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How do Households Value the Future? Evidence from Property Taxes*

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Abstract Despite the near ubiquity of inter-temporal choice, there is little consensus on the rate at which individuals trade present and future costs and benefits. We contribute to this debate by estimating discount rates from extensive data on housing transactions and spatio-temporal variation in property taxes in England. Our findings imply long-term average discount rates that are between 3 and 4%. The close correspondence to prevailing market interest rates gives little reason to suggest that households misoptimise by materially undervaluing very long term financial flows in this high stakes context.

Keywords housing, property taxes, discount rate, capitalisation rate, undervaluation

JEL codes G10, R30

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Assumptions about discount rates feature in an array of economic models and in policy appraisals in settings such as climate change, infrastructure investment, and place-based policies. The rate at which we discount is a description of how we make decisions relating to the future, and may be informative about responses to policy interventions.[1] Revealed discount rates also provide a means to test whether individuals systematically undervalue the future (e.g. Busse *et al.*, 2013; Allcott and Wozny, 2014; Cohen *et al.*, 2017; De Groote and Verboven, 2018).

In this paper we exploit rich property tax and transaction data, the durability of bricks and mortar, and the high stakes nature of home purchases to make progress in addressing two related questions: *(i)* what rates do households use to discount over very long time horizons; and *(ii)* when compared to appropriate benchmark interest rates, do these rates imply departures from fully optimising behaviour? The starting point for our analysis is that for two houses identical in all respects except the second is liable to pay higher property taxes, one would expect the first house to trade at a premium to the second. This premium should equal the present value of the tax difference in perpetuity, from which we can work out how individuals are implicitly discounting the future. Should discount rates be close to individuals' inter-temporal opportunity cost of funds, then there is little evidence that households are misoptimising in their valuation of future taxes.

Taking this intuition to extensive house sales and tax data spanning around 20 years in England, our baseline approach exploits *inter-jurisdictional* variation in property taxes (Council Tax) by focusing on repeat sales of homes close to local administrative boundaries and including boundary-year fixed effects. We generate estimates by comparing homes of near-identical quality, exploiting assessment practices that group homes with similar values into 8 tax bands, and controlling for potential differences in the provision of public goods. In this way we mitigate the issue of correlation of (changes in) taxes with (changes in) unobserved characteristics of houses and neighbourhoods that have plagued

[1] Applying private discount rates to social projects is of course extremely contentious; e.g. the debates around Stern (2007) present opposing views within an environmental context.

previous studies (further described in Hilber, 2015). To counter any residual concerns that differences in public good provision across boundaries are driving our estimates, we show that very similar results can be obtained when we use *intra-jurisdictional* variation in taxes. This alternative approach relies on comparisons of neighbouring properties within the same jurisdiction, and hence which have access to the same public goods. Identification is achieved by retaining homes close to tax band thresholds and including threshold-year-location fixed effects. A causal interpretation of our findings is bolstered because findings across these specifications are largely insensitive to the inclusion of control variables, and a range of further sample and specification changes.

A well-established insight from urban public finance is that the effect of taxes on home prices is governed by both a capitalisation rate and a discount rate (e.g. Yinger, 1982; Ross and Yinger, 1999).[2] Hence, the discount rate can only be truly identified if we know the capitalisation rate. Given that the literature presents a very wide range of capitalisation parameter estimates, we next use ancillary data to identify the rental capitalisation rate for the period 2013-2016. We estimate that this rate is close to and not significantly different from 100%; that is to say, taxes are fully capitalised into rents in our setting. We then corroborate the rental capitalisation parameter by testing whether tax coefficients are different between places with elastic and inelastic housing supply, which is relevant because inelastic housing supply is consistent with full capitalisation (Hilber, 2015). We find that in the price regressions coefficients are only slightly (and not significantly) larger in absolute terms in places where housing supply is highly inelastic, which is not so surprising given the considerable evidence of very stringent regulatory restrictions on land and a scarcity of developable land in urban areas right across England (e.g Cheshire and Hilber, 2008; Hilber and Vermeulen, 2016).

[2] For the most part, scholars have focussed on estimating the former by making assumptions about how home-buyers discount the future. Findings overwhelmingly suggest that property taxes have non-negligible effects on home values, although the degree of capitalisation is less certain with the more plausible estimates falling in the range of 50%-100%; see e.g. the recent studies by Basten *et al.* (2017) and Lutz (2015) and reviews contained in Yinger (1982) and Ross and Yinger (1999).

Armed with estimates of tax implied discount rates, our next aim is to shed light on whether households are making systematic optimisation errors by undervaluing future property tax liabilities. Under the conservative assumptions of full capitalisation and that taxes are expected to grow at 0.8% per year in real terms, our baseline findings indicate that tax implied discount rates are at most 4.1% on average across our sample time frame. Comparing this to benchmark saving and borrowing rates prevailing during the same period gives us little reason to believe that households materially undervalue future tax liabilities when purchasing homes. We advance beyond previous studies by tracking the evolution of tax implied discount rates over time. Interestingly, we observe that implied discount rates only diverge from borrowing and savings rates following the onset of the financial crisis.

We contribute to several literatures. First, our article complements endeavours that estimate personal discount rates from field data or from experimental choices between relatively small stakes (and often hypothetical) rewards at specified future dates. The experimental evidence, which focuses on short time horizons, suggests that households place little weight on the future.[3] The evidence for longer horizons typically derives from observational data, is more sparse, and encompasses a very wide range of estimates (0-30%). Some studies centre on narrow groups in society or relatively unusual circumstances such as military downsizing (Warner and Pleeter, 2001), or energy efficient durable purchases (e.g. Hausman, 1979). Others obtain discount rates from structural models underpinned by a variety of assumptions (e.g. Gourinchas and Parker, 2002; Laibson *et al.*, 2007).

To the best of our knowledge, ours is the first paper that uses nationwide data on property taxes and housing transactions to obtain robust estimates of discounting parameters. The longevity of real estate makes housing markets amenable to the analysis of discounting over long horizons. Rates obtained from housing settings also benefit from a high degree of external validity because of widespread market participation and because households

[3] Frederick *et al.* (2002) review the experimental literature. Experiments that elicit rates over longer time spans are rare, although Grijalva *et al.* (forthcoming) (20 years) is an exception.

devote a significant share of spending to their homes.[4] In this context, the narrow range in our estimates of 3-4% provides a valuable new reference point for the literature on revealed personal discount rates. Besides this, understanding discounting in housing markets is also important in its own right as it may shed light on the relationship between interest rates and house prices, and because it may be useful to researchers attempting to establish annualised amenity values (see e.g. Chay and Greenstone, 2005).[5]

We also contribute to a literature that uses implied discount rates to test for departures from the standard assumptions that underpin traditional models of behaviour in economics (see DellaVigna, 2009). To date, work in this area has focussed on the valuation of energy efficient features of durable goods. In contrast to our findings, some such papers imply systematic undervaluation of short to medium term financial flows. For example, in an early study Hausman (1979) finds that purchases of air conditioners imply discount rates of around 20% while a later study in the same vein, Cohen *et al.* (2017), obtain discount rates of 11% in refrigerator transactions. De Groote and Verboven (2018) estimate that consumers apply discount rates of 15% in solar PV adoption decisions, which they interpret as considerable undervaluation of future benefits. Recent work has also centred on the pricing of fuel efficiency in automobile purchases. Allcott and Wozny (2014) find that a 15% discount rate rationalises purchases (against a real inter-temporal cost of funds of 6%), whereas Busse *et al.* (2013) and Sallee *et al.* (2016) interpret their results as being consistent with no (or else modest) undervaluation.

Our article also complements recent strands of housing market research. Beginning with

[4]Around 75% of households in the United Kingdom owned their homes in 2008. Piazzesi and Schneider (2016) show that housing services account for slightly under a fifth of total consumption (including durables) in the US.

[5]On the first point Glaeser *et al.* (2013) note that *"...the link between house prices and interest rates can be reduced substantially by weakening the connection between private discount rates and market interest rates. The standard asset market approach presumes that private discount rates and market rates always move together. This relationship means that lower current rates raise the present value of future appreciation, and hence increase current willingness to pay. The sizable impact of current discount rates on the value of future gains leads standard models to predict a large impact of interest rates on prices, especially in high price growth environments. But if private discount rates do not move with market rates, because buyers are credit constrained, then this channel is eliminated, and the connection between interest rates and prices is substantially muted."*

Genesove and Mayer (2001), researchers have looked for evidence of non-rational behaviour in housing markets. In a useful reference point for our study, Bradley (2017) suggests consumers are inattentive to property taxes in Michigan, although in contrast to our work this paper relates to specific shrouded tax features. Besides this, our study complements recent research that uses fixed-term leasehold tenure to reveal housing market discount rates that are low at very distant horizons (around 2%) and declining over the time horizon (Wong *et al.*, 2008; Giglio *et al.*, 2015a,b; Bracke *et al.*, 2018; Fesselmeyer *et al.*, 2016). Our paper can be distinguished from this literature because here we explicitly focus on the question of whether households are misoptimising, and because we use perpetual financial flows associated with property taxes rather than leasehold tenure to estimate discount rates. One advantage of this source of variation is that, unlike residential leasehold, property taxes are not specific to a small number of countries or a small share of homes. More crucially, our approach allows us to be more precise about the extent to which risk and expectations about future growth drive discount rate estimates.[6]

The remainder of the paper is structured as follows. In Section 1 we motivate our empirical work and discuss the institutional setting of our study. Section 2 describes the econometric framework, and is followed by a discussion of the data and the descriptives in Section 3. Section 4 presents our main results and in Section 5 we focus on recovering and interpreting discount rates. Section 6 reports some ancillary regressions and in Section 7 we conclude.

[6]Intuitively, property taxes are set within a policy framework and grow fairly steadily, while in contrast, housing is inherently risky and house price growth expectations are difficult to gauge, both because they are highly location and time specific and because households are prone to wild over-optimism (Case and Shiller, 2003; Shiller, 2015).

1 Background

1.1 Empirical framework

Our empirical work builds upon the urban public finance literature relating to the capitalisation of property taxes into home values. Following standard household bidding model assumptions including full household mobility, the equilibrium value (V_i) of home i can be decomposed into the present value of the flow of housing services minus the present value of the future stream of property tax payments:[7]

$$V_i = \underbrace{\frac{\rho}{r_H} H_i}_{pre-tax\ value} - \underbrace{\frac{\beta}{r_T} T_i}_{tax\ discount} \tag{1}$$

The first term in this capitalisation equation – the before tax value of the home – is the product of units of housing services (H_i) and the before tax implicit unit price of housing services ρ. The second term – the discount in home value due to tax – is the product of the annual property tax payment (T_i) and a tax capitalisation parameter (β). Both terms are expressed as present values by dividing by annualised discount rates, which can be interpreted as implied rates of return.

We denote the discount rate on the housing characteristics as r_H and the discount rate on taxes as r_T. Following earlier work, we assume that the pre-tax value and taxes are expected to grow at constant growth rates $E(g_H)$ and $E(g_T)$ such that r_H and r_T can be interpreted as net of growth discount rates. We put further structure on the gross discount rates by assuming they can be decomposed into a (common) risk free rate r^f and idiosyncratic risk premia r_H^p and r_T^p, which may vary across the two terms according to the riskiness of housing and tax flows respectively. Under these assumptions, $r_H = r^f + r_H^p - E(g_H)$ and $r_T = r^f + r_T^p - E(g_T)$.

[7] This equation can be equivalently derived from asset pricing or utility maximisation approaches (see e.g Yinger, 1982; Yinger *et al.*, 1988; Ross and Yinger, 1999)

Returning to equation (1), the underlying bidding model – which assumes perfect mobility of households and fixed housing supply – and a no arbitrage condition both suggest that the full present value of future taxes should be reflected in home values i.e. $\beta = 1$. Notwithstanding, the magnitude of β has been treated as an empirical question in a voluminous literature going back to Oates (1969). Faced with a fundamental difficulty in separately identifying β and r_T using home values, the vast majority of studies, reviewed in Yinger *et al.* (1988), Ross and Yinger (1999) and Hilber (2015), have estimated β from house prices and property taxes given assumptions about r_T.[8]

Estimates of capitalisation rates range from 0 (i.e. 0%, no capitalisation) to 1.4 (i.e 140%, more than full capitalisation). Yinger *et al.* (1988) show that part of this very substantial heterogeneity follows from variation in discount rate assumptions, but at least two further issues could plausibly drive differences. First, researchers have met identification challenges with varying degrees of success.[9] Second, capitalisation rates may themselves be determined by a number of factors including: *(i)* incomplete information; *(ii)* housing market frictions such as search costs and taxes, which lead to imperfect mobility; *(iii)* housing supply elasticities; and *(iv)* expectations about future taxes (Yinger, 1982; Ross and Yinger, 1999; Hilber, 2015). Arguably the most plausible estimates of β use quasi-experimental approaches to mitigate endogeneity concerns – in particular, Gallagher *et al.* (2013) find close to full (100%) capitalisation of property taxes into home values, whereas Lutz (2015) estimates fall in the range of 70% to 97% for homes in urban areas.

The advantage of using rents (R) rather than prices to estimate capitalisation rates is that a capitalisation parameter can be obtained without recourse to assumptions about

[8]The capitalisation rate is important because the extent to which house prices respond to fiscal variables *(i)* quantifies household responsiveness to fiscal conditions and is directly informative to understanding the consequences of policy; *(ii)* may influence behaviour through generating incentives that could differ between home-owners and renters; and *(iii)* may imply (possibly unintended) redistribution between different groups. For a more complete discussion of these issues see Hilber (2015).

