How do Households Value the Future? Evidence from Property Taxes

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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 net of growth nominal 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.

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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

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to policy interventions.[1] Revealed discount rates also provide a means to test whether individuals systematically undervalue the future (e.g. Busse, Knittel and Zettelmeyer, 2013; Allcott and Wozny, 2014; Cohen, Glachant and Söderberg, 2017; De Groote and Verboven, 2019), and hence whether they are optimising.

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 estimate the rates households use to discount over very long time horizons. Motivated by findings of optimisation errors in home purchases (e.g. Genesove and Mayer, 2001; Keys, Pope and Pope, 2016) and in decisions relating to taxes (e.g. Chetty, Looney and Kroft, 2009; Bradley, 2017) we next evaluate if these implied 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 (adjusted for any differences in expected growth) 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.

We take this intuition to extensive house sales and tax data spanning around 20 years in England, where the local property tax – Council Tax – is an annually determined, jurisdictionally set, per-property tax that levies a flat sum for all homes within coarse and historically determined value bands. Our baseline approach exploits *inter-jurisdictional* variation in Council Tax by focusing on repeat sales of perpetually owned 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 previous studies (further described in Hilber, 2015). This baseline approach conditions out unobservable price determinants that vary smoothly over space. To counter any residual concerns that differences

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

in expectations about future tax growth or public good provision across boundaries are driving our estimates, we show that very similar results can be obtained when using *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 and the exact same path of future percentage tax increases. Identification is achieved by retaining homes close to tax band thresholds and including threshold-year-location fixed effects. A causal interpretation of our results is bolstered because findings across these specifications are largely insensitive to the inclusion of control variables, and 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). Hence, the discount rate can only be truly identified if we know the capitalisation rate.[2] Given this, we 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 further test 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; this result is not so surprising as very stringent regulatory restrictions on land and scarcity of developable land render housing supply relatively inelastic across most of urban England (e.g Cheshire and Hilber, 2008; Hilber and Vermeulen, 2016).

Our preferred specification implies net of growth nominal discount rates of 3.7%.[3] We next turn to the question of whether households make systematic optimisation errors over future property tax liabilities. If nominal tax growth expectations are fixed at the long-term tax growth rate of 3.8% per year, growth-adjusted discount rates are maximally 7.5%. Comparing this to prevailing saving and borrowing rates gives us little reason to believe that households materially

[2]For the most part, scholars have focussed on estimating capitalisation rates by making assumptions about how homebuyers discount the future. The literature presents a wide range of capitalisation parameter estimates; however, estimates in more convincing studies are generally close to one.

[3]The 95% confidence interval for this is 2.3% to 9.8%, which narrows to 2.0% to 6.4% if we use a larger boundary sample to obtain more precision. Throughout the paper, we refer to nominal net of growth discount rates ($r - g$) following Giglio, Maggiori and Stroebel (2015) unless stated otherwise.

undervalue future tax liabilities. Furthermore, implied discount rates track benchmark interest rates over time relatively closely if we assume tax-limiting policies introduced in the latter part of our sample window dampened long-term growth expectations.

We then explore further heterogeneity across characteristics that our priors suggest will modify how households value the future. Although imprecision prevents firm conclusions, this points to lower discount rates for more sophisticated homebuyers (proxied by education), and those with a lower opportunity cost of capital (proxied by income, borrowing, and the assessed value of the home). Of course, a possible explanation for these findings is that the capitalisation parameter also varies across the same homebuyer characteristics, but we do not find support for this in the data.

Related literature. Our article complements efforts to estimate personal discount rates in the field or in the lab. The experimental literature, which typically examines choices between relatively small stakes, and often hypothetical, money rewards within short time horizons, mostly suggests that households place little weight on the future (see e.g. Frederick, Loewenstein and O'Donoghue, 2002). The evidence for longer horizons largely derives from observational data, is more sparse, and encompasses a very wide range of estimates. 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, Repetto and Tobacman, 2007).

We establish a valuable new reference point for this literature. To the best of our knowledge, ours is the first paper that uses nationwide property tax and housing transaction data to generate robust estimates of discounting parameters. One of the merits of this setting is that the rates we obtain refer to extremely long time horizons, for which discount rate estimates are rare. Another is that the setting suggests a high degree of external validity both because of widespread market participation, and because households devote a sizeable share of spending to their homes.[4]

[4]Around 70% of households in England owned their homes in 2008. Piazzesi and Schneider (2016)

Besides this, we contribute to literature strands that test for deviations from standard assumptions that underpin traditional economics models of behaviour (see DellaVigna, 2009). Our article is methodologically aligned to papers that use undervaluation of future financial flows as a test for misoptimisation. To date, this literature has focussed exclusively on energy efficient features of durable goods, and has yielded conflicting findings and interpretations.[5] While this diversity and contextual differences preclude direct parallels with our findings, our contribution highlights that this class of misoptimisation test is applicable beyond energy efficiency. Our work is particularly relevant to research that tests for departures from fully rational behaviour in housing and taxation domains. Previous studies find that home sellers display loss aversion (e.g. Genesove and Mayer, 2001; Engelhardt, 2003) and buyers projection bias (Busse et al., 2012); and many unconstrained households fail to refinance mortgages optimally (Keys, Pope and Pope, 2016). Furthermore, individuals under-react to sales taxes that are not salient (Chetty, Looney and Kroft, 2009), and – in a useful reference point for our study – appear to be inattentive to shrouded features of property taxes in Michigan (Bradley, 2017). Beyond bringing a new misoptimisation test, our contribution to this literature is twofold. First, our conclusion that property tax valuations look near-rational on average provides some counterweight to evidence of misoptimisation, and likely follows from the simplicity and transparency of the tax we study, the availability of discount rates cues in the form of well-publicised mortgage interest rates, and a public discourse that regularly links house prices with central bank interest rate decisions. Second, we provide evidence that less sophisticated and poorer home buyers apply higher discount rates to taxes, consistent with greater propensity to make optimisation errors. This heterogeneity in the optimality of housing consumption choices for different groups in society suggests that Council Taxes imply potentially unintended transfers (Chetty, Looney and Kroft, 2009; Bradley, 2017).

Finally, our paper ties into a recent literature that reveals discount rates in housing markets. Understanding discounting in housing markets is a worthwhile

show that housing services account for slightly under a fifth of total consumption (including durables) in the US.

[5]The seminal study by Hausman (1979) finds significant undervaluation in air conditioner purchases. More recently, De Groote and Verboven (2019) reach similar conclusions for solar PV adoption decisions. Estimates on the pricing of fuel efficiency in automobile purchases have been variously interpreted as undervaluation, moderate undervaluation, or else no undervaluation (Busse, Knittel and Zettelmeyer, 2013; Allcott and Wozny, 2014; Sallee, West and Fan, 2016; Grigolon, Reynaert and Verboven, 2018).

endeavour because it can shed light on the relationship between interest rates and house prices, and because it is useful to researchers attempting to establish annualised amenity values (see e.g. Chay and Greenstone, 2005).[6] Related research using fixed-term leasehold tenure has found real-terms discount rates for housing services that are low at very distant horizons and declining over the time horizon (e.g. Giglio, Maggiori and Stroebel, 2015; Bracke, Pinchbeck and Wyatt, 2018). Our study departs from this work because we explicitly focus on misoptimisation, and because we use perpetual financial flows associated with property taxes on perpetually owned (freehold) homes 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 critically, using taxes allows us to be more precise about the extent to which risk and expectations about future growth drive discount rate estimates.[7]

The remainder of the paper is structured as follows. In Section I we motivate our empirical work and discuss the institutional setting of our study. Section II describes the econometric framework, and is followed by a discussion of the data and the descriptives in Section III. Section IV presents our main results and in Section V we focus on recovering and interpreting discount rates. Section VI reports some ancillary regressions and in Section VII we conclude.

