conclusions and areas for improvement

This paper has provided the first evidence on the net effect of conservation area designation on economic welfare. The results suggest that designations (at least those between 1997 and 2007) may lead to higher expenditure on housing that is not outweighed by any benefits to quality of life. In fact the benefits are found to be statistically insignificant. Before coming to any strong conclusions or policy recommendations, it is important to acknowledge these results are preliminary. The following areas are to be improved in ongoing research. Firstly, the wages that go into the quality of life measure must control for individual characteristics. This would change the quality of life results. Secondly, the protected statuses should be broken down and their effects examined individually in the cross-sectional regression. The impact of Green Belts for example may be quite different to the impact of National Parks. Thirdly, the empirical model should be adapted to allow for factor non-neutral productivity factors. Providing that the results are robust to these important empirical steps, the conclusion would be that there is too much conservation area designation. This does not imply that there is no requirement for designation at all, but rather, that they are being applied excessively and should be relaxed to enable more development.

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APPENDIX TO CHAPTER V

1. Introduction

This appendix complements the main paper and is not designed to stand alone or as a replacement. Each section provides additional material on the section from the main paper with the same section number. As such there is no section 2 or section 3 since there is no additional theoretical or empirical is this appendix. Section 4 complements the data section from the main paper providing extra detail on their sources and how they are processed. Section 5 complements the empirical results section of the main paper by showing the results of a variety of robustness tests and model alterations not reported in the main paper for brevity.

4. Data

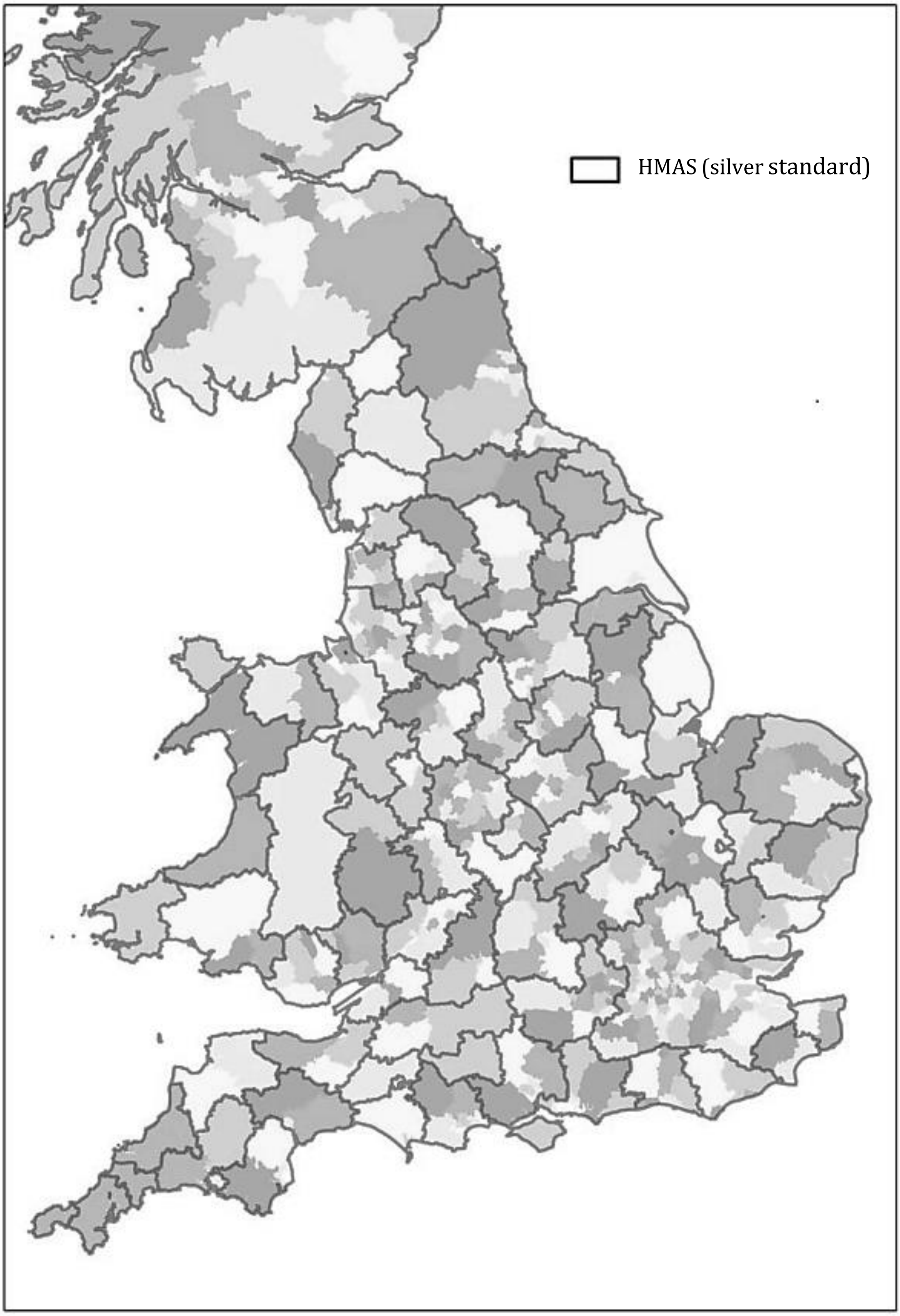
4.1 Housing market areas (HMAs)