[9]For example, in their review Yinger *et al.* (1988) find serious methodological shortcomings with all prior studies finding zero capitalisation. To the best of our knowledge, other than Elinder and Persson (2017), no more recent papers have found less than 40% capitalisation.

the discount rates r_T:

$$R_i = \rho H_i - \tilde{\beta} T_i \tag{2}$$

The parameter $\tilde{\beta}$ here can be related to the parameter in equation (1) if we assume that $R_i \approx V_i r_H$ and then multiply through equation (1) on both sides by a discount rate r_H. This yields a relationship between rents, home characteristics, and property taxes. In particular, when $r_T = r_H$, $\tilde{\beta}$ is directly informative about β. When $r_T \neq r_H$, the extent to which the capitalisation parameter in the rents equation provides a good proxy for the capitalisation parameter in the price equation depends on the extent of expected growth and the relative size of the risk premia since $\tilde{\beta} = \beta(r_H/r_T) = \beta(r^f + r^p_H - E(g_H))/(r^f + r^p_T - E(g_T))$.[10]

To date, only two studies have explicitly attempted to estimate r_T or r_H within a tax capitalisation setting. Using a small sample of home sales in California in the early 1990s and a cross-sectional research design, Do and Sirmans (1994) estimate a nominal discount rate r_T= 4% given assumed full capitalisation of taxes. The second, Palmon and Smith (1998), is perhaps the closest antecedent to our work. These authors use price and rent data to estimate capitalisation and discount parameters simultaneously (assuming $r_T = r_H$) by regressing imputed rent price ratios for some 450 homes in 1989 on effective property tax rates. Results suggest close to full capitalisation of taxes, and housing discount rates upwards of 9%. Our work improves on these studies by using better data and a much more convincing identification strategy.

[10] Two further points are worth noting. First, if rents can be obtained by dividing V_i by r_T then $\tilde{\beta}$ can be directly interpreted as β even if $r_T \neq r_H$. Second, the parameter $\tilde{\beta}$ in regressions of rents on property taxes has traditionally been taken to represent a "tax shifting" coefficient that measures the incidence on taxes on renters. The standard formula for the incidence of tax falling on the demand side is determined by the ratio of the demand elasticity ε_D to the sum of the demand and supply elasticities ε_S i.e. $\varepsilon_D/(\varepsilon_S - \varepsilon_D)$. This is analogous to the theoretical determinants of the capitalisation rate.

1.2 *Institutional setting*

We next describe core institutional information relevant to our empirical work, relegating a more complete account to Appendix A.1. The main units of local government in England are Local Authorities (LAs). LAs are responsible for local services including schools, social services, transport, local parks, and planning matters. Around of quarter of LA funding is raised from a local property tax, the Council Tax. Key features of this tax are: *(i)* it is payable on all domestic homes with the main exemptions being a 25% discount for those living alone and 100% discount for full time students; *(ii)* the tax falls on the home occupier, whether homeowner or renter; *(iii)* it is not deductible from income tax; *(iv)* LAs have wide-reaching enforcement powers and collection rates are very high; *(v)* the tax is simple and transparent.

Council Tax varies according to two main factors: annual LA tax setting decisions and a well publicised nationwide tax schedule for homes in different "tax bands" (Table 1). Tax bands are determined by an assessment of home value in 1991 (see Table 1 for the valuation thresholds). Older homes were assigned to tax bands in the early 1990s, while those built subsequently are assessed following construction. Re-banding is very rare: official data for 2010/11-2014/15 indicates that 0.2% of homes switch tax band each year, so the stock of homes in each band is essentially fixed.[11] As with the tax schedule, households are able to obtain information about the Council Tax band for individual homes easily e.g through online portals or through home sales agents.

Because homes rarely move tax bands, variation in Council tax largely arises through LA tax setting decisions. Figure 1 maps the tax for home in the middle tax band (Band D) for LAs in 2016/17. Some of the lowest Band D levies are in London, with Westminster and

[11] This reflects that there has been no systematic revaluation of homes in England since the initial valuations in the early 1990s. Homes can be "re-banded" following a successful appeal to the Valuation Office Agency (VOA), or when changes to the property are detected by officials and a new valuation concludes the property should be placed in a new band. Where physical improvements result in a re-valuation, the band is changed at the time of the next sale.

Table 1: Council Tax Bands and levies

Band	Value in 1991	Ratio to Band D levy
A	up to £40,000	6/9
B	£40,001 to £52,000	7/9
C	£52,001 to £68,000	8/9
D	£68,001 to £88,000	9/9
E	£88,001 to £120,000	11/9
F	£120,001 to £160,000	13/9
G	£160,001 to £320,000	15/9
H	£320,001 and above	18/9

Wandsworth the outliers with Band D levies of under £700 per year. At the other end of the spectrum are a mix of LAs including some cities (such as Nottingham and Oxford) and some rural areas (such as Weymouth and Portland and East Dorset). In some places adjacent LAs have very different council taxes with annual tax differences for comparable homes easily exceeding £500 per year. Figure 2 shows the average annual change in taxes between 1998/99 and 2016/17. One may observe some correlation between the level and the growth of the level of taxes (e.g. in Southwest London). However, the correlation between the level of taxes in 1998 and the average annual growth in taxes 1998/99-2016/17 is essentially zero (the correlation is only -0.014).

Council taxes increased more than inflation during the late 1990s and early 2000s under Labour governments. During this time, central government had powers to intervene to prevent "excessive" tax rises in LAs but rarely did so. Taxes grew more slowly thereafter. From around 2004 Council taxes have on average increased in line with inflation. This in part reflects policy interventions. In particular, since 2012/13 LAs wishing to raise taxes above 2%, or in some cases 4%, needs to put this to a local referendum (a path which no LAs has yet pursued). In addition, between 2010/11 and 2014/15, successive governments provided financial support to LA to fix nominal taxes. Under these policies, LAs that froze tax received some portion of foregone increases (usually up to 1%) from central government. The subsidies were withdrawn in 2015/16 and taxes have again begun to

Figure 1: Tax in 2016/17

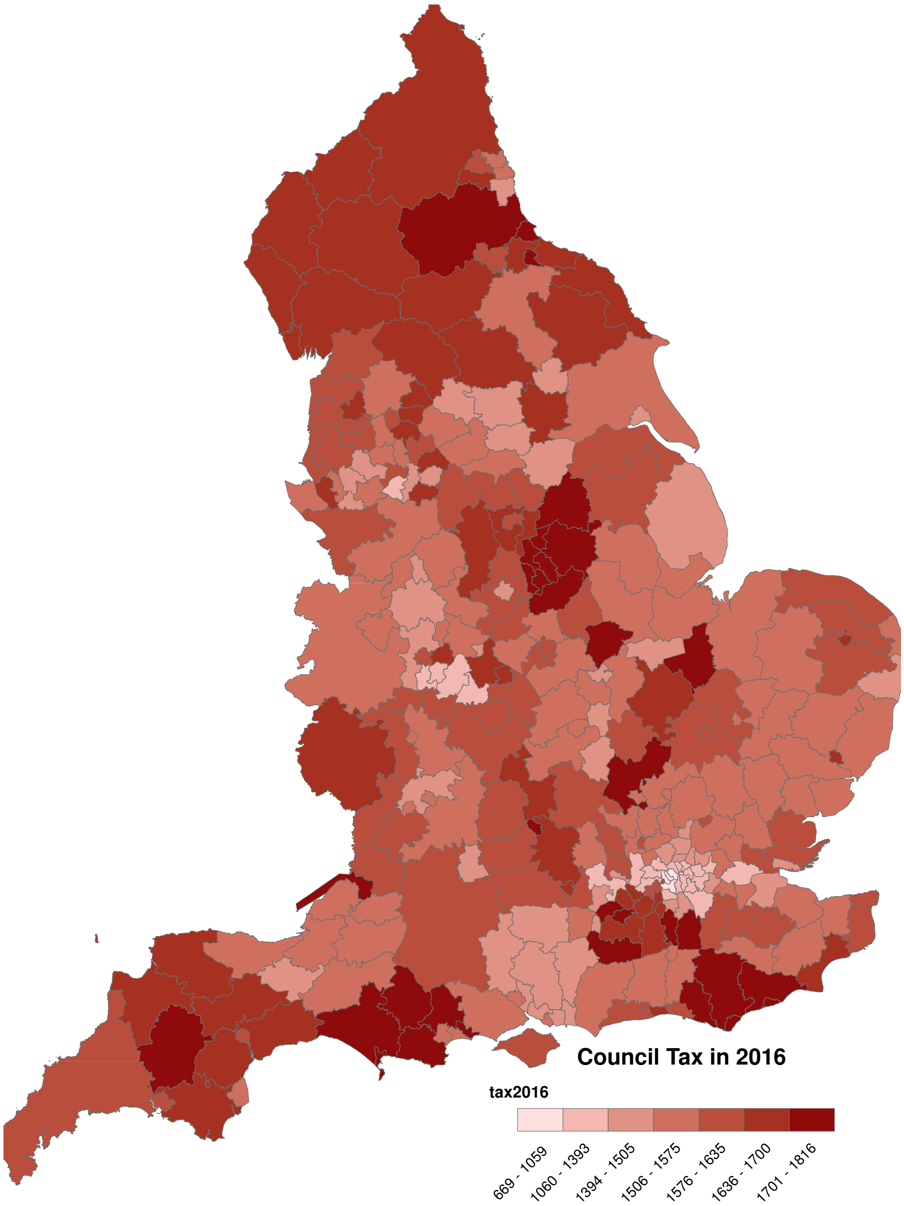
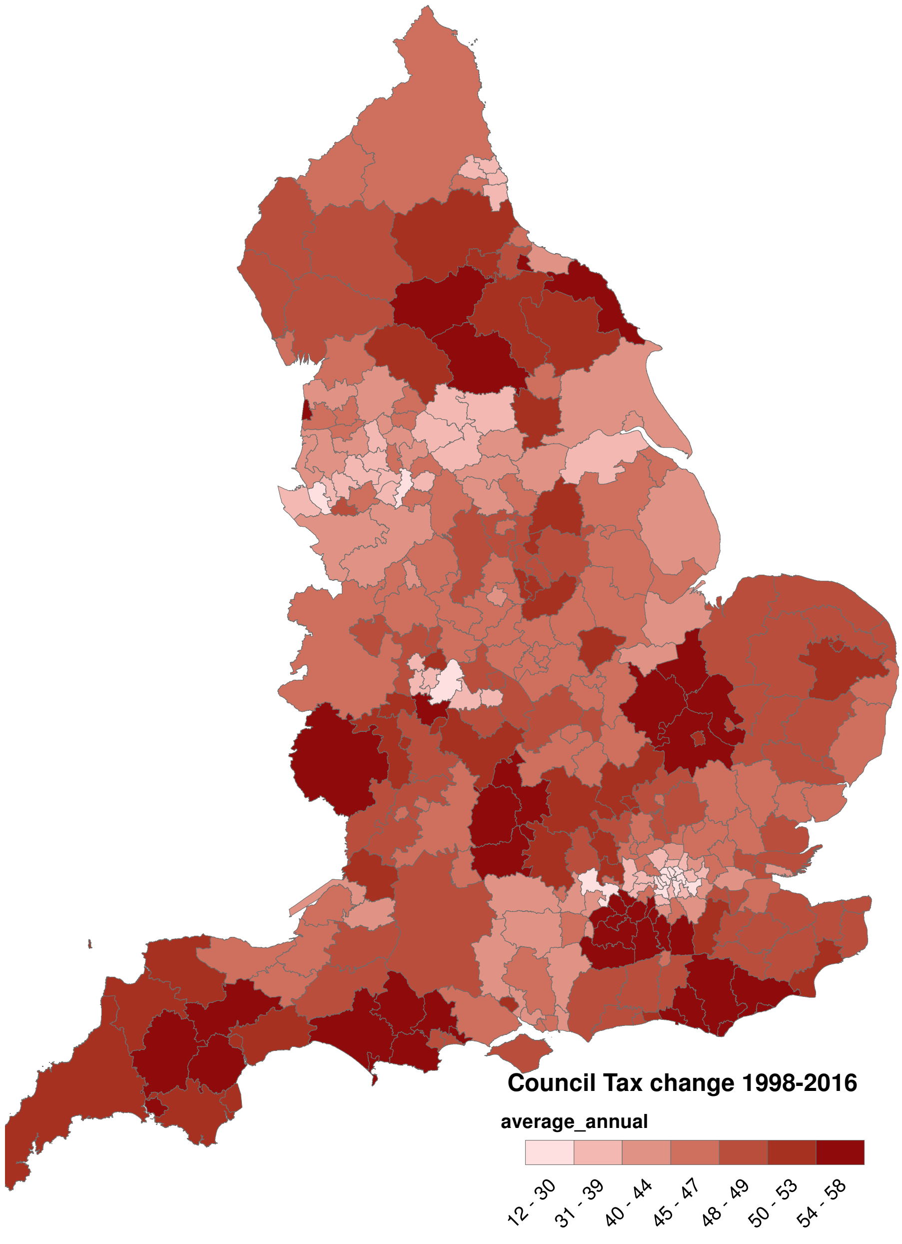


Figure 2: Tax change 1998/99-2016/17



rise more rapidly.

