How do Households Value the Future? Evidence from Property Taxes – Appendix

By Hans R.A. Koster and Edward W. Pinchbeck*

Draft: April 8, 2021

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

JEL: G10, R30
Keywords: housing, property taxes, discount rates, undervaluation

* Koster: Department of Spatial Economics. Vrije Universiteit Amsterdam, `h.koster@vu.nl`. He is also research fellow at the Higher School of Economics (St. Petersburg) and the Tinbergen Institute. Pinchbeck: Department of Economics, University of Birmingham, `e.w.pinchbeck@bham.ac.uk`. We thank Hayoung Kim for excellent research assistance. This work was part completed at the Centre for Economic Performance which is funded by the ESRC under grant number ES/M010341/1. Further, Koster acknowledges support of the HSE University Basic Research Program. We are grateful for helpful comments and suggestions from the editor and two anonymous referees, Felipe Carozzi, Steve Gibbons, Vernon Henderson, Christian Hilber, Henry Overman, and Olmo Silva, as well as seminar participants at City University, London School of Economics, Kraks Fond Copenhagen, the Universities of Exeter and Reading. The authors do not have relevant or material financial interests that relate to the research described in this paper.

Appendix A Data and context

A1. Institutional details

Although generally considered to operate under a highly centralised model, services provided by local government organisations in England – including schools, social services, roads, planning and housing, and policing – account for roughly a quarter of all public spending. Local government spending for 2007/8 is shown in Table A1 below, reproduced from Hilber, Lyytikäinen and Vermeulen (2011).

Local government features multiple organisational layers, with some spatial variation in the way service delivery is structured. The chief organisational unit is the Local Authority (LA). LA boundaries changed once in our sample period, in 2009, when a series of mergers reduced the number of LAs from 354 to 326 – see Figure A1.

A2. Local Authority services

Schooling in England is divided into five Key Stages. Primary school education encompasses Key Stage 1 (age 5-7) and Key Stage 2 (age 7-11). Compulsory secondary schooling covers Key Stage 3 (age 11-14) and Key stage 4 (age 14-16). Pupils are assessed at the end of each Key Stage using nationally set assessments, with test results then published each year. Over 90% of students in England are educated in state schools. Local Authorities with responsibility for state education are known as Local Education Authorities (LEA). LEAs are responsible for state schools in their district, pass through central government funds to schools, and administer admissions. Single tier LAs are LEAs throughout our sample period.

Table A1 – Local Government Spending 2007/8

Service	**Net expenditure (£million)**
Education	39,602
Highways & Transport	5,595
Social Services	18,385
Housing	2,469
Culture, planning, environment	9,830
Police & Fire	13,358
Other	3,645
Total	92,884

Source: Reproduced from Hilber, Lyytikäinen and Vermeulen (2011)

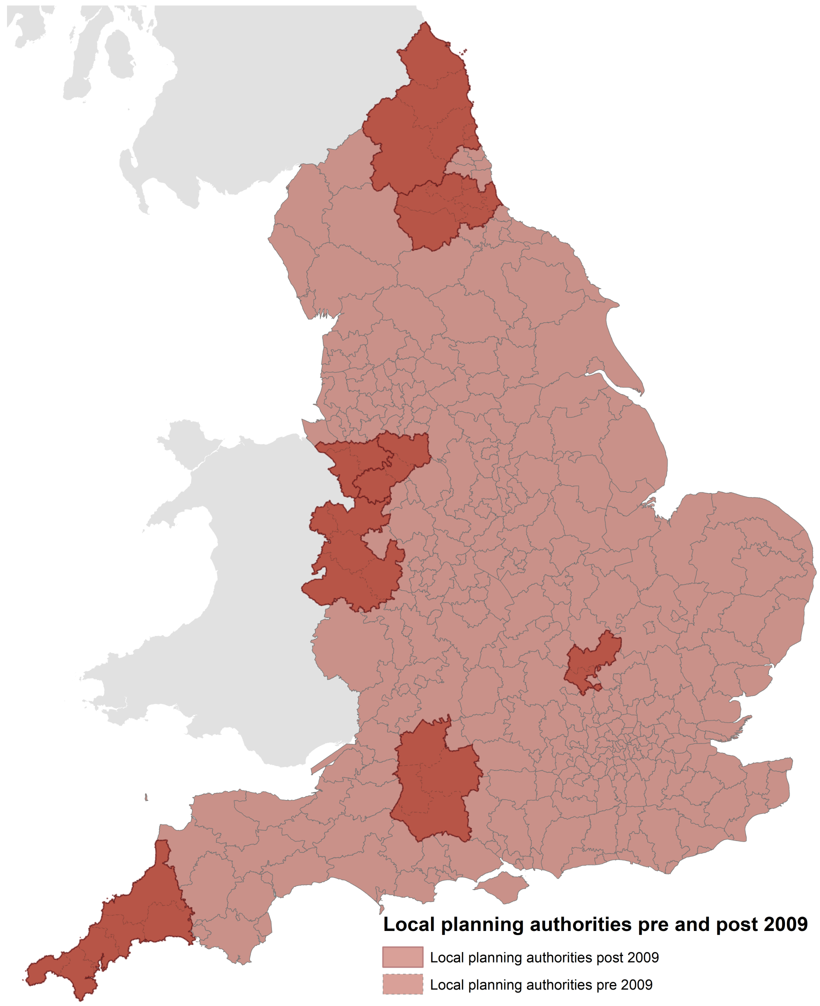

Figure A1 – Local authorities pre and post 2009

For two-tier LAs, responsibility for education passed from the upper to the lower tier of the LA in 2010. For simplicity in the main paper we refer to LEAs as LAs.

As noted in the main paper, a choice system operates in both primary and secondary state education. In practice, choices may be constrained by capacity, in which case LEAs allocate places based on school admission criteria. These capacity issues are not a significant factor for secondary schooling because some 84% of parents are offered their most preferred secondary school, while 95% get an offer from a school in their top three.[1] Even where capacity is constrained, the dominant way over-subscription is managed is by straight line distance of a family's home to the secondary school without reference to the LEA boundary. Burgess, Greaves and Vignoles (2019, pp. 694) report that other admission criteria for schools that do not select on ability or religion typically include whether the child has a statement of special educational need; is looked-after by the local authority; or has a sibling at the school already. Note that the term "looked after by a local authority" is defined in section 22 of the Children Act 1989 to indicate a child (0-18 years of age) who is subject to a care order (or an interim care order) or who is accommodated by the local authority.

The situation for primary schools is less clear-cut. Here, around two thirds of schools are controlled by LEAs and must organise admissions in line with a School Admission Code, while the remainder are faith and charity run schools which choose who to admit. For the former group, the evidence is mixed as to whether the LA boundary is a binding constraint on admission. Burgess et al. (2015) note that there is no legal constraint on applying outside an LEA and their empirical work assumes distance to school is again paramount in determining choice sets. On the other hand, Gibbons, Machin and Silva (2013) argue that LEA boundaries do create barriers for school admission for primary schools, at least for the period 2003-2006, and that LEAs were not under a legal obligation to accommodate non-resident pupils. They go on to show discontinuities in primary school quality at LEA boundaries.

A3. Local Authority finance

As we discuss in the main paper, there are three main sources of local government funding: grants from central government, locally raised taxes, and fees and charges. Besides Council Tax, a second locally raised tax on commercial properties, business rates, is collected locally but is passed in full or in part to central government and then redistributed. There are no other meaningful local taxes. Fees and charges cover services such as car parking, adult social care, planning applications, cremations and burials. Legislation requires that income from these fees and charges is restricted to the cost of providing services i.e. they cannot generate profits.

Grants from central government provide the majority of funding. The grant

[1] See `https://www.bbc.co.uk/news/education-36527159`.

system is complex and has changed over time – see Hilber, Lyytikäinen and Vermeulen (2011) for details. Grants include a number of specific grants targeted to particular activities such as education and social care, and a non-specific general grant allocated on the basis of funding formulae (Formula grant). This grant is based on an empirical assessment of local need, and adjusted to take account of revenues that central government expects should be raised locally from Council Taxes.[2] Until 2013 all income from local commercial property taxes was passed to central government and then redistributed to LAs. In 2013 a scheme was introduced to incentivise LAs to promote local economic growth. From that time LAs have been able to retain up to 50% of locally raised business rates revenues and revenue growth. However, in practice a system of tariffs and top-ups continues to redistribute these business rates, and a safety net guarantees LAs receive at least 92.5% of their baseline funding from this source.

A4. Further details about Council Tax

In the main paper, we show a Figure for LA Council Tax in 2016/17. Figure A2 below shows the change in taxes between 1998/99 and 2016/17.

Liability for the payment of the tax is set out in the Local Government Finance Act 1992 (LGFA 1992) and The Council Tax (Liability for Owners) Regulations 1992. Liability is determined on a daily basis. Where a home is empty the owner is liable, and when occupied usually one occupant is the liable person.[3] However, in some situations an owner can be liable for an occupied home: the home is a house in multiple occupation (HMO), all residents are under the age of 18, a home where people staying temporarily, the house is accommodation for asylum seekers. Notwithstanding these small exceptions, liability therefore rests with the occupant. In line with this, it is standard practice for rental agreements in England and Wales to specify that tenants pay the tax themselves rather than channel payments through landlords, and some agreements contain a clause to this effect. Although there are no institutional constraints to prevent alternative arrangements, we are aware of advisors that caution tenants to be wary of agreements for taxes to be paid through landlords because the tenant

[2]This local tax base element is determined by the number of homes in each tax band and as noted in the main paper, homes very rarely change bands.

[3]A hierarchy of liability determines the liable person. This hierarchy is (i) resident owner, (ii) a resident tenant, (iii) a resident with permission to stay in the home, (iv) another resident e.g. a squatter. Where a home is shared through joint ownership or a joint tenancy, the parties are jointly and severally liable. This means they are equally responsible for payment and the Council can pursue any parties for payment of the tax.

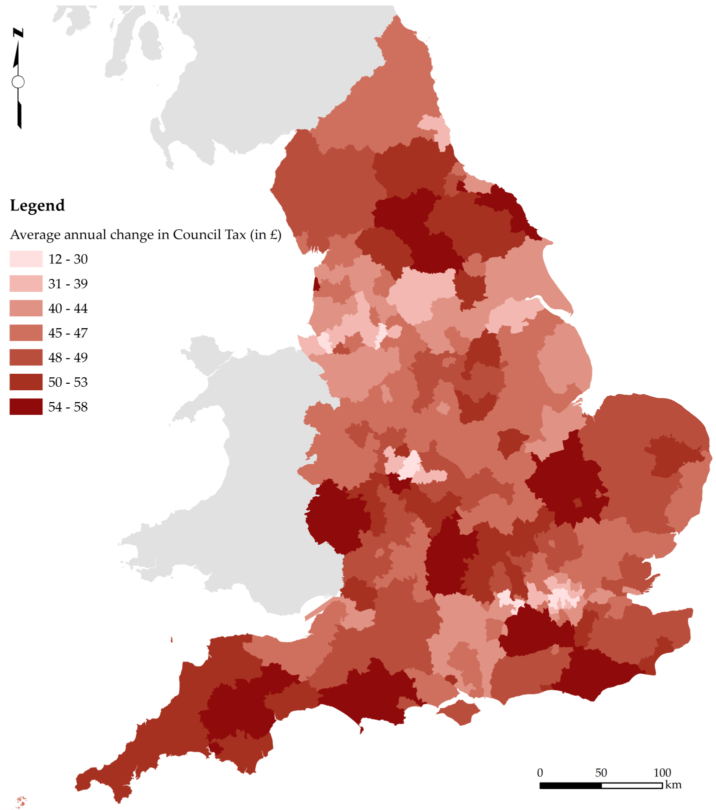

Figure A2 – Tax change 1998/99-2016/17

remains liable if the landlord fails to pass on the funds.