I. Background

A. *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

[6] On the first point Glaeser, Gottlieb and Gyourko (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 sizeable 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."*

[7] 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 (e.g. Shiller, 2015). Although leasehold prices imply a declining term structure of discount rates for housing services, we are not aware of any evidence on the term structure of tax rates. In this paper we confine ourselves to estimating average tax discount rates and assessing whether this implies misoptimisation.

of housing services minus the present value of the future stream of property tax payments:[8]

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

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 π. 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 growth-adjusted 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. As with earlier applied work (e.g. Giglio, Maggiori and Stroebel, 2015), 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 $r_T = r^f + r_T^p - E(g_T)$. Note that in this formulation with constant common expected growth in taxes, expectations are wholly captured in the discount rate r_T and do not enter the capitalisation rate β. Separating β and r_T is more complicated if the trajectory of future taxes is expected to be idiosyncratic or uneven, e.g. because of a tax revaluation (e.g. Yinger, Bloom and Boersch-Supan, 1988).

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 val-

[8]This equation can be equivalently derived from asset pricing or utility maximisation approaches (see e.g Yinger, 1982; Yinger, Bloom and Boersch-Supan, 1988; Ross and Yinger, 1999)

ues, the vast majority of studies, reviewed in Yinger, Bloom and Boersch-Supan (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, Bloom and Boersch-Supan (1988) show that part of this 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 (e.g. because of revaluations) (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, Kurban and Persky (2013) find close to full (100%) capitalisation of property taxes into home values, whereas estimates reported in Lutz (2015) fall in the range of 70% to 97% for homes in urban areas.[10]

The advantage of using rents (R_i) 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, or the path of future taxes:

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

Given our earlier assumption that expectations do not enter the capitalisation rate, the parameter $\tilde{\beta}$ here can be related to the parameter β 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

[9]For example, in their review Yinger, Bloom and Boersch-Supan (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.

[10]This is broadly supported by evidence relating to other real estate taxes. Dachis, Duranton and Turner (2012) show that a land transfer tax is approximately fully capitalised into land prices in Toronto whereas Besley, Meads and Surico (2014) find that buyers capture around sixty percent of a transfer tax holiday in the UK.

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 $\tilde{\beta} = \beta(r_H/r_T) = \beta(r^f + r^p_H - E(g_H))/(r^f + r^p_T - E(g_T))$.[11] In Section V we present evidence supporting a close correspondence between r_H and r_T for a subset of the sample, which suggests that $\tilde{\beta} \approx \beta$. Although we do not observe the individual components of r_H and r_T, this likely reflects that risk premia and expected growth are both higher for housing than for property taxes.

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.

B. *Institutional setting*

Organisation of local government. The chief organisational units in our setting are Local Authority (LA) districts. LA district 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. All LAs set taxes, but there is some heterogeneity in the scope of services because some LAs operate within a two-tier structure in which a larger upper tier (a County Council) delivers some specific services across several districts. In the English system fire and policing services are operated by distinct authorities that work across several (single or two-tier) LAs.

Finally, in some but not all places parish and town councils may provide a limited set of local facilities like community centres, parks, and play areas, and can also have a say in local land-use and planning decisions.

[11] 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 of 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.

Local government services. Local government accounts for roughly a quarter of public spending in England. The main components of revenue spending are education (40%) social care (20%), policing and fire services (15%), culture, planning, and environment (10%), and transport (5%). As our principal identification strategy relies on LA boundaries, we next discuss how services are delivered, paying particular emphasis to whether local public goods are excludable at boundaries. We elaborate on this in Appendix A.A1.

A choice system operates in both primary and secondary state education, whereby parents have a right to express preferences for particular schools. Secondary schools are rarely over-subscribed, and where they are places are allocated based on straight line distance of a family's home to the school without reference to the LA boundary (Burgess, Greaves and Vignoles, 2019). As detailed in Appendix A.A2, the situation for primary school is less clear cut. According to Gibbons, Machin and Silva (2013), between 2003-2006 LAs were not under a legal obligation to accommodate pupils from outside the LA. These authors show discontinuities in primary school quality at LA boundaries for this period. We therefore proceed as if LA boundaries are not a consequential determinant of secondary school access, but may be influential in primary school access.

Local government provides a number of local public goods and services besides education. Social care services for children, young people, and adults represents the next largest budget share. However, only a small fraction of the population (around 2%) use LA social care each year, quality is likely very hard to observe, and throughout our sample period anyone with assets above a low income threshold is ineligible for support so this is unlikely to be relevant to home buyers. For fire and policing, administrative areas are larger than LAs and even where boundaries do coincide, quality is unlikely to vary sharply at boundaries because of legal duties to collaborate. Of the remaining 25% of local spending, services are either non-excludable at the boundary (such as transport services, parks, and museums), or are commonly subject to reciprocal agreements between neighbouring LAs (such as library access).

Council Tax. Local Authority income comes from three sources: grants from central government; locally raised taxes; and fees and charges levied to cover service costs. The main local tax is the Council Tax: a tax levied on domestic homes which represents around a quarter of LA funding.

We provide a detailed discussion of the Council Tax and its legislative basis in Appendix A.A1. Council Tax is payable on all domestic homes. In contrast to many property taxes, liability rests with occupiers rather than owners of homes. The legislation sets out a number of qualifications to liability, including a 25% discount for single occupiers, and exemptions for some residences.[12] The tax is not deductible from income tax, and cannot be paid through a mortgage lender. Collection rates are very high: for example, in 2014/15 97% of taxes were collected, reflecting considerable LA information gathering and enforcement powers.

Council Tax is simple and transparent to both renters and buyers. The tax varies according to two main factors: annual tax setting decisions and a well publicised nationwide tax schedule for homes in different 'tax bands' (Table 1). Tax levies, or precepts, can arise from authorities within the layers of local government described above. 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 authority. Importantly, the vast bulk of Council Tax represents precepts from LAs; levies from parishes made up only 0.6% of the total LA budget requirement in 2011/12.

Tax bands are determined by an assessment of home value in 1991 (see Table 1 for the valuation thresholds). All existing homes were assigned to tax bands in a large initial valuation exercise completing in 1992, while those built subsequently are assessed following construction. Homes can be moved to a new tax band for a number of reasons, but in practice this is very rare as only around 0.2% of homes were rebanded each year throughout our whole sample period. We therefore treat the stock of homes in each band as essentially being fixed.[13] As with the tax

[12]These include long term unoccupied and unfurnished homes, homes undergoing structural alternations, unoccupied buildings owned by charities, homes of religious officials and people living in care or hospital, homes fully occupied by students, homes of deceased people, and homes repossessed by lenders.

[13]This statistic is generated from official data and news reports. The source of our official data is the Valuation Office Agency. Data for 2010/11, 2011/12, 2013/14 is held in Table 3.2 of "Council Tax Valuation Lists: Changes" in the UK government's webarchive. Data for 2009/10 and 2012/13 was released under Freedom of Information and is available on the VOA website, while later years are available from "Valuation Office Agency: Council Tax statistics". National rebanding statistics for 1997/8-2008/9 were published by Money Saving Expert on 19th April 2010. Homes can be 'rebanded' 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. That there are so few changes in bands reflects that there has been no systematic revaluation of homes in England since the initial valuations in the early 1990s. Successive governments have ruled out wholescale revaluations, most recently in April 2016. We discuss more details regarding rebanding in Appendix A.A1.

