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# How Do Households Value the Future? Evidence from Property Taxes†

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*Despite the near ubiquity of intertemporal 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 spatiotemporal variation in property taxes in England. Our findings imply long-term average net of growth nominal discount rates that are between 3 and 4 percent. The close correspondence to prevailing market interest rates gives little reason to suggest that households misoptimize by materially undervaluing very long-term financial flows in this high-stakes context.* (*JEL* D15, H71, R31)

Assumptions about discount rates feature in an array of economic models and in policy appraisals in settings such as climate change, infrastructure investment, and place-based policies. The rate at which we discount is a description of how we make decisions relating to the future and may be informative about responses to policy interventions.[1] Revealed discount rates also provide a means to test whether individuals systematically undervalue the future (e.g., Busse, Knittel, and Zettelmeyer 2013; Allcott and Wozny 2014; Cohen, Glachant, and Söderberg 2017; De Groote and Verboven 2019) and hence whether they are optimizing.

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 that households use to discount over very long time horizons. Motivated by

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[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.

findings of optimization errors in home purchases (e.g., Genesove and Mayer 2001; Keys, Pope, and Pope 2016) and decisions relating to taxes (e.g., Chetty, Looney, and Kroft 2009; Bradley 2017), we next evaluate whether these implied rates imply departures from fully optimizing behavior. The starting point for our analysis is that for two houses identical in all respects except that 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' intertemporal opportunity cost of funds, then there is little evidence that households are misoptimizing 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 *interjurisdictional* 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 neighborhoods that have plagued previous studies (further described in Hilber 2017). This baseline approach conditions out unobservable price determinants that vary smoothly over space. To counter any residual concerns that differences 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 *intrajurisdictional* variation in taxes. This alternative approach relies on comparisons of neighboring properties within the same jurisdiction that hence 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 capitalization 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 capitalization rate.[2] Given this, we use ancillary data to identify the rental capitalization rate for the period 2013–2016. We estimate that this rate is close to and not significantly different from 100 percent; that is to say, taxes are fully capitalized into rents in our setting. We further test whether tax coefficients are different between places with elastic and inelastic housing supply, which is relevant

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

because inelastic housing supply is consistent with full capitalization (Hilber 2017). 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 percent.[3] We next turn to the question of whether households make systematic optimization errors over future property tax liabilities. If nominal tax growth expectations are fixed at the long-term tax growth rate of 3.8 percent per year, growth adjusted discount rates are maximally 7.5 percent. Comparing this to prevailing saving and borrowing rates gives us little reason to believe that households materially undervalue future tax liabilities. Furthermore, implied discount rates track benchmark interest rates over time relatively closely if we assume that 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 home buyers (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 capitalization parameter also varies across the same home buyer 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 the lab. The experimental literature, which typically examines choices between relatively small-stake—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 center 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

[3] The 95 percent confidence interval for this is 2.3 percent to 9.8 percent, which narrows to 2.0 percent to 6.4 percent 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.

degree of external validity both because of widespread market participation and because households devote a sizable share of spending to their homes.[4]

Besides this, we contribute to literature strands that test for deviations from standard assumptions that underpin traditional economics models of behavior (see DellaVigna 2009). Our article is methodologically aligned to papers that use undervaluation of future financial flows as a test for misoptimization. To date, this literature has focused 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 misoptimization test is applicable beyond energy efficiency. Our work is particularly relevant to research that tests for departures from fully rational behavior in housing and taxation domains. Previous studies find that home sellers display loss aversion (e.g., Genesove and Mayer 2001; Engelhardt 2003) and buyers display projection bias (Busse et al. 2012) and that many unconstrained households fail to refinance mortgages optimally (Keys, Pope, and Pope 2016). Furthermore, individuals underreact 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 misoptimization 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 misoptimization and likely follows from the simplicity and transparency of the tax we study, the availability of discount rate cues in the form of well-publicized 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 optimization 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 endeavor because it can shed light on the relationship between interest rates and house prices and because it is useful to researchers attempting to establish annualized amenity values (see, e.g., Chay and Greenstone 2005).[6] Related research using fixed-term

[4] Around 70 percent of households in England owned their homes in 2008. Piazzesi and Schneider (2016) show that housing services account for slightly under a fifth of total consumption (including durables) in the United States.

[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).

[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 sizable impact of current discount rates on the value of future gains leads standard models to predict a large impact of interest rates on prices, especially in high price growth*

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 misoptimization and 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 capitalization of property taxes into home values. Following standard household bidding model assumptions including full household mobility, the equilibrium value $(V_i)$ of home $i$ can be decomposed into the present value of the flow of housing services minus the present value of the future stream of property tax payments:[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 capitalization 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 $\pi$. 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 capitalization parameter $(\beta)$. Both terms are expressed as present values by dividing by annualized growth adjusted discount rates, which can be interpreted as implied rates of return.

*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 overoptimism (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 misoptimization.

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

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 pretax 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 that 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 capitalization rate $\beta$. Separating $\beta$ 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 et al. 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 $\beta$ has been treated as an empirical question in a voluminous literature going back to Oates (1969). Faced with a fundamental difficulty in separately identifying $\beta$ and $r_T$ using home values, the vast majority of studies, reviewed in Yinger et al. (1988); Ross and Yinger (1999); and Hilber (2017), have estimated $\beta$ from house prices and property taxes given assumptions about $r_T$.