The Council Tax (Administration and Enforcement) Regulations 1992 set out the administration of the tax. This requires resident, owner, or managing agent of home to supply information to Councils relevant to tax liability, and grants powers to Councils to collect additional information from other public bodies. In practice Councils bill previous occupants until notified otherwise so it is common for occupants to contact Councils in advance of a home being sold or a rental tenancy ending to end their liability. Subsequently the new liable person will register with a Local Authority with details of who lives in the property, whether it is rented or owned, and when the period of occupancy started. In the case of rental properties, owners and rental agents may also notify the Council of the start and end of tenancy agreements.

Part 1 of the LGFA 1992, The Council Tax (Exempt Dwellings) Order 1992, The Council Tax (Discount Disregards) Order 1992 and The Council Tax (Administration and Enforcement) Regulations 1992 set out a number of qualifications to liability. First there is a 25% discount for single occupiers, which is defined as living alone or only with those "disregarded" for the purpose of computing liability as is defined in Schedule 1 of LGFA 1992 and including full time students, professional or unpaid full-time live-in carers, and mentally incapacitated individuals.[4] Besides this, there are various exemptions for some residences including 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.

Under the legislation, Councils have a variety of enforcement powers to check details about the liability and to recover unpaid taxes. At the start of the year, or the start of a new occupancy, the liable person is sent the figure for the full year's Council Tax. They can choose to pay the full amount in a lump sum, or can elect to pay in instalments throughout the year. Penalties for non-payment of the tax increase progressively. If one payment is missed, occupants will get a reminder notice requesting payment in 7 days. Missing three payments each year will result in a request to pay the year's bill in full in 7 days. If the bill remains unpaid, the Council has several enforcement options. These include a Liability order (a legal demand for payment from a magistrate), recovering unpaid tax directly from wage earnings from employers, of from benefits; and using bailiffs to take belongings to be sold to cover debts. Ultimately non-payment can result in prison sentences and repossession. Besides non-payment Councils also have a number of mechanisms to detect fraudulent reporting of liability information, such as falsely claiming a single occupier discount. These include checking data with other public bodies, including from credit referencing agencies. Providing false information regarding to the Council is an offense under the legislation noted above, and can be punished with a fine and/or a prison sentence.

Under the relevant legislation, all dwellings must be valued for the purposes of Council Tax and assigned to one of eight tax bands noted in Table 1. A dwelling is defined in English law as any substantial self-contained residential unit that is occupied or could be occupied, and includes single family homes as well as

[4]Note that if all occupants are disregarded for liability purposes a 50% discount applies unless there is a full exemption.

individual flats/apartments which are therefore each valued separately.

The assignment of any dwelling to a tax band follows a valuation in accordance with the Council Tax Manual issued by the Valuation Office Agency (VOA). The valuation assesses the value of the dwelling in a hypothetical open market sale at a fixed past date, known as the Antecedent Valuation Date (AVD). The AVD for England, unchanged since the introduction of the tax, is 1 April 1991. The valuation is complex and performed by specialist surveyors. As the Council Tax Manual notes, "*The basis of valuation states that the value of any dwelling shall be taken to be the amount which, subject to certain assumptions, it might reasonably have been expected to realise if it had been sold by a willing vendor.*" These assumptions include: *(i)* that the sale was with vacant possession; *(ii)* that the interest sold was the freehold or, in the case of a flat, a lease for 99 years at a nominal rent; *(iii)* that the dwelling was sold free from any rent charge or other encumbrance; *(iv)* in most cases that the size, layout and character of the dwelling, and the physical state of its locality, were the same as at the relevant date; *(v)* that the dwelling was in a state of reasonable repair; *(vi)* in the case of a dwelling, the owner or occupier of which is entitled to use common parts, that those parts were in a like state of repair and the purchaser would be liable to contribute towards the cost of keeping them in such a state; *(vii)* in the case of a dwelling which contains fixtures to which this sub-paragraph applies, that the fixtures were not included in the dwelling; *(viii)* that the use of the dwelling would be permanently restricted to use as a private dwelling; and *(ix)* that the dwelling had no development value other than value attributable to permitted development.[5]

An initial valuation exercise conducted in 1991 and 1992 assigned all homes to a Council Tax band by 1993. The Council Tax Manual sets outs nine reasons why a new valuation may be required. These are *(i)* a dwelling was omitted in error from the compiled list or its initial entry in the compiled list is thought to be incorrect; *(ii)* a new (post 1 April 1993) dwelling requires to be inserted; *(iii)* there has been a "material increase" in the value of a dwelling and a "*relevant transaction*"; *(iv)* a dwelling has become or ceased to be a composite dwelling or where, continuing to be a composite, there has been a change in the domestic/non-domestic balance of the dwelling; *(v)* there has been a "*material reduction*" in

[5]See for a fuller discussion of the valuation procedure `https://www.gov.uk/government/publications/council-tax-manual`.

the value of a dwelling, caused (in whole or in part) by the demolition of any part of the dwelling; *(vi)* there has been a "*material reduction*" in the value of a dwelling, caused (in whole or part) by any change in the physical state of the dwelling's locality; *(vii)* there has been a "*material reduction*" in the value of a dwelling, caused (in whole or part) by any adaptation of the dwelling to make it suitable for use by a physically disabled person; *(viii)* a mistake was made when altering the list for any of the above reasons; and *(ix)* a clerical error requires correction. As defined in Section 24 of LGFA 1992, a material increase is, "*any increase in the value of a dwelling which is caused (in whole or in part) by any building, engineering or other operation carried out in relation to the dwelling.*", and a relevant transaction is, "*a transfer on sale of the fee simple, a grant of a lease for a term of 7 years or more, or a transfer on sale of such a lease.*"

A new valuation that takes place for reasons *(i)* or *(ii)* above will result in a home being placed into one of the eight tax bands in Table 1 for the first time. A new valuation for reasons *(iii)*-*(ix)* may result in the home remaining in the same band or else moving up or down one or more valuation bands, which we denoted 'rebanding'. A downwards rebanding happens immediately, but for the reasons set out above, in the case of a material improvement that warrants a move to a higher band, the move does not occur until the home is sold. Solicitors should inform prospective buyers that this is to take place.

As discussed in the main paper, very few homes are rebanded each year. Although we lack definitive evidence why this is, it could plausibly reflect a low volume of new valuations, or that few new valuations result in a move to a new tax band. In practice, sources suggest that the VOA does not continuously assess all homes, but rather assesses properties if *(i)* a Council Tax appeal is made by an occupier or owner or *(ii)* it becomes aware that a property may have had a material increase or reduction in value. A key source of information is likely to be the planning and building control process of Local Authorities. These processes are invoked when major alterations are made to homes, including almost all external changes and extensions, as well as internal changes that involve plumbing, electrical, ventilation, or heating systems. Another possible source of information is the rebanding of one or more properties of a similar size, type and construction in the immediate vicinity of a home. Although we are aware this can happen, we anticipate it is rare given the small overall number of rebandings that do occur.

A5. Additional information about our data

We supplement our sales and rentals data with additional characteristics from Energy Performance Certificates (EPCs) (Department for Communities & Local Government, 2017). Since 2007 an EPC has been required whenever a home is constructed or marketed for sale or for rent and a dataset for all EPCs issued since 1 October 2008 has recently been released by the UK government. The certificates contain information of the energy performance of buildings and their physical characteristics that are obtained by a physical inspection of the interior and exterior of the home by an independent assessor. We extract various characteristics from this dataset before merging the information into the Land Registry database (HM Land Registry, 2017). We use the May 2017 release of the EPC data as our house price data runs until April 2017 and we do not wish to use information from subsequent EPCs. Our merging strategy is to sequentially match individual sales to the EPC data using the full address or a subset of the address and the date of the sale and certificate. Specifically, we first match a sales to certificates using the primary address object name (PAON; usually the house name or number), secondary address object name (SOAN; usually flat number), street name, and full postcode then retain the certificate that is closest in days to the sale or taking the median value of characteristics where there is more than one EPC in the same year as the sale. We then repeat this exercise for unmatched properties but allowing one of the PAON or SOAN to be different. Our final round of matching matches on the full postcode. Any sales that remain after without a match following this process are considered unmatched. The matched dataset provides us the number of rooms; floor area; and the wall construction type (solid wall or cavity wall). The EPC data also records the number of extensions that have been added to the property at the time of the certificate, but provides no detail on the size or nature of any such extension.

We harvest tax band data originating from the VOA (Valuation Office Agency, 2017) from web sources for an arbitrarily chosen subset of postcodes. Because the tax data and the home sales data have never been linked before, we conduct a second matching exercise to link sales in the combined Land Registry-EPC dataset. We again match homes using the full property address and the postcode of the house, but now use a more conservative matching strategy given the potential for the measurement error in our main variable when matches are incorrect. We then link home to actual annual tax payments as described in the main paper.

Combining these data in this way gives us the approximate annual tax payable for each house at the time of its sale. However, this tax payable will not exactly correspond to the actual amount of tax payable in all cases because the tax data gives us the average Band D amount for homes in the administrative region, where the average is computed across all parishes in the LA. While this should accurately capture any precepts from higher layers of local government (such as levies for the GLA in London), it will not accurately capture sub-LA variation in parish precepts.

Parish precept data is available from the UK government, but only for financial years 2013/14 to 2016/17 (Ministry of Housing, Communities & Local Government, 2014, 2016*a*). We extract this data and use it in cross-sectional regressions to investigate whether this correction has any impact on the results. To compute home level taxes using the parish level data, we first match parish names in the MHCLG data to parish codes using a file from the ONS Open Geography portal (Office for National Statistics, 2018). We then deduct the average tax-band specific parish level precept for the LA in the relevant financial year from our LA tax data, then add back in the actual tax-band specific parish precept for the given parish.

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

We draw on various further sources of information. To examine sorting, we use statistics for small area geographies from the 2001 and 2011 Censuses for England (Office for National Statistics, 2011*b*, 2017*a*). Our housing supply elasticity measures are drawn from disparate sources. We compute the share of land that is developable from Land Cover Maps for 1990, 2000, and 2007 (UK Centre for Ecology & Hydrology, n.d.). The planning refusal rate is calculated from data from the UK government (Ministry of Housing, Communities & Local Government, 2016*b*). The share of homes in Conservation Areas is computed using shapefiles provided to us by Historic England (Historic England, n.d.), and the share of homes in the

Green Belt is computed using a shapefile published online (Pope, Addy, 2017). To test whether discount rates are systematically different for mortgaged homes we link Land Registry sales to transaction level data from the Nationwide Building Society (Nationwide Building Society, n.d.). The Nationwide also provides us with a historical house price index (1995-1998) (Nationwide Building Society, 2017) which we use to supplement indices we generate from the Land Registry data for 1998 onwards. Data from The Elections Centre (The Elections Centre, n.d.) is used to compute variables relating to voting in Local Authority elections. The Office for National Statistics provides us with estimated household income for neighbourhoods in England in 2004/5 (Office for National Statistics, 2010). We compute the share of homes rebanded using statistics for various years from the Valuation Office Agency (Valuation Office Agency, 2016), and data on the stock of homes in LAs in 2015 from the same source (Valuation Office Agency, 2015). Market interest rates are from the Bank of England: mortgage rates are calculated using data series IUMBV34 and IUMTLMV (Bank of England, n.d.), and the long risk-free rate is computed from the Government Liability curve (Bank of England, 2020). We deflate house prices into 2015 prices for some specifications using Consumer Prices Index (CPI) data available from the Office for National Statistics (Office for National Statistics, 2017*b*), and we use Retail Prices Index (RPI) data from the same organisation to compute the long-term average annual growth rate in local property taxes (Office for National Statistics, 2020). We use data from the English Housing Survey 2013-14 to compute the average length of ownership for UK homes (Department for Communities and Local Government, 2020), and data from the Department of the Environment to compute growth in domestic taxes at the LA level between 1978 and 1988 (Department of the Environment, 1988).

