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The preservation of historic districts – is it worth it?

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Abstract

I investigate the welfare effect of conservation areas that preserve historic districts by regulating development. Such regulation may improve quality of life but does so by reducing housing productivity – that is, the efficiency with which inputs (land and non-land) are converted into housing services. Using a unique panel dataset for English cities and an instrumental variable approach, I find that conservation areas lead to higher house prices for given land values and building costs (lower housing productivity) and higher house prices for given wages (higher quality of life). The overall welfare impact is found to be negative.

Keywords: housing, planning, regulation, historic preservation, construction, land
JEL: H89, L51, L74, D62, R21, R31, R38, R52, R58

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1 Introduction

Conservation areas protect historic neighbourhoods by placing restrictions on the aesthetic quality of new development.[1] Conservation areas are particularly widespread in England with more than 9,600 designations since the legislation came into effect in 1967. Inside a conservation area, any new development is required to preserve or enhance the existing character of the neighbourhood. Similar policies exist in different forms internationally, for example, as local historic districts in the United States or as *Ensembleschutz* in Germany. By regulating the subjective quality of new buildings, conservation areas may reduce the productivity of the housing sector. Historic neighbourhoods are no doubt an important urban amenity, however, their preservation may hinder cities in affordably housing their current and future populations.

I estimate the welfare effect of conservation areas by assembling a unique panel dataset for English cities. The dataset comprises eleven years of conservation area designations over 1997–2007, as well as house prices, land values, construction costs, and other city characteristics. I construct my dataset at the city-level in order to capture the full costs and benefits of the policy at the level of the housing market. Specifically, I use the Housing Market Area (HMA) definition of urban areas.[2] I estimate a net welfare effect comprised of quality of life effects (benefits) and housing productivity effects (costs). The quality of life effect is derived from the amenity value that households place on the historic built environment in their city and its conservation. A city-level analysis captures benefits at the neighbourhood level – since residents are more likely to live inside or near a conservation area if they are widespread in a city – and at the city-level i.e. the value that residents place on the amount of preservation in their urban area as a whole.

The costs of conservation are modelled as a housing productivity effect i.e. the effectiveness with which land and non-land inputs are converted into housing services. Underlying this is the assumption that productivity, and not quantity, is the major channel for the supply-side effects of designation.[3] Indeed, the purpose of conservation areas is not to prevent development but to ensure that new buildings preserve the character of a neighbour-

[1]Conservation areas protect groups of buildings within certain boundaries. Single buildings are usually protected by different legislation, e.g. 'listed buildings' status in England.

[2]Housing Market Areas (HMAs) are defined to capture individual housing markets, based on evidence from patterns of commuting, migration and house prices [DCLG, 2010]. As such, they are characterised by a high level of self-containment – 77.5% of working residents of an HMA have their workplace inside the HMA – and typically approximate recognisable city regions.

[3]I provide some empirical support for this assumption by showing that conservation area designations do not impact negatively on dwelling stock in England over 2001–2010.

hood. Such aesthetic restrictions may lower housing productivity in several ways. Firstly, developers wishing to build inside conservation areas must navigate an extra layer of regulation. Secondly, the planned buildings must meet certain standards, which may not be the most cost effective way of providing housing services. Thirdly, the extra costs of developing inside conservation areas may push development out to less favourable sites in a city. For these reasons, cities with lots of conservation areas will be less productive in the housing sector than other cities. Developers will be able to produce fewer units of housing services for given amounts of land and non-land inputs resulting in higher housing costs. A city-level analysis is required to capture productivity effects that determine prices at the level of the housing market.

There is a growing body of literature on the economic effects of conservation areas. The majority of this literature has focussed on estimating the quality of life effects of conservation areas by examining property prices. A distinction commonly made in the literature [e.g. by Coulson and Leichenko, 2001] is between (negative) internal effects related to restrictions to property rights and (positive) external effects related to the conservation of neighbourhood character. Quasi-experimental evidence has shown that the overall effect of designating conservation areas is to increase property prices, which suggests that the positive effects dominate [Ahlfeldt et al., 2017, Koster et al., 2014].[4] Furthermore, Koster et al. [2014] find that households with higher incomes have a higher willingness to pay for living inside conservation areas in the Netherlands. Ahlfeldt et al. [2017] find that the pattern of house price effects in England is consistent with a situation where local planners designate conservation areas according to the interests of local owners. If local owners who benefit from designation are indeed able to game the planning system to their advantage then it important to know what the effects of designation on housing costs are at the wider market level.

There is a current lack of evidence on the supply-side effects of conservation areas. The only evidence to date is presented by Been et al. [2015], who show that construction is slightly lower inside historic districts in New York City. However, they do not examine quantity effects at the city level or supply-related effects on housing costs. The evidence from other forms of regulation suggests that the costs of development restrictions are significant.[5] For example, Hilber and Vermeulen [2014] find that house prices in England would be 35% lower

[4]Other studies that examine the impact of conservation areas on property prices include Asabere et al. [1989], Asabere and Huffman [1994], Asabere et al. [1994], Coulson and Lahr [2005], Leichenko et al. [2001], Noonan [2007], Noonan and Krupka [2011], Schaeffer and Millerick [1991].

[5]See Gyourko and Molloy [2015] for a good overview of the evidence.

if planning constraints were removed. The available evidence finds that the quality of life benefits associated with planning are smaller than the costs. Glaeser et al. [2005] 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 development restrictions led to large increases in house prices that left residents worse off, even after accounting for the policy benefits. This 'regulatory tax' finding is repeated in other studies such as Albouy and Ehrlich [2012] and Turner et al. [2014], both for regulatory constraints in the U.S., and Cheshire and Sheppard [2002] for land use planning in Reading, England.[6]

In this paper, I investigate the extent to which conservation areas explain differences in housing productivity across cities and whether there are associated quality of life improvements that compensate. As such, I provide an estimate of the net welfare effect of conservation areas for the average (owner occupier) household. Evaluation of the welfare effects of conservation areas in cities is challenging since both quality of life (via demand) and housing productivity (via supply) result in increased house prices. In order to disentangle these effects, I make use of Albouy and Ehrlich's [2012] two-step approach. In the first step I estimate a cost function regressing house prices on input prices (land and non-land) and city characteristics that may shift productivity. Housing productivity is defined as the amount of physical housing that can be produced for given quantities of inputs. The key assumption behind this step is perfect competition. If designation makes building more costly then house prices will be higher for given input prices to maintain zero profits. I find that the average increase in conservation area designation share at the HMA level over 1997–2007 decreases housing productivity by 4.3%, implying a cost-driven increase in house prices of the same magnitude. In the second step, I construct an expenditure-equivalent quality of life index based on house prices and wages and regress it on the same productivity shifters, including designation. The key assumption here is of household mobility. Spatial equilibrium implies that if designation improves quality of life in a city then house prices must be higher for given wage levels. I find that designation increases quality of life, but not by enough to compensate for the greater expenditure on housing resulting from lower productivity. The results imply that designations in England over 1997–2007 were welfare-decreasing for an average owner-occupier household in these cities.

While I make use of the Albouy and Ehrlich [2012] approach, my key contribution is dif-

[6] Further studies find that planning policies have damaging effects on the retail sector [Cheshire and Hilber, 2008, Cheshire et al., 2011].

ferent. I focus on estimating the welfare effect of a particular form of regulation, conservation areas, rather than of housing regulation in general. Conservation areas are a particularly fitting application for the approach since they are expected to impact on housing productivity less than quantity, specifically. Moreover, focussing on a particular form of regulation allows me to identify a causal impact by employing an instrumental variables approach. Finally, my paper distinguishes itself by focussing on England rather than the United States, and by constructing a panel dataset that allows me to control for fixed unobservables.

My identification strategy involves an instrumental variables approach. The instrument for designation is a shift-share of the Bartik type [Bartik, 1991]. The closest previous approach is Koster and Rouwendal [2017] who use national-level changes in spending on cultural heritage weighted by the local share of listed dwellings as an instrument for local investment in historic amenities. My instrument uses changes in the national-level designation shares for the dwelling stock of particular build periods weighted by the HMA shares of dwellings in those build periods. The national level changes in designation are assumed to reflect changes in the subjective evaluation of the dwelling stock of particular build periods. As such, the instrument is a fairly novel application of the shift-share approach. The identifying assumption is that the instrument is unrelated to unobserved shocks to housing productivity or quality of life, conditional on pre-trends in house prices, trends related to the initial value of the instrument (capturing the initial stock), and trends related to other city characteristics. I support the validity of this assumption by showing that the instrument is not related to gentrification.

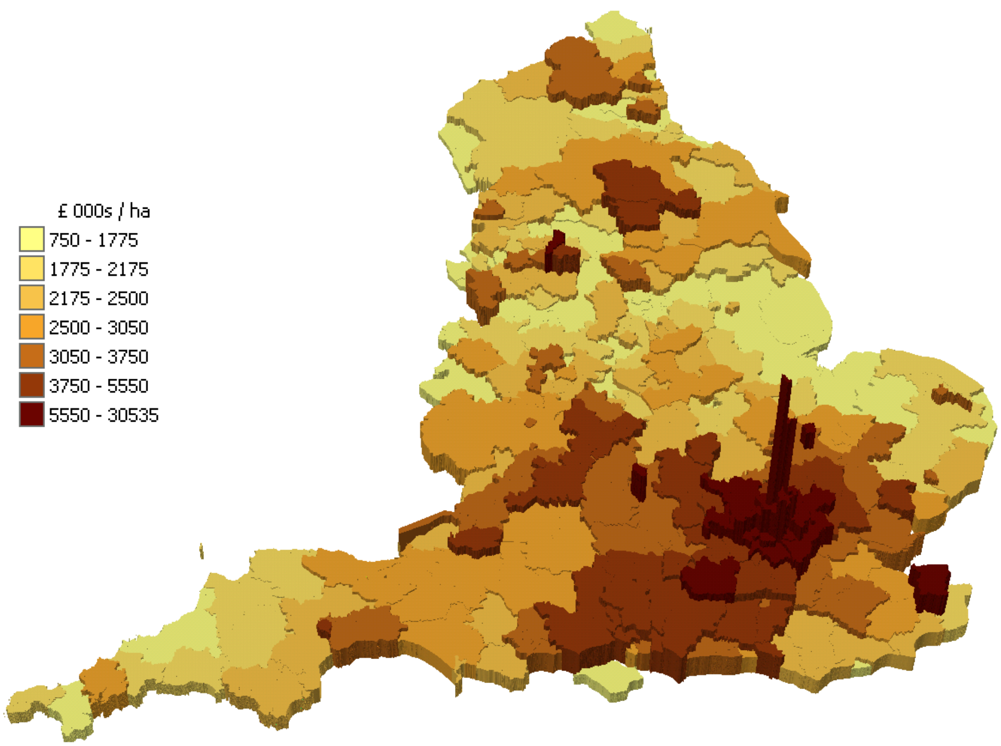
The key contribution of this paper is to estimate both the supply-side costs and demand-side benefits of conservation areas; evidence that is currently missing from the growing body of literature on the policy. This paper also contributes to a literature that investigates the costs and benefits of regulation and planning more generally. I present some of the first causal estimates of the welfare effects of a form of housing regulation. To my knowledge the only previous paper to examine both the costs and benefits of housing regulation using exogenous policy variation is Turner et al. [2014]. I also contribute to a literature on the value of locational amenities, by estimating the quality of life effect of a regulation policy instrumented at the city level.[7]

Furthermore, my results contribute to the literature on housing production functions by estimating what is, to my knowledge, one of the first housing production functions for

[7] The amenities literature is large, but a few examples are Albouy [2016], Bayer et al. [2007], Brueckner et al. [1999], Chay and Greenstone [2005], Cheshire and Sheppard [1995], Gibbons et al. [2011], Glaeser et al. [2001], Moeller [2018].

the UK. I follow Albouy and Ehrlich [2012] who take the traditional approach of regressing house prices on input prices [e.g. McDonald, 1981, Thorsnes, 1997]. A more recent literature attempts to estimate the production function, treating housing as a latent variable [Ahlfeldt and McMillen, 2014, Combes et al., 2016, Epple et al., 2010]. According to Combes et al. [2016], the two major challenges with estimating any type of housing production function are data availability and disentangling housing quantity from its price. Data availability is a challenge since the approach usually requires data on both house prices and land values. As discussed, I construct a unique panel dataset of cities that includes house prices and some previously unused data for land values and constructions costs for England. The land value data, depicted in Figure 1 for 2007 play a key role in the production function step and in estimating the productivity impact of designation.

Figure 1: Residential land values by local authority, 2007



Notes: Valuation Office Agency data. Assessed residential land value for small sites with outline planning permission. Areas are extruded proportionally with land value.

The second challenge is disentangling housing quantity from its price, which is difficult due to unobservable property characteristics that impact on price. Having collected a panel dataset, I am able to estimate the cost function using fixed effects, which helps to overcome the problem of unobserved property characteristics. My preferred estimates of the land cost share (0.29) and the elasticity of substitution between land and non-land inputs (0.53) fall within the range in the literature.[8]

The outline of the rest of the paper is as follows. In section 2 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 go over the data used in empirical analysis. In section 4 I outline the two-step empirical approach and the identification strategy. In section 5 I present the results. Section 6 concludes.

2 Model

In this section, I describe how conservation area designation impacts on housing productivity and quality of life in a general equilibrium context. I use the model of Albouy and Ehrlich [2012], which is an inter-city spatial equilibrium framework based on work by Roback [1982] and Albouy [2016]. Each city j is small relative to the national economy and produces a traded good X and housing Y that is non-traded. 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 homogeneous 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 housing services are derived from the characteristics of the physical unit itself. Benefits derived from neighbourhood quality will come in to the individual utility function via a quality of life measure defined later on. Firms produce housing services in each city according to:[9]

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

[8] For example, Albouy and Ehrlich [2012] find a land cost share of about one third and an elasticity of substitution of 0.5 for the United States.

[9] 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, N^X is traded good labour (paid wages w_j^X) and K is mobile capital paid a price i everywhere.

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 materials (non-land) input to housing (paid price v_j). Materials are conceptualised as 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 factors into housing services and is a function of city-specific attributes which may include the level of conservation area designation. Specifically, designation will impact negatively on A_j^Y if the policy makes it more difficult to produce housing services. As discussed in the introduction, I assume that the major supply-side effects of designation come through the productivity rather than the quantity channel. The validity of this assumption has important empirical implications that are discussed in subsection 2.4 below. Changes in A_j^Y are assumed to be factor-neutral productivity shifts i.e. the relative factor productivity remains unchanged. However, I demonstrate robustness to this the factor-neutrality assumption in the empirics.

Firms choose between factors to minimise the unit cost at given factor prices $c_j(r_j, v_j; A_j) = \min_{L,M}\left\{r_j L + v_j M : F^Y(L, M; A_j) = 1\right\}$. Zero profits imply the unit price of housing is equal to this unit cost, i.e. $p_j = c_j(r_j, v_j; A_j)$. Log-linearisation and taking deviations around the national average gives the zero profit condition:[10]

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

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, ϕ_L is the land cost share for housing and ϕ_M is the non-land cost share. This condition tells us that for each city the house price is given by the sum of the factors' prices (weighted by their cost shares) minus productivity. Cities with lower housing productivity must have higher house prices for given factor prices in order to maintain zero profits.

Figure 2 illustrates this point by plotting house prices against land values (holding materials costs constant) in an illustrative diagram. The average productivity curve shows how house prices relate to input prices (land values) for cities of average productivity, such as York and Cambridge. As the input price increases, house prices must also increase to maintain zero profits. The curve is concave since developers can substitute away from land as it becomes more expensive. Cities above the curve, such as Brighton, are considered to have low housing productivity because they have higher house prices for the same input price.

