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. We find that discount rates implied by perpetual property tax financial flows and house prices are rather low (between 3 and 4%) and relatively stable over time and space. Interpreting the effect of taxes on home values as discount rates is supported by results that indicate property taxes are fully capitalised into rents.

Keywords housing, property taxes, discount rates, capitalisation rates

JEL codes G10, R30

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Assumptions about discount rates underpin an array of economic models and feature as a key parameter in policy appraisals in settings such as climate change, infrastructure investment, and place-based policies. Understanding how individuals trade present costs and benefits against those in the mid to far future in practice may help to guide or inform these assumptions.[1] More generally, understanding discounting over these horizons can help to understand how individuals behave when making decisions relating to the long–term, and may be informative about how individuals respond to policy interventions.

Personal discount rates can be estimated using experiments in which subjects choose between relatively small–stakes (and often hypothetical) rewards at specified future dates, or may be obtained from observational data. The experimental evidence typically focuses on short time horizons and suggests that households place little weight on the future.[2] The evidence for longer horizons is more sparse and comes from observational data sources. In some cases estimates are derived from narrow groups in society or relatively unusual circumstances such as military downsizing (e.g. Warner and Pleeter, 2001) or energy efficient durable purchases (e.g. Hausman, 1979). In others, long–term rates have been estimated using structural models underpinned by a variety of assumptions (e.g. Gourinchas and Parker, 2002; Laibson *et al.*, 2007; Fang and Silverman, 2009).

The longevity of real estate makes housing markets amenable to the analysis of discounting over very long time periods that go beyond the term of most products available in financial markets. Rates obtained from housing settings should also benefit from a high degree of external validity because of widespread market participation and because households devote a significant share of their spending to their homes.[3] These factors underline

[1]The application of private discount rates to social projects is of course extremely contentious - see Nordhaus (2007); Weitzman (2007) and Stern (2007) for opposing views within an environmental context.

[2]Frederick *et al.* (2002) review the experimental literature. Studies attempting to elicit rates over longer time spans are rare but include Eckel *et al.* (2005) (7 years) and Grijalva *et al.* (2017) (20 years).

[3]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. 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 (Glaeser *et al.*, 2013) and predict the extent to which households will consume out of house price growth (Aladangady, 2017).

the contribution of recent research endeavours that use leasehold tenure to make direct inferences about how households discount the future (Wong *et al.*, 2008; Giglio *et al.*, 2015a,b; Bracke *et al.*, 2017; Fesselmeyer *et al.*, 2016). The evidence emerging from this literature suggests that housing market discount rates are low at very distant horizons (around 2%) and declining over the time horizon, but as yet provides limited insight into how rates evolve over time or correlate with covariates. Furthermore, residential leasehold is specific to a small number of countries so that the robustness of findings can not be readily replicated elsewhere.

In this paper we estimate discount rates from perpetual financial flows associated with property taxes using extensive house sales and tax data spanning around 20 years. Identifying discount rates from property taxes benefits from the external validity of housing market settings while yielding discount rates that are plausibly unrelated to housing risks and expectations of future housing prices. The starting point for our analysis is that for two houses identical in all respects except the second is liable to pay an additional annual £100 in property taxes, one would expect the second house to trade at a discount to the first. When taxes are fully reflected in prices, under competitive bidding in housing markets the discount should be equal to the present value of £100 in perpetuity, from which in turn we can work out how individuals are implicitly discounting the future.

Taking this intuition to the data, our results across a number of specifications imply that households apply discount rates of between 3 and 4 percent when valuing perpetual flows of property taxes. Our baseline approach exploits *inter–jurisdictional* variation in property taxes by focusing on properties close to local administrative boundaries and including boundary-year fixed effects. We generate estimates by comparing homes of similar quality, exploiting assessment practices that group homes with similar values into a series of 8 tax bands. Moreover, we carefully control 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 many previous studies. To counter any residual concerns that public goods may be driving our estimates, we then show that very similar results can be obtained when we use *intra–jurisdictional* variation in taxes. This alternative approach generates estimates from comparisons of neighbouring properties within the same jurisdiction, and hence which have access to the same pubic goods. Identification is achieved by retaining homes which are close to tax band thresholds and including threshold-year fixed effects.

Our work is related to an extensive urban public finance literature going back to Oates (1969) that analyses the extent to which house prices respond to fiscal variables. A well-established insight from this literature is that the effect of taxes on home prices is governed by both a capitalisation rate and a discount rate.[4] Hence, the discount rate can only be truly identified if we know the capitalisation rate. Given that the public finance literature is far from conclusive, we use ancillary data on rents to estimate the capitalisation rate for the period 2013-2016. We show that the estimated rental capitalisation rate is close to and not significantly different from 100% and around 85% which supports the range of discount rates that we compute.[5]

The contribution of this paper is threefold. To our knowledge, we are the first paper that uses nationwide data on property taxes and housing transactions to estimate discounting parameters. Second, our work complements an emerging literature that estimates discount rates in housing markets, but using a different source of variation: property taxes

[4]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 40%–140% (see e.g. the recent study by Basten *et al.* (2017) and reviews contained in Yinger (1982) and Ross and Yinger (1999)). The capitalisation rate is important because the extent to which house prices respond to fiscal variables (a) quantifies household responsiveness to fiscal conditions and is directly informative to understanding the consequences of policy; (b) may influence behaviour through generating incentives that could differ between home-owners and renters; and (c) may imply (possibly unintended) redistribution between different groups. For a more complete discussion see Hilber (2015).

[5]This finding of full capitalisation into rents is consistent with full capitalisation into prices and suggests highly inelastic housing supply. There is considerable evidence that stringent regulatory restrictions on land and a scarcity of developable land in urban areas in England do indeed lead to very tight housing supply (e.g Hilber and Vermeulen, 2016). We corroborate the rental capitalisation parameter by showing that tax coefficients for prices are only slightly smaller in absolute terms in places with more elastic housing supply in our setting.

rather than lease length. The third contribution lies in robustly identifying the effect of property taxes on housing values. Given the sheer size of our dataset, by identifying effects by using changes in taxes, and by controlling for boundary fixed effects, we ostensibly control for all unobserved traits that may be correlated to taxes. A causal interpretation of our findings is further bolstered because findings are largely insensitive to the source of identification, the inclusion of control variables, and a range of sample and specification changes.

The remainder of the paper is structured as follows. In Section 1 we analyse the current state of the literature and 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 results and in Section 5 we conclude.