The spatial units we use in the course of the empirical work include several iterations of Local Authority geographies, LA boundary samples, parishes, labour-market areas, postcodes, output areas, and a rural-urban indicator based on the 2011 Rural-Urban Classification. We link homes to other spatial units and variables using postcode directories (Office for National Statistics, 2016), as well as other mapping and lookup files we create specifically for this project. Mapping files and boundary samples were created in Geographical Information Systems (GIS) using LA boundary shapefiles accessed via the UK Data Service (Office for National Statistics, 2011*a*) and postcode polygon shapefiles. The latter are available under licence from Ordnance Survey (Ordnance Survey, n.d.), and were accessed for this project via an organisational subscription to Digimap (`https://digimap.edina.ac.uk`), an online service operated by the University of Edinburgh. Besides this, we hand collate two lookup files that match historical LA codes to the contemporary coding systems used by the Office for National Statistics (Koster and Pinchbeck, 2021*a*,*b*).

A6. Sample restrictions

The theory underpinning our work indicates that regression of prices (or rents) on taxes should be estimated in levels (see equation (1)), an issue often neglected in the capitalisation studies reviewed by Ross and Yinger (1999). We take heed of the theory in our choice of functional form, and remove outliers which we usually define as the top and bottom 1% of prices (or rents) in each region and the top and bottom 1% of prices (or rents) in each tax-band-region combination to ensure that extreme prices are not driving our findings. We also drop band H homes and a small number of Local Authorities which are extreme outliers in terms of population size or expenditure on local services. In particular we drop 2 LAs – the City of London and the Isles of Scilly, which is in any case has no boundaries with other LAs – which both have populations that are less than half the 1st percentile LA population. We also drop one further LA: Birmingham which is vastly bigger than all other LAs – its population and expenditure on services are both more than double the value of the LA at the 99th percentile. We also note that an article in the Birmingham Post highlights that this LA is also an outlier as it generates the least income from Council Tax despite having a relatively high charge due to it's cheap housing.[6]

A7. Sorting

Using the same approach underpinning Figure 2 in the main text, we use Census data for Output Areas (OAs) to assess the extent to which demographic variables are correlated with property taxes across LA boundaries in 2011 in Figure A3). We first select OAs in 2011 in the 1km boundary samples, then assign them low or high tax side of boundary using taxes in 2011.[7] Some OAs are close to multiple LA boundaries so we drop any on the high tax of one boundary but the low side

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

[7]We restrict attention to those boundaries that have large, above median, differences in taxes. We obtain near-identical results if we keep all boundaries.

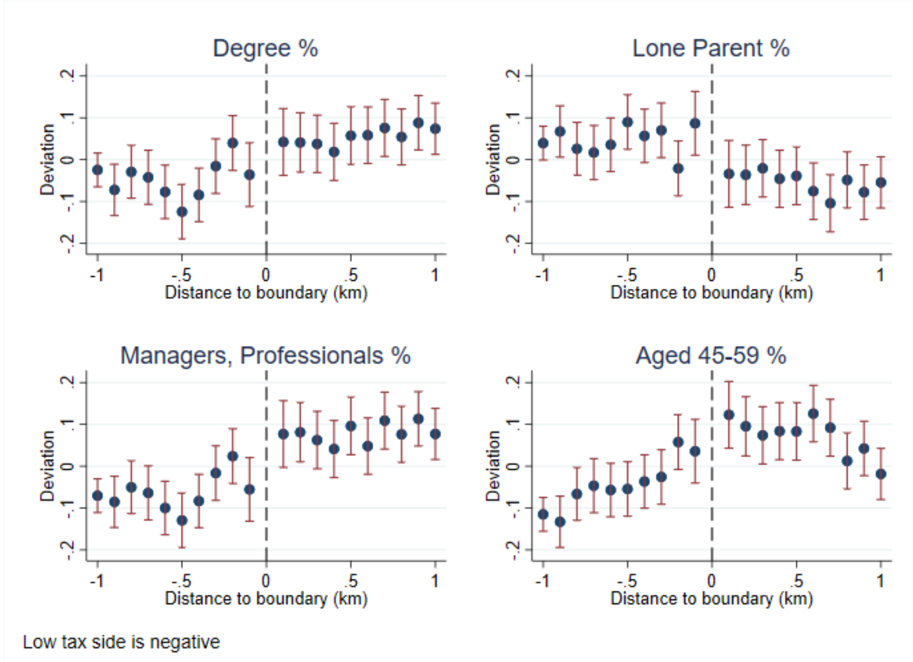

Figure A3 – 2011 Census

of another. We assign each OA to a distance bin for each boundary sample they fall in, based on the median distance to the boundary of postcodes that lie both within the OA and the boundary sample. Distance is coded as negative for the lower tax side of the boundary. We then run cross-sectional OA regressions of various Census variables on distance bin dummy variables. Figure A3 reports the results.

We find some cross-sectional differences across LA borders. It seems that the share of people with higher education is higher in places with higher taxes. Also the share of employed people in managerial and professional occupations has a higher representation in areas with higher taxes. This may indicate that higher skilled people may have a stronger preference for the services provided by the higher taxes. Despite these cross-sectional, possibly long-run, differences, we re-emphasise here that our first identification strategy uses temporal differences in taxes, rather than cross-sectional differences. Moreover, the second identification strategy that uses intra-jurisdictional variation in taxes should be immune to any concerns related to sorting. Finally, we show in Table B4 that our baseline

Table A2 – Geographical units in main samples

Unit	Number in 1km boundary sample	Number in full sample
TTWAs	134	155
Boundary regions	621	775
Postcodes	68,431	314,265

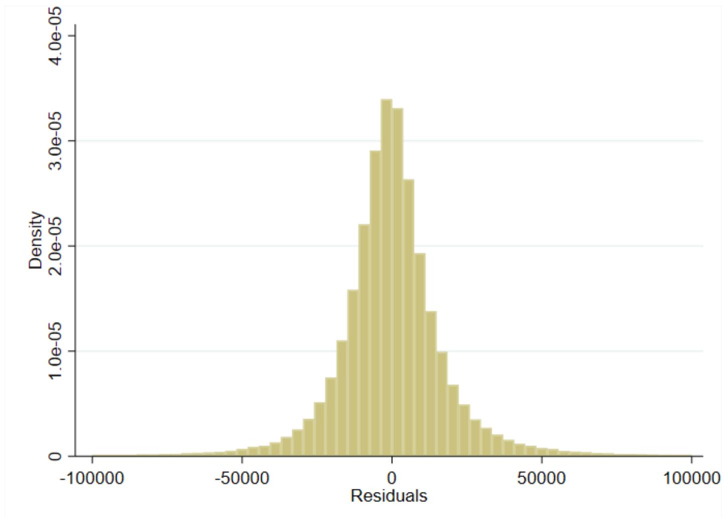

Figure A4 – Residuals from regression of Δ price on fixed effects (1km sample)

repeat sales results are unaffected by controlling for neighbourhood characteristics interacted with year of sale pairs.

A8. Further details of our main estimation sample

Our main estimation sample is comprised of home within 1km of LA boundaries. Table A2 below shows how many geographical units are represented in this sample alongside the number contained in the full (boundary and non-boundary) sample. Hence, this provides information on the effective number of observations in the regressions.

Figure A4 is a histogram of the residuals from a regression of Δ prices on the fixed effects in our main estimation on the 1km sample. This shows that residuals are normally distributed and that outliers are unlikely to drive our results.

Appendix B Other results

B1. Narrower boundaries

In our baseline regressions we use homes located within 1 kilometre of a Local Authority boundary. In Table B1 column (2) we show that the interaction between distance to the boundary and tax is not statistically significant. We then assess whether findings are robust to using narrower boundaries. As we reduce the boundary size, results become less precisely estimated but are statistically indistinguishable from our baseline result. Moreover, implied discount rates (assuming full capitalisation) are never higher than 4% using the narrower boundaries.

Table B1 – Repeat sales, narrower boundary samples
(Dep var: Δ sale price in £)

	(1)	(2)	(3)	(4)
Δ Council Tax	-27.26	-28.29	-27.63	-31.87
	(8.67)	(8.66)	(12.92)	(17.04)
Δ Council Tax $\times$ distance		0.00		
		(0.00)		
Years×band×BFE	✓	✓	✓	✓
Boundary:	1km	1km	200m	100m
Observations	262560	262560	30664	9580
R^2	0.81	0.81	0.85	0.87

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities.

B2. Time variation in β/r_T

One assumption in the above estimations is that discount and capitalisation rates are constant across time, i.e. $r_t^T = r_T$, $r_t^H = r_H$, and $\beta_t = \beta \ \ \forall t$. Here, we allow the discount rate to evolve over time, which is consistent with findings in Bracke, Pinchbeck and Wyatt (2018). Differencing between time t_1 and t_0, assuming $\beta_t = \beta = 1$, and ignoring P for expositional simplicity we obtain:

$$\Delta V_{i\tilde{t}} = -\frac{1}{r^T_{t_1}} T_{it_1} + \frac{1}{r^T_{t_0}} T_{it_0} + \frac{\pi_{t_1}}{r^H_{t_1}} H_i - \frac{\pi_{t_0}}{r^H_{t_0}} H_i \tag{B.1}$$

Note that when estimating the above equation $r^T_{t_1}$, and $r^T_{t_0}$ etc. should be internally consistent. For example, in one transaction pair $t_1 - t_0$, $r^T_{t_0}$ should be the

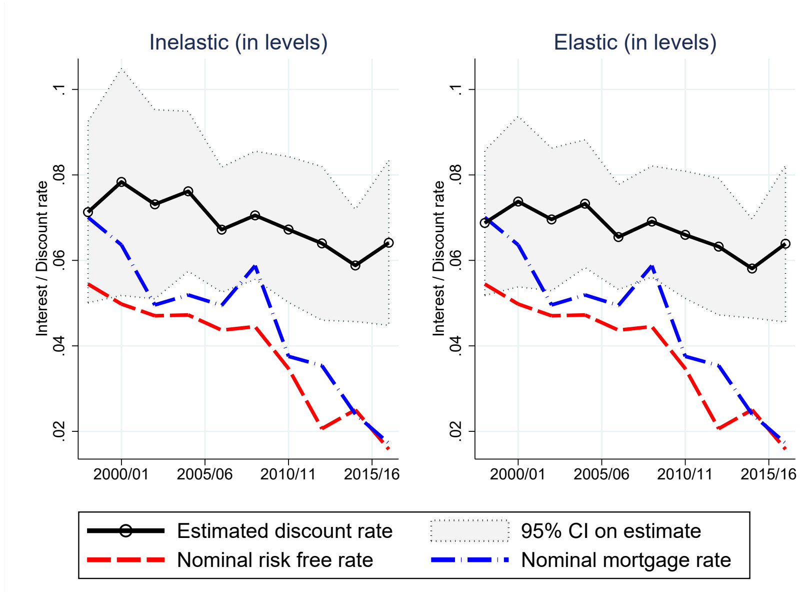

Figure B1 – Time variation: above/below median share developable land in LA

equal to $r^T_{t_0}$ in another transaction pair $t_0 - t_{-1}$. Our solution is to constrain coefficients to be the same for each year by interacting time dummies with variables, but assigning a positive or negative sign depending on whether they represent the first sale in year t_1 or the second sale in year t_0 for the sales pair in question.