Although our main focus is Council Tax, the degree of housing supply elasticity plays an important role in our later empirical work. It is worthwhile, therefore, to note at this point that the planning system in Britain is widely viewed as one of the most restrictive regimes in the world. Numerous planning restrictions – in the form of an unpredictable decision regime (with no zoning); and extensive urban growth boundaries, building height restrictions, and preservation policies – severely curtail the supply of space (e.g. Cheshire, 2018). Importantly, while restrictions have been shown to be most drastic in the affluent South East, the evidence points to tight regulation right across the country. For example, Cheshire and Hilber (2008) show that restrictions on the supply of office space are equivalent to a tax on construction costs of more than 200% in cities such as Leeds, Glasgow, Edinburgh, Birmingham, and Manchester (for reference the implied tax in London's West End is 800%).

2 Empirical approach

2.1 Estimating β/r_T

In the first step in our estimation procedure we exploit the full size of the dataset to estimate β/r_T by using the effect of changes in the Council Tax on changes in housing values. The basic equation to be estimated yields:

$$V_{it} = \frac{\rho}{r_H} H_i - \frac{\beta}{r_T} T_{it} + \phi_t + \omega_{it}, \tag{3}$$

where H_i are time-invariant housing attributes, the vector ρ indicates the impact of housing attributes, β/r_T is the (combined) parameter of interest, ϕ_t are year fixed effects and ω_{it} denotes an identically and independently distributed error term.

The above equation is unlikely to identify a causal effect β/r_T because the Council Tax is not uniform over space and likely correlated to features which make places attractive and that yield higher housing values. Moreover, to the extent H_i does not capture all relevant housing attributes, a higher Council Tax may be correlated to positive unobserved housing attributes, because houses with high prices are in higher tax bands. The first step to mitigate the latter problem is to focus on temporal variation in Council Taxes. Let us consider a sale in year t and τ (where $\tau < t$) and denoting $\Delta V_{it\tau} = V_{it} - V_{i\tau}$ and $\Delta T_{it\tau} = T_{it} - T_{i\tau}$. We then would have:

$$\Delta V_{it\tau} = -\frac{\beta}{r_T}\Delta T_{it\tau} + \phi_{\kappa t\tau} + \Delta\omega_{it\tau}, \tag{4}$$

where $\phi_{\kappa t\tau}$ is now a year pair×tax band κ specific fixed effect. The large advantage of using repeat sales is that we plausibly control for many unobserved housing and location attributes that are fixed over time. Note that the above equation only identifies a causal effect of taxes if housing and location attributes H_i are indeed fixed over time, or that changes in housing attributes are uncorrelated to changes in T_{it}. Our sample restrictions described in the next Section indeed give us confidence that the homes in our sample do not undergo significant changes between sales. Moreover, it is assumed that ρ is constant over time. Given the long time period (1998-2016), the latter seems a more heroic assumption. We therefore will estimate specifications where we include time-specific preferences for observable housing and location attributes H_i (e.g. size, an age proxy, as well as access to open space).

A more problematic assumption in the above equation is that changes in Council Taxes are uncorrelated to changes in unobserved locational characteristics. This assumption fails to hold when an LA aims to finance an increase in public goods by increasing Council Taxes. Since local public goods are thought to capitalise in housing values, β/r_T would be biased towards zero (so that r_T would be biased upwards). Another problem may be that areas with strong price appreciation have fewer incentives to increase Council Taxes to keep the current level of public goods. Hence, to reduce this potential bias, we will focus on repeated sales that occur close (1 or 1.5km) to an LA boundary and include boundary fixed effects $\phi_{\kappa bt\tau}$ for each boundary b and each year t-τ combination. Including boundary fixed effects should effectively control for changes in public good provision (and other local amenities) to the extent the benefits are continuous over space. We test this more directly by gathering data on total local spending per LA and information on test scores, denoted by P_{it}.

A familiar problem in spatially differencing the data is that sorting of households may occur (Bayer *et al.*, 2007). In our setting, households that disproportionately value certain public goods may sort themselves in LAs with higher taxes. The changed demographic composition of an LA may then be valued (or disliked) by incoming households. In other words, β/r_T would not measure the effect of taxes, but captures preferences for neighbours. In the next Section we indeed show that there seems to be sorting of different household types along the LA boundary. However, when we compare *changes* in taxes to *changes* in demographics along the LA boundary we do not find any meaningful dynamic sorting effects.

The preferred specification to be estimated yields:

$$\Delta V_{it\tau} = -\frac{\beta}{r_T}\Delta T_{it\tau} + \frac{\rho_t - \rho_\tau}{r_H}H_i + \frac{1}{r_P}\Big(f(P_{it}) - f(P_{i\tau})\Big) + \phi_{\kappa bt\tau} + \Delta\omega_{it\tau}, \tag{5}$$

where $\rho_{t\tau}$ and $f(\cdot)$ are both estimated with second-order polynomials to allow for non-linear preferences.

2.2 *Intra-jurisdictional estimates of β/r_T*

Until this point, all specifications have relied on *inter-jurisdictional* variation in taxes, i.e. the identifying variation derives from differences in LA tax setting decisions. We can also use *intra-jurisdictional* variation to estimate β/r_T by comparing tax and price changes for homes in the same LA but different tax bands. The advantage of this approach is that it abstracts from differences in LA-wide local public goods, but on the downside it also means that we are unable to use the year pair×tax band fixed effects employed in our baseline approach above. This is a considerable disadvantage as these controls condition out unobserved factors common to homes in the same tax band, which for example could include trends associated with unobserved home quality characteristics, as well as expectations (both about future price trends and future taxes) relating to homes in specific tax bands.

To counter this latter disadvantage, we use the narrowest geographical fixed effects available to us (postcodes), retain homes with prices close to the tax band thresholds that are shown in Table 1, and include postcode×year×threshold fixed effects. The identifying assumption is that the prices of these neighbouring homes in different tax bands would evolve in the same way absent differences in property tax changes. To determine which homes lie close to thresholds, all sales prices are deflated to 1995 values using average price trends in postcode sectors computed using the universe of transactions, then deflated to 1991 values using the Nationwide price index.[12] We then estimate:

$$\Delta V_{it\tau} = -\frac{\beta}{r_T}\Delta T_{it\tau} + \frac{\rho_t - \rho_\tau}{r_H} H_i + \phi_{\gamma dt\tau} + \Delta\omega_{it\tau}, \tag{6}$$

where $\phi_{\gamma dt\tau}$ is a fixed effect specific to years of first and second sale, postcode d, and threshold bands (e.g. homes with 1991-equivalent prices close to the threshold between bands A and B of £40,000). Note that the term $P_{it\tau}$ is not included here as public good provision is the same within postcodes, and in any case our measures contain no variation at this spatial scale due to the way we specify them.

[12]Other strategies to deflate to 1991 values are of course possible. For this reason we view this approach as a robustness check on the inter-jurisdictional approach where we can rely on unambiguous district boundaries.

2.3 Estimating capitalisation and discount rates separately

The next step is to obtain information about the capitalisation rate, so that we can identify r_T in the previous analysis. We therefore use a subset of the data for which we also have information on rents R_{it} to estimate $\tilde{\beta}$, which we anticipate will be a good proxy for β. The rentals data are only available for a short-time period (2013-2016). Hence, we cannot identify the effect of a change in taxes on a change in rents. Nevertheless, we can spatially difference the data as outlined above. In the spirit of equation (2), we estimate:

$$R_{it} = -\tilde{\beta} T_{it} + \rho H_i + f(P_{it}) + \phi_{\kappa bt} + \omega_{it}. \tag{7}$$

Here the identifying assumption is that the effects of spatial differences in unobserved housing or neighbour attributes at the LA boundary are uncorrelated to spatial differences in the Council Tax. Because we will show that there is sorting along the LA boundary that may thwart a causal interpretation of $\tilde{\beta}$, we repeat the above analysis for prices:

$$V_{it} = -\frac{\beta}{r_T} T_{it} + \frac{\rho}{r_H} H_i + \frac{1}{r_P} f(P_{it}) + \phi_{\kappa bt} + \omega_{it}, \tag{8}$$

where the estimated β / r_T should be (very) comparable to the previous analysis using repeat sales. Hence, equation (8) is an over-identification test of whether $\tilde{\beta}$ measures a causal effect of taxes on rents.

3 Data

3.1 Data sources

To measure the discount rate, we use data on home sales, rentals, and property taxes. We provide key information about our data here and further details in Appendix A.3.

The Land Registry Price Paid dataset provides us with the universe of home sales in

England between 1995 and April 2017. The data records the transaction price, the date the sale was registered (which proxies for the actual date of sale), the full address including postcode, the type of house (flat, detached house, semi-detached house, terraced (or row) house), a new build indicator, and the tenure (leasehold or freehold). There is no publicly available data for home rentals for England, so we rely on a dataset obtained from Homelet, the UK's largest tenant referencing and specialist lettings insurance company. This data covers 2013-2017 and includes no information on the characteristics of homes other than the full address of the property, the date of the rental agreement, and the monthly rent. Due to the paucity of home characteristics in these data, we match in additional variables – including number of rooms; floor area; wall construction type, and the number of home extensions – from Energy Performance Certificates (EPCs).

Council Tax band information was obtained from the website *mycounciltax.org.uk* using web scraping techniques in early to mid 2017. The home level data contains the full property address and the contemporaneous tax band. The fact that we are unable to observe previous tax assessments is unlikely to be a major threat as re-banding is so rare (0.20-0.25% each year). Using this tax band information, tables from the Department of Communities and Local Government (DCLG) allow us to compute the annual Council Tax payable at the time of each home sale or rental.[13]

We geocode and append geographical variables using Postcode Directories, and use GIS techniques to identify transactions that lie within fixed distances of Local Authority boundaries.[14] The Chartered Institute of Public Finance and Administration (CIPFA) provides us with LA expenditure on services per head of population for financial years 1997/98-2016/17. We generate yearly postcode level school quality measures by averaging

[13]Specifically, we compute the annual tax payable using the Local Authority-wide average Band D Council Tax for each financial year in the DCLG data and then scale this to match the band of the property in question using the ratio shown in Table 1. We also compute for robustness checks tax payments at the parish level for a subset of our data – see Appendix B.2 for details. The correlation between taxes measured at the parish and LA levels in our data is 0.997.

[14]Boundary samples are computed for both pre-2009 and post-2009 LAs. These samples are highly similar: we use the post-2009 boundaries which contain fewer boundaries.

Maths and English test scores for pupils aged 8-11 (Key Stage 2) available from the Department of Education. We create two measures which are both based on the inverse-distance weighted score of this school quality measure in the nearest four schools in a given year. Our primary measure is constructed using tests scores only for schools in the associated Local Education Authority (LEA), and as such can vary discontinuously at LA boundaries. A second measure, which we use for robustness, is computed across the nearest four schools regardless of LEA. We calculate a time invariant measure of access to green space at the postcode level using data for 2015 from Ordnance Survey and data on parks and gardens from the National Heritage List for England. The share of green space is computed using two distance buffers (0-500m and 500-1,000m).

3.2 Sample restrictions

We make a number of sample restrictions to remove outliers, to minimise unobserved home changes, and to mitigate measurement error. Further details are listed in the Appendix A.4. To remove outliers we exclude the top and bottom 1% of prices (or rents) and the top and bottom 1% of prices (or rents) in each tax-band. We also drop homes in three LAs which are extreme outliers in terms of population size or expenditure on local services, which we define as more than double the 99$^{\text{th}}$ percentile or less than half the 1$^{\text{st}}$ percentile. We next remove homes for which characteristics change during our sample timespan.[15] This entails dropping homes with 1 or more extension at the time of any certificate, homes where the floor area of the property moves by more than 20% from the median value for the home in the data, and homes that are recorded as being "new" more than once, which likely indicates redevelopment. We also remove homes that

[15]There are at least three reasons why we wish to remove these homes. First, time-varying characteristics renders repeat sales approaches invalid and removing homes that change characteristics is a common strategy in research using repeat sales (see e.g Bajari *et al.* (2012) and Standard and Poors Case-Shiller Home Price Indices Index Methodology). Second, removing homes with time- varying characteristics means we can use time-invariant home characteristics to control for changing preferences and/or variation in maintenance costs between property types (Harding *et al.*, 2007). Third, time-varying characteristics may imply measurement error in the tax variable because we are unable to access the full tax band history of the house and are therefore unable to tell whether each home has been reassigned to a different Council Tax band during our sample time-frame.

were new at the previous sales from our repeat sales data as these homes are likely to depreciate at a different rate to other homes. We make one additional restriction each for rentals and sales. For sales, we drop leasehold homes as we cannot observe the lease length. For rentals, we remove homes that only appear once in our rental sample (some 40% of rentals) on the basis that these rentals may represent long-term agreements in which the agreed rent may not reflect the market value of renting the home for one year i.e. the capitalisation rate may partly capture a discount rate. We show sensitivity to many of these sample selections in Table B2 in Appendix B.2.

3.3 Descriptive statistics

Our primary dataset is composed of 2.3 million consecutive repeat home sales pairs that have a second sale taking place between 9 months and 8 years of the original sale. Descriptive statistics for this dataset are shown in Table 2. Panel A describes the full dataset both without sample restrictions (LHS) and with restrictions (RHS). Panel B of Table 2 repeats this format but describes the sales that lie within 1 km of a boundary with a different LA, which is our main boundary buffer distance. Due to the nature of the sample restrictions, we expect the mean sales price, size of home, and Council Tax in the restricted sample to be lower than the full sample. We indeed find that this is the case. Table 2 also highlights that sales in the restricted 1km boundary sample have a slightly lower average Council Tax than the full unrestricted sample and benefit from a somewhat higher LA spending per head.