Figure 1 illustrates the HMAs for England with a solid black outline and how they aggregate up from the (multi-coloured) local authority districts. Note: this is Map P11.4 from 'Geography of housing market areas' by DCLG (2010).

4.2 Hedonic regression

Table 1 below present the results of the hedonic regression of equation (9) from the main paper. The 1,184 MSA-year effects themselves are omitted to save space. The coefficients on the property characteristic are significant and in line with expectations. Most interesting are the results for building age and build year. Houses built during historical periods are associated with a higher price, in particular those built pre 1900, which are 36% more expensive than houses built post-2000. Houses built between 1910 and 1939 are also associated with very large premia of 21%-24%. The lowest premium is observed for houses built in the 1970s.

Figure 1: Housing markets areas over original local authority districts



Since the data cover a period of 15 years it is possible to identify both age and build year separately. Given that the general trend is for earlier build dates to have higher prices, one might expect house age to be positively correlated with price as well. However, the opposite is true. After controlling for build date, which captures the effect of architectural styles and build materials associated with a particular period, the effect of ageing is to lower the housing value. This ageing penalty is incurred fairly linearly with age up until about 100 years when it begin to reverse. Houses with 90-99 and over 100 years are less valuable than new houses (controlling for build date) but more valuable than houses of 80-89, 70-79 and even 60-69 years. This could be attributed to the effect of an accumulation of 'character' over the years which begin to really set in at around 90 years.

Table 1: hedonic regression of house prices on characteristics and HMA-year effects

	ln (price)
Number of bathrooms	0.009**
	(0.004)
Number of bedrooms	0.093***
	(0.007)
Number of bedrooms × Number of bedrooms	-0.009***
	(0.001)
House type: Detached house	-0.032
	(0.026)
House type: Semi-detached house	-0.157***
	(0.023)
House type: Terraced house/Country cottage	-0.251***
	(0.021)
House type: Bungalow	0.052**
	(0.023)
Floorsize (m^2)	0.008***
	(0.000)
Floorsize (m^2) × Floorsize (m^2)	-0.000***
	(0.000)
New property	0.070***
	(0.004)
Parking: Single Garage:	0.106***
	(0.010)
Parking: Double garage	0.163***
	(0.016)
Parking: Parking space	0.050***
	(0.010)
Central heating	0.133***
	(0.003)
Building age: 10-19 years	-0.061***
	(0.007)
Building age: 20-29 years	-0.098***
	(0.015)
Building age: 30-39 years	-0.125***
	(0.025)

Building age: 40-49 years	-0.152***
	(0.039)
Building age: 50-59 years	-0.180***
	(0.051)
Building age: 60-69 years	-0.201***
	(0.059)
Building age: 70-79 years	-0.212***
	(0.067)
Building age: 80-89 years	-0.226***
	(0.074)
Building age: 90-99 years	-0.190***
	(0.072)
Building age: Over 100 years	-0.147**
	(0.069)
Build date: pre 1900	0.355***
	(0.079)
Build date: 1900-1909	0.174**
	(0.074)
Build date: 1910-1919	0.223***
	(0.077)
Build date: 1920-1929	0.237***
	(0.074)
Build date: 1930-1939	0.211***
	(0.066)
Build date: 1940-1949	0.145**
	(0.057)
Build date: 1950-1959	0.108**
	(0.048)
Build date: 1960-1969	0.101***
	(0.035)
Build date: 1970-1979	0.068***
	(0.025)
Build date: 1980-1989	0.104***
	(0.016)
Build date: 1990-1999	0.093***
	(0.009)
Constant	10.664***
	(0.019)
R^2	0.850
AIC	232410.5
Numbers of HMA-years effects	1,184
Observations	904,075

Standard errors in parentheses are clustered on HMA-years. The omitted category for House Type is 'Flat/Maisonette' and for Parking it is 'No parking'.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.3 Construction price index

The construction price index data was taken from the *Regional Supplement* to the *Guide to House Rebuilding Cost* published by the Royal Institute of Chartered Surveyors (RICS). The factors and sample sizes were available at the LAD level but not for every LAD in every year. Figure 2 plots the share of districts that are missing in each year and shows that the problem is worse at the beginning of the data period. In order to fill these missing values, data were taken from a higher level geography (48 counties) which was fully available over the whole period. The following provides a description of how this filling procedure was carried out.