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

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.

Spatial variation in Council Taxes. Because homes rarely move tax bands and parish taxes are minimal, variation in Council Tax largely arises through LA tax setting decisions. Figure 1 maps the tax for homes in a 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 A2 in Appendix A.A4 shows the average annual change in taxes between 1998/99 and 2016/17. This suggests 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 close to zero (the correlation is only -0.048). At the national level, taxes move in step with Local Authority spending (the correlation is 0.90 for the same period).

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 have subsequently grown more slowly. This in part reflects two policy interventions introduced by the Conservative-Liberal Democrat coalition in 2010, and first announced by the Conservatives in policy papers in 2008 and 2009. The first – arguably the most important for our work because it governs tax setting

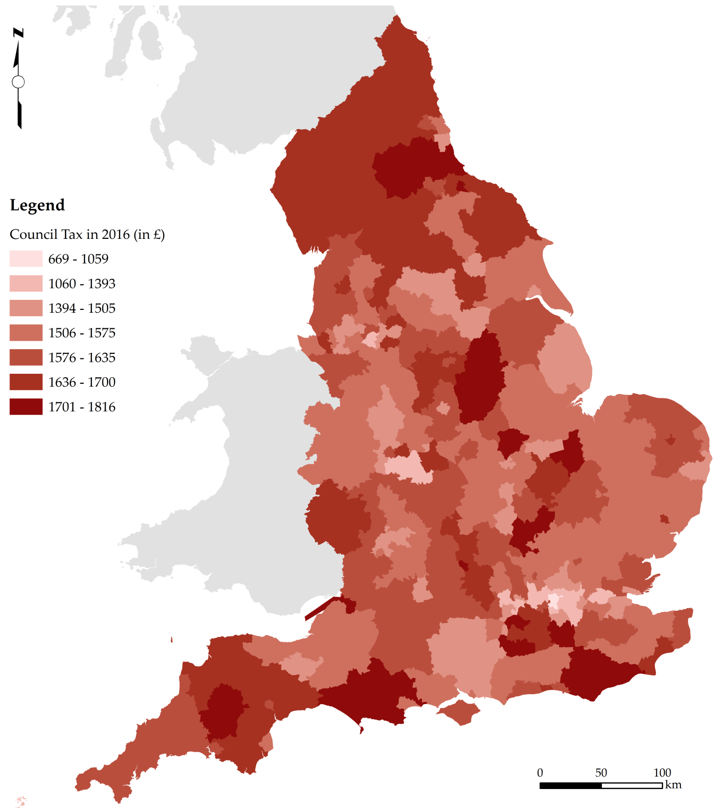

Figure 1 – Tax in 2016/17

in the long-term – was a policy that subjects large tax rises to local approval. Consequently, since 2012/13 LAs wishing to raise taxes above a threshold set annually by Parliament, and usually in practice between 2% and 4%, have needed to put this to a local referendum (a path which no LA has yet pursued). The second was a short-term tax freeze policy, under which LAs that froze nominal taxes received a capped subsidy from central government. The first freeze was announced in 2010 and offered LAs a 2.5% subsidy for freezing nominal taxes in 2011/12. Although initially announced as a one year policy with no commitment beyond 2011/12, similar but less generous freeze subsidies were then subsequently announced each year until 2014/15. The freezing policy was dropped in 2015/16 and taxes have again begun to rise more rapidly, although the referendum policy remains in place as of today.

Land use regulation. Although our main focus is Council Tax, the degree of the housing supply elasticity plays a material 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, Hilber and Vermeulen (2016) show that house prices in an average local authority in England in 2008 would be 21.5-38.1% lower if the planning system were completely relaxed, while 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, Birmingham and Manchester.

II. Empirical approach

A. *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:

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

where H_i are time-invariant housing attributes, the vector π 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_1 and t_0 (where $t_0 < t_1$) and denoting $\tilde{t} = t_1 - t_0$, $\Delta V_{i\tilde{t}} = V_{it_1} - V_{it_0}$ and

$\Delta T_{i\tilde{t}} = T_{it_1} - T_{it_0}$. We then have:

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

where $\phi_{\kappa\tilde{t}}$ 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 π 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).

Another 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, for example because there is a lower need for spending on social care or crime prevention. Equally, strong price appreciation could signal a higher demand for more or better pubic goods. Hence, to reduce this potential bias, we will focus on repeated sales that occur close (1, 1.5km, or 2km) to an LA boundary and include boundary fixed effects $\phi_{\kappa b\tilde{t}}$ for each boundary b and each tax band-year κ-$\tilde{t}$ combination. The coefficient of interest, β/r_T, is identified by the differential growth in tax liability across jurisdictional boundaries – i.e. the difference in $\Delta T_{i\tilde{t}}$ across boundaries b – within tax bands and sales year combinations, κ-$\tilde{t}$.

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.[14] We test this more directly by gathering data on total local spending per LA and information on test scores, denoted by P_{it}. A remaining concern is newly constructed homes. First, newly constructed homes command an initial price premium but are then likely to depreciate at a different (faster) rate when compared to older homes. Second, these homes were necessarily assessed for tax purposes outside of the initial rebanding exercise conducted in the early 1990s, and hence may systematically fall in different tax bands. Third, new homes could imply a greater need to raise Council Tax, e.g. because of the need for LAs to provide additional infrastructure and services. In acknowledgement of these factors, we therefore specify separate sets of fixed effects for homes build after or before 1995 and show robustness to the exclusion of homes constructed after 1995.

A familiar problem in spatially differencing the data is that sorting of households may occur (Bayer, Ferreira and McMillan, 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:

$$\Delta V_{i\tilde{t}} = -\frac{\beta}{r_T}\Delta T_{i\tilde{t}} + \frac{\pi_{t_1} - \pi_{t_0}}{r_H} H_i + \frac{1}{r_P}\bigg(f(P_{it_1}) - f(P_{it_0})\bigg) + \phi_{\kappa b\tilde{t}n} + \Delta\omega_{i\tilde{t}}, \tag{5}$$

where n is an indicator for built since 1995, and $f(\cdot)$ is estimated with second-order polynomials.

B. *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

[14] Note that this does not mean we assume that LAs with higher taxes cannot provide more or better services. The argument is that benefits vary continuously over space so that there is no difference in benefits *at the boundary.* The 1km, 1.5km, and 2km boundary distances are selected as they yield sample sizes that are sufficient to obtain relatively precise estimates.

decisions. We can also use *intra-jurisdictional* variation to estimate β/r_T by comparing tax and price changes for neighbouring homes in the same LA but in different tax bands. This approach has two main incremental advantages. First, it eliminates any residual concerns that differences in LA provided local public goods could confound estimates. Second, because all homes in an LA are subject to the same tax setting decisions, estimates are generated from buyers that plausibly share the same expectations about future tax growth. On the downside, the *intra-jurisdictional* approach means that we are unable to use the year pair×tax band fixed effects employed in our baseline approach above. This is a considerable drawback 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.

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.[15] We then estimate:

$$\Delta V_{i\tilde{t}} = -\frac{\beta}{r_T}\Delta T_{i\tilde{t}} + \frac{\pi_{t_1} - \pi_{t_0}}{r_H} H_i + \phi_{\gamma d\tilde{t}n} + \Delta\omega_{i\tilde{t}}, \tag{6}$$

where $\phi_{\gamma d\tilde{t}n}$ is a fixed effect specific to years of first and second sale, postcode d, threshold bands (e.g. homes with 1991-equivalent prices close to the threshold between bands A and B of £40,000), and built before or since 1995. Note that the term $P_{i\tilde{t}}$ 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.