We use this approach to estimate the evolution of implied discount rates in Figure 3. In Figure B1 below we can further show that the time pattern is very similar for places with above and below median share developable land averaged over 1990, 2000, and 2007@. In Figure B2 we show that the time pattern is also very similar for places with above and below median change in share developable land between 1990 and 2007. In unreported results we obtain highly similar results when we use alternative measures of the housing supply elasticity.

B3. Robustness

Measurement error. Taxes could potentially be mis-measured in our data because: *(i)* we assign a home sale an incorrect tax band, either because of a bad match or because we only observe the tax band only at the end of our sample period; *(ii)* we assign the correct tax band but the tax payable is incorrect due to local variation in parish taxes; or *(iii)* correct tax band but tax payable is incorrect

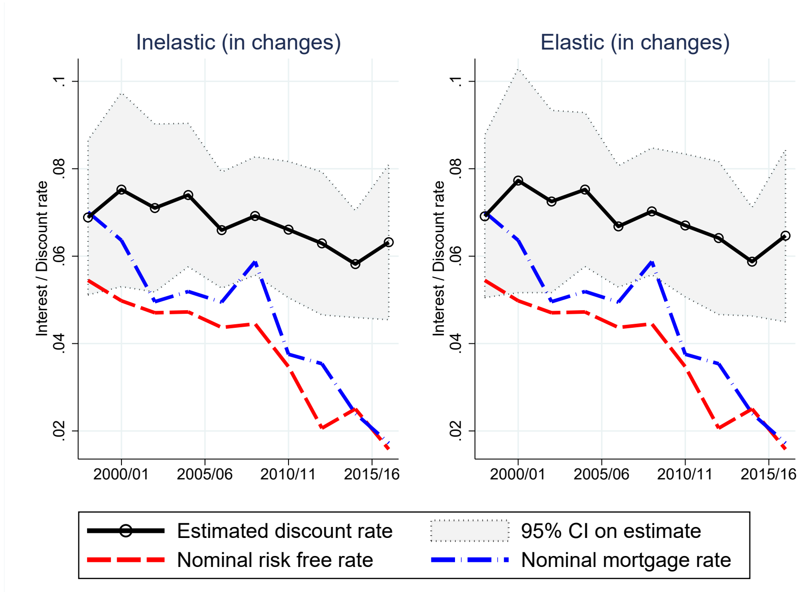

Figure B2 – Time variation: above/below median change in share developable land in LA

due to exemption or discounts.

We assess the impact of measurement issues for our baseline result (column (6) of Table 3) in Table B2. One reason why we may mismeasure taxes is because single occupiers are eligible for a tax discount and students are exempt. In the first two columns we show that removing homes with only 1 or 2 habitable rooms (such as lounges, living and dining rooms), and very large homes with more than 8 habitable rooms, makes little difference to our findings. Although we conservatively only matched tax data to homes when the address exactly corresponded, it may be that we have some false positives. To check this in column (3) we only use homes where the name in the sales and tax data also exactly matches the address in the EPC data (which have slightly more idiosyncratic address fields, possibly because EPCs are not as legally consequential as tax and home sales data). The coefficients are slightly larger in absolute terms suggesting slightly lower discount rates, but this is no more than half a percentage point.

Table B2 – Measurement error
(Dep var: Δ sale price in £)

Dep var: Δ sale price	(1)	(2)	(3)	(4)	(5)	(6)
Δ Council Tax	-28.01 (8.94)	-24.29 (8.66)	-34.10 (11.85)	-28.80 (12.14)	-23.42 (10.95)	-38.28 (14.49)
Baseline controls	✓	✓	✓	✓	✓	✓
Change to baseline:	drop < 3 rooms	3 to 8 rooms	perfect match	few re-bands	short held	3-8 rms, match, & few rebands
Sales pairs	253205	251950	165570	184343	63652	111761
R^2	0.81	0.81	0.82	0.81	0.63	0.83

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities.

Rebanding. Because of our sample selections, we do not expect homes moving tax band should be a major threat to identification. Note that if a home has been downwardly rebanded between sales, we overstate the true tax increase and we will again be biased towards finding higher discount rates. Conversely, if a home has been upwardly rebanded between sales, we understate the true tax change and estimates will be biased towards finding lower discount rates. Given that we are concerned not to conclude that households value the future rationally when in fact they do not, this latter case is arguably most problematic.

We investigate rebanding in Figure B3 where we interact the tax variable with deciles in the LA share of homes rebanded in the period for which we have disaggregated data (2009/10-2016/17).[8] Importantly, the implied discount rate in the lowest decile is very close to our baseline estimate. The remaining estimates are somewhat scattered across deciles with possible outliers at the 4^{th} and 10^{th} decile. While an F-test indicates we can reject the null of coefficient equality (p-value=0.0039), we cannot reject equality if we exclude the fourth decile (p-value=0.1739) or the highest decile (p-value=0.1006) from the test. However, we think it is not surprising that the highest decile coefficient is closer to zero than the others, as measurement error in the highest deciles will imply that the estimated coefficient is biased towards zero.

While we suspect that another source of heterogeneity is behind this finding, for reassurance in column (4) of Table B2 we show that our baseline result is robust to excluding homes in the top quartile of share rebanded. Another way

[8] The average annual share rebanded in the lowest, 5^{th}, and highest deciles is 0.09%, 0.18% and 0.37%.

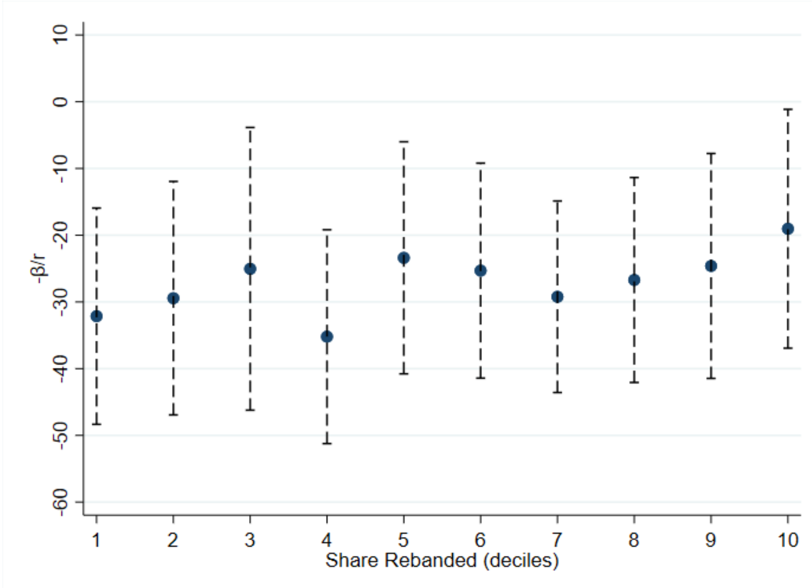

Figure B3 – Effect of taxes on home values for different LA share homes rebanded

to provide reassurance is to restrict attention to homes that are held for a short period, and so have a lower probability of being rebanded. In column (5) we find that focusing on homes resold within two years yields very similar results. Finally, in column (6) we combine the restrictions applied in columns (2) and (3) and (4). We again obtain a coefficient that is broadly similar to, but somewhat more negative than, our baseline estimate. Overall we conclude that measurement error is not a major threat to our main finding that households do not materially undervalue the future in this setting.

Including Parish-specific taxes. In Table B3 we provide robustness checks for the cross-sectional regressions reported in the main paper. In our main repeat sales results we specify taxes at the LA level because data for parish level taxes is not available for our whole repeat sales sample timeframe. Our proposition is that this is unlikely to be a problem as parish taxes made up only 0.6% of the LA budget requirement in 2011/12. We confirm here that this is indeed the case. Findings for rents (columns (1) and (2)) and prices (columns (3) and (4)) are robust to using Parish-specific taxes.

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

	(1)	(2)	(3)	(4)
	- rents -		- prices -	
Council Tax	-1.04	-1.08	-27.99	-26.95
	(0.44)	(0.45)	(14.02)	(13.99)
Quadratic in LA spend/head		✓		✓
Quadratic in KS2 test scores		✓		✓
Local green space		✓		✓
Home characteristics	✓	✓	✓	✓
$D_t \times D_\kappa \times D_{b1km}$	✓	✓	✓	✓
Tax measured at:	Parish	Parish	Parish	Parish
Observations	16697	16697	82990	82990
R^2	0.89	0.89	0.93	0.93

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

Bias-adjusted estimates. Here we investigate whether omitted variable bias may affect our estimates. We employ 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. Hence, she proposes a estimator that uses coefficient movements (*i.e.* housing controls, test scores, open space) and changes in the R^2 once controls have been included. Oster (2019) shows that a consistent estimate of the bias-adjusted coefficient can be calculated given assumptions on two key parameters: (i) the relative degree of selection of unobservables and observables, δ, and (ii) the maximum variation in house prices that can hypothetically be explained by observables and unobservables, $R^2_{\max}$. Following Oster (2019) we assume that $\delta = 1$, which is considered to be a useful *upper* bound. To choose $R^2_{\max}$, we first demean the data by all the fixed effects and then obtain the R^2 once all controls have been added. Oster (2019) argues that $R^2_{\max}$ is unlikely to be one, because of measurement error and randomness in human behaviour. More specifically, she suggests that $R^2_{\max}$ should be set as a multiplication factor Π of the R^2 with controls. Based on experimental studies she considers $\Pi = 1.3$ to be reasonable.

In Figure B4 we report results. For $\Pi = 1.3$ we find almost identical effects to the baseline specification reported in column (7) in Table 3. Even for very

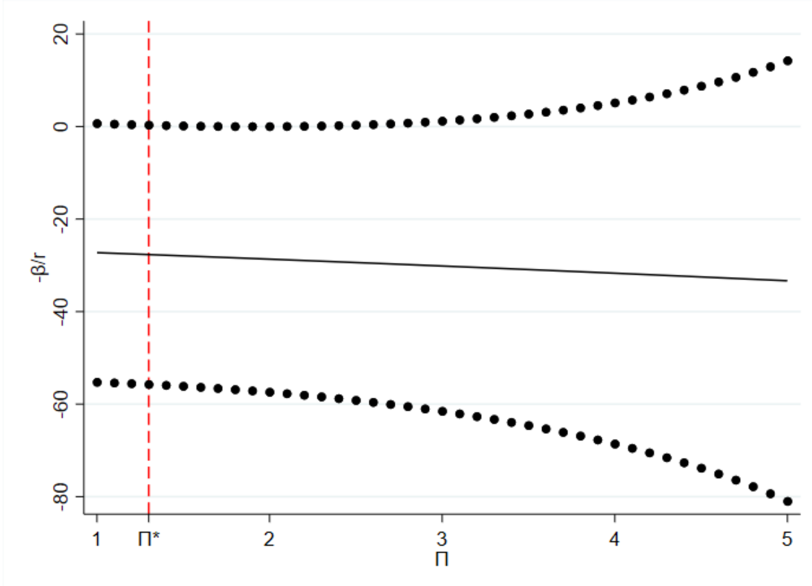

Figure B4 – Bias-adjusted estimates

Notes: The dotted lines denote 95% confidence bands. We run 250 bootstrap replications where we cluster at the local authority level

high values of Π, so if included controls are only explaining very little of the total variation in the dependent variables, we obtain similar estimates, although they become somewhat imprecise for $\Pi > 3$.

We think this provides additional evidence that omitted variable bias is unlikely to materially affect our estimates.