Figure 2: Share of missing construction price factors at LAD level, 1997-2007

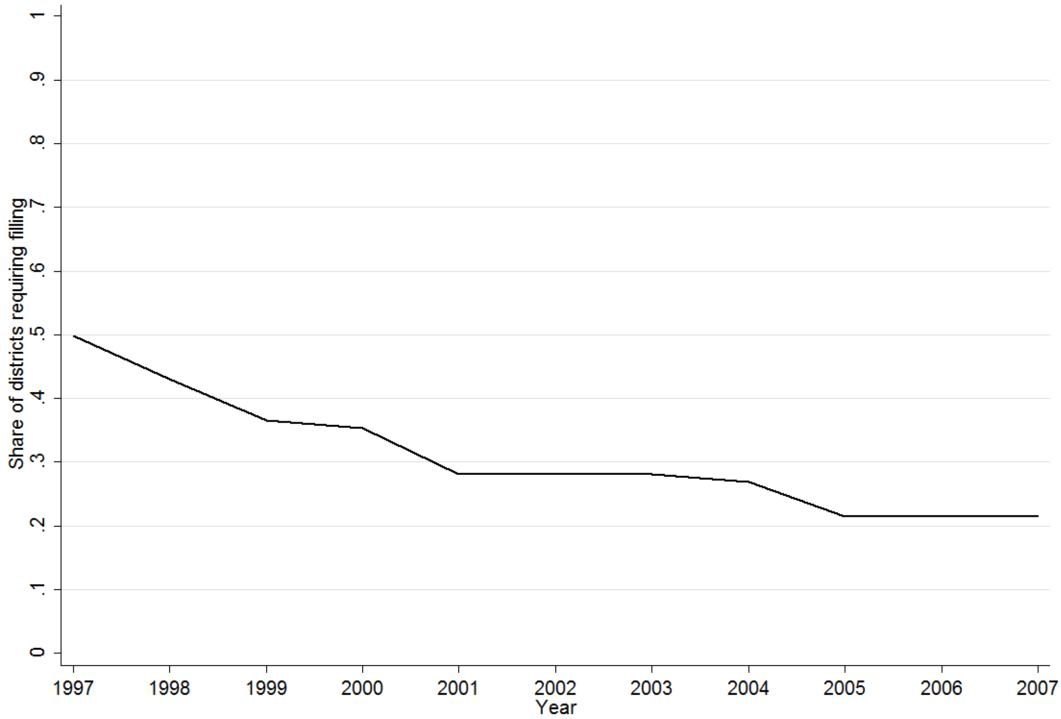


Table 2 presents a (fictitious) example to illustrate the filling procedure. Table 2a presents the fictitious data for County 1, which is made up of three districts. Factors are missing in some of the years for some districts. Starting with 2008 LAD 1 is filled by first comparing the sample for the districts that are observed, 21+28= 49, with the whole county sample, 57. We know there are 8 observed tender prices in the county total that must have come from LAD 1. This value was, however, not reported presumably because the sample size was not considered large enough to give a reliable location factor. It is simple to recover the value, though, using the following equation:

$$f_{ROC,c} = \frac{f_c s_c - \sum_d f_{d,c}\, s_{d,c}}{s_c - \sum_d s_{d,c}}$$

where $f_{ROC,c}$ is the factor for the 'rest of county' (i.e. aggregate of all missing districts) for county c, f_c is the county fact, s_c is the county sample, $f_{d,c}$ is the factor for all available districts d in county c, and $s_{d,c}$ is the sample for those districts.