1 Background

1.1 Capitalisation literature

Our empirical work is grounded in 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, in equilibrium the value (V) 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:[6]

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

The first term in this capitalisation equation – the before tax value of the home – is the product of units of housing services (H) 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

[6]The tax capitalisation equation can be equivalently derived from an pricing approach or a household utility maximisation problem (see e.g Yinger, 1982; Yinger *et al.*, 1988; Ross and Yinger, 1999)

of the annual property tax payment (T) and a tax capitalisation parameter (β). Both terms are expressed as present value 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 might assume that the pre–tax value and taxes are expected to grow at constant growth rates g_H and g_T such that r_H and r_T can be interpreted as net of growth discount rates $r_H = r'_H$ - g_H= and $r_T = r'_T - g_T$. We can put some further structure on the gross discount rates r'_H and r'_T by assuming they can be decomposed into a (common) risk free rate r^f and risk premia r_H^{RP} and r_T^{RP} which may vary across the two terms according to the riskiness of housing and tax flows respectively.

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. $\beta = 1$. However, 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.

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 differences in discounting assumptions, but at least two further issues could drive differences in estimates. First, researchers have met identification challenges with varying degrees of success.[7] 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)*

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

housing supply elasticities; and *(iv)* expectations about future taxes (Yinger, 1982; Ross and Yinger, 1999; Hilber, 2015).[8] These factors could plausibly drive the residual variation in estimates of β from studies that use boundary designs alongside other strategies to mitigate endogeneity concerns – in particular, Gallagher *et al.* (2013) find close to full (100%) capitalisation of property taxes into home values, whereas Basten *et al.* (2017) show the rate at which income taxes capitalise into rents falls in the range 44%-57%.

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 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. We assume that by dividing through equation 1 on both sides by a discount rate r_H 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 $\tilde{\beta} = \beta \frac{r_H}{r_T} = \beta(r^f + r_H^{RP} - g_H)(r^f + r_T^{RP} - g_T)^{-1}$.[9]

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

[8] Ross and Yinger (1999) demonstrate the potential importance of the last of these factors by showing that β will be 26% if taxes fully capitalise but differences are only expected to persist for 10 years due to e.g. a revaluation.

[9] Two further points are worth noting. First, if rents can be obtained by dividing V 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 to the sum of the demand and supply elasticities i.e. $\epsilon_D/(\epsilon_S - \epsilon_D)$. This is analogous to the theoretical determinants of the capitalisation rate.

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 arguably a much more convincing identification strategy.

An emerging and as yet unconnected literature estimates housing market discount rates (r_H) by regressing home sales prices on measures of the length of future ownership at the time of sale and observable controls. The resulting estimates of price discount for tenures of varying lengths against perpetual (or very long) ownership can then be transformed to generate estimates of implied housing market discount rates. Giglio *et al.* (2015a) focus on the very long term by comparing discounts for homes in Singapore and England with ownership lengths in the range 80–999. Their findings are consistent with households using very low gross discount rates at these horizons (around 1.9%). Bracke *et al.* (2017) instead focus on the range 1-99 years using datasets for central London. Their findings are broadly consistent with Giglio *et al.* (2015a) but their research design permits them to further show that the term structure of rates is declining, and additionally that average implied gross rates in 1987-1992 (4.1%) are significantly higher then in 2004–2013 (2.5%). In the next section, we describe literature that has estimated personal discount rates from other sources of variation.

1.2 Discounting literature

Private discount rates can be revealed in individual behaviour or consumption patterns, or can be elicited in experiments. Studies can focus on short– or long–term rates or some combination of the two. Regardless of the method adopted or the time frame under consideration, a central issue in any such undertaking is the extent to which measured discount rates reflect time preferences or other factors that affect inter-temporal decisions, for example consumption smoothing, inter–temporal arbitrage, uncertainty, inflation, and the curvature of the utility function (Frederick *et al.*, 2002).

Recent experimental evidence has sought to address these confounding issues, for example by explicitly separating risk and time preferences (e.g. Andersen *et al.*, 2008; Andreoni and Sprenger, 2012; Attema *et al.*, 2016).[10] Although the bulk of experiments still use financial rewards, the extent to which choices over time–dated income flows (or indeed any tradeable reward) can reveal time preferences is debateable (Cubitt and Read, 2007), so that some studies such as Augenblick *et al.* (2015) utilise time–dated consumption choices. The underlying concern with using money rewards is that given access to capital markets, rational individuals should simply maximise (minimise) the net present value of rewards (payments) on offer and adjust savings or borrowing behaviour accordingly. Revealed discount rates will then converge on market interest rates, with deviations suggesting that subjects are unaware of, ignore, or are unable to access inter-temporal arbitrage opportunities, for example because of credit constraints.

Experiments almost wholly focus on inter–temporal tradeoffs in the short–term, so that we largely need to look to studies using observational data for evidence about discounting over time frames longer than 10 years. Again, studies can be split into those that use consumption or fungible financial flows. Within the first group is a body of work that uses structural models to back out time preference parameters. Under preferred assumptions, recent research using consumption and saving data are consistent with long–run annual discount rates in the region of 4% (e.g. Gourinchas and Parker, 2002; Laibson *et al.*, 2007). In contrast, in a structural exercise using welfare participation decisions, Fang and Silverman (2009) suggest long–term rates (or patience factors) of 13.5%.[11]

The second group comprises studies that obtain discount rates from financial flows over

[10] Aside from the confounds, experimental studies may face challenges because of selection into participation by subjects, framing effects, transaction costs, hypothetical or unreliable rewards – see Chabris *et al.* (2008).

[11] Several other studies using structural models could be included here but are not for reasons of brevity. It should be noted that in some other settings, structural models have been unable to identify long–run patience parameters e.g. Paserman (2008).

long time spans. In experimental studies, Grijalva *et al.* (2014, 2017) estimate annual discount rates of 1.9 to 5.5% at 20 year time horizons. This group also contains some inferential studies that reveal rates from purchases of durable goods or the choice of retirement packages. In an early study, Hausman (1979) estimates implied discount rates of around 20% from purchases of air conditioners that have an average life of around 10 years. More recent work has centred pricing of fuel efficiency in automobiles purchases. Findings in Allcott and Wozny (2014) suggest a discount rate of around 15% rationalises purchases while Busse *et al.* (2013) obtain (nominal) rates that range from 2 to 20% and which are generally comparable to the interest rates paid when purchasing. Warner and Pleeter (2001) estimate discount rates of 0–30% using choices in retirement packages in military downsizing decisions, although the validity of results has been questioned (Harrison and List, 2004). Using a similar setting Brown *et al.* (2015) show that only around a quarter of their sample of 2,500 Croatian retirees took an annuity with an implied nominal IRR of 28% over a lump–sum, which is suggestive of very high rates.