[15] 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.

C. 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 analyses. We therefore revert to a dataset for which we 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 (2013-2017). 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:

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

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:

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

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.

III. Data

A. 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.A5. The Land Registry Price Paid dataset captures the universe of home sales in England from 1995 (HM Land Registry, 2017). The data records the transaction price, sale registration date (which proxies for the actual date of sale), the full address, the type of house (flat, detached house, semi-detached house, terraced (or row) house), a new build indicator, and tenure (leasehold or freehold). There is no publicly available data for home rentals for England, so we rely on data obtained from Homelet, the UK's largest tenant referencing and specialist lettings insurance company. Our dataset covers 2013-2017 and includes the full address of the property, the date of the rental agreement, the monthly rent, and the number of tenants listed on the agreement (Homelet, 2018). 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) (Department for Communities & Local Government, 2017).

Home-specific Council Tax bands are published by the UK government (Valuation Office Agency, 2017). The data we use were obtained from the website *mycounciltax.org.uk* using web scraping techniques in early to mid 2017, and record the contemporaneous tax band as well as the home address which allows us to merge with the sales and rentals files. Mindful of mis-measurement, we adopt a conservative matching strategy by retaining matches only when the address and postcode fields both coincide.[16] Given the tax band, we then compute the annual Council Tax payable at the time of each home sale or rental using tables issued by the UK government (Ministry of Housing, Communities & Local Government, 2017).[17]

We geocode and append geographical variables using mapping files and postcode directories (Office for National Statistics, 2016), and identify transactions that lie within fixed distances of Local Authority boundaries using Geographical Information System (GIS) and boundary shapefiles (Ordnance Survey, 2017; Office for National Statistics, 2011).[18] LA expenditure on services per head of population is added from records held by the Chartered Institute for Public Finance & Accountancy (Chartered Institute for Public Finance & Accountancy, 2017). We generate school quality measures by averaging published Maths, English, and Science test scores for primary school pupils aged 8-11 (Key Stage 2) (Department for Education, 2017). We create two annually varying postcode level 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

[16]That we are unable to observe previous tax assessments is unlikely to be a major threat as rebanding is so rare (0.20-0.25% each year), but in any case we provide several robustness checks in Appendix B.B3.

[17]Specifically, we compute the annual tax payable using the Local Authority-wide average Band D Council Tax for each financial year in the published tables, 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.B3 for details. The correlation between taxes measured at the parish and LA levels in our data is 0.997.

[18]When homes are close to multiple LA boundaries, we assign them to boundary region corresponding to the closest boundary. 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.

measure is constructed using tests scores only for schools in the associated Local Authority, 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 administrative area. Postcode level time invariant measures of access to green space at two distance buffers (0-500m and 500-1,000m) are added using data for parks and gardens from the National Heritage List for England (Historic England, 2013).

B. Sample restrictions

We make a number of sample restrictions to remove outliers, minimise unobserved home changes, and mitigate measurement error. Further details are listed in the Appendix A.A6. We remove outliers in three ways. First, we exclude the top and bottom 1% of prices (or rents) in each region and the top and bottom 1% of prices (or rents) in each tax-band in each region. This ensures that exclusions are not highly concentrated in particular high- or low-price regions or in higher or lower price segments of the market. Second, we 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. Third, we remove homes in the top tax band (Band H). These make up around 0.6% of the stock of homes and in many cases are exceptional properties with unique features, which is reflected in mean price of 2.2 million with standard deviation of 2 million. In any case, our rental data contains no homes in this band and including them in our repeat sales boundary regressions with preference controls sometimes results in the variance matrix becoming highly singular due to very small numbers of homes. We therefore elect to drop them throughout. Besides these outliers, we also remove homes for which characteristics change during our sample timespan.[19] 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

[19]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 Poor's 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, Rosenthal and Sirmans, 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.

likely indicates redevelopment. We make one additional restriction for rentals and sales. For sales, we drop leasehold homes as price variation by lease length implies discount rates (Giglio, Maggiori and Stroebel, 2015; Bracke, Pinchbeck and Wyatt, 2018), and we do not observe lease length in our data. For rentals, we remove homes where there is a single tenant listed on the rental agreement, as in some cases this will mean a reduction in the Council Tax liability. We show sensitivity to many of these sample selections in Table B6 in Appendix B.B3.

C. 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.[20] 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 examines sales that lie within 1km 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 slightly higher LA spending per head.

D. 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 changes in property taxes between census years 2001 and 2011 in Figure 2.[21] To obtain the figure, we first

[20]The 2.3 million sales pairs represent 1.6 million unique homes. Our sample is composed of homes held for a shorter duration than the average because the English Housing Survey for 2013/14 suggests the median length of ownership tenure in the UK is 13 years (8 years for mortgaged homes). We focus on these short duration homes because it reduces the chance that homes are rebanded between sales and because it ensures that truncation of the sample on duration is consistent over a large part of our sample. We show in Appendix B.B3 that estimates are similar for short and long duration homes.

[21]Output Areas are administrative geographies that were created for the Census, and represent the smallest geographical level at which census estimates are provided. They were designed to have similar populations and be as socially homogeneous as possible based on tenure of household and dwelling type. They typically contain around 125 households and the minimum OA size is 40 households.

Table 2 – Descriptive statistics: repeat sales

Restrictions:	**Without**				**With**			
	mean	sd	min	max	mean	sd	min	max
Panel A: full sample								
Price	199684	182787	195	17000000	174914	96297	14750	1585000
Tax	1184.13	355.83	331.89	3450.88	1154.72	323.37	331.89	2970.57
KS2 score %	0.82	0.08	0.15	1.00	0.81	0.08	0.15	1.00
LA spend/head	673.53	639.04	60.32	2854.64	653.24	630.50	60.32	2854.64
Greenspace 0-500m %	0.07	0.08	0.00	1.00	0.06	0.08	0.00	0.96
Rooms	4.73	1.43	0.00	85.00	4.48	1.32	1.00	77.00
Built after 1995 %	0.19	0.39	0.00	1.00	0.26	0.44	0.00	1.00
Extensions	0.55	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	15.74	7.78	4.00	32.00
Sales pairs:	2,287,023				1,195,690			
Panel B: 1km boundary sample								
Price	235862	277035	1500	17000000	178383	109508	14750	1585000
Tax	1227.42	375.77	331.89	3450.88	1117.81	292.68	331.89	2804.42
KS2 score %	0.82	0.08	0.28	1.00	0.81	0.08	0.28	1.00
LA spend/head	782.10	676.77	60.32	2854.64	768.11	662.98	64.09	2854.64
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	4.36	1.18	1.00	45.00
Built after 1995 %	0.17	0.38	0.00	1.00	0.19	0.39	0.00	1.00
Extensions	0.54	0.72	0.00	4.00	0.02	0.10	0.00	0.50
Quarters b/w sales	16.05	7.80	4.00	32.00	15.58	7.53	4.00	32.00
Sales pairs:	649,295				262,560			

select OAs in boundary samples, then assign them low or high tax change side of boundary using changes in taxes between 2001 and 2011.[22] 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 2 reveals no clear patterns with regard to *changes* in taxes between 2001 and 2011. This is important, as one of our two main identification strategies relies on temporal variation in taxes and house prices around LA boundaries.

[22]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.