Estimates of capitalization rates range from 0 (i.e., 0 percent, no capitalization) to 1.4 (i.e., 140 percent, more than full capitalization). 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, capitalization 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 2017). Arguably the most plausible estimates of $\beta$ use quasi-experimental approaches to mitigate endogeneity concerns. In particular, Gallagher, Kurban, and Persky (2013) find close to full (100 percent) capitalization of property taxes into home values, whereas estimates reported in Lutz (2015) fall in the range of 70 percent to 97 percent for homes in urban areas.[10]

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

[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 capitalized into land prices in Toronto, whereas Besley, Meads, and Surico (2014) find that buyers capture around 60 percent of a transfer tax holiday in the United Kingdom.

The advantage of using rents $(R_i)$ rather than prices to estimate capitalization rates is that a capitalization 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 capitalization rate, the parameter $\tilde{\beta}$ here can be related to the parameter $\beta$ 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 $\beta$. When $r_T \neq r_H$, the extent to which the capitalization parameter in the rents equation provides a good proxy for the capitalization parameter in the price equation depends on the extent of expected growth and the relative size of the risk premia since $\tilde{\beta} = \beta(r_H/r_T) = \beta\big(r^f + r_H^p - E(g_H)\big)/\big(r^f + r_T^p - E(g_T)\big)$.[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 capitalization 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 $r_T = 0.04$ given assumed full capitalization 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 capitalization 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 capitalization of taxes and housing discount rates upwards of 9 percent. Our work improves on these studies by using better data and a much more convincing identification strategy.

### B. *Institutional Setting*

*Organization of Local Government*.—The chief organizational 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 online Appendix 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

[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 $\beta$ 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 $\varepsilon_D$ to the sum of the demand and supply elasticities $\varepsilon_S$, i.e., $\varepsilon_D/(\varepsilon_S - \varepsilon_D)$. This is analogous to the theoretical determinants of the capitalization rate.

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 centers, parks, and play areas and can also have a say in local land use and planning decisions.

*Local Government Services*.—Local government accounts for roughly a quarter of public spending in England. The main components of revenue spending are education (40 percent); social care (20 percent); policing and fire services (15 percent); culture, planning, and environment (10 percent); and transport (5 percent). 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 online Appendix A.1.

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 oversubscribed, 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 online Appendix A.2, the situation for primary school is less clear cut. According to Gibbons, Machin, and Silva (2013), between 2003 and 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 represent the next largest budget share. However, only a small fraction of the population (around 2 percent) 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 percent of local spending, services are either nonexcludable at the boundary (such as transport services, parks, and museums) or commonly subject to reciprocal agreements between neighboring LAs (such as library access).

*Council Tax*.—LA 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 that represents around a quarter of LA funding.

We provide a detailed discussion of the Council Tax and its legislative basis in online Appendix A.1. Council Tax is payable on all domestic homes. In contrast to many property taxes, liability rests with occupiers rather than with owners of homes. The legislation sets out a number of qualifications to liability, including a 25 percent

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 |

*Source:* Reproduced from Slack (2002)

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 2014–2015, 97 percent 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-publicized 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 subdivision is determined both by the number of layers of local government that 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 percent of the total LA budget requirement in 2011–2012.

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 completed 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 percent 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 schedule, households are able to obtain information about the Council Tax band for individual homes easily—e.g., through online portals or home sales agents.

[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 (VOA). Data for 2010–2011, 2011–2012, and 2013–2014 are held in Table 3.2 of "Council Tax Valuation Lists: Changes" in the UK government's web archive. Data for 2009–2010 and 2012–2013 were released under Freedom of Information and are 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 a magazine in 2010 (Anker, Guy. 2010. "Over half a million homes in wrong council tax band since 1997." *Money Saving Expert*, April 19. https://www.moneysavingexpert.com/news/2010/04/half-a-million-homes-in-wrong-council-tax-band/). Homes can be "rebanded" following a successful appeal to the VOA or when changes to the property are detected by officials and a new valuation concludes that the property should be placed in a new band. Where physical improvements result in a revaluation, 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 wholesale revaluations, most recently in April 2016. We discuss more details regarding rebanding in online Appendix A.A1.