Controlling for sorting. Figure 2 in the main paper reveals that there is little evidence for sorting along the boundary in response to changes in taxes. However, as we show in Figure A3, there are some cross-sectional differences across LA borders in that the share of people with higher education and the share of employed people in managerial and professional occupations is larger on the higher tax side of boundaries. To examine whether these cross-sectional correlations should be a concern for our main estimates, we supplement our baseline specification with controls for a series of neighbourhood characteristics (measured in 2001) interacted with year pairs in Table B4. In all cases, our baseline estimate is unaffected by the addition of these further controls. This provides further reassurance that

Table B4 – Further sorting controls
(Dep var: Δ sale price in £)

	(1)	(2)	(3)	(4)	(5)	(6)
Δ Council Tax	-26.04	-27.52	-26.87	-28.02	-28.59	-28.42
	(8.51)	(8.61)	(8.31)	(8.72)	(8.39)	(8.28)
Baseline controls	✓	✓	✓	✓	✓	✓
$D_{\tilde{t}} \times$ degree share	✓					✓
$D_{\tilde{t}} \times$ lone parent share		✓				✓
$D_{\tilde{t}} \times$ managers/professionals share			✓			✓
$D_{\tilde{t}} \times$ middle age share				✓		✓
$D_{\tilde{t}} \times$ income					✓	✓
Year pairs	261121	261121	261121	261121	262560	261121
R^2	0.81	0.81	0.81	0.81	0.81	0.81

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities.

sorting does not present a threat to our inter-jurisdictional estimates.

Sample selection. Sensitivity to changes in specification and sample are investigated in Tables B5 and B6. The first of these tables shows that estimates of β/r_T are not significantly different to our baseline result under a variety of specification changes. First, we express all currency variables in 2015 values using the Consumer Price Index (column (1)). This specification yields a coefficient of -22.81, which implies *real* net of growth discount rate in the range $(0.033, 0.044)$. Adjusting this for real average growth in taxes (of 0.008) gives real gross rate of $(0.041, 0.051)$, which is as expected around 3 percentage points below the nominal rates we estimate in the main paper. In remaining columns we return to variations on our baseline nominal specification. In column (2), we find that results are robust to interacting our boundary trend controls with a property type indicator, which implies identification is achieved by comparisons between homes of the same type across LA boundaries, e.g. detached houses in Band D. Columns (3)-(5) examines robustness to alternative public good controls. We find results are unchanged by using linear public good controls; test scores that vary smoothly over space, rather than discontinuously at the LA boundary; or a specification in which we control for LPG changes as well as interactions between the levels of LPGs at the time of the first sale and dummies for year of sale pairs. Finally in column (6) of Table B5, we also find that results are robust to including more LA level controls variables (population, total LA spending on all services, value

Table B5 – Sensitivity – specification
(Dep var: Δ sale price in £)

Dep var: Δ sale price	(1)	(2)	(3)	(4)	(5)	(6)
Δ Council Tax	-22.81	-25.05	-25.93	-26.72	-30.36	-26.52
	(7.41)	(9.00)	(8.87)	(8.61)	(8.56)	(8.08)
$D_{\tilde{t}} \times D_{\kappa} \times D_{\geq 95} \times D_{b1km}$	✓		✓	✓	✓	✓
$D_{\tilde{t}} \times D_{\kappa} \times D_{\geq 95} \times D_{b1km} \times D_H$		✓				
Change to baseline:	in 2015 prices	fixed effects	linear LPGs	smooth test scores	initial LPG trends	more LA controls
Number of sales pairs	262560	218344	262560	262560	262560	262560
R^2	0.80	0.82	0.81	0.81	0.81	0.81

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. In column (2) D_H refers to a categorical variable for type of property (detached house, semi-detached house, terraced house, flat). Smooth test scores in column (4) vary smoothly over space and are not constrained by LA boundaries. Column (5) controls for LPG changes as in the baseline, but also interacts initial LPGs at the time of the first sale with year of sale pairs. Additional LA controls in column (6) are population, LA total service expenditure, and value of commercial property in LA (rateable value).

of commercial property).

Table B6 explores sensitivity to sample selections. The first column relaxes the restriction on price outliers while in column (2) we increases the scale of the restrictions on prices by cutting the top and bottom 5% of prices overall in each region and in each tax band. The coefficients are similar to, and not statistically discernible from our baseline specification, but less precisely estimated. In columns (3)-(6) we revert to the baseline outlier restriction and progressively relax sample restrictions intended to remove homes that change between sales in a pair. In column (3) we allow homes with 1 extension and in column (4) we relax this further by allowing homes with any number of extensions. In column (5) we also include homes for which we observe material changes in size, or that is listed as new more than once. In column (6) we allow any time gap between sales in a pair. Finally, in column (7) we estimate the effect of taxes on prices when we restrict attention to homes that have a gap of more than 8 years between sales in the pair. The coefficient is similar to our baseline results, albeit the standard errors are inflated, which gives us reassurance that our main results should generalise to homes held for long periods. All told, our baseline estimate is not highly sensitive to sample selections, but is somewhat less precisely estimated without those selections.

Table B6 – Sensitivity – sample restrictions
(Dep var: Δ sale price in £)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Council Tax	-29.17	-26.36	-32.00	-27.26	-25.87	-29.37	-35.19
	(12.10)	(7.02)	(9.01)	(8.94)	(8.70)	(12.37)	(20.55)
Baseline contols	✓	✓	✓	✓	✓	✓	✓
Sample changes:	no price cuts	cut 5% prices	allow 1 extension	allow any extension	and allow redev.	and any time gap	long held
Observations	266830	242451	409746	473158	479061	624956	115943
R^2	0.79	0.80	0.79	0.79	0.79	0.84	0.90

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. Cut 5% of prices indicates that the top and bottom 5% of prices in each region and in each tax band in each region are dropped. Any time gap relaxes the restriction that the gap between sales must be between 1 and 8 full years. Long held means gap between sales is greater than 8 years.

B4. Expected tax growth and idiosyncratic risk

In this Appendix section we examine the role of expected tax growth and idiosyncratic risk in our estimates. Previous related work that recovers discount rates from housing markets has assumed that expected growth is common across units and equates to long-term national averages e.g. Giglio, Maggiori and Stroebel (2015) and Bracke, Pinchbeck and Wyatt (2018) modify their leasehold-implied estimates of discount rates by very long term national real rental growth of 0.8%. These researchers also assume no role for for idiosyncratic risk, presumably on the basis these risks can be diversified away.[9]

We go beyond this assumption-driven approach in Table B7, where we explore the sensitivity of our estimates to measures of risk and expected growth. In places where the composition of the local Council is prone to change, we might expect to find a greater variability in taxes as local parties seek to implement their preferred policies. In column (1) of Table B7, we repeat our baseline specification but adding the interaction between ΔTax and the (standardised) standard deviation of the share of seats held by the largest party in the LA throughout our sample period. We find no evidence that political instability is associated with discount rates. In column (2), we replace the political measure with the (standardised) standard deviation of the annual percentage change in Council Tax in the LA

[9]As Giglio, Maggiori and Stroebel (2015) and Bracke, Pinchbeck and Wyatt (2018) estimate the term structure of discount rates, they do effectively allow for riskiness of housing to vary according to time horizon. In our setting, we are only considering homes that are perpetually owned, so our discount rates cannot be driven by varying aggregate risk at different maturities.

over our sample period. Again, the interaction is not significant. These results could reflect that idiosyncratic risks inherent in property taxes (which are small relative to total portfolios) can be eliminated by diversification.

The remaining columns of Table B7 are intended to assess whether buyers' tax growth expectations drive our estimates. Given that houses are highly durable and the houses in our sample represent perpetual claims, we would ideally consider infinite-horizon growth expectations. We are of course unable to observe these expectations nor how they are formed, so we are limited to using proxies. In column (3), we deploy the mean percentage change in taxes in the LA over our near-20 year sample period in the interaction term, and in column (4) we include both moments simultaneously. We find that neither is significant. This may indicate that buyers expect cyclicality in growth rates (i.e. higher medium term growth will be balanced by lower growth in the future), but another possible explanation is that our measure is simply a poor proxy for buyers' growth expectations (e.g. because tax growth is hard to predict). To explore this, we obtain historic data for LAs for the period 1978 and 1988 from the Department for the Environment, and compare compound annual growth rates between these two years with the corresponding rate for 1998-2008 for the 68 LAs for which we can match codes. We find a strong negative correlation between average growth ($\rho = -0.4$) in the two 10 year periods, suggesting that the assumption that rapid tax growth will be later compensated by lower growth is not unreasonable.[10] Finally, in columns (5) - (7) we similarly find that LA-specific future tax growth over the next 1, 2, or 3 years respectively also do not significantly modify the effect of taxes on transaction prices.[11]

We can also explore the role of expectations by examining the effects of the tax limiting policies described in Section I.B. The policies may have influenced buyers' expectations about the path of future taxes as they incentivised tax freezes in the short-term and also put in place the need for a local referenda to approve large tax changes in the future. It is possible the policies may have had differential effects on buyers expectations, and hence on discount rates, depending on whether the

[10]The source of the LA tax data for 1978 and 1988 is Local Authority Rates and Rateable Values, 1978-1989, held in the UK Data Archive under dataset reference SN 2528. The LAs are listed in this data under bespoke Department for the Environment codes. We were able to hand match 68 using hard copies of CIPFA's Local Authority Rating Manual for different years.

[11]We estimate $\Delta P_{it} = -\frac{\beta}{r_T}\Delta T_{it} + \mu(T_{it} \times g_{T,it}) - T_{it_0} \times g_{T,it_0} ...$ where $g_{T,it}$ and g_{T,it_0} indicate the percentage growth in taxes over the next 1, 2, or 3 years at the later and earlier sales in the pair respectively, which we use as proxies for expected growth $E(g_T)$.

Table B7 – Risk and average tax growth interactions
(Dep var: Δ sale price in £)

Dep var: Δ sale price	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Council Tax	-27.62	-33.74	-29.24	-33.82	-25.79	-25.36	-28.80
	(8.59)	(8.97)	(7.47)	(9.39)	(9.57)	(10.91)	(12.58)
× SD election seats	-1.22						
	(1.39)						
× SD Council Tax		2.67		2.87			
		(2.45)		(3.16)			
× MT % tax growth			0.81	-0.28			
			(1.59)	(2.05)			
× ST % tax growth					0.05	0.07	-0.13
					(0.11)	(0.20)	(0.32)
Short term definition					1 year	2 years	3 years
Number of of sales pairs	262560	262560	262560	262560	261860	261860	261860
R^2	0.80	0.80	0.80	0.80	0.81	0.81	0.81

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. All regressions include the controls in our main specification (column (7) of Table 3), and additional interactions as indicated. MT % tax growth refers to LA specific medium term tax growth. It is a time invariant measure computed as the average annual change in tax in the LA over our whole sample period. ST % tax growth refers to LA specific short term tax growth. It is a time varying measure that is computed as the average annual change in tax in the LA in t years subsequent to a sale. As noted in the Table we set t as 1, 2, or 3.

LAs were effectively constrained from tax rises. To test this we assume LAs are constrained by the policies if the immediate pre-policy growth rate in taxes was above 3% (we find that alternative assumptions about how to define the constrained group yield similar results). We then estimate implied discount rates over time for constrained and unconstrained LAs and report estimates in Figure B5. If growth expectations of buyers in the constrained group are reduced more than those in the unconstrained group, we should expect to see a relative increase in implied discount rates for the constrained group from around 2010 onwards. In fact, there is little evidence for any such difference in the data as the discount rates for both groups trend in a similar way post 2010. As with previous results this again suggests that recent LA tax setting decisions are not central to setting long-term expectations, perhaps because buyers anticipate that periods of rapid tax growth will be later compensated by periods of lower growth and vice-versa.