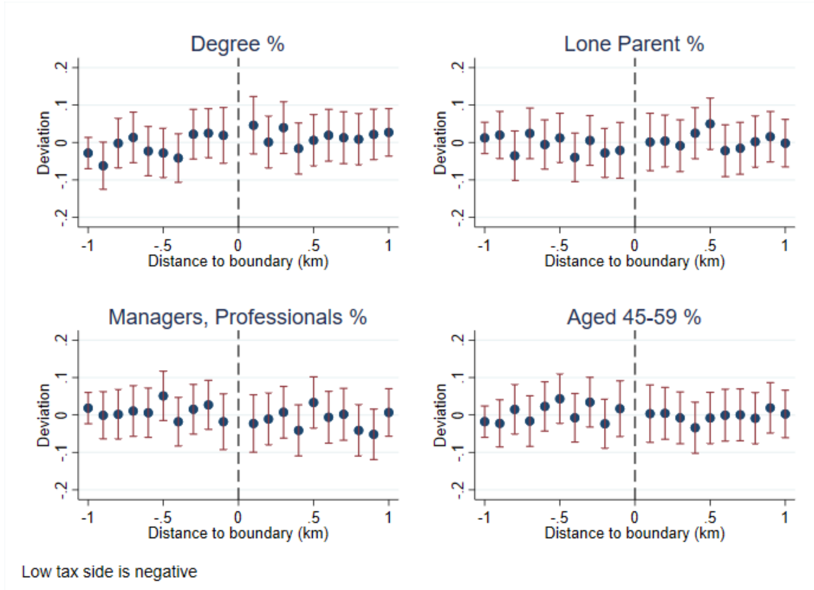

Figure 2 – Changes between 2001 and 2011 Censuses

We also present a similar analysis for cross-sectional taxes in 2011 in Figure A3 in Appendix A.A7. Here we do find some modest 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. That we find no evidence of sorting on changes in taxes but some in the cross-section likely reflects that sorting is a slow process – e.g. Heblich, Trew and Zylberberg (2016) find that neighbourhoods in London that were deprived in 1881 remain so today, and Ambrus, Field and Gonzalez (2020) show that a 19$^{\text{th}}$ Century cholera epidemic is evident in house prices 160 years later. While we feel it is unlikely in light of the tipping point literature, it is possible that contemporaneous tax changes could shift prices in expectation of future sorting. However, it is important to acknowledge that our second identification strategy that uses intra-jurisdictional variation in taxes should be immune to any such concerns.

IV. Estimates of β/r_T

A. 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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Council Tax	-102.31	-54.14	-32.74	-33.81	-26.96	-29.30	-27.26
	(21.99)	(24.64)	(9.12)	(10.67)	(9.55)	(9.01)	(8.67)
Quadratic LA spend per cap						✓	✓
Quadratic in KS2 test score						✓	✓
Local green space×years							✓
Home characteristics×years							✓
$D_{\tilde{t}} \times D_\kappa \times D_A \times D_{\geq 95}$	✓						
$D_{\tilde{t}} \times D_\kappa \times D_A \times D_{\geq 95} \times D_d$		✓					
$D_{\tilde{t}} \times D_\kappa \times D_{\geq 95} \times D_{b2km}$			✓				
$D_{\tilde{t}} \times D_\kappa \times D_{\geq 95} \times D_{b1.5km}$				✓			
$D_{\tilde{t}} \times D_\kappa \times D_{\geq 95} \times D_{b1km}$					✓	✓	✓
Implied r; β=0.75	0.007	0.014	0.023	0.022	0.028	0.026	0.028
	(0.002)	(0.006)	(0.006)	(0.007)	(0.010)	(0.008)	(0.009)
Implied r; β=1	0.010	0.018	0.031	0.030	0.037	0.034	0.037
	(0.002)	(0.008)	(0.009)	(0.009)	(0.013)	(0.010)	(0.012)
Number of sales pairs	1208216	1008061	512316	398458	262754	262754	262560
R^2	0.68	0.75	0.79	0.79	0.80	0.80	0.81

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. 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 $D_{\tilde{t}}$) interacted with tax band (D_κ), TTWA (D_A), and built since 1995 indicator ($D_{\geq 95}$). 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 (D_d). Columns (3)-(7) replace TTWA with boundary fixed effects (D_{bXkm}) with distance Xkm. Home characteristics interacted with year pairs in Column (7) are property type, no of rooms, wall construction type, built after 1995 indicator. Standard errors for implied r computed using the delta method.

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 price changes 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 fixed effect for each interaction between year pair, tax band, Travel to Work Area (TTWA), and an indicator for built since 1995.[23] Results imply that a one pound increase in tax leads to a house price decrease of £102.31. Based on the assumption that β is between 0.75 and

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

1 (i.e. the range implied by Lutz, 2015), the implied discount rate r_T is between 0.007 and 0.010. 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. To counter the impact of this potential confounder, in column (2) 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 (computed as the average x and y co-ordinates of all home sales). 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 0.018.

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 2km of an LA boundary. The coefficient is smaller in absolute terms and also considerably more precise than in column (2). When we only include observations within 1.5km of an LA boundary in column (4), coefficients are highly similar.

The estimates are slightly closer to zero but statistically indistinguishable from the larger boundary samples if we use a 1km buffer (see column (5), Table 3).[24] To further investigate whether differences in public goods across LA boundaries are correlated to tax changes, column (6) includes quadratic terms in LA spending per head and school test scores. This leads to comparable results. Column (7) 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 nominal discount rate r_T is 0.037 under full capitalisation, and 0.028 when $\beta = 0.75$.

B. Intra-jurisdictional estimates

Table 4 reports intra-jurisdictional estimates based on very narrow geographical fixed effects. Columns (1) to (3) applies the approach described in equation (6) which retains homes with prices close to the tax band thresholds that are shown in Table 1. Homes are allocated to a threshold using a bandwidth set at

[24]In Appendix B.B1 we show that results are highly similar when using narrower boundary definitions, although we lose precision. For this reason we use the 1km boundary for further tests.

10% of the relevant threshold.[25] New homes are problematic as they command a very significant price premium in the UK, and our understanding is that this new build premium should not be factored in to Council Tax valuations. We therefore initially exclude these homes. Column (1) is a basic specification that includes only fixed effects and no home characteristics. These are then added in column (2). Estimated coefficients are somewhat imprecise, but broadly similar to our baseline results in column (6) of Table 3. We add in new homes, adjusting estimated 1991 values for an assumed 20% new build premium in column (3), which makes the coefficient slightly larger in absolute terms.[26] A possible deficiency of the threshold strategy used in columns (1)-(3) is that it may sharpen measurement error to the extent that homes close to tax band thresholds may be misclassified in our data. In Column (4) we instead control flexibly for historic home values using deciles of 1991 home values and use all home sales. The tax coefficient is now more precisely estimated and imply discount rates which are again reasonably close to (within around 0.5%) our baseline estimate. In column (5) we find no significant difference for homes that are close to the tax band thresholds (i.e. within 10% of a threshold) to those that are further away (i.e. not within 10% of a threshold). We conclude that inter- and intra-jurisdictional variation imply similar discount rates.

V. Discount rates

A. *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 $r_T \in [0.028, 0.037]$. The 95% confidence interval assuming full capitalisation is $[0.023, 0.098]$, which narrows to $[0.020, 0.064]$ if we use the larger 2km boundary sample, and slightly widens $[0.017, 0.098]$ if we allow for the β range in the literature. Given the wide diversity in discount rates 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

[25]For example homes with estimated 1991 values in the range £35,000-45,000 are allocated to the A-B threshold and those with 1991 values of £79,200-96,800 are allocated to the D-E threshold.

[26]This premium is supported by various sources. For example Figure 1.1.1 of the 2018 UK housing Review from the Chartered Institute of Housing `https://www.ukhousingreview.org.uk/Contemporary-Issues/2018-Chapter1.pdf` shows a premium that varies over time but averages around 20% between 1995 and 2017.