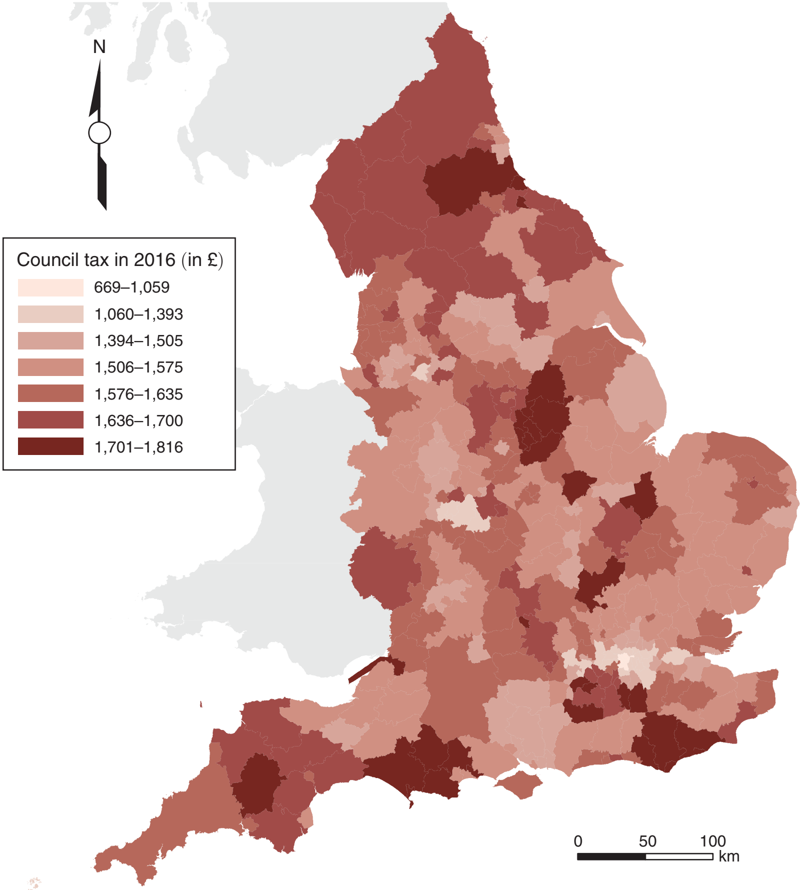


Figure 1. Tax in 2016–2017

*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 2016–2017. 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 online Appendix A.4 shows the average annual change in taxes between 1998–1999 and 2016–2017. 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 in 1998–1999 and 2016–2017 is close to 0 (the correlation is only $-0.048$). At the national level, taxes move in step with LA 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, the 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 in the long term—was a policy that subjects large tax rises to local approval. Consequently, since 2012–2013, LAs wishing to raise taxes above a threshold set annually by Parliament, usually in practice between 2 percent and 4 percent, have needed to put this to a local referendum (a path that 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 percent subsidy for freezing nominal taxes in 2011–2012. Although initially announced as a one-year policy with no commitment beyond 2011–2012, similar but less generous freeze subsidies were then subsequently announced each year until 2014–2015. The freezing policy was dropped in 2015–2016, 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 LA in England in 2008 would be 21.5–38.1 percent 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 percent in cities such as Leeds, Birmingham, and Manchester.

## II. Empirical Approach

### A. *Estimating* $\beta/r_T$

In the first step in our estimation procedure, we exploit the full size of the dataset to estimate $\beta/r_T$ 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 $\pi$ indicates the impact of housing attributes, $\beta/r_T$ is the (combined) parameter of interest, $\phi_t$ are year fixed effects, and $\omega_{it}$ denotes an identically and independently distributed error term.

The above equation is unlikely to identify a causal effect $\beta/r_T$ because the Council Tax is not uniform over space and likely correlated to features that make places attractive and yield higher housing values. Moreover, to the extent that $H_i$ 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 denote $\tilde{t} = t_1 - t_0$, $\Delta V_{i\tilde{t}} = V_{it_1} - V_{it_0}$, 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 $\times$ tax band $\kappa$–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 if 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 $\pi$ 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, and 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 capitalize in housing values, $\beta/r_T$ would be biased toward zero (so that $r_T$ would be biased upward). 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 public goods. Hence, to reduce this potential bias, we will focus on repeated sales that occur close (1, 1.5, or 2 kilometers (km)) to an LA boundary and include boundary fixed effects $\phi_{\kappa b\tilde{t}}$ for each boundary $b$ and each tax band–year $\kappa$-$\tilde{t}$ combination. The coefficient of interest, $\beta/r_T$, 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 band and sales year combinations, $\kappa$-$\tilde{t}$.

Including boundary fixed effects should effectively control for changes in public good provision (and other local amenities) to the extent that the benefits are continuous over space.[14] We test this more directly by gathering data on total local

[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*

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 acknowledgment of these factors, we therefore specify separate sets of fixed effects for homes built 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, $\beta / r_T$ would not measure the effect of taxes, but it captures preferences for neighbors. 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

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

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

## B. *Intrajurisdictional Estimates of* $\beta / r_T$

Until this point, all specifications have relied on *interjurisdictional* variation in taxes, i.e., the identifying variation derives from differences in LA tax setting decisions. We can also use *intrajurisdictional* variation to estimate $\beta / r_T$ by comparing tax and price changes for neighboring 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 *intrajurisdictional* approach means that we are unable to use the year pair $\times$ 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.

*boundary*. The 1, 1.5, and 2 km boundary distances are selected as they yield sample sizes that are sufficient to obtain relatively precise estimates.

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 $\times$ year $\times$ threshold fixed effects. The identifying assumption is that the prices of these neighboring 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 being 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.

### C. *Estimating Capitalization and Discount Rates Separately*

The next step is to obtain information about the capitalization 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 $\beta$. 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 neighbor 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:

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

[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 interjurisdictional approach where we can rely on unambiguous district boundaries.

where the estimated $\beta/r_T$ should be (very) comparable to the previous analysis using repeat sales. Hence, equation (8) is an overidentification 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 online Appendix A.5. The Land Registry Price Paid dataset captures the universe of home sales in England from 1995 (HM Land Registry 2014). The data record the transaction price, the sale registration date (which proxies for the actual date of sale), the full address, the type of house (flat, detached house, semidetached house, terraced (or row) house), a new build indicator, and tenure (leasehold or freehold). There are no publicly available data for home rentals for England, so we rely on data obtained from Homelet, the United Kingdom'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 n.d.). 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 number of home extensions—from energy performance certificates (Department for Communities and 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 they record the contemporaneous tax band as well as the home address, which allows us to merge them with the sales and rentals files. Mindful of mismeasurement, 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 and Local Government 1993).[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 LA boundaries using Geographical Information System and boundary shapefiles (Ordnance Survey; Office for National Statistics 2011).[18] LA expenditure on services per head of population is added from records held by the

[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 percent each year), but in any case, we provide several robustness checks in online Appendix B.3.