The data are then filled as follows. The $f_{ROC,c}$ value is computed in each year for all counties with missing districts. Then starting from the last year (2008) all the missing districts in a county are made equal to the rest of county value $f_{ROC,c}$ only if they have are at least 20 observations among them (note it may be only one district). Otherwise, if there are less than 20 observations among them they are simply made equal to the county factor f_c. Then for 2007, the growth rate is computed between the rest of county factor for 2007 and the aggregate factors (imputed or otherwise) for the same districts in 2008. Note that this may not be simply the rest of county factor for 2008 since there may be additional missing factors in 2007. In this case a sample weighted average is computed between the rest of county factor for 2008 and the factors observed in 2008 that were missing in 2007. Finally if there are more than 20 observations the missing 2007 factors are computed by applying this rest of county growth rate to the relevant 2008 factors. If there are less than 20 observations then they are computed by applying the overall county growth rate to the relevant 2008 factors. The same growth rate based filled procedure is then applied to all previous years working backwards one year at a time. The filled values for the dummy example above via an application of this procedure is presented in Table **2**b.

So whilst the number of missing districts is quite large, especially at the beginning, the method used to fill them makes use of actual information on their values imputed from higher tier geographies. This will be more accurate where the missing districts within a county have similar factors, since they are all treated as an aggregate 'rest of county'. Where they differ significant, this will only matter where they end up being aggregated to different HMAs later on in the process. Finally, the filled districts are likely to be smaller local authorities with less dwellings so will contribute less when eventually aggregated with other districts to the HMAs level.

Table 2: Filling example

a. Fictitious construction price index data

	Factors			Samples		
	2006	2007	2008	2006	2007	2008
County 1	0.99	1	1.02	50	55	57
LAD 1	n/a	n/a	n/a	n/a	n/a	n/a
LAD 2	n/a	n/a	1.01	n/a	n/a	21
LAD 3	1.01	1.02	1.03	19	24	28

b. Illustration of filling procedure

	Factors			Samples		
	2006	2007	2008	2006	2007	2008
County	0.99	1	1	50	55	57
LAD1	0.988*	1.014*	1.000*	n/a	n/a	n/a
LAD2	0.998*	1.024*	1.01	n/a	n/a	21
LAD3	1.04	1.02	1.03	19	24	28
Rest of County	0.959*	0.985*	0.869*	31*	31*	8*
RoC Growth	0.974*	1.014*				

Note: * imputed values

4.4 Predicted refusal rates

In order to generate the trend in refusal rates, that vary between zero and one, I carried out a 'quasi-probit' regression. This involves generating probit scores for refusal rates i.e. $refusal\_score_t$ = probit ($refusals_t$) and regressing this on a time trend variable (in a 354 separate regressions, one for each local authority):

$$refusal\_score_t = \alpha + \beta t + \varepsilon_t$$

The predicted refusal rates are then computed as:

$$predicted\_refuals_t = \text{normal}\left(\alpha + \hat{\beta} t\right)$$

since the normal function is the inverse of the probit function. Alternative specifications were tried using predicted trends from a OLS regression and simply 3-year moving averages of the refusal rates with no substantive differences in the results.

4.5 Quality of life rankings and other variables

Table 3 presents the HMAs when ranked by quality of life index 1. It also lists values or quality of life index 2 and various differentials used in the cost function. The quality of life ranking in many cases corresponds to that presented in Gibbons et al. (2011) with areas such as Penzance (West Cornwall), Brighton and London coming near the top and areas such as Coventry, Grimsby and Scunthorpe coming near the bottom. This is of course no confirmation of its validity but nevertheless reassuring.

5. Results

5.1 Weights applied to variables

The variables in the cost function estimations in the main paper are weighted in most cases by the local authority district (LAD) dwelling stock in 2003. This is to ensure that land values and construction costs that are only available for LADs contribute proportionally to their respective HMAs in aggregation. Other variables such as house prices are more finely disaggregated so finer weight could be used. Further, there is less need for weighting since the number of observed housing transactions in each area is likely to be quite reflective of the number of units in that area. However, in order to treat all the data in the same way, the same dwelling stock weights at LAD level were applied. The same concept applies to the designation share which could have been computed directly at the LAD level but in order to remain consistent was computed at the LAD level then aggregated with dwelling stock weights. The wages data were aggregated using employment weights since this represent a more appropriate weight in this case than dwelling stock.

5.2 Unweighted cost functions

As a robustness check to ensure that the results are not driven by the application of the above described weights I ran the same cost function regressions without applying any weights. The results presented in Table 4 (cross section) and Table 5 (fixed effects) below confirm the findings are not particularly sensitive to this alternative specification.

5.3 Full quality of life regressions

Finally, Table 6 has the quality of life regressions reporting coefficients for the full set of amenities. Since the amenities do not have time variation this is only possible for the cross sectional model. Most of the amenities are insignificant, apart from employment potentiality, perhaps due to the fact that this is a regression of time varying quality of life on cross-sectional controls.