These studies collectively suggest considerable uncertainty surrounding the discount rate that households apply to long–term money and consumption flows. It is also largely unknown whether whether money and consumption discount rates change over time and, with the exception of Bracke *et al.* (2017), the available evidence is wholly from experiments over time–dated money flows. Meier and Sprenger (2015) find a correlation of around 0.5 in implied rates for the same individuals when rates are elicited one year apart. Using panel data with individual fixed effects for 1972 to 1974, Krupka and Stephens (2013) find large changes in individual discount rates. Their findings support the idea that rates are sensitive to financial circumstances, and are consistent with sensitivity of discount rates to the real interest rates that household face.

1.3 Institutional setting

In this section we describe the institutional details about Council Tax in England. We focus on the core information relevant to our empirical work here and relegate further details to the Appendix. The main unit of local government in England are Local Authorities (LAs). LAs are responsible for a range of 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 that be a 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 information about the level of the tax on each home is transparent and easy for home buyers to obtain.

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). Homes are allocated to tax bands by an assessment of the value of the home in 1991 (see Table 1 for the valuation thresholds). Contemporaneous homes were assigned to tax bands in the early 1990s, while homes built since this time are assessed upon construction. It is very rare for homes to move tax band: official data for 2010/11–2014/15 indicates that only 0.2% of the housing stock is re-banded each year, so the stock of homes in each band is essentially fixed – see Figure 1.[12] 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, the chief source of variation in Council tax arises

[12]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.

through LA tax setting decisions. Figure 2 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 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 3 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 is only - 0.014.[13]

Council taxes increased above 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 that wish to raise taxes above 2% (in some cases 4%) need to gain approval of a local referendum. 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 rise more rapidly.

[13]We obtain this figure from an LA level regression of average annual growth for Band D homes 1998/99–2016/17 on the Band D levy in 1998/99 and a constant. The R^2 from this regression is 0.031.

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 tax band κ-year pair 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), this 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 seems the assumption 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 2km) 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 spatial boundary discontinuity designs 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 sorting.

The preferred specification to be estimated would then be:

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

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

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. However, on the flip side this approach means that we are unable to use the tax band×year fixed effects that we adopt 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 most narrow geographical fixed effects available to us (postcode×year), retain homes at the boundaries of the tax band thresholds that are shown in Table 1, and include 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. 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 one of the thresholds e.g. homes with 1991–equivalent prices close to the threshold between bands A & B of £40,000. Note that the term $P_{it\tau}$ is not included as the individual elements are subsumed within the spatial fixed effects in this specification.

2.3 Estimating capitalisation and discount rates separately

The next step is to obtain information about β 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 apply a similar boundary design 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 seems to be 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.

3 Data

3.1 Home sales and rentals data

To make headway in disentangling the parameters of the tax capitalisation equation we use data on home sales, rentals, and property taxes. We provide key information about our data here and further details in the Appendix.

Our chief source of house price information is the Land Registry Price Paid dataset, which provides information on the universe of home sales in England that were registered in the period 1995 to 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 set of 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 we obtain 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. Given that these records provide us with very few home characteristics we match in additional characteristics — number of rooms; floor area; wall construction type, and the number of extensions to the property — from Energy Performance Certificates (EPCs).

3.2 Property taxes

We obtained current Council Tax band data for houses from the website mycounciltax.org.uk using web scraping techniques in early to mid 2017. The data contains the full property address and the *current* Council Tax band for the house, i.e. we are unable to access data on the tax assessment for earlier periods which means when we assign taxes to homes we may do so with error. We do not consider this to be a major issue since the number of properties changing tax band is *very* small (0.20-0.25% each year). We then use tables available from the Department of Communities and Local Government (DCLG) to compute the annual Council Tax payable at the time of each home sale or rental. 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.[14]

3.3 Additional variables

We geocode and append geographical variables to our home sales and rental datasets using the National Postcode database available from EDINA. We use GIS to identify home sales and rentals that lie within fixed distances of a boundary with another Local Authority. Boundary samples are computed for both pre–2009 and post–2009 LPAs, but as the composition of these samples are highly similar and the post–2009 boundaries contain fewer boundaries we use these samples in our main regressions.

The Chartered Institute of Public Finance and Administration (CIPFA) provides us with Local Authority expenditure on services per head of population for financial years 1997/98–2016/17. We also generate postcode level school quality measures for the same time period using data on 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 average 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 LPA boundaries. A second measure, which we use for robustness is computed across the nearest four schools regardless of LEA.

We calculate access to green space using data for 2015 from Ordnance Survey, as well as data on parks and gardens from the National Heritage List for England. The share

[14] We also compute for robustness checks tax payments at the parish level for a subset of our data - see the Appendix for details. The correlation between taxes measured at the parish and LA levels in our data is 0.997.

of green space for each postcode is computed using two distance buffers – 0-500m and 500m-1,000m.

3.4 Sample restrictions

We make a number of sample restrictions to ensure that findings are not driven by outliers, to minimise our results being driven by unobserved changes to homes, and to mitigate measurement error issues. Further details of our sample restrictions are listed in the Appendix. 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 as well as data points for 3 LAs which are extreme outliers in terms of population size or expenditure on local services which we define as more than double the 99th percentile or less than half the 1st percentile. We then remove homes for which characteristics such as size have changed throughout 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 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 i.e. they are changing in a way we cannot observe or measure. 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

[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.

rate may partly capture a discount rate. We show sensitivity to many of these sample selections in Table 9.

3.5 Descriptive statistics

The central dataset used in our analysis 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 of the Table describes the full dataset both without sample restrictions (LHS) and with restrictions (RHS). Panel B of the Table repeats this format but describes the sales that lie within 1 km of a boundary with a different LPA, 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. The Table 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 LPA spending per head.

3.6 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 do 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 4), and changes in taxes between census years 2001 and 2011 (Figure 5). To obtain the figures, we first work out which OAs are in boundary samples. We then assign them low or high tax side of boundary based on taxes in 2011 or changes in taxes between 2001 and 2011. Some OAs are close to multiple LA boundaries so we drop any that are on are high tax on 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 share 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 4 shows that there is 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 5)). The latter is important, as our main identification strategy relies on temporal variation in taxes and house prices around LA boundaries.

4 Results

4.1 Inter–jurisdictional estimates of average β/r_T (1998-2016)

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 set of restrictions described in the previous Section. 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 are subject to different LA–wide tax levies. In other words, we are estimating the parameters of tax capitalisation from inter-jurisdictional variation in taxes here.

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).[16] Results imply that a one pound increase in tax leads to a house price decrease of £73.79. On the assumption that β is indeed between 0.75 and 1, the implied discount rate r_T is between 0.010 and 0.014.[17] One potential problem with this specification is that changes in taxes may be correlated with 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. To counter the impact of this potential confounder, in column (3) we control for distance to city centre by interacting the fixed effects with a categorical variable capturing 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 is around 0.03.