Table 2: Descriptive statistics: repeat sales

	Without restrictions				**With restrictions**			
	mean	sd	min	max	mean	sd	min	max
Panel A: full sample								
Price	199684.93	182787.62	195.00	17000000.00	173236.99	92195.98	31000.00	775000.00
Tax	1184.13	355.83	331.89	3450.88	1139.88	316.71	331.89	2970.57
KS2 score %	0.82	0.08	0.00	1.00	0.82	0.08	0.00	1.00
LA spend/head	673.53	639.04	60.32	2854.64	666.97	634.70	60.32	2854.64
Greenspace 0-500m %	0.07	0.08	0.00	1.00	0.07	0.08	0.00	0.96
Rooms	4.73	1.43	0.00	85.00	4.39	1.26	1.00	77.00
Built after 1995 %	0.19	0.39	0.00	1.00	0.14	0.35	0.00	1.00
Extensions	0.53	0.72	0.00	4.00	0.00	0.00	0.00	0.00
Quarters b/w sales	15.83	7.80	4.00	32.00	15.76	7.76	4.00	32.00
Observations (pairs)	2,287,002				1,070,284			
Panel B: 1km boundary sample								
Price	235862.45	277036.35	1500.00	17000000.00	170782.33	89589.30	31000.00	775000.00
Tax	1227.42	375.77	331.89	3450.88	1093.09	283.84	331.89	2804.42
KS2 score %	0.82	0.08	0.00	1.00	0.81	0.08	0.00	1.00
LA spend/head	782.10	676.77	60.32	2854.64	793.02	657.82	64.09	2797.94
Greenspace 0-500m %	0.07	0.08	0.00	0.97	0.07	0.08	0.00	0.96
Rooms	4.76	1.44	0.00	71.00	4.28	1.15	1.00	45.00
Built after 1995 %	0.17	0.38	0.00	1.00	0.12	0.32	0.00	1.00
Extensions	0.53	0.72	0.00	4.00	0.00	0.00	0.00	0.00
Quarters b/w sales	16.05	7.80	4.00	32.00	15.40	7.25	4.00	32.00
Observations (pairs)	649,287				187,051			

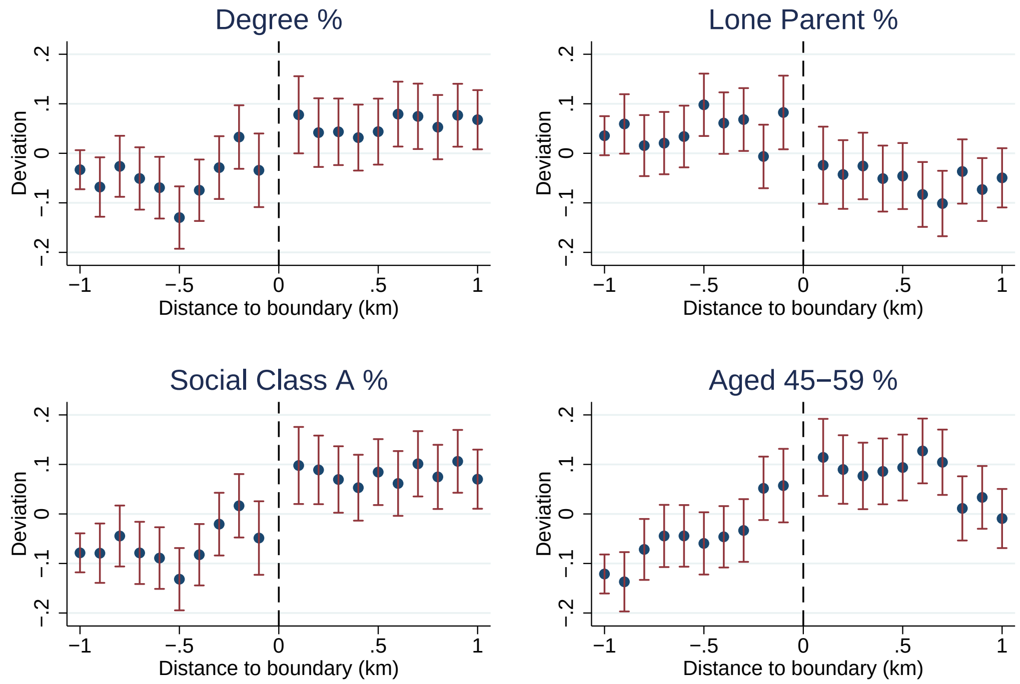
3.4 Sorting

Sorting of households may threaten our identification to the extent that households move to LAs not because of their preferences for public goods and taxes, but to be close to other households that sort for these considerations. In other words, β/r_T would not just capture the effect of taxes but also preferences for neighbours. We use Census data for Output Areas (OAs) to assess the extent to which demographic variables are correlated with property taxes across LA boundaries in 2011 (Figure 3), and changes in taxes between census years 2001 and 2011 (Figure 4). To obtain the figures, we first select OAs in boundary samples, then assign them low or high tax side of boundary based on taxes in 2011 or changes in taxes between 2001 and 2011.[16] Some OAs are close to multiple LA boundaries so we drop any on the high tax of one boundary but the low side of another. We assign each OA to a distance bin for each boundary sample they fall in, based on the median distance to the boundary of postcodes that lie both within the OA and the boundary sample. Distance is coded as negative for the lower tax side of the boundary. We then run OA regressions of various Census variables on distance bin dummy variables, where the dependent variables are standardised by deducting the boundary sample mean and dividing by the boundary sample standard deviation.

Figure 3 shows some evidence that individuals with higher income and education levels are located on the higher tax side of boundaries in 2011, possibly because they have a stronger preference for the public goods that are provided by the (higher) Council Tax. However, there are no clear patterns with regard to *changes* in taxes between 2001 and 2011 (see Figure 4). The latter is important, as our main identification strategy relies on temporal variation in taxes and house prices around LA boundaries.

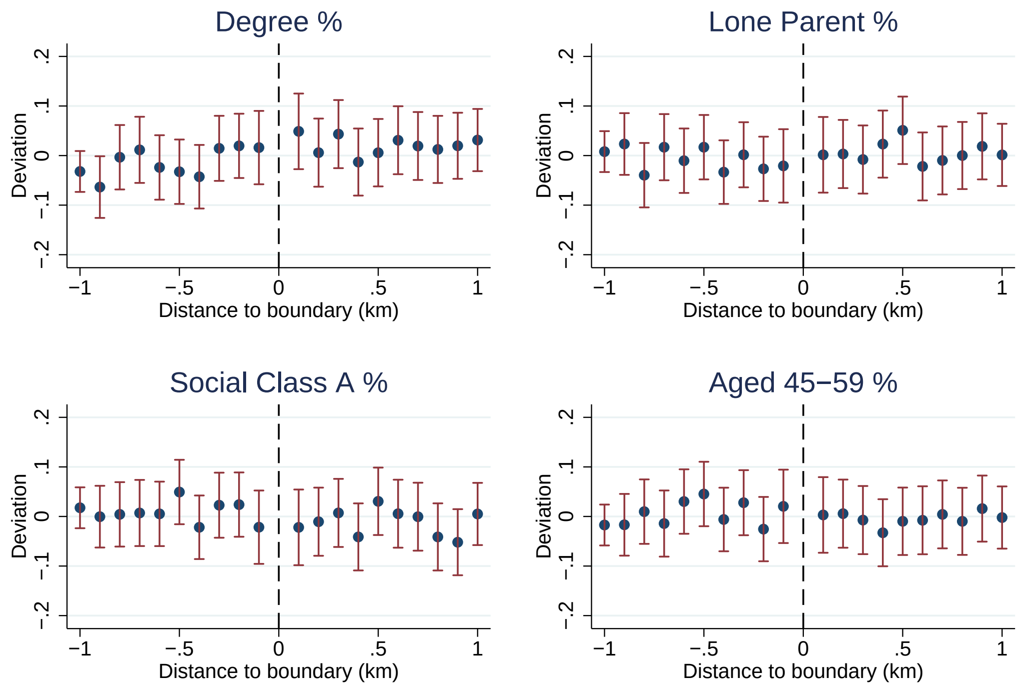
[16] Note that here we restrict attention to those boundaries that have large (above median) differences in tax. We obtain near-identical results if we keep all boundaries.

Figure 3: 2011 Census



Low tax side is negative

Figure 4: Changes between 2001 and 2011 Censuses



Low tax side is negative

4 Estimates of β/r_T

4.1 Inter-jurisdictional estimates

Table 3 reports estimates of β/r_T in which we regress sale prices on property taxes and control variables. In all cases regressions are performed on data samples using the restrictions described above. Standard errors are clustered on post-2009 Local Authorities. Furthermore, the inclusion of year pair×tax band fixed effects in all regressions in this Table implies that identification is achieved by comparing across properties that are in the same tax band, but subject to different LA-wide tax levies. In other words, we are estimating tax capitalisation parameters from inter-jurisdictional variation in taxes.

Column (1) is the most basic specification which absorbs common trends in different labour market areas by using a three-way fixed effects interaction between year pair, tax band, and Travel to Work Area (TTWA).[17] Results imply that a one pound increase in tax leads to a house price decrease of £73.79. Based on the assumption that β is between 0.75 and 1 (i.e. the range implied by Lutz, 2015), the implied discount rate r_T is between 0.010 and 0.014.[18] One potential problem with this specification is that changes in taxes may be correlated with price dynamics of urban areas. In particular, the resurgence and gentrification of city centres in our sample period may have reduced relative pressure on budgets in LAs in the centre of TTWAs while simultaneously pushing up local house prices.[19] To counter the impact of this potential confounder, in column (3) we control for distance to the city centre by interacting the fixed effects with a categorical variable capturing the decile of postcode distance to the TTWA centre. The result is that impact of the Council Tax becomes considerably smaller such that the implied discount rate r_T with full capitalisation ($\beta = 1$) is around 0.03.

[17] TTWAs are defined by commuting patterns and can be thought of as labour-market areas. These are 149 TTWA areas in England in the most recent data recorded by the Office for National Statistics

[18] The standard errors of the implied discount rate are calculated using the delta method.

[19] We define the centre of the TTWA using the average x and y co-ordinates of all sales in the Land Registry.

Table 3: Inter-jurisdictional estimates of average β/r
(Dep var: Δ sale price in £)

Dependent variable: Δ sale price	(1)	(2)	(3)	(4)	(5)	(6)
Δ Council Tax	-73.79***	-32.59*	-30.75***	-26.30***	-29.46***	-30.38***
	(17.896)	(19.223)	(9.492)	(8.925)	(8.407)	(8.429)
Quadratic in LA spend per head					✓	✓
Quadratic in KS2 test score					✓	✓
Local green space×years						✓
Home characteristics×years						✓
Year pairs×band×TTWA	✓					
Year pairs×band×TTWA×Distance		✓				
Year pairs×band×1.5km BFE			✓			
Year pairs×band×1km BFE				✓	✓	✓
Implied r; β=0.75	0.010***	0.023*	0.024***	0.029***	0.025***	0.025***
	(0.002)	(0.014)	(0.008)	(0.009)	(0.007)	(0.007)
Implied r; β=1	0.014***	0.031*	0.033***	0.038***	0.034***	0.033***
	(0.003)	(0.018)	(0.010)	(0.013)	(0.010)	(0.009)
Number of sales pairs	1070255	931259	372430	186843	186843	186843
R^2	0.663	0.731	0.768	0.759	0.759	0.767

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. *** p < 0.01, ** p <0.05, * p < 0.1. All regressions are first difference specifications estimated in levels that include only repeat sales with fixed characteristics. Column (1) include dummies for financial year of first and subsequent sale (year pairs) interacted with tax band and TTWA. Column (2) further interact these effects with a categorical variable that puts each postcode in one of ten bins according to distance to TTWA centre. Columns (3)-(6) replace TTWA with boundary fixed effects as indicated. Home characteristics interacted with year pairs in Column (6) are property type, no of rooms, wall construction type, built after 1995 indicator. Standard errors for implied r computed using the delta method.

All remaining columns in Table 3 are based on boundary samples and include boundary fixed effects (BFE) instead of TTWAs. In column (3) we only include observations within 1.5km of an LA boundary. Results are similar to column (2) but are now more precisely estimated. The estimates are essentially unchanged if we use a 1km buffer (see column (4), Table 3). To further investigate whether differences in public goods across LA boundaries are correlated to tax changes, column (5) includes quadratic terms in LA spending per head and school test scores. This leads to comparable results. Column (6) adds interactions between year pairs and home or neighbourhood characteristics (property type, number of rooms, wall construction type, access to green space) to allow for time-varying preferences for these features. The implied discount rate r_T is 0.033 under full capitalisation, and 0.025 when $\beta = 0.75$.

4.2 Intra-jurisdictional estimates

Table 4 reports results from the intra-jurisdictional approach described in equation (6) which uses very narrow geographical fixed effects, and retains homes with prices close to the tax band thresholds that are shown in Table 1. Homes are allocated to a threshold using two alternative rules. The specification in columns (1) and (2) include homes with 1991 values within £5,000 of a threshold e.g. homes with 1991 values in the range £35,000-45,000 for the A-B threshold and £315,000-325,000 for the G-H threshold. In column (3) the bandwidth is instead set at 10% of the relevant threshold. Column (1) is a basic specification that includes only fixed effects and no home characteristics. These are added in the later columns. Estimated coefficients are somewhat less precise, but all are broadly similar to our baseline results in column (6) of Table 3. For example, the estimates in column (2) indicate that the implied discount rate is 0.030 with full capitalisation, which is very close to the baseline estimates. We conclude that inter- and intra-jurisdictional variation imply similar discount rates.