[17] Specifically, we compute the annual tax payable using the LA-wide average Band D Council Tax for each financial year in the published tables, and then we 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 online Appendix B.3 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 a boundary region corresponding to the closest boundary. Boundary samples are computed for both pre- and post-2009 LAs. These samples are highly similar: we use the post-2009 boundaries that contain fewer boundaries.

Chartered Institute for Public Finance and Accountancy (CIPFA) (n.d.). 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 n.d.). We create two annually varying postcode-level measures that are both based on the inverse distance weighted score of this school quality measure in the nearest four schools in a given year. Our primary measure is constructed using tests scores only for schools in the associated LA 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 2 distance buffers (0–500 meters (m) and 500–1,000 m) 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, minimize unobserved home changes, and mitigate measurement error. Further details are listed in online Appendix A.6. We remove outliers in three ways. First, we exclude the top and bottom 1 percent of prices (or rents) in each region and the top and bottom 1 percent 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 that are extreme outliers in terms of population size or expenditure on local services, which we define as more than double the ninety-ninth percentile or less than half of the first percentile. Third, we remove homes in the top tax band (Band H). These make up around 0.6 percent of the stock of homes and, in many cases, are exceptional properties with unique features, which is reflected in the mean price of 2.2 million with standard deviation of 2 million. In any case, our rental data contain 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 the 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 time span.[19] This entails dropping homes with one or more extension at the time of any certificate, homes where the floor area of the property moves by more than 20 percent from the median value for the home in the data, and homes that are recorded as being "new" more than once, which likely indicates redevelopment. We 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

[19] There are at least three reasons why we wish to remove these homes. First, time-varying characteristics render 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 that 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.

TABLE 2—DESCRIPTIVE STATISTICS: REPEAT SALES

| Restrictions: | Without | | | | With | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | SD | min | max | Mean | SD | min | max |
| *Panel A. Full sample* | | | | | | | | |
| Price | 199,684 | 182,787 | 195 | 17,000,000 | 174,914 | 96,297 | 14,750 | 1,585,000 |
| Tax | 1,184.13 | 355.83 | 331.89 | 3,450.88 | 1,154.72 | 323.37 | 331.89 | 2,970.57 |
| KS2 score percent | 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 | 2,854.64 | 653.24 | 630.50 | 60.32 | 2,854.64 |
| Greenspace 0–500 m percent | 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 percent | 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. 1 km boundary sample* | | | | | | | | |
| Price | 235,862 | 277,035 | 1,500 | 17,000,000 | 178,383 | 109,508 | 14,750 | 1,585,000 |
| Tax | 1,227.42 | 375.77 | 331.89 | 3,450.88 | 1,117.81 | 292.68 | 331.89 | 2,804.42 |
| KS2 score percent | 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 | 2,854.64 | 768.11 | 662.98 | 64.09 | 2,854.64 |
| Greenspace 0–500 m percent | 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 percent | 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 | | | |

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 online Appendix B.3.

## 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 after the original sale.[20] Descriptive statistics for this dataset are shown in Table 2. Panel A describes the full dataset both without (LHS) and with (RHS) sample restrictions. Panel B of Table 2 examines sales that lie within 1 km of a boundary with a different LA, which is our main boundary buffer distance. Due to the nature of the sample restrictions, we expect the mean sales price, size of home, and Council Tax in the restricted sample to be lower than in the full sample. We indeed find that this is the case. Table 2 also highlights that sales in the restricted 1 km boundary sample have a slightly lower average Council Tax than the full unrestricted sample and benefit from slightly higher LA spending per head.

[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 2013–2014 suggests that the median length of ownership tenure in the United Kingdom is 13 years (8 years for mortgaged homes)(Department for Communites and Local Government 2020). We focus on these short-duration homes because it reduces the chance that homes are rebanded between sales and ensures that truncation of the sample on duration is consistent over a large part of our sample. We show in online Appendix B.3 that estimates are similar for short- and long-duration homes.