All remaining columns in Table 3 are based on boundary samples and include boundary fixed effects (BFE) instead of TTWAs in a three-way fixed effect interaction. In column (3) we only include observations within 2km of an LA boundary. Results are similar to column (2) but more precisely estimated. The estimates are virtually the same 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 $\beta = 0.75$.

[16]TTWAs are defined by commuting patterns. These are 149 TTWA areas in England in the most recent data recorded by the Office for National Statistics

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

4.2 *Intra–jurisdictional estimates of average β/r_T (1998-2016)*

Table 4 reports results from the intra–jurisdictional approach described in equation (6) which uses very narrow geographical fixed effects, and retains homes at the boundaries of the tax band thresholds that are shown in Table 1. Homes are allocated to a threshold using various rules. The baseline specification in column (1) includes 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 (2) the bandwidth is set at 10% of the relevant threshold; in column (3) the bandwidth is £5,000 for the A–B threshold and increases by £1,000 at each subsequent increment. Estimated coefficients are somewhat sensitive to the method but all are broadly similar to our baseline results in column (6) of Table 3. We conclude that both inter–jurisdictional and intra–jurisdictional variation implies similar discount rates.

4.3 *Evidence on $\tilde{\beta}$ and β*

In Table 5 we aim to identify $\tilde{\beta}$ directly by using data on rents. Note that we now identify the effect of the Council Tax on rents using spatial variation only. In the first column we estimate a specification that includes controls for housing attributes but not public goods. This indicates a capitalisation rate of around 0.86. Column (2) is the preferred specification that adds controls for LA spending, test scores., and access to green space. This specification seems to suggest that the capitalisation rate $\tilde{\beta}$ is very close to and not statistically significantly different from one, meaning that one pound increase in taxes leads to a one pound decrease in rents.[18] To the extent that areas with higher taxes and correspondingly higher levels of public goods disproportionately attract higher educated people or households with a higher income (see Figure 4), we would expect $\tilde{\beta}$ to be

[18]This is largely consistent with findings in the literature. For example, Carroll and Yinger (1994) find 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)"* (text from John Yinger's website). More recently, Löffler and Siegloch (2015) find that the full burden of property taxes in Germany falls on landlords in the short–run.

more positive. Hence, the current estimate is, if anything, an underestimate. In column (3) we correct the Council Tax using ancillary data on Parish-specific taxes. This new tax measure likely reduces measurement error in the Council Tax. Nevertheless, the estimate is very similar to the coefficient in the previous column. 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. This becomes particularly apparent when we only include rental observations within 1km of a LA boundary and repeat the same set of specifications in columns (4)-(6), Table 5. The point estimates are very similar to the previous specifications and close to one, but only marginally significantly different from zero.

A main worry is that the cross-sectional identification strategy is less convincing in identifying a causal effect of taxes on property values, e.g. because of sorting. In Table 6 we therefore repeat the previous analysis but take 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 repeat sales and temporal variation in taxes and prices, this will increase the confidence that $\tilde{\beta}$ can be interpreted as a causal estimate. The results in Table 6 indeed strongly suggests that the results are robust, as the effects are remarkably similar to the preferred specifications reported in Table 3. Given a β–estimate of unity, the implied discount rate r_T sits between 0.030 and 0.040.

These results suggest that rents fully capitalise property taxes. However, given the considerations in Section 1.1 we have no direct way to ascertain that $\tilde{\beta} = \beta$.[19] In Appendix Table A1, we therefore report further findings to suggest that variation in β is unlikely to alter our main findings. Specifically, we interact the tax variable in column (6) of Table 3 with various indicators capturing housing supply elasticity, building on theoretical and empirical findings in the capitalisation literature that β should be higher (in absolute

[19] The comparison in Section 4.4 shows that when using a similar geography our estimates of r_T for 1998–2016 closely match estimates on r_H reported in Bracke *et al.* (2017) for the period 2004–2013. This gives us confidence that we can safely interpret $\tilde{\beta}$ as a good proxy for β since $\tilde{\beta} = \beta\frac{r_H}{r_T}$.

terms) when housing supply is less elastic (Cheshire and Sheppard, 2004; Hilber and Mayer, 2009; Hilber *et al.*, 2011; Hilber, 2015). This approach should be considered to be descriptive as we cannot simultaneously control for other possible sources of heterogeneity in discount and capitalisation rates. Nevertheless, should there be large differences in the capitalisation rate, we would expect these to show up in these results.

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 (2)). However, in the remaining columns of the Table 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)). This likely reflects that many urban places in England are subject to stringent land supply restrictions. Results are consistent with full capitalisation of taxes into prices in our setting and give us further confidence that we can interpret previous coefficients as $1/r$.[20]

4.4 Discount rates

On the basis of full capitalisation, our baseline inter–jurisdictional specification implies that households value future tax flows using net of growth discount rates (r_T) of 0.033, or in percentage terms 3.3%. This estimate is corroborated by our baseline inter–jurisdictional estimate of 0.031, or 3.1%. A natural comparison for our work is provided by Bracke *et al.* (2017), who estimate net of growth average discount rates on future housing service flows (r_H in our notation) in Prime Central London of 4.1% in 1987–1991 and 2.5% for the period 2004–2013. Although we do not directly estimate tax discount rates for the same period and geographical area, we estimate that r_T= 2.8% in Inner

[20] 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).

London across the period 1998–2016 in Table A1.[21] The similarity of our findings with the latter results reported in Bracke *et al.* (2017) suggests that r_T and r_H are of similar magnitude.

Following Giglio *et al.* (2015a), we can interpret our estimates using the expression $r_T = r^f + r_T^{RP} - g_T$. In our setting, property taxes grew in real terms on average at a slightly faster rate than inflation between 1989 and 2016 but have been flat in real terms since 2004.[22] 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 consumption risk. However, tax movements are tightly correlated with changes in Local Authority spending (correlation 0.86 1998-2016 using HMTs Public Expenditure Statistical Analyses (PESA) data) so that average real net increases in taxes over spending are approximately zero and uncorrelated with consumption growth. We thus anticipate that the risk premium should be weakly negative. Several institutional features described in Section 1.3 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 risk, uncertainty, 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

[21]Prime Central London (PCL) describes the highly urbanised core of London covering Mayfair, Chelsea, and Kensington. Inner London subsumes PCL but is a larger area (roughly 320 km^2) and contains 15 of the 32 LAs in London.

[22]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).

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 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.