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

Brighton has the same land value as Cambridge but ends up with more expensive housing because it is less effective at converting the inputs into outputs. The productivity difference between two cities such as Brighton and Cambridge can be inferred from the vertical difference between them.[11]

2.2 Location choice under spatial equilibrium

Households with homogeneous preferences have a utility function $U_j(x, y; Q_j)$ that is quasi-concave in the traded good x and housing y and increases in city-specific quality of life Q_j.[12] Quality of life is determined by non-market amenities that are available in each city, such as air quality or employment access. These may also include conservation area designation. An increase in designation impacts positively on Q_j if preservation has amenity value. As the designation share in a city increases it becomes more likely that a representative household lives inside or close to a conservation area. Furthermore, the general level of preservation in a city may be of amenity value to all residents. 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 $e(p_j, u; Q_j) = \min_{x,y} \{x + p_j y : U_j(x, y; Q_j) \geq u\}$. Households are assumed to be perfectly mobile, therefore, spatial equilibrium occurs when all locations offer the same utility level $\bar{u}$. Perfect mobility is consistent with an extensive empirical literature that shows migration flows follow economic incentives [e.g. Linneman and Graves, 1983, Graves and Waldman, 1991, Gyourko and Tracy, 1991]. A more direct test of spatial equilibrium is provided by Greenwood et al. [1991] who find little evidence of disequilibrium pricing in income across cities. Indeed, Notowidigdo [2011] estimates mobility costs finding they are "at most modest and are comparable for both high-skill and low-skill workers" (p. 4).

Locations with higher house prices or lower levels of quality of life amenities must therefore be compensated with higher income after tax τ, i.e. $e(p_j, \bar{u}; Q_j) = (1 - \tau)(w_j + I)$. Log-linearised around national average this spatial equilibrium condition is:

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

[11] 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 $\tilde{v}_j = \alpha \tilde{w}^Y$, where α is the labour cost share of the non-land input.

[12] There are two types of worker: housing sector and 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.

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 wages. The spatial equilibrium condition tells us that in each city the (expenditure-equivalent) quality of life must be equal to the unit house price minus the wage (weighted by expenditure shares). Cities with a higher quality of life must have higher house prices for given wages to compensate.

2.3 The impacts of designation on house prices

The two conditions, zero profit and spatial equilibrium, both suggest that conservation areas may increase house prices but the two channels are entirely separate. It is worth underlining here exactly how these two channels operate since this is the mechanism on which the subsequent empirical approach is based. Firstly, if designation impacts on housing productivity only then house prices must be higher *for given input prices* in order to maintain zero profits (equation 2). This point is illustrated by the example of Brighton over Cambridge in Figure 2. Since quality of life is left unaffected, higher house prices in cities with designation must be compensated for by higher wages in order to maintain spatial equilibrium (equation 3). In this way, the quality of life index remains unaffected by housing productivity shocks from designation. Secondly, if designation impacts on quality of life only then house prices must be higher *for given wage levels* in order to maintain zero spatial equilibrium (equation 3). Since housing productivity is unaffected, higher house prices must be associated with higher input prices to maintain zero profits (equation 2). Figure 2 does not include wages so quality of life differences cannot be illustrated. However, if wages were held constant then an increase in quality of life could look like a movement from Cambridge to York. Thus, the quality of life effect cannot be confused with the housing productivity effect, and the housing productivity effect cannot be confused with the quality of life effect.

2.4 Quantity effects, preference heterogeneity and sorting

So far the costs of designation have been treated as a productivity effect, rather than a quantity effect. This assumption has been motivated by the fact that conservation areas do not ban development (as in zoning) or specifically restrict the amount of housing (as in height restrictions), but instead impose aesthetic standards that may make building more costly. Whilst there is no specific provision for height restrictions in conservation area policy, the local planning authority is fairly free to decide which buildings they feel preserve the character of the neighbourhood and which do not. Therefore, it cannot be ruled out that they favour lower-rise buildings, effectively imposing a height restriction. Furthermore, even if conservation areas do not restrict the amount of housing directly, there may be quantity adjustments resulting from housing productivity reductions.

The Albouy and Ehrlich [2012] model neatly sidesteps quantity effects by the assumption of homogeneous individuals. Even if there are quantity effects, this has no impact on city prices because the new marginal resident has the same willingness to pay as the old marginal resident (holding quality of life constant). In a similar model, Albouy and Farahani [2017] introduce a degree of preference heterogeneity which delivers a downward sloping demand curve at the city level. Taking an assumed value for the elasticity of population to housing costs, the model predicts that housing productivity reductions lead to larger increases in prices and smaller decreases in land values, compared with the homogeneous preferences case. The reason for the difference is that the quantity reduction pushes the marginal resident up the demand curve where they have a higher willingness to pay resulting in both higher prices and higher land values, compared with a flat demand curve.

The empirical implication of this is that the quality of life step will now capture both (a) quality of life increases due to amenity changes and (b) quality of life increases of the marginal resident due to quantity rationing. The empirical implication is not an identification problem as such—since the quality of life for the marginal resident continues to be correctly identified—but rather a problem to do with interpreting the quality of life parameter in the welfare calculations. Clearly, using (b) as a welfare increase misses whatever happens to the residents who would have been living in the city had it not been quantity-rationed.

A related problem is that of household sorting. Sorting occurs if conservation area designation leads to a migration of residents into a city who have a higher willingness to pay for that type of quality of life amenity compared with existing residents. As before, the quality of life effect for the marginal resident will continue to be correctly identified in that empirical step. However, it leaves open the possibility that the quality of life effect has an additional component (c) a quality of life increase of the marginal resident due to changes in the willingness to pay as a result of sorting.

If effects (b) or (c) represent a relatively large proportion of the overall quality of life estimate than the model would likely be over-estimating the welfare benefits of conservation. The welfare conclusions are justified to the extent that the actual sorting and quantity effects of designation are relatively small over the time period considered. Such effects would be small if price adjustment to the quality of life effects is immediate (since residents value security over the future character of their neighbourhood) but if adjustment through quantities and sorting between cities is limited and occurring over a longer time frame. In order to somewhat alleviate the concerns related to quantity, I conduct a regression of conservation areas on the number of dwelling units at both the local and city level. In Table 3 I show that conservation area designations did not impact on the number of dwelling units in England over 1997–2007, either at the city-level (HMA) or at the very local level (output area).[13] However, there remains the possibility that conservation areas impact on some other measure of housing quantity such as floorspace. Overall then, whilst the assumption that sorting and quantity effects are limited may be reasonable, the existence of large effects cannot be empirically ruled out and the welfare results are therefore caveated accordingly.

3 Data

3.1 Housing market areas

The empirical analysis is conducted at the housing market area (HMA) level. These areas are defined by DCLG [2010] using a grouping algorithm applied to ward-level census data on commuting patterns, house prices and migration flows. The use of commuting patterns makes them similar to the better-known travel-to-work area (TTWA) definition. However, HMAs have a higher commuting self-containment rate of 77.5%, compared with just 66.7% for the TTWAs [DCLG, 2010]. For this reason in particular, they are considered a better empirical counterpart to the theoretical j-locations. In addition, HMAs are defined such that similar houses have a similar within-area price (after adjusting for observed characteristics). Furthermore, HMAs have a 50% closure rate for migration flows, implying a good deal of both within- and between-market integration.

For much of the data described below, I aggregate from local authorities (LAs) based on the relationship mapped in Figure 3. I use weighted aggregation to address potential spatial mismatch. In order to improve the precision of the house price index, I drop eight (of 74) HMAs – those with fewer than 100 housing transactions per year.[14] Since the greatest period of overlap of the different data is 1997–2007, the final panel dataset has $T = 11$ and $N = 66$. While this is a fairly small N, any loss of precision is worth it to ensure self-contained areas prescribed by the theory. The wider costs and benefits of designation

[13]Output areas (OAs) are the smallest geographical units available for most UK data. They cover 0.78 square kilometres on average, which is only three times the size of the average conservation area (0.26 square kilometres). In comparison, HMAs cover 1,762 square kilometres, on average.

[14]This is an arbitrary cut-off point. Results for the full dataset are presented in robustness checks in Appendix B in the web appendix.

may only be fully captured at the market level. As described in the introduction, part of the benefits of conservation may be from living in a city with lots of well-preserved heritage neighbourhoods. On the cost side, designation that lowers productivity in one part of the city will impact on prices elsewhere in the city, since the units offer access to the same labour market and are therefore substitutable. However, in Appendix B in the web appendix I demonstrate that the approach is robust to using LAs as the unit of observation.

All variables used in the analysis are expressed as log deviations from the national average in each year, denoted by tilde (e.g. $\tilde{p}$). For each variable, I first log-transform it and then subtract the mean of the log-transformed values across HMAs in each year. For the productivity shifters, I additionally normalise the standard deviation to one. Descriptive statistics for the panel dataset are given in Table 1.

3.2 House prices

The production function relies on the theoretical concept of 'housing services', which represents the flow of value derived from physical housing for the occupant. Housing quality and housing quantity are assumed to be entirely substitutable in that each simply delivers a flow of 'services' to the occupant. This assumption is a useful simplification since it implies that the unit price of housing services can be estimated from house prices in a hedonic approach that controls for housing quality and quantity.

House prices for 1,087,896 transactions in England over the period 1995–2010 come from Nationwide, the largest building society in the UK. All transactions in the Nationwide data are for owner-occupied units.[15] 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. 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:

$$p_{ijt} = X_{ijt}\beta + \varphi_{jt}(HMA_j \times YEAR_t) + \epsilon_{ijt} \tag{4}$$

The house price index is then constructed by taking the predicted HMA-year effects $\hat{\varphi}_{jt}$ and

[15] This implies that the quality of life index will reflect homeowner spatial equilibrium only. However, during the time period considered, private renters represented only around 10% of households, whereas homeowners represented nearly 70%. The small share of private renters and the fact that rents are likely to be closely related to house prices means that a homeowner spatial equilibrium is likely to be broadly representative.

subtracting the national average in each year, i.e. $\tilde{p}_{jt} = \hat{\varphi}_{jt} - \bar{\hat{\varphi}}_t$. The result represents log deviations from the national average since house prices are log transformed for the hedonic regression. The results of the hedonic regression and a brief discussion of the coefficients are presented in Appendix A in the web appendix. Since the distribution of observed transactions within each HMA-year may differ from the actual distribution of housing stock in the HMA, each observation is weighted by the LA dwellings count in 2003 divided by the LA-year transaction count.[16]

3.3 Land values

Residential land values are obtained from the Valuation Office Agency (VOA). The land values are produced for the *Property Market Report* which has been released biannually since 1982. Land values for the full set of local authorities were, however, not made available until recently (2014). As such, this research is one of the first empirical applications of the full dataset. The assessments are based on a combination of expert opinion and observed values for transactions of land. The values are assessed for small sites (< 2 ha), bulk land ($>$ 2 ha) and flat sites (for building flats) for vacant land with outline planning permission. In order to produce an overall land value index I adjust for the price differences by site category using a regression discussed in Appendix A in the web appendix. Notably, the regression results show that bulk land is considerably cheaper (by 4.9% to 11.2%) than small plots in every year. It is reassuring that the valuations conform to the well-documented 'plattage effect' [by e.g. Colwell and Sirmans, 1993]. In order to validate the valuations, I make use of transaction data in the form of land auctions between 2001–2012.[17] There is a very high correlation with the valuation data as discussed in Appendix A in the web appendix. Land valuations for 1995–1998 are reported using a slightly different LA definition due to a local government reorganisation that occurred over this period. I converted the earlier LA definition to the new definition using the relevant lookup table.[18] I then took the mean of the biannually reported land values and aggregated them to the HMA level, again using the distribution of housing stock in 2003 as weights. As a final step, I computed log differentials.

[16] Further detail on the weighting procedure and regressions without weights are reported in Appendix B in the web appendix. The main results without weights are similar.

[17] It is not possible to use these transactions as the main source of data for land values since in many of the smaller cities there are not enough observations.

[18] Most LAs were unaffected. Of the original 366, 21 were merged into nine new areas, making the new total 354 LAs.

3.4 Construction costs

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 had it been entirely destroyed. The index 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 index is also reflective of local build quality.[19] The data is based on hedonic regression using 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 annually from 1997–2008 at the LA-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 software. The separate years were then matched to form a panel dataset. Some LAs 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 Appendix A in the web appendix. Finally, the LA level data were aggregated to HMAs weighted by dwelling stock, and then log differenced.

3.5 Conservation area designation

A spatial dataset of conservation areas (CAs) was obtained from English Heritage. The dataset contains polygons that map the borders of all CAs in England on the British National Grid coordinate system. The full dataset has only been used once before in empirical analysis by Ahlfeldt et al. [2017]. The data include the date of designation, which lies between 1966 and 2011. Using this information I calculated in a geographical information systems (GIS) environment the share of land in each HMA that was covered by CAs in each year over 1997–2007. Figure 4 plots the initial designation share in 1997 against the change in share over the study period. The chart shows variation in both the initial share and the change over the period. Although the changes are small as a proportion of all land, they may still have large productivity or quality of life effects as outlined below. The CA designation share is first

[19] This is beneficial since otherwise unobserved build quality would lead to designation being associated with higher house prices for given construction costs. Note that time-invariant build quality differences are captured by fixed effects in the cost function.

computed at the LA level in order to be aggregated to HMAs weighted by dwelling stock, ensuring all the data are produced comparably. The log land shares are then normalised to have a mean of zero and a standard deviation of one which 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.

The designated share of all HMA land is a proxy for the extent to which designation might impact on housing productivity or quality of life. So while the increase in designated land area over the period for the average HMA is relatively small, at 0.13%, the actual effects may be much larger.[20] A specific reason for this is that housing productivity effects will depend on the impact of designation on *marginal* developments which may disproportionately occur in existing residential areas where designations are more common. Moreover, designations might occur specifically to ensure that potential new developments maintain the neighbourhood character. On the benefits side, designations may also have quality of effects outside of the designated areas themselves via spillovers as documented, for example, by Ahlfeldt et al. [2017].

3.6 Planning restrictions and other housing productivity factors

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. This data was first used by Hilber and Vermeulen [2014] to analyse the effect of planning restrictiveness on housing costs in England. The LA data were aggregated to HMAs weighted by dwelling stock. The variation in refusal rates is volatile over time such that it is unlikely that every fluctuation represents actual changes in planning restrictiveness. The data were, therefore, smoothed in order to eliminate the short-term noise while keeping the longer-run trends in planning restrictiveness. This smoothing was done by regressing the refusal share on a binomial time trend and using the predicted values.

In order to estimate whether designation effects vary with geographic constraints, I compute the undevelopable share of land within 25km of each HMA centroid, following Saiz [2010].[21] Developable land is defined as land that is flat (<15 degree slope) and dry (solid

[20] In fact, the average increase of 0.13% in designated land share already looks much larger when looking at the proportion of designated buildings, according to the Nationwide transaction dataset – this is three and a half times larger at 0.45%.

[21] Saiz [2010] uses 50km circles around U.S. MSA centroids – whereas I define 25km circles to adjust for the smaller size of English HMAs. The average area of a U.S. MSA is about 7,000 km^2, which corresponds

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 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., following Hilber and Vermeulen [2014]. The final undevelopable land share is computed for each HMA as the total land area that is not developable divided by the total area in the 25-km circle.

3.7 Quality of life index

I construct a quality of life index according to equation (3) as follows:

$$QoL^1_{jt} = 0.31 \times \tilde{p}_{jt} - (1 - 0.225) \times 0.64 \times \tilde{w}_{jt} \tag{5}$$

where 0.31 is the average share of expenditure on housing, which comes from the *Expenditure and Food Surveys 2001–2007*. In different empirical specifications, I demonstrate robustness to using different values for the housing expenditure share, as well as using shares that vary by average city income. The price differential $\tilde{p}_{jt}$ is the same as that used in the cost function step, computed via hedonic regression. The annual wages $\tilde{w}_{jt}$ come 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 differentials. The average marginal income tax rate of 0.225 was computed using data from the HM Revenue and Customs for 2005/06 and the average share of income from wages of 0.64 is from the Department for Work and Pensions for 2005/06. I estimate additional specifications where the marginal income tax rate depends on the average income specific to each HMA-year observation and where the share of income from wages varies across regions. The results are robust to such changes as presented in Appendix B in the web appendix suggesting the use of average figures is suitable. A ranking of HMAs according to this quality of life index is presented in Appendix A in the web appendix.

to the area of a circle with a radius of around 50km and is perhaps 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 25km.

4 Empirical strategy and identification

My empirical strategy is based on the two-step approach of Albouy and Ehrlich [2012]. In the first step I estimate a cost function for housing production. The unit value of housing is regressed on land values, construction prices and productivity shifters, including designation. In the second step the quality of life index is regressed on housing productivity factors to reveal the overall welfare impact of designation. My identification strategy is based on implementing a panel fixed effects approach and instrumenting for designation with a shift-share.