Table 3: Quality of life indices, and other variables used in regression for HMAs ranked by QoL^1

HMA name	Quality of life 1	Quality of life 2	House price differential	Land value differential	Construction differential	Designation (z-score)	Refusals (z-score)	Saiz index (z-score)	Pop. Dens. (z-score)
Penzance	.1748749	4.041012	.1201186	-.5154164	.0205554	.4027876	1.063867	1.742146	.3841833
Launceston & Bude	.1209284	3.061542	-.0034237	-.5928693	-.0181151	-.8495921	-.0345297	1.088675	-1.40396
Dorchester & Weymouth	.1162691	2.486161	.2024558	.2588946	.0248221	.4217503	.4247046	.8817478	-.6226798
Torquay	.1094972	2.528172	.0996985	.1270983	.0072404	-.1985833	.493426	.9432183	-.2392697
Eastbourne & Hastings	.0957999	1.901977	.2191557	.0891539	.1128275	-.9755577	1.211647	.4845025	.10153
Barnstaple	.0933124	2.402505	.0020034	-.148629	.0128873	-1.239168	.8658185	.6525307	-1.173521
Truro	.0835354	2.00567	.0576955	-.4213167	-.0297201	-.0087404	.2311356	1.398037	-.0548038
Berwick-upon-Tweed	.0825197	2.914932	-.2066538	-.7894496	-.0096095	-.2189362	-1.939814	.9942439	-1.303494
Whitby & Malton	.0777847	2.01629	.0001219	-.2524159	-.0474827	-.7302982	.4994154	.0265572	-1.615291
Exeter	.0767771	1.614198	.1529287	.2849956	-.0055938	-.1372057	.6094738	.2143445	-.3274927
Isle Of Wight	.0709547	1.634968	.0811052	-.4321653	.0390167	.3399208	1.183168	1.637262	.1348928
Hereford	.0688622	1.620825	.032801	.0902431	-.0521581	-.1415526	.1110398	-.2277416	-1.96225
Brighton	.0681903	.5908297	.4389919	.6182157	.1003152	.626514	1.10135	.5253294	1.182136
Salisbury	.0668977	.845564	.3011554	.4869249	.0327468	.3165572	.9456481	-.9170603	-1.017824
Portsmouth	.0633265	.7199718	.2827992	.6773527	.0680665	.2658433	.4612454	.4039899	.8577416
Bournemouth	.0580704	.5651845	.2845983	.5277739	.031433	.7329295	1.586967	.2243664	1.114399
London	.045809	-.6283653	.6729144	1.466262	.1608097	1.56711	1.055731	-.4844624	2.11523
St. Austell	.0419626	1.22459	-.0257393	-.4912469	-.0234298	-.7432572	.4712216	1.183882	.0605792
Worcester	.0407854	.7036474	.0995754	.3534656	.0038802	.2893965	.4240983	-.8757589	.3328288
Oxford	.0401428	-.4014238	.4478698	1.034714	.0296097	1.058582	.4791276	-1.62609	-.0264372
Northallerton	.037721	.9795524	.0305561	.0891995	-.0239968	.7174332	.2731695	-.4551694	-1.439301
Kendal	.03555	.9757729	.0344136	-.0059003	-.0449821	-2.872396	.0694618	1.061351	-1.957032
Bury St. Edmunds	.03391	.6047176	.0685332	.3040491	.0147841	.6769759	.1371992	-1.744014	-.2404939
Penrith	.0337431	1.337003	-.1359842	-.646639	.0506416	-1.026646	.161751	.7426788	-2.181731