5 Discount rates

5.1 Disentangling r_T and β

Based on our reading of the existing property tax capitalisation literature, in the analysis above we assumed $\beta \in [0.75, 1]$ to provide a range of discount rates for our baseline specification of $r_T \in [0.025, 0.033]$. Given the wide diversity in estimates from other settings, this range is sufficiently narrow to be a valuable addition to the literature on revealed discount rates. Notwithstanding, to recover a single discount rate from β/r_T requires a single value of β. One proposition is to assume full capitalisation i.e. $\beta = 1$. This is attractive both because it provides a plausible upper bound on the discount rate,

Table 4: Intra-jurisdictional estimates of average β/r
(Dep var: Δ sale price in £)

Dep var: Δ sale price	(1)	(2)	(3)
Δ Council Tax	-40.36***	-33.60***	-37.11***
	(12.191)	(12.507)	(13.043)
Home characteristics×years		✓	✓
Yr pairs×postcode×ThresholdFE	✓	✓	✓
Threshold rule:	fixed 5k for all	fixed 5k for all	10% of cut–off
Implied r; β=0.75	0.019***	0.022***	0.020***
	(0.006)	(0.008)	(0.007)
Implied r; β=1	0.025***	0.030***	0.027***
	(0.007)	(0.011)	(0.009)
Number of sales pairs	37699	37699	33384
R^2	0.916	0.916	0.918

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. *** p < 0.01, ** p <0.05, * p < 0.1. All regressions rely on observations close to tax band thresholds set out in Table 1 and include threshold fixed effects interacted with years of sales and postcode indicators. To determine which homes lie close to thresholds, all sales prices are deflated to 1995 values using average price trends in postcode sectors computed using the universe of transactions, then deflated to 1991 values using the Nationwide price index. Homes are allocated to a threshold using two different rules. Specifications in columns (1) and (2) include homes with 1991 values within £5,000 of a threshold e.g. homes with 1991 values in the range £35,000-45,000 for the A-B threshold and £315,000-325,000 for the G-H threshold. In column (3) the bandwidth is set at 10% of the relevant threshold.

and because it is consistent with a vast number of studies that value amenities such as school and environmental quality or transport innovations using house prices under the assumption that capitalisation is full. In this section, we provide further evidence to assess the validity of such an assumption.

Although we cannot estimate β directly using house prices, we can estimate $\tilde{\beta}$, the rental capitalisation rate. Panel A of Table 5 reports specifications in which we identify this parameter using cross-sectional spatial variation.[20] In the first column we estimate a (non-significant) capitalisation rate of 0.97 with a specification that controls for housing

[20] We use inter-jurisdictional variation here. We cannot use intra-jurisdictional variation because we do not observe the sales prices of homes in the rental sample, so we cannot tell if they are close to taxband thresholds.

attributes but not public goods using the 1km boundary sample. Column (2) adds controls for LA spending, test scores, and access to green space. This specification suggests the capitalisation rate $\tilde{\beta}$ is slightly above but not statistically significantly different from one, meaning that one pound increase in taxes leads to a one pound decrease in rents. We note that these results are not very precise, due to a much lower number of observations and often little variation in taxes between adjacent LAs. In Columns (3) and (4) we therefore use the larger 1.5km boundary sample. The point estimates are very similar to the previous specifications and close to one, and slightly more precisely estimated. We thus find that renters bear little of the property tax burden. This is largely consistent with findings in the literature (Carroll and Yinger, 1994).[21]

A main worry is that a cross-sectional identification strategy is less convincing in identifying a causal effect of taxes on rents, e.g. because of sorting. In Panel B of Table 5 we therefore repeat the rental analysis but using our repeat sales sample and again taking the sales price again as the dependent variable. This implies that we again identify β/r_T. When these estimates are similar to the analyses using temporal variation in taxes and prices, this will increase the confidence that $\tilde{\beta}$ can be interpreted as a causal estimate. The results in Panel B of Table 5 indeed strongly suggests that the results are robust, as the effects are remarkably similar to the preferred specifications reported in Table 3.

These results suggest that property taxes fully capitalise in rents. However, given the discussion in Section 1.1 we have no direct way to ascertain that $\tilde{\beta} = \beta$.[22] In Table 6, we

[21]More specifically, Carroll and Yinger (1994) find that in a US setting where the legal incidence of the tax is on the landlord that, *"a £1.00 increase in property taxes results in a rent increase of only about £0.15, on average, even if the underlying supply curve for housing is very elastic (that is, even if landlords have many options)"*.

[22]Although comparing our results to Bracke *et al.* (2018) suggests that $\tilde{\beta}$ is a good proxy for β since $\tilde{\beta} = \beta\frac{r_H}{r_T}$. Bracke *et al.* (2018) estimate net of growth average discount rates on future housing service flows (r_H in our notation) in Prime Central London (PCL) of 4.1% in 1987-1991 and 2.5% for the period 2004-2013. We cannot directly replicate the Bracke *et al.* (2018) geographical setting since we have too few data points in PCL (the highly urbanised core of London containing parts of Westminster and Kensington & Chelsea (2 LAs)). But we can use Inner London – an area that subsumes PCL but is larger (roughly 320 km^2, containing 15 of the 32 LAs in London). In Table 6 we estimated that r_T= 2.8% in Inner London across the period 1998-2016. Replicating this for the period 2004-2013, we obtain an estimate of 2.3%. This suggests a close correspondence between r_H and r_T.

Table 5: Cross sectional rent and price regressions
(Dep var: rent or sale price in £)

	(1)	(2)	(3)	(4)
	—1km buffers—		—1.5km buffers—	
Panel A: taxes and rents				
Council Tax	-0.97	-1.14*	0.94**	-1.09**
	(0.59)	(0.63)	(0.46)	(0.51)
Observations	24106	24106	34970	34970
R^2	0.742	0.742	0.742	0.742
Panel B: taxes and sale prices				
Council Tax	-27.65*	-26.58*	-30.83*	-29.94*
	(14.65)	(14.57)	(16.23)	(16.43)
Observations	81093	81093	118038	118038
R^2	0.935	0.936	0.931	0.932
Quadratic in LA spend per head		✓		✓
Quadratic in KS2 test scores		✓		✓
Local green space		✓		✓
Home characteristics	✓	✓	✓	✓
Year×taxband×BFE	✓	✓	✓	✓

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. *** p < 0.01, ** p <0.05, * p < 0.1. All regressions are cross-sectional specifications estimated in levels and exclude *(i)* outliers which are defined at the top and bottom 1% of rents or prices and the top and bottom 1% of rents or prices in each tax-band *(ii)* homes that have more than one extension. Rental regressions further exclude homes that appear only once in the sample. Price regressions further exclude leaseholds. Home characteristics are number of rooms and number of rooms squared, energy efficiency rating, and a three-way interaction between property type, wall type (cavity, solid, unknown), has fireplace.

therefore take a different approach to assessing β. Our starting position is to assume that $\beta = 1$ in places with very inelastic housing supply such that estimates can be interpreted as $1/r_T$, building on theoretical and empirical findings (detailed in Appendix A.2) in the capitalisation literature that β should be higher (in absolute terms) when housing supply is less elastic (Cheshire and Sheppard, 2004; Hilber and Mayer, 2009; Hilber *et al.*, 2011; Hilber, 2015; Lutz, 2015). Specifically, we interact the tax variable in column (6) of Table 3 with various indicators capturing housing supply elasticity.

In the first two columns we find that the the tax coefficient is smaller in absolute terms in rural places than our baseline findings (column (1)), and larger in inner London (column

Table 6: Housing supply elasticities
(Dep var: Δ sale price in £)

Dep var: Δ sale price	(1)	(2)	(3)	(4)	(5)	(6)
Elastic=1 × Δ Tax	-23.14***	-29.21***	-28.30***	-33.52***	-31.01***	-28.93***
	(8.822)	(8.144)	(8.540)	(9.339)	(9.793)	(9.923)
Elastic=0 × Δ Tax	-30.45***	-35.82***	-32.42***	-30.79***	-31.24***	-31.11***
	(8.420)	(10.688)	(8.136)	(8.689)	(10.413)	(8.042)
Housing supply measure:	rural vs urban	other vs inner London	share land dev'able	LA refusal rate	share homes CA	share homes GreenBelt
Implied r; Elastic=0	0.033***	0.028***	0.031***	0.032***	0.032***	0.032***
(β=1)	(0.009)	(0.008)	(0.008)	(0.009)	(0.011)	(0.008)
Number of sales pairs	186843	186843	186843	186843	161406	186843
R^2	0.767	0.767	0.767	0.767	0.768	0.767

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. *** p < 0.01, ** p <0.05, * p < 0.1. All regressions are as column (6) of Table 3 but interacts Δ Tax with a dummy variable taking the value of 1 when housing supply is expected to be more elastic. In column (1) this is postcodes in a non-urban setting; in column (2) postcodes outside inner London; in column (3) above median share of LA land that is developable (average in 1991, 2001, and 2011); column (4) below median LA refusal rate on major housing development planning applications (average 1991-2013); column (5) below median LA share of homes in Conservation Areas (2005); column (6) below median LA share of homes in Green Belt (2011).

(2)). The difference is statistically significant for urban vs rural (where a β parameter of around 0.75 for rural places would imply a discount rate of 3.3%) but not for inner London vs elsewhere. In the remaining columns of Table 6 we find little evidence of material differences in the tax coefficients in places with different housing supply elasticity as measured by above or below median share of developable land (column (3)), planning refusal rate (column (4)), proportion of homes in Conservation Areas (column (5)), or share homes in Green Belts (column (6)).[23] Overall these results suggest that estimates are largely insensitivity to variation in housing supply elasticity. Moreover, in support of our previous findings in all cases we find the implied discount rate in places with tighter housing supply elasticity (where β is plausibly equal to one) is close to 3%.

[23]We obtain the counter-intuitive result that the coefficient is slightly more negative in places with below median LA refusal rate on major housing developments in column (4). This may reflect a well-known endogeneity issue with the refusal rate that arises because highly restrictive LAs may discourage developers from making planning applications (e.g Hilber and Vermeulen, 2016).

5.2 Tests for inter-temporal optimisation

One general test for optimising behaviour, widely used in studies of purchases of energy efficient durable goods, is that households should be indifferent between £1 in purchase cost and £1 of future costs discounted at the appropriate inter-temporal opportunity cost rate. Previous studies have typically found some degree of undervaluation of future financial flows relative to those in the present. To apply this test in our setting, we adopt a null hypothesis that households discount future property taxes at the opportunity costs of funds, and an alternative hypothesis that households undervalue the future.

To conduct the test, we assume that $\beta = 1$. This value for β is supported by the evidence in previous sections. In addition, to the extent that full capitalisation provides an upper bound on the β parameter value, in light of our hypotheses this is a conservative assumption. This is because values of β less than 1 would imply lower discount rates, and hence make it less likely that we conclude that households are undervaluing the future.[24]

As r_T is a net of growth discount rate, we must also adjust our baseline value of $r_T = 0.033$ for expected tax growth. In our setting, property taxes – as measured by the 'Council tax and & rates' element of the Retail Price Index (series DOBR) adjusted into real terms using the RPI all items index (series CHAW) – grew on average at a slightly faster rate (0.8%) than inflation between 1989 and 2016. However, tax movements are tightly correlated with changes in Local Authority spending (correlation 0.82 1998-2016 using the CIPFA spending data) so that average real net increases in taxes over spending are approximately zero. Using these values as bounds we conclude that average growth adjusted discount rates for the period 1998-2016 lie in the range $[0.033, 0.041]$. The implied 95% confidence intervals place discount rate in the range $[0.015, 0.059]$.

[24]More generally, one may argue that when $\beta < 1$ for reasons other than elastic housing supply, this also may be considered as a departure from rationality. As such, even if $\beta \neq 1$ then we can undertake a test for rationality by assuming $\beta = 1$ and comparing the implied discount rate to appropriate opportunity cost rates.

We cannot directly observe the opportunity cost of funds for individual home purchasers in our data so we compare this range to benchmark opportunity cost rates for the period 1998-2016 obtained from aggregate data. Our first benchmark rates is the real long risk free rate. We obtain an estimate of 1.1% from the average annual real yield for the Government Liability curve for maturities of 1-25 years between 1998 and 2016 using Bank of England data. Our second benchmark rate is a candidate real mortgage rate. We use the 1998-2016 average of the fixed 2 year 75% LTV mortgage rate of 2.3%. This is derived from the Bank of England (series IUMBV34) adjusted into real terms using the contemporaneous inflation rate. Together these provide a range of benchmark interest rates of $[0.011, 0.023]$.[25]

We interpret these estimates as presenting no strong evidence that households materially undervalue future property taxes in this context. Although average tax implied discount rates are slightly higher than the lowest benchmark rate, this gap is an order of magnitude smaller than those obtained in the literature on energy efficient durable goods described above. Moreover, it is important to recall that these rates are long-term averages. As we show in the next section, the residual difference in these average values is driven by the emergence of very low market rates following the 2008 financial crisis.

5.3 Time variation

We next shed further light on the relationship between discount rates and benchmark market interest rates by plotting the evolution of r_T over time. Because these regressions require a considerable number of sales in each period, we use the 1.5km boundary sample.[26]

[25] As an alternative we could use the CAPM to derive a benchmark rate. Changes in real taxes are positively correlated with changes in real household final consumption expenditure per head which indicates that taxes fall when consumption falls, i.e. taxes hedge aggregate consumption risk. However, average real net increases in taxes over spending are uncorrelated with consumption growth (correlation 0.09). We thus anticipate that the risk premium should be approximately zero, and hence using the risk free rate should be sufficient.