4.1 First step: Cost function

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

$$\tilde{p}_{jt} = \beta_1 \tilde{r}_{jt} + \beta_2 \tilde{v}_{jt} + \beta_3 (\tilde{r}_{jt})^2 + \beta_4 (\tilde{v}_{jt})^2 + \beta_5 (\tilde{r}_{jt} \tilde{v}_{jt}) + \pi \tilde{R}_{jt} + \delta \tilde{D}_{jt} + f_j + u_{jt} \tag{6}$$

where $\tilde{R}_{jt}$ is the predicted refusal rate and $\tilde{D}_{jt}$ is the conservation area designation share. The fixed effects f_j capture all time-invariant productivity shifters, such as geographic constraints. The parameter δ is an inconsistent estimate of the housing productivity impact of conservation areas if designation is correlated with the error term. According to the model, quality of life factors are absent from u_{jt} as they are capitalised in land values $\tilde{r}_{jt}$. However, unobserved housing productivity shocks may be correlated with designation as discussed in the identification strategy below.

In this panel format, the log-differentials are taken around the national average in each year t. These differentials are equivalent to using year effects in the regression; however, I prefer to stick to the format suggested by the theory.[22] Imposing the restrictions 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 and McFadden, 1978]. Comparing equation (6) with equation (2) reveals that housing productivity is given by:

$$\tilde{A}_j^Y = -\pi \tilde{R}_{jt} - \delta \tilde{D}_{jt} - f_j - u_{jt} \tag{7}$$

[22] Taking differentials is necessary in certain parts of the model, e.g. to eliminate the interest rate i or reservation utility u.

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 impacts negatively on housing productivity then its coefficient δ is expected to be positive i.e. it will raise house prices above what is predicted by factor prices alone.

4.2 Second step: Quality of life

Increasing the cost of housing is not the intended effect of designation. Rather, conservation areas reduce housing productivity in order to preserve or improve the attractiveness of neighbourhoods. The second step investigates the demand side effect of conservation areas by relating the same productivity shifters, including designation, to a measure of quality of life. The regression takes the form:

$$\tilde{Q}_{jt} = \mu_1 \tilde{R}_{jt} + \mu_2 \tilde{D}_{jt} + \mu_3 u_{jt} + g_j + \varepsilon_{jt} \tag{8}$$

where g_j are fixed effects that capture time-invariant quality of life factors. The parameter μ_3 gives the relationship between designation and quality of life. According to the model, productivity factors are absent from ε_{jt}, despite the fact that house prices go into the quality of life index. They are absent because higher wages will compensate for higher prices from productivity factors in order to maintain spatial equilibrium. However, unobserved quality of life factors may lead to a bias of μ_2, as discussed in the identification strategy below.

If conservation areas increase quality of life then μ_2 will be positive. The coefficient gives the quality of life impact expressed as a share of expenditure. Combining this with the estimate from the first step gives the total welfare as $\mu_2 - (0.31 \times \delta)$, since 0.31 is the housing expenditure share.

4.3 Identification

There are two features to the identification strategy. Firstly, I make use of the panel nature of the data by estimating a fixed effects model. Secondly, I combine fixed effects with a time-varying instrument for designation based on the Bartik [1991] shift-share. It is worth noting that identification here focusses only on the impacts of designation. Consistent estimates of the land cost share are obtained by instrumenting land values in an alternative specification outlined in subsection 4.5, but this step is neither necessary nor desirable for consistent estimation of the impacts of designation, as explained in that subsection.

Fixed effects estimation alone provides a major improvement over pooled OLS estimation by controlling for time-invariant housing productivity factors or quality of life factors. For example, on the cost function side, a time-invariant factor such as soil type may both affect housing productivity and be correlated with today's conservation areas (if it drove the location of historical settlements). Likewise on the quality of life side, many urban amenities such as job accessibility, natural factors and cultural amenities are relatively fixed over the period of one decade. Furthermore, the fixed effects models removes any effect from unobservable housing characteristics that biased the house price index in the hedonic regression stage. Thus, they help to deal with a common problem with estimating housing production functions [Combes et al., 2016]. I additionally include individual HMA trends to capture the effect of unobservable trends in housing productivity and quality of life factors that may be related to designation. A relationship in trends could come about if, for example, trends in unobservable housing productivity or quality of life factors and trends in designation are both related to the initial heritage endowment of a city. In terms of the theoretical model, estimation of a fixed effects model assumes spatial equilibrium in each year.[23]

The fixed effects strategy does not help when unobservables are time-variant. To illustrate, consider the example of a city with ongoing transport improvements that increase housing productivity and/or quality of life. Such improvements may also be the result of (or may result in) gentrification, which itself has been empirically demonstrated to lead to designation [Ahlfeldt et al., 2017]. In general, changes in city attributes that impact on housing productivity or quality of life will likely be interlinked with gentrification and, therefore, designation. To address this I employ an instrumental variable approach similar in spirit to a Bartik instrument [Bartik, 1991]. The instrument provides HMA-level 'shocks' that are a weighted average of the national level designation share of buildings in different build date categories. The weights used for a given HMA are the share of its dwelling stock in each build date band. Specifically, the instrument is computed as:

$$Z_{jt} = \sum_{b=1}^{14} D_{j-1,bt} H_{jb0} \tag{9}$$

[23]This will be the case if prices adjust quickly to quality of life changes. This assumption seems reasonable since consumers will immediately be willing to pay more for locations with improved amenity value. Theoretically, all prices (house prices, land values and construction costs) will immediately reflect changes to housing productivity and quality of life due to market competition. For example, developers buying land will pay a price that takes into account the latest information on what their buildings will sell for. The available evidence shows that house prices do respond quickly to amenity changes. For example, Gibbons and Machin [2005] show house prices in 2000 and 2001 adjusting to rail improvements from 1999.

where Z_{jt} is the counterfactual designation share in HMA-j and year t, $D_{j-1,bt}$ is the designation share in each age band b for all HMAs other than HMA-j, and H_{jb0} is the initial share of dwelling stock in HMA-j in age band. The national level designation share in each of the age bands are based on the Nationwide transactions data and are described in Appendix A in the web appendix.

The counterfactual designation share is expected to be a relevant predictor of HMA designation even conditional on fixed effects and trend controls. National changes in the designation share for buildings of certain build periods capture shifts in preferences for heritage. If an HMA has a high proportion of buildings in those build periods then the chances of designation are increased. Relevance is confirmed by the F-stats in Table 4 and Table 6 for the cost function and quality of life regressions, respectively. Furthermore, the instruments are significant and (mostly) have the expected signs in the first-stage regressions in tables presented in Appendix A in the web appendix. The charts in Figure 5 illustrate the counterfactual designation share and the actual designation share conditional on HMA fixed effects and trend interactions for a selection of cities.[24]

In order to be a valid instrument, the counterfactual designation share must be orthogonal to the error terms u_{jt} and ε_{jt}. The argument for exogeneity is that changes in the national level designation share are unrelated to anything going on at the individual city level, like gentrification. In order to capture general trends in unobservables that might be correlated with the initial stock, I include a trend variable interacted with the initial value (in 1997) for the instrument. In order to capture further possible trends I include interactions of a trend variable with city characteristics: the initial designation share, the initial refusal rate, the city population, protected land share, and undevelopable land share.[25] Therefore, the identifying assumption is that the instrument is unrelated to unobserved shocks to housing productivity or quality of life, conditional on controlling for trends related to the initial stock (as captured in the initial value of the instrument) and trends related to other city characteristics.

The exclusionary restriction requires that the instrument not lead directly to changes in the outcome variable. The exclusionary restriction could be violated if national-level changes in preferences for buildings of an HMA lead to the gentrification of that HMA, which in turns

[24]The designation share in these figures may appear to have a slight tendency towards a downward trend, however, this is just due to the selection of cities. The average trend is in fact zero after conditioning on trend interactions.

[25]Note that individual HMA trends are not used in order to ensure the instrument is relevant in the first stages.

impacts on housing productivity or quality of life. I argue, however, that such a correlation is unlikely to continue conditional on HMA fixed effects and trend controls. Gentrification is a complex process that depends on many more factors than the build date of the dwelling stock. In Table 2 I present evidence to support this argument. Here, the designation share and the counterfactual designation share are regressed on a measure of the share of residents who hold a degree certificate. This dependent variable comes from the UK census and proxies gentrification of a city. The positive and significant relationship with designation in the pooled OLS model implies that gentrification and designation are indeed interlinked. The size of this coefficient decreases as fixed effects and trends are introduced. However, for the instrument there is no relationship at all in either of the models. Given that gentrification is the most likely source of unobserved shocks, it is reassuring that it is not related to the instrument.

Another potential violation of the exclusionary restriction is if the instrument captures increased valuations placed on specific property characteristics. As it stands, these are not controlled for in the hedonic regression. In order to deal with this, for the IV models only, I re-estimate the hedonic regression with interactions between year effects and the build date categories. In a robustness check in Appendix B in the web appendix, I demonstrate that the results are not sensitive to this change.

4.4 Alternative specifications

I estimate three alternative specifications. Firstly, I investigate whether the effects of designation depend on the quantity of available land around a city. If there is an abundance of land, designation may have less effect on productivity as developers can easily build outside the city. To test this idea I create two dummy variables, one for HMAs that are above-average on the Saiz index and one for those below average. I interact the designation variable with each of these dummies and include the interactions in the two regression steps in place of the uninteracted version of the designation variable. These separate dummy interactions give the effect on housing productivity or quality of life in HMAs that have a scarcity of land or an abundance of land. Secondly, I investigate whether the benefits/costs of designation take time to materialise. I create a cumulative version of the designation share that is the sum of the designation share across periods, i.e. $C_{jt} = \sum_{t=1}^{T} D_{jt}$. If this is significant in either step it may indicate that the productivity or quality of life effects build up over time. Thirdly, I investigate whether designation is associated with factor non-neutral productivity shifts. I follow Albouy and Ehrlich [2012] by interacting the designation share with the factor price difference. This interaction captures whether designation impacts on the productivity of land more than it does on the productivity of non-land.

4.5 Consistent estimation of the land cost share

Since the land cost share is of independent interest it should be estimated consistently. The land cost share will be inconsistent if there are unobserved housing productivity factors in the error term of equation (6) since according to the model these capitalise in land values. The theory provides guidance as to a potential instrument since both quality of life and non-housing productivity factors will also capitalise into land values. If any of these factors are unrelated to housing productivity then they could serve as suitable instruments.

I create such an instrument based on the original Bartik [1991] shift-share where initial local employment shares across industries act as weights on national level changes in gross value added in those industries. This instrument predicts local changes in productivity (which capitalise into land values) that arise from national-level shocks that are unrelated to local-level housing productivity factors (conditional on city-level fixed effects and trends). The initial local employment shares come from the 1991 UK Census [Office of Population Censuses and Surveys, 1991]. This Census is a number of years before the panel begins to remain as exogenous as possible. The annual national level GVA over 1997–2007 comes from Cambridge Econometrics [2013]. Both are available for some 30 different industries.

Instrumentation of land values is kept to a separate specification because such instrumentation prevents the cost function of equation (6) from properly disentangling the housing productivity from quality of life effects. If land values are predicted by an exogenous factor then they do not include variation as a result of capitalised quality of life effects from conservation area designation. These effects will therefore instead be captured in δ, making it a mixture of quality of life and housing productivity effects, and difficult to interpret. In effect, for the cost function to work as desired, land values are required to be endogenous.

5 Results

5.1 Housing cost function

Figure 6 plots mean house price differentials ($\bar{\tilde{p}}_j$) against mean land value differentials ($\bar{\tilde{r}}_j$) and serves as an introduction to the regression results. The slope of the linear trend suggests a land cost share of $\phi_L = \beta_1 = 0.436$ and the binomial slope suggests convexity ($\beta_3 = 0.076$)

and an elasticity of substitution less than one. Holding all else constant the HMAs above (below) these lines have lower (higher) than average housing productivity. However, some of the price differences will be explained by construction costs. Furthermore, construction costs are correlated with land values, therefore, the land cost share itself is biased.

Table 4 presents the results from the panel fixed effects and IV estimation of equation (7). I estimate the Cobb-Douglas and translog production functions with and without CRS restrictions. The key parameter is conservation area designation, and this is positive and significant across all specifications, implying that conservation areas lead to higher house prices by reducing housing productivity. A standard deviation increase (an increase of 0.013) in designation is associated with a 0.159–0.169 house price effect in the fixed effects models and a 0.433–0.600 effect in the instrumented models. Since the fixed effects results are inconsistent under the existence of time-variant unobservables and since the first stage F-stats indicate the instrument is not weak, the IV estimates are the preferred results. The estimates imply that designation over 1997–2007 would have increased house prices (via reduced productivity) by 4.3–6.0% for the average HMA.[26] The instrumented estimates are significantly larger than their uninstrumented versions, implying that unobservables that are positively related to designation (such as gentrification) are positively related to housing productivity (i.e. reduce house prices for given input prices). As discussed in the empirical strategy this could be the case if gentrification is associated with, for example, transport improvements at the city level that increase productivity in the housing sector.[27]

The refusal rate control is also positive and significant in all models, implying planning restrictiveness decreases housing productivity. Here, the magnitude of the coefficient in the instrumented models suggests an increase in house prices of 6.0–7.8% over the period.[28] However, the refusals data has been smoothed making the coefficients unreliable. Furthermore, only the designation variable has been instrumented.

The land cost share is around 0.17-0.18 in all columns, which compares with a land cost share of 0.35–0.37 when estimating a housing cost function for the U.S. [Albouy and Ehrlich,

[26]The average HMA increased its designation share by about one tenth of the (between-group) standard deviation over the period 1997–2007. One tenth is multiplied by the coefficients to arrive at the average effect on prices. As argued in the data section, the increase in designation of 0.13% of all HMA land may produce large productivity effects if it disproportionately affects marginal developments. Therefore, the estimated effect sizes are plausible.

[27]Transport is also a likely quality of life amenity that would *increase* house prices via the quality of life route. However, it would also increase land values and therefore be captured in the land cost share of the cost function step.

[28]The effect implied by the coefficients of 2.6-3.4% multiplied by the average increase in refusals of 2.3 (between-group) standard deviations.

2012]. However, as outlined in Appendix B in the web appendix my preferred estimate of the land cost share is 0.29 after instrumenting for land values. The elasticity of substitution is 0.755 in the (restricted translog) panel fixed effects model and 0.213 in the instrumented model (compared with 0.367 for the U.S. in Albouy and Ehrlich [2012]). The parameter from the instrumented model falls at the lower end of the range of estimates in the literature, however, a robustness check in Appendix B in the web appendix using only new properties finds it increases to 0.402 which is the preferred estimate of the elasticity of substitution since new properties will not be subject to a depreciation of the capital component of the cost of housing, as argued by Ahlfeldt and McMillen [2014].

In terms of model selection, I focus on the tests of the restrictions in the instrumented models. The Cobb-Douglas restrictions are easily rejected in columns (5) and (6). The CRS restrictions are not rejected in column (6) but are rejected in column (8). Although CRS is rejected in column (8) I choose to proceed with the restricted translog model assumed in the theory. This decision is also justifiable given the results of interest do not differ greatly between restricted and unrestricted models.

5.2 Alternative specifications

Table 5 presents the alternative specifications of the restricted translog cost function. The baseline fixed effects and instrumented models are repeated in columns (1) and (5) for comparison. The first stages in Appendix B in the web appendix indicate that the instrumental variable approach encounters varying degrees of success across these alternative specifications. Therefore, in cases where the instrument appears to fail, evidence from the 'second-best' fixed effects model will be drawn upon to form tentative findings.

Columns (2) and (6) report the results for designation interacted with dummy variables for the Saiz undevelopable land index. In the instrumented model, there is a surprising negative effect for the land-abundant HMAs. However, the instrument has an unexpected sign for the subsample of land-abundant HMAs, suggesting this result may be disregarded. Instead, I focus on the fixed effects results where the productivity effect of designation in land constrained HMAs is larger than the baseline effect. For HMAs with plenty of land, however, the effect is far smaller and is statistically insignificant. Therefore, the results from the fixed effects model imply there is a greater effect of designation on housing productivity where there is a lack of land availability. This result conforms to expectations, since regulating development in conservation areas should have a smaller impact on productivity if there is an abundance of land elsewhere in the city.

Columns (3) and (7) report the factor non-neutral specification. Again, the first stage coefficient of the instrument has the wrong sign for the non-neutral variable, suggesting that instrumentation is not successful in this model. In the fixed effects model, however, the designation parameter is unaffected by the inclusion of the interaction with the factor price difference. The interacted variable itself is insignificant, implying factor neutrality is a reasonable assumption. This result is in line with that found by Albouy and Ehrlich [2012] for regulation across U.S. MSAs.

Finally, columns (4) and (8) report the effect of the cumulative version of the designation variable. The variable is insignificant in the fixed effects specification but significant in the instrumented model. Since the instrument is strong, the instrumented version is preferred, suggesting that the housing productivity effects of designation may increase over time.

The alternative specifications support the main result of the cost function step that designation increases housing costs by reducing housing productivity. The additional specification suggest that the effect may decrease with land availability and may be cumulative with time. The results also support the assumption of a factor neutral housing productivity impact.

5.3 Quality of life and conservation areas

Table 6 presents the estimates from the quality of life regression of equation (8). The same productivity shifters are used as in the respective first step cost functions. Columns (1)–(4) use the fixed effects model in both the cost function and quality of life steps and columns (5)–(8) use the instrumented model in both steps. In general, the parameters for the designation variables are positive and significant, implying designation increases quality of life. In the baseline fixed effects model of column (1), a one point decrease in designation is associated with a 0.077 point increase in quality of life expressed as a share of expenditure. Instrumenting designation in column (5) reveals a similar quality of life effect of 0.071. A similar estimate makes sense if designation was associated with trends in both positive and negative quality of life factors.

As stated, the overall welfare effect is computed as the quality of life effect minus the housing expenditure share times the housing productivity effect. Using the baseline fixed effects specification results in a welfare effect of $0.077 - (0.31 \times 0.159) = 0.028$ and using the baseline instrumental variables specification results in a welfare effect of $0.071 - (0.31 \times 0.433) = -0.063$. The fixed effects model suggests that designations are welfare-improving, whereas the instrumented model suggests designation worsens welfare. Since the fixed effects results are inconsistent under the presence of time-variant unobservables, and since the F-stats indicate the instrument is strong, the instrumented model is preferred. The average HMA increased its designation share by 1/10 points over the period, suggesting an effect on welfare of -0.63% of expenditure.[29] Given the mean income of £22,800 in 2004–05 this is equivalent to an income reduction of about £1,500 over the study period. As discussed in the theory, the validity of the welfare effect relies on there being relatively little quantity adjustments or sorting. In the case where such effects are large, the welfare benefits of designation are overestimated, implying that the welfare loss from designation is an underestimate.

Further, it should be noted that I have computed the overall welfare effect using point estimates. In order to obtain confidence intervals in another specification I estimate both steps together using seemingly unrelated regressions (SUR). This approach allows for computation of the welfare effect as a linear combination of coefficients across models. Following this approach implies a 90% confidence interval with a lower bound of -0.005 and an upper bound -0.126. Essentially, the welfare loss could range from almost zero to around double the point estimate.

For planning refusals, the quality of life effects are very small in the instrumented specification. Given that the housing productivity effects were negative, this suggests that planning refusals are welfare-decreasing. However, since the planning variable is volatile and is not instrumented, this should not be taken at face value.

Columns (2) and (6) examine the quality of life effects when interacting the designation variable with the Saiz index dummies. As in the cost function step, the first stage for land abundant cities has an unexpected sign, suggesting that instrumentation was unsuccessful and that the fixed effects results are preferred. The fixed effects model shows a significant positive impact on quality of life for areas with land scarcity but an insignificant effect in areas with a land abundance. The coefficient for land scarce areas implies slightly larger quality of life effects than in the baseline specification. Together with the first step, these results imply designations will have both larger housing productivity and larger quality of life effects in cities with a scarcity of developable land. This result makes sense since it is unlikely that designation will have quality of life effects without impacting on housing productivity.

Columns (3) and (7) investigate the quality of life effects allowing for non-factor-neutral productivity shifts from designation. As with the first step, the instrumentation appears to fail when predicting the non-neutral designation variable. I therefore concentrate on

[29] This effect refers to an average homeowner in a city, and there may be a distribution of effects depending on whether the household lives inside or nearby a conservation area.

the fixed effect results where the substantive conclusions are unchanged from the baseline specification. This result supports the evidence from the cost function step that indicated that factor neutrality was a reasonable assumption.

Finally, columns (4) and (8) investigate the quality of life effects for the cumulative version of the designation variable. The cumulative designation effects in the instrumented specification are larger than the baseline effects, but the overall welfare effect remains negative (although smaller in magnitude).

5.4 Robustness checks

In order to check the sensitivity of these results, I estimate a number of robustness checks in Appendix B in the web appendix. These checks are (i) unweighted aggregation of the data to the HMA-level, (ii) using only new properties (<5 years old),[30] (iii) using the full sample of 74 HMAs (not excluding the eight with few transactions), (iv) using a quality of life measure that uses regional variation in the marginal tax rate and the share of wages in income, (v) using a quality of life measure with a high expenditure share on housing, (vi) using a quality of life measure with a low expenditure share on housing, (vii) using a quality of life measure where the expenditure share varies according to city income, and (viii) repeating the IV specification using prices from the hedonic model without date bands interacted with year effects. As discussed in Appendix B, the broad results are robust to these changes. In Appendix B, I also demonstrate that the approach is robust to using local authorities (LAs) as the unit of observation.

6 Conclusion

This paper provides evidence of the effects of conservation area designation on economic welfare. I constructed a unique panel dataset for English housing market areas (HMAs) that resemble city regions. In the first step of the empirical strategy I estimated the housing productivity effect of designation using a cost function approach. Here, I regressed house prices on factor prices (land and construction costs) and productivity shifters (including designation). In the second step I estimated the quality of life benefits and the overall welfare impact of designation. I regressed a quality of life index on housing productivity

[30] Using only new properties follows Ahlfeldt and McMillen [2014] who argue that accurate estimates of the land cost share and elasticity of substitution will be biased by a depreciation of the capital component for older housing stock.

factors used in the first step. I implemented both stages using panel fixed effects and IV approaches.

The main results imply that conservation area designations (in England, 1997–2007) are associated with both negative housing productivity effects and positive quality of life effects. In the cost function step, increases in designation share lead to higher house prices (compared with land values) indicating a negative shift in housing productivity. The second step reveals that designation leads to increases in quality of life. However, the overall welfare effect is negative in the instrumented and preferred specification. This result is in line with previous evidence that suggests housing regulation is welfare-decreasing.

The negative welfare impact of designation over 1997–2007 makes sense if there are concave returns to designation. The areas most worthy of protection may have been designated in the policy's first 30 years of operation (1966–1996). More recent designations then would be of a less distinctive heritage character, and perhaps only of local significance. Designation has continued despite large costs incurred at the housing market level, since, as argued by Ahlfeldt et al. [2017], designation status is largely determined by local homeowners who stand to gain from the localised benefits of the policy.

Nevertheless, though, there may be significant heterogeneity in the quality of conservation areas over the studied period and the results of this paper do not suggest that all of these conservation areas reduced welfare. Furthermore, there may be significant heterogeneity across individuals, for example, the designation would be more welfare-improving for individuals with a greater than average preference for heritage or with a less than average expenditure share on housing. Overall, though, the results suggest that the average household would have been better off without the average conservation area being designated in the period between 1997 and 2007 in England. This overall welfare improvement of these designation not being made would have been equivalent to about £1,500 per household.

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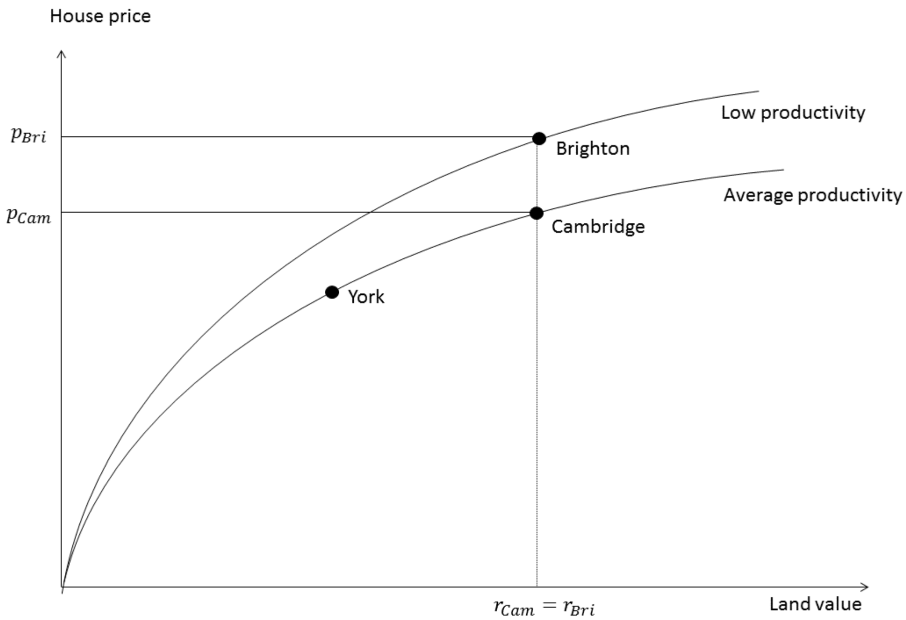
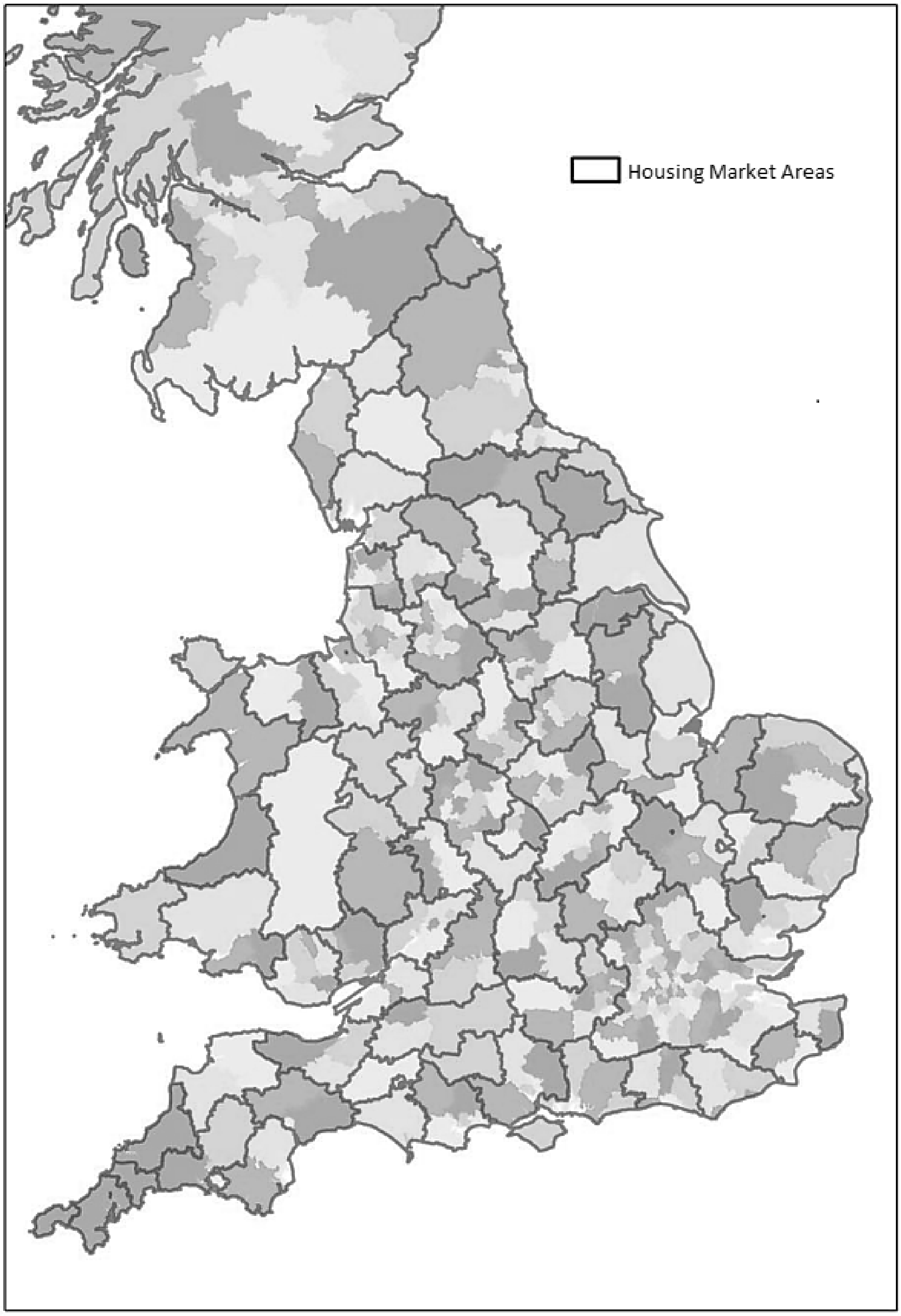
Figure 2: Housing productivity example



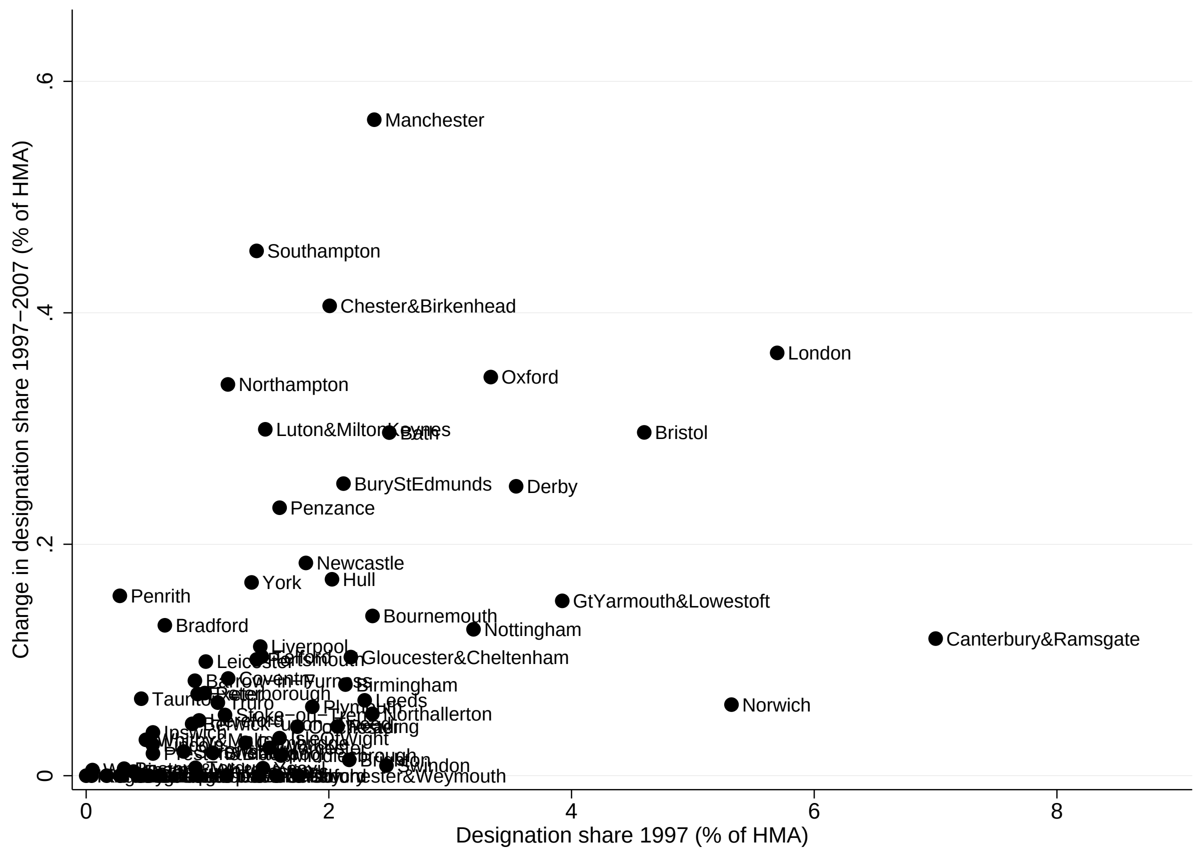
Notes: This figure is an adaptation of Figure 1A from Albouy and Ehrlich [2012].

Figure 3: Housing markets areas over local authorities



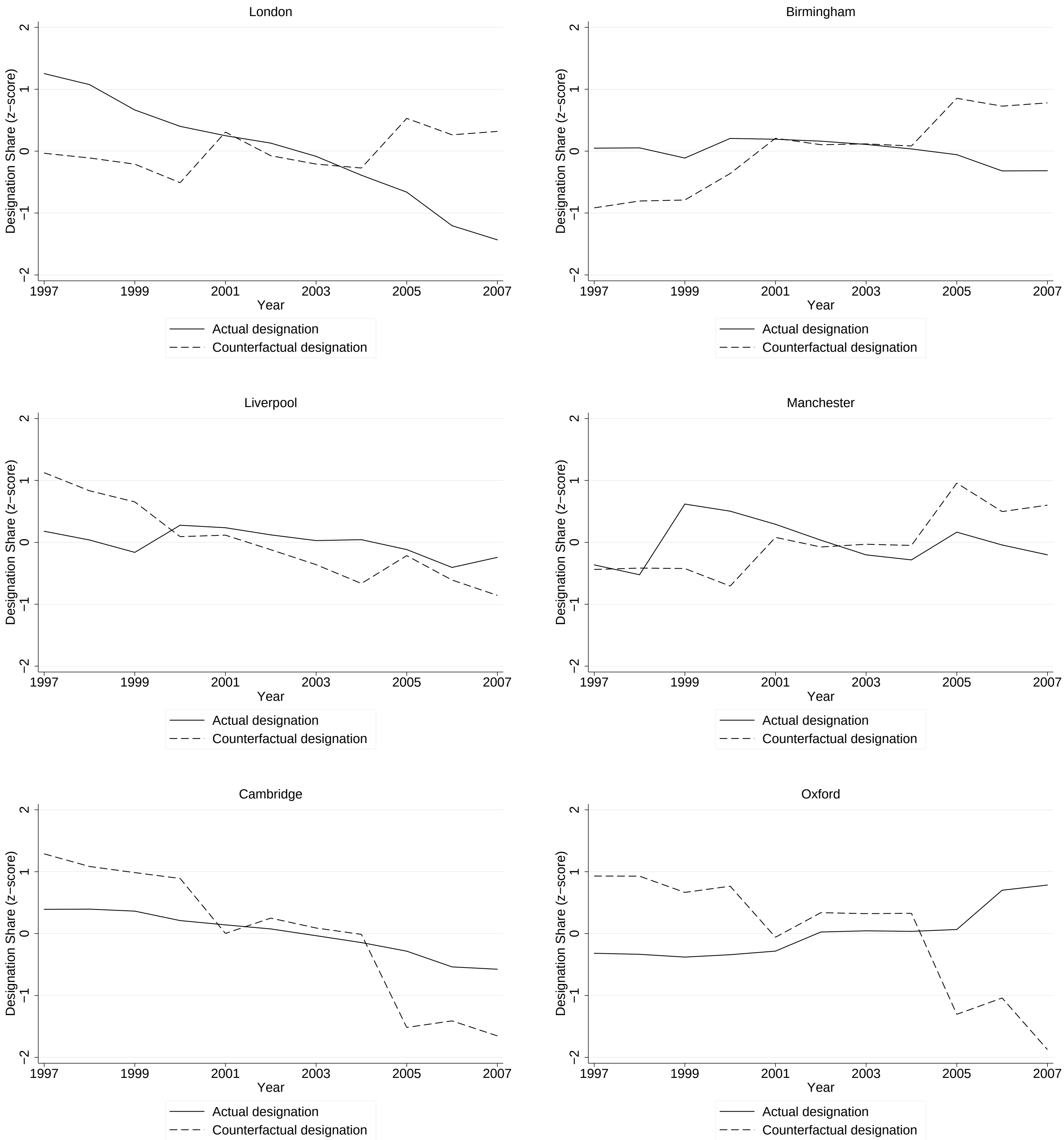
Notes: Map P11.4 from *Geography of housing market areas* by DCLG [2010].

Figure 4: Initial designation share against change for housing market areas (HMAs)



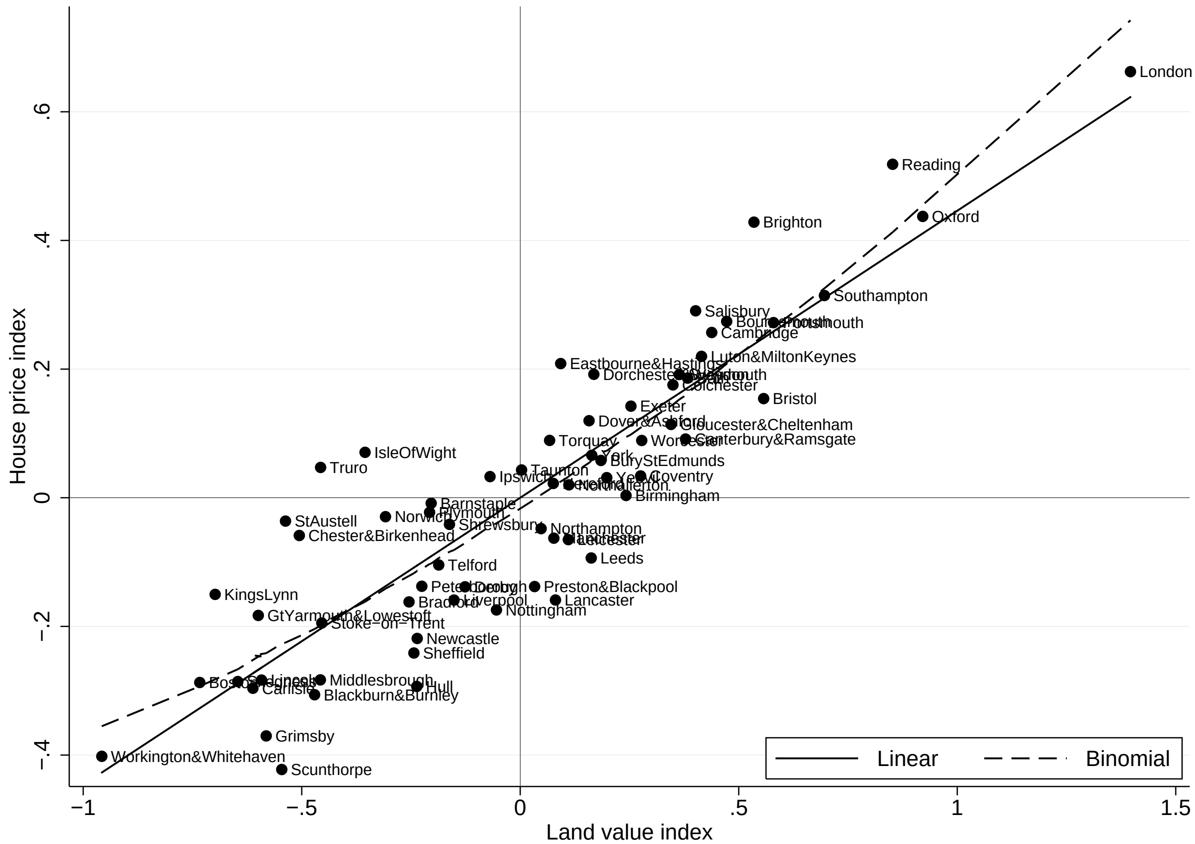
Notes: Blackburn & Burnley HMA is not depicted since the change in designation share over the period is off the chart at 2.6% of the land area.

Figure 5: Actual designation against counterfactual designation for selected HMAs



Notes: Designation (z-scores) have been adjusted for HMA fixed effects giving them a zero mean across years for each HMA. The shares are also conditional on trend interactions.

Figure 6: House prices vs land values for English Housing Market Areas



Notes: These trend lines depict predicted values from simplified versions of equation (7). The linear version of the simple regression is: $\bar{\bar{p}}_j = \beta_1 \bar{\bar{v}}_j + \beta_3(\bar{\bar{r}}_j)^2$ where the bar accent signifies the average across years for each HMA.

Table 1: Descriptive statistics for panel dataset

	Overall statistics				Between-group			Within-group		
	Mean	Sd	Min	Max	Sd	Min	Max	Sd	Min	Max
Variables (standard)										
House price (£000s)	118.2	52.90	41.6	345.9	27.30	76.6	220.2	45.43	7.077	243.8
Land value (£000s / ha)	1486.5	1046.2	183.4	7964.4	740.1	600.1	5197.3	744.5	-1420.7	4253.7
Const. cost index	136.7	26.31	93.1	207.0	7.025	123.2	160.4	25.37	93.20	193.7
Designation share	0.0176	0.0136	0.000004	0.0712	0.0136	0.000004	0.0711	0.00126	0.0103	0.0363
Refusal rate	0.230	0.112	0	0.625	0.0601	0.1	0.389	0.0952	-0.0893	0.588
Variables (normalised)										
Price differential	-0.000	0.231	-0.573	0.775	0.225	-0.422	0.662	0.055	-0.176	0.164
Land value diff.	-0.000	0.483	-1.450	1.458	0.455	-1.012	1.216	0.169	-0.569	0.639
Const. cost diff	-0.000	0.054	-0.160	0.192	0.050	-0.101	0.160	0.021	-0.063	0.102
Desig. share (z-value)	-0.000	0.993	-4.824	1.765	0.997	-4.798	1.725	0.072	-0.603	1.040
Pred. refusal (z-value)	0.000	0.993	-4.514	2.352	0.910	-2.850	1.942	0.411	-1.879	1.335

Notes: Descriptive statistics for panel dataset with 66 cities $\times$ 11 years = 726 observations overall. Standard variables are: the price of a house with average characteristics in each HMA (based on predictions from hedonic regression), the residential land value (mean of bulk, small and flats), the construction cost index (100=UK average in 1996), the share of HMA land that is designated, and the planning application refusal rate. The normalised versions of the variables are those used in the empirical analysis after being processed as described in the data section. The 'diff.' variables are log differentials (in each year) and hence have a mean of zero. The 'z-value' variables are additionally divided by the standard deviation in each year and hence have a between-group standard deviation of approximately one.

Table 2: Degree share regression

	(1) OLS	(2) OLS	(3) FE & Trends	(4) FE & Trends
Designation share	0.010*		−0.002	
	(0.005)		(0.002)	
Counterfactual designation		0.001		−0.062
		(0.005)		(0.043)
F-stat	3.619	0.029	0.965	2.069
R^2	0.068	0.000	0.002	0.023
AIC	-543.0	-532.7	-1239.3	-1242.4
Numbers of HMAs	74	74	74	74
Observations	148	148	148	148

Notes: The dependent variable is degree share (differential) in 2001 and 2011. Fixed effects and trends are implemented by demeaning and detrending the variables beforehand. This pre-step was carried out using two separate samples: (i) annual data over 1997–2007 for the designation shares and (ii) Census data for 1991, 2001 and 2011 for the degree share. The data were then merged for two years, 2001 and 2011. The designation shares for 2007 were used for 2011 as this is the closest possible match. Standard errors in parentheses are clustered on HMAs.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Housing quantity regression

Dep. Var.: Log Dwelling count	Output Area		Housing Market Area		
	OLS (1)	FE (2)	OLS (3)	FE (4)	IV (5)
Conservation area land share	0.022***	0.001	0.003*	−0.000	−0.001
	(0.001)	(0.002)	(0.002)	(0.000)	(0.001)
R^2	0.010	0.872	0.085	1.000	1.000
Number of Areas	165665	165663	74	74	74
Observations	1655727	1655725	740	740	740

Notes: Regressions of logged dwelling count on conservation area land share at the city (Housing Market Area, HMA) and very local (Output Area, OA) levels. Conservation area land share has been scaled to give the effect of an average-sized designation for each geography. The fixed effects specification includes HMA trends, and the IV specification includes a trend variable interacted with the initial value of the instrument and designation share. The regressions demonstrate that designation does not significantly decrease housing quantity. The pooled OLS specifications in columns (1) and (3) demonstrate a significant *positive* relationship most likely due to unobservables. However, there is no effect once fixed effects have been included in columns (2) and (4) and when instrumenting in column (5) for HMAs with the counterfactual designation share used for the main specifications in this paper. Standard errors in parentheses are clustered on the geographical units.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Cost function

	Panel Fixed Effects				Instrumental Variable			
	Cobb-Douglas		Translog		Cobb-Douglas		Translog	
	Unrestr.	Restrict.	Unrestr.	Restrict.	Unrestr.	Restrict.	Unrestr.	Restrict.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Land value differential	0.178***	0.172***	0.180***	0.175***	0.176***	0.173***	0.185***	0.181***
	(0.022)	(0.022)	(0.021)	(0.021)	(0.013)	(0.014)	(0.012)	(0.012)
Constr. cost differential	0.553***	0.828***	0.527***	0.825***	0.635***	0.827***	0.544***	0.819***
	(0.138)	(0.022)	(0.123)	(0.021)	(0.119)	(0.014)	(0.110)	(0.012)
Conservation area land	0.159***	0.158***	0.169***	0.165***	0.480***	0.600***	0.496***	0.433***
share (z-value)	(0.036)	(0.036)	(0.038)	(0.037)	(0.131)	(0.117)	(0.123)	(0.087)
Predicted refusal rate	0.099***	0.092***	0.101***	0.087***	0.027***	0.030***	0.034***	0.026***
(z-value)	(0.021)	(0.019)	(0.022)	(0.019)	(0.008)	(0.009)	(0.008)	(0.007)
Land value differential			0.021	0.018			0.074***	0.058***
squared			(0.026)	(0.020)			(0.013)	(0.011)
Constr. cost differential			−1.409	0.018			-0.261	0.058***
squared			(1.249)	(0.020)			(1.257)	(0.011)
Land value differential			−0.101	-0.035			-0.717***	−0.117***
× Constr. cost diff.			(0.262)	(0.041)			(0.234)	(0.022)
R^2	0.975	0.975	0.976	0.975	0.950	0.940	0.951	0.954
AIC	-2488.9	-2480.9	-2491.1	-2482.2	-2348.7	-2381.4	-2363.0	-2363.0
Number of HMAs	66	66	66	66	66	66	66	66
Observations	726	726	726	726	726	726	726	726
p-value for CRS		0.048		0.095		0.105		0.001
p-value for CD	0.595	0.098			0.000	0.000		
p-value for all restrictions		0.169				0.000		
Elasticity of substitution	1.000	1.000		0.755	1.000	1.000		0.213
F-stat of instruments					33.73	33.73	38.08	38.08

Notes: Fixed effects and IV regressions of equation (7). The dependent variable is the house price differential. All columns include HMA fixed effects. Fixed effects models include individual HMA trends. IV models include a trend variable interacted with the initial value (in 1997) for the instrument, and with other city characteristics. The instrument is the counterfactual designation share given by equation (9) and first stages for the restricted translog model are reported in Appendix B in the web appendix. The elasticity of substitution is computed as $\sigma^Y = 1 - 2\beta_3/[\beta_1(1-\beta_1)]$. Standard errors in parentheses are clustered on HMAs.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Alternative specifications for cost function

	Panel Fixed Effects				Instrumental Variable			
	(1) Baseline model	(2) Undev. interact	(3) Factor non-neut.	(4) Cumulat. desigat.	(5) Baseline model	(6) Undev. interact	(7) Factor non-neut.	(8) Cumulat. designat.
Conservation area land share (z-value)	0.165***		0.164***		0.433***		0.947***	
	(0.037)		(0.038)		(0.087)		(0.235)	
CA land share × Above-average undev. share		0.189***				0.327***		
		(0.034)				(0.119)		
CA land share × Below-average undev. share		0.052				-3.437***		
		(0.117)				(0.995)		
CA land share × (land value diff. − construction cost diff.)			−0.008				0.238***	
			(0.027)				(0.088)	
Cumulative conservation area designation (z-value)				0.767				1.368***
				(0.510)				(0.270)
R^2	0.975	0.975	0.975	0.975	0.954	0.886	0.893	0.946
AIC	-2482.2	-2745.5	-2743.3	-2481.9	-2363.0	-2385.7	-2381.4	-2370.3
Numbers of HMAs	66	66	66	66	66	66	66	66
Observations	726	726	726	726	726	726	726	726
p-value for CRS	0.095	0.093	0.075	0.108	0.001	0.521	0.401	0.013
Elasticity of substitution	0.755	0.740	0.724	0.740	0.213	-0.022	2.535	0.350
F-stat of instruments					38.08	6.45	3.60	48.98

Notes: Alternative regressions of the restricted translog cost function. Columns (1)–(4) are variants of fixed effects model from Tab. 4, col. (4) and columns (5)–(8) of the IV model from Tab. 4, col. (8). The dependent variable is the house price differential. Only conservation area variables differ from baseline specification and to save space the other variables are not presented in this table. The instrument is the counterfactual designation share given by equation (9) and first stages are reported in Appendix B in the web appendix. The reported F-stat of instruments is the Cragg-Donald statistic in columns (6) and (7) where there is more than one instrument. Standard errors in parentheses are clustered on HMAs.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Quality of life regressions

	Panel Fixed Effects				Instrumental Variable			
	(1) Baseline model	(2) Undev. interact	(3) Factor non-neut.	(4) Cumulat. desigat.	(5) Baseline model	(6) Undev. interact	(7) Factor non-neut.	(8) Cumulat. designat.
Conservation area land share (z-value)	0.077***		0.074***		0.071**		0.163***	
	(0.019)		(0.020)		(0.033)		(0.045)	
CA land share × Above-average undev. share		0.088***				0.046*		
		(0.025)				(0.028)		
CA land share × Below-average undev. share		0.024				-0.845***		
		(0.032)				(0.234)		
CA land share × (land value diff. − construction cost diff.)			−0.008				0.029***	
			(0.009)				(0.008)	
Cumulative conservation area designation (z-value)				0.294***				0.388***
				(0.074)				(0.087)
Predicted refusal rate (z-value)	0.040***	0.040***	0.042***	0.041***	0.002	0.005*	0.006**	−0.000
	(0.010)	(0.010)	(0.009)	(0.010)	(0.002)	(0.003)	(0.003)	(0.002)
Residuals	0.266***	0.266***	0.266***	0.266***	0.245***	0.256***	0.190***	0.291***
	(0.026)	(0.026)	(0.024)	(0.026)	(0.024)	(0.046)	(0.031)	(0.026)
1st step specification	Tab. 4, Col. (4)	Tab. 5, Col. (2)	Tab. 5, Col. (3)	Tab. 5, Col. (4)	Tab. 4, Col. (8)	Tab. 5, Col. (6)	Tab. 5, Col. (7)	Tab. 5, Col. (8)
R^2	0.955	0.955	0.955	0.954	0.934	0.934	0.928	0.937
AIC	-4121.2	-4120.1	-4259.9	-4247.5	-3836.5	-3829.3	-3769.5	-3864.8
Observations	726	726	726	726	726	726	726	726
F-stat of instruments					63.62	18.51	32.21	70.04

Notes: Fixed effects and IV regressions of equation (8). Dependent variable is a quality of life index computed according to equation (5). All columns include HMA fixed effects. Fixed effects models include individual HMA trends. IV models include a trend variable interacted with the initial value (in 1997) for the instrument, and with other city characteristics. The instrument in columns (5)–(8) is the counterfactual designation share given by equation (9) and first stages are reported in Appendix B in the web appendix. The reported F-stat of instruments is the Cragg-Donald statistic in columns (6) and (7) where there is more than one instrument. The welfare impact can be computed as the coefficients here minus the share of expenditure on housing times the coefficients from the first step specification. For example, for column (5) a one-point increase in designation results in a 0.071-(0.31*0.433)=-0.063 decrease in welfare expressed as a share of expenditure. Standard errors in parentheses are clustered on HMAs.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Web appendix for: The preservation of historic districts – is it worth it?

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Abstract

This is the web appendix to 'The preservation of historic districts – is it worth it?'. It is not intended as a standalone document but presents data and results that support but were not central to the analysis presented in main paper. Appendix A presents the appendix that relates to the data section of the main paper. Appendix B presents the appendix that relates to the results section of the main paper.

Keywords: housing, planning, regulation, historic preservation, construction, land
JEL: H89, L51, L74, D62, R21, R31, R38, R52, R58

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Appendix A Data

A.1 Weights

As described in the data section, the different data are aggregated to Housing Market Areas (HMAs) using weights. The weighting variable used in each case is described in Table A1. Weights were used even where it would have been possible to compute the data directly at the HMA level, such as for designation share, in order to maintain consistency across variables. In tables B8 and B9 I estimate the model with unweighted versions of all these variables and the results are robust.

A.2 Hedonic regression

Table A2 below presents the results of the hedonic regression of equation (4) from the main paper. The 1,184 HMA-year effects themselves are omitted to save space. The coefficients on the property characteristics are mostly significant and are 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 large premiums of between 21% and 24%. The lowest premium is observed for houses built in the 1970s. 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 up until about 100 years of age when it begins to reverse. Houses with 90–99 and over 100 years of age 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 of age. This could be attributed to the effect of an accumulation of character over the years which begins to set in at around 90 years of age.

A.3 Land value data and validation

A.3.1 Land valuations regression

In order to produce an overall land value index from the valuations data, I regress logged value on a dummy variable for the bulk site category plus local authority (LA) fixed effects in each year. Small sites were therefore the baseline category. I ignored values for flat sites since these were not available for all LAs and were not likely to be representative values. The results of this regression are presented in Table A3. These results confirm that bulk land is considerably cheaper (4.9%–11.2%) than small plots in each year, consistent with the 'plattage effect' [as documented by e.g. Colwell and Sirmans, 1993]. The land value index is then constructed as the average of the two predicted values (small and bulk sites) for that LA. This adjusts for price differences between site categories.

A.3.2 Valuations data

The Valuation Office Agency describes the need to base its land value index on expert valuations rather than on transactions:

> The problem with the transactions data was that in London and, to some extent the South East, the number of building plots with planning permission sold on the open market has, in recent years, fallen to such a trickle that it is no longer possible to calculate a reliable average price based on transactions. Developers tend now to buy up old houses, factories or disused warehouses – paying the market price for the buildings – then demolish the existing buildings and put up a new development. The advantage of the approach based on valuation, is that the data set is based on a consistent set of valuations over time and is not skewed by atypical sales that may distort regional averages when sale volumes are limited (VOA document. File reference: hlpr4-r-cy).

Since land transaction prices will be discounted for (unobservable) demolition costs which vary systematically across location and time, an index based on a hedonic approach would be unreliable. Expert assessors, on the other hand, apply detailed knowledge of the market to provide estimates of the value of land when transacted under normal conditions.

A.3.3 Validating land valuations with transaction prices

I make use of data on 923 plots of land sold at auction over the period 2000–2012.[1] This represents too few observations to be used to construct a time-varying land value index suitable for empirical analysis. However, they can serve to validate the index based on valuation. Figure A1 plots the auction land values against land valuations for individual LAs.[2] There is a strong fit with the land valuations, especially given that the auctions-based index uses an average of only eight observations per LA. The fitted values align closely with the 45 degree line and the (highly significant) coefficient for the valuations is almost exactly equal to one (0.999). The R^2 of 0.721 demonstrates a very strong correlation between the auction prices and the valuations.

The valuations-based index from Figure A1 is equal to the LA fixed effects from a regression of logged price per hectare on site category (as Table A3 above, but pooled across years). The auctions-based index is equal to the LA fixed effects from a regression of logged price per hectare on plot characteristics (Table A4). Both regressions also include year effects. The baseline category in the auctions regression is vacant land with outline residential planning permission – the same as the type of land reflected by the land valuations data. The estimate for plot size confirms that the plattage effect holds for the auction prices, too.

A.4 Construction price index

The construction price index data was taken from the Regional Supplement to the Guide to House Rebuilding Costs published by the Building Cost Information Service (BCIS). The BCIS produces the index using a hedonic approach and data on tender prices for accepted construction projects. The index and sample sizes are available at the local authority (LA) level but not for every LA in every year. Figure A2 plots the share of LAs 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.

For counties with missing LAs, the county value was compared with the available LAs in that country to interpolate the values of the missing LAs. This was made possible by the fact that the index reports the sample size for each LA and for the county as a whole.

[1] I begin with a dataset of 12,845 plots but drop those which lack information on the size of the plot and the sale price, plus those that do not have a residential planning permission status of 'applied for', 'outlined', or 'fully granted' and are thus too dissimilar to the land valuations.

[2] In order to improve precision, I only include the 61 LAs that have at least five auction transactions.

This enabled a calculation of the number of observations that were in missing LAs and their average value. The average value for the 'rest of county' for county c was computed as:

$$v_{ROC,c} = \frac{v_c s_c - \sum_d v_{d,c} s_{d,c}}{s_c - \sum_d s_{d,c}} \tag{A1}$$

where v_c is the county value, s_c is the county sample size, $v_{d,c}$ is the value for available LA d in county c, and $s_{d,c}$ is the sample size for those LAs. I then worked backward from the most complete year (2008) applying 'rest of county' growth rates to actual figures where necessary to interpolate for LAs missing in earlier years.

Whilst the number of missing LAs 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 LAs within a county have similar factors, since they are all treated as an aggregate 'rest of county'. Where they differ significantly, this will only matter when they end up being aggregated to different HMAs later on in the process. Finally, the filled LAs are likely to be smaller ones with less dwellings and so will contribute less when eventually aggregated with other LAs to the HMAs level.

A.5 Quality of life rankings

Table A5 presents the HMAs when ranked by the quality of life index. It also lists values for the differentials and z-scores 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 is nevertheless reassuring.

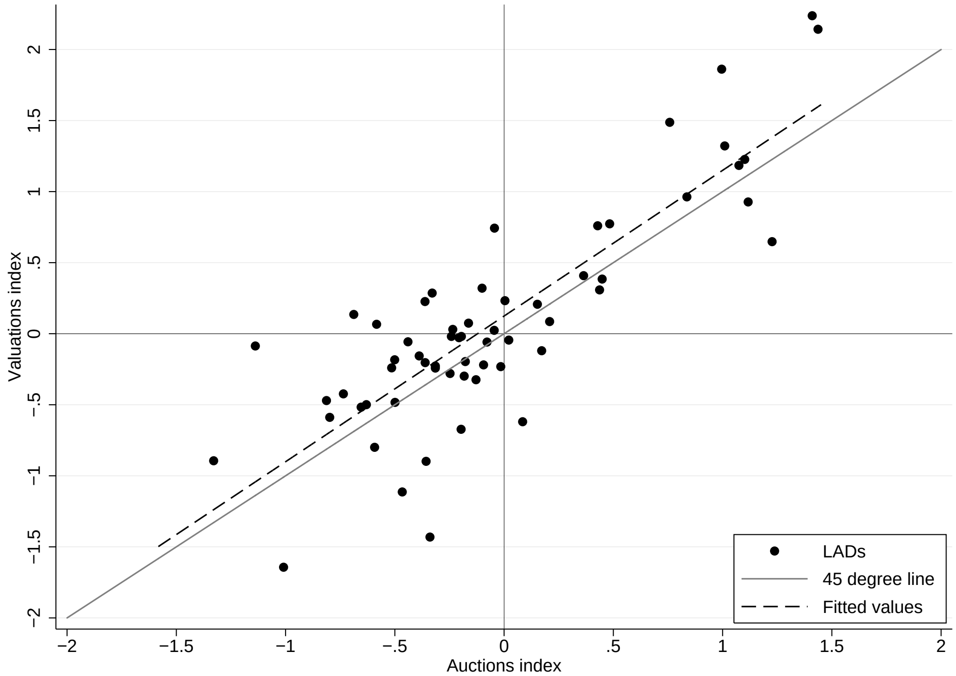
A.6 Counterfactual designation shares

The instrument for designation is the interaction of the national level (excluding own HMA-j) designation share in each build date band $D_{b-1,t}$, with HMA-j's initial shares of housing stock in each build band H_{jb0}. Of the 1,095,945 observations in the transaction dataset I keep only units where the build date was pre-1997 (the study period). The first column of Table A6 illustrates the distribution of the remaining 969,417 observations across build date bands. I assume that a unit transacted in any year exists at the same location from their build year y until the end of the study period in 2007. I define the unit as having designation status in year t (= 1997, 1998, ...2007) if it is inside a conservation area with a designation date before or equal to t. The remaining columns of Table A6 illustrate the national-level designation shares for units within each build date band. This national level designation shares in this table are similar to the $D_{b-1,t}$ variable, except the influence of own HMA-js has not been removed. The share is computed as $D_{bt} = N_{D=1,bt}/N_b$ where $N_{D=1,bt}$ is the number of units in age band b that are inside a designated conservation area in time period t and N_b is the total number of units in age band b.

To compute age-band shares for the initial HMA-stock I used only Nationwide observations for 1995–1996. The calculation is $H_{jb0} = N_{jb0}/N_{j0}$ where N_{jb0} is the number of units in HMA-j and age band j sold in $t = 0$ (1995-1996), and N_{j0} is the number sold across all bands.

The Nationwide data is, of course, not a representative sample of all housing in each HMA. Some units may not have been transacted in the period and will not appear in the dataset at all. Others will appear multiple times. However, a transactions dataset may be the best source of detailed property characteristics (unit build date) with a precise geographical breakdown (inside/outside conservation areas). The fixed effects and year effects (differentials) used in the empirical specification should remove most of the bias introduced by regional and annual differences in the types of property sold.

Figure A1: Auctions data against land valuations



Notes: The auctions index is equal to the LA fixed effects from a regression of logged price per hectare on plot characteristics (Table A4). The valuations-based index is equal to the LA fixed effects from logged price per hectare on site category (as Table A3 but pooled across years). In order to improve precision, I plot here only the 61 LAs that have at least five auction transactions.

Figure A2: Share of missing construction price factors at LA level, 1997–2007

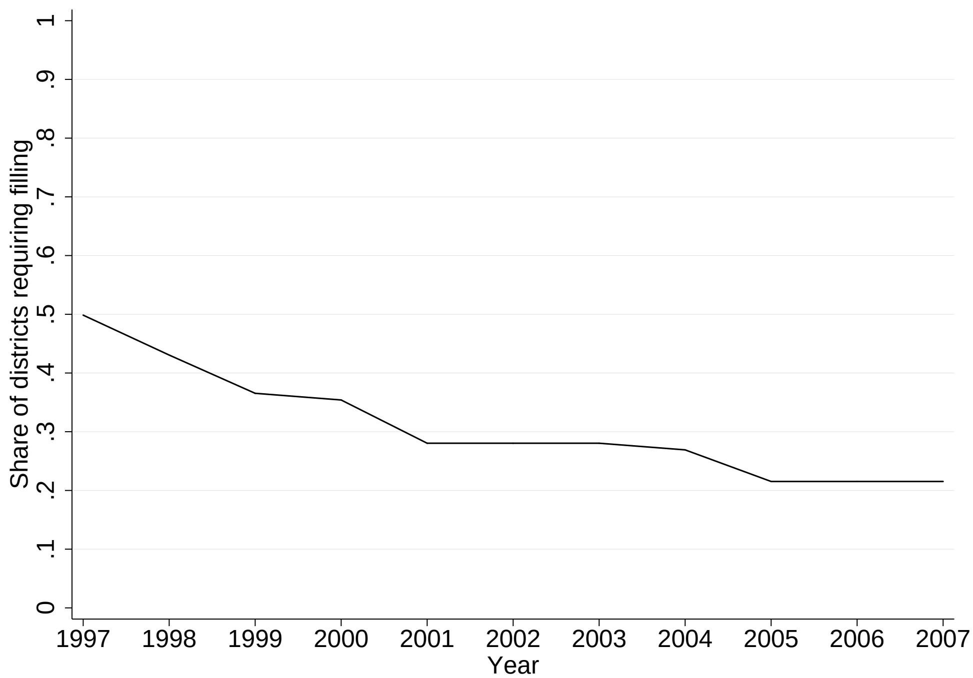


Table A1: Weights used to aggregated to HMA level

Variable	Source data unit	Weight variable
Land values	LA	LA Dwellings 2003
Refusals	LA	LA Dwellings 2003
Construction costs	LA	LA Dwellings 2003
Protected land shares	LA	LA Dwellings 2003
House prices	Postcode	LA Dwellings 2003÷LA count
Council tax	LA	LA Dwellings 2003
Amenities	Postcode	LA Dwellings 2003÷LA count
Wages	LA	LA Employment 2003
Population density	LA	no weight
Designation	LA	LA Dwellings 2003
Saiz index	HMA	no weight

Notes: The Saiz index is left unweighted as it reflects exogenous factors. The population variable does not require weighting. LA refers to local authority. HMA refers to Housing Market Areas. LA count refers to the transaction count in the LA in each year from Nationwide data.

Table A2: Hedonic regression

	(1) log 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)
Detached house	−0.032
	(0.026)
Semi-detached house	−0.157***
	(0.023)
Terraced house/Country cottage	−0.251***
	(0.021)
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)
Single garage	0.106***
	(0.010)
Double garage	0.163***
	(0.016)
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***

Continued on next page

Table A2: Hedonic regression – continued from previous page	
	(1)
	log price
	(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: 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)
Build date: pre-1900	0.355***
	(0.079)
R^2	0.850
AIC	232410.5
Number of HMA-Years	1184
Observations	904075

Notes: Regression of equation (4) from main paper. The dependent variable is log house price. The omitted category for Build date is post-2000; for Building age it is 0–9 years; for House type it is Flat/Maisonette; and for Parking type it is No parking. Standard errors in parentheses are clustered on HMA-years.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Land valuations regression

	(1) 1997	(2) 1998	(3) 1999	(4) 2000	(5) 2001	(6) 2002	(7) 2003	(8) 2004	(9) 2005	(10) 2006	(11) 2007
Bulk site	−0.112***	−0.096***	−0.078***	−0.067***	−0.059***	−0.056***	−0.049***	−0.051***	−0.061***	−0.068***	−0.070***
	(0.009)	(0.008)	(0.008)	(0.008)	(0.009)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.004)
Constant	6.587***	6.711***	6.809***	6.955***	7.105***	7.277***	7.505***	7.675***	7.818***	7.894***	7.982***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
R^2	0.985	0.988	0.989	0.991	0.990	0.995	0.994	0.995	0.995	0.994	0.995
Observations	703	706	706	707	707	707	708	708	708	708	708

Notes: The dependent variable is logged land values. Bulk sites are valuations for sites of two hectares or larger. The omitted category is small sites (<2 ha). Standard errors in parentheses are clustered on LAs.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Land value auctions regression

	(1) Dep. var.: log value per ha
Plot size (hectares)	−0.046***
	(0.010)
Commericial land use	0.379
	(0.430)
Agricultural land use	−2.750***
	(0.235)
Non-vacant	0.099
	(0.511)
Res. planning: applied for	−0.364
	(0.455)
Res. planning: fully granted	−0.042
	(0.104)
Com. planning: outlined	0.004
	(0.378)
Com. planning: fully granted	0.156
	(0.268)
Constant	6.613***
	(0.352)
R^2	0.600
Observations	923

Notes: The dependent variable is logged land value (£000s) per hectare. Year effects are also included. Only residential land is included in the regression but some plots have multiple land use types. The variables 'commercial land use' and 'agricultural land use' refer to suggested land use type, whereas the planning variables describe the extent to which permission has been given to build. The baseline category is vacant land with outline residential planning permission. Standard errors in parentheses are clustered on LAs.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: HMA characteristics ranked by quality of life

HMA name	Quality of life	House price diff.	Land value diff.	Constr. diff.	Des. share (z-val.)	Re-fusals (z-val.)	Saiz index (z-val.)	Pop. dens. (z-val.)
Penzance	0.175	0.120	-0.523	0.021	0.403	0.788	1.742	-1.578
Launceston and Bude	0.121	-0.003	-0.589	-0.018	-0.850	0.215	1.089	-1.323
Dorchester and Weymouth	0.116	0.202	0.222	0.025	0.422	0.464	0.882	-0.731
Torquay	0.109	0.100	0.121	0.007	-0.199	0.449	0.943	-0.026
Eastbourne and Hastings	0.096	0.219	0.146	0.113	-0.976	1.233	0.485	0.150
Barnstaple	0.093	0.002	-0.150	0.013	-1.239	0.840	0.653	-0.768
Truro	0.084	0.058	-0.403	-0.030	-0.009	0.331	1.398	-0.582
Berwick-upon-Tweed	0.083	-0.207	-0.663	-0.010	-0.219	-3.825	0.994	-2.403
Whitby and Malton	0.078	0.000	-0.235	-0.047	-0.730	-1.033	0.027	-1.760
Exeter	0.077	0.153	0.307	-0.006	-0.137	0.598	0.214	-0.104
IsleOfWight	0.071	0.081	-0.302	0.039	0.340	1.251	1.637	-0.879
Hereford	0.069	0.033	0.129	-0.052	-0.142	0.352	-0.228	-0.425
Brighton	0.068	0.439	0.588	0.100	0.627	1.005	0.525	0.842
Salisbury	0.067	0.301	0.455	0.033	0.317	0.770	-0.917	-1.031
Portsmouth	0.063	0.283	0.633	0.068	0.266	0.558	0.404	0.844
Bournemouth	0.058	0.285	0.526	0.031	0.733	1.473	0.224	0.409
London	0.046	0.673	1.450	0.161	1.567	1.018	-0.484	3.277
StAustell	0.042	-0.026	-0.484	-0.023	-0.743	0.757	1.184	-1.161
Worcester	0.041	0.100	0.332	0.004	0.289	0.313	-0.876	0.088
Oxford	0.040	0.448	0.975	0.030	1.059	0.601	-1.626	0.546
Northallerton	0.038	0.031	0.165	-0.024	0.717	-0.190	-0.455	-0.904
Kendal	0.036	0.034	0.007	-0.045	-2.872	-1.049	1.061	-1.126
BuryStEdmunds	0.034	0.069	0.238	0.015	0.677	0.243	-1.744	-0.740
Penrith	0.034	-0.136	-0.636	0.051	-1.027	-0.117	0.743	-1.777
Colchester	0.033	0.186	0.403	0.059	0.424	0.239	0.434	0.610
Taunton	0.032	0.054	0.057	-0.017	-0.727	-0.004	1.129	-0.301
Bath	0.031	0.197	0.436	0.005	0.835	0.932	-0.903	0.300
Canterbury and Ramsgate	0.027	0.102	0.432	0.113	1.744	0.302	1.411	0.076
Southampton	0.023	0.325	0.750	0.038	0.427	1.317	-0.342	0.704
Plymouth	0.022	-0.012	-0.154	-0.027	0.502	0.497	0.265	0.078
Yeovil	0.015	0.042	0.251	-0.001	0.251	-0.493	-0.736	-0.747
Norwich	0.013	-0.019	-0.255	-0.020	1.474	-0.403	-1.539	0.491
Dover and Ashford	0.008	0.130	0.211	0.069	0.032	0.779	0.843	-0.487
Telford	0.000	-0.094	-0.133	-0.045	0.295	-0.107	-0.870	-0.452
Ipswich	-0.006	0.043	-0.016	-0.002	-0.642	-0.434	-0.361	0.168
Shrewsbury	-0.006	-0.031	-0.108	-0.033	-0.296	0.684	-0.770	-0.551
Skegness	-0.006	-0.275	-0.593	-0.047	-1.290	0.080	0.323	-0.870
Gloucester and Cheltenham	-0.012	0.124	0.399	0.014	0.654	0.616	-0.022	0.333
GtYarmouth and Lowestoft	-0.016	-0.172	-0.546	-0.029	1.198	0.060	1.206	-0.466
York	-0.017	0.077	0.217	-0.014	0.264	0.269	-1.474	-0.242
KingsLynn	-0.018	-0.140	-0.645	-0.001	-4.437	-0.697	0.500	-0.848
Scarborough	-0.019	-0.143	-0.268	-0.014	-1.744	-2.070	1.227	-1.089
Luton and MiltonKeynes	-0.024	0.230	0.468	0.067	0.333	0.705	-1.544	0.961

Continued on next page

Table A5: HMA characteristics – continued from previous page								
HMA name	Quality of life	House price diff.	Land value diff.	Constr. diff.	Des. share (z-val.)	Re-fusals (z-val.)	Saiz index (z-val.)	Pop. dens. (z-val.)
Cambridge	-0.024	0.267	0.492	0.035	0.167	0.293	-1.132	0.436
Carlisle	-0.024	-0.286	-0.558	-0.005	-0.577	-2.641	0.112	-1.116
Swindon	-0.026	0.202	0.417	0.019	0.750	0.238	-0.791	0.127
Birmingham	-0.028	0.014	0.296	-0.027	0.634	0.096	-1.441	2.019
Stoke-on-Trent	-0.028	-0.184	-0.400	-0.039	0.053	0.239	-1.145	0.770
Boston	-0.029	-0.277	-0.680	-0.043	-1.174	-0.191	0.718	-0.840
Northampton	-0.035	-0.037	0.101	0.006	0.142	-0.540	-1.467	0.589
Bristol	-0.038	0.165	0.611	0.012	1.380	0.765	0.794	0.837
Peterborough	-0.039	-0.127	-0.172	-0.015	-0.086	-0.533	-0.957	0.171
Leicester	-0.039	-0.055	0.164	-0.050	-0.068	-0.616	-1.852	0.911
Preston and Blackpool	-0.040	-0.127	0.086	0.008	-0.639	-0.035	0.930	0.613
Newcastle	-0.041	-0.208	-0.182	-0.037	0.507	-1.517	-0.483	1.622
Blackburn and Burnley	-0.043	-0.296	-0.417	0.016	-0.051	-0.419	0.245	0.262
Lincoln	-0.043	-0.273	-0.538	-0.052	-0.630	-0.527	-1.525	-0.229
Manchester	-0.045	-0.052	0.130	0.001	0.826	0.084	0.497	1.865
Chester and Birkenhead	-0.047	-0.048	-0.452	0.011	0.619	-0.028	0.477	0.370
Reading	-0.050	0.529	0.906	0.097	0.588	1.559	-1.197	1.277
Leeds	-0.051	-0.083	0.216	-0.049	0.680	0.010	-0.877	1.556
Coventry	-0.053	0.044	0.329	-0.013	0.072	-0.966	-1.545	0.588
Bradford	-0.056	-0.151	-0.201	-0.092	-0.458	0.345	0.290	0.702
Nottingham	-0.059	-0.164	-0.001	-0.059	1.006	-0.167	-1.395	1.034
Hull	-0.068	-0.283	-0.183	-0.014	0.583	-0.037	0.255	0.487
Liverpool	-0.068	-0.149	-0.098	-0.001	0.272	-0.582	0.298	1.592
Sheffield	-0.069	-0.231	-0.190	-0.030	-0.059	-0.338	-1.072	1.336
Derby	-0.070	-0.128	-0.073	-0.075	1.124	-0.670	-0.497	0.542
Middlesbrough	-0.089	-0.273	-0.404	-0.046	0.346	-1.496	1.111	0.447
Lancaster	-0.094	-0.148	0.134	-0.055	-0.181	-0.472	1.037	-0.861
Grimsby	-0.098	-0.360	-0.528	-0.100	0.228	0.256	0.966	-0.726
Barrow-in-Furness	-0.109	-0.361	-0.624	0.037	-0.185	-0.889	1.742	-1.463
Workington and Whitehaven	-0.146	-0.391	-0.904	0.037	-2.712	-0.571	1.248	-0.693
Scunthorpe	-0.172	-0.412	-0.492	-0.048	-0.720	0.032	0.006	-0.731

Notes: Figures for time-variant data represent the mean across all time periods for each HMA. The Saiz index and Population density are time-invariant.

Table A6: Distribution of transactions across build date bands

Band	Total	Share designated by year											
		1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	Growth 97-07
Built 0-1870	41,251	0.396	0.397	0.398	0.399	0.400	0.401	0.402	0.402	0.403	0.404	0.405	0.025
Built 1870s	7,630	0.353	0.355	0.357	0.359	0.360	0.361	0.363	0.364	0.366	0.366	0.368	0.041
Built 1880s	20,317	0.302	0.304	0.305	0.306	0.307	0.310	0.312	0.314	0.315	0.316	0.317	0.052
Built 1890s	30,149	0.200	0.201	0.202	0.203	0.205	0.208	0.212	0.212	0.213	0.214	0.215	0.074
Built 1900s	134,455	0.114	0.115	0.117	0.117	0.119	0.121	0.122	0.123	0.124	0.125	0.126	0.103
Built 1910s	23,111	0.092	0.093	0.094	0.094	0.095	0.096	0.097	0.098	0.099	0.099	0.099	0.083
Built 1920s	28,363	0.069	0.070	0.071	0.071	0.072	0.073	0.073	0.073	0.074	0.074	0.075	0.080
Built 1930s	144,424	0.028	0.029	0.029	0.029	0.030	0.031	0.031	0.031	0.031	0.031	0.032	0.140
Built 1940s	13,073	0.026	0.026	0.027	0.027	0.027	0.027	0.028	0.028	0.028	0.028	0.028	0.079
Built 1950s	80,764	0.024	0.024	0.024	0.024	0.024	0.025	0.025	0.025	0.026	0.026	0.026	0.094
Built 1960s	103,778	0.029	0.030	0.030	0.030	0.030	0.031	0.031	0.031	0.031	0.031	0.031	0.067
Built 1970s	123,645	0.026	0.026	0.026	0.027	0.027	0.027	0.027	0.027	0.027	0.027	0.027	0.048
Built 1980s	125,409	0.035	0.036	0.036	0.036	0.036	0.037	0.037	0.037	0.037	0.037	0.037	0.046
Built 1990-1996	93,048	0.038	0.038	0.038	0.038	0.038	0.038	0.038	0.038	0.039	0.039	0.039	0.030
Total	969,417	0.073	0.074	0.075	0.075	0.075	0.076	0.077	0.077	0.077	0.078	0.078	0.065

Notes: Share of units inside designated conservation areas based on Nationwide transaction data. Each observation sold in any year 1995–2010 is assumed to exist throughout the sample period. Time variation in designation share is, therefore, driven only by new designations.

Appendix B Results

B.1 Estimations using alternative spatial units

As discussed, for the approach taken in this paper it is important to use areas that are highly self-contained (i.e. people who live in a given area also work in that area) in order to relate to the theoretical model and to capture supply (and demand) effects that play out at the level of the housing market. However, it is common to check that results are robust to using observational units of different scales to address the modifiable areal unit problem, or MAUP [Fotheringham and Wong, 1991]. It is not possible to use an alternative city definition such as travel-to-work-areas (TTWAs) since these do not aggregate from local authorities (LAs). However, I am able to use the LA level itself to estimate both the cost function and the quality of life step at this spatial scale. This represents a potentially useful robustness check under the caveat that LAs are not self-contained areas. People often live in one LA and work in another LA in the same city, making them incompatible with the theoretical locations, and unlikely to capture the full effects of designation.

I present the results in Table B1 where column (1) is the baseline HMA specification and column (2) reports the LA specification. The results imply a smaller housing productivity effect but a similar quality of life effect when using the LA scale. To some extent these results are to be expected since cost-driven increase in prices will impact on the housing market level, whereas quality of life impacts are more likely to be spatially confined. If designation impacts on housing productivity in one LA in a city, it will impact on prices throughout the city since housing units that offer access to the same labour market are substitutable and in competition with one another. This explains why examining the LA specification shows a lower productivity effect. On the demand side, Ahlfeldt et al. [2017] demonstrate that the amenity effects of conservation areas tend to be spatially localised, which helps to explain why the quality of life estimates are almost unchanged moving to the smaller LA units. Overall, at the LA level, conservation areas are approximately welfare-neutral. However, as discussed this is likely because it misses the broader supply-side impact on housing costs. The robustness check is useful since it helps to reveal the different spatial scales of the effects and to confirm that approach works using alternative spatial units.

B.2 Investigating potential employment effects

The analysis assumes that the major effects of conservation area designation are captured through the quality of life and housing productivity effects. However, if designation fosters tourism, it could impact on welfare by creating jobs in a city. I investigate this possibility in Table B2 by regressing the logged job count on the designation share at the HMA level. Whilst the fixed effects specification reveals a small negative effect, this unexpected result is likely due to unobservables not captured by the trends. In the instrumented specification, there is no effect of designation on jobs. This finding is supportive of the approach taken in this paper and reflects existing evidence that suggests improvements to the amenity value of housing mainly impacts on the property market, and not on productivity or employment [What Works Centre for Local Economic Growth, 2015].

B.3 Estimating the cost function with instrumented land values

Table B3 shows that instrumentation of land values increases the land cost share from 19% to 29% in both the Cobb-Douglas and translog specifications. The larger instrumented land cost share is consistent with an "old land buyer's rule of thumb" that land represents about one third of the total value of housing [Hudson, 2015]. The Durbin-Wu-Hausman test easily rejects the null hypothesis that land values are exogenous, suggesting that instrumentation is necessary to obtain a consistent estimate of the land cost share. The increase in the land cost share suggests that unobserved housing productivity is negatived correlated with land values. Since instrumentation addresses this bias, the 29% value is the preferred estimate for the land cost share presented in this paper.

The F-stat for excluded instruments is 8.02 in the Cobb-Douglas model (with one endogenous regressor and one instrument) and 2.6 in the translog model (with three endogenous regressor and three instruments). According to Stock and Yogo [2005], the 20% maximal size critical value for one endogenous regressor and one instrument is 6.66. Therefore, for the Cobb-Douglas the null hypothesis of a weak instrument is rejected for a maximal IV size somewhere between 15% and 20%. The same critical value for three endogenous regressors is not reported, therefore, it is not possible to make a similar comparison for the translog F-stat. Nevertheless, the critical values are declining with both number of endogenous regressors and number of instruments and the value for two regressors and two instruments is as low as 3.95. Given this, it is possible that the translog model is of similar strength to the Cobb-Douglas specification.

B.4 Robustness checks

Table B8 and Table B9 report robustness specifications for the fixed effects and IV models, respectively. The first row gives the cost function results for designation variables and the last two rows give the quality of life results. The six regression statistics, including the elasticity of substitution, refer to the cost function step. The baseline model is repeated in column (1) in each case for comparison. Column (2) in both tables gives the results without weighting the variables in aggregation to HMA from LA. The results do not differ greatly from the baseline specification, suggesting they are not sensitive to the inclusion of weights. In column (3) in both tables, I estimate the model using only new properties from the house price dataset. Here, the focus is on the elasticity of substitution parameter which increases to 0.402 in the instrumented version. The housing productivity effect is slightly lower in magnitude and the quality of life effect is larger, but this is perhaps mainly due to reduced sample size and decreased precision. In column (4) in both tables I estimate the model using the full sample of HMAs, without excluding the eight HMAs with fewer than 100 transactions per year. Here, the results in the cost function step are broadly consistent with the main specifications, although the elasticity of substitution is smaller. The quality of life effects are insignificant in the instrumented model, most likely because of the inclusion of imprecise unit house price estimates.

In columns (5)–(8) I test the robustness of the welfare results to different specifications of the quality of life measure. Column (5) allows for regional variation in the share of wages in total income and regional-year variation in the marginal tax rate. There is almost no change in the effect on quality of life, suggesting the results are not sensitive to using average values for these shares. In columns (6), (7) and (8) I test for robustness to the assumed share of housing in total expenditure. In the main specifications I use a share of 0.31 which results in an overall welfare effect of -0.06 of expenditure. However, as recreated in Table B7, the *Expenditure and Food Survey, 2006* reports that the share of expenditure on housing varies significantly by income decile. Overall, the shares tend to be larger for poorer households than for richer households. Notably, though, the smallest share is for the poorest households, most likely an effect of housing benefits. The highest share across income deciles is 0.415 and the smallest is 0.263.

In columns (6) and (7) I use a quality of life index computed according to equation (5) from the main paper using these highest and lowest shares, respectively. Notably, the highest value is almost exactly equivalent to using a quality of life index that follows Albouy and Ehrlich [2012] in taking account of local variation in non-housing prices by adding one sixth of housing costs to the index. Therefore the result for this specification may be interpreted as either a test for local non-housing price variation or for a high expenditure share on housing. Column (6) reports a larger quality of life impact compared with the baseline specification and column (7) a smaller impact. However, while changes to the share of expenditure on housing imply different quality of life effects, they also imply different costs due to housing productivity. In fact, the overall welfare effect for the higher expenditure share is $0.101 - 0.433 \times 0.415 = -0.07$ which is a larger welfare reduction than for the baseline model, despite the greater quality of life benefit. For the smaller cost share it is $0.057 - 0.433 \times 0.263 = -0.05$.

In column (8), I use a definition of the quality of life measure where the share of expenditure varies for each city based on its average income. Specifically, I compute for each city the income decile that it falls in based on its average income, and use the equivalent housing expenditure share from Table B7 in calculating the quality of life index for that city. Based on their average incomes, the cities fall anywhere from the 4th to the 8th decile resulting in a variation in the housing expenditure share between 0.272 and 0.364. The resulting quality of life estimate of 0.082 is slightly larger, although broadly similar to the baseline. However, the average expenditure share when shares vary according to city income is slightly larger at 0.325. Therefore the final welfare effect is $0.082 - 0.433 \times 0.325 = -0.06$, i.e. the same as in the baseline specification. Overall then, the columns (6)-(8) show that welfare effects do not deviate very far from the baseline specification despite using a very wide range of housing expenditure shares.

Finally, the instrumented table has an extra column (9) where I use house prices without interacting age bands with trends in the hedonic regression. The main results are robust to this change, suggesting that this factor does not threaten the exclusionary restriction. Overall, these alternative models appear to support the broad conclusions of the main specifications.

Table B1: Local authority specification

	(1) HMA (baseline)	(2) LA
Conservation area	0.433***	0.247***
land share (z-value)	(0.087)	(0.047)
Land value differential	0.181***	0.244***
	(0.012)	(0.011)
Constr. cost differential	0.819***	0.756***
	(0.012)	(0.011)
Land value differential	0.058***	0.032***
squared	(0.011)	(0.010)
Constr. cost differential	0.058***	0.032***
squared	(0.011)	(0.010)
Land value differential	−0.117***	−0.063***
× Constr. cost diff.	(0.022)	(0.020)
Predicted refusal rate	0.026***	0.031***
(z-value)	(0.007)	(0.004)
R^2	0.954	0.783
AIC	-2363.0	-5712.6
Number of areas	66	282
Observations	726	2970
p-value for CRS	0.001	0.003
Elasticity of substitution	0.213	0.658
Desig. effect on QoL	0.071**	0.078***

Notes: The first column is the baseline model at the HMA level and the second column is exactly the same but at the LA level. Other variables are same as in baseline specifications. Standard errors in parentheses are clustered on geographical unit.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: Employment effects

Dep. Var.: Log Employment count	FE (4)	IV (5)
Conservation area land share	−0.000**	0.002
	(0.000)	(0.003)
R^2	0.999	0.998
HMAs	74	74
Observations	1036	1036

Notes: Regressions of logged jobs count on conservation area land share at HMA level, 1997–2010. Conservation area land share has been scaled to give the effect of an average-sized designation. The fixed effects specification includes HMA trends, and the IV specification includes a trend variable interacted with the initial value of the instrument and designation share. Standard errors in parentheses are clustered on HMAs.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: Cost function with instrumented land values

	Cobb-Douglas (1)	Translog (2)	Cobb-Douglas (3)	Translog (4)
Land value differential	0.184***	0.189***	0.274***	0.287***
	(0.011)	(0.011)	(0.047)	(0.046)
Constr. cost differential	0.816***	0.811***	0.726***	0.713***
	(0.011)	(0.011)	(0.047)	(0.046)
Land value differential squared		0.028***		0.090**
		(0.010)		(0.039)
Constr. cost differential squared		0.028***		0.090**
		(0.010)		(0.039)
Land value differential × Constr. cost diff.		−0.055***		−0.180**
		(0.020)		(0.078)
R^2	0.973	0.973	0.970	0.969
Observations	726	726	726	726
p-value for CRS			0.005	0.029
p-value for CD			0.021	
p-value for all restrictions			0.038	
Elasticity of substitution		0.640	1.000	0.117
F-stat of instruments			8.02	2.61
Durbin-Wu-Hausman (p-value)			0.000	0.000

Notes: Columns (1) and (2) are panel fixed effects and columns (3) and (4) additionally instruments land values with Bartik shift share. HMA fixed effects and trends in all columns.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: First stages for land value instrumentation

	(1)	(2a)	(2b)	(2c)
Bartik	7.082***	7.454***	−0.022	−0.121
	(2.501)	(2.569)	(2.936)	(0.177)
Constr. cost differential	2.757***	2.784***	−0.150	−0.157***
	(0.321)	(0.327)	(0.374)	(0.022)
Bartik squared		−2.965	81.179***	0.759
		(15.357)	(17.552)	(1.056)
Bartik × Constr. cost		5.154	21.881***	3.637***
		(5.007)	(5.722)	(0.344)
Constr. cost differential squared		−5.019	14.990***	3.355***
		(3.088)	(3.529)	(0.212)
R^2	0.929	0.930	0.811	0.927
Observations	726	726	726	726
F-stat of instruments	8.02	3.04	12.98	38.80

Notes: Column (1) relates to (3) and columns (2a), (2b) and (2c)to (4) in Table B3.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B5: First stage IV results for cost function regressions

	(1)	(2)		(3)		(4)
Second stage model:	Baseline Model	Undev. interact.		Factor non-neut		Cumulat. designat.
Instrumented var.:	Designat.	Des. × >avg. undev.	Des. × <avg. undev.	Designat.	Des. × (land val. − cons. cost)	Cumulat. designat.
Excluded Instruments						
Counterfactual designation (z-value)	2.241***			2.238***	−3.054***	
	(0.363)			(0.363)	(0.800)	
Counterfactual designation (z-value) × Above-average undev. share		3.576***	0.105			
		(0.420)	(0.128)			
Counterfactual designation (z-value) × Below-average undev. share		0.534	−0.550***			
		(0.517)	(0.158)			
Counterfactual des. share × (land value diff. − constr. cost diff.)				0.013	−0.137***	
				(0.017)	(0.036)	
Cumulative counterfactual designation (z-value)						1.709***
						(0.244)
Other variables						
Land value differential	−0.010	−0.011	0.000	−0.010	0.001	−0.006
	(0.017)	(0.015)	(0.005)	(0.017)	(0.036)	(0.006)
Constr. cost differential	−0.147	−0.170	0.056	−0.152	−0.068	−0.039
	(0.139)	(0.129)	(0.039)	(0.139)	(0.306)	(0.054)
Land value differential squared	−0.035**	−0.029*	0.005	−0.027	0.849***	−0.003
	(0.016)	(0.015)	(0.005)	(0.019)	(0.042)	(0.006)
Constr. cost differential squared	−4.343***	−4.499***	−0.254	−4.323***	14.478***	−1.234**
	(1.566)	(1.455)	(0.444)	(1.566)	(3.446)	(0.610)
Land value differential × Constr. cost diff.	1.171***	0.994***	0.069	1.108***	−5.669***	0.228**
	(0.258)	(0.240)	(0.073)	(0.270)	(0.595)	(0.102)
Predicted refusal rate (z-value)	−0.036***	−0.035***	0.005**	−0.036***	0.024	−0.001
	(0.009)	(0.008)	(0.002)	(0.009)	(0.019)	(0.003)
R^2	0.996	0.996	0.998	0.996	0.954	0.999
Observations	726	726	726	726	726	726
F-stat of instruments	38.08	36.38	6.68	19.33	14.56	48.98

Notes: The first stage estimations for the cost function regressions in Table 5 columns (5)–(8) from the main paper. The instrumented variables (dependent variables) are variations of the conservation area designation variable. The expected sign of the instrument is positive. All columns include HMA fixed effects and a trend variable interacted with the initial value (in 1997) for the instrument and with other city characteristics. Standard errors in parentheses are clustered on HMAs.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B6: First stage IV results for quality of life regressions

Second stage model:	(1) Baseline Model	(2) Undev. interact.		(3) Factor non-neut		(4) Cumulat. designat.
Instrumented var. (productivity from):	Designat.	Des. × Above-average undev.	Des. × Below-average undev.	Designat.	Non-neutral Designat.	Cumulat. Designation
Excluded Instruments						
Counterfactual designation (z-value)	2.493***			2.177***	−3.535***	
	(0.313)			(0.260)	(0.965)	
Counterfactual designation (z-value) × Above-average undev. share		3.739***	0.085			
		(0.408)	(0.072)			
Counterfactual designation (z-value) × Below-average undev. share		0.422	−0.536***			
		(0.513)	(0.091)			
Counterfactual des. share × (land value diff. − constr. cost diff.)				0.026**	−0.501***	
				(0.010)	(0.039)	
Cumulative counterfactual designation (z-value)						1.598***
						(0.191)
Predicted refusal rate (z-value)	−0.041***	−0.040***	0.005***	−0.042***	−0.005	−0.003
	(0.007)	(0.007)	(0.001)	(0.006)	(0.022)	(0.002)
Residuals	−0.657***	−0.136***	0.188***	−0.593***	−0.669***	−0.279***
	(0.045)	(0.030)	(0.005)	(0.024)	(0.090)	(0.015)
R^2	0.997	0.996	0.999	0.998	0.928	1.000
Observations	726	726	726	726	726	726
F-stat of excluded instruments	63.62	42.10	18.54	38.84	91.97	70.04

Notes: The first stage estimations for the quality of life regressions in Table 6 columns (5)–(8) from the main paper. The instrumented variables (dependent variables) are variations of the conservation area designation share. The expected sign of the instrument is positive. Included variables not shown are: land values, construction costs and their squares, and the interaction of land values and construction costs. All columns include HMA fixed effects and a trend variable interacted with the initial value (in 1997) for the instrument and with other city characteristics. Standard errors in parentheses are clustered on HMAs.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B7: Expenditure shares on housing by income decile

Decile	Weekly income	Share
1	Up to £142	0.263
2	£143 to £214	0.415
3	£215 to £292	0.356
4	£293 to £383	0.272
5	£384 to £496	0.274
6	£497 to £623	0.364
7	£624 to £762	0.301
8	£763 to £938	0.301
9	£939 to £1,273	0.295
10	£1,274 and over	0.328

Notes: Reproduced from *Expenditure and Food Survey, 2006*.

Table B8: Robustness checks for fixed effects models

	(1) Baseline model	(2) Unweighted	(3) Only new properties	(4) Full sample	(5) Local tax & wage share	(6) High housing share (0.415)	(7) Low housing share (0.263)	(8) Income-varying housing share
Conservation area land share (z-value)	0.165*** (0.037)	0.136** (0.060)	0.136*** (0.031)	0.202*** (0.049)	0.165*** (0.037)	0.165*** (0.037)	0.165*** (0.037)	0.165*** (0.037)
R^2	0.975	0.973	0.958	0.969	0.975	0.975	0.975	0.975
AIC	-2482.2	-2453.1	-2376.0	-2642.0	-2482.2	-2482.2	-2482.2	-2482.2
Numbers of HMAs	66	66	66	74	66	66	66	66
Observations	726	726	726	814	726	726	726	726
p-value for CRS	0.095	0.093	0.000	0.036	0.095	0.095	0.095	0.095
Elasticity of substitution	0.755	0.673	0.742	0.554	0.755	0.755	0.755	0.755
Desig. effect on QoL	0.077***	0.057***	0.066***	0.076***	0.076***	0.106***	0.064***	0.120***

Notes: Robustness checks for the fixed effects model. The first row and six regression statistics are variations on the baseline cost function regression (first step) from Table 4, column (4) from the main paper. The last row reports the quality of life effect from a second step regression that relates to the first step in the same column. Only the conservation area variable is reported here. Remaining variables are: land values, construction costs and their squares, the interaction of land values and construction costs, and predicted planning refusal rate. All columns include HMA fixed effects and HMA trends. Standard errors in parentheses are clustered on HMAs.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B9: Robustness checks for instrumented models

	(1) Baseline model	(2) Unweighted	(3) Only new properties	(4) Full sample	(5) Local tax & wage share	(6) High housing share (0.415)	(7) Low housing share (0.263)	(8) Income-varying housing share	(9) Original prices
Conservation area land share (z-value)	0.433***	0.500***	0.440***	0.456***	0.433***	0.433***	0.433***	0.433***	0.461***
	(0.087)	(0.102)	(0.089)	(0.106)	(0.087)	(0.087)	(0.087)	(0.087)	(0.089)
R^2	0.954	0.950	0.930	0.948	0.954	0.954	0.954	0.954	0.952
AIC	-2363.0	-2357.6	-2302.8	-2534.7	-2363.0	-2363.0	-2363.0	-2363.0	-2359.2
Numbers of HMAs	66	66	66	74	66	66	66	66	66
Observations	726	726	726	814	726	726	726	726	726
p-value for CRS	0.001	0.004	0.000	0.006	0.002	0.002	0.002	0.002	0.002
Elasticity of substitution	0.213	0.161	0.402	0.054	0.213	0.213	0.213	0.213	0.217
Desig. effect on QoL	0.071**	0.076**	0.115***	−0.116	0.072**	0.101***	0.057*	0.082**	0.066**

Notes: Robustness checks for the instrumented model. The first row and six regression statistics are variations on the baseline cost function regression (first step) from Table 4, column (8) from the main paper. The last row reports the quality of life effect from a second step regression that relates to the first step in the same column. Only the conservation area variable is reported here. Remaining variables are: land values, construction costs and their squares, the interaction of land values and construction costs, and predicted planning refusal rate. All columns include HMA fixed effects and a trend variable interacted with the initial value (in 1997) for the instrument and with other city characteristics. Standard errors in parentheses are clustered on HMAs.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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