B5. Heterogeneity

Heterogeneity by tax level and tax band. In this Appendix section, we explore heterogeneity. We first examine the effect of taxes on house prices over the distribution of tax changes in our data by generating a categorical variable for

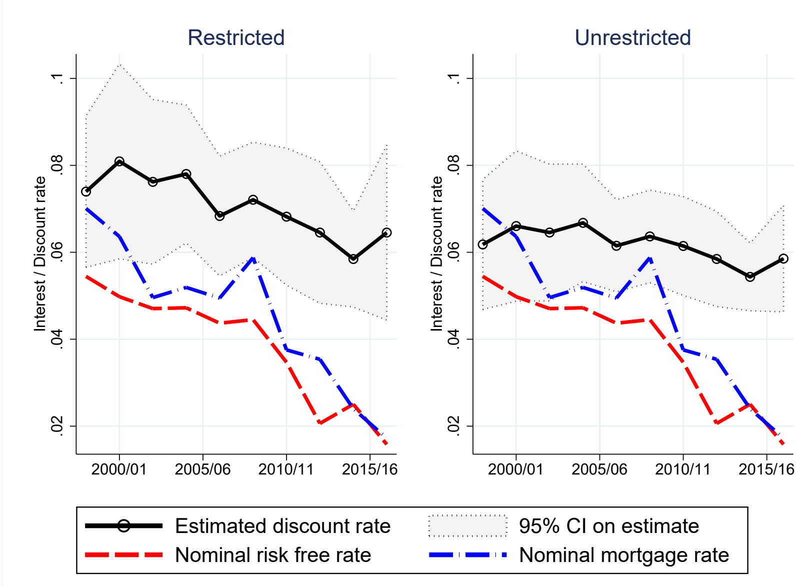

Figure B5 – Time variation: restricted/unrestricted by 2011 policy changes

the decile of the tax change in our main sample, and then interacting this bin variable with the tax change. This permits us to use a more flexible functional form but is also motivated by experimental findings that small future amounts are discounted at a higher rate than larger amounts (e.g. Frederick, Loewenstein and O'Donoghue, 2002). Point estimates (blue dots) and the 95% confidence interval (black whiskers) are plotted in Figure B6. We find that the effect of taxes on home values is stable across the distribution of tax changes, although for the smallest changes the estimates are imprecise. A formal test indicates insufficient evidence to reject the null that the coefficients are all equal (the F-test p-value is 0.21).

We repeat this exercise but taking deciles in the initial tax level (rather than the tax change) in Figure B7. While the coefficient on the first bin is somewhat closer to zero, results are otherwise similar across bins and moreover there is not sufficient evidence to reject the null hypothesis that the coefficients are all equal to one another (the p-value of the F-test is 0.11).

An alternative avenue to explore heterogeneity is to interact Δ Tax with categorical variables for the tax band of each sale pair. As this exercise typically yields imprecise estimates we test this using the 2km boundary sample, which

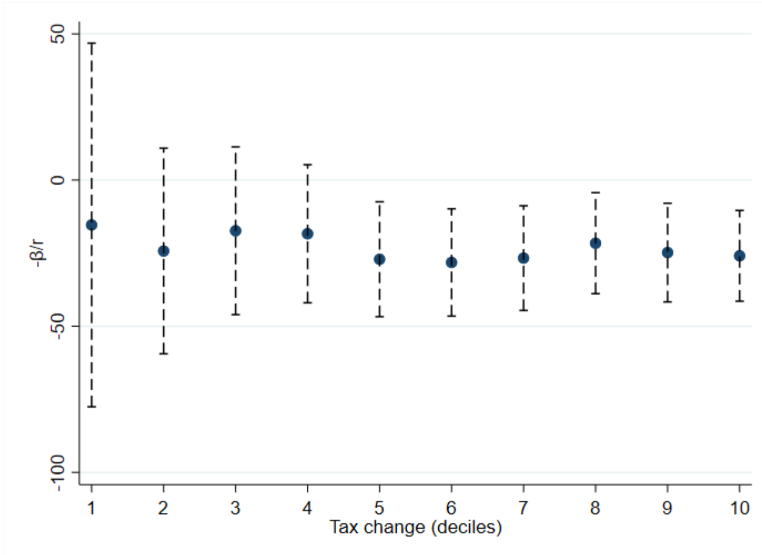

Figure B6 – Effect of taxes on home values for different size tax changes

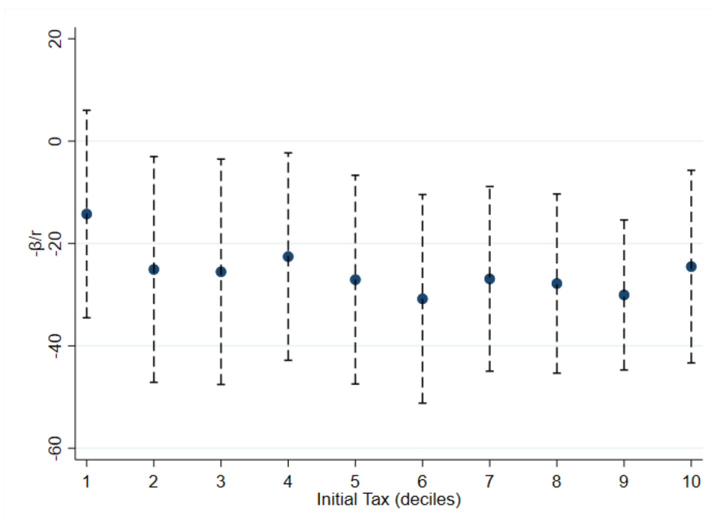

Figure B7 – Effect of taxes on home values for different initial taxes

comprises more observations, and we allow band G homes with 1 extension into the estimation sample to expand the sample size for this group. Our findings are shown in Figure B8. We find that the effect of taxes on prices is close to zero

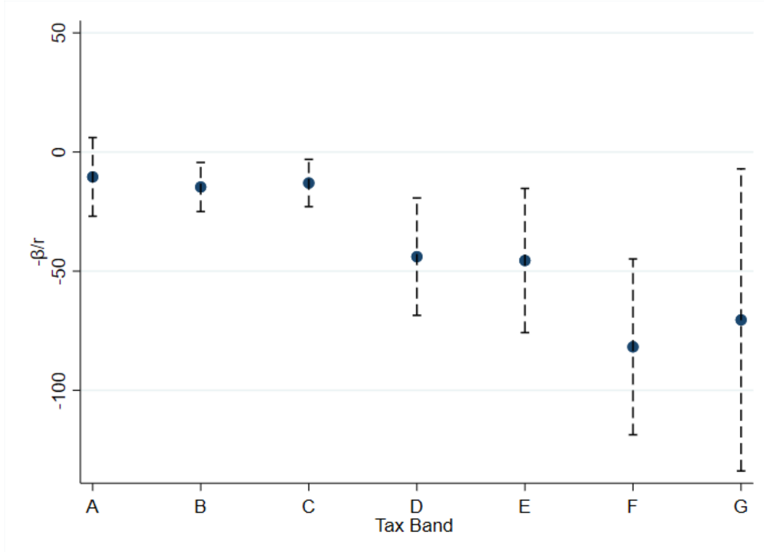

Figure B8 – Effect of taxes on home values by tax band

Notes: Regression uses 2km boundary region and allows for 1 extension for Band G homes, but otherwise as baseline model.

Coefficients and implied discount rates assuming β=1:
Band A: coeff= -10.5; implied r=9.5%; Band B: coeff= -14.7; implied r=6.8%;
Band C: coeff= -13.0; implied r=7.7%; Band D: coeff= -43.9; implied r=2.3%;
Band E: coeff= -45.6; implied r=2.2%; Band F: coeff= -81.8; implied r=1.2%;
Band G: coeff= -70.5; implied r=1.4%;

for lower tax bands and increases in magnitude at higher bands.[12] In particular, assuming β=1 throughout, the coefficient for homes in bands A-C imply discount rates in the range of 5-10% whereas those in higher bands imply rates under 3%. These findings thus suggest that implied discount rates are higher in the lowest tax bands in the inter-jurisdictional approach. In Appendix Table B9, we obtain qualitatively similar findings when we use the intra-jurisdictional approach.

Heterogeneity by regions. We next estimate discount rates in each region, reporting results in in Figure B9. Here we find that the parameter estimate is slightly larger in absolute terms in London but otherwise highly similar in other

[12] Note that when we retain only band G homes with no extensions in the estimation sample, the estimate for this tax band is close to zero with large standard errors. The results for the band G estimate should therefore be interpreted with caution.

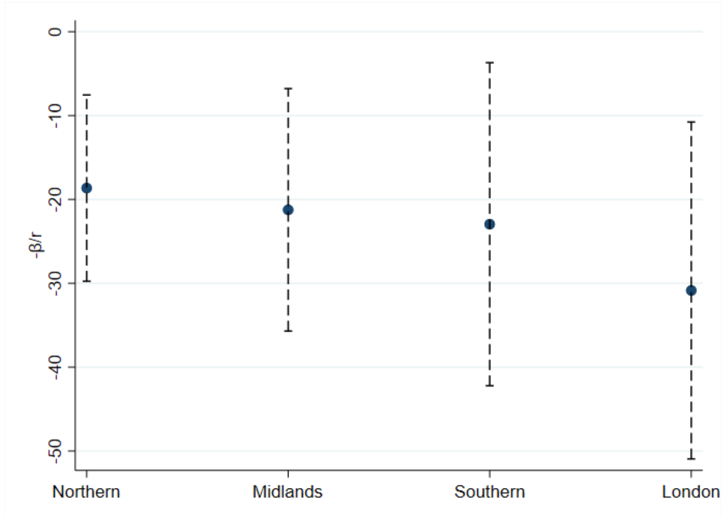

Figure B9 – Effect of taxes on home values in different regions

regions. An F-test reveals that the coefficients across all regions are not significantly different (the p-value 0.35).

Heterogeneity by homebuyer characteristics. Table B8 reports further heterogeneity tests. In theory, discount rates may vary with the level of sophistication and patience of home buyers, and with individuals' inter-temporal opportunity cost of funds. As we cannot observe these buyer characteristics directly in our data, we generate proxies using supplementary information.

All else equal we would expect less borrowing constrained households to apply lower discount rates. Merging our sales records with data from Nationwide – the second largest mortgage provider in the UK – allows us to identify homes purchased with Nationwide loans in our data, although we note this is a subset of all mortgaged homes, and the match is imperfect.[13] Notwithstanding these caveats, we can evaluate the qualitative link between mortgages and discount rates via interactions between a Nationwide mortgage indicators and tax variables.[14]

[13]This is because matching fields are not common. Nationwide does not contain full address information or date of sale, but instead full postcode and date of mortgage offer, while prices do not always match in the two datasets. Our strategy is to assume true merges if postcodes coincide, dates are within 9 months, and prices are within 10%. Other matching strategies yield qualitatively similar findings.

[14]In particular we modify the tax terms to $\Delta P_{it} = -\frac{\beta}{r_T}\Delta T_{it} + \gamma(T_{it} \times M_{it} - T_{i\tau} \times M_{i\tau})$... where M_{it} and $M_{i\tau}$ indicate mortgage at the later and earlier sales in the pair respectively.

We report this in column (1) of Table B8. Results suggest that tax implied discount rates on homes mortgaged with Nationwide are on average slightly higher (here by around 15 basis points) than for other homes, which will be a mix of mortgaged and un-mortgaged homes.