[26] Regressions from this exercise generate a high number of coefficients. We plot transformations of these coefficients along with standard errors in various Figures below. Tabulations of results can be

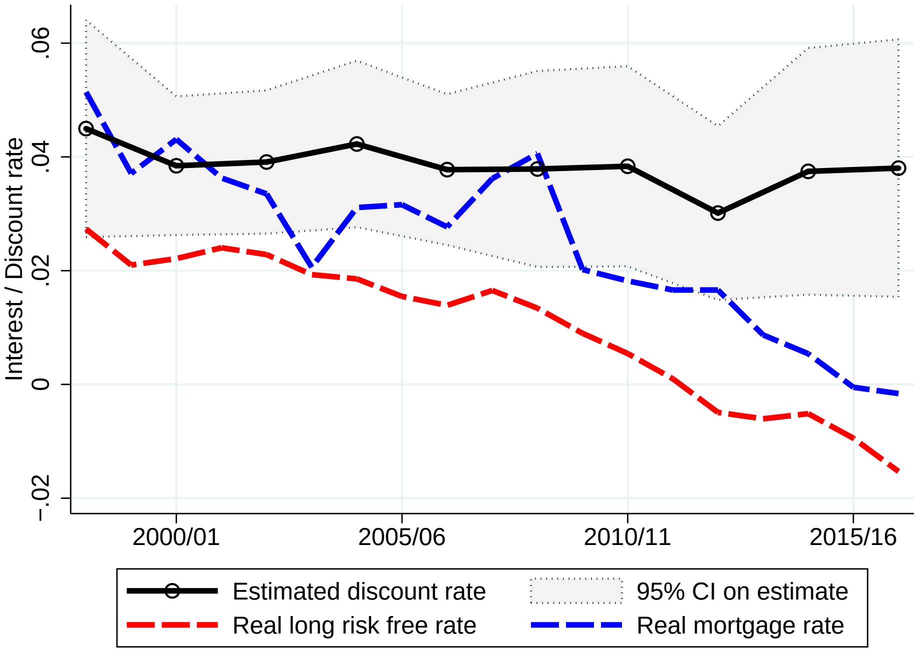
We first estimate a specification in which we estimate time variation in r_T under the assumption of full capitalisation. Our approach involves interacting time dummies with the tax variable (transformed appropriately – see equation (B.1) in Appendix B.1 for more details) and plotting the reciprocal of the resulting coefficients on the tax variables in Figure 5. Here, the black line represents the time path of r_T (under the assumption of full capitalisation and adjusted for growth of 0.8%.), and the shaded area represents the bounds of the 95% confidence interval around the point estimates. The resulting pattern is somewhat scattered but most estimates fall in the range of 3 to 4%. The point estimates are statistically indistinguishable from one another, suggesting that r_T is stable over the full span of our sample.

Figure 5 also plots the same proxies for the real long risk-free rate (dashed red line) and the real mortgage rate (dashed blue line) described above. A comparison of the two figures indicates a close correspondence between our estimates of r_T and the real risk-free rate in the period up to and including 2007/2008, but thereafter this relationship breaks down: the tax implied discount rates remain fairly flat while the risk-free rate falls towards and then under zero. In other words, implied discount rates become disconnected from the real long yield from 2008 onwards, a finding which is consistent with Bracke *et al.* (2018) who similarly find no evidence of a drop in r_H in samples either side of the period October 2008 and March 2009 in their study of leaseholds.

What is behind these patterns? One possible explanation is tightening credit conditions. For example Butt *et al.* (2014) highlight that spreads between borrowing rates facing households and nominal risk free rates increased sharply from 2008 onwards. Another is sticky borrowing rates that arise because of fixed rate mortgages. Central government interventions to limit Council tax increases from around 2008 suggest it is less likely that changes in risk or heightened expectations about future tax hikes underpin these results. A further possibility is that unobserved changes in the capitalisation rate are driving

provided on request.

Figure 5: Implied changes in r_T



these patterns in the data. Although we cannot fully rule this out, we show in Appendix B.1 that when we estimate r_T over time, but interact taxes with above/below median share developable land averaged over 1991 and 2011, or above/below median change in share developable land between 1991 and 2011, we obtain very similar results for the evolution of r_T in elastic and inelastic places. We obtain highly similar results when we use other measures of the housing supply elasticity.

6 Ancillary regressions

6.1 Sensitivity

We perform a number of sensitivity tests on our preferred repeat sales specification in column (6) of Table 3. These are reported in Appendix B.2. In summary, we find that our results are insensitive to a number of specification changes e.g. when all currency variables are expressed in 2015 values using the Consumer Price Index, when we introduce more LA level controls variables, or when we allow test scores to vary discontinuously at

LA boundaries. Furthermore, findings are robust to various alternative sample selections, including removing a greater proportion of outlying observations, relaxing selections on home extensions, and removing small homes that may have only a single occupier (and hence may qualify for reduced taxes).

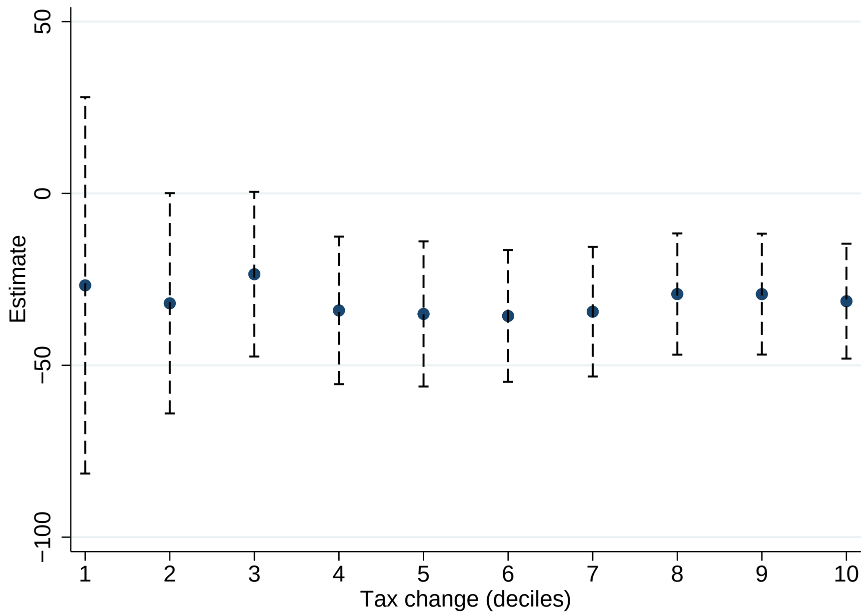
6.2 Heterogeneity

We allow for heterogeneous effects by testing the effect of taxes on house prices over the distribution of tax changes in our data, by first generating a categorical variable for the decile of the tax change in our main sample and then interacting this bin variable with the tax change. This permits us to use a more flexible functional form but is also motivated by experimental findings that small future amounts are discounted at a higher rate than larger amounts (e.g. Frederick *et al.*, 2002). Point estimates (blue dots) and the 95% confidence interval (black whiskers) are plotted in Figure 6. The effect of taxes on home values is stable across the distribution of tax changes (although for the smallest changes the estimates are imprecise).[27]

We subject our analysis range of further heterogeneity tests in which we interact property taxes with a variety of indicator variables in Appendix B.3. Although we cannot observe characteristics of home buyers, in general we find little evidence for heterogeneity. Estimated tax coefficients are highly similar for: homes of different value; homes in different regions; and homes in neighbourhoods with different socio-economic characteristics. This is at least suggestive evidence that the discount rates we uncover can be considered to be a broadly applicable parameter.

[27] We also show in Appendix B.3 that we obtain similar results when we repeat this exercise but taking deciles in the initial tax level (rather than the tax change).

Figure 6: Effect of taxes on home values for different size tax changes



6.3 Expected tax growth and risk

In recovering discount rates in Section 5, we assumed homogeneous tax growth and no role for idiosyncratic risk. Several institutional features described in Section 1.2 preclude substantial tax increases. First, homes rarely move to higher tax bands in our setting. Second, the scope for LAs to make substantial across-the-board hikes is limited by policies (central government interventions prior to 2008, the need to obtain approval from local referenda and interventions by central government to incentivise tax freezes thereafter). Taken together, these factors suggest that idiosyncratic risk and tax growth expectations are unlikely to be major determinants of discount rates in this context.

This intuition is tested in estimates reported in Table 7 where we find little evidence that discount rates covary with measures of uncertainty and a proxy for expected tax growth. One way to measure uncertainty is political instability. In places where the composition of the local Council is prone to change, we might expect to find a greater variability in taxes as local parties seek to implement their preferred policies. In column (1) of Table 7, we repeat our baseline specification but adding the interaction between Δ Tax and

Table 7: Risk and average tax growth interactions
(Dep var: Δ sale price in £)

Dep var: Δ sale price	(1)	(2)	(3)	(4)
Δ Council Tax	-29.89***	-30.78***	-29.20***	-31.00***
	(8.231)	(8.021)	(8.190)	(8.229)
× SD highest seat share in local elections	-0.76			
	(1.343)			
× SD Council Tax		0.19		1.14
		(2.271)		(2.401)
× Mean annual % Council Tax growth			-0.91	-1.34
			(1.454)	(1.520)
Number of sales pairs	186843	186843	186843	186843
R^2	0.767	0.767	0.767	0.767

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. *** p < 0.01, ** p <0.05, * p < 0.1.

the (standardised) standard deviation of the share of seats held by the largest party in the LA throughout our sample period. We find no evidence that political instability is associated with discount rates. In column (2), we replace the political measure with the (standardised) standard deviation of the annual percentage change in Council tax in the LA over our sample period. Again we find no evidence for differences in discount rates in LAs with more or less volatile tax changes. In column (3), we examine whether differences in expected growth in taxes affects our estimates by deploying the mean percentage change in taxes in the LA over the sample period in the interaction term. The interaction is not significant. Finally, in column (4), we again obtain no significant effect when we include interactions with both the first and second moments simultaneously.

7 Conclusions

Discount rates are central in many fields of economics and finance as well as in policy appraisals. Revealed discount rates also facilitate a test for deviations from the standard assumptions that underpin traditional models of behaviour in economics. In this paper we assess how home buyers value the very long term using a novel source of variation: property taxes. Such taxes are used in a wide range of institutional settings and are usually economically large.[28]

Our empirical work draws on extensive home transaction data and spatio-temporal variation in property taxes in England in the period 1998-2016. Across a variety of samples and specifications, our research implies that average discount rates implied by taxes are in the region of 3 to 4%. These rates are – as far as we can tell with available data – largely homogeneous. Our estimates add to a sparse literature that estimates long-term discount rates using observational data (e.g. Hausman, 1979; Warner and Pleeter, 2001; Laibson *et al.*, 2007), and complement experimental work focussed on shorter horizons. Findings may be of particular interest to researchers that wish to estimate annualised amenity values using house prices.

We also provide new evidence on the extent of property tax capitalisation rate for England. In common with recent quasi-experimental evidence from the US (Lutz, 2015), but in contrast to recent work using Swiss data that finds less than full capitalisation of income taxes differentials into rents (Basten *et al.*, 2017), we find that the capitalisation rate for property taxes in England is indistinguishable from one. Alongside other tests, this result supports the range of discount rates that we obtain.

Finally, we complement work on energy efficient durable purchases by assessing whether households undervalue future property taxes for the first time. Home purchases are high stakes but yet are complex and infrequent decisions which suggests households may be prone to making optimisation errors. In contrast to some findings from other settings, we find little evidence for such errors: property tax implied discount rates of 3 to 4% correspond quite closely to benchmark inter-temporal opportunity cost rates of 1 to 2.5%. However, interestingly from 2008 we observe that tax-implied rates remain flat, and thus become somewhat disconnected from prevailing real borrowing and lending rates. We put forward some tentative explanations, but must largely leave understanding this

[28] For example, according to the Institute for Fiscal Studies, in 2017/18 property taxes accounted for 9% of UK tax revenues. Corporation taxes accounted for a slightly lower share (8%).

phenomenon to future work.