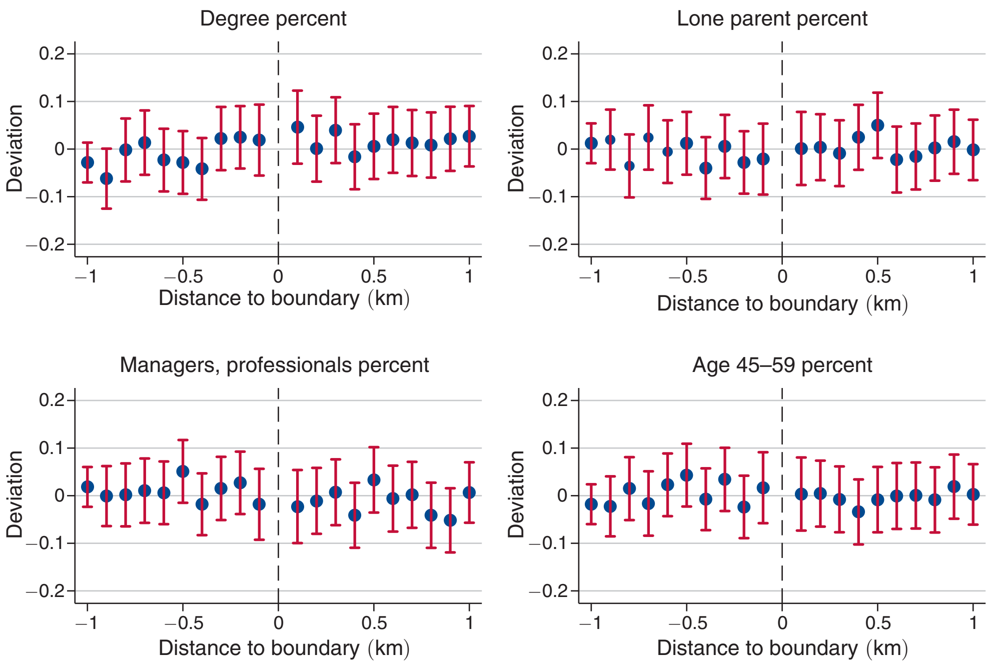


Figure 2. Changes between 2001 and 2011 Censuses

*Note:* Low-tax side is negative.

## 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, $\beta / r_T$ would capture not just the effect of taxes but also preferences for neighbors. 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 select OAs in boundary samples, then assign them the low- or high-tax-change side of the boundary using changes in taxes between 2001 and 2011.[22] Some OAs are close to multiple LA boundaries, so we drop any of these on the high-tax side of one boundary but the low side of another. We assign each OA to a distance bin for each boundary sample that 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 standardized by

[21] OAs 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.

[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.

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. We also present a similar analysis for cross-sectional taxes in 2011 in Figure A3 in online Appendix A.7. 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 neighborhoods in London that were deprived in 1881 remain so today, and Ambrus, Field, and Gonzalez (2020) show that a nineteenth-century cholera epidemic is evident in house prices 160 years later. While we feel that 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 intrajurisdictional variation in taxes should be immune to any such concerns.

## IV. Estimates of $\beta/r_T$

### A. *Interjurisdictional Estimates*

Table 3 reports estimates of $\beta/r_T$ in which we regress sale prices on property taxes and control variables. In all cases, regressions are performed on data samples using the restrictions described above. Standard errors are clustered on post-2009 LAs. Furthermore, the inclusion of year pair $\times$ 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 capitalization parameters from interjurisdictional variation in taxes.

Column 1 is the most basic specification that absorbs common trends in different labor 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 £1 increase in tax leads to a house price decrease of £102.31. Based on the assumption that $\beta$ is between 0.75 and 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 centers in our sample period may have reduced relative pressure on budgets in LAs in the center 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 center by interacting the fixed effects with a categorical variable capturing

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

TABLE 3—INTERJURISDICTIONAL ESTIMATES OF AVERAGE $\beta/r$ (DEP. VAR.: $\Delta$ SALE PRICE IN £)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| $\Delta$ 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 $r$; $\beta = 0.75$ | 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 $r$; $\beta = 1$ | 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 | 1,208,216 | 1,008,061 | 512,316 | 398,458 | 262,754 | 262,754 | 262,560 |
| $R^2$ | 0.68 | 0.75 | 0.79 | 0.79 | 0.80 | 0.80 | 0.81 |

*Notes:* Standard errors in parentheses clustered on post-2009 LAs. All regressions are first difference specifications estimated in levels that include only repeat sales with fixed characteristics. Column 1 includes dummies for financial year of first and subsequent sale (year pairs $D_{\tilde{t}}$) interacted with tax band ($D_\kappa$), TTWA ($D_A$), and built-since-1995 indicator ($D_{\geq 95}$). Column 2 further interacts these effects with a categorical variable that puts each postcode in one of ten bins according to distance to TTWA center ($D_d$). Columns 3–7 replace TTWA with boundary fixed effects ($D_{bXkm}$) with distance $X$ km. Home characteristics interacted with year pairs in column 7 are property type, number of rooms, wall construction type, built-after-1995 indicator. Standard errors for implied r computed using the delta method.

the decile of postcode distance to the TTWA center (computed as the average $x$ and $y$ coordinates 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 capitalization $(\beta = 1)$ is around 0.018.

All remaining columns in Table 3 are based on boundary samples and include boundary fixed effects instead of TTWAs. In column 3, we only include observations within 2 km of an LA boundary. The coefficient is smaller in absolute terms and also considerably more precise than it is in column 2. When we only include observations within 1.5 km of an LA boundary in column 4, coefficients are highly similar.

The estimates are slightly closer to 0 but statistically indistinguishable from the larger boundary samples if we use a 1 km 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 neighborhood 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 capitalization and 0.028 when $\beta = 0.75$.