We next exploit that notch points in UK Loan-to-Value ratios (e.g. at 75%, 80% etc.) imply large jumps in borrowing costs (Best et al., 2020). Buyers can potentially manipulate the LTV through several channels. These include negotiating price with sellers, by finding small amounts of additional down-payment (e.g. by postponing a purchase for a short period in which they build savings, or by reducing consumption), or ultimately by choosing a slightly less expensive home. We hypothesise that more sophisticated buyers would utilise any or all of these strategies to ensure they purchase at or slightly below LTV notch points, and hence that relatively unsophisticated buyers will be found above LTV notch points. In column (2) we find that buyers with loans at or just below notch points indeed have slightly but significantly lower implied discount rates than other mortgages in our data.

Borrowers facing higher interest rates might be expected to apply higher discount rates to tax payments. Although our loans data contains no information on loan type or interest rate, we generate a proxy for higher borrowing costs using deciles of residuals from a regression of LTV on a three way interaction of region, a dummy for existing loan holder (which captures inter alia first time home buyers), and mortgage year. In column (3), we find no evidence that this measure is economically or statistically relevant to discount rates. This perhaps reflects the limitations of our loan data.

The remaining columns of Table B8 interact Δ Tax with neighbourhood level variables. Small area estimates of income are not available, so we use a model-based estimate of median household income for 2004 which is computed for relatively large neighbourhoods of between 2,000 and 6,000 households. We interpret this as a proxy for borrowing constraints, albeit income may also capture sophistication and patience (e.g. Belgibayeva et al. (2020) find inertia in refinancing UK home loans varies by income). The second, which we interpret as a more direct measure of homebuyer sophistication, is the share of population aged 16-74 with at least degree (or degree equivalent) education in the 2001 Census.[15] This is

[15]Specifically this includes the following qualifications: First degree, Higher degree, NVQ levels 4 and 5, HNC, HND, Qualified Teacher Status, Qualified Medical Doctor, Qualified Dentist, Qualified Nurse,

Table B8 – Heterogeneity
(Dep var: Δ sale price in £)

	(1)	(2)	(3)	(4)	(5)
Δ Council Tax	-27.40	-27.41	-27.41	-27.35	-21.06
	(8.66)	(8.66)	(8.66)	(7.12)	(6.37)
× Mortgage	1.06	1.54	1.47		
	(0.12)	(0.16)	(0.22)		
× Mortgage × Notch indicator		-0.96	-1.02		
		(0.18)	(0.22)		
× Mortgage × Borrowing cost proxy			0.02		
			(0.04)		
× N'hood income (standardised)				-2.77	
				(7.47)	
× N'hood share degree (standardised)					-4.87
					(4.03)
$D_{\tilde{t}}\times$ N'hood income				✓	
$D_{\tilde{t}}\times$ N'hood share degree					✓
Number of sales pairs	262560	262560	262560	262560	261121
R^2	0.81	0.81	0.81	0.81	0.81

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. All regressions include the controls in our main specification (column (7) of Table 3). Columns (4) and (5) contain additional controls interacted with year pairs ($D_{\tilde{t}}$). Neighbourhood income is a model-based estimate of median household income in 2004 that is defined over neighbourhoods of 2,000-6,000 households. Neighbourhood share degree is the share of residents holding a degree or higher level qualification in the 2001 Census, and is computed over areas containing around 125 households.

defined over very small administrative areas (Output Areas) of around 125 households and should therefore proxy well for buyer characteristics. As expected, in columns (3) and (4) we find that the interactions between each characteristic and Δ Tax is negative and meaningful, albeit in both cases the estimate is insignificant.[16] If we compare neighbourhoods where everyone has a degree to a neighbourhood where no one has a degree, the implied discount rate is about 20% lower in the former.

To probe this less parametrically, we next create deciles in the two neighbour-

Midwife, Heath Visitor.

[16]Note that we control for trends in income and education respectively in these regressions by interacting the relevant measure with year pairs. These controls are important here because without them we obtain positive coefficients for the interactions of tax changes and standardised versions of these measures. The counter-intuitive implication would be that better educated buyers with higher incomes are more impatient than buyers in markets further down the income and education distributions. As we show in Table B4, our baseline estimates are unaffected by the inclusion of these and other trend controls.

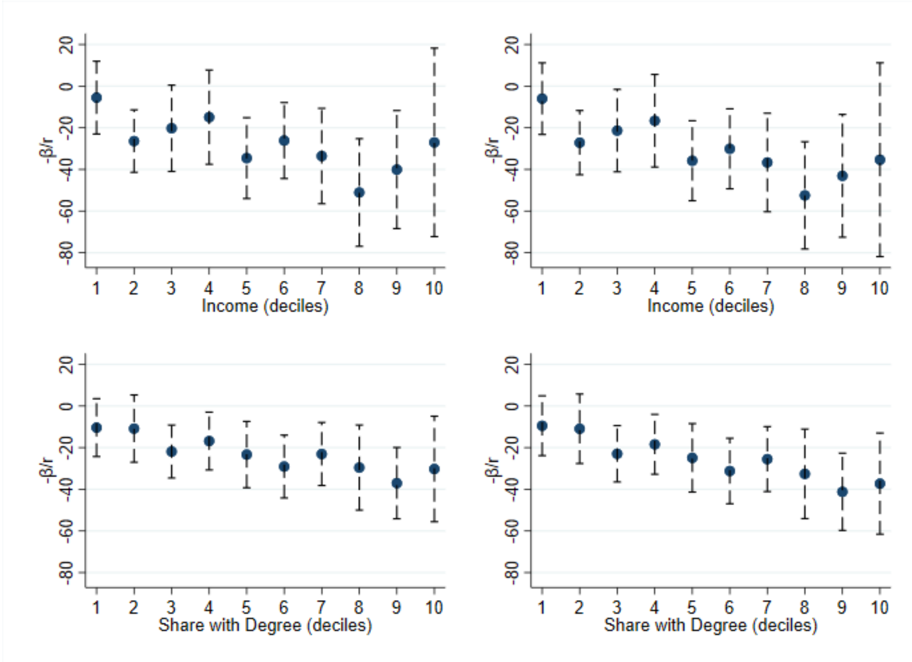

Figure B10 – Effect of taxes on home values by neighbourhood income and education

Notes: Each plot denotes coefficients from a separate regression. Regression in top left and bottom left quadrants controls for decile trends but otherwise as baseline model (column (7) of Table 3). Regressions in top right and bottom right quadrants additionally control for Δ Tax $\times$ the other characteristic.

hood characteristics, 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 the top left and bottom left quadrants of Figure B10. Consistent with priors we find that the coefficients become more negative at higher deciles for both characteristics, suggesting higher discount rates in higher income and more highly educated neighbourhoods. Although statistically insignificant, if one assumes full capitalisation, the coefficients in the lowest one or two deciles in each case imply high discount rates in the range of 10% to 20%, which are consistent with misoptimisation, whereas coefficients in the third decile upwards imply discount rates of 5% or lower. Hence, heterogeneity in discount rates is sizeable.

Estimates become imprecise if we include interactions with deciles in both characteristics simultaneously as to make a distinction between borrowing constraints and buyer sophistication. In lieu of this, the right hand quadrants of Figure B10

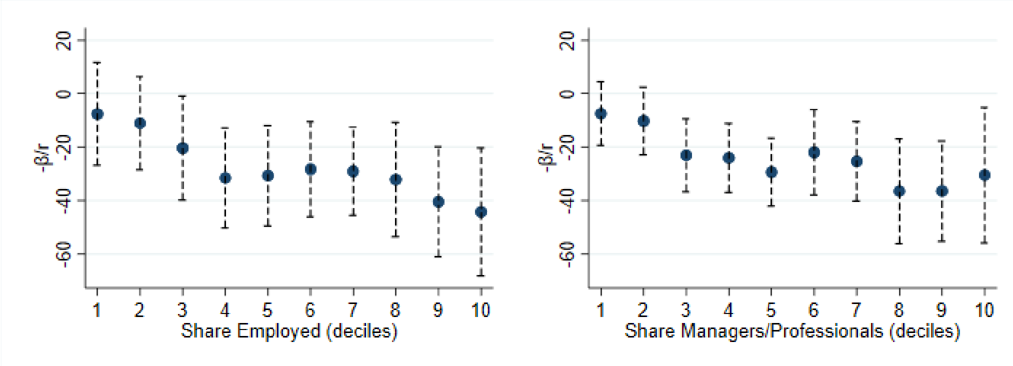

Figure B11 – Effect of taxes on home values by employment and professional share

Notes: Each plot denotes coefficients from a separate regression. Regressions control for decile trends but otherwise as baseline model.

repeats the exercise but additionally controlling for continuous interactions of Δ Tax and the other characteristic. These yield similar findings. Although we are unable to fully disentangle income and sophistication, this suggests that both factors may correlate with implied discount rates.

In Figure B11 we examine heterogeneity on the effect of taxes on rents in two additional characteristics: share of people aged 16-74 who were employed as at the 2001 Census, and share of employed people aged 16-74 working as Managers and senior officials or in Professional occupations at the same point in time. In both cases the variables are computed over the tighter neighbourhood with around 50 households. These additional results yield similar findings to those in the main paper.

Heterogeneity in the capitalisation parameter. One possible explanation for the finding of heterogeneity in $beta/r$ is that the capitalisation parameter, β,varies across income and education level. To explore this, we repeat the heterogeneity tests on rental data, but now using 5 quantiles to reflect the smaller samples available.

Results are shown in Figure B12. Here we find that estimates are less sensitive to controlling for the quantiles separately, but these controls considerably inflate standard errors. We show graphs both without (left) and with (right) the quantile

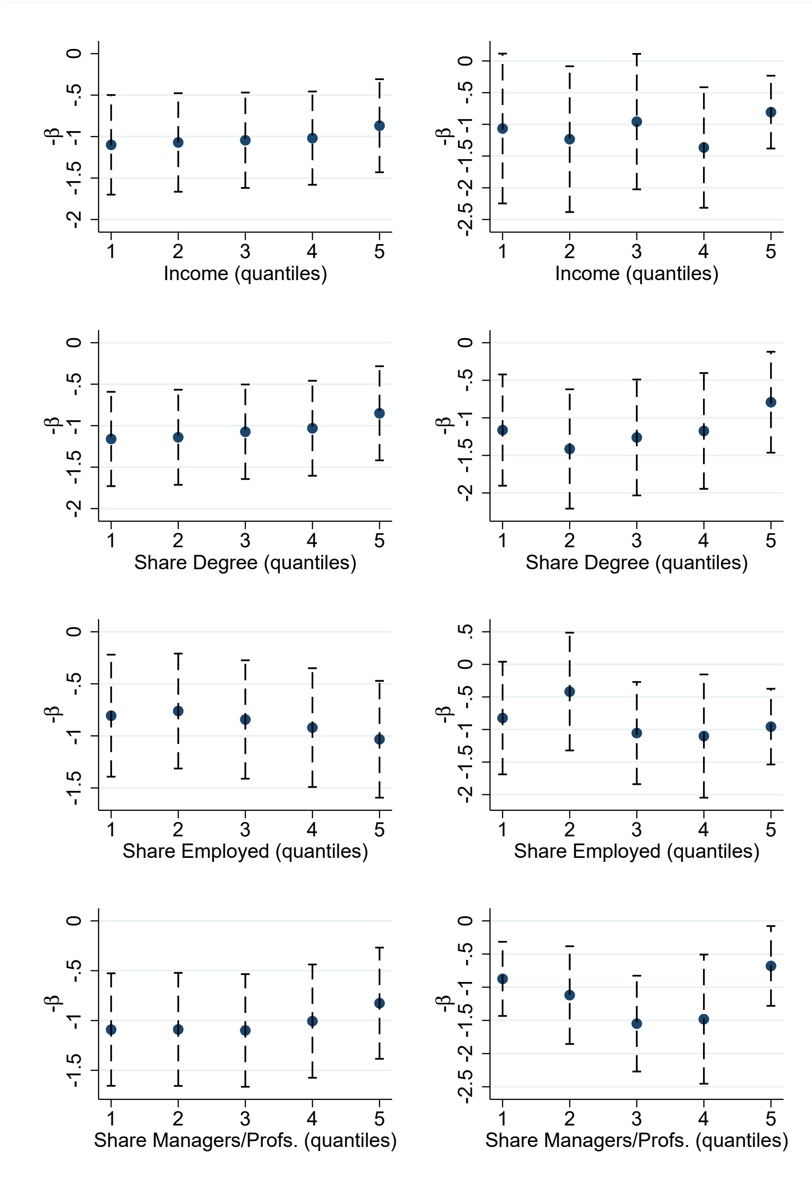

Figure B12 – Effect of taxes on home rents by homebuyer characteristics

Notes: Regressions on right control for quantile fixed effects.

controls for transparency. In each case there is little evidence to suggest that $\tilde{\beta}$ strongly varies by neighbourhood characteristics as most of the estimates are very close and not significantly different from one.