Colchester	.0326705	.1281692	.1859719	.4773568	.0587389	.4243762	.1356406	.4337539	.2122067
Taunton	.0323786	.6581894	.0537481	.0964978	-.0167749	-.7269992	-.0715295	1.128597	-.2670196
Bath	.0314628	.1196719	.1965416	.4293391	.0053724	.8348815	.8495953	-.9032843	-.260534
Canterbury & Ramsgate	.0270131	.2250142	.1017886	.3011911	.1127833	1.743533	.1835109	1.410649	.8476974
Southampton	.0232588	-.6404214	.324944	.79694	.0376942	.4274551	1.23917	-.3421783	.2252178
Plymouth	.0217539	.5442572	-.0121209	-.183727	-.0274279	.5018246	.2806453	.2648701	.3308013
Yeovil	.0150851	.2692574	.0417652	.2939881	-.0006801	.2513024	-.6695567	-.7363273	-.9831621
Norwich	.0130271	.3945286	-.0189269	-.2011903	-.0202074	1.473927	-.3359978	-1.538954	-.6802416
Dover & Ashford	.0082121	-.2718897	.1303035	.1626383	.0692776	.032474	.5790684	.8432153	-.0357876
Telford	.0004606	.31522	-.0939855	-.0435052	-.0446569	.2947777	-.1399163	-.8698649	.3713332
Ipswich	-.0059641	-.3420231	.0434211	.0400581	-.0017854	-.642316	-.5778478	-.3611045	-.3280059
Shrewsbury	-.0063295	-.189459	-.0308219	-.0294618	-.0331013	-.2957175	.5585713	-.7702245	-.2372272
Skegness	-.0063977	.9545041	-.2751873	-.7936414	-.0467874	-1.290385	-.2124492	.3226032	-1.913885
Gloucester & Cheltenham	-.0118531	-.8899357	.1244772	.5199157	.0143194	.6542948	.5254654	-.0215238	.7120445
Gt. Yarmouth & Lowestoft	-.0157651	.2366053	-.172397	-.506815	-.0285266	1.198439	-.0134713	1.205927	.5883129
York	-.0174523	-.9319368	.0766135	.2319583	-.0142185	.2641008	.0414582	-1.474025	.3683264
Kings Lynn	-.0181789	.0474466	-.1396545	-.6680577	-.0005372	-4.436656	-.4363794	.4997105	-1.698803
Scarborough	-.0188904	.0514419	-.1430337	-.2626733	-.014353	-1.743852	-1.885918	1.226753	-.9501941
Luton & Milton Keynes	-.0236189	-1.7161	.230356	.5772777	.0669386	.3326925	.580462	-1.543522	.3218922
Cambridge	-.0236919	-1.964639	.2674485	.7332953	.0350737	.1668022	.0986282	-1.132483	-.411296
Carlisle	-.0238237	.527517	-.2856138	-.6408678	-.0049096	-.5769531	-2.248244	.1122106	-1.157776
Swindon	-.0259481	-1.751079	.2020796	.5093177	.019086	.7501397	.131756	-.7908713	-.3293924
Birmingham	-.0278887	-.9080172	.014102	.357072	-.0271343	.6342802	-.0667836	-1.440721	1.140164
Stoke-on-Trent	-.0284236	-.049347	-.1841703	-.4483824	-.0394365	.0527071	.2412704	-1.144556	.5731892
Boston	-.0290396	.3425296	-.2766386	-.7550336	-.043035	-1.174484	-.4050871	.7180421	-.4258476
Northampton	-.0352644	-.879933	-.0374386	.1796185	.0062151	.1416526	-.7358488	-1.466784	.6694034
Bristol	-.0383921	-1.875332	.1646922	.5927054	.0120666	1.379932	.5521712	.7936411	.4106238

Peterborough	-.0387341	-.578837	-.1270543	-.1285913	-.0153099	-.0864503	-.539092	-.9565665	-.2504492
Leicester	-.038744	-.9171153	-.0545564	.2015361	-.0496952	-.0684525	-.3871188	-1.851532	1.091083
Preston & Blackpool	-.0395169	-.548206	-.1274953	.0804635	.0080509	-.6386784	-.0801868	.9303237	1.455531
Newcastle	-.0406363	-.2250763	-.2081874	-.1382696	-.0366937	.5070782	-1.098418	-.4833578	.8496456
Blackburn & Burnley	-.0425029	.155392	-.295668	-.61082	.0164479	-.0513102	-.3680387	.2451761	1.322036
Lincoln	-.0428959	-.0109036	-.2728742	-.610543	-.0519543	-.6299734	-.7065682	-1.52467	-.5540223
Manchester	-.0452312	-1.110136	-.0523245	.2040993	.0006325	.8264966	-.1088838	.4968396	1.451962
Chester & Birkenhead	-.0470149	-1.078217	-.0481329	-.3407554	.0110274	.6193644	-.2678246	.4772341	.6702492
Reading	-.0500049	-4.023621	.5289406	.9701431	.0971964	.5882065	1.55285	-1.196539	.8579007
Leeds	-.0506516	-1.160767	-.0830887	.2083186	-.0487053	.6802205	-.4047064	-.8770029	-.1272039
Coventry	-.0528183	-1.794884	.0444801	.3272323	-.0128265	.0722621	-1.182278	-1.545372	1.444338
Bradford	-.0556584	-.9469037	-.1513738	-.2168574	-.091564	-.4579769	.0507732	.2902476	.164763
Nottingham	-.05856	-1.010447	-.1638802	-.0138851	-.0593015	1.005685	-.1778953	-1.395453	1.316201
Hull	-.0678501	-.6308898	-.2828378	-.3012572	-.014088	.5831054	-.2788041	.2546056	-1.52862
Liverpool	-.0680634	-1.278547	-.1485327	-.0993855	-.0009579	.2717642	-.824234	.2979475	1.29969
Sheffield	-.0690217	-.9420145	-.2307913	-.2252075	-.0299915	-.0594628	-.4454331	-1.072128	.2992715
Derby	-.0697926	-1.435417	-.1279016	-.1197283	-.0750313	1.123991	-.7222173	-.4974088	.6916831
Middlesbrough	-.0891621	-1.202655	-.2725548	-.312224	-.0463525	.3455042	-1.445998	1.110684	1.460353
Lancaster	-.0938686	-1.9768	-.1483829	.1641139	-.0554756	-.1813166	-.6489716	1.036746	-.4363867
Grimsby	-.0982928	-1.03629	-.3597342	-.670701	-.1003745	.2284887	.1040826	.9658943	1.131006
Barrow-in-Furness	-.1087013	-1.366392	-.3606662	-.6114543	.0372966	-.1854468	-.9998756	1.741746	1.674028
Workington & Whitehaven	-.1463763	-2.281826	-.391403	-.9909865	.0372966	-2.712434	-.8770335	1.248103	-1.239445
Scunthorpe	-.1715267	-2.748983	-.411935	-.6433497	-.0481687	-.726763	-.2288348	.0063662	-.8535949