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

	(1)	(2)	(3)	(4)	(5)
Δ Council Tax	-45.84	-38.36	-35.69	-34.48	-34.12
	(12.50)	(12.79)	(13.21)	(9.02)	(8.94)
Δ Council Tax×Far from threshold					-0.52
					(1.12)
Home characteristics×years		✓	✓	✓	✓
$D_{\tilde{t}} \times D_{\gamma} \times D_{PCD} \times D_{\geq 1995}$	✓	✓	✓		
$D_{\tilde{t}} \times D_{PCD} \times D_{\geq 1995}$				✓	✓
Deciles of estimated 1991 home value				✓	✓
Implied r; β=0.75	0.016	0.020	0.021	0.022	0.022
	(0.004)	(0.007)	(0.008)	(0.006)	(0.006)
Implied r; β=1	0.022	0.026	0.028	0.029	0.029
	(0.006)	(0.009)	(0.010)	(0.008)	(0.008)
Number of sales pairs	31299	31285	45625	104478	104478
R^2	0.92	0.92	0.92	0.90	0.90

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. Regressions in columns (1) to (3) rely on observations close to tax band thresholds set out in Table 1 and include threshold fixed effects (D_{γ}) interacted with years of sales ($D_{\tilde{t}}$), postcode indicators (D_{PCD}), and built since 1995 indicator ($D_{\geq 95}$). 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 group if the 1991 value lies within 10% of the threshold value e.g. homes with 1991 values in the range £36,000-44,000 for the A-B threshold and £46,800-57,200 for the B-C threshold. Columns (1) and (2) exclude homes new at the last sale. Column (3) includes these homes and assumes they command a 20% price premium. Regressions in columns (4) and (5) control for deciles in estimated home value in 1991.

assume full capitalisation i.e. $\beta = 1$. This is attractive both because it provides a plausible upper bound on the discount rate, 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. That said, as we discuss in Section I.A, there are several reasons why capitalisation rates may differ from unity. 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 $\tilde{\beta}$, which captures the rental capitalisation rate. Panel A of Table 5 reports specifications in which we identify this parameter using cross-sectional spatial variation, and hence which yields an estimate of rental capitalisation in the long-term.[27] In the first column we estimate a capitalisation rate of 1.05 with a

[27] We use inter-jurisdictional variation here. We do not use intra-jurisdictional variation because we do

specification that controls for housing attributes, including leasehold tenure, 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 sometimes 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 again essentially equal to one, but much more precisely estimated. We thus find that renters bear nothing of the property tax burden. This is largely consistent with the more credible findings in the literature (e.g Carroll and Yinger, 1994).

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 in the long-term. As outlined in Section I, $\tilde{\beta} = \beta(r_H/r_T)$. Bracke, Pinchbeck and Wyatt (2018) estimate net of growth average real-terms 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 generate estimates for PCL, the urban core of London containing parts of Westminster and Kensington & Chelsea, as we have too few data points. However, we can estimate a comparable real term discount rate for Inner London – an area that subsumes PCL but is somewhat larger – for the period 2004-2013, to assess comparability. This yields an implied real-terms net of growth tax discount rate of 3.0%, which suggests a close correspondence between r_H and r_T.

not observe the sales prices of homes in the rental sample, so we cannot tell if they are close to tax band thresholds, and in any case we do not have enough rental observations or property controls to obtain reliable estimates using this approach here.

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	-1.05	-1.09	-0.96	-1.01
	(0.45)	(0.46)	(0.28)	(0.29)
Observations	16697	16697	24305	24305
R^2	0.89	0.89	0.89	0.89
Panel B: same regressions with prices				
Council Tax	-28.85	-27.79	-30.83	-29.97
	(14.63)	(14.68)	(16.32)	(16.65)
Observations	82990	82990	120451	120451
R^2	0.93	0.93	0.93	0.93
Quadratic in LA spend per cap		✓		✓
Quadratic in KS2 test scores		✓		✓
Local green space		✓		✓
Home characteristics	✓	✓	✓	✓
$D_t \times D_\kappa \times D_{b1km}$	✓	✓		
$D_t \times D_\kappa \times D_{b1.5km}$			✓	✓

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. All regressions are cross-sectional specifications estimated in levels and exclude *(i)* outliers which are defined at the top and bottom 1% of rents/prices in each region and the top and bottom 1% of rents/prices in each tax-band in each region *(ii)* homes that have more than one extension. Rental regressions further exclude homes that have one tenant listed on the rental agreement. Price regressions further exclude leaseholds. Fixed effects are specific to year of sale/rental (D_t), tax band (D_κ), and boundary (D_{bXkm}). Home characteristics are number of rooms, number of rooms squared, extensions (1 or 0), built since 1995, energy efficiency rating, and a three-way interaction between property type, wall type (cavity, solid, unknown), has fireplace. Rent regressions also control for leasehold tenure, number of tenants, and number of tenants per room.

To the extent that one is still concerned that $\tilde{\beta}$ deviates from β, in Table 6 we 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 in the capitalisation literature that β should be higher (in absolute terms) when housing supply is less elastic. Specifically, we interact the tax variable in column (6) of Table 3 with various indicators capturing the housing supply elasticity. In the first two columns we find that the tax coefficient is larger in urban areas and smaller in absolute terms in rural places. The difference is statistically significant. The implied discount rate assuming $\beta = 1$ for urban places is 3.7%. In rural places β is lower or r is higher. Assuming the urban discount rate applies would imply $\beta_{rural} = 0.74$. For inner London vs. elsewhere we do not find a statistically significant difference. In the remaining columns of Table 6 we find

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	-20.28	-27.05	-24.21	-29.71	-28.94	-25.90
	(8.86)	(8.16)	(8.25)	(10.05)	(9.92)	(10.23)
Elastic=0 × Δ Tax	-27.39	-28.33	-28.94	-27.55	-29.30	-27.88
	(8.66)	(12.69)	(8.79)	(8.94)	(10.37)	(8.26)
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	0.037	0.035	0.035	0.036	0.034	0.036
(β=1)	(0.012)	(0.016)	(0.011)	(0.012)	(0.012)	(0.011)
Number of sales pairs	262560	262560	262560	262560	225406	262560
R^2	0.81	0.81	0.81	0.81	0.81	0.81

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. All regressions are as column (7) of Table 3 but interact 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 1990, 2000, and 2007); 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).

little evidence of material differences in the tax coefficients in places with different housing supply elasticities as measured by above or below median share of developable land (column (3)), planning refusal rate for residential developments of larger than 20 dwellings (column (4)), proportion of homes in Conservation Areas (column (5)), or share homes in Green Belts (column (6)).[28] Overall these results suggest that estimates are largely insensitive to variation in the 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 3.5%.

[28]Using the Local Authority share of new build homes in our main sample to define the interaction term yields very similar results: the coefficient for elastic places is -24.18 and the coefficient for inelastic places is -27.71. Note that 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). When we conduct these same tests but specifying housing supply measures as continuous variables, the interaction between tax and the continuous measure of share developable land is statistically significant, but the interactions with other continuous measures are not. Assuming $\beta = 1$, the effect for share developable implies full capitalisation discount rates of 3.8% at the mean value of share land that is potentially developable (47%). Two standard deviations below and above the mean indicate represent places where the share of developable land is 7% and 87% respectively. The implied discount rates for the former again assuming $\beta = 1$ is 3.2%.

B. *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 the previous subsection. 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.