4.5 Time-variation

In this section, we shed further light on the relationship between discount rates and risk–free rates by plotting the evolution of r_T over time under the assumption of full capitalisation. We estimate a specification (see equation (10) in the Appendix for more details) in which we retain transaction pairs where the first sales occur in a base period which we define as the first or second years of our panel.[23] Because this regression requires a considerable number of sales in the base period, we use a 1.5km boundary sample, group pairs of years together, and include any pair of sales which has a first sale in either the first or second year in our data (i.e. 1998/99 and 1999/2000). We also drop repeat sales with a second sale in 2016/17 due to the small number of such pairs.

Our approach involves interacting time dummies with T_{it} and plotting the reciprocal of the resulting coefficients on the tax variables in Figure 6. Here, the blue circles denote

[23]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 provided on request.

the point estimates with the size of the circle representing the number of underlying data points from which the coefficient is estimated, the black line represents the time path of r_T (under the assumption of full capitalisation), 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 2 to 4%. The point estimates are statistically indistinguishable from one another, suggesting that r_T is stable over the full span of our sample.

The Figure also plots (dashed grey line) the real long risk–free rate as measured by the average annual real yield for the Government Liability curve for maturities of 1–25 years.[24] 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.* (2017) 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. Possible explanations for this divergence include 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. Given central government interventions to limit Council Tax increases around this time, it seems less likely that changes in risk and future tax expectations around tax changes are behind these results.

4.6 Sensitivity

Sensitivity of our main result in column (6) of Table 3 to changes in specification and sample are investigated in Tables 8 and 9. 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

[24] We construct this curve using data available from the Bank of England.

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 re–specify the public good controls. The coefficient is also robust to interacting our boundary trend controls with a property type indicator in column (3), 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.

Table 9 explores sensitivity to sample selections. The first column increases the 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).

5 Conclusions

Despite offering strong external validity relative to experimental studies and scope to examine discounting over long horizons, housing market settings have only recently been used to study inter–temporal choice (e.g Giglio *et al.*, 2015a; Bracke *et al.*, 2017). In this paper we build on well–established urban public finance theories to estimate household discount rates over perpetual time horizons using a novel source of variation: property taxes.

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 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 complements experimental work focussed on shorter horizons. Our results for London sit comfortably alongside those recently obtained from leasehold transactions in that city (Bracke *et al.*, 2017).

We also provide new evidence on the extent of property tax capitalisation rate for England. 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 advance the existing literature by examining the evolution of discount rates over time and by directly comparing estimates to risk–free rates. We find that discount rates implied by property taxes are closely aligned to the long risk-free rate in the first half of our sample (1998-2008), which is consistent with market rates disciplining the discounting of future tax cash in this period. However, from 2008 we observe that tax–implied rates remain flat at around 4%, and thus become disconnected from the prevailing risk–free rate. These findings are consistent with a role for credit market conditions.

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Tables and Figures

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

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			

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***	-32.52*	-30.17***	-26.30***	-29.46***	-30.24***
	(17.896)	(19.078)	(8.596)	(8.925)	(8.407)	(8.412)
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×2km BFE			✓			
Year pairs×band×1km BFE				✓	✓	✓
Implied r; β=0.75	0.010***	0.023*	0.025***	0.029***	0.025***	0.025***
	(0.002)	(0.014)	(0.007)	(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)	(0.009)	(0.013)	(0.010)	(0.009)
Number of sales pairs	1070255	931131	466128	186843	186843	186843
R^2	0.663	0.731	0.759	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.

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	-31.86**	-35.04***	-21.41**
	(12.334)	(12.756)	(10.794)
Years×postcode×ThresholdFE	✓	✓	✓
Years×LA×ThresholdFE	✓	✓	✓
Threshold rule:	fixed 5k for all	10% of cut–off	5k for first & 1k increments
Number of sales pairs	38110	33718	40037
R^2	0.917	0.919	0.912

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. 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 various rules. The baseline specification in column (1) includes 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 (2) the bandwidth is set at 10% of the relevant threshold; in column (3) the bandwidth is £5,000 for the A–B threshold and increases by £1,000 at each subsequent increment.

Table 5: Taxes and rents
(Dep var: rent in £)

Dependent variable: rent	(1)	(2)	(3)	(4)	(5)	(6)
	———2km buffers———			———1km buffers———		
Council Tax	-0.86**	-1.00**	-0.91*	-0.97	-1.14*	-1.07*
	(0.40)	(0.45)	(0.46)	(0.59)	(0.63)	(0.64)
Tax measured at Parish			✓			✓
Quadratic in LA spend per head		✓	✓		✓	✓
Quadratic in KS2 test scores		✓	✓		✓	✓
Local green space		✓	✓		✓	✓
Home characteristics	✓	✓	✓	✓	✓	✓
Year×taxband×BFE	✓	✓	✓	✓	✓	✓
Observations	43406	43406	43406	24106	24106	24106
R^2	0.752	0.752	0.752	0.742	0.742	0.742

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)* homes that appear only once in the sample; *(ii)* outliers which are defined at the top and bottom 1% of rents in each region and the top and bottom 1% of rents in each tax–band in each region *(iii)* homes that have more than one extension. 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.

Table 6: Taxes and prices using same approach
(Dep var: sale price in £)

Dependent variable: sale price	(1)	(2)	(3)	(4)	(5)	(6)
	———2km buffers———			———1km buffers———		
Council Tax	-30.35**	-29.50*	-28.72**	-27.01*	-25.87*	-25.21*
	(15.28)	(15.63)	(14.58)	(14.76)	(14.68)	(14.00)
Tax measured at Parish			✓			✓
Quadratic in LA spend per head		✓	✓		✓	✓
Quadratic in KS2 test scores		✓	✓		✓	✓
Local green space		✓	✓		✓	✓
Home characteristics	✓	✓	✓	✓	✓	✓
Year×taxband×BFE	✓	✓	✓	✓	✓	✓
Observations	144777	144777	144777	78931	78931	78931
R^2	0.925	0.926	0.926	0.933	0.934	0.934

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 prices in each region and the top and bottom 1% of prices in each tax–band in each region; *(ii)* homes that have more than one extension; *(iii)* 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.

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	-29.77***	-30.65***	-29.10***	-30.79***
	(8.22)	(7.91)	(8.18)	(8.12)
× SD highest seat share in local elections	-0.74			
	(1.32)			
× SD annual % Council Tax growth		0.17		0.94
		(1.85)		(1.96)
× Mean annual % Council Tax growth			-0.73	-1.08
			(1.17)	(1.23)
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.