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

TABLE 4—INTRAJURISDICTIONAL ESTIMATES OF AVERAGE $\beta/r$ (DEP. VAR.: $\Delta$ SALE PRICE IN £)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| $\Delta$ Council tax | −45.84 | −38.36 | −35.69 | −34.48 | −34.12 |
| | (12.50) | (12.79) | (13.21) | (9.02) | (8.94) |
| $\Delta$ Council tax $\times$ far from threshold | | | | | −0.52 |
| | | | | | (1.12) |
| Home characteristics $\times$ years | | ✓ | ✓ | ✓ | ✓ |
| $D_{\tilde{t}} \times D_{\gamma} \times D_{PCD} \times D_{\geq 95}$ | ✓ | ✓ | ✓ | | |
| $D_{\tilde{t}} \times D_{PCD} \times D_{\geq 95}$ | | | | ✓ | ✓ |
| Deciles of estimated 1991 home value | | | | ✓ | ✓ |
| Implied $r$; $\beta = 0.75$ | 0.016 | 0.020 | 0.021 | 0.022 | 0.022 |
| | (0.004) | (0.007) | (0.008) | (0.006) | (0.006) |
| Implied $r$; $\beta = 1$ | 0.022 | 0.026 | 0.028 | 0.029 | 0.029 |
| | (0.006) | (0.009) | (0.010) | (0.008) | (0.008) |
| Number of sales pairs | 31,299 | 31,285 | 45,625 | 104,478 | 104,478 |
| $R^2$ | 0.92 | 0.92 | 0.92 | 0.90 | 0.90 |

*Notes:* Standard errors in parentheses clustered on post-2009 LAs. Regressions in columns 1 to 3 rely on observations close to tax band thresholds set out in Table 1 and include threshold fixed effects ($D_{\gamma}$) 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 percent 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 percent price premium. Regressions in columns 4 and 5 control for deciles in estimated home value in 1991.

## B. *Intrajurisdictional Estimates*

Table 4 reports intrajurisdictional estimates based on very narrow geographical fixed effects. Columns 1 to 3 apply 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 10 percent of the relevant threshold.[25] New homes are problematic, as they command a very significant price premium in the United Kingdom, 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 percent 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

[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 percent between 1995 and 2017.

values and use all home sales. The tax coefficient is now more precisely estimated and implies discount rates that are again reasonably close to (within around 0.5 percent of) 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 percent of a threshold) to those that are further away (i.e., not within 10 percent of a threshold). We conclude that inter- and intrajurisdictional variation imply similar discount rates.

## V. Discount Rates

### A. *Disentangling $r_T$ and $\beta$*

Based on our reading of the existing property tax capitalization 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 percent confidence interval assuming full capitalization is $[0.023, 0.098]$, which narrows to $[0.020, 0.064]$ if we use the larger 2 km boundary sample and slightly widens to $[0.017, 0.098]$ if we allow for the $\beta$ 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 $\beta / r_T$ requires a single value of $\beta$. One proposition is to assume full capitalization, 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 capitalization is full. That said, as we discuss in Section IA, there are several reasons why capitalization rates may differ from unity. In this section, we provide further evidence to assess the validity of such an assumption.

Although we cannot estimate $\beta$ directly using house prices, we can estimate $\tilde{\beta}$, which captures the rental capitalization rate. Panel A of Table 5 reports specifications in which we identify this parameter using cross-sectional spatial variation and hence which yield an estimate of rental capitalization in the long term.[27] In the first column, using the 1 km boundary sample, we estimate a capitalization rate of 1.05 with a specification that controls for housing attributes including leasehold tenure but not public goods. Column 2 adds controls for LA spending, test scores, and access to green space. This specification suggests that the capitalization rate $\tilde{\beta}$ is slightly above but not statistically significantly different from 1, meaning that a £1 increase in taxes leads to a £1 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 1.5 km 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

[27] We use interjurisdictional variation here. We do not use intrajurisdictional variation because we do 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 km buffers | | 1.5 km buffers | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A. Taxes and rents* | | | | |
| Council tax | −1.05 | −1.09 | −0.96 | −1.01 |
| | (0.45) | (0.46) | (0.28) | (0.29) |
| Observations | 16,697 | 16,697 | 24,305 | 24,305 |
| $R^2$ | 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 | 82,990 | 82,990 | 120,451 | 120,451 |
| $R^2$ | 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 parentheses clustered on post-2009 LAs. All regressions are cross-sectional specifications estimated in levels and exclude (i) outliers that are defined at the top and bottom 1 percent of rents/prices in each region and the top and bottom 1 percent of rents/prices in each tax band in each region and (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 ($D_\kappa$), and boundary ($D_{bXkm}$). Home characteristics are number of rooms, number of rooms squared, extensions (one or zero), built since 1995, energy efficiency rating, and a three-way interaction between property type, wall type (cavity, solid, unknown), and has fireplace. Rent regressions also control for leasehold tenure, number of tenants, and number of tenants per room.

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 as the dependent variable. This implies that we again identify $\beta/r_T$. 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 suggest 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 capitalize in rents in the long term. As outlined in Section I, $\tilde{\beta} = \beta\left(r_H/r_T\right)$. 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 percent in 1987–1991 and 2.5 percent for the period 2004–2013. We cannot generate estimates for PCL, the urban core of London containing parts of Westminster and Kensington and Chelsea, as we have too few data points. However, we can estimate a comparable real terms 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

Table 6—Housing Supply Elasticities (Dep. var.: Δ sale price in £)

| Dep. var.: Δ sale price | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Elastic $= 1 \times \Delta$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 $= 0 \times \Delta$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 green belt |
| Implied $r$; Elastic $= 0$ ($\beta = 1$) | 0.037 | 0.035 | 0.035 | 0.036 | 0.034 | 0.036 |
| | (0.012) | (0.016) | (0.011) | (0.012) | (0.012) | (0.011) |
| Number of sales pairs | 262,560 | 262,560 | 262,560 | 262,560 | 225,406 | 262,560 |
| $R^2$ | 0.81 | 0.81 | 0.81 | 0.81 | 0.81 | 0.81 |

*Notes:* Standard errors in parentheses clustered on post-2009 LAs. All regressions are as column 7 of Table 3 but interact $\Delta$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 nonurban setting; in column 2, postcodes outside inner London; 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 belts (2011).

implied real terms net of growth tax discount rate of 3.0 percent, which suggests a close correspondence between $r_H$ and $r_T$.