As r_T is a net of growth discount rate, we must also adjust our baseline value of $r_T = 0.037$ 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) grew by 3.8% per year in nominal terms between 1989 and 2016. However, tax movements are tightly correlated with changes in Local Authority spending ($\rho = 0.90$ for 1998-2016 using the CIPFA spending data) so that average nominal net increases in taxes over spending are approximately zero. Using these values as bounds we conclude that our point estimates suggest that average nominal *growth adjusted* or *gross* discount rates for the period 1998-2016 lie in the range $[0.037, 0.075]$.

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 nominal long risk free rate. We obtain an estimate of 3.8% from the average annual nominal yield for the Government Liability curve for all maturities between 1998 and 2016 using Bank of England data. We also use candidate mortgage rates: the 1998-2016 average of the fixed 2 year 75% LTV mortgage rate of 4.4% (obtained from Bank of England series IUMBV34) and the corresponding standard variable rate (SVR) of 5.7% (series IUMTLMV). Together these provide a range of benchmark interest rates of $[0.039, 0.057]$.[29]

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 at the top of our range 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 subsection, the residual difference in these average values is driven by the emergence of very low market rates and a policy of tightly constraining tax rises following the 2008 financial crisis.

C. Time variation

We next shed further light on the relationship between discount rates and benchmark market interest rates by plotting the evolution of the growth adjusted discount rate over time, assuming $\beta = 1$. Because this estimation requires a considerable number of sales in each period, we use the 2km boundary sample. Our approach involves interacting time dummies with the tax variable (transformed appropriately – see equation (B.1) in Appendix B.B2 for more details) and plotting the reciprocal of the resulting coefficients on the tax variables in Figure 3. In the upper left plot, the black line represents the time path of r_T adjusted for long-term annual nominal tax growth of 3.8%, and the shaded area represents the bounds of the 95% confidence intervals. The resulting pattern is somewhat scattered but most estimates fall in the range of 6 to 7%. The point estimates are statistically indistinguishable from one another, suggesting that r_T is stable over the full span of our sample. Figure 3 also plots the nominal long risk-free rate (dashed red line) and the 75% LTV mortgage rate (dot dashed blue line) described above. Visual inspection indicates a reasonably close correspondence between our estimates of r_T and the market interest rates in the period up to and including 2008, but thereafter this relationship seems to break down: the tax implied discount rates remain fairly flat while nominal interest rates fall towards and then under zero. In other words, implied discount rates become disconnected

[29]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. We thus anticipate that the risk premium should be approximately zero, and hence using the risk free rate should be sufficient.

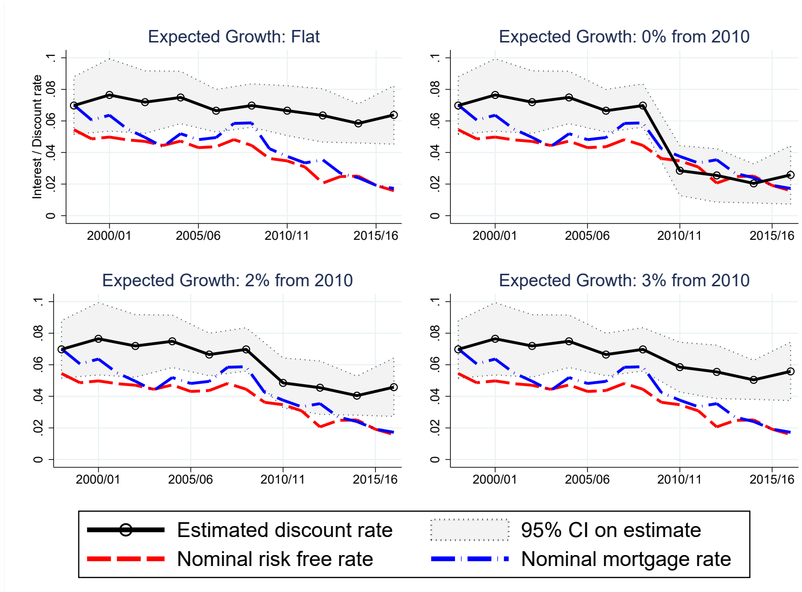

Figure 3 – Implied changes in $1/r_T$

from the benchmark rates from 2008, a finding which is consistent with Bracke, Pinchbeck and Wyatt (2018) who similarly find no evidence of a drop in discount rates in samples either side of the period October 2008 and March 2009 in their study of leaseholds.

Several factors could explain the divergence of estimates from benchmark interest rates after 2008 including changes in expected growth, widening spreads between borrowing rates facing households and published rates, and sticky borrowing due to fixed rate mortgages.[30] We speculate that the most likely explanation is that tax growth expectations were revised downwards by the tax-constraining policies – freezes on short-term tax rises, and local referenda that limit tax rises

[30] One concern might be that truncation of the sample on ownership duration could be behind these results because at the start of the sample we are necessarily restricted to short held homes i.e. the gap between the sales in sales pairs is short. However, this is ruled out because here we are restricting attention to pairs with a maximum gap between sales of 8 years, which means truncation is constant from 2006 onwards. We also obtain similar results if we set the maximum gap to 6 years in which case truncation is constant from 2004 onwards. More generally, we do not find strong evidence for coefficient differences when we use pairs with a long time gap between sales (see Appendix B.B3).

A further possibility is that unobserved changes in the capitalisation rate are driving these patterns in the data. Although we cannot fully rule this out, we show in Appendix B.B2 that when we estimate r_T over time, but interact taxes with above/below median share developable land averaged over 1990, 2000, and 2007, or above/below median change in share developable land between 1990 and 2007, we obtain very similar results for the evolution of r_T in elastic and inelastic places. Furthermore, we obtain highly similar results when we use other measures of the housing supply elasticity.

longer-term – of the incoming coalition government in 2010. Although we are unable to provide definitive evidence to support this hypothesis, we illustrate in the remaining plots of Figure 3 that if we allow expected nominal tax growth to fall to 0, 2, or 3%, the coefficients closely track the benchmark rates.[31]

D. *Heterogeneity*

We report a number of heterogeneity tests in Appendix B.B5. Our chief goal is to examine whether discount rates vary with the level of sophistication and patience of home buyers, and with individuals' inter-temporal opportunity cost of funds. As we lack micro-data on buyer characteristics, we triangulate across a number of alternative approaches. A first set of tests using loan data yields evidence consistent with tax implied discount rates being slightly higher for mortgage-financed homes. Interestingly, this discrepancy is largely offset for homes mortgaged at or just below notch points in UK Loan-to-Value ratios. Borrowing marginally above an LTV notch point implies a large jump in borrowing costs and can be plausibly be avoided at little cost (see Best et al., 2020). Hence, we interpret this latter result as being consistent with sophisticated buyers applying lower rates. A second set of tests uses neighbourhood characteristics. Specifically, we create deciles in two neighbourhood characteristics: income (which we interpret as a proxy for borrowing constraints), and education (which we interpret as a measure of sophistication). We then estimate the effect of taxes on prices in each bin, controlling for the decile interacted with year pairs to partial out any confounding decile trends. We report results in Figure 4. Consistent with priors we find that the coefficients become more negative at higher deciles, suggesting discount rates are decreasing in neighbourhood income and education levels. Although statistically insignificant, if one assumes full capitalisation the coefficients imply sizeable heterogeneity, as those for the lowest deciles imply discount rates in the range of 10% to 20%, whereas those for the third decile upwards imply discount rates of 5% or lower. Our third set of tests explores heterogeneity across tax bands. We find that the effect of taxes on prices is close to zero for lower tax bands and increases in magnitude at higher bands. Assuming $\beta = 1$ throughout

[31] We present several plots here as we consider it plausible that the policies dampened expectations about tax growth. However, we are unable to pin this down precisely as we do not observe buyers' expectations and the relevant horizon is very long. We would not expect growth expectations to fall to zero as the tax freezes were temporary, but this provides a useful lower bound. The referenda policy set caps on long term growth to as low as 2%, but the cap is fixed annually by Parliament, and in any case buyers may anticipate the revocation of this policy, and potentially higher growth rates in the future to offset periods of lower growth.

implies discount rates are thus declining in 1991 home value.