Table 8: Sensitivity – specification
(Dep var: Δ sale price in £)

Dep var: Δ sale price	(1)	(2)	(3)	(4)	(5)	(6)
Δ Council Tax	-26.25***	-31.54***	-34.66***	-27.62***	-27.59***	-26.11***
	(7.136)	(8.134)	(9.037)	(8.484)	(7.986)	(7.607)
Years×band×BFE	✓			✓	✓	✓
Years×band×BFE×type		✓				
Years×band×BFE×post95			✓			
Change to baseline:	in 2015 prices	fixed effects	fixed effects	linear LPGs	continuous test scores	more LA controls
Observations	186843	165237	176728	186843	186843	186843
R^2	0.870	0.905	0.896	0.892	0.892	0.892

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

Dep var: Δ sale price	(1)	(2)	(3)	(4)	(5)	(6)
Δ Council Tax	-26.43***	-27.29***	-34.73***	-28.78***	-30.67***	-28.81***
	(7.206)	(8.475)	(9.078)	(7.811)	(9.349)	(8.189)
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	122771
R^2	0.774	0.768	0.767	0.745	0.766	0.775

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.

Figure 1: Stock of housing by regions and tax bands

Figure 2: Tax in 2016/17

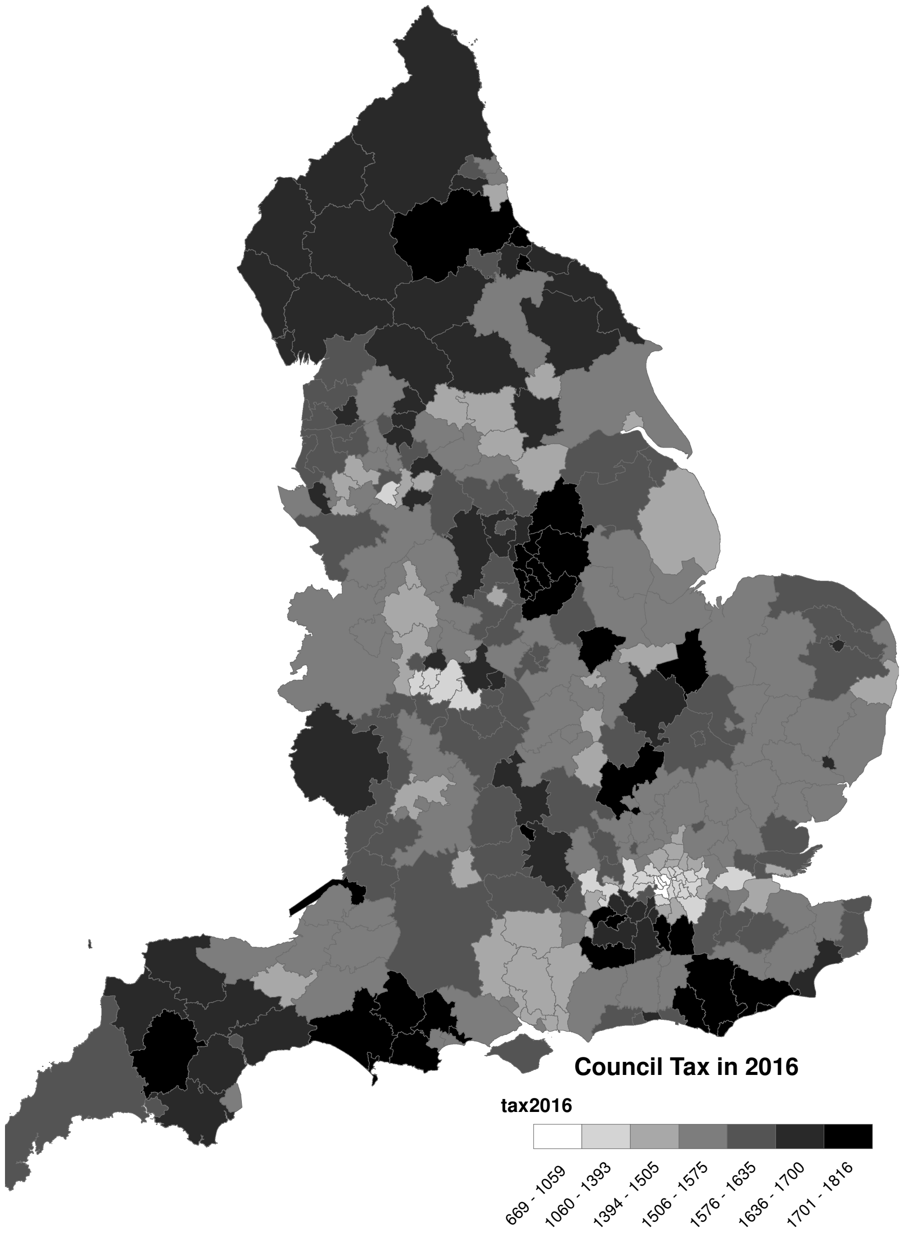


Figure 3: Tax change 1998/99–2016/17

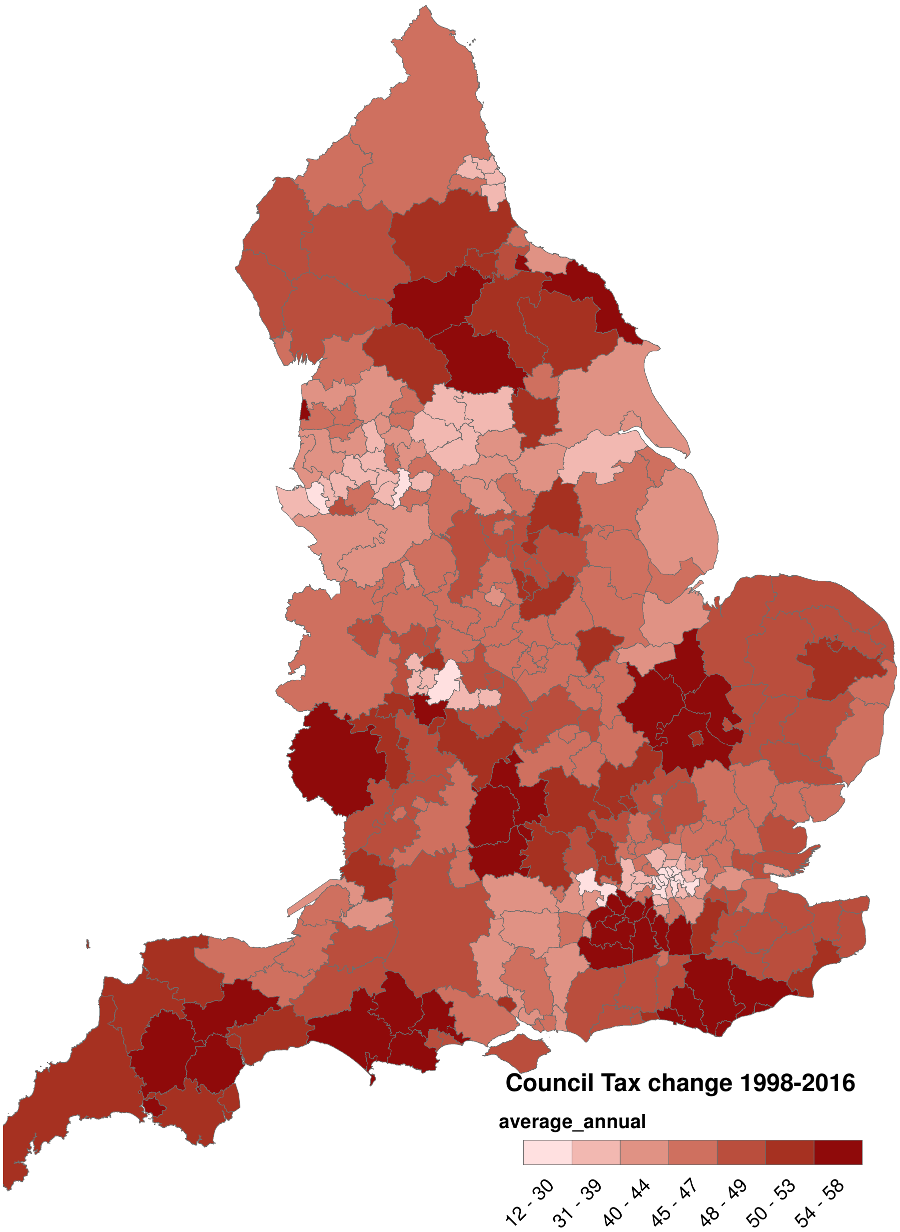
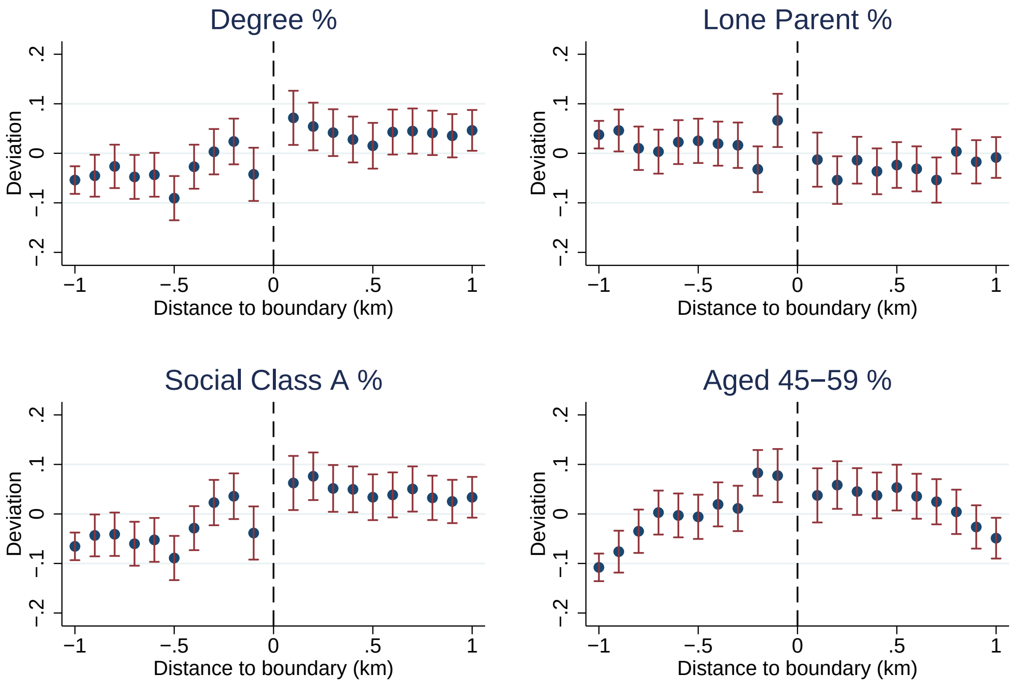
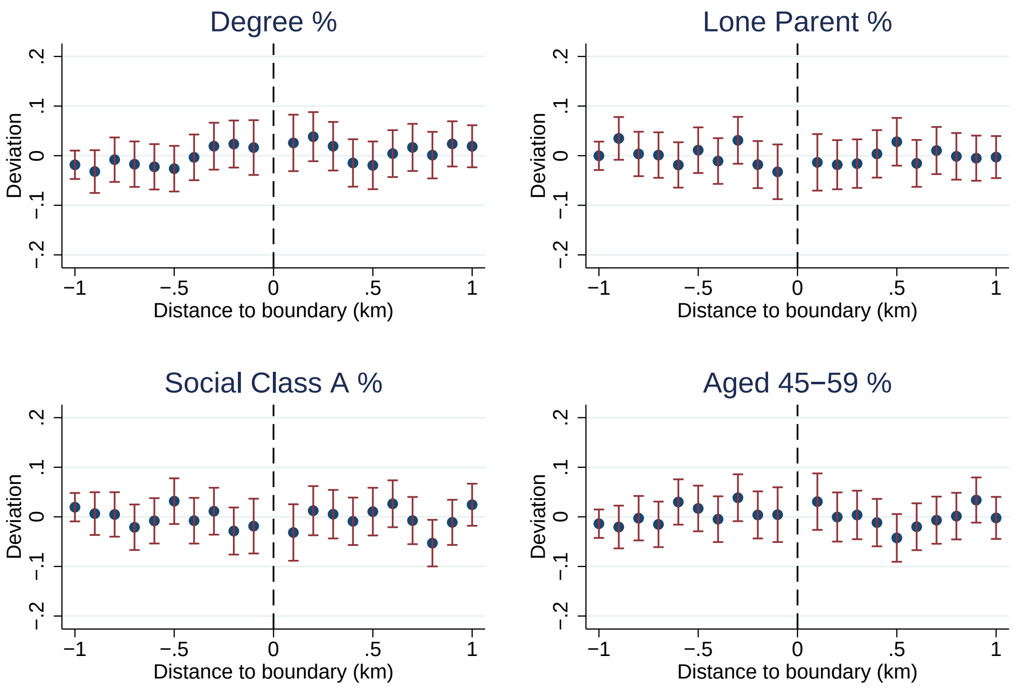