Table 4: Unweighted cost function regression (cross-sectional)

	(1)	(2)	(3)	(4)
	Dependent variable: ln house price			
Land value differential	0.278***	0.296***	0.256***	0.293***
	(0.024)	(0.024)	(0.025)	(0.023)
Construction price differential	1.148***	0.704***	1.169***	0.707***
	(0.208)	(0.024)	(0.185)	(0.023)
Conservation area land share	0.010	0.009	0.009	0.012
(z-score)	(0.014)	(0.013)	(0.013)	(0.014)
Protected land share (z-score)	0.031**	0.031**	0.036***	0.031**
	(0.013)	(0.013)	(0.013)	(0.013)
Predicted refusal rate (z-score)	0.041***	0.046***	0.043***	0.045***
	(0.011)	(0.012)	(0.011)	(0.011)
Undevelopable land share	0.004	0.010	0.006	0.010
<25km (z-score)	(0.010)	(0.011)	(0.010)	(0.010)
Population density (z-score)	-0.020	-0.019	-0.017	-0.019
	(0.015)	(0.015)	(0.015)	(0.015)
Land value differential squared			0.019	0.066***
			(0.019)	(0.023)
Construction price differential			-6.316***	0.066***
squared			(2.317)	(0.023)
Land value differential x			1.164***	-0.133***
Construction price differential			(0.336)	(0.046)
Constant	0.000	0.000	-0.003	-0.016*
	(0.009)	(0.009)	(0.010)	(0.010)
R^2	0.811	0.804	0.829	0.815
AIC	-1448.7	-1420.6	-1525.2	-1465.2
Numbers of HMAs	74	74	74	74
Observations	814	814	814	814
p-value for CRS		0.036		0.001
p-value for CD	0.000	0.000		
p-value for all restrictions		0.000		
Elasticity of substitution	1.000	1.000		0.358

Standard errors in parentheses are clustered on HMAs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Unweighted cost function regression (fixed effects)

	(1)	(2)	(3)	(4)
	Dependent variable: ln house price			
Land value differential	0.142***	0.141***	0.151***	0.152***
	(0.020)	(0.020)	(0.017)	(0.017)
Construction price differential	0.544***	0.859***	0.532***	0.848***
	(0.115)	(0.020)	(0.104)	(0.017)
Designation (z)	0.074***	0.088***	0.076***	0.089***
	(0.021)	(0.021)	(0.024)	(0.023)
Predicted refusal (z)	0.012	0.012	0.014*	0.013
	(0.008)	(0.008)	(0.008)	(0.008)
Land value differential			0.059***	0.059***
squared			(0.012)	(0.013)
Construction price differential			-1.255	0.059***
squared			(1.147)	(0.013)
Land value differential x			-0.184	-0.119***
Construction price differential			(0.216)	(0.026)
Constant	-0.031	-0.027	-0.049	-0.049
	(0.041)	(0.041)	(0.035)	(0.035)
R^2	0.953	0.952	0.956	0.955
AIC	-2583.5	-2571.1	-2629.8	-2618.5
Numbers of HMAs	74	74	74	74
Observations	814	814	814	814
p-value for CRS		0.006		0.012
p-value for CD	0.000	0.000		
p-value for all restrictions		0.000		
Elasticity of substitution	1.000	1.000		0.076