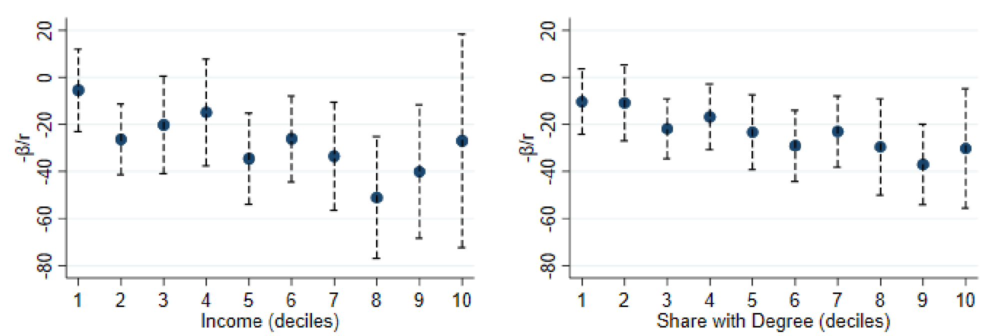

Figure 4 – Effect of taxes on home values by neighbourhood income and education

Notes: Each plot denotes coefficients from a separate regression. Regressions control for decile trends but otherwise as baseline model (column (7) of Table 3).

In summary, although we lack the micro-data necessary to make strong claims, we find consistent support for a proposition that β/r_T is more negative for homebuyers that are more sophisticated, and that face a lower inter-temporal opportunity cost of capital. Of course, a possible explanation for these findings is that the capitalisation parameter, β, also varies across the same homebuyer characteristics. To explore this, we perform similar heterogeneity tests using rental data and report these in Appendix B.B5. We do not find evidence for systematic heterogeneity in $\tilde{\beta}$, which suggests that the variation in β/r_T we find is due to heterogeneity in the discount rate r_T.

VI. Ancillary regressions

A. Measurement error and sensitivity

A battery of sensitivity tests on our preferred repeat sales specification are reported in Appendix B.B3. One might be concerned that taxes could be mismeasured in our data because: *(i)* we assign a home sale an incorrect tax band, either because of a bad match or because we only observe the tax band only at the end of our sample period; *(ii)* we assign the correct tax band but the tax payable is incorrect due to local variation in parish taxes; or *(iii)* correct tax band but tax payable is incorrect due to exemption or discounts.

Mindful of these issues, our baseline approach embodies a conservative strategy in merging tax data to homes, and drops all homes with changing characteristics or extensions, while parish taxes are extremely small and rebanding of homes is rare. Reassuringly, we also find: *(a)* that estimates are robust to dropping small and large homes (which may be subject to tax exemptions or discounts), and retaining only perfect matches; *(b)* that estimates are broadly similar in areas with more and less rebanding, and robust to excluding areas with the highest share of rebanding, or focussing only on homes held for short periods (and that are therefore less likely to be rebanded between sales); and *(c)* that we obtain similar results when we use parish taxes instead of our baseline LA-level measure.

We investigate the extent to which omitted variables may bias our results by employing Oster's (2019) methodology to obtain bias-adjusted estimates. This methodology exploits the intuitive idea that selection on observables is informative about selection on unobservables. Using her GMM-estimator, we confirm that omitted variable bias is unlikely to be an issue. This is corroborated by additional results where we control for sorting based on demographics in our baseline specification.

Finally, we report sensitivity to specification and sample restrictions. In summary, we find that our results are robust 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, and when we allow school test scores to vary continuously over space. Furthermore, findings are robust to various alternative sample selections, including removing a greater or lesser proportion of outlying observations, and relaxing (in part or in full) selections on home extensions and ownership duration.

B. *Expected tax growth and risk*

The role of idiosyncratic risk and expected tax growth in our discount rate estimates are explored by adding a series of interaction terms into our baseline specification. Specifications and results are reported in Appendix B.B4.

We first examine the sensitivity of our estimates to LA-level measures of risk. Here, we find no strong evidence that idiosyncratic risks inherent in property taxes – as measured by political instability or the standard deviation of the annual local tax growth rate in our sample period – significantly alter our discount rate estimates. These results could reflect that risks associated with property taxes can be eliminated by portfolio diversification.

We next assess whether buyers' tax growth expectations drive our discount rate estimates. Given that houses are highly durable and the houses in our sample represent perpetual claims, we would ideally consider infinite-horizon growth expectations. We are of course unable to observe these expectations nor how they are formed, so we are limited to using proxies. Our main measure is the average annual percentage change in taxes in the LA over our near-20 year sample period. We find the interaction of taxes with this proxy for local growth expectations is not significant. This may indicate that buyers expect cyclicality in growth rates (i.e. higher medium term growth in the LA will be balanced by lower growth in the future), but another possible explanation is that our measure is simply a poor proxy for buyers' growth expectations (e.g. because tax growth is hard to predict). To explore this, we obtain historic data for LAs for the period 1978 and 1988 from the Department for the Environment, and compare compound annual growth rates between these two years with the corresponding rate for 1998-2008 for the 68 LAs for which we can match codes. We find a strong negative correlation between average growth ($\rho = -0.4$) in the two 10 year periods, suggesting that the assumption that rapid tax growth will be later compensated by lower growth is not unreasonable. Finally, we also do not find strong evidence for differences in the time pattern of discount rates for LAs that are effectively constrained by the 2010 tax-limiting policies compared to those that are not, which again suggests that buyers growth expectations are formed over very long-term factors rather than short-term LA specific factors.[32]

VII. 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.

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 net of

[32]The findings here may seem at odds with our interpretation of the results in Section V.C. Note that here we are examining local tax setting decisions, whereas in Section V.C we are examining national policy changes which may be perceived as more binding. It is important to stress that we cannot observe expectations nor how they are formed and while we find our explanations plausible, we remain open to alternative formulations.

growth nominal discount rates implied by taxes are in the region of 3 to 4%. 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, Repetto and Tobacman, 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 contribute to a literature that tests for departures from optimising behaviour by repurposing a test extensively used for energy efficient durable purchases to a property tax setting. Previous work suggests households are prone to optimisation errors when information is shrouded or not salient. In contrast, we find little evidence for material undervaluation of property taxes on average. This likely reflects several factors. First, the tax is very simple and Councils ensure tax information is readily available; agents usually list the tax band on adverts and advise clients to register for the tax, and the tax cannot be paid through a mortgage lender. Overall, people know about the tax, and almost everyone pays it on time. Second, discount rates cues in the form of well publicised mortgage and central bank rates make market interest rates salient to home buyers. Third, the housing decisions we study relate to very long horizons where there is already evidence for low rates (Giglio, Maggiori and Stroebel, 2015; Bracke, Pinchbeck and Wyatt, 2018), perhaps because home purchases offer commitment to reduced non-housing consumption (Chetty and Szeidl, 2016; Chetty, Sàndor and Szeidl, 2017). That said, in line with our priors, we do find some evidence that implied discount rates vary with inter-temporal costs of capital and proxies for buyer sophistication. Exploring this heterogeneity further may provide a fruitful avenue for future research.

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