To the extent that one is still concerned that $\tilde{\beta}$ deviates from $\beta$, in Table 6, we take a different approach to assessing $\beta$. 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 capitalization literature that $\beta$ 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 percent. In rural places, $\beta$ is lower or $r$ is higher. Assuming that the urban discount rate applies would imply that $\beta_{rural} = 0.74$. For Inner London versus elsewhere, we do not find a statistically significant difference. In the remaining columns of Table 6, we find 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

[28] Using the LA 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 counterintuitive 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 specify housing supply measures

to variation in the housing supply elasticity. Moreover, in support of our previous findings, in all cases, we find that the implied discount rate in places with tighter housing supply elasticity (where $\beta$ is plausibly equal to one) is close to 3.5 percent.

### B. *Tests for Intertemporal Optimization*

One general test for optimizing behavior, 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 intertemporal 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 $\beta$ is supported by the evidence in the previous subsection. In addition, to the extent that full capitalization provides an upper bound on the $\beta$ parameter value, in light of our hypotheses, this is a conservative assumption. This is because values of $\beta$ 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 percent per year in nominal terms between 1989 and 2016. However, tax movements are tightly correlated with changes in LA spending ($\rho = 0.90$ for 1998–2016 using the CIPFA spending data) so that average nominal net increases in taxes over spending are approximately 0. 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 rate is the nominal long risk-free rate. We obtain an estimate of 3.8 percent 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 percent loan-to-value (LTV) mortgage rate of 4.4 percent (obtained from Bank of England series IUMBV34) and the corresponding standard variable rate of 5.7 percent (series IUMTLMV). Together, these provide a range of benchmark interest rates of $[0.039, 0.057]$.[29]

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 capitalization discount rates of 3.8 percent at the mean value of share land that is potentially developable (47 percent). Two standard deviations below and above the mean indicate places where the share of developable land is 7 percent and 87 percent, respectively. The implied discount rate for the former, again assuming $\beta = 1$, is 3.2 percent.

[29] As an alternative, we could use the Capital Asset Pricing Model to derive a benchmark rate. Changes in real taxes are positively correlated with changes in real household final consumption expenditure per head, which

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 2 km boundary sample. Our approach involves interacting time dummies with the tax variable (transformed appropriately—see equation (B.1) in online Appendix B.2 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 percent, and the shaded area represents the bounds of the 95 percent confidence intervals. The resulting pattern is somewhat scattered, but most estimates fall in the range of 6 to 7 percent. 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 percent 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 toward and then under zero. In other words, implied discount rates become disconnected from the benchmark rates from 2008, a finding that is consistent with Bracke, Pinchbeck, and Wyatt (2018), who similarly find no evidence of a drop in discount rates in samples on either side of the period October 2008 through 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 downward by the tax-constraining

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.

[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 eight years, which means that truncation is constant from 2006 onward. We also obtain similar results if we set the maximum gap to six years, in which case truncation is constant from 2004 onward. More

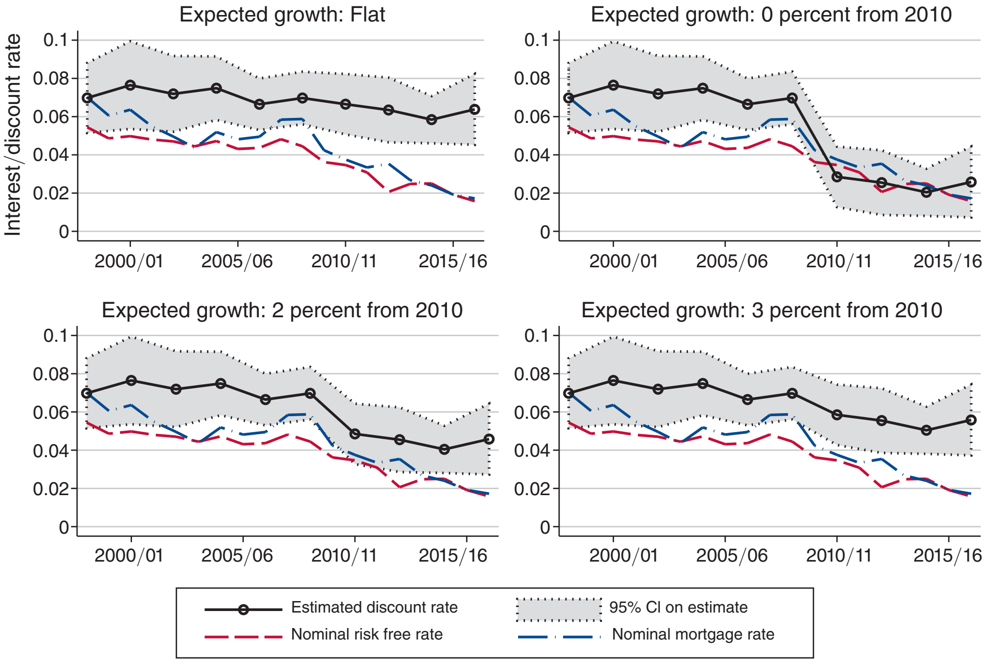


FIGURE 3. IMPLIED CHANGES IN $1/r_T$

policies—freezes on short-term tax rises and local referenda that limit longer-term tax rises—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 percent, the coefficients closely track the benchmark rates.[31]