Standard errors in parentheses are clustered on HMAs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Quality of life regression reporting full set of amenities

	(1)	(2)	(3)	(4)
Dependent variable:	Constant share (QoL^1_{jt})		Interest-based (QoL^2_{jt})	
Predicted housing productivity				
Total ($\hat{A}_{jt}$)	-0.349***		-6.002***	
	(0.045)		(0.989)	
From designation (-$\delta\widehat{D}_{jt}$)		-1.140**		-13.827
		(0.488)		(11.630)
From planning (part of -$\hat{Z}_{jt}\gamma$)		-0.255**		-2.561
		(0.100)		(2.637)
From protected (part of -$\hat{Z}_{jt}\gamma$)		-0.435***		-3.589
		(0.140)		(3.313)
From geo. constraints (part of -$\hat{Z}_{jt}\gamma$)		-2.011***		-41.281**
		(0.752)		(17.532)
From unobserved factors (-ε_{jt})		-0.320***		-5.765***
		(0.043)		(0.972)
Employment potentiality	0.253**	0.188	8.043**	7.847**
	(0.120)	(0.128)	(3.626)	(3.797)
Distance to rail station	0.002	0.004	0.014	0.056
	(0.003)	(0.003)	(0.070)	(0.074)
Distance to airport	-0.000	0.000	0.003	0.003
	(0.000)	(0.000)	(0.009)	(0.010)
Cafes (kernel density)	0.042	0.039	1.118	1.109
	(0.037)	(0.038)	(1.116)	(1.106)
Food establishment (kernel density)	-0.016	-0.019	-0.130	-0.225
	(0.026)	(0.027)	(0.709)	(0.724)
Bar (kernel density)	-0.020	-0.020	-0.720	-0.665
	(0.018)	(0.018)	(0.461)	(0.453)
Museum (kernel density)	0.071	0.210	-0.343	2.746
	(0.155)	(0.153)	(3.880)	(3.863)
Theatre (kernel density)	0.457**	0.421**	8.903*	9.750*
	(0.193)	(0.185)	(5.020)	(4.924)
National Park (kernel density)	-0.001	-0.002*	-0.025	-0.041
	(0.001)	(0.001)	(0.024)	(0.026)
Key Stage 2 score (IDW)	0.000	0.000	0.025	-0.021
	(0.011)	(0.011)	(0.253)	(0.262)
Income 2005	-0.002	-0.000	-0.189**	-0.153*
	(0.003)	(0.003)	(0.074)	(0.080)
Ethnicity Herfindahl index	0.160	0.077	7.860*	7.580
	(0.182)	(0.191)	(4.581)	(4.737)
Distance to Lake	0.001	-0.000	0.021	0.005
	(0.001)	(0.001)	(0.029)	(0.033)
Distance to River	0.015	0.008	0.476*	0.348
	(0.010)	(0.010)	(0.252)	(0.272)
Distance to Coastline	0.000	0.000	0.012	0.015*
	(0.000)	(0.000)	(0.008)	(0.008)
Mountains, moors, and heathland (land share)	-0.177	0.212	-12.673	-2.657
	(0.825)	(0.747)	(19.557)	(17.951)
Semi-natural grasslands (land share)	-0.554***	-0.550***	-10.214**	-11.548***
	(0.158)	(0.156)	(3.942)	(3.895)
Broad-leaved/mixed woodland (land share)	0.227	0.213	3.853	6.440
	(0.187)	(0.198)	(4.972)	(5.058)
Urban (land share)	-0.143	-0.055	-2.227	-1.154

	(0.138)	(0.144)	(3.423)	(3.545)
Gardens (land share)	-0.364	-0.230	-10.261	-7.427
	(0.325)	(0.309)	(8.180)	(7.919)
Greenspace (land share)	-0.149	-0.021	-2.577	-0.237
	(0.191)	(0.191)	(4.673)	(4.544)
Water (land share)	-0.224	-0.269	-7.630	-9.628
	(0.417)	(0.410)	(10.466)	(10.585)
Constant	0.101	0.023	1.070	-0.765
	(0.315)	(0.320)	(8.235)	(8.455)
Controls	YES	YES	YES	YES
Fixed effects	NO	NO	NO	NO
R^2	0.670	0.685	0.615	0.625
AIC	-2892.4	-2920.3	2315.0	2302.0
Observations	814	814	814	814

Standard errors in parentheses are clustered on HMAs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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