Lastly, in Table B9, we provide supporting evidence for the main results by

Table B9 – Heterogeneity by tax band
(Dep var: as indicated)

	(1) Prices: inter	(2) Prices: intra	(3) Rents: inter
Council Tax × Group 1	-13.53	-24.07	-0.90
	(8.06)	(22.09)	(1.13)
Council Tax × Group 2	-16.43	-46.45	-1.05
	(4.97)	(16.17)	(0.82)
Council Tax × Group 3	-37.52	-37.58	-0.99
	(10.54)	(19.96)	(0.32)
Observations	511976	31285	30011
R^2	0.79	0.92	0.89

Notes: Standard errors in parenthesis clustered on post 2009 Local Authorities. Regressions in columns (1) and (2) are repeat sales using inter and intra jurisdictional variation. Rent regression in column (3) is cross-sectional estimated in levels. Inter-jurisdictional regressions use a 2km boundary sample. Group 1 is tax band A in columns (1) and (3) and homes close to the AB threshold in column (2). Group 2 is tax band B in columns (1) and (3) and homes close to the BC threshold in column (2). Group 3 is all other tax bands or thresholds combined.

examining the effect of taxes on prices and rents in different tax bands, adopting a consistent approach across the different approaches in the main paper. In columns (1) and (3) we estimate the effect of taxes on prices and rents respectively in three groups using the inter-jurisdictional approach. For these columns, the first group (Group 1) is tax band A, the second is tax band B, and the third group combines all other tax bands. For column (2) we estimate the effect of taxes on prices using the intra-jurisdictional approach. Here the first group is homes close to the AB threshold in column, Group 2 is homes close to the BC threshold, and Group 3 is all other thresholds.

We find somewhat higher implied discount rates for properties in tax band A and B. Using the intra-jurisdictional regressions we also find suggestive evidence that discount rates are higher for properties in bands A and B, although the effects are harder to identify because the intra-jurisdictional regression exploit variation in prices and taxes of properties that are close to the threshold of two tax bands. For the capitalisation parameter we do not find material differences between properties in different bands.

*

REFERENCES

Bank of England. 2020. "Monthly Government Liability curve (nominal) archive (1970 to 2015, and 2016 to present)." Accessed December 20, 2020 `https://www.bankofengland.co.uk/statistics/yield-curves`.

Bank of England. n.d.. "Mortgage rates: series IUMBV34 and IUMTLMV." Accessed December 20, 2020 `https://www.bankofengland.co.uk/boeapps/database/FromShowColumns.asp?searchText=IUMBV34+IUMTLMV&Travel=`.

Belgibayeva, Adiya, Teresa Bono, Philippe Bracke, João Cocco, and Tommaso Majer. 2020. "FCA occasional papers in financial regulation."

Best, Michael Carlos, James S Cloyne, Ethan Ilzetzki, and Henrik J Kleven. 2020. "Estimating the elasticity of intertemporal substitution using mortgage notches." *The Review of Economic Studies*, 87(2): 656–690.

Bracke, Philippe, Edward W Pinchbeck, and James Wyatt. 2018. "The time value of housing: Historical evidence on discount rates." *The Economic Journal*, 128(613): 1820–1843.

Burgess, Simon, Ellen Greaves, and Anna Vignoles. 2019. "School choice in England: evidence from national administrative data." *Oxford Review of Education*, 45(5): 690–710.

Burgess, Simon, Ellen Greaves, Anna Vignoles, and Deborah Wilson. 2015. "What parents want: School preferences and school choice." *The Economic Journal*, 125(587): 1262–1289.

Department for Communities and Local Government. 2020. "English Housing Survey, 2013-2014: Household Data." *UK Data Service. SN 7801*, Accessed August, 2020 `https://beta.ukdataservice.ac.uk/datacatalogue/studies/study?id=7801`.

Department for Communities & Local Government. 2017. "Domestic Energy Performance Certificates." *Open Data Communities*, Accessed May 26, 2017 `https://epc.opendatacommunities.org`.

Department for Education. n.d.. "School Key stage 2 test scores 1995-96 to 2015-16." *GOV.UK*, Accessed August 8, 2017 `https://www.compare-school-performance.service.gov.uk/download-data`.

Department of the Environment. 1988. "Local Authority Rates and Rateable Values, 1978-1989." *UK Data Service. SN 2528*, Accessed August, 2020 `https://beta.ukdataservice.ac.uk/datacatalogue/studies/study?id=2528`.

Frederick, Shane, George Loewenstein, and Ted O'Donoghue. 2002. "Time Discounting and Time Preference: A Critical Review." *Journal of Economic Literature*, 40(2): 351–401.

Gibbons, Stephen, Stephen Machin, and Olmo Silva. 2013. "Valuing school quality using boundary discontinuities." *Journal of Urban Economics*, 75: 15–28.

Giglio, Stefano, Matteo Maggiori, and Johannes Stroebel. 2015. "Very long-run discount rates." *The Quarterly Journal of Economics*, 130(1): 1–53.

Hilber, Christian AL, Teemu Lyytikäinen, and Wouter Vermeulen. 2011. "Capitalization of central government grants into local house prices: Panel data evidence from England." *Regional Science and Urban Economics*, 41(4): 394–406.

Historic England. n.d.. "Shapefiles for Conservation Areas in England, 2008 and 2012." Unpublished data. Accessed August, 2013.

HM Land Registry. 2017. "Price Paid Data." *GOV.UK*, Accessed May 24, 2017 `https://www.gov.uk/government/statistical-data-sets/price-paid-data-downloads#single-file`.

Koster, Hans R. A., and Edward W. Pinchbeck. 2021*a*. "E-code mapping file for: How do Households Value the Future? Evidence from Property Taxes." *American Economic Association [publisher], Inter-university Consortium for Political and Social Research [distributor]*, `http://doi.org/10.3886/E130601V1`.

Koster, Hans R. A., and Edward W. Pinchbeck. 2021*b*. "LA code 1978-1988 mapping file for: How do Households Value the Future? Evidence from Property Taxes." *American Economic Association [publisher], Inter-university Consortium for Political and Social Research [distributor]*, `http://doi.org/10.3886/E130601V1`.

Ministry of Housing, Communities & Local Government. 2014. "Table 4: local precepting authorities in England 2014 to 2015." *GOV.UK*, Accessed September 7, 2017 `https://www.gov.uk/government/statistics/parishes-and-other-local-precepting-authorities-in-england-2014-to-2015`.

Ministry of Housing, Communities & Local Government. 2016*a*. "Council Tax statistics for town and parish councils in England: 2016 to 2017." *GOV.UK*, Accessed December 7, 2016 `https://www.gov.uk/government/statistical-data-sets/live-tables-on-council-tax`.

Ministry of Housing, Communities & Local Government. 2016*b*. "Local Authority planning application statistics, 1991-2013." Unpublished data. Accessed November 6, 2016.

Nationwide Building Society. 2017. "House Price Index." Accessed November 11, 2017 `https://www.nationwide.co.uk/about/house-price-index/headlines`.

Nationwide Building Society. n.d.. "Transactions data, 1995 to 2015." Unpublished data. Accessed December 30, 2016.

Office for National Statistics. 2010. "Household earnings estimates: Model-based estimates of income for MSOAs (2004/5)." Accessed September 16, 2015. `https://data.gov.uk/dataset/7dec3388-30d6-4b8c-8510-de15cbb8f765/household-earnings-estimates-model-based-estimates-of-income-for-msoas`.

Office for National Statistics. 2011*a*. "2001 and 2011 Census: boundary data (England and Wales)." *UK Data Service*, Accessed December 1, 2017 `https://borders.ukdataservice.ac.uk/easy_download.html`.

Office for National Statistics. 2011*b*. "2001 Census Aggregate Statistics Datasets (with digital boundary data)." *Casweb, UK Data Service*, `http://casweb.ukdataservice.ac.uk`.

Office for National Statistics. 2016. "Postcode Directories, 2006-2016." *UK Data Service Census Support*, Accessed August 10, 2017 `https://borders.ukdataservice.ac.uk/pcluts.html`.

Office for National Statistics. 2017*a*. "2011 Census aggregate data. UK Data Service (Edition: February 2017)."

Office for National Statistics. 2017*b*. "CPI All Items Index (Series ID: D7BT), August 2017 release." Accessed December 1, 2020 `https://www.ons.gov.uk/economy/inflationandpriceindices/timeseries/d7bt/mm23/previous`.

Office for National Statistics. 2018. "Parishes (December 2015) Full Extent Boundaries in England and Wales Version 2." Accessed April 1, 2020. `https://geoportal.statistics.gov.uk/datasets/1ba5ae2ea4934281b9fa961923b5933f_1`.

Office for National Statistics. 2020. "RPI: housing: council tax & rates (Jan 1987=100), Series ID: DOBR." Accessed December 1, 2020 `https://www.ons.gov.uk/economy/inflationandpriceindices/timeseries/dobr/mm23?referrer=search&searchTerm=dobr`.

Ordnance Survey. n.d.. "Code-Point with polygons." Accessed December 1, 2017 `https://www.ordnancesurvey.co.uk/business-government/products/code-point-polygons`.

Oster, E. 2019. "Unobservable Selection and Coefficient Stability: Theory and Evidence." *Journal of Business and Economic Statistics*, 37(2): 187–204.

Pope, Addy. 2017. "Green Belt England 2001." *University of Edinburgh*, Accessed July 1, 2017.

Ross, Stephen, and John Yinger. 1999. "Sorting and voting: A review of the literature on urban public finance." *Handbook of Regional and Urban Economics*, 3: 2001–2060.

The Elections Centre. n.d.. "Council compositions by year." Accessed October 30, 2017 `http://www.electionscentre.co.uk/?page_id=3802`.

UK Centre for Ecology & Hydrology. n.d.. "Land Cover Maps (for 1990, 2000, 2007)." Accessed July 1, 2017 `https://www.ceh.ac.uk/ukceh-land-cover-maps`.

Valuation Office Agency. 2015. "Local Authority stock of homes 2015." Accessed December 17, 2020 `https://www.gov.uk/government/statistics/council-tax-stock-of-properties-2015`.

Valuation Office Agency. 2016. "Local Authority re-banding statistics 2009/10-2016/17." Accessed May 7, 2020 `https://www.gov.uk/government/collections/valuation-office-agency-council-tax-statistics`.

Valuation Office Agency. 2017. "Home-level taxbands." *mycounciltax.org.uk*, Last Accessed August 1, 2017 `https://www.gov.uk/government/statistical-data-sets/live-tables-on-council-tax`.