Figure 4: 2011 Census



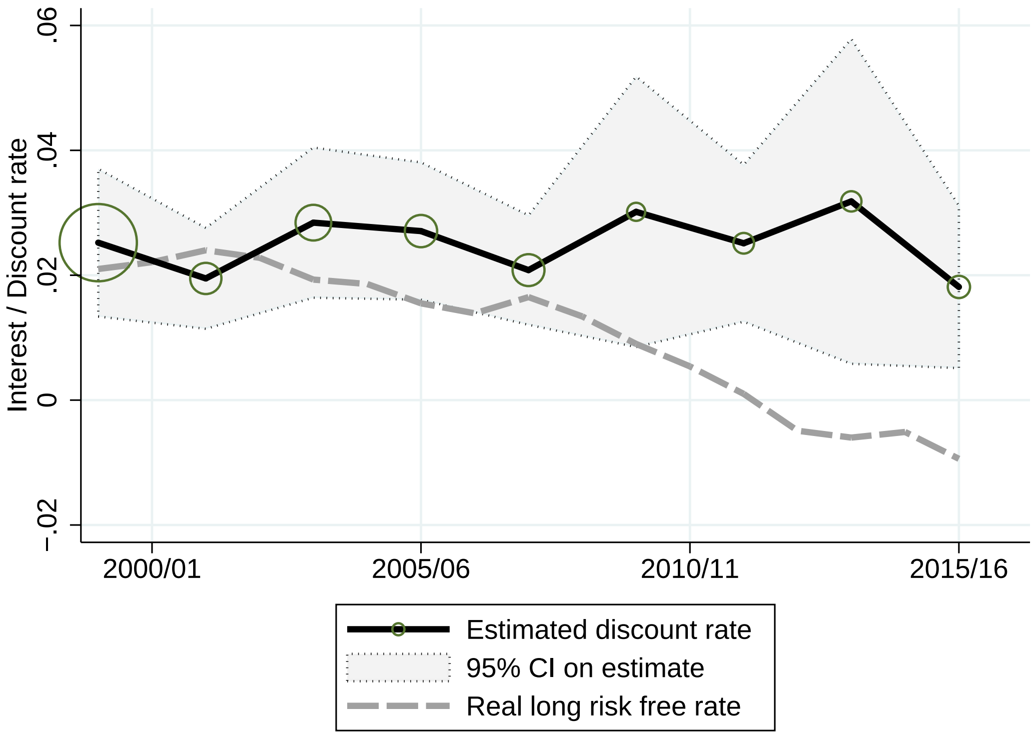
Low tax side is negative

Figure 5: Changes between 2001 and 2011 Censuses



Low tax side is negative

Figure 6: Implied changes in r_T



Appendices

A 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 Figures A1 and A2.

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 centers, 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, 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.

B 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. This 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.[25] 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 LPA tax data, then add back in the actual tax–band specific parish precept for the given parish.

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.

[25]More specifically, we run identical regression using taxes measured at the two different spatial levels in Section **??**. We obtain very similar results.

C 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 - and we find that it acts as an outlier and has a large effect on our findings. 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 – see http://www.birminghampost.co.uk/news/regional-affairs/birminghams-council-tax-income-lowest-9746303.

D Time variation in β/r_T

One critical assumption in the above estimation is that the discount rate and capitalisation rate are both constant across time, i.e. $r_{Tt} = r_T$ and $\beta_t = \beta \ \ \forall t$. We next consider the situation where we allow the discount rate and the capitalisation coefficient to evolve over time, which is consistent with findings in Bracke *et al.* (2017). By differencing between time t and τ, and removing the subscripts on discount rates and ignoring P for expositional simplicity:

$$V_{it} - V_{i\tau} = \Delta V_{it\tau} = -\frac{\beta_t}{r_t} T_{it} + \frac{\beta_\tau}{r_\tau} T_{i\tau} + \left(\frac{\rho_t}{r_t} - \frac{\rho_\tau}{r_\tau} \right) H_i \tag{9}$$

The above equation would be cumbersome to estimate for an unbalanced panel of housing transactions because r_τ, r_t, β_t and β_τ should be internally consistent. For example, in one transaction pair $t - \tau$, β_t should be the equal to β_τ in another transaction pair $\tau - \tilde{\tau}$. We therefore simplify the above equation by only taking transaction pairs into account that occur in the first or second year, denoted by $\underline{t}$. We thus estimate:

$$\Delta V_{it\underline{t}} = -\frac{\beta_t}{r_t} T_{it} + \frac{\beta_{\underline{t}}}{r_{\underline{t}}} T_{i\underline{t}} + \left(\frac{\rho_t}{r_t} - \frac{\rho_{\underline{t}}}{r_{\underline{t}}} \right) H_i + \frac{1}{r_t} f(P_{it}) - \frac{1}{r_{\underline{t}}} g(P_{i\underline{t}}) + \phi_{\kappa b t \underline{t}} + \omega_{it\underline{t}}, \tag{10}$$

where the function $g(\cdot)$ is equivalent to $f(\cdot)$ but acknowledges the possibility that capitalisation parameter on public goods may differ between the sale years.

E Additional Tables and Figures

Table A1: Housing supply elasticities
(Dep var: Δ sale price in £)

Dep var: Δ sale price	(1)	(2)	(3)	(4)	(5)	(6)
Elastic=1 × Δ Tax	-22.93***	-29.06***	-28.18***	-33.30***	-30.79***	-28.73***
	(8.81)	(8.13)	(8.53)	(9.31)	(9.79)	(9.92)
Elastic=0 × Δ Tax	-30.31***	-35.72***	-32.27***	-30.64***	-31.07***	-31.01***
	(8.40)	(10.66)	(8.11)	(8.67)	(10.41)	(8.03)
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***	0.033***	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	161405	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).

Figure A1: Pre-2009 LPAs

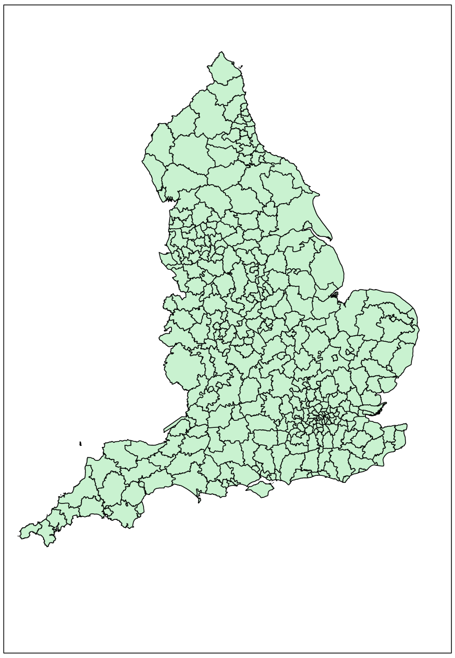


Figure A2: Post-2009 LPAs