## D. *Heterogeneity*

We report a number of heterogeneity tests in online Appendix B.5. Our chief goal is to examine whether discount rates vary with the level of sophistication and patience of home buyers and with individuals' intertemporal opportunity cost of

generally, we do not find strong evidence for coefficient differences when we use pairs with a long time gap between sales (see online Appendix B.3).

A further possibility is that unobserved changes in the capitalization rate are driving these patterns in the data. Although we cannot fully rule this out, we show in online Appendix B.2 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 of 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.

[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 percent, 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.

funds. As we lack microdata 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 LTV ratios. Borrowing marginally above an LTV notch point implies a large jump in borrowing costs and can 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 neighborhood characteristics. Specifically, we create deciles in two neighborhood 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 that discount rates are decreasing in neighborhood income and education levels. Although statistically insignificant, if one assumes full capitalization, the coefficients imply sizable heterogeneity, as those for the lowest deciles imply discount rates in the range of 10 percent to 20 percent, whereas those for the third decile upward imply discount rates of 5 percent 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 that $\beta = 1$ throughout implies that discount rates are thus declining in 1991 home value.

In summary, although we lack the microdata necessary to make strong claims, we find consistent support for a proposition that $\beta / r_T$ is more negative for home buyers that are more sophisticated and face a lower intertemporal opportunity cost of capital. Of course, a possible explanation for these findings is that the capitalization parameter, $\beta$, also varies across the same home buyer characteristics. To explore this, we perform similar heterogeneity tests using rental data and report these in online Appendix B.5. We do not find evidence for systematic heterogeneity in $\tilde{\beta}$, which suggests that the variation in $\beta / r_T$ 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 online Appendix B.3. 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 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) we assign the correct tax band, but the 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

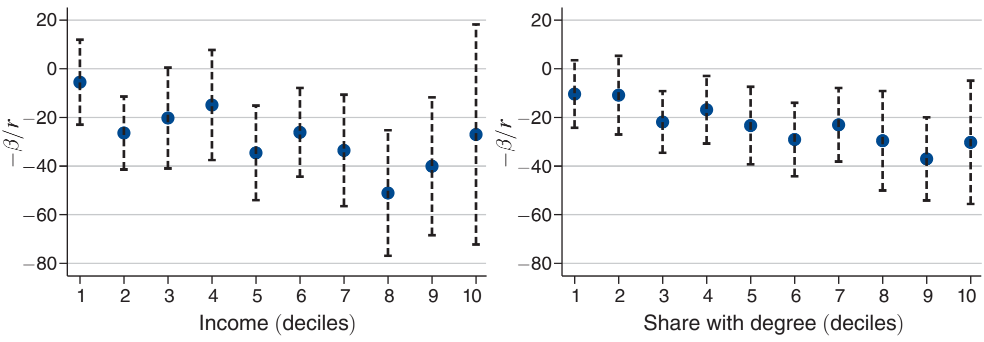


FIGURE 4. EFFECT OF TAXES ON HOME VALUES BY NEIGHBORHOOD INCOME AND EDUCATION

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

or extensions, while parish taxes are extremely small and rebanding of homes is rare. Reassuringly, we also find that (i) estimates are robust to dropping small and large homes (which may be subject to tax exemptions or discounts) and retaining only perfect matches, (ii) estimates are broadly similar in areas with more and less rebanding and are robust to excluding areas with the highest share of rebanding or focusing only on homes held for short periods (and that are therefore less likely to be rebanded between sales), and (iii) 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 generalized method of moments 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 online Appendix B.4.

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 or 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 that 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 1978–1988 from the Department for the Environment and compare compound annual growth rates between these 2 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 ten-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 behavior in economics. In this paper, we assessed 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 spatiotemporal 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 growth nominal discount rates implied by taxes are in the region of 3 to 4 percent. 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 focused on shorter horizons. Findings

[32] The findings here may seem at odds with our interpretation of the results in Section VC. Note that here, we are examining local tax setting decisions, whereas in Section VC, we are examining national policy changes that may be perceived as more binding. It is important to stress that we cannot observe expectations or how they are formed, and while we find our explanations plausible, we remain open to alternative formulations.

may be of particular interest to researchers that wish to estimate annualized amenity values using house prices.

We also contribute to a literature that tests for departures from optimizing behavior by repurposing a test extensively used for energy-efficient durable purchases to a property tax setting. Previous work suggests that households are prone to optimization 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 that 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 rate cues in the form of well-publicized 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 of low rates (Giglio, Maggiori, and Stroebel 2015; Bracke, Pinchbeck, and Wyatt 2018), perhaps because home purchases offer commitment to reduced nonhousing 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 intertemporal costs of capital and proxies for buyer sophistication. Exploring this heterogeneity further may provide a fruitful avenue for future research.

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