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Online Appendix A Data and context

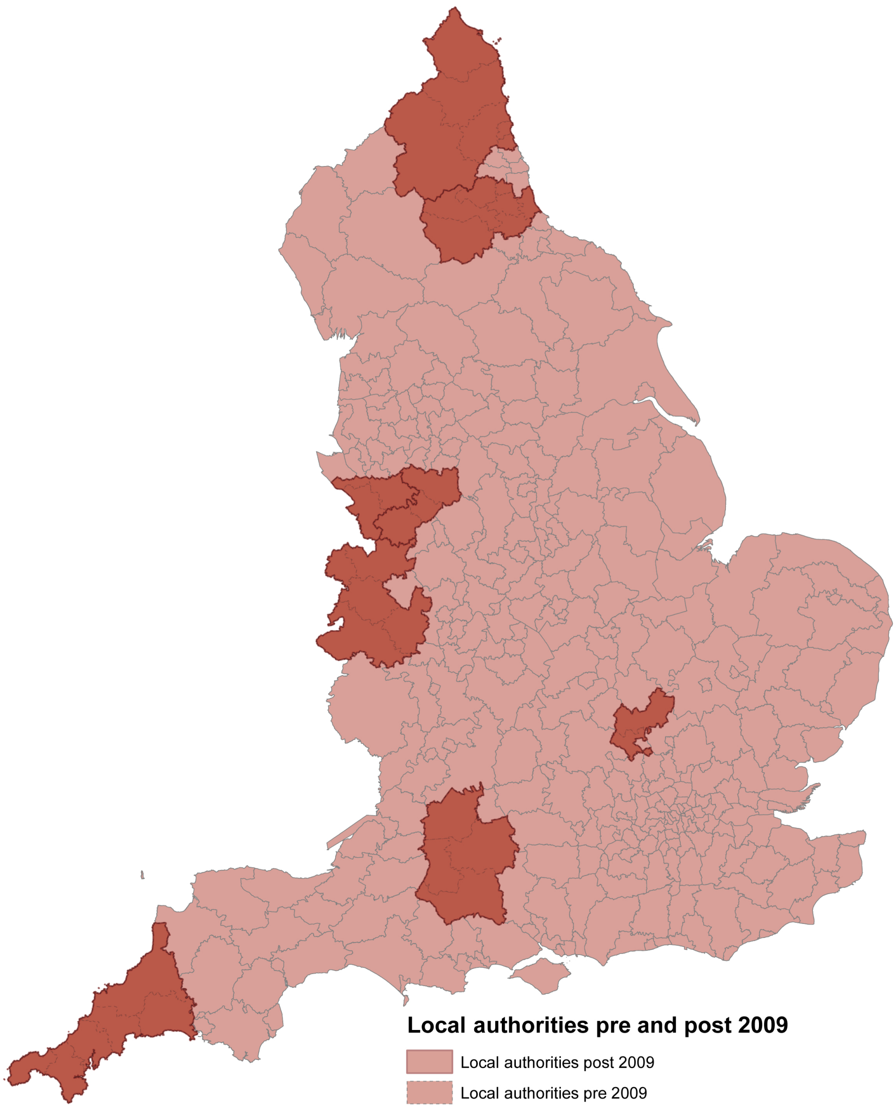
A.1 Institutional details

Although generally considered to operate under a highly centralised model, services provided by local government organisations in England – including schools, social services, roads, planning and housing, and policing – account for roughly a quarter of all public spending. Local government features multiple organisational layers, with some spatial variation in the way service delivery is structured. The chief organisational unit is the Local Authority (LA). LA boundaries changed once in our sample period, in 2009, when a series of mergers reduced the number of LAs from 354 to 326 – see Figure A1.

LAs can have either a single-tier or two-tier structure. Single-tier authorities include London Boroughs, Metropolitan Authorities, and Unitary Authorities. While LAs with a single tier structure are responsible for delivering the majority of local services, in some case such as the Greater London Authority in London, higher authorities may provide services such as policing, fire protection, and transport across several LAs. In two-tier authorities, the upper tier (or County Council), is responsible for the majority of services such as schooling and social services while the lower tier (District Council) is responsible for other services such as local planning decisions and some housing services. In some but not all places an additional lower layer of local government exists in the form of parish and town councils. The 10,000 or so parish and town councils in England provide local services, including those such as community centres, parks, and play areas, and can also have a say in local planning decisions.

The majority of these local services are paid for through grants from central government with locally raised taxes on domestic homes covering some 24.3% of local government spending in 2014/15. Council Tax has been the main instrument of local taxation on households in the UK since 1993 when it replaced the highly unpopular Community Charge (also known as the Poll Tax). Rateable Values, which preceded the Poll Tax,

Figure A1: Local authorities pre and post 2009



are still used to levy taxes on commercial properties. Rosenthal (1999) presents one of few empirical studies looking at the capitalisation of property taxes in the UK by using this taxation system. In general terms the tax is payable on all domestic homes, and in contrast to many property taxes the liability for the tax rests with occupiers rather than owners of homes and taxes are not deductible from income taxes. Collection rates are very high: for example in 2014/15 97% of taxes were collected. This in part reflects that Councils have significant enforcement powers e.g. the ability to collect regular payments out of wages or benefits, and can even apply to the Courts to enforce the sale of owned home to cover unpaid taxes.

Council Tax is essentially a property tax but has a personal element insofar as some occupants are exempt from the tax or qualify for discounts. Chief among these are that individuals living alone benefit from a 25% discount, whereas full time students and a small number of other occupants, for example members of religious communities, people who are severely mentally impaired and live-in carers, are fully exempt. In addition, a small number of homes are also exempt from Council Tax, for example furnished homes owned by a charity, homes where the previous occupant has died, been imprisoned or hospitalised, and homes that have been repossessed by creditors.

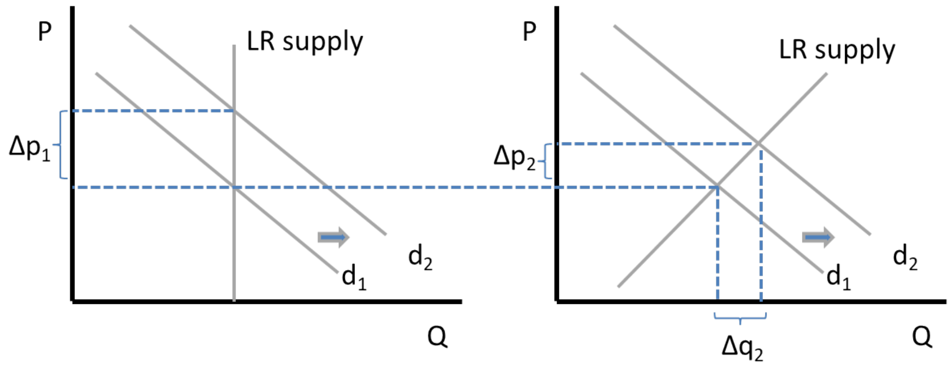
Although Council tax is collected by LAs, it will usually be composed of a series of tax levies, or precepts, from various authorities within the layers of local government described above. Any authority that raises funding through Council taxes in this way is known as a "precepting authority". Prior to the start of each financial year, each precepting authority agrees the amount that will be collected by the relevant Local Authority (or Authorities) on its behalf. Hence, the total amount of Council Tax to be collected in each administrative sub-division is determined both by the number of layers of local government that area falls within, and the sum being levied by each precepting authorities. Importantly, the vast bulk of Council Tax represents precepts from LAs with levies from parishes making up only 0.6% of the total tax burden in 2011/12.

A.2 Housing supply elasticity and capitalisation

As we discuss in the main text, regulatory restrictions on the supply of space are endemic in the British planning system. This has the effect of making the supply of housing highly inelastic. This is important in this context because it means that changes in demand will be reflected in prices: that is to say changes in demand will be capitalised into prices.

We illustrate why this is the case in Figure A2 below. On the left hand side of the Figure, with perfectly inelastic housing supply, a shift in demand leads to a change in price but no change in quantity. In contrast, with elastic housing supply (right hand side of the Figure), the same demand shift leads to both a quantity and a price change. Hence the shift in demand is not fully capitalised into prices. Hilber (2015) provides a more complete discussion.

Figure A2: Housing supply elasticity and capitalisation



A.3 Additional information about our data

We supplement our sales and rentals data with additional characteristics from Energy Performance Certificates (EPCs). Since 2007 an EPC has been required whenever a home is constructed or marketed for sale or for rent and a dataset for all EPCs issued since 1 October 2008 has recently been released by the UK government. The certificates contain information of the energy performance of buildings and their physical characteristics that are obtained by a physical inspection of the interior and exterior of the home by an independent assessor. We extract various characteristics from this dataset before merging the information into the Land Registry database. Our merging strategy is to sequentially match individual sales to the EPC data using the full address or a subset of the address and the date of the sale and certificate. Specifically, we first match a sales to certificates using the primary address object name (PAON; usually the house name or number), secondary address object name (SOAN; usually flat number), street name, and full postcode then retain the certificate that is closest in days to the sale or taking the median value of characteristics where there is more than one EPC in the same year as the sale. We then repeat this exercise for unmatched properties but allowing one of the PAON or SOAN to be different. Our final round of matching matches on the full postcode. Any sales that remain after without a match following this process are considered unmatched and dropped from the analysis. This group represents around 9% of sales in the Land Registry dataset. The matched dataset provides us the number of rooms; floor area; and the wall construction type (solid wall or cavity wall). The EPC data also records the number of extensions that have been added to the property at the time of the certificate, but provides no detail on the size or nature of any such extension.

We harvest tax data from web sources. Because the tax data and the home sales data have never been linked before, we conduct a second matching exercise to link sales in the combined Land Registry-EPC dataset. We again match homes using the full property address and the postcode of the house, but now use a more conservative matching strategy given the potential for the measurement error in our main variable when matches are incorrect. We then link home to actual annual tax payments as described in the main paper. Combining these data in this way gives us the approximate annual tax payable for each house at the time of its sale. However, this tax payable will not exactly correspond to the actual amount of tax payable in all cases because the DCLG data gives us the average Band D amount for homes in the administrative region, where the average is computed across all parishes in the LA. While this should accurately capture any precepts from higher layers of local government (such as levies for the GLA in London), it will not accurately capture sub-LA variation in parish precepts.

Parish precept data is available from CLG, but only for financial years 2013/14-2016/17. We extract this data and use it in cross-sectional regressions that use data within this time-frame to investigate whether this correction has any impact on the results. To compute home level taxes using the parish level data, we first deduct the average tax-band specific parish level precept for the LA in the relevant financial year from our LA tax data, then add back in the actual tax-band specific parish precept for the given parish.

Table A1: Descriptives statistics: repeat and non-repeat sales

	mean	sd	min	max
Panel A: 1km boundary repeat sales				
Price	170816	89613	31000	775000
Tax	1093.15	283.88	331.89	2804.42
KS2 score %	0.81	0.08	0.00	1.00
LA spend/head	792.74	657.82	64.09	2797.94
Greenspace 0-500m %	0.07	0.08	0.00	0.96
Rooms	4.28	1.15	1.00	45.00
Built after 1995 %	0.12	0.33	0.00	1.00
Extensions	0.00	0.00	0.00	0.00
Panel B: 1km boundary non-repeat sales				
Price	177877	113737	21500	785000
Tax	1136.45	365.59	214.69	3182.24
KS2 score %	0.81	0.10	0.20	1.00
LA spend/head	858.76	657.04	56.78	2854.64
Greenspace 0-500m %	0.08	0.09	0.00	0.96
Rooms	4.39	1.39	0.00	79.00
Built after 1995 %	0.13	0.34	0.00	1.00
Extensions	0.00	0.00	0.00	0.00

The geographical variables we use in the empirical work include the LA (both pre- and post-2009), the parish, and the labour-market area in which the home is located in 2011, and a rural-urban indicator based on the 2011 Rural-Urban Classification.

Regarding school test scores, the only data covering the full span of our sample is the percentage of pupils obtaining level 4 or higher in Maths, English, and Science tests and teaching assessments. Using GIS, we create measures of test scores from these data by averaging across all tests and teaching assessments available for each academic year and then matching to sales in the subsequent financial year. This means that for example test scores for academic year 2015/16 (which are published from September 2016) are linked to our house transactions in financial year 2016/17.

We provide descriptive statistics for our main repeat sales sample in the paper. In Table A1 we show that repeat sales are closely matched to other sales on observable dimensions.

A.4 Sample restrictions

The theory underpinning our work indicates that regression of prices (or rents) on taxes should be estimated in levels (see equation (1)), an issue often neglected in the capitalisation studies reviewed by Ross and Yinger (1999). We take heed of the theory in our choice of functional form, and remove outliers which we usually define as the top and bottom 1% of prices (or rents) and the top and bottom 1% of prices (or rents) in each tax-band to ensure that extreme prices are not driving our findings. We also drop a small number of Local Authorities which are extreme outliers in terms of population size or expenditure on local services. In particular we drop 2 LAs – the City of London and the Isles of Scilly, which is in any case has no boundaries with other LAs – which both have populations that are less than half the 1st percentile LA population. We also drop one further LA: Birmingham which is vastly bigger than all other LAs – its population and expenditure on services are both more than double the value of the LA at the 99th percentile. We also note that an article in the Birmingham Post highlights that this LA is also an outlier as it generates the least income from Council tax despite having a relatively high charge due to it's cheap housing.[29]

[29]See http://www.birminghampost.co.uk/news/regional-affairs/birminghams-council-tax-income-lowest-9746303.

Online Appendix B Other results

B.1 Time variation in β/r_T

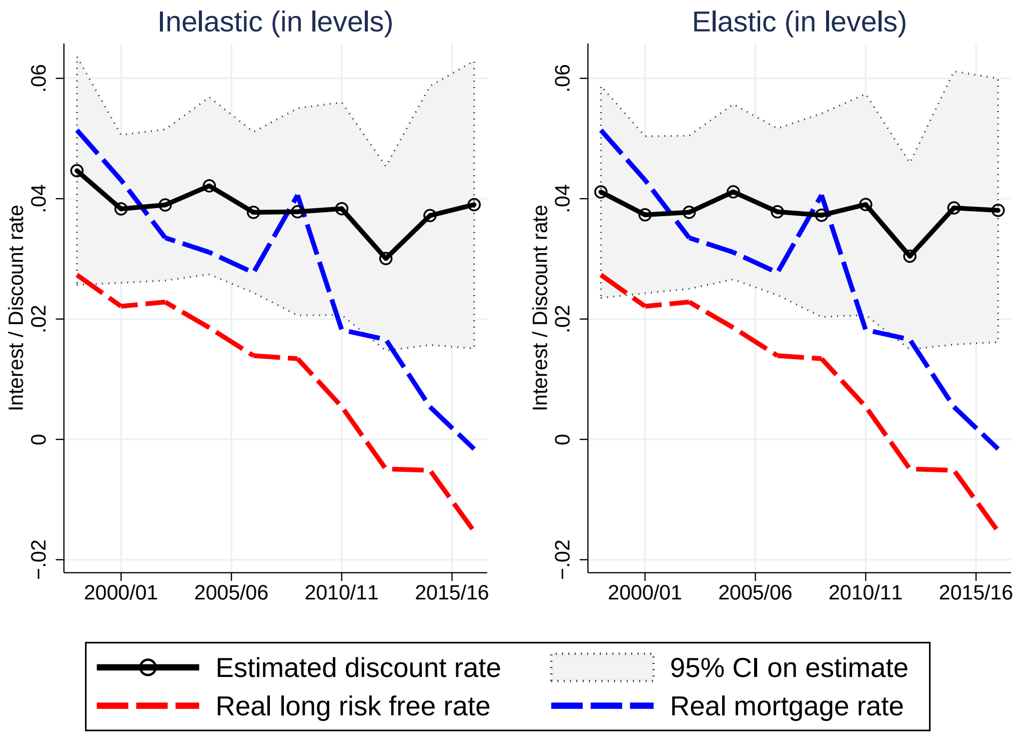
One assumption in the above estimations is that discount and capitalisation rates are constant across time, i.e. $r_t^T = r_T$, $r_t^H = r_H$, and $\beta_t = \beta \quad \forall t$. Here, we allow the discount rate to evolve over time, which is consistent with findings in Bracke *et al.* (2018). Differencing between time t and τ, assuming $\beta_t = \beta = 1$, and ignoring P for expositional simplicity we obtain:

$$\Delta V_{it\tau} = -\frac{1}{r_t^T} T_{it} + \frac{1}{r_\tau^T} T_{i\tau} + \frac{\rho_t}{r_t^H} H_i - \frac{\rho_\tau}{r_\tau^H} H_i \tag{B.1}$$

Note that when estimating the above equation r_t^T, and r_τ^T etc. should be internally consistent. For example, in one transaction pair $t - \tau$, r_t^T should be the equal to r_τ^T in another transaction pair $\tau - \tilde{\tau}$. Our solution is to constrain coefficients to be the same for each year by interacting time dummies with variables, but assigning a positive or negative sign depending on whether they represent the first sale in year t or the second sale in year τ for the sales pair in question.

We use this approach to estimate the evolution of implied discount rates in Figure 5. In Figure B1 below we can further show that the time pattern is very similar for places with above and below median share developable land averaged over 1991 and 2011. In Figure B2 we show that the time pattern is also very similar for places with above and below median change in share developable land between 1991 and 2011. In unreported results we obtain highly similar results when we use alternative measures of the housing supply elasticity.

Figure B1: Time variation: above/below median share developable land in LA



B.2 Robustness

Sensitivity of our main result in column (6) of Table 3 to changes in specification and sample are investigated in Tables B1 and B2. The first of these tables shows that estimates of β/r_T are not significantly different to our baseline result under a variety of specification changes. First, we express all currency variables in 2015 values using the Consumer Price Index (column (1)). The coefficient is also robust to interacting our boundary trend controls with a property type indicator in column (2), which implies identification is achieved by comparisons between homes of the same type across LA boundaries, e.g. detached houses in Band D. The coefficient is slightly more negative when we replace property type in this interaction with an indicator for the home being built after 1995 (column (3)). When we re-specify the public good controls (columns (4) and (5)), or introduce more LA level controls variables (column (6)), the results are robust.

Table B2 in the explores sensitivity to sample selections. The first column increases the

Figure B2: Time variation: above/below median change in share developable land in LA

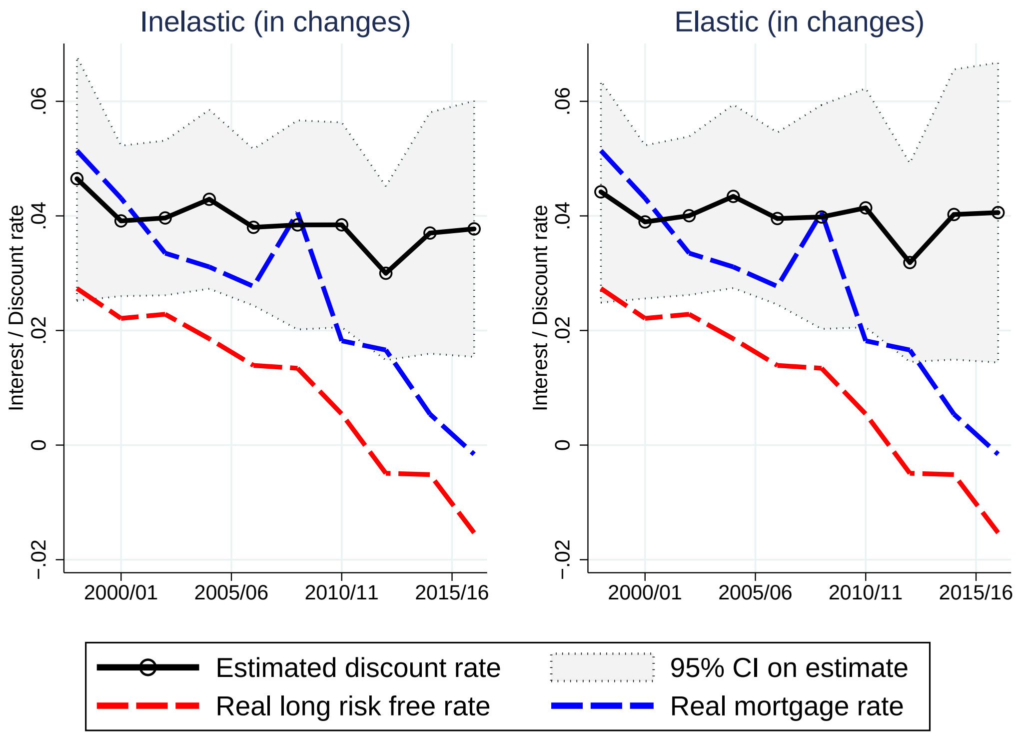


Table B1: Sensitivity – specification
(Dep var: Δ sale price in £)

Dep var: Δ sale price	(1)	(2)	(3)	(4)	(5)	(6)
Δ Council Tax	-27.48***	-32.50***	-34.74***	-29.01***	-29.83***	-27.09***
	(7.234)	(8.256)	(9.041)	(8.493)	(8.379)	(7.627)
Years×band×BFE	✓			✓	✓	✓
Years×band×BFE×type		✓				
Years×band×BFE×post95			✓			
Change to baseline:	in 2015 prices	fixed effects	fixed effects	linear LPGs	smooth test scores	more LA controls
Observations	186843	165236	176708	186843	186843	186843
R^2	0.779	0.791	0.772	0.767	0.767	0.767

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. *** p $<$ 0.01, ** p $<$0.05, * p $<$ 0.1. Type in column (2) refers to a categorical variable for the type of property (detached house, semi-detached house, terraced house, flat). Post95 in column (3) refers to an indicator that takes value 1 if the home was built after 1995. Continuous test scores in column (5) vary smoothly over space and are not constrained by LA boundaries. Additional LA controls in column (6) are population, LA total service expenditure, and value of commercial property in LA (rateable value).

Table B2: Sensitivity – restrictions
(Dep var: Δ sale price in £)

Dep var: Δ sale price	(1)	(2)	(3)	(4)	(5)	(6)
Δ Council Tax	-26.59***	-27.60***	-34.82***	-28.91***	-30.82***	-29.47***
	(7.225)	(8.468)	(9.080)	(7.825)	(9.346)	(8.419)
Years×band×BFE	✓	✓	✓	✓	✓	✓
Change to baseline:	cut 5% prices	include new at last sale	drop new since 1995	allow 1 extension	any time gap	≥ 3 hab. rooms
Observations	180136	213178	161549	276998	195857	179465
R^2	0.773	0.768	0.766	0.744	0.765	0.767

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. *** p < 0.01, ** p <0.05, * p < 0.1. Cut 5% of prices indicates that the top and bottom 5% of prices overall and in each tax band are dropped. Any time gap relaxes the restriction that the gap between sales must be between 1 and 8 full years.

scale of the restrictions on prices by cutting the top and bottom 5% of prices overall and in each tax band. The coefficient is somewhat less negative, albeit it is not statistically discernible from our baseline specification. Consistent with the the result above, we again find that the coefficient is more negative when we drop homes which we know were built after 1995. The final three columns show that findings are insensitive to relaxing selections on extensions and the imposition that the gap between sales must be between 1 and 8 full years and removing small homes that may have only a single occupier (and hence may qualify for reduced taxes).

In Table B3 we provide a number of robustness checks for the cross-sectional regressions reported in the main paper. In our main repeat sales results we specify taxes at the LA level because data for parish level taxes is not available for our whole repeat sales sample timeframe. Our proposition is that this is unlikely to be a problem as parish taxes made up only 0.6% of the total tax burden in 2011/12. We confirm here that this is indeed the case. Findings for rents (columns (1) and (2)) and prices (columns (3) and (4)) are robust to using Parish-specific taxes, which likely reduces measurement error in the Council Tax variable. In columns (5) and (6), we also show that non-repeat sales do not yield significantly different coefficients to repeal sales, which is unsurprising since

Table B3: Cross sectional robustness
(Dep var: rent or sale price in £)

	(1)	(2)	(3)	(4)	(5)	(6)
	- rents -		- prices: repeats -		- prices: all -	
Council Tax	-0.90	-1.06*	-24.33*	-25.79*	-28.22*	-27.27*
	(0.60)	(0.64)	(13.86)	(13.89)	(13.80)	(14.01)
Council Tax × non-repeat					4.02	5.62
					(14.30)	(14.11)
Quadratic in LA spend/head		✓		✓		✓
Quadratic in KS2 test scores		✓		✓		✓
Local green space		✓		✓		✓
Home characteristics	✓	✓	✓	✓	✓	✓
Year×taxband×1km BFE	✓	✓	✓	✓	✓	✓
Tax measured at:	Parish	Parish	Parish	Parish	LA	LA
Observations	24906	24906	81103	81093	190155	190145
R^2	0.742	0.742	0.928	0.936	0.914	0.921

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. *** p < 0.01, ** p <0.05, * p < 0.1. All regressions are cross-sectional specifications estimated in levels. Sample restrictions and controls as in main cross-sectional results.

they have highly similar characteristics to repeat sales (see Appendix Table A1).

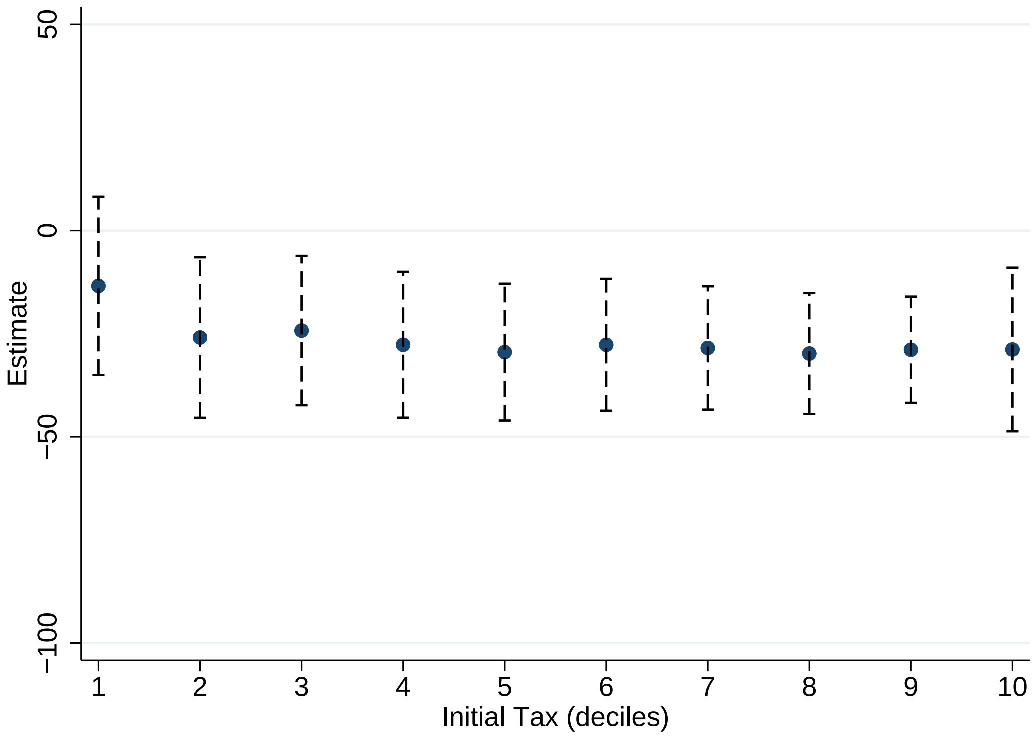
B.3 Heterogeneity

Section 6 tested for possible non-linearities by interacting decile of the tax change in our main sample and then interacting this bin variable with the tax change. We obtain similar results when we repeat this exercise but taking deciles in the initial tax level (rather than the tax change) in Figure B3. This leads us to conclude that there is little evidence that smaller and higher amounts are discounted at different rates in our setting.

We subject our analysis range of further heterogeneity tests using a variety of indicator variables, displaying results in Figure B4. Estimated tax coefficients are highly similar for: homes in different tax bands (top left);[30] homes in different regions (top right); homes in neighbourhoods with different median incomes in 2004 (bottom left) and different share

[30]Note that the coefficient for band F homes is based on a relatively small numbers of observations (4000 repeat sales), many of which are in London, which may explain the difference in coefficients. Our main results are insensitive to excluding these sales.

Figure B3: Effect of taxes on home values for different initial taxes



of residents with degree in 2011. This is at least suggestive evidence that the discount rates we uncover can be considered to be a broadly applicable parameter.

Figure B4: Effect of taxes on home values: further